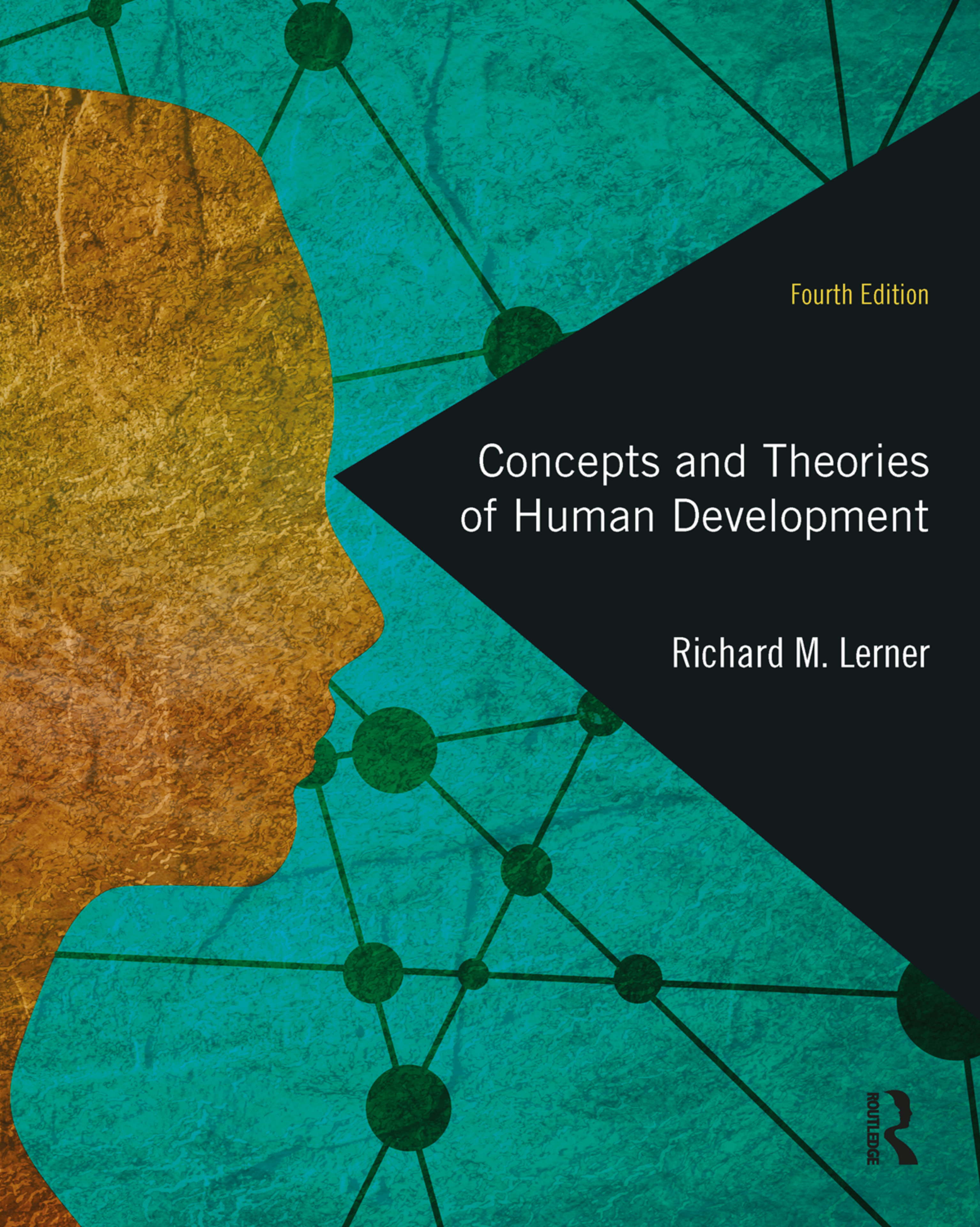


Fourth Edition

Concepts and Theories of Human Development

Richard M. Lerner

ROUTLEDGE

Concepts and Theories of Human Development

Concepts and Theories of Human Development is the most comprehensive and in-depth overview of the foundational theoretical contributions to understanding human development and the influence of these contributions for contemporary research and application in developmental science. Since its initial publication in 1976, it has been an essential resource for students and professionals alike, and has become the go-to book for graduate students studying for their comprehensive exam on human development. In this new Fourth Edition, Richard M. Lerner concentrates his focus on advanced students and scholars already familiar with the basic elements of major psychological theories.

The book discusses the assumptions involved in such topics as stage theories, the nature–nurture issue, the issue of continuity–discontinuity, and the important role of philosophical ideas about theories—in particular, metatheories—in understanding the links between theory and research. It particularly focuses on relational developmental systems (RDS) metatheory, exploring its roots in the 1930s, following its development into the present day, and contrasting it with the fundamentally flawed genetic reductionist models that continue to be circulated by scientists, the media, and the general public. It discusses implications of theory for research methods and for applications aimed at the promotion of health, positive development, and social justice among diverse people across the life span.

Richard M. Lerner is the Bergstrom Chair in Applied Developmental Science and the Director of the Institute for Applied Research in Youth Development, in the Eliot Pearson Department of Child Study and Human Development, at Tufts University, USA.

Concepts and Theories of Human Development

FOURTH EDITION

Richard M. Lerner

Routledge
Taylor & Francis Group
NEW YORK AND LONDON

Fourth edition published 2018
by Routledge
711 Third Avenue, New York, NY 10017

and by Routledge
2 Park Square, Milton Park, Abingdon, Oxon, OX14 4RN

Routledge is an imprint of the Taylor & Francis Group, an informa business

First edition published by Addison-Wesley 1976
Second edition published by Random House 1986
Third edition published by Laurence Erlbaum Associates 2002

Library of Congress Cataloging-in-Publication Data
Names: Lerner, Richard M., author.
Title: Concepts and theories of human development / Richard M. Lerner.
Description: Fourth edition. | New York, NY : Routledge, 2018. |
Includes bibliographical references and index.
Identifiers: LCCN 2017050731| ISBN 9781848728318 (hbk : alk. paper) |
ISBN 9780203581629 (ebk : alk. paper)
Subjects: LCSH: Developmental psychology.
Classification: LCC BF713 .L47 2018 | DDC 155—dc23
LC record available at https://lccn.loc.gov/2017050731

ISBN: 978-1-84872-831-8 (hbk)
ISBN: 978-0-203-58162-9 (ebk)

Typeset in TimesTen
by Keystroke, Neville Lodge, Tettenhall, Wolverhampton

Contents

About the Author xiii
Foreword xv
Preface xvii

1 On the Primacy of Concepts and Theories 1

Philosophy, Theory, and Research 3
  Some Reasons for Doing Research 4
This Book's Approach 7
  RDS-Based Theories and the Concept of Development 7
  Sources of Action in Human Development 8
Philosophy, Theory, and the Work of Developmental Scientists 11
Conclusions 14
The Plan of This Book 15

2 Concepts and Theories within Contemporary Developmental Science: An Overview 17

Contemporary Foci of Developmental Science 17
The Past 19
The Present 21
The Relational Developmental Systems (RDS) Metatheory 23
Three Moments of Analysis in the RDS Approach to Developmental Science 26
Developmental Regulations, Adaptive Developmental Regulations, and Human Agency in RDS Metatheory 28
Evolutionary Biology and Epigenetics 31
Epigenetics: An Overview 33
Epigenetics Invalidates Ideas of Genetic Reductionism 37
Epigenetics within the Context of RDS Metatheory 38
  Conclusions 41
Person-Centered, Idiographic Methods and Systems Science Methodology 42
Econometric Methods 44
Applications of RDS-Based Research: The Promotion of Social Justice 44
Conclusions 45
The Future of Developmental Science 46

3 Philosophical and Scientific Roots of Contemporary Developmental Science 48

The Historical Role of the Nature–Nurture Issue 49

Philosophical Roots 51
  Plato (427–347 B.C.) 51
  Aristotle (384–322 B.C.) 52
  The Medieval Christian Era 54
  René Descartes (1596–1650) 54
  John Locke (1632–1704) 55
Scientific Roots of Development 56
  Charles Darwin (1809–1882) 56
  G. Stanley Hall (1844–1924) 58
  The Contributions of Ernst Haeckel 59
  Hall's Theory of Recapitulation 60
  Behaviorism and Learning Theory 63
  World War II 64
  The 1950s and 1960s 66
  The 1970s, 1980s, and 1990s 69
  Into the Twenty-First Century 82

4 Metatheoretical Models of Development 92

Philosophical Models and Developmental Science 92
The Mechanist Model 95
  Translating the Mechanist Model into a Theory of Development 99
  A Nurture, Mechanist Theory of Development 100
  Problems of the Mechanist Model 106
The Organismic Model 107
  Translating the Organismic Model into a Theory of Development 111
Mechanist and Organismic Models and Issues of Development 113
  Elementarism versus Holism 113
  Antecedent–Consequent versus Structure–Function Relations 114
  Behavioral versus Structural Change 115
  Continuity versus Discontinuity 116
  Stages of Development 117
  Sources of Development 117
  Conclusions 121
The Contextual Model 123
  Contextualism and Probabilistic Epigenesis 124
  The Concept of Development in Probabilistic Epigenesis 125
  Probabilistic Epigenesis as a "Compromise" Conception 128
  Limitations of the Concept of Probabilistic Epigenesis 132
  Issues for Intervention 134
  Conclusions 135
Implications of Philosophical Models of Development for Developmental Science 135
Conclusions 138

5 Theoretical Roots of Contemporary Developmental Science: Nomothetic (Stage), Differential, and Idiographic (Ipsative) Approaches 139

The Stage Theory Approach to Development 140
  The Definition of a Developmental Stage 141
  The Issue of "Abruptness": What Is the Nature of Stage Transition? 145
  The Issue of "Concurrence:" Is There Synchrony in the Development of the Items within a Stage? 151
  Individual Differences within Stage Theories 153
  Relation of Concepts of Development to Stage Theories 153
  Conclusions 155
The Differential Approach 155
  Individual Differences within the Differential Approach 158
  The Study of Development within the Differential Approach 158
  Continuity–Discontinuity 160
  Stability–Instability 160
  Relation of Concepts of Development to the Differential Approach 162
  Conclusions 162
The Idiographic (Ipsative) Approach to Development 162
  Individual Differences within the Ipsative Approach 165

    Developmental Changes within the Ipsative Approach 166
    Relation of Concepts of Development to the Ipsative Approach 167
    Conclusions 168
From Theory to Core Conceptual Issues 169

6 Toward Resolving the Nature–Nurture Controversy: Contributions and Implications of the Scholarship of Anne Anastasi 170

Anne Anastasi (1958): Heredity, Environment, and the Question "How?" 173
Heredity–Environment Relations 177
    Nature Effects Are Indirect 177
    Levels of the Environment 180
    The Continuum of Indirectness 183
    Nurture: The Continuum of Breadth 188
    Conclusions about the Implications of Anastasi's Contributions 191
The Norm of Reaction 192
    Limitations of the Norm-of-Reaction Concept 194
Conclusions 196

7 Toward Resolving the Nature–Nurture Controversy: Contributions and Implications of the Scholarship of T. C. Schneirla 197

Moving Beyond the Concept of Interaction 198
    Past Uses of the Term "Interaction" within Attempts to Resolve the Nature–Nurture Controversy 199
The Contributions of T. C. Schneirla 201
    Structure–Function Relations 202
    Behavioral Stereotypy versus Behavioral Plasticity 204
    Donald Hebb's A/S Ratio 206
    Ontogenetic Implications of Stereotypy–Plasticity and of the A/S Ratio 207
    Intersensory Integration: An Illustration 208
    Conclusions 211
Concepts Representing Development 213
    A Definition of Development 213
    The Role of Maturation↔Experience Relations in Development 214
    Conclusions 216
The Critical-Periods Hypothesis 217
    Weak and Strong Versions of the Hypothesis 219
    Conclusions 221
Instinct: Innate Behavior 221
    Conclusions 223
Circular Functions and Self-Stimulation in Development 225
    A "Third Source" of Development 225
Conclusions 227

8 The Orthogenetic Principle and the Resolution of the Continuity–Discontinuity Issue: Contributions and Implications of the Work of Heinz Werner 229

Defining the Issue 230
    Descriptions of Intraindividual Change 230
    Explanations of Intraindividual Change 231
    Descriptive and Explanatory Combinations 231
    Quantitative versus Qualitative Changes 233
Continuity–Discontinuity as a Theoretical Issue 234
    The Role of Theory 235

- Werner and the Conceptualization of Quantitative and Qualitative Change 239
  - Quantitative Change 240
  - Qualitative Change 241
- The Orthogenetic Principle 242
  - The Orthogenetic Principle and the Continuity–Discontinuity Issue 243
- The Phylogeny of Learning: Continuity or Discontinuity? 245
- Ontogenetic Implications of the Continuity–Discontinuity Issue 249
  - The Presupposition of Limits and the Presupposition of Plasticity 250
  - Plasticity and Probabilistic Epigenesis 253
  - Parameters of Plasticity 254
  - Conclusions about Plasticity in Development 256
- The Stability–Instability Issue 256
  - Relation of Continuity and Discontinuity to Stability and Instability 259
  - Changes Characteristic of Development 261
- Continuity and Discontinuity in Development: Metatheoretical and Theoretical Proscriptions and Prescriptions 262
- Conclusions 264

9 Relational Developmental Systems-Based Theories: Comparative, Evolutionary, and Ontogenetic Conceptions 266

- Gilbert Gottlieb's Theory of Developmental Systems 267
- Returning to Gottlieb's View of Epigenesis 268
  - Modes of Experiential Contribution 271
  - What Maintains the Split in the Study of Human Development? 273
  - Implications of Gottlieb's Ideas for Developmental Methodology and Collaboration 275
- The Multilevel Context of Probabilistic Epigenesis 276
  - Probabilistic Epigenesis and Human Evolution 277
- Gould's Views of Ontogeny and Phylogeny: Evolutionary Bases of Individual↔Context Relations 278
  - Individual↔Context Relations in Evolution: Paleoanthropological Perspectives 280
  - Conclusions 282
- Ford And Lerner (1992) on Design Criteria for Developmental Systems Theories 283
  - Human Development as a Living System 283
  - Design Criteria for RDS-Based Theories 286
- Examples of RDS-Based Models 287
  - The Scholarship of Turiel and Nucci 289
  - The Scholarship of Walter Mischel 291
  - Conclusions 293
- The Sample Case of Developmental Contextualism 293
  - From Developmental Contextualism to Positive Youth Development 297
  - Positive Youth Development 298
  - William Damon and the Study of Purpose 300
  - The Work of Pamela Ebstyne King 301
  - Margaret Beale Spencer and the PVEST Model 302
- The Lerner and Lerner Model of PYD 304
  - Why Focus on the Lerner and Lerner Model? 305
  - The Five Cs Model of PYD 305
  - Tests of the Lerner and Lerner PYD Model 307
  - Extending the Assessment of the Lerner and Lerner Model of PYD to Global Youth 309
  - Future Directions 310
- Conclusions 311

10 Relational Developmental Systems-Based Theories: Sample Cases 312

- Life-Span Developmental Theory 313
  - Concepts Framing Life-Span Developmental Scholarship 314
  - Ontogenetic and Historical Contextualism as Paradigm 316
  - The Baltes, Baltes, and Freund Selective Optimization with Compensation (SOC) Model 317
  - Conclusions Regarding Life-Span Developmental Theory 321
- Action Theories of Human Development 321
  - Regulation and Plasticity in Human Development 322
  - The Contributions of Jochen Brandtstädter 323
  - Contextual and Developmental Constraints on Action 324
  - Conclusions Regarding the Ideas of Brandtstädter 325
  - Jutta Heckhausen's Life-Span Theory of Control 326
  - Conclusions Regarding the Ideas of Heckhausen 328
- Glen H. Elder, Jr. and Life-Course Theory 328
  - Constructing the Life Course 332
  - Conclusions about the Ideas of Elder 334
- Urie Bronfenbrenner's Bioecological Model of Human Development 335
  - Conclusions about Bronfenbrenner's Bioecological Model 338
- Thelen and Smith's Dynamic Systems Theory 339
  - The Development of Novel Forms across Life 340
  - The Dynamics of the Developmental System 342
  - Stability and Change in Dynamic Systems 343
  - Transitions in Systems 344
  - Time Scales within Dynamic Systems 345
  - Conclusions about the Ideas of Thelen and Smith 345
- Magnusson's Holistic Person↔Context Interaction Theory 346
  - Causality in Holistic Relations 347
  - Features of the Person↔Environment System 348
  - Conclusions about the Ideas of Magnusson 351
- Wapner's Holistic, Developmental, Systems-Oriented Perspective 351
  - Person-in-Environment Functioning within the Holistic, Developmental System 354
  - Conclusions about the Ideas of Wapner 355
- Kurt W. Fischer's Dynamic Coactive Systems Theory 355
  - Dynamic Systems Theory as an Alternative to Neo-Nativism 356
  - The Concept of Dynamic Skill 360
  - Conclusions about the Ideas of Fischer and His Colleagues 363
- Rogoff's Sociohistoric-Cultural Theory 363
  - Conclusions about the Ideas of Rogoff 367
- Levels of Integration and the Explanation of Cognitive Development 368
- Conclusions: Using RDS-Based Ideas as a Frame for Discussing Genetic Reductionist Models 369

11 Genetic Reductionism in Developmental Science: Sample Cases from the Twentieth Century 372

- The Conceptual and Empirical Failures of Genetic Reductionism 373
- Some Twentieth-Century Examples of Genetic Reductionism: 1. Konrad Lorenz 375
  - Lorenz's Conception of "Instinct" 376
  - Criticisms of Lorenz's Conception of Instinct 377
  - Lorenz's Responses to His Critics 378

Lorenz's Application to Humans of His Concept of Instinct 378
The Nazi-Era Work of Lorenz 380
Conclusions 386
The Science and Politics of Lorenz's Work: Evaluating the Evidence 387
Lorenz's Work after World War II 391
The Example of Human Aggression 393
Is There a Militant Enthusiasm Instinct? 395
Claims of Selection and Ethical Degeneration in Modern Civilization 396
Lorenz and Sociobiology 399
Some Twentieth-Century Examples of Genetic Reductionism: 2. The Work of Sir Cyril Burt and Arthur Jensen 400
The Genetic-Differences Hypothesis 401
Estimating the Heritability of Intelligence 403
The Work of Sir Cyril Burt 405
Reactions to Kamin's (1974) Analysis and Subsequent Developments 409
Other Data in Purported Support of the Hereditarian Position 412
Correlations between IQ and Amplitude and Latency of Evoked Potentials 412
Reviews of Familial Studies of Intelligence after the Burt "Scandal" 414
Findings from Molecular Genetics Research 418
Other Problems with Twin Research 420
Continuing Misuses of the Heritability Concept in the Discussion of Race Differences in Intelligence 421
Conclusions 424
The Calculation of the Heritability of Intelligence: Models and Mistakes 424
Problems of Generalizing Heritability Estimates 425
Problems with Modeling Heritability 426
Conclusions 430
Conclusions about the Study of Heritability and Intelligence 433
The Continuation of Genetic Reduction 434

12 Genetic Reductionism in Developmental Science: Sample Cases from the Twentieth and Twenty-First Centuries 436

The Field of Behavior Genetics 436
Fatal Flaws in the Behavior Genetics Model of Gene Function 438
Behavior Genetics as the Emperor's New Clothes 440
Failures to Replicate 441
The Continuing Story of Shortcomings of Heritability Analysis 442
Calculating Heritability 443
Statistical Problems Associated with Heritability Analysis: Genotype–Environment Correlation 446
Heritability Does *Not* Mean Inherited 447
Criticisms of Behavior Genetics Have Increased, Not Diminished 449
Rewriting History 450
Conclusions 451
Sociobiology 452
The Scientific Goals of Sociobiology 452
Genetic Determinism as Sociobiology's Key to Interdisciplinary Integration 453
Sociobiology and Human Aggression 454
Sex Differences in Gametic Potential 455
Gametic Potential and Social and Sexual Development 456
Sociobiology, Genetic Determinism, and Human Development 458
Evaluating Sociobiological Claims 458
Comparisons of Humans and Nonhumans: The Concept of Homology 460
Sociobiology and Heritability Analyses 461

    - Are Adaptations Everywhere? 462
    - The Concept of "Exaptation" 464
    - The Role of the Organism in Its Own Evolution 466
    - The Relational Developmental System and the Role of the Concept of "Behavioral Neophenotypes" in Evolutionary Change 467
    - Conclusions about the Presence of Evidence in Support of the Sociobiological View of Human Development 468
- The Work of J. Philippe Rushton 469
    - Rushton's Tripartite Theory of Race, Evolution, and Behavior 470
    - Rushton's Ideas about Different Reproductive Strategies across Race Groups 471
    - Evaluations of Rushton's Evidence 472
    - Conclusions about the Quality of Rushton's Hereditarian Views of Race Differences 473
- Conclusions: Why Isn't Nativism "Dead?" 473
- Evolutionary Developmental Psychology 474
    - Two Approaches to Developmental Complexity 479
    - Conclusions about EDP 482
- Conclusions about Genetic Reductionist Theories of Human Development 483

13 Some Implications of Relational Developmental Systems-Based Theories for Research Methods and for the Application of Developmental Science 486

- Using RDS-Based Ideas as a Frame for Developmental Methods 487
    - Problematics in the Study of Change within RDS-Based Models 488
    - Variable versus Person-Centered Foci 489
    - Designs of Developmental Research: Problematics of Time and Timing in the Longitudinal Study of Human Development 491
    - Three Bases of Methodological Decisions in Longitudinal Research 493
    - Dividing the x-Axis: The Temporal Spacing of Observations 494
    - Selection of Appropriate Ontogenetic Points to Index a Change Process 496
    - The Nonequivalent Temporal Metric: Relations among Levels within the Relational Developmental System 497
- Some Data Analysis Issues in Developmental Research 499
    - Problems with Aggregating Observational Points 501
    - The Importance of Qualitative Methods and of Mixed-Methods Research 504
    - Quantitative Innovations 505
    - The Decline of the RCT "Gold Standard" 506
    - Qualitative Innovations 507
    - Conclusions: From Research Methods to Application 510
- Applied Developmental Science 511
    - The Emergence and Definition of Applied Developmental Science 514
    - RDS-Based Approaches to the Design of Program Evaluation: The Sample Case of Evolutionary Evaluation 517
    - Applied Developmental Science, the Future of Civil Society, and the Promotion of Social Justice 518
- Conclusions: Towards the Future of Developmental Science 520

References *521*
Photo Credits *586*
Name Index *587*
Subject Index *596*

About the Author

Richard M. Lerner is the Bergstrom Chair in Applied Developmental Science and the Director of the Institute for Applied Research in Youth Development at Tufts University. He went from kindergarten through Ph.D. within the New York City public schools, completing his doctorate in developmental psychology at the City University of New York in 1971. He was the founding editor of the *Journal of Research on Adolescence* and of *Applied Developmental Science*. Among the more than 80 books he has authored or edited, Lerner was the Editor-in-Chief of the 7th edition of the *Handbook of Child Psychology and Developmental Science* and of the *Handbook of Life-Span Development*.

He has been a fellow at the Center for Advanced Study in the Behavioral Sciences and is a fellow of the American Association for the Advancement of Science, the American Psychological Association, and the Association for Psychological Science. He is the 2013 recipient of the American Psychological Association (Division 7) Urie Bronfenbrenner Award for Lifetime Contribution to Developmental Psychology in the Service of Science and Society. He is also the 2014 recipient of the American Psychological Association Gold Medal for Life Achievement in the Application of Psychology, the 2015 recipient of the American Psychological Association (Division 1) Ernest R. Hilgard Lifetime Achievement Award for distinguished career contributions to general psychology, the 2016 recipient of the International Society for the Study of Behavioral Development Distinguished Scientific Award for the Applications of Behavioral Development, and the 2017 recipient of the Society for Research in Child Development Distinguished Contributions to Public Policy and Practice in Child Development Award. Lerner has been a member of the Board of Scientific Counselors of the National Institute of Child Health and Human Development and of the Developmental Science Advisory Board of the National Science Foundation, and he serves on the Board of Directors of the Military Child Education Coalition. In 2017, he was appointed by Pope Francis to a five-year term as a Corresponding Member of the Pontifical Academy for Life.

He is married to Dr. Jacqueline V. Lerner, Professor in the Department of Applied Developmental and Educational Psychology in the Lynch School of Education at Boston College. They live in Wayland, Massachusetts. Their children are Justin, a director and screenwriter, and his fiancée Sarah, who is a novelist; Blair, an advertising executive, and her husband, Jamie, a contractor; and Jarrett, a novelist and editor, and his wife, Danni, a postpartum doula. Rich and Jackie have four grandchildren, Harper Rose Ramsey, who is 5 years old, Dylan Maxwell Ramsey, who is 3 years old, Bodie Anthony Ramsey, who is 1 year old, and Isla Terese Lerner, who is 6 months old.

Richard M. Lerner

Foreword

When the first edition of Richard M. Lerner's *Concepts and Theories of Human Development* was published more than four decades ago, it provided a welcome beacon of guidance and clarification to developmental psychologists. During most of the preceding decades, a growing cadre of developmentalists had been divided between the yin and yang of nature and nurture, drawn by the competing visions of Arnold Gesell and the maturationists, on the one hand, and the confident assertions of the learning theorists who had assumed the mantle of John B. Watson and B. F. Skinner, on the other. The falseness of this dichotomy had been demonstrated late in the 1950s by Anastasi, but her conclusions were cited rather than understood by most researchers active in the middle of the last century. Even the belated discovery of Piaget by English-speaking scholars in the early 1960s did not move the theoretical needle much. Although increasingly characterized by empirical research, the conceptual frameworks loosely embraced by developmental psychologists were sufficiently vague to accommodate any findings.

When it first appeared, therefore, *Concepts and Theories of Human Development* not only laid out the assumptions and implications that characterized each of the extant approaches to understanding development, but also elucidated their origins in other domains of psychology (especially comparative and personality psychology). Furthermore, the book emphatically documented how and why clear conceptual frameworks were essential for the field to realize its potential.

As Lerner showed so powerfully and lucidly, theories are essential to the scientific process, and if developmental psychology wanted (as it did) to be taken seriously as a science, then it needed to develop and employ theories scientifically. Importantly, this achievement involved developing theories that were consistent with the understanding of human physiology and behavior developed by scientists in sister disciplines and sub-disciplines, it demanded that the theories themselves be internally consistent, and it required that they be sufficiently precise and robust that they offered testable predictions for researchers to address.

Then, as now, Lerner's *Concepts and Theories of Human Development* explains what developmental theorists needed to achieve and details the characteristics, assumptions, and implications of the different theoretical frameworks in use, drawing attention to internal contradictions and to incompatibilities between different approaches and limitations in their individual reach and generality.

Both of us were at the beginning of our academic careers when *Concepts and Theories of Human Development* was first published. We had both been mentored by one of the most cerebral and intellectually demanding scholars of the era so we were familiar with fearsome demands to justify and explain the concepts and theories relevant to our research. Lerner's new book provided an eloquent and satisfying guide as we launched our own programs of research.

Over the ensuing decades, developmental science (as it is now called) has burgeoned as a discipline. The numbers of students and researchers who study human development have increased massively, and their efforts are reported and synthesized in numerous journals and at huge conferences. For those who 'came of age' academically in the last quarter of the twentieth century, successive editions of *Concepts* *and Theories of Human Development* have served as a singular and unique resource even as pressures to publish and the narrowing of individual intellectual horizons have pushed many scholars to pursue their research. Indeed, one could argue that clear conceptual and theoretical frameworks have never been more important than at a time when we must make sense of a deluge of data.

In this fourth incarnation, *Concepts and Theories of Human Development* provides a magisterial overview of the field, explaining and outlining the theories and approaches that have nourished the field and are relevant today. Many are descendants of theories articulated and evaluated in the first edition, now modified to accommodate the results of scholarship in the intervening years. Those that are new reflect the integration of diverse strands of theorizing and research. In contemporary developmental science, the dynamic, relational, integrative, organizational life-span view of development has come to predominate, and Richard Lerner, who is a key figure in the development of that approach, beautifully describes why this more sophisticated and nuanced approach is indispensable if we are to fully understand the vicissitudes and complex contours of human development across the life span.

Concepts and Theories of Human Development was a critical reference for us and our peers when it first appeared. This latest edition promises to play an equivalently important role for a new generation of scholars now entering or coming to prominence in the field. The book is essential reading for any advanced level course on developmental science, it should be *primus inter pares* on the virtual or solid bookcases of graduate students in developmental science, and it will be given pride of place in the offices of developmental scientists around the world, just as the previous editions were for us. This new edition builds on and enhances the extraordinary eloquence and excellence of its predecessors and makes a much needed and very welcome contribution to the field. No developmental scientist should be without it.

Michael E. Lamb
University of Cambridge
Marc H. Bornstein
National Institute of Child Health
and Human Development

Preface

The editions of *Concepts and Theories of Human Development* have framed my career. I began to write the first edition of the book in the early 1970s, while I was an assistant professor at Eastern Michigan University in Ypsilanti, Michigan. Stuart Johnson, then the traveling book salesman and publisher's representative for Addison-Wesley, signed the book, thinking it would a competitor for one of the then top-selling texts in child development, *Child Development and Personality* written by Paul Mussen, John Conger, and Jerry Kagan. I explained to Stu that the book was not aimed at an introductory audience but, it was not until reviewers of the manuscript gave Stu this same information, that he believed it. Nevertheless, he was shocked. However, thankfully, he recovered his bearings and helped convince the company to publish the book anyway. A review of the manuscript by Shep White convinced Stu and the folks from Addison-Wesley that there might be some merit in a book that focused on concepts and theories, and not on a smorgasbord of summaries of the most recent empirical reports published in *Child Development* or *Developmental Psychology*. However, the book's production was demoted from "A List" to some other level. For example, the production budget was so low that only one photo appeared in the first edition.

I had wanted to include photos of the various theorists I discussed in the book but, given the budget, I was told that photo fees would be too expensive. The "compromise" we reached was that an undergraduate student of mine, Cathy Gendron, found photos of these scholars and then drew portraits of them. Addison-Wesley paid her $5.00 per drawing and, as well, a bit more for the art she drew for the cover of the book. Based on the reactions of the people whose likenesses appeared in the book and on evaluations from readers, the drawings may have been the best part of the first edition. In any event, the book was eventually published in 1976.

Subsequent to the publication of the book, Addison-Wesley decided to get out of the business of publishing in developmental psychology. I did not think there was any causal connection between the company's publication of my book and their decision to cease publishing in my area of scholarship. However, Addison-Wesley then sold the rights for the book to Random House, who then published the second edition of the book, in 1986. After its publication, Random House promptly decided to also cease publishing in developmental psychology. I now began to worry a bit more about causality.

After Random House ended publication in my area of scholarship, I learned that I was free to take the book to another publisher. I spoke with my good friend, Larry Erlbaum, the owner of LEA Publications. I had by that point in my career published about a half-dozen books with LEA, and Larry picked up the contract for the book and authorized me to write a third edition. This edition was published under the editorial supervision of his then editor, Bill Weber. The book appeared in 2002. However, soon thereafter, Larry retired from the publication business, and he sold his list of books to Taylor & Francis. This excellent company is the publisher of the fourth edition of the book, which will have a 2018 copyright date. I wish them well in their future publication efforts in developmental science, however long this future might be.

Although I have not collected comparative information about other, multi-edition books, I believe that there are probably very few ones that, over the course of more than 40 years, have undergone four editions with four different publishers and, especially, with the publishers of the first three editions getting out of the business after publishing the book. Although I might begin to believe the fallacy of *post hoc ergo propter hoc* after this series of "publication à going out of business" occurrences, I cannot regard my history with *Concepts and Theories* as personally unfortunate. I have been blessed by having extraordinary colleagues and students who, across the four-plus decades of my working on *Concepts and Theories*, have helped me grow intellectually and, through their mentorship, criticism, and tutelage, enabled me to try to improve the quality of the book.

So, then, what is this fourth edition of *Concepts and Theories* about? What does this edition try to accomplish and what might colleagues and students gain from reading it? First, it has become clear to me that prior editions of the book were rarely used in undergraduate courses. Graduate students were the primary readers of the book and, although publishers of college textbooks are challenged financially by the used book marketplace, the graduate-student readers of the book did not sell it back to the bookstore at rates comparable to other textbooks. Students kept the book after the ends of the courses wherein the book was used. I was given some reports that they used the book as a reference in studying for comprehensive exams or, in a few reports I received, for organizing their lectures when they transitioned from graduate student to professor. This situation did not create an overall great number of books sold (after all, there are not that many graduate students in developmental science, especially when compared to the numbers of students taking undergraduate introductory courses). However, new copy sales tended to remain fairly steady across the decade following the appearance of an edition.

As a consequence of this history with the first three editions, the fourth edition has deleted almost all of the introductory-level information about theories that was present in the prior editions. For example, if graduate students are the primary readers of the book, I saw no need to describe the stages of Piaget, Freud, Erikson, etc. In the main, I assume the reader possesses this basic information—descriptive information that is in fact ably presented in most introductory books—and I spend my time in the fourth edition discussing instead the assumptions involved in stage theories and the conceptual issues that divide developmental scientists regarding the concept of "developmental stages." In short, although I do find reason to include in the book a few tables that list the stages in various theories, I assume the readers have read about and mastered descriptive information about these stages, or about other introductory-level material (e.g., the defining features of classical and operant conditioning).

However, despite my reliance on the retention among graduate students of information about developmental science encountered during their undergraduate education, I am less sanguine about their knowledge of when the foundations of their field of study were established and who established it. Seymour Sarason, the renowned community psychologist, once commented to me that his Yale University graduate students had the assumption that the field began about five years prior to their entrance into graduate school. He reported that, when he referred them to publications dating back 10 or more years, they questioned the relevance of such information to contemporary science. In addition, in my role as editor of the journal *Applied Developmental Science*, I invited Michael Lewis (2009) to publish an analysis he did of citations to the work of acknowledged, eminent scientists in the years just prior to and after their deaths. Michael found that by about five years after death, the number of published references to, for instance, both Clark Hull and Jean Piaget had dropped precipitously.

I think that good scholars should know the history of their fields and, for this reason alone, I could rationalize the approach I have taken in the fourth edition: I discuss both foundational contributions to developmental science (I have chapters that focus on the contributions of Anne Anastasi, T. C. Schneirla, and Heinz Werner) and, as well, I discuss the influence of these contributions to contemporary (at this writing) developmental science. For instance, in Chapter 8, the discussion of the contributions of Werner's work involves citations that range from the 1940s through 2016, with the publication of an important book by Catherine Raeff (2016) that frames the field of developmental science through the use of Werner's ideas about developmental systems and orthogenesis.

In addition, in my discussion of the relational developmental systems (RDS) metatheory, and therefore of the contributions of Bill Overton (e.g., in 2015), discussions that provide the perspective that frames the approach to developmental science I use throughout the fourth edition, I discuss the roots of RDS metatheory from the 1930s (and the work of von Bertalanffy) through Bill's writing in the 1970s through 1990s and then across the years to date in the twenty-first century. Similarly, in my discussion of genetic reductionist models, I draw on work involving Ernst Haeckel and the Monist League that began in the late 19th century. This work continued in the early twentieth century, and involved Konrad Lorenz, beginning with his writings about the concept of instinct, in the 1930s, and extending through the 1960s, with his writings about innate militant enthusiasm. Genetic reductionist models continue to be promulgated in contemporary (at this writing) developmental science, as exemplified in the work of behavior geneticists (e.g., Robert Plomin et al., 2016) and evolutionary developmental psychologists (e.g., David Bjorklund, 2015, 2016, and he and his colleague, Bruce Ellis, e.g., Bjorklund & Ellis, 2005).

Genetic reductionist models pertain to another feature of the fourth edition. I focus a great deal on evaluating these instances of split, essentialist formulations of human development. Although I present and evaluate several instances of nurture-based versions of these approaches (e.g., through discussing Skinner and Bijou and Baer), my main emphasis is on critiquing nature-based versions. I base this focus on the continuing prominence of these egregiously flawed ideas in both the literature of developmental science and in the minds of media leaders and the general public. Although I discuss several instances of genetic reductionist models, I spend most of my time focusing on behavior genetics, sociobiology, and evolutionary developmental psychology. Other examples of this approach to theory (e.g., Five Factor Theory and cognitive neo-nativism) are also discussed but in less detail.

A final distinctive feature of the fourth edition is something that (I hope) might be appropriately labeled "useful redundancy." In prior editions, my approach to some key ideas that I used throughout the book (e.g., probabilistic epigenesis, orthogenesis, or plasticity) was to try to discuss a topic in detail in one part of the book and to then refer the reader to a prior discussion when, later in the book, the concept was again relevant. Although I still use cross-referencing to prior discussions throughout the fourth edition, I recognize now that readers may not read the book in sequence from Chapter 1 through Chapter 13, that is, that some users of the book may either reorder the sequence of chapters to meet their specific purposes or that only some chapters of the book may be used. Therefore, because the concepts of probabilistic epigenesis, orthogenesis, plasticity and, in the fourth edition, of epigenetics, the specificity principle, and idiographic or non-ergodic change (and even the problematic concepts of instinct and heritability) are among the ideas that are relevant to several topics I discuss, I reiterate some of the key points associated with these ideas in several places throughout the book and, of course, refer the reader to fuller discussions of the concepts that may be found in other chapters.

As emphasized in the prefaces I wrote for the prior editions of *Concepts and Theories of Human Development*, there are scores of people I must thank for their support and help in the years between the third edition and the present one. Sixteen years have elapsed between the third and fourth editions of the book and when, in about 2011, I began to think about revising the third edition, I thought the task was too daunting to accomplish. In the decade between the completion of my writing of the third edition and contemplating the fourth edition (2001–2011), I had written more than a score of articles and chapters pertinent to the focus of the book, Bill Damon and I had co-edited the sixth edition of the *Handbook of Child Psychology*, and I had edited or written several other relevant books. In addition, the literature had expanded due to dozens of new pertinent contributions by scholars from several disciplines. I discussed this situation with my colleague, Amy Alberts Warren, and she was not at all intimidated by all this new information. Over the next year or so she helped me integrate all the writing I had done between 2001 and 2011 with my past writing and, as well, suggested several creative ways I could bring other, new literatures into the frame of the book. Simply, without her help the fourth edition would have never happened and I am deeply grateful for her brilliance, generativity, generosity, and friendship. In that I expect that this edition of *Concepts and Theories* will be the last that I author (given the inter-edition interval for the book!), I am hoping that, if a new edition of the book is available someday, Amy will take the lead in writing it!

Gary Greenberg, Professor Emeritus at Wichita State University, is one of the most accomplished and renowned comparative psychologists in the history of this field. He has been a truly singular source of knowledge, mentorship, and friendship. Since his retirement, he seems to spend his days finding books, chapters, and articles for me to read that he believes will enhance my work. He is invariantly correct. His advice and wisdom have enhanced immeasurably this edition of the book.

Over the years that I worked on this edition of the book, the scholarship of numerous colleagues greatly influenced my thinking. The scholarship, and photos, of many of these colleagues are represented through this edition of the book. A count of the citations I have made to the works of these scholars will serve as one indication of the magnitude of the debt I owe to them. I want to mention several of them here (and do so alphabetically): Jason Baehr, Patrick Bateson, Peter Benson, Janette Benson, Marvin Berkowitz, Mark Blumberg, Rich Bollinger, Marc Bornstein, Jeanne Brooks-Gunn, Marlis Buchmann, Pam Cantor, Anne Colby, Bill Damon, Ann Easterbrooks, Jacque Eccles, Glen Elder, David Henry Feldman, Kurt Fischer, Celia Fischer, Alexandra Freund, Al Gore, Gilbert Gottlieb, Jutta Heckhausen, Jim Heckman, Jerry Hirsch, Mae-wan Ho, Fran Jacobs, Eranda Jayawickreme, Jay Joseph, Evelyn Fox Keller, Pamela Ebstyne King, Deanna Kuhn, Michael Lamb, Jackie Lerner, Tama Leventhal, Michael Lewis, Richard Lewontin, Lynn Liben, Bob Lickliter, Iris Litt, Todd Little, Michael Mascolo, Mike Mathews, George Mischel, Walter Mischel, Jayanthi Mistry, Peter Molenaar, David Moore, Ulrich Müller, John Nesselroade, Larry Nucci, David Osher, Bill Overton, Anne Petersen, Erin Phelps, Catherine Raeff, Ken Richardson, Barbara Rogoff, Todd Rose, Diane Ryan, Kimon Sargeant, Peter Saunders, Seth Schwartz, Scott Seider, Alistair Sim, Dan Smith, Graham Spanier, Margaret Beale Spencer, Larry Steinberg, Bob Sternberg, Ethel Tobach, Patrick Tolan, Elliot Turiel, Deborah Vandell, Alex von Eye, Doug Wahlsten, Don Wertlieb, and David Witherington. My intellectual debt to these colleagues and, as well, to many, many others, is enormous.

Indeed, over the course of the years between the completion of the third edition of this book and the completion of the present edition, I had the privilege of editing several major handbooks in developmental science and, as well, several other edited volumes that I regard as important in advancing the field. Across these volumes, major contributions to developmental science were made by literally hundreds of superb scholars whose generosity and commitment to the field enabled these works to be completed and to be compelling. Listing these colleagues would extend this Preface beyond reasonable bounds. However, I want to thank all of these scholars for so greatly enriching the field and for providing me with wonderful continuing education experiences.

Another group of colleagues also contributed enormously to my intellectual growth across the years I worked on this edition of *Concepts and Theories*: my students and colleagues who have collaborated with me in the Institute for Applied Research in Youth Development. I am grateful to: Jen Agans, Jason Almerigi, Pam Anderson, Mimi Arbeit, Aida Balsano, Rumeli Banik, Milena Batanova, Deb Bobek, Ed Bowers, Michelle Boyd, Dylan Braun, Aerika Brittian, Mary Buckingham, Brian Burkhard, Kristina Callina, Robey Champine, Paul Chase, Sarah Clement, Lisette DeSouza, Elizabeth Dowling, Andrea Ettekal, Kaitlyn Ferris, Ettya Fremont, Patricia Gansert, G. John Geldhof, Steinunn Gestsdóttir, Larry Gianinno, Katie Greenman, Elise Harris, Rachel Hershberg, Lacey Hilliard, Helena Jelicic, Sara Johnson, Nicole Zarrett Kivita, Lily Konowitz, Sonia Koshy, Ken Lee, Alicia Lynch, Lang Ma, Taryn Morrissey, Megan Mueller, Elise Murray, Chris Napolitano, Sophie Naudeau, Nancy Pare, Kristen Fay Posten, Kathy Robinson, Rachel Rubin, Danielle Stacey, Christina Theokas, Jonathan Tirrell, Jen Urban, Jun Wang, Dan Warren, Michelle Weiner, and Jon Zaff.

Several colleagues provided invaluable critiques of drafts of the chapters of the book. Their comments were important and helped me improve the book enormously. I am deeply grateful for the scholarly excellence and insights of: Jennifer Agans, Gary Greenberg, Anthony Steven Dick, Ulrich Müller, Lisette DeSouza, Catherine Raeff, Kristina Schmid Callina, Evan Charney, Jay Joseph, and Sara Johnson.

As is evident in my discussions throughout the chapters of the present edition, Marc Bornstein and Michael Lamb are two of the most significant developmental scientists across the past half-century. Their contributions are of historical importance to the field. I am therefore honored and deeply appreciative of their willingness to write a foreword to this edition of the book. Their comments are perhaps far too generous but I am nevertheless sincerely grateful for their words and, even more, for their collegiality and friendship across, now, more than four decades.

My assistant, Lori Campbell, is a creative and indefatigable colleague. She provided invaluable support and demonstrated often spectacular resourcefulness in accomplishing the myriad organization tasks involved in moving from notes, to drafts, to submitted manuscript. I am deeply grateful for her extraordinary efforts and her constant support of my work.

Jarrett Lerner is the Managing Editor in the Institute. He is also my son. As his colleague and as his parent I could not be prouder of the contributions he made to every facet of the writing and completion of this book. Both as an astute and skilled editor, a superb writer (he is a published novelist), and a wise counselor, he helped me clarify my ideas, advised me on the paths I should take in developing the organization of the chapters, kept track of the hundreds of references I was using, corresponded with the editorial staff at Taylor & Francis, helped me get in touch with colleagues who I needed to consult with about facets of the book, and kept my spirits up during the cycles of optimism and pessimism I encountered as I worked to complete the book on the schedule to which I promised the publisher I would adhere. He never failed to buoy my spirits and his ideas and feedback were always superb. I always followed his advice and the book is immensely better because I did so. In fact, I see his intellectual stamp and the excellence of his editorial stewardship on each page of the book. I am profoundly grateful to him.

My editor at Taylor & Francis, Georgette Enriquez, and her assistant, Brian Eschrich, are exceptionally skilled professionals and generous and supportive colleagues. Without Georgette's kind understanding of the several, often competing professional tasks I had to complete as I tried to write the present edition of this book, and her warm but quite effective persistence in checking in with me across several years of less than highly productive progress, I doubt if I would, today, be writing the Preface. In turn, when I did complete a draft suitable for review and then, when I completed my revisions and the book went into production, Brian and Georgette were wonderfully efficient and able colleagues. Together, Georgette and Brian are a great team, and I am fortunate to have had the chance to work with them. I was also fortunate to have Sarah Pearsall serve as the copy editor for this edition. Her judgment, precision, insight, and dedication to the book greatly improved its quality. I am very grateful to her.

Many of the ideas I present in this book were sharpened by their use in the research I conducted at the Institute across the last 15 years. I am deeply appreciative of the investments made in this research by the John Templeton Foundation, The Templeton Religion Trust, the Poses Family Foundation, the Bergstrom Family Foundation, National 4-H Council, and the Altria Corporation. The generous support provided by these organizations—and by the grant officers within them—has enabled my work to flourish. I thank them for the confidence they placed in my work and me, and I hope they are proud of the scholarship my students, colleagues, and I were able to produce because of their deeply appreciated support.

In the past three editions of *Concepts and Theories of Human Development*, I dedicated the book to three men named Sam: Sam Goldfarb, my maternal grandfather, Sam Korn, my dissertation advisor, and Sam Karson, my first department chair. Each of these men were models and mentors to me. Their support made me the person I am today. Once again, I dedicate this edition of the book to them but, in addition, I add a fourth person to this group. She is not named Sam, however. I also dedicate this book to my wife, Jacqueline Rose Verdirame Lerner. I began this Preface by noting that the editions of *Concepts and Theories* have framed my career. They have also framed by life with Jackie. She helped me review the page proofs of the first edition while we were still dating and now, after 40 years of marriage, she has helped me "write" the ensuing years of my life. There is nothing that I accomplished in my career or my life that is not due to what she has given to our family and to me. This dedication is only a small token of what I owe to her.

RML
Medford, MA
September, 2017

CHAPTER ONE

On the Primacy of Concepts and Theories

> *A collection of data no more makes a science than does a heap of bricks make a house!*
> Ludwig von Bertalanffy, 1933, biologist and inventor of General Systems Theory

What builds the bricks of a science into a house? I believe it is the conceptual and theoretical issues of a science. Certainly, the listings in a telephone directory are facts. But a knowledge of the names in the phone book would certainly be, to quote singer–songwriter and Nobel Laureate Bob Dylan (1964), "useless and pointless knowledge." However, if one could relate such data to some conceptual framework, then perhaps some meaning could be provided.

I believe that science advances best when research is derived from good questions and, in turn, when good questions are theory-predicated. Answers to theoretically predicated questions provide data that are useful for understanding how facets of the empirical world may be best assembled to construct an understanding of the phenomena in which a scientist is interested. Theory-predicated research in developmental science has the best potential for enabling researchers to understand the process of development, that is, to better describe, explain, and optimize the bases and characteristics of development across the life span.

Suppose that a developmental scientist had a theory-predicated hypothesis that ethnic identity develops most strongly when individuals of specific ethnic and cultural backgrounds grow up in neighborhoods wherein there is close proximity to other people with the same specific backgrounds (e.g., Leventhal, Dupéré, & Shuey, 2015). How might a researcher test this idea? He or she might spread out a large street map of the city and cut out each name and address in the phone book and place it on the appropriate place on the map. After a while, a pattern may begin to emerge. The researcher might find some areas of the city (neighborhoods) where people with Italian names seemed to cluster, areas where people with Irish names clustered, areas where people with Latino names clustered, areas where people with Chinese names clustered, areas where people with Jewish names clustered, etc. In turn, the researcher might find neighborhoods that have a mix of names associated with these ethnicities and, as well, with names associated with other ethnicities, for instance, African, Indian, Muslim, or Korean. What the researcher might then do is sample children in each of the identified neighborhoods and study the development of their ethnic identities. If children living in the ethnically more homogeneous neighborhoods develop stronger ethnic identities than children in the more ethnically heterogeneous neighborhoods, then the data would support the hypothesis.

This finding would not only support the hypothesis, however. It would also illustrate the point that a study derived from a theory-predicated question provides a way of organizing a seemingly meaningless or obscure body of data—the names in a phone book—into a meaningful and, perhaps, important body of factual knowledge.

A major function of theory is to integrate existing facts; to organize them in such a way as to give them meaning. A second function of theory is to provide a framework for the generation of new information. *A theory may be defined as a system of statements that integrates existing information and leads to the generation of new information*.

Developmental scientists studying the human life span may often have numerous facts available to them (e.g., facts relating to children's thinking at various ages). The results of empirical studies might indicate that young children tend to use relatively general, global, and concrete categories to organize their thinking, but older children use more differentiated, specific, and abstract categories (e.g., see Raeff, 2011, 2016). For instance, younger children might label all furry, four-legged creatures as "doggies," whereas older children might have different labels (e.g., "dogs," "cats," and "horses") and a shared, superordinate label ("animals"). Older children might also recognize that all these creatures share the common but abstract quality of "life." Whereas such facts are interesting in and of themselves, their meaning is not obvious; certainly, the implications of such facts for more general psychological development and functioning may not be clear.

Thus, when scientists such as Heinz Werner (1948, 1957) or Jean Piaget (1950, 1970) offer a theory of the development of thought that allows such facts to be integrated and understood, and, moreover, specifies the empirically testable implications of such theoretical integrations for other areas of human development, the importance is obvious (Chapter 8 discusses Werner's work). Such theories are useful to developmental science because they integrate existing factual knowledge and lead to the generation of new information that advances understanding.

The point is that, although facts are important, they alone do not make a science. The development of science, I would argue, relies more fundamentally on the advancement of theory (Overton, 2015a; Raeff, 2016). As surveys of the history of developmental science bear witness (e.g., Cairns & Cairns, 2006; Looft, 1972), the scientific study of human development has itself evolved through an increasing emphasis on theory and conceptual integration. In Chapter 3, I review this history. As I move through a historical account, from the prescientific, philosophical discussions of development to (at this writing) current theoretical discussions, I explain that a few issues continue to be central, for example, issues about the roles of nature and nurture in human development or about whether constancy or change characterizes specific portions of the life span.

Yet, although the scientific status of theory per se and the need for, and the roles of, theory remain essentially invariant across this history of developmental science, research cannot be ignored. If there were no research, theories would be empty exercises. If there were no way to test a given theoretical integration, the formulation would be scientifically useless. There would be no empirical observation capable of falsifying, verifying, or moderating the theoretical statements. Empirical observation is the primary defining feature of science, and if theoretical ideas cannot ever be empirically tested they are essentially scientifically useless. They become speculations or unfounded opinions. Although I discuss the role of research in developmental science at several points throughout the book, it is appropriate to indicate here some of the important interrelations that exist between research and theory.

Research is often done to try to answer the questions raised by science. Such issue-based research results in data, as does all research. A theory may exist or be devised to integrate the facts of a science—the first role of a theory—and to lead to the generation of new facts—the second role of a theory. Someone, however, may think that these same facts can be integrated in another way—that is, with another theory. Theoretical arguments come about from such differences. Yet, because each different theory attaches different meanings to the same facts, research is done in order to clarify the differing theoretical interpretations. Even if such theoretical differences did not exist, research would be done to see whether ideas (i.e., questions, hypotheses) derived from the theory could be shown to be empirically supported. Simply, then, research is needed to show the integrative usefulness of a theory or its usefulness in leading to new facts.

Thus, in the abstract, theory and research are inextricably bound; nevertheless, some concrete interrelational problems exist. Because of the complexity and abstractness of many of the controversies of a science, the interrelation of research and theoretical issues is not always evident or unequivocal. A complaint of some researchers is that there seems to be a widening gap between theory and research (e.g., see Overton, 2015b). There is certainly some truth to this statement. However, I suggest that, if one looks at the relation between research and theory at a more basic level, an interrelation may be seen (Overton, 2015a).

PHILOSOPHY, THEORY, AND RESEARCH

Everything a scientist does involves at least three points:

1. Assumptions about the nature of the subject matter.
2. Preferences for the topic of study within the subject matter.
3. Preferences for the methods of study.

Many researchers are interested in studying how human behavior develops. If I assume, for purposes of illustration, that all behavioral development can be regarded as the acquisition of a series of responses to specific stimuli, then I would look for the stimuli in a person's environment that evoke these responses. Consistent with Point 1, I would assume that even complex adult behaviors could be understood on the basis of these stimulation-produces-responding relationships, and my job as a scientist would be to tease out the basic stimulus–response relations.

Accordingly, and in regard to Point 2, the topics that my work would bear on could perhaps be best subsumed by a term such as learning or, more precisely, conditioning. Moreover, as suggested by Point 3, the methods I would employ would be those involved with, for instance, classical or operant conditioning (e.g., Bijou & Baer, 1961; Skinner, 1938, 1950). I would probably prefer not to study topics such as "alterations in the relations among the Id, Ego, and Superego in determining changes in the development of people's object relations" (e.g., Freud, 1954), or "the need for the development of a sense of trust in the first year of life in order for healthy personality development to proceed" (e.g., Erikson, 1959). The methods used to study these non-preferred topics (e.g., clinical interviews and retrospective verbal reports) would not rank very high on my list of preferred methods.

If another developmental scientist asked me how my work related to general issues in human development, I would point out that all scientific research, no matter what topic it bears, is underlain by a particular philosophy of science or of human beings. The developmental scientist querying me could then ask where my assumption—that behavioral development can be viewed as the cumulative acquisition of responses—came from. Could other assumptions be made, for example, that there is something inborn (innate) in human beings that serves to shape their behavioral development? The answer is yes. The point here is that the particular assumptions I make are influenced by the philosophical views I hold about the nature of human development (e.g., Kagan, 1980, 1983; Kantor, 1959; Kuhn, 1962, 1970; Overton, 2015a; Overton & Reese, 1973; Pepper, 1942; Reese & Overton, 1970).

These assertions lead to a second response to the question of how my work is related to general conceptual issues in development. I have noted that research is underlain by theory and, more primarily, by a philosophy of science or of humanity. Therefore, the work of a developmental scientist would be related to general conceptual issues in that it would lead to a determination of the tenability (the defensibility) of his or her position. As the developmental scientist continued to work from a particular point of view, he or she would eventually be able to see how well this viewpoint accounted for the phenomena of behavioral development. The developmental scientist would be able to see if his or her research, based as it is on an underlying philosophical premise, continued to account for these phenomena. For instance, was the theory useful? Did it lead to statements or hypotheses that helped explain substantial amounts of the differences among—the variance in—the scores constituting a particular set of data? Ultimately, the developmental scientist would learn whether the variables being studied were capable of explaining behavioral development or whether other variables necessarily entered the picture.

The developmental scientist would learn whether the exclusive study of the functioning of environmentally based variables—stimuli and responses—could usefully explain behavioral development. If he or she found this not to be the case—if the developmental scientist found, for example, that hereditary-related variables seemed to play a crucial role—he or she would be forced either to give up the initial philosophical/theoretical position and adopt another one or to revise the position so that it could account for the functioning of these other variables with ideas consistent with the original philosophical/theoretical position.

In a third way, too, the outcome of my research can be seen to have general theoretical relevance. This third way, however, can be indirect, and its relevance to general issues or theory may not even be intended. Another developmental scientist might be able to use the facts that a first researcher has found. To explain this third way more completely, I consider some of the reasons why a scientist might conduct a research study.

Some Reasons for Doing Research

The reason why particular scientists conduct particular studies may be idiosyncratic and, in general, diverse. However, three reasons illustrate the ways in which the outcomes of research can have conceptual relevance.

First, a scientist may be interested in illuminating some theoretical controversy. For instance, as I just noted, there may be an observed phenomenon that is accounted for by two different theoretical positions. In adolescent development, for example, it is typically found that there is a marked increase in the importance (saliency) of the peer group. Why does this occur? Both Anna Freud (1969) and Erik Erikson (e.g., 1950, 1959, 1968) have devised theories.

Consistent with the work of her father, Sigmund Freud (e.g., 1954), Anna Freud took what is termed a psychosexual position and tied the occurrence of changed (and increased) salience of peers primarily to a biological change in the person (i.e., the emergence of a genital drive). Erikson (1959, 1968), however, diverges somewhat from strict psychoanalytic (i.e., Freudian) theory (he was an ego psychologist; Rapaport, 1959). Erikson explained increased peer group salience in what he termed a psychosocial model, and he specified some possible relations between the developing person and his or her society. For instance, the need to find and test roles that may be played in society, and not only a new, sexual motive, may promote relationships between a young adolescent and his or her peer (who, for instance, may tolerate role-testing behaviors more so than parents, perhaps because he or she too may need an accepting audience for enacting such behaviors). Such relationships may provide a youth with opportunities to “try on” different roles with other young people without having to make a full or long-term commitment to them.

Which theory can best account for the empirical facts? This question constantly arises in the course of scientific inquiry. A clever researcher may be able to devise a study that would put the two different interpretations to a so-called critical or crucial test—a study whose results would provide support for one theoretical position and non-support for the other. If the results came out one way, Theory “A” would be supported; if they came out another way, Theory “B” would be supported.

It is important to note, however, that whether a scientist can perform a crucial test of two theories, or only a test of specific competing hypotheses derived from these theories, is itself a controversial issue. According to Hempel (1966), a philosopher of science, two hypotheses derived from two different theories can neither be proved nor disproved in any absolute sense. Hempel argues that this situation is true even if many tests of these two hypotheses are performed by the most sophisticated researchers using the most careful and extensive methods available to them, and even if all test outcomes result in completely favorable results for one hypothesis and completely unfavorable results for the other. Such results would not establish any absolute, conclusive validity for one hypothesis, but rather only relatively strong support for it. It is always possible that future tests of the two hypotheses would result in favorable outcomes for the previously unfavored hypothesis and in unfavorable outcomes for the previously favored one. In addition, it is also possible that, if other hypotheses were derived from the two different theories, tests of these two new competing hypotheses would result in favorable outcomes for the theory that was not supported when the first set of derived hypotheses was tested. Thus, as Hempel argues, in an absolutely strict sense, a crucial test is impossible in science.

But the results of testing two competing theoretical positions may be "crucial" (and extremely useful) in a less strict sense. Results of tests of two rival positions can indicate that one theory is relatively untenable, whereas the other position is relatively tenable. This contrast may arise because the theories are found to be differentially useful in explaining (accounting for) the findings of research. For instance, all the differences among the scores (e.g., of children on a test of reading ability) in a particular set of data (e.g., the data derived from a study of, say, 100 third-grade children) equal, by definition, 100% of the variance in a data set. One of two tested theories ("A") may account for substantially more (a greater proportion) of the variance (e.g., 55% as compared to 10%) in this data set, and this result means that in this case Theory "A" was more useful.

In other words, because tests of Theory "A" resulted in (more) favorable outcomes (i.e., more variance was accounted for), it is more tenable; that is, it may be considered more useful as, for instance, operationalized by accounting consistently for more variance in different data sets. It is the theory that appears best able to account for existing facts. Because of the theory's demonstrated usefulness, it might play a more prominent role in any further work in the field. However, even if one construes crucial tests in a relatively unstrict way (i.e., in respect only to relative use), such tests are few and far between in human development. Still, they remain a potentially important and useful impetus for research.

A second reason for doing research is to test ideas (hypotheses) derived from a theory. Such deductions are made in order to see whether they can be empirically supported through research. Researchers would start by saying that, if their theory is making appropriate statements, then certain observations should necessarily be seen. For example, suppose a developmental scientist has a theory that, as children develop, the conceptual categories they can actively use to designate specific classes of things in their environment become more differentiated. For instance, returning to an earlier-used example, the researcher might suspect that, no matter what animal was shown to a 2-year-old, the child would respond by saying "doggie" (or some equivalent term, such as "woof-woof"). The researcher might also suspect that, if he or she looked at a somewhat older child, say a 4- to 5-year-old, the ability to correctly classify different animals (dogs, cats, and elephants) would be seen and, also, the ability to correctly classify different types of dogs (collies, German shepherds, and poodles) would be observed. Thus, in accordance with the theory, the researcher might hypothesize (predict) that, as the children increased in age, their ability to correctly classify different animals would also increase. If the theory is useful, the hypothesis, deduced from the theory, should be supported by the results (observations) of the study.

By testing deductions, researchers can provide support or refutation for their theory. Research based on such deductive reasoning is an important component of scientific thinking, and it will be discussed in further detail below.

However, as noted, there is a third way in which research can be found to be relevant to theory. Sometimes a researcher may conduct a study just to find out what exists. A person may have no theoretical issue in mind but may only be interested in describing the characteristics of a specific phenomenon or aspect of behavioral development, or in seeing what will be the behavioral result of a specific manipulation.

Suppose a developmental scientist is employed as a researcher in a summer camp program for children and adolescents, and finds that there is a problem with some youth taking the possessions of other youth with whom they are sharing living quarters. Faced with such a problem, the researcher might reason that, in order to design an effective strategy to prevent or reduce such behavior, it would be useful to know the reasons that the 5- through 15-year-old campers might give to explain why a person should not steal from friends. The researcher might then ask groups of 5- and 15-year-old campers to give their reasons for not stealing from friends. The results might be that 5-year-olds' reasons seem to be rather concrete, reflecting a fear of punishment and an orientation toward obedience to rules or to camp authorities (e.g., counselors). A 5-year-old might say that children should not steal because their friends would hit them or their camp counselor would punish them for doing something that is wrong. The 15- year-old's reasons might be more abstract, reflecting the notion that stealing from a friend violates implicit rules of mutual trust and respect, or that, as a member of society, one implicitly has to respect the rights of others. Researchers might find this result of such interest that they report it in some formal way (e.g., through a presentation at a professional meeting or in a journal article), and this dissemination adds additional facts to the literature of the science.

Although researchers may not intend to relate their facts to any theories, the theoretical relevance of the facts can be found after the research is done. In attempting to ascertain the validity of a particular theory, someone may be able to use the facts as a means to support that theory. Thus, the facts reported in the above-noted example could, after their communication, be seen to fit into a theoretical formulation. In fact, Kohlberg (1963a, 1963b) formulated a theory of moral development that could incorporate the hypothetical findings. As outlined in Table 1.1, his theory divided moral reasoning into three levels, with two stages within each level.

In other words, although a fact may be "loose"—not related to a theory—this situation does not exclude the possibility that, at some later time, the fact may be seen as related to or consistent with a theory-based concept. Some facts not initially intended to be directly related to a theory may, eventually, find their way into one. This linking takes place through another major type of scientific thinking process: inductive reasoning. In this process, a scientist will start with sets of facts and then try to find some conceptual formulation to organize and perhaps explain them. A scientist using such reasoning proceeds from observed facts to integrative concepts or theories.

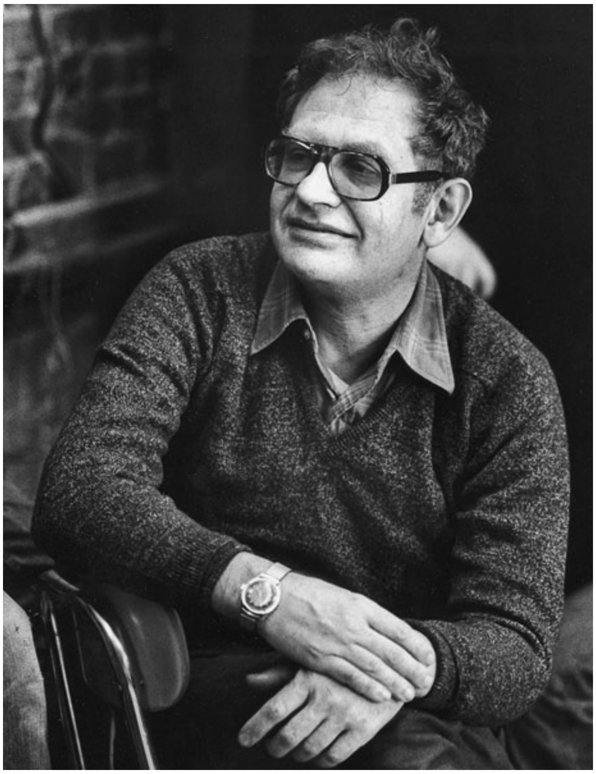
Table 1.1 Kohlberg's theory of moral reasoning development

Building on Piaget's (1932) distinction between objective and subjective moral reasoning, Lawrence Kohlberg (1958, 1963) proposed a theory of the development of moral reasoning that included three levels of moral reasoning with two stages within each level:

Level 1. Preconventional moral reasoning
Stage 1. Obedience and punishment orientation
Stage 2. Naively egotistic orientation

Level 2. Conventional moral reasoning
Stage 3. Good-person orientation
Stage 4. Authority and social order maintenance orientation

Level 3. Post-conventional moral reasoning
Stage 5. Contractual-legalistic orientation
Stage 6. Conscience, or principle, orientation



Lawrence Kohlberg

In the various ways outlined above, the outcome of all research may bear on the general conceptual and theoretical issues of a science (assuming, of course, that the results of research are not methodologically flawed). Although a researcher's reasons for undertaking the study of a topic may not relate to these general considerations, it is important to be aware of this perspective if only to gain an appreciation of the cumulative and dynamic aspects of a science such as human development. From this perspective, a student of developmental science will be able to see several things, such as:

1. Why some people study one topic whereas others investigate another. Differences in underlying philosophies of science and/or of humanity lead to differences in the assumptions a scientist makes about the nature of the subject matter. This variation leads scientists to look at different aspects of development and, hence, to investigate different topics.
2. Why abstract theoretical debates occur. When scientists adopt a particular philosophical or theoretical point of view, they become committed to it; they attempt to defend it, to show its tenability (e.g., Kuhn, 1962, 1970). They will attempt to justify their positions through logic and empirical research. Commitments to different theoretical points of view may lead one scientist to interpret a given fact one way, and another scientist to interpret the same fact a different way (Overton, 2015a; Overton & Reese, 1973; Pepper, 1942; Reese & Overton, 1970).
3. Why an understanding of these theoretical concepts is crucial for an adequate understanding and appreciation of the research, data, and the accumulated facts of a science. If students are given this conceptual perspective, they will know not only some implications of the results of one or more research studies but also the meaning and relevance of research as it bears on the general concepts of a science.

I believe students who understand these three points will better appreciate the intellectual excitement that many developmental scientists have for their scholarship. This belief frames the approach taken in this book.

THIS BOOK'S APPROACH

First and foremost, I must admit that I am biased. I have specific beliefs about how best to conceptualize the concept of development and, from this conceptualization, how best to devise theories or models (terms that, unless specifically noted otherwise, I will use interchangeably) of the process of development. I believe that science should be approached from integrated, philosophical, conceptual, and theoretical points of view and my reading of the literature of philosophy of science, the history of science, and developmental science has persuaded me that the appropriate philosophical frame needed to understand development is derived from what Willis F. Overton (2015a) has described as a process-relational paradigm. In Chapter 2 I shall explain the meaning of the term paradigm (Kuhn, 1962, 1970) and, as well, the role of paradigms in science (Pepper, 1942). I will also discuss the specific features of the process-relational paradigm. I shall explain, as well, that my bias towards the process-relational paradigm will lead me to adopt a theory of how theories about human development should be constituted—that is, the paradigm I favor leads me to a belief in the use of a particular *metatheory* (or, simply, a theory about theories).

I explain in Chapter 2 that the metatheory I use to frame my ideas about theory is termed by Overton (2015a) relational developmental systems (RDS). Although a fuller discussion of RDS metatheory will be presented in Chapter 2, I should note here that, in addition to framing the features believed to be needed in useful theories or models of human development, RDS metatheory also shapes the very conception, or definition, of development involved in these models. As such, it is useful to note briefly the link between theories associated with RDS metatheory and the concept of development.

RDS-Based Theories and the Concept of Development

Developmental science seeks to *describe* (depict or represent), *explain* (account for), and *optimize* (maximize the probability of health and thriving) changes within an individual (*intraindividual change*) and differences between people (*interindividual differences*) in intraindividual change *across the life span* (Baltes, Reese, & Nesselroade, 1977; Lerner, 2012). However, what sorts of changes characterize an individual as he or she develops across the life span? Where do these changes come from? Do all of these changes pertain to the development of the person? Questions such as these are inevitably involved in any theoretical consideration of human development (Raeff, 2016). Theories of development using ideas from RDS metatheory have specific answers to such questions.

Essentially, RDS metatheory-based ideas or, more simply (for ease of exposition), RDS-based ideas, involve the view that human beings are active rather than passive and that humans are part of an integrated system of relations involving the individual and the ecology (the context) of human development; and it is this system of relations that is the source of the course of development across life (Lerner, 2015a; Raeff, 2016; Witherington & Lickliter, 2016). The integrated (relational) developmental system provides the necessary and sufficient conditions to account for the structure and function of development across the life course. As such, humans are self-constructors of their development; they are *autopoietic* (Overton, 2015a; Lerner, 1982, 1984, 2012; Lerner & Busch-Rossnagel, 1981a; Lerner & Callina, 2014a; Lerner & Walls, 1999; Witherington & Lickliter, 2016).

RDS-based ideas therefore also emphasize that the world around the developing person—both the physical and the social ecology of human life—is active and changing. According to all RDS-based models of human development (see Overton & Molenaar, 2015, for examples), the basic process of development involves, then, the integration or fusion of actions within the relational development system (Tobach & Greenberg, 1984). Specifically, then, in RDS-based models, there is an integration of variables across all levels of organization within the system; this integration involves (a) the actions of people in and on their world, and (b) the actions of the world on people. I may use a bidirectional arrow— ↔ —to represent these integrative and mutually influential relations. Integrated actions shape the quality and course of human behavioral, psychological, social, and cultural structure and function across the life span (Brandtstädter, 1998, 1999; Brandtstädter & Lerner, 1999; Lerner & Busch-Rossnagel, 1981a; Mascolo & Fischer, 2015; Raeff, 2016; Witherington & Lickliter, 2016).

Sources of Action in Human Development

But, where do the actions that propel human development come from? Consistent with its conceptualization of the character of human life—of focusing on the integration of levels of organization—RDS-based ideas emphasize that the source of the actions involved in human development is derived from the mutually influential relations within the relational developmental system, for instance, between the individual and his or her context, represented as individual↔context relations.

These dynamic (mutually influential) changes may involve both quantitative and qualitative changes in the processes of development. For instance, processes involved with a person's perceptual, motivational, or cognitive development undergo changes in kind or type (quality), and in amount, frequency, magnitude, or duration (quantity). This conception of change does not deny that there are some aspects of a person that remain the same throughout life; rather, it asserts that human development is a synthesis between processes that promote change and processes that promote constancy (Brim & Kagan, 1980a; Lerner, 1985; Overton, 2015a).

In other words, due to the integration of the organizational levels of human life, ranging from biology, through the individual and social relationships, to the community, institutional, cultural, and historical, RDS-based models indicate that the laws that govern the functioning of both constancy and change are relational ones; they pertain to processes of interrelated relations across all levels of organization within the developmental system. No one level, and no one variable within a level (e.g., genes), is privileged as the key to these integrated relations (Noble, 2015). *The process of development, therefore, cannot be reduced to one level or variable.* As I will discuss across the chapters of this book, this rejection of reductionism, and more specifically, of genetic reductionism, is the key difference between models derived from RDS and many other contemporary models associated with human development (e.g., behavioral genetics, sociobiology, or evolutionary developmental psychology).

The rules, or laws, that are discovered to explain the specific features of a human's development are, then, relational laws, and not reductionist ones. By relational here I mean that the laws are ones that pertain to coactions within the integrated developmental system and, as such, that the laws of development may apply (at least in some part) idiographically (i.e., they may apply only to an individual), as that individual develops at a specific time and in a specific place within the relational developmental system (Bornstein, 2017; Molenaar & Nesselroade, 2015; Rose, 2016). Hempel (1966) defines a law as a statement asserting some invariant characteristics about a phenomenon or process. However, given the RDS-based view of laws that I have just noted, I will also grant the status of law to statements that apply only approximately, with certain qualifications, under specific and specifiable conditions, or with a given level of probability. In other words, reductionist laws involve the assumption that the universe is uniform and permanent; that is, that all phenomena of human development may be reduced to laws that apply invariably across time and place. In contrast, the laws associated with RDS-based models involve the assumption that the universe is variegated and changing; that is, time and place matter (e.g., Elder, Shanahan, & Jennings, 2015) in human development.

Given, then, the RDS-based approach I use to frame my ideas about the laws of human development, what are the features of a definition of development that I would emphasize? The meaning of the term development has engaged, and continues to engage, scholars in philosophical and theoretical debate (e.g., Collins, 1982; Ford & Lerner, 1992; Harris, 1957; Kaplan, 1966, 1983; Lerner, 1978, 1985; Lerner & Murray, 2016; Overton, 2015a; Overton & Reese, 1973; Raeff, 2016; Reese & Overton, 1970). The existence of the debate is itself indicative of a key feature of the meaning of the term. That is, development is not an empirical concept. If it were, inspection of a set of data would indicate to any observer whether development was present.

However, different scientists can look at a data set and disagree about whether development has occurred. This situation arises because development is a theoretical concept. It is, as Kaplan (1966, 1983) put it, a concept of postulation. One's study of development begins with some implicit or explicit concept of what development is. Then, when one inspects a given set of data, it can be determined whether the features of the data match, or fit with, one's concept.

In other words, a given scientist's concept of development serves as a conceptual template. For instance, if a developmental scientist believes that developmental changes are only changes that reflect an alteration from global, or undifferentiated, functioning (e.g., labeling any four-legged creature with the verbal label "doggie") to differentiated functioning (e.g., responding to such creatures with labels appropriate to their species, such as doggie, kitty, and mouse), then if and only if a given change conforms with this format will it be labeled a developmental change (see Raeff, 2016; and see the discussion of Werner's orthogenetic principle, 1948, 1957). Scientists would use such a conceptual template when they look at data. Observations that coincide with the structure of the template are labeled developmental ones; observations that do not match the template are judged non-developmental.

Debates among scientists about the meaning of development arise because different scientists have different templates. These conceptual differences exist because different scientists are committed to distinct philosophical and theoretical beliefs about the nature of the world and of human life. For instance, some scientists find it useful to view the world as analogous to a machine and to study humans in terms of the energies needed to set the discrete parts of the machine in motion (see von Bertalanffy, 1933; Reese & Overton, 1970). Other scientists do not find it useful to use the machine metaphor. Instead, they conceive of humans as integrated wholes, and they study how the structure or the organization of this whole changes over time (see Reese & Overton, 1970).

Despite the philosophical and theoretical differences that exist among scientists in their conception of development, there is some agreement about the minimal features of any concept of development.

In its most general sense, development refers to change. But clearly, change and development are not equivalent terms. If they were, there would hardly be a need for the more abstract term development, and there would seem to be little reason for the philosophical and theoretical debates about the meaning of the term. Thus, although whenever development occurs there is change, not all changes are developmental ones.

The ups and downs of one's checkbook balance, for example, involve changes, but few if any scientists would label such changes as developmental. In addition, random (stochastic), chaotic, completely disorganized, or totally dispersive changes cannot readily be construed as developmental change. Changes must have a systematic, organized character in order for them to be labeled developmental.

But systematicity, or organization, does not suffice to completely define development. An office organized by one office manager may run by one system, whereas another office, organized by another manager, may run by a completely different system. If the first manager leaves his or her job and is replaced by the second manager, the latter person may change the former manager's system to the one he or she prefers. A system, or an organization, exists during the tenure of the first manager and, as well, during the tenure of the second. Yet, the second system is not an outgrowth of the first; there is no necessary connection between the two. In fact, if the first manager returns to the job, the first system can be reinstated, and in such a case, there would again be no necessary connection between the immediate past and the present organizations. Thus, although change occurred and although a system existed at the two points in time across which change was observed, there was no connection between the two systems. The character of the first system in no way influenced the character of the second system. Accordingly, the change in the office was not developmental, although it did involve an organized systematic structure.

For organized, or systematic, changes to be developmental, they have to have a successive character. The idea of successive changes indicates that the changes seen at a later time are at least in part influenced by the changes that occurred at an earlier time, if only to the extent that the range of changes probable at the later time is limited by earlier occurrences. In short, in a most general sense, the concept of development implies systematic and successive changes in an organization.

Virtually without exception, however, developmental scientists go considerably beyond this minimum definition. For instance, historically the concept of development is biological (Harris, 1957) and, in most cases, there was an implicit, and often an explicit, assumption that biological meant genetic. For instance, the founder of developmental science in the United States, G. Stanley Hall (e.g., 1904), labeled the first journal in the field the *Journal of Genetic Psychology* (a name it still bears), and Piaget (e.g., 1970, 1972) labeled his theory of the development of cognition *genetic epistemology* (although, certainly, Piaget was not a genetic reductionist; e.g., Piaget, 1978).

However, because of the general inclination to link the concept of development with biology, the unit of concern (or analysis) for most developmental scientists was typically an individual organism, or its biological components (e.g., the brain, or genes). Furthermore, because the intellectual roots of the concept of development lie in biology, developmental changes were held to be only those systematic, successive changes in the organization of an organism that are thought to serve an adaptive function (i.e., to enhance survival; Schneirla, 1957). Often, when a change results in increased adaptation, it is regarded as a progressive change (Ford & Lerner, 1992; Nisbet, 1980).

Other developmental scientists postulated that organized, successive changes must have a specific form in order for one to say that a developmental progression exists. In other words, only when the structure of an organization changes in a specific way and/or when a specific sequence of changes happens is development said to occur. For example, building on the illustration of a template for developmental change that I provided earlier, we may note that Werner (1948, 1957), Werner and Kaplan (1956), and Kaplan (1983) postulate that development exists when a system changes from being organized in a very general or global way (wherein few, if any, differentiated parts exist) to having differentiated parts that are organized into an integrated hierarchy (see too Raeff, 2011, 2016). Werner and Kaplan label this concept of development the *orthogenetic principle* and indicate that only those structural changes that coincide with this sequence of globality to differentiated and integrated parts fulfill the requirements for a developmental progression (Werner & Kaplan 1956, 1963; see too Raeff, 2011, 2016; see too Chapter 8).

The point of these examples is that, despite a relatively high degree of consensus about development being a theoretical concept that, at the least, connotes systematic and successive change in an organization, there is a good deal of disagreement among developmental scientists about what particular ideas need to be added in order to define the term adequately. These differences in definitions are associated with philosophical and theoretical differences which also divide scientists. As I shall explain in more detail in Chapters 2 and 3, the theoretical differences among scientists are ultimately based on their commitments to different philosophical positions (Kuhn, 1962; Pepper, 1942). Here, however, I should note that philosophical views also influence the day-to-day work of developmental scientists.

PHILOSOPHY, THEORY, AND THE WORK OF DEVELOPMENTAL SCIENTISTS

The philosophical ideas that developmental scientists follow are not only associated with different theories of human development and with specific conceptions of key features of the definition of development. As emphasized earlier in this chapter, theory should also shape the questions to be addressed in the research of a developmental scientist. In addition, a scientist's questions should shape the specific empirical methods he or she uses to address his or her questions. In the conduct of good science, the questions one asks should determine the methods one uses to address them (although, unfortunately, there are some researchers whose preference for a given method, e.g., a randomized control trial, or RCT, shapes the questions they ask; see Lerner, Agans, DeSouza, & Hershberg, 2014; Lerner, Lerner, Urban, & Zaff, 2016, for discussions of this point; see too Chapter 13). However, in the flow of ideas from philosophy of science to the empirical conduct of a scientific study that I have described, there is eventually a link between the philosophical ideas from which one starts and the data one collects.

Figure 1.1 depicts these connections between philosophy, theory, method, and data within the context of a funnel. Paradigms and metatheories are at the top, broadest portion of the funnel and the ideas associated with these sets of philosophical ideas frame the various theories that can be derived from them. For instance, the process-relational paradigm is associated with the RDS metatheory and the split, Cartesian paradigm (discussed later in this chapter and again in Chapters 3 and 4) is associated with reductionist, essentialist, and mechanistic metatheories (Overton, 2015a). Theories at different levels of abstraction can be generated from a paradigm and a metatheory derived from it. As shown in the funnel diagram, at the highest level of abstraction, *grand theories*, or theories of everything, can be associated with these philosophical bases. For instance, grand theories derived from essentialist, reductionist, and mechanist theories are behaviorism (e.g., Skinner, 1971) and sociobiology (e.g., Wilson, 1975a).

Mid-range theories, that is, models that pertain to one or some domains of human development but not to all possible domains, are sets of ideas that exist further down the funnel. Several instances of process-relational- and RDS metatheory-based models are examples of mid-range theories. For instance, the respective models of moral development presented by Turiel (2015), Nucci (2017), and Turiel and Nucci (2018) and, as discussed in Chapter 10, the model formulated by Fisher and his colleagues regarding the development of skills within the dynamic, relational developmental system (e.g., Fischer & Bidell, 2006; Mascolo & Fischer, 2015) and the sociohistoric-cultural model of cognitive development proposed by Rogoff (1998, 2003, 2011) are examples of mid-range theories. Still further down the funnel are *topic- or variable-specific theories*. Examples here are Damon's (e.g., 2008; Damon, Mennon, & Bronk, 2003) model of youth purpose and the Lerner and Lerner model of positive youth development (e.g., Lerner, Lerner, Bowers, & Geldhof, 2015).

When methods are used correctly in work aimed at advancing the understanding of human

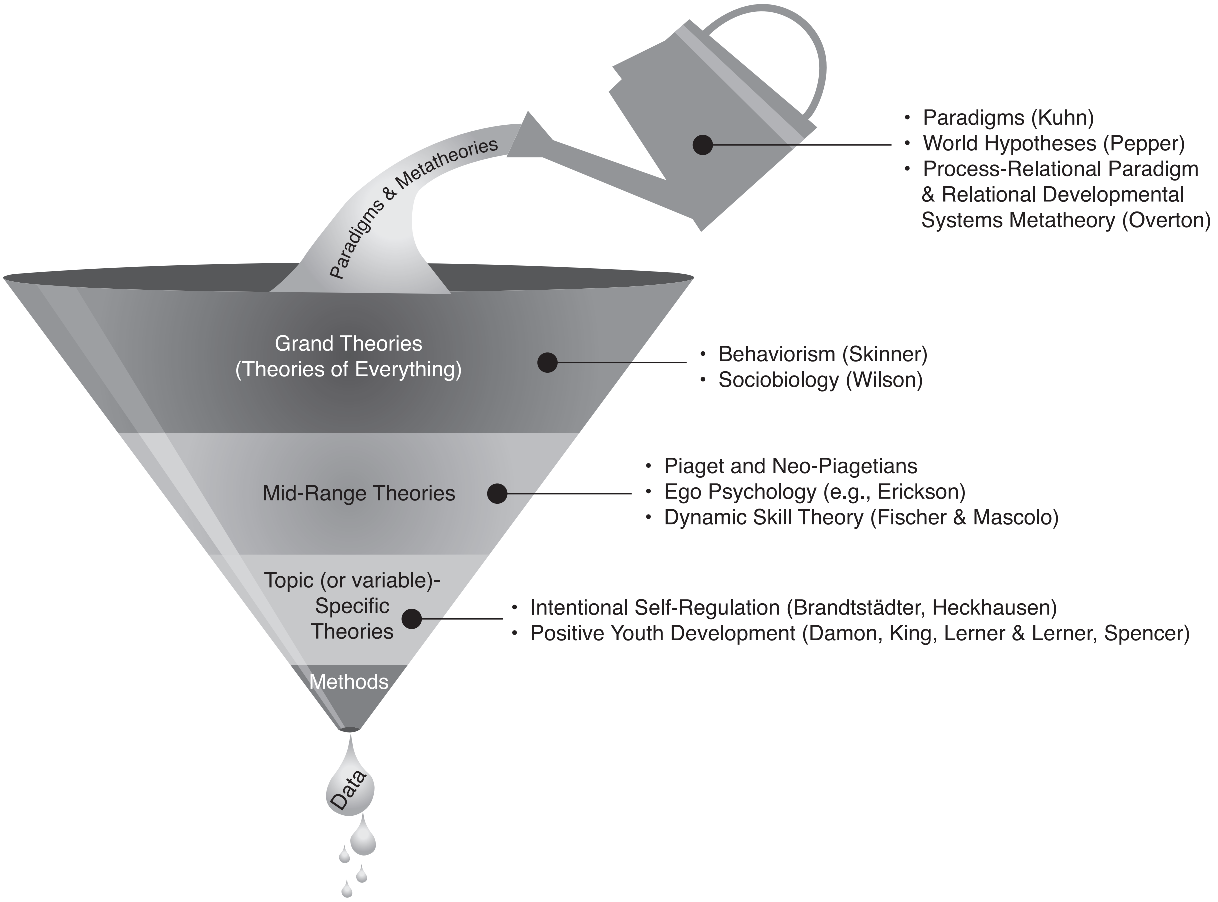


Figure 1.1 A funnel model of levels of theoretical integration in human development.

development, specific methods are used because they are appropriately derived from specific, theory-predicated questions or hypotheses. This relation is illustrated as well in Figure 1.1. Finally, what comes out of the funnel are data. If the funnel model is used appropriately, these data will help to build a useful theoretical edifice within developmental science. They will not exist in a manner analogous to bricks piled onto a heap (von Bertalanffy, 1933).

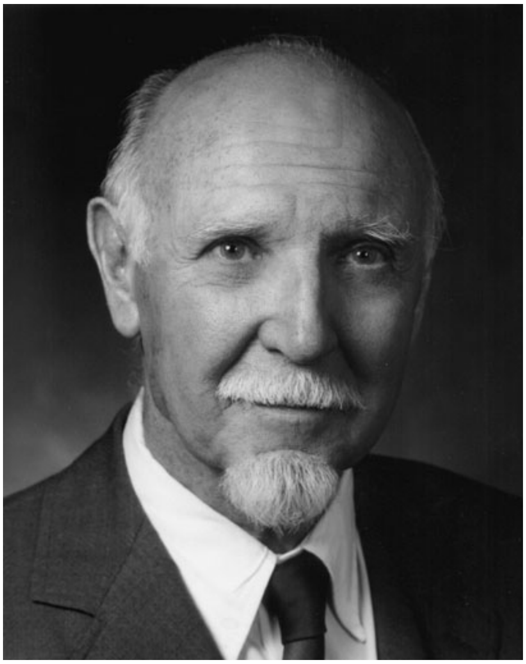
I believe that the biases I bring to developmental science have the happy outcome of enabling theory-predicated research (i.e., research derived from theoretical models framed by the ideas associated with RDS metatheory) to best account for the actual (empirical) characteristics of human development across the life span. RDS-based ideas best explain these characteristics, and best provide ideas for how developmental science may be applied to optimize the course of human development. In fact, I shall argue in Chapter 13 that the application of developmental science framed by RDS-based models will enable evidence-based actions to promote social justice (Fisher, Busch, Brown, & Jopp, 2013; Lerner, 2015b, 2017; Lerner & Overton, 2008).

Therefore, I shall argue in this book that my approach to developmental science (my biases) enables me to point to theories that best fulfill the role of scientific theories. That is, as noted earlier, RDS-based ideas allow me to provide a set of statements (e.g., concepts and principles) that integrate existing facts and lead to the generation of new ones.

Hempel (1966) too suggests that a theory explains empirical uniformities that have been previously discovered and usually predicts new regularities of similar kinds. To be consistent with this definition, I also believe that:

1. *To be considered sound and tenable (manifesting consistent use in accounting for substantively meaningful proportions of the variance in studies of human development), a theory should be able to integrate existing facts*. That is, a theory should be able to account for the established empirical findings on which it has been established. For example, operationally, to be useful, a theory should be able to account for the differences (the variance) in a set of data. To be more useful than other approaches, a theory should account for further variance (i.e., the theory would possess greater "deployability"), and should do so more readily, with more precision (i.e., with fewer statements, assumptions, and positing of unobservable constructs), and in greater scope (i.e., it is useful for a greater number of data sets than are other theories). The usefulness of a theory may be evaluated, then, in regard to its attributes of precision, scope, and deployability (see Baltes & Nesselroade, 1973; Baltes, Reese, & Nesselroade, 1977; Overton & Reese, 1973).
2. *Based on theory, a scientist should be able to devise some statements that—if found to be borne out by research—would provide support for a theory (and if found false, would not support it)*. Put in more formal terms, a scientist should be able to generate testable hypotheses from a theory. The hypotheses should be open, then, to falsification (Popper, 1959) through empirical tests. In a scientific theory, these statements must be empirical ones that, if found not to be the case (through methodologically-rigorous observations), would falsify the hypothesis. Although hypothesis falsification does not, in itself, falsify the theory, it provides no support for it. The hypotheses scientists construct usually take the form of "if–then . . ." propositions; that is, "if my theory is appropriate in saying so and so, then such and such should be the case." Scientists would reason that if their theories were useful, their deductions, which were open to falsification by empirically appropriate and rigorous observations, should be supported by the outcome of research (by the empirical relations observed).
3. *It is essential, then, that a scientist puts his or her deductions to an empirical test, by making some sort of relevant observation, for example, within an experimental or correlational study*. If the results yield the predicted findings, then this new fact will be appropriately placed within a theoretical system. This fact will be a brick added to a house rather than piled onto a heap.

Of course, as scientists function in their day-in-day-out endeavors the deductive process I have described may not always occur. In the real-world actions of scientists, deductive reasoning and inductive reasoning may occur, and Raymond B. Cattell (1966) suggested that the two types of reasoning may go hand-in-hand in the actual work of a scientist. Cattell (1966) explained that a given scientist may attempt to establish "general laws which can be empirically tested and which lead to deductions extending our theoretical understanding and practical control" (p. 11). However, he pointed out as



Raymond B. Cattell

well that at different times the same scientist may do a particular study for many different reasons. For instance, the initial impetus for research may be the observation of a curious empirical phenomenon or regularity. In such a case, research may be hypothesis-searching (or "exploratory") as well as hypothesis-testing (Cattell, 1966).

Thus, research may begin with interesting observations of empirical reality obtained while the scientist is working in the general context of a theoretical orientation. For example, believing in a theory that specifies that all behavior is a response to stimulation, the scientist may present a novel stimulus to a 3-year-old child (e.g., a slice of purple bread) just to see what sorts of responses are elicited. From this empirical observation, the researcher might induce that a fact (e.g., the child responds with a negative facial expression to the purple bread) is representative of some more general regularity (e.g., color is an important aspect of food stimuli, and atypical colors are disliked). As a consequence, the researcher might formulate a hypothesis to test the validity of this induction (e.g., if food stimuli are presented that are typically colored, such as an orange tangerine, to a young child, then more positive reactions will be elicited than will be the case if an atypically-colored food stimulus, such as a blue tangerine, is presented). Then, the researcher might deduce what empirical consequences would have to be obtained in order for the hypothesis to be confirmed (e.g., children may smile and reach for orange tangerines and may frown and withdraw from blue ones). Accordingly, the researcher may make another, higher-order empirical observation, and the whole process would start anew.

In actuality, then, the method that perhaps best characterizes the reasoning of the practicing scientist is neither purely deductive nor purely inductive. Rather, it may be what Cattell (1966) terms inductive-hypothetico-deductive in nature, and what others term retroductive or abductive reasoning (Overton, 2006). As illustrated in Figure 1.2, this method begins with some empirical observation, which in turn serves as the basis for the induction of some empirical regularity. This induction needs to be subjected to empirical verification; however, in order to ascertain its validity, a hypothesis is derived from the induction, and the empirical consequences of this hypothesis are deduced and tested. The result of this test is, of course, another empirical observation, which continues the inductive-hypothetico-deductive spiral again.

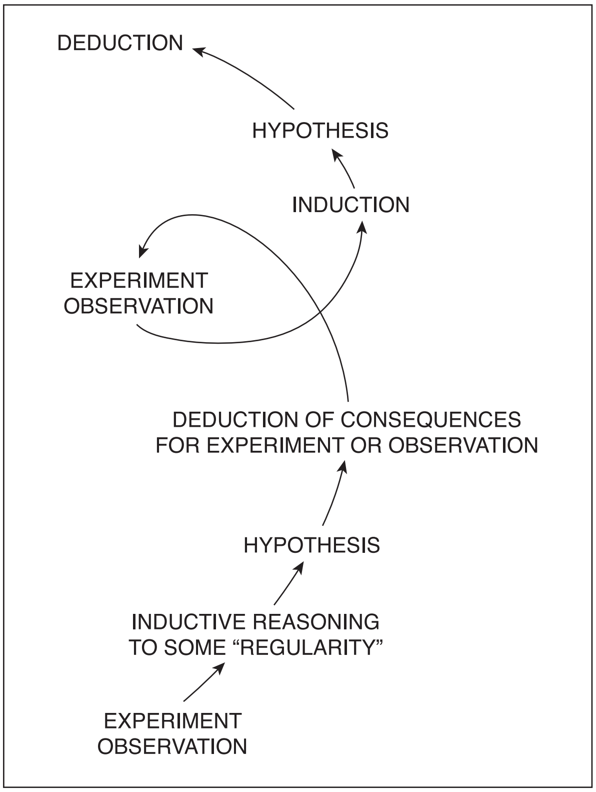


Figure 1.2 Cattell's notion of the inductive-hypothetico-deductive spiral.

Source: R. B. Cattell (ed.). (1966) *Handbook of multivariate experimental psychology*, Diagram 1.1, p. 16. Copyright © 1966 Rand McNally & Company, Chicago. Reprinted by permission of Rand McNally College Publishing Company

CONCLUSIONS

Although a distinction between inductive and deductive reasoning is valid, in actuality the practicing research scientist often uses both techniques. Yet, it still remains the case that inductions are made in the context of a scientist's explicit or implicit theoretical understanding of the phenomena he or she is studying (Cattell, 1966) and, as well, in his or her implicit or explicit recognition of the philosophically-based presuppositions he or she holds about the nature of the world (Kagan, 1980, 1983; Kuhn, 1962, 1970; Pepper, 1942). Accordingly, then, across the chapters of this book I privilege the discussion of the philosophy–theory portions of the funnel diagram in Figure 1.1. It is useful, therefore, to describe how I will discuss these ideas across the book.

THE PLAN OF THIS BOOK

Chapter 2 will provide an overview of the key concepts and theories within *contemporary* (at least at the time that this fourth edition of this book was written) developmental science. The discussion in Chapter 2 will continue to use RDS-based ideas to frame the concepts and theories that are presented and, as such, Chapters 1 and 2 provide the overall conceptual framework with which subsequent literature will be described and explained.

The roots of contemporary concepts and theories in developmental science lie in the history of philosophical ideas and scientific discussions pertinent to the field. This history is reviewed in Chapter 3, which then provides the grounding in philosophy and science needed to review, in Chapter 4, the various metatheoretical models of development that have framed, and, to varying extents, continue through this writing to frame, theories of development (Overton, 2015a). The discussion of metatheory in Chapter 4 will, in turn, provide, in Chapter 5, the philosophical grounding need to understand the different approaches to developmental theory, and the roots of these approaches, used within developmental science. Accordingly, Chapter 5 will describe nomothetic (e.g., stage), group differential, and idiographic (e.g., ipsative) approaches to developmental theory.

Across Chapters 2 through 5, I will explain that there are several key conceptual issues that have been involved in philosophical and theoretical approaches to human development. These issues pertain to the nature–nurture controversy and to the controversy of whether continuity or discontinuity characterizes development across the life span. Chapter 6 discusses the fundamental contributions of Anne Anastasi to resolving the nature–nurture controversy and, in turn, Chapter 7 discusses the similarly significant contributions of T. C. Schneirla to resolving this controversy. Schneirla's concept of levels of integration in development is a key idea framing his contribution. In turn, in Chapter 8, I discuss the important contributions of Heinz Werner in resolving the continuity–discontinuity issue and, as such, I focus on the concept of orthogenesis advanced by Werner and his colleagues (Raeff, 2016).

In discussing the issues of nature–nurture and continuity–discontinuity I note that different stances about these issues have derived, in the main, from scholars advancing views derived from one of two *paradigms* (world views or world hypotheses about the way the world exists or is structured, i.e., about ontology; see Figure 1.1 and Kuhn, 1962, 1970; Pepper, 1942). One world view, derived from the philosophy of René Descartes, splits the world into essential, or real, phenomena or elements versus derivative, or epiphenomenal, elements (e.g., see Overton, 2015a). The second world view is termed a process-relational paradigm (Overton, 2015a), and it rejects the splits associated with the Cartesian paradigm (i.e., the paradigm inspired by writings of Descartes) and, instead, provides an integrated and dynamic, systems-orientated approach to the character of being (ontology) of the world. The process-relational paradigm is the world view from which RDS metatheory is derived. Chapter 9 presents the roots of RDS-based theories that are associated with the study of evolution, comparative psychology, and human development across the life span. In turn, Chapter 10 reviews several instances of RDS-based theories of human development.

Using the grounding in RDS-based theories presented in Chapters 9 and 10, I use the ideas linked to RDS metatheory and comparative and developmental theories as the lens for evaluating theories of development derived from Cartesian, split models. I focus on models that seek to study human development through attempts to *reduce* it to purported essential elements, specifically genes or genetic "mechanisms." In Chapter 11, I discuss past genetic reductionist models and focus on the ideas of Konrad Lorenz, Sir Cyril Burt, and Arthur Jensen. In Chapter 12, I evaluate some contemporary (at this writing) genetic reductionist models, and I focus on behavior genetics, sociobiology, and evolutionary developmental psychology.

Chapter 13 turns first to methodological issues in the study of human development but does not depart from using the lens of theory in discussing method. Reasserting the point that I made earlier in this chapter, that, in good science, theoretically-predicated questions should frame a researcher's methodological choices, I discuss how RDS-based models lead to elevating the importance of specific change-sensitive and idiographically-oriented research designs, measures, and data analysis procedures.

Finally, Chapter 13 returns to the third of the key foci of developmental science, that is, optimization. I explain that the ideas associated with RDS metatheory, and the evidence derived from RDS-based theoretical models and methods, lend credence to the idea that, across the life span, humans retain at least some capacity for systematic change, for plasticity, in structure and function. Such plasticity provides a rationale, then, to find or devise individual↔context relations that can alter positively the course of human development. By applying knowledge of the bases of these relations, developmental scientists can, then, improve, or optimize, the course of development for diverse people.

Theory, then, can contribute not only to the description and explanation of developmental change but, as well, to its optimization. Developmental scientists may be able to use their theory-predicated research tools to describe how to enhance every individual and every group or community. Developmental science may be, then, an essential arrow in the quiver of a civil society aimed at promoting a better life, and social justice, for the diverse people of the world.

The work of such developmental scientists may underscore the truth of the observation of Kurt Lewin (1952), that there is nothing more practical than a good theory. We continue in Chapter 2 this "practical" journey toward the application of developmental science.

CHAPTER TWO

Concepts and Theories within Contemporary Developmental Science: An Overview

In this chapter, I will discuss three key sets of ideas. First, I will discuss *approaches to what developmental scientists study*, that is, what are the ideas we need to use to understand why developmental scientists approach the substance of their research in specific ways? What are the concepts and theories that contemporary developmental scientists (at this writing) use to frame their empirical study of human development? Second, I will discuss some ideas pertinent to *how developmental scientists study whatever they study*; that is, I will discuss contemporary (again at this writing) approaches to the methods used in developmental science to study human development.

Third, I will discuss *some of the implications of the contemporary concepts and methods used* by developmental scientists. I will discuss implications for theory, research, and the application of developmental science and, in regard to the latter focus, I will pay particular attention to implications for applications to policies and programs aimed at enhancing *social justice*. That is, social justice is advanced in a world wherein all individuals have equal access to the opportunities and resources to thrive across their lives and, as well, equal freedom from constraints on their positive development based on characteristics such as race, ethnicity, religion, national origin, gender, sexual orientation, and physical ability status (Fisher, Busch, Brown, & Jopp, 2013; Lerner & Overton, 2008).

Across all three sets of ideas that I will discuss, I will illustrate that contemporary developmental science draws ideas from multiple disciplines. For example, in regard to approaches to what is studied, I will devote a good deal of the discussion in this chapter to ideas drawn from philosophy and, in particular, to philosophical ideas about the nature of being (*ontology*) and about the nature of knowledge (*epistemology*); as well, I will draw ideas for this first focus of my discussion in the chapter from biology, and the study of a field within biology termed *epigenetics*. In regard to how developmental scientists study, I will draw ideas from mathematics, statistics, systems science, and economics (and, more specifically, from its subfield of econometrics). Finally, in regard to implications, especially in regard to applications to social justice, I will draw ideas from social policy and evaluation science.

CONTEMPORARY FOCI OF DEVELOPMENTAL SCIENCE

As I noted in Chapter 1, developmental science seeks to describe, explain, and optimize intraindividual (within-person) change and interindividual (between-person) differences in intraindividual change across the life span (Baltes, Reese, & Nesselroade, 1977; Lerner, 2012; Lerner, Lerner, Bowers, & Geldhof, 2015). At this writing, contemporary developmental science is characterized by the centrality of theoretical models derived from the relational developmental systems (RDS) metatheory (Overton, 2015a) I discussed briefly in Chapter 1. Methodologically, the field involves embracing quantitative, qualitative, and mixed-methods research as integral to understanding the meaning and course of human development (e.g., Tolan & Deutsch, 2015; Yoshikawa, Weisner, Kalil, & Way, 2008).

In addition, new foci of developmental science are emerging as central to the field. As I have noted already, these foci are drawn from other disciplines. There is a new understanding of the role of biology in human development, one predicated on an integrative understanding of evolution and of the field of epigenetics (e.g., Jablonka & Lamb, 2005; Meaney, 2010, 2014; Moore, 2015a, 2016; Lester, Conradt, & Marsit, 2016). There are methodological innovations in the study of development that are predicated on the idiographic (individually distinct) nature of within-person (intraindividual) change (e.g., Molenaar & Nesselroade, 2015; Rose, 2016) and on the use of econometric methods to provide evidence about causal processes in community-based programs aimed at promoting positive human development (e.g., Heckman, Ichimura, & Todd, 1997, 1998). As well, greater attention is being given to the use of RDS-based evidence to enact applications (involving social policies or program interventions) to optimize human development and to promote social justice (Fisher et al., 2013; Lerner & Overton, 2008). In this chapter, I will provide an overview of the past and the present status of developmental science (see too Lerner, 2012; Lerner, Agans, DeSouza, & Hershberg, 2014) and describe in some detail the nature and implications of these new foci. Subsequent chapters will provide fuller discussions of the ideas that I present in overview in this chapter.

Edwin G. Boring (1950, p. ix) noted that Hermann Ebbinghaus, one of the nineteenth-century pioneers of the science that was to be termed psychology, once observed that the field "has a long past, but only a short history." The same point is true for the field we term "developmental science." However, the roots of developmental science are not only in psychology but, as well, in fields such as philosophy (e.g., Baltes, 1983; Overton, 2015a), biology (e.g., Bateson, 2015, 2016; Gissis & Jablonka, 2011; Gottlieb, 1997, 2004; Gould, 1977; Ho, 2010, 2013, 2014; Ho & Saunders, 1979, 1984; Jablonka & Lamb, 2005; Keller, 2010; Lewontin, 2000; Moore, 2015a, 2016; Saunders, 2014), sociology (e.g., Brim & Wheeler, 1966; Elder, 1974, 1980; Elder & Shanahan, 2006; Elder et al., 2015; Riley, 1979), medicine (e.g., Blum & Nelson-Mmari, 2004; Ozer & Irwin, 2009; Zuckerman & Keder, 2015), economics (e.g., Cunha & Heckman, 2014; Cunha, Heckman, & Schennach, 2010), and education (e.g., Hyson, Copple, & Jones, 2006; Rose, 2016).

As I have noted already, contemporary developmental theory is framed by theories derived from relational developmental systems (RDS) metatheory. A *metatheory* is, in effect, a theory about how one formulates theories. A metatheory is a set of philosophical ideas (or principles) about what are the set of ideas that should be included (and/or excluded) in a theory that seeks to integrate existing information about a component of the world (e.g., human development) and that aspires to be a basis for the generation of new information about this component.

RDS metatheory emphasizes that change across life occurs through mutually influential relations between individuals and their contexts, represented as individual↔context relations (Brandtstädter, 1998, 2006; Lerner, 2006b, 2010; Overton, 2010, 2015a; Overton & Müller, 2013). Individuals influence (or regulate, that is, they contribute to the nature, scope, and timing of) changes in the context and, as well, contexts (e.g., social relationships, societal institutions, events in the natural or designed ecology) regulate changes in individuals (Lerner, 2012, 2015a; Rose, 2016). When such relations are beneficial to both individual and context, they may be termed *adaptive* developmental regulations (Brandtstädter, 1998, 2006). The goal of developmental science is to describe, explain, and optimize intraindividual changes in adaptive developmental regulations and, as well, interindividual differences in such relations, across life (Lerner, Easterbrooks, & Mistry, 2013; Baltes, Reese, & Nesselroade, 1977).

These foci have not always been among the key interests of scientists involved in the study of human development. It is useful to consider briefly here the past approaches to the study of human development. I then discuss the relatively short history of developmental science, and describe current scholarship in the field. Finally, although cognizant of the purported observation by New York Yankees Hall-of-Fame catcher, Yogi Berra, that prediction is very hard, especially in regard to the future, I present ideas about the future of developmental science (Lerner et al., 2014).

THE PAST

During the latter years of the nineteenth century and for much of the twentieth century, and perhaps especially in the United States and Western Europe, the study of human development became a visible subfield of psychology (Cairns & Cairns, 2006). In this literature, and its antecedents in philosophy (Baltes, 1983), development was envisioned to be a life-span phenomenon (e.g., Erikson, 1959; Hall, 1904, 1922). However, the majority of the scholarship about human development in the United States and Western Europe was focused on the early years of life (infancy and childhood) (e.g., Binet & Simon, 1905a, 1905b; Gesell, 1929; Piaget, 1923; Preyer, 1882; Terman, 1925).

As a consequence, across this historical period, child psychology emerged as a specific subarea of psychology, spurred on by the research of scientists studying this portion of the life span, by the founding of several university centers and institutes devoted to the study of children (e.g., in Iowa, involving scholars such as Boyd R. McCandless; and in Minnesota, involving scholars such as Dale B. Harris), and by the work in the field of home economics that was focused on children (and families) and that was occurring within land-grant universities in the United States (Cairns & Cairns, 2006; Lerner & Simon, 1998). At the same time, many of these bases of child psychology also created a purportedly multidisciplinary instantiation of scholarship devoted to the study of children, that is, child *development*. In 1933, the Society for Research in Child Development (SRCD) was founded to promote such a multidisciplinary approach to the study of children (and to the application of child development research) but, in actuality, SRCD was from its outset and remains at this writing dominated by scholars whose training is in psychology. It is not surprising, then, that whether labeled child psychology or child development, the study of the early portion of the life span was framed in very similar (i.e., "psychologized") ways by scholars studying children.

At its inception, the child development (or child psychology) field was framed by what Overton (2015a) has explained to be a conception of reality derived from the philosophy of René Descartes, that is, a conception of nature that split it into the fundamental or essential versus the non-fundamental or derived. This Cartesian split conception of reality resulted in split conceptions of change across the human life span (i.e., ontogeny) and involved accounts of the bases of human development that sought to explain change by reducing it to its fundamental, or essential, elements (Lerner, 2016; Overton, 2006, 2010). Cartesian splits came to dominate thinking in the study of human development, as represented by the core conceptual issues that framed the study of development: the nature–nurture, continuity–discontinuity, and stability–instability controversies (Lerner, 2012).

Cartesian, split thinking also framed "solutions" to these debates, involving, for instance, reducing development to being a phenomenon explained by either nature variables (genes or maturation; e.g., Hamburger, 1957) or operantly-conditioned, stimulus–response connections (e.g., Bijou & Baer, 1961). This split, reductionist view of the nature of existence or being (this "ontology") meant that the route to knowledge about (i.e., the "epistemology" of) development was to identify the *essential* (nature *or* nurture) explanatory variable(s) and reduce what one observed to these variables. Accordingly, the study of development was also marked by variable-centered analyses, as exemplified by the tables of contents of the editions of the *Handbook of Child Psychology* published during this period (e.g., Carmichael, 1946, 1954; Mussen, 1970, 1983) as well as by the tables of contents of other major compendiums published during this period (e.g., Reese & Lipsitt, 1970; Stevenson, 1963).

However, as early as 1970, Mussen, the editor of the third edition of the *Handbook of Child Psychology*, pointed to the potential implications of a growing interest among some scientists to move away from a reductionist approach, involving descriptions of the variables purportedly involved in development, and towards an approach that viewed development as involving interrelations among variables (from multiple levels of organization). Mussen said that "the major contemporary empirical and theoretical emphases in the field of developmental psychology . . . seem to be on *explanations* of the psychological changes that occur, the mechanisms and processes accounting for growth and development" (Mussen, 1970, p. vii). By pointing to the interest in change *processes*, Mussen was implying that scientists needed something more to explain the process of development, unless they believed that nature or nurture variables explained themselves!

That "something more" was already emerging within the study of development—at a series of conferences held at the University of West Virginia in the late 1960s and early 1970s. Scholars at these meetings discussed the nature and implications of a life-span view of human development (e.g., Baltes & Schaie, 1974; Nesselroade & Reese, 1973; Schaie, 1970). These West Virginia University conferences, and the edited books that derived from them, laid the foundation for contemporary theories derived from RDS metatheory (see Overton & Molenaar, 2015, for examples). First, the conferences and associated edited books (e.g., Goulet & Baltes, 1970; Nesselroade & Reese, 1973) discussed the philosophical, theoretical, and methodological problems associated with split/reductionist accounts of development. Second, they introduced ideas about the potential for *plasticity* (i.e., the potential for systematic change) in development across life, and pointed to the role of potentially mutually influential relations between individuals and their normative age- and history-related experiences and, as well, non-normative experiences, in instantiating this plasticity. Finally, they underscored the fundamental necessity to study intraindividual changes (and interindividual differences in intraindividual changes) involved in these individual↔context relations in order to describe, explain, and optimize the course of human development.

Paul H. Mussen

These ideas would act synergistically with growing scholarship in Europe that provided theory and data fostering a "reversal" of focus for developmental inquiry—from variable-centered to person-centered approaches to human development (e.g., Magnusson, 1999a, 1999b). These ideas were also synergistic with life-course work in sociology at the time that demonstrated that the course of life was shaped by historical events that one encountered at particular times and in particular places (Elder, 1974, 1980).

The approach to development, as an intraindividual process that was potentially idiographic (individually distinct) across people, was also different from another emphasis in the study of child development during the period wherein Mussen (1970) noted that major conceptual changes were occurring in the field. In addition to being the *primarily* descriptive and normative field that Mussen (1970) described, developmental science at that time had a narrow approach to what was regarded as normative. The norms usually generated by researchers involved studying only a small portion of humanity—in the main, European American middle-class children (Hagen, Paul, Gibb, & Wolters, 1990). In addition, the extant set of methodological approaches were as likely (if not more likely) to use cross-sectional research to study development as they were to employ longitudinal methods.

The use of cross-sectional designs and data-analysis methods (e.g., R-technique analyses; e.g., see Cattell, 1966, and for more current versions of these ideas see Molenaar & Nesselroade, 2015, and Nesselroade & Molenaar, 2010) was predicated on the assumption of the applicability of the *ergodic theorems*, which I shall discuss in more detail later

Todd Rose

in the chapter (e.g., Molenaar, 2007). These mathematical ideas enabled researchers to assume that one could model changes *within* a person across his or her ontogeny by modeling differences *among* people at one point in time. In effect, this approach means that one could assume that an average score across people at one point in time was equivalent to scores that would occur for every person who was studied across time (Rose, 2016). Thus, this ergodic-theorem-based approach essentially ignored the individuality of development. Drawing on the scholarship of Peter C. M. Molenaar, John R. Nesselroade, and Todd Rose, I shall explain later in this chapter some of the flaws in such an argument. As emphasized in Rose's book, *The end of average* (2016), these flaws arise because of the fact that human development is a non-ergodic phenomenon (see too Chapter 13).

When taken together, these ideas point to the vacuity of split/reductionist models (and their attendant methodologies). In turn, this work underscores the importance of time and place, person↔context relations, plasticity, and the need for a focus on longitudinal (change-sensitive) methods to study intraindividual change across life and, as well, the diverse life paths of these intraindividual changes.

The work being generated through the advent of the life-span and life-course perspectives, and the emergence of what are now termed RDS-based theoretical models that were linked (at least initially through the West Virginia University conferences) to these perspectives (e.g., Riegel's, 1975, 1976a, 1976b, dialectic model, Lerner's, 1979, 2004, developmental contextual model, Magnusson's, 1999a, 1999b, individual-context model, and Overton's, 1984, organismic contextual model), coalesced to shift the "paradigm," that is, the set of ideas, about the nature of the world (ontology) and how knowledge of the world can be gained (epistemology), that framed what is considered "normal" or good science. These strands merged in the 1970s, 1980s, and 1990s and created a focus on individual↔context models (Cairns & Cairns, 2006; Lerner, 2002, 2006b). Such models involved the belief that time and place matter in regard to shaping the course of life (e.g., Bronfenbrenner, 2005; Elder, 1998; Elder et al., 2015).

Developmental scientists of the time had neither sufficient conceptual tools nor methodological means to gather and to interrelate variables from the multiple levels of analysis needed to describe the individual↔context relations involved in human development, to explain these relations, or to test one's explanations through optimization efforts. As such, ideas from the several disciplines noted earlier in this chapter were drawn on to create integrative, non-reductionist, and RDS-based models of human development. In addition, methods from numerous fields (e.g., computer science, economics, neuroscience, molecular biology, sociology, and statistics, as well as psychology) were used to study development. A true developmental science had been created (Magnusson & Cairns, 1996).

THE PRESENT

Within developmental science at this writing, RDS-based theories are at the forefront of the field (e.g., Lerner, 2006, 2012, 2015; Overton, 2015a; Overton & Lerner, 2014; Overton & Molenaar, 2015). Indeed, these models of development derive from a new paradigm for developmental science. Instead of the former, Cartesian split, essentialist and reductionist paradigm, a process-relational paradigm emerged (Overton, 2015a). This paradigm framed RDS metatheory and theoretical models derived from the metatheory. Key examples of such models of human development are action theories of human motivation and goal-directed behaviors (e.g., Baltes & Baltes, 1990; Brandtstädter, 1998, 1999, 2006; Freund, Li, & Baltes, 1999; Heckhausen, 1999; Heckhausen, Wrosch, & Schulz, 2010). Other examples of such theories may be found in Volume 1 of the 2006 edition of the *Handbook of Child Psychology* (Lerner, 2006a) and in Volume 1 of the *Handbook of Child Psychology and Developmental Science* (Overton & Molenaar, 2015).

Overton (2011, p. 260) noted that "The relational developmental systems approach has lacked a toolbox of nonlinear analytic methods and, as a consequence, has often been in the unfortunate position of attempting to express nonadditivity effects in an additive context." However, due to the work of methodologists interested in data derived from RDS-based theoretical models, this toolbox is rapidly filling, and in markedly innovative ways (e.g., Molenaar, 2010; Molenaar, Lerner, & Newell, 2014; Molenaar & Nesselroade, 2015; Nesselroade & Molenaar, 2010; Nesselroade & Ram, 2004; Ram & Grimm, 2015; Tolan & Deutsch, 2015; von Eye, Bergman, & Hsieh, 2015). Indeed, systems science methods (e.g., state–space grids, system dynamics, agent-based modeling, and network analysis) and nonlinear models are among the innovative methods now moving to the forefront of developmental analysis (Urban, Osgood, & Mabry, 2011). Accordingly, Overton (2011, p. 260) has struck a note of optimism: "The fact that . . . nonlinear analytic methods have been emerging and are being employed with increasing frequency is refreshing and encouraging. Certainly, the continuing development of nonlinear analytic methods will go a long way to avoiding conceptual confusions."

Use of these methods is enabling developmental scientists to study the links between intraindividual change and the individual and ecological contributions to the diverse developmental trajectories that characterize the human life span (e.g., Baltes, Lindenberger, & Staudinger, 2006; Elder & Shanahan, 2006; Elder et al., 2015). The elucidation of the diversity of human development that has emerged from this research has underscored the substantive importance of diversity in the life course.

Prior to the advent of RDS-based approaches to human development, it was a moot point within the study of the life span whether a concern with diversity was a central or a secondary focus of scientific inquiry. Often, the belief that developmental analysis should be directed to the description of the generic human being led developmental researchers to consider findings about variation in the course of human development to be construed as error variance (e.g., see Nesselroade & Molenaar, 2010; Rose, 2016). However, the emphasis on intraindividual change and on interindividual differences in intraindividual change means that the diversity of human life is of central, indeed of core, concern in developmental science (Rose, 2016; Rose, Rouhani, & Fischer, 2013).

In addition, in rejecting split approaches to ontogenetic analysis, proponents of RDS-based theoretical models believe that the relative plasticity of the individual↔context relations, relations that constitute the fundamental unit of analysis in research framed by these conceptions, creates a synthesis between the explanatory and optimization goals of developmental science (Baltes et al., 1977; Lerner, 2012). To test explanations of developmental change, scholars need to institute or evaluate actions that are aimed at altering the bidirectional relations theoretically expected to produce changes in behavior and development. These actions must necessarily be embedded in the actual ecology of human development in order to have generalizability to the lived experiences of individuals and, as such, they constitute instances of intervention (applied) research and, *at the same time*, research testing basic explanatory processes of human development. As such, in contemporary developmental science any splits between basic and applied research are regarded as anachronistic representations of the reductionist, Cartesian approaches of earlier eras. In short, the application of developmental science (optimization) is a co-equal partner with description and explanation within contemporary developmental science.

One additional feature pertinent to the potential for plasticity and the interest in optimization brought to the fore by RDS-based theories is the promotion of social justice (Fisher et al., 2013; Lerner & Overton, 2008). Arguably, the most arduous test of the integrated explanatory/optimization scholarship conducted by developmental scientists involves efforts to bring to scale changes in the developmental system for diverse individuals. If the explanatory models of developmental science can fully account for the system of individual↔context relations that alter the course of development for all individuals, then means should exist to promote more positive development among all individuals. Accordingly, developmental scientists should be able to address (and, through systematic research, answer) a multi-part question derived from what Bornstein (2017) has termed the specificity principle of developmental science, a principle that derives from the idiographic pathway through life taken by every individual (Rose, 2016). That is, developmental scientists should be able to indicate what specific characteristics, of what specific individuals, should be integrated with what specific features of the ecology of human development, at what specific points across ontogeny, to produce what specific instances of (more optimal) changes in behavior and development (Bornstein, 2017).

Simply, to the extent that developmental scientists have successfully validated their theoretical models of the intraindividual change trajectories linked to specific instantiations of individual↔context relations, their work should be able to be applied to enhance the likelihood that diverse individuals will have better (i.e., healthier, more positive) trajectories. Such work would enable diverse individuals to have enhanced opportunities for thriving (positive development) across their life spans. These enhanced opportunities will contribute, across individuals, to creating a more socially just world. Although developmental science will only move asymptotically towards this knowledge base, current emphases in developmental science (Fisher et al., 2013) stress that scholarship should be directed to identifying the answers to the above-noted set of specificity-principle-based questions *if* scholars are to use their integrated explanatory/optimization work to create a more socially just world for the diversity of individuals in our global community.

This contemporary interest in the connection between basic and applied developmental science that serves social justice may be the harbinger of scholarly emphases that characterize developmental science in the future. At the end of this chapter, I close with some projections about the developmental science that lies beyond the horizon. Here, however, I turn to a more in-depth discussion of the ideas associated with RDS metatheory. Given that, at the time of this writing, key, theory-based contributions to contemporary developmental science are framed by ideas associated with RDS metatheory (Lerner, 2015a; Overton, 2015a, 2015b), and this role of RDS-based ideas *may* extend into the future of the field as well (Lerner, 2015b, 2015c; Lerner et al., 2014), it is important to appreciate the concepts and implications of this approach to developmental theory.

THE RELATIONAL DEVELOPMENTAL SYSTEMS (RDS) METATHEORY

Due to the contributions of Willis F. Overton (e.g., 2015a; Overton & Müller, 2013) and others (e.g., Gottlieb, 1997, 1998, 2004), the sun has set on split, reductionist accounts of development, for example, involving nature and nurture. From the late 1960s through this writing, the study of human development evolved from a field dominated by split, reductionist (psychogenic or biogenic) approaches to a multidisciplinary (and, in regard to aspirations of many developmental scientists, an interdisciplinary) scholarly domain that seeks to integrate variables from biological through cultural and historical levels of organization across the life span into a synthetic, coactional system (e.g., Elder, 1998; Elder & Shanahan, 2006; Elder et al., 2015; Ford & Lerner, 1992; Gottlieb, 1997, 1998; Lerner, 2012). Prior reductionist accounts of development that adhered to a Cartesian dualism pulled apart (split) facets of the integrated developmental system (Overton, 2015a). These split approaches are rejected by proponents of theories derived from an RDS metatheory (e.g., Mistry & Dutta, 2015) which, in turn, are derived from a process-relational paradigm (Overton, 2015a).

Willis F. Overton

A classic instance of such reductionist, split conceptions involves the trait approach to understanding human development, as exemplified by the Five Factor Theory (FFT) championed by Costa, McCrae and their colleagues for more than four decades (e.g., Costa & McCrae, 1980, 2006; McCrae et al., 2000). The purported "Big Five" personality traits (conscientiousness, agreeableness, neuroticism, openness to experience, and extraversion) that form the FFT are still very much in use in the literature of personality and social psychology; in fact, the Big Five remains the predominant model within these fields (John & Naumann, 2010; Roberts, Walton, & Viechtbauer, 2006).

The Big Five traits are held to be fixed, stable, and biologically-set fundamental facets of individual functioning. For instance, McCrae et al. (2000, pp. 175–176) believe that personality traits reflect "nature over nurture" and that, "personality traits are more or less immune to environmental influences . . . significant variations in life experiences have little or no effect on measured personality traits." They argue that, "Barring interventions or catastrophic events, personality traits appear to be essentially fixed after age 30" (Costa, McCrae, & Siegler, 1999, p. 130). Costa and McCrae (2006) continue to maintain this view, despite the fact that a meta-analysis has provided strong evidence that personality traits change in adulthood past the age of 30 (Roberts et al., 2006).

Split approaches, such as the nature-based, developmentally-fixed conception of trait forwarded within the FFT, are rejected by proponents of theories linked to the RDS metatheory developed from the process-relational paradigm (Lerner & Callina, 2014a; Mistry & Dutta, 2015; Mistry & Wu, 2010; Overton, 2015a; Overton & Lerner, 2014). Across the past four-plus decades, several scholars have provided ideas contributing to the evolution of this paradigm (e.g., Baltes, 1997; Baltes, Lindenberger, & Staudinger, 2006; Brandtstädter, 2006; Bronfenbrenner, 1979, 2005; Bronfenbrenner & Morris, 2006; Elder, 1998; Elder et al., 2015; Ford & Lerner, 1992; Nesselroade, 1988; Overton, 1973, 2015a; Overton & Reese, 1981; Riegel, 1975, 1976a, 1976b; and, even earlier, see von Bertalanffy, 1933). However, the work of Overton (e.g., 2013, 2015a) has been the major scholarly force integrating and extending this line of thinking.

Overton (2015a) explains that, compared to a Cartesian world view, the process-relational paradigm focuses on process (systematic changes in the developmental system), becoming (moving from potential to actuality; a developmental process as having a past, present, and future; Whitehead, 1929/1978), holism (the meanings of entities and events derive from the context in which they are embedded), relational analysis (assessment of the mutually influential relations within the developmental system), and the use of multiple perspectives and explanatory forms (employment of ideas from multiple theory-based models of change within and of the developmental system) in understanding human development. Within the process-relational paradigm, the organism is seen as inherently active, self-creating (autopoietic), self-organizing, self-regulating (agentic), nonlinear/complex, and adaptive (Overton, 2015a; see too Sokol, Hammond, Kuebli, & Sweetman, 2015).

The process-relational paradigm results in RDS metatheory, which eschews split conceptions in favor of ideas that emphasize the study and integration of different levels of organization, ranging from biology/physiology to culture and history, as a means to understand life-span human development (Lerner, 2006b; Overton, 2013, 2015a). Accordingly, the conceptual emphasis in RDS-based theories is placed on mutually influential relations between individuals and contexts, or individual↔context relations.

This representation of the coactions between person and setting within RDS-based models is not meant to convey a person–context interaction (which is typically represented in the developmental literature as person x context). An interaction connotes that the entities involved in the relation are separate and independent (as in a statistical interaction) and that, as such, their association involves a linear combination of discrete and separate variables. Both before and after the interaction these entities (variables) are independent and unchanged by each other. The bidirectional arrow used in the RDS conception of person↔context relations is intended to emphasize that the coaction of individual and context involves the entire developmental system. As such, the relations among levels of the autopoietic system, and not independent linear combinatorial attributes, are the focus in such a model. Indeed, the fusion of individual and context within the developmental system means that any portion of the system is inextricably embedded with—or embodied by, in Overton's (2013, 2015a) conceptualization—all other portions of the developmental system. Embodiment refers to the way individuals behave, experience, and live in the world by their being active agents with particular kinds of bodies; the body is integratively understood as form (a biological referent), as lived experience (a psychological referent), and as an entity in active engagement with the world (a sociocultural referent) (Overton, 2015a).

Of course, a simple bidirectional arrow is a less than ideal figural representation of these system

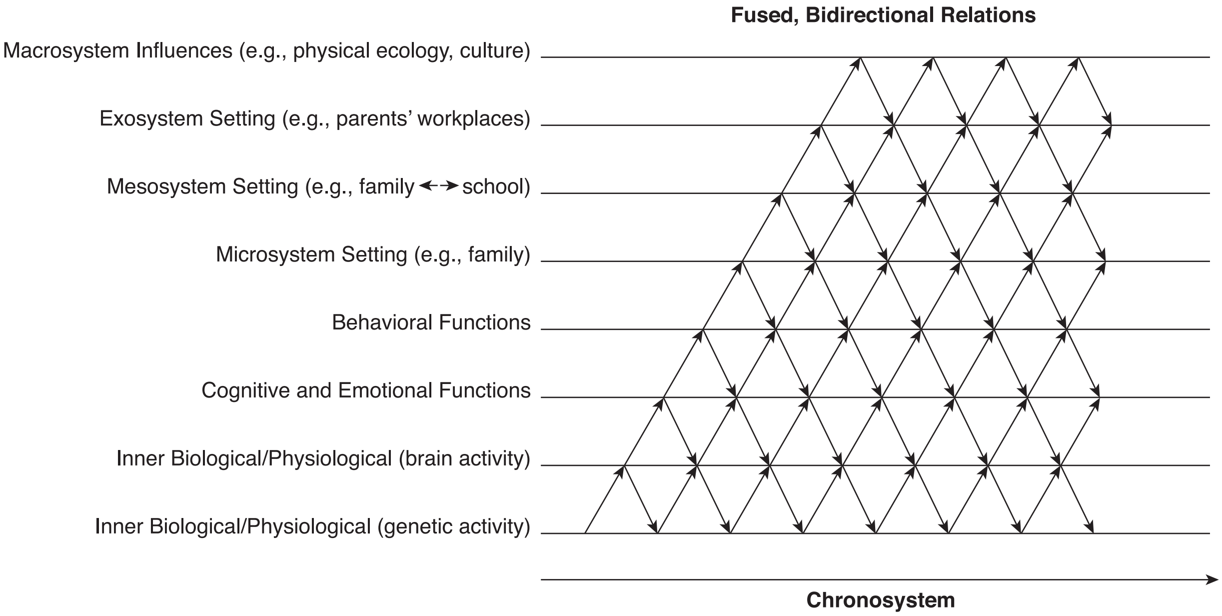


Figure 2.1 A relational developmental systems-based model of the fused relations among the levels of organization in the ecology of human development: Only a subset of relations (involving adjacent levels) are illustrated.

Source: Inspired by Gottlieb (e.g., 1992, 1997, 1998), Bronfenbrenner (e.g., 1979, 2005), and Lerner (2002, 2004).

relations. The well-known figure used by Gottlieb in his 1992 book, *Individual Development and Evolution: The Genesis of Novel Behavior* (p. 186), is a better figural representation of the integrated, multilevel relations I describe. Lerner, Johnson, and Buckingham (2015a) constructed a version of Gottlieb's (1992, 1998) figure that more fully reflects the RDS-based ideas about the bidirectional influences between the environment (physical, social, and cultural), behavior, neural activity, and genetic activity, and depicts these influences coacting across individual development. A version of this figure is presented in Figure 2.1.

The use of a representation such as the one in Figure 2.1 to depict the relational developmental system within textual material is not efficient, however. Therefore I use the symbol ↔ to evoke such multilevel, bidirectional relations. These relations occur across ontogenetic, family, and historical time and place (Elder et al., 2015), and do not connote interaction but rather coaction.

Within the context of such a bidirectional relational system, the embeddedness within history (temporality) is of fundamental significance (Elder, 1998; Elder et al., 2015). It is important to note that the developmental system is embedded in history. This embeddedness means that change is constant in the developmental system and that, as such, there may be either stochastic (or random) or systematic changes in person↔context relations *across time and place* (Elder, 1998; Elder et al., 2015; Misteli, 2013). The presence of such temporality in the developmental system means that there always exists some potential for systematic change and, thus, for (relative) plasticity in human development. In short, potential plasticity in individual↔context relations derives from the "arrow of time" (Lerner, 1984; Lerner & Benson, 2013a, 2013b; Overton, 2015a) running through the integrated (relational) developmental system.

THREE MOMENTS OF ANALYSIS IN THE RDS APPROACH TO DEVELOPMENTAL SCIENCE

To understand the role of time and place in contributing to the bidirectional relations of focal concern within RDS metatheory, developmental scientists may focus on either the role of the individual and/or the context in particular instantiations of individual↔context exchanges. This either/or focus may seem contradictory to the fusion among levels of organization emphasized in this approach. However, as I noted earlier, Overton (2015a) embeds the RDS metatheory in the process-relational paradigm (see, as well, Sokol et al., 2015). Overton uses this paradigm to explain the possibility of this changing focus in developmental analysis. He notes that the process-relational paradigm involves different moments within a research program.

One moment involves the idea of *the identity of opposites*, a second moment involves *the opposites of identity*, and a third (relationally integrative) moment involves *the synthesis of wholes*. In discussing these three moments of scientific analysis within RDS approaches to developmental science, I point to the predominant trait model of individuality, the FFT (Costa & McCrae, 1980, 2006; McCrae et al., 2000), as a means to explain these RDS-based moments and to contrast their use with thinking associated with Cartesian, split, reductionist approaches to the study of individual development.

The first moment recognizes that both individual and context define—and are mutually constituted by—each other in one moment, or point, in programmatic developmental inquiry. That is, Overton (2010, p. 14) notes that:

> The principle of the identity of opposites establishes the identity among parts of a whole by casting them not as exclusive contradictions as in the split epistemology, but as differentiated polarities (i.e., coequals) of a unified (i.e., indissociable) inclusive matrix; as a relation. As differentiations, each pole defines and is defined by its opposite.

The identity of opposites, therefore, emphasizes the fused person↔context relation as the primary unit of analysis for understanding development. As such, in this moment of research developmental scientists would reject the idea that there are any aspects of human development—for instance, entities such as traits—that "are more or less immune to environmental influences" (McCrae et al., 2000,

p. 175); the idea that such entities are indicators of split notions reflecting "nature over nurture" (McCrae et al., 2000, p. 173) would also be rejected by developmental scientists working within this first moment of analysis.

The second moment that Overton (2010, 2013, 2015a; Overton & Müller, 2013) discusses is the opposites of identity. This moment allows one, in effect, to hold the other parts of the integrated system in abeyance, and focus on one part of the system; however, the ultimate aim is one of reintegrating the part into the whole at a subsequent moment. Overton (2013, pp. 47–48) explains that:

> The limitation of the identity moment of analysis is that, in establishing a flow of categories of one into the other, a stable base for inquiry that was provided by bedrock material *atoms* of the split metatheory is eliminated . . . Reestablishing a *stable base*—not an absolute fixity, nor an absolute relativity, but a relative relativity (Latour, 1993)—within relational metatheory requires moving to a second moment of analysis. In this moment of opposition, the law of contradiction is reasserted and categories again exclude each other. As a consequence of this exclusion, parts exhibit *unique* identities that differentiate each from the other.

Therefore, when functioning within this second moment of analysis, developmental scientists could focus solely on attributes of individuals, for instance, the purported traits of conscientiousness, agreeableness, neuroticism, openness to experience, and extraversion that comprise the Big Five components of the FFT (Costa & McCrae, 1980, 2006; McCrae et al., 2000), and, for instance, study the psychometric properties of these constructs to provide "objective," or quantitative, indices of these attributes. Such psychometric work has often been a part of research programs framed by RDS models (e.g., Damon, 2008; Geldhof et al., 2014a, 2014b; Lerner et al., 2015). Indeed, working within this second moment of analysis, developmental scientists following an RDS-based model, and social or personality researchers using a Cartesian, split model, would be engaging in commensurate work. However, the difference between these two groups of scholars is brought to the fore when the third moment of analysis discussed by Overton (2013, 2015a) is considered.

The third moment, the synthesis of wholes, occurs when the first two moments are embedded in a multi-perspective, process-relational paradigm and are recognized as mutually necessary in a systematic, integrative program of research, one wherein both of the first two moments are needed. That is, "A complete relational program requires principles according to which the individual identity of each concept of a formerly dichotomous pair is maintained while simultaneously it is affirmed that each concept constitutes, and is constituted by the other" (Overton & Müller, 2013, p. 35).

Accordingly, the developmental scientist working within an RDS model would use an "objective" measure studied within the second moment of analysis within an integrated, relational empirical approach that focused on the individual↔context relation. Clearly, the trait theorist would not take such a step, given that the work of such scholars is framed by the idea, noted above, that context is irrelevant to the understanding (read, successful prediction) of the life-course manifestation of traits. Indeed, in their belief "that personality traits are more or less immune to environmental influences" (McCrae et al., 2000, p. 175), trait theorists maintain that contextual conditions, whether similar or not, are irrelevant to prediction; given the purported biological base of traits, only nature variables have predictive efficacy. In contrast to RDS-based models, such as the life-course model of Elder (1998) and Elder and Shanahan (2006; Elder et al., 2015; see too Chapter 10), where time and place are essential (embodied) parts of the dynamic, relational developmental system, to trait theorists following FFT, time and place are irrelevant!

However, there is abundant evidence that purported traits are in fact not "trait-like" at all, that is, these attributes reflect relations between individuals and contexts, as they occur at particular times and places (Ardelt, 2000; Block, 1995, 2010; Elder, 1998; Elder et al., 2015; Roberts et al., 2006). Indeed, methodological work framed by RDS concepts (e.g., Molenaar & Nesselroade, 2014, 2015; Nesselroade, 1988; Nesselroade & Molenaar, 2010; Rose, 2016; Rose et al., 2013) indicates that the purported life-span stability of traits, as well as the purported immunity to contextual influences, are empirically counterfactual. Moreover, and underscoring the importance of this third moment of analysis discussed by Overton (2015a), these methodological innovations demonstrate the ability to index with psychometric precision integrative, individual↔context (including individual↔individual) units of analysis (e.g., Molenaar, 2014; Molenaar, Lerner, & Newell, 2014; Molenaar & Nesselroade, 2015).

DEVELOPMENTAL REGULATIONS, ADAPTIVE DEVELOPMENTAL REGULATIONS, AND HUMAN AGENCY IN RDS METATHEORY

Given the analytic moment of the identity of opposites—that each component of the developmental system constitutes and is constituted by the other components of the system—RDS metatheory focuses on the "rules" or processes which govern, or regulate, exchanges between individuals and their contexts. Such processes are the *function* of the developmental system. An RDS program of research might seek to understand *the nature of relations* between individuals and their contexts, including the dynamics of those relations across the life course. For instance, RDS-based research might ask how specific features of the individual and specific features of the context coalesce to influence the substantive course of individual↔individual relations.

Brandtstädter (1998) termed these bidirectional relations "developmental regulations," and noted that, when developmental regulations involve mutually beneficial individual↔context relations, then these developmental regulations are *adaptive*. Developmental regulations are the fundamental feature of human life; that is, all human life exists in a context and involves bidirectional exchanges with it (Darwin, 1859; Schneirla, 1965; Tobach & Schneirla, 1968). These exchanges involve physiological systems and functions (e.g., respiration, circulation, digestion, reproduction) and behaviors (e.g., social affiliation and cooperation, as might be involved in protection, hunting, and scavenging; Johanson & Edey, 1981), and involve both organismic self-regulation (e.g., hypothalamic functioning, circadian rhythms) and intentional self-regulation (e.g., goal selection, resource recruitment, and executive functioning; Gestsdóttir & Lerner, 2008).

Consistent with these RDS-based ideas about adaptive developmental regulation, comparative psychologist T. C. Schneirla (1965, pp. 351–352) "envisages behavioral development as a program of progressive, changing relationships between organism and environment in which the contributions of growth are always inseparably interrelated with those of the effects of energy changes in the environment . . . This formulation corresponds to a fundamental concept of modern embryology . . . according to which organism and developmental medium are inseparably related" (see too Chapter 7). Moreover, consistent with my earlier point about needing to go beyond the concept of interaction to depict the coactions within the relational developmental system, Schneirla (1965, p. 352) also notes that, in seeking to understand the relations between organism and ecological processes involved in these adaptive developmental regulations, genetic reductionists "typically underestimate the subtlety, indirectness and variety of relationships prevalent in development between the complexes denoted by the terms 'maturation' and 'experience,' which are not simply interrelated but constitute a *fused* system in each stage . . . This theory, then, is much more than 'interactionistic.'"

Similarly, Gestsdóttir and Lerner (2008) note that self-regulation is a multidimensional construct, involving a range of behaviors, from basic physiological functions to complex intentional cognitive processes (e.g., Bandura, 2001; Brandtstädter, 1998; McClelland, Geldhof, Cameron, & Wanless, 2015). As such, self-regulation pertains to all aspects of adaption, as people alter their thoughts, attention, emotions, and behaviors to react to contextual events and, as well, to influence selected features of the context. Here, culture plays a key moderating role (e.g., Cole, 2006; Mistry & Dutta, 2015; Rogoff, 1998, 2003, 2011). Trommsdorff (2012) notes that self-regulation "is assumed to develop by organizing inner mental processes and behavior in line with cultural values, social expectations, internalized standards, and one's self-construal" (p. 19).

The developmental course of self-regulation is, in effect, the developmental course of human agency in the context of individual desires, purposes, needs, goals and identity, other people, the physical ecology, and culture (e.g., Damon, 2008; Geldhof, Little, & Columbo, 2010; Gestsdóttir & Lerner, 2008; Lerner, Freund, DeStefanis, & Habermas, 2001). *Agency* is a defining feature of the active, self-creating (autopoietic, enactive), and nonlinear adaptive living system (Overton, 2015a; Overton & Lerner, 2014; Narvaez, 2008; Witherington, 2014). Such agency is the individual's contribution to adaptive developmental regulations (Brandtstädter, 1998, 1999). The development of agency begins in early life, primarily with organismic self-regulation processes. By the time of adolescence, self-regulation is increasingly intentional and purposeful (Damon, 2008), and involves the self system and the phenomena associated with identity development (Gestsdóttir & Lerner, 2008; Lerner et al., 2001).

However, whereas all adaptive developmental regulations must involve individual↔context relations, not all exchanges between an individual and context are adaptive developmental regulations; not all developmental regulations maintain (sustain) and enhance *both* individual and context. When such maintenance and enhancement exist, then a person is contributing (through his/her agency, his/her self-regulatory processes) to a context that supports him or her (Lerner, 2004).

It is important to understand what may make specific developmental regulations adaptive. One needs both conceptual and empirical criteria to define this concept within an RDS-based model. Conceptually, developmental regulations are adaptive when, and only when, they are beneficial to the maintenance of positive, healthy functioning of the components of a bidirectional relation (e.g., both individual and context). Whereas all organisms manifest such adaptive developmental regulations (Tobach & Schneirla, 1968), scholarship from both comparative psychology and evolutionary biology converges in indicating that, in humans, *individual↔individual* adaptive developmental regulations embedded within culture have a privileged position among the possible set of adaptive developmental regulations (Lerner & Callina, 2014a; Nucci, 2017; Rogoff, 1998, 2003, 2011). These relational adaptive regulations are—as Bronfenbrenner (2005) argued—what makes human beings human.

Comparative psychologists Ethel Tobach and T. C. Schneirla (1968) distinguished between the biosocial functioning of insects (e.g., ants) and the psychosocial functioning of organisms with higher psychological levels, levels that are marked by greater plasticity, rather than stereotypy, in their eventual highest levels of ontogenetic change. Such higher levels provide the physiological base for symbolic functioning. In turn, evolutionary biologists Jablonka and Lamb (2005) note that both psychological processes and cultural processes are integrated with the genetic and epigenetic processes of evolution to make human adaptiveness and contributions qualitatively different than corresponding instances of adaptiveness of social contributions among other organisms. Together these comparative psychology and evolutionary biology literatures suggest that, among humans, adaptiveness and positive contributions reflect integrated cognitive, emotional, and behavioral processes that involve abstract, symbolic constructs (see too Chapter 9).

Empirically, indexing such adaptive attributes may involve both point-in-time (cross-sectional) assessments and historical (longitudinal) assessments (Lerner, 2004). Scientists should conduct such assessments within the context of recognizing that contexts are complex (e.g., they exist at multiple levels of organization; Bronfenbrenner, 1979; Bronfenbrenner & Morris, 2006). Individuals cannot necessarily act in ways that benefit all levels and all components of the context at all times and places (Elder, 1998). Thus, adaption should not be treated as a categorical concept (as something that either exists or not) but, instead, as a multivariate concept comprised of ordinal or interval dimensions. Researchers studying adaption would not ask, then, whether adaption exists or not; rather, the question would be how beneficial is the developmental regulation (the individual↔context relation) for specific people or specific social institutions of the context, at specific times and in specific places (e.g., see Bornstein, 2017). Again, then, the use of asking questions associated with the specificity principle is illustrated.

For instance, researchers may wish to evaluate the benefits of the values (or moral virtues; Berkowitz, 2012; Lerner & Callina, 2014a) of a person. Here again both cross-sectional and longitudinal empirical assessments may be needed. A person (e.g., someone protesting an unjust law) may act in ways that provide no immediate benefit to one facet of the context (the institutions having the power to maintain the unjust law), but his/her behaviors may provide both immediate benefit to other facets of the context (e.g., promoting hope in marginalized social groups that the unjust law can be changed) and, in turn, can have historical benefit by altering society in regard to the removal of the unjust law (Lerner & Callina, 2014a).

The contributions of Mahatma Gandhi or Martin Luther King, Jr. are cases in point. Given that some facets of the context may have regarded the actions of these individuals as positive and valuable while others may have had diametrically opposed views of these actions, the role of values and contextual specificity are clearly underscored in discussions of adaptive developmental exchanges (see Berkowitz, 2012). As well, given that historical changes may need to occur before the full benefits of individual actions become apparent (e.g., Martin Luther King, Jr. never lived to see his dream fulfilled), the concept of adaptive developmental regulations must perhaps be best understood as a "shades of gray" term (cf. Berkowitz, 2012).

As illustrated by the variation across time and place that may be associated with the identification of adaptive developmental regulations within the RDS metatheory, the integration of different levels of organization frames understanding of life-span human development (Lerner, 2006b; Overton, 2013, 2015a). Accordingly, the conceptual emphasis in RDS-based theories that is placed on individual↔context relations varying across place and across time (Elder et al., 2015) means that the "arrow of time," temporality or history, is the broadest level within the ecology of human development. History imbues all other levels with change. Such change may be stochastic (e.g., non-normative life or historical events; Baltes, Lindenberger, & Staudinger, 2006) or systematic, and the potential for systematic change constitutes a potential for (at least relative) plasticity across the life span.

Developmental scientists may focus on either the role of the individual and/or the context in seeking to understand particular instantiations of individual↔context exchanges. Overton (2015a) explained that, in the process-relational paradigm, this changing focus in developmental analysis involves the different moments of analysis within a research program that I discussed earlier in this chapter: the identity of opposites, the opposites of identity, and the synthesis of wholes.

In sum, embedded within a process-relational paradigm, models derived from RDS metatheory emphasize that all levels of organization within the ecology of human development are systemically integrated across life (see Noble, 2015). As such, any variable from any level is *embodied* in, fused with, variables from all other levels; the structure and function of one variable is thus governed, or regulated, by the structure and function of other variables and, for the developing person, these developmental regulations mean that individual↔context relations are the basic unit of analysis within human development. Moreover, history (temporality) imbues individual↔context relations with the potential for relative plasticity in human development.

Plasticity is always a relative phenomenon within the relational developmental system because the temporal events in the life or lives of an individual or a group, respectively, may also constrain change as well as provide *affordances* for it, that is, provide opportunities to act in ways (with people, objects, or within contexts) that may promote systematic change (Lerner, 1984). A system that promotes change can also function to diminish it. Nevertheless, because of relative plasticity across the life span, developmental scientists may be optimistic that instances of these relations may be found or created to promote more positive human development among all people (see Chapter 13).

Developmental scientists may also be optimistic about the possibility of promoting social justice by providing opportunities for all individuals to optimize their chances of positive, healthy development (Lerner & Overton, 2008). Instantiation of such promotion and optimization efforts rests on the conduct of multidisciplinary research, the use of change-sensitive methodologies, and the translation of research into policies or programs (see Chapter 13).

Contemporary developmental science is marked by such scholarship within and across several substantive and methodological areas framing the field. In addition, I have noted that the field involves burgeoning attention to the interrelated areas of evolutionary biology and of epigenetics; to methodological innovations appropriate to understand the fundamental idiographic nature of human development; and to the nature of systems change. There is, as well, a growth in attention to the use of econometric methods in testing causal statements about the bases of change. These methods, especially when added to a developmental science "methodological toolbox" (Overton, 2014) that includes qualitative and mixed-methods approaches (Tolan & Deutsch, 2015), have important implications for assessing causality. As I describe in the succeeding sections of this chapter, the coalescing of these substantive and methodological areas has important, indeed profound, implications for the application of developmental science.

EVOLUTIONARY BIOLOGY AND EPIGENETICS

According to the concept of embodiment associated with the RDS metatheory, biological, psychological, and behavioral attributes of the person, in fusion with culture, have a temporal (historical) parameter (Overton, 2013, 2015a; see too Raeff, 2016). As such, embodiment—the fusion among all variables and all levels of organization within the relational developmental system—has implications across both ontogeny (the life span of a species) and phylogeny (the evolutionary history of species) (e.g., Bateson, 2015, 2016; Ho, 2010, 2013, 2014; Ho & Saunders, 1979; Jablonka & Lamb, 2005; Noble, 2015; Saunders, 2014).

It is important to note, however, that the view I am espousing, that development (ontogeny) contributes to evolution (phylogeny) is controversial. This controversy exists both traditionally among biologists (at least since the middle decades of the twentieth century; Mayr & Provine, 1980) and, at this writing, among some psychologists who take an essentialist, genetic reductionist view of development (e.g., Bjorklund, 2015, 2016; Bjorklund & Ellis, 2005; Del Giudice & Ellis, 2016).

In the middle decades of the twentieth century, biologists such as Ernst Mayr, Julian Huxley, and Theodosius Dobzhansky created what was termed "the modern synthesis," which linked Charles Darwin's ideas about evolution with Gregor Mendel's ideas pertinent to genes. As explained by comparative psychologist Robert Lickliter (2016), this synthesis created a definition of evolution as "*a change in the genetic composition of populations*" (p. 3) and, as such, assigned no role to development across life in regard to providing a basis of the observed attributes of individuals (phenotypes). The foundational, essential basis of phenotypes was genes.

Lickliter (2016, p. 3) indicates that this genetic reductionist view was predicated on these assumptions:

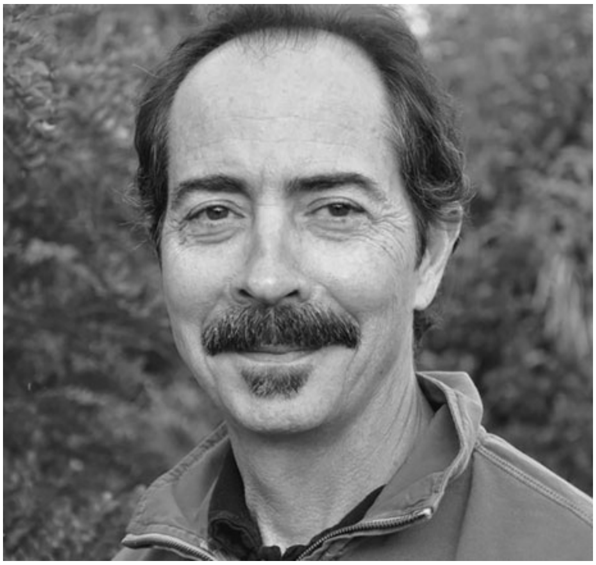
1. Instructions for building organisms reside in genes.
2. Genes are the exclusive means by which these instructions are faithfully transmitted from one generation to the next.
3. There is no meaningful feedback from the environment or the experience of the organism to its genes.

Moreover, Lickliter (2016, p. 3) goes on to note that:

> If genes contain all the necessary information for phenotypes and if events and experiences during individual development could not directly influence the phenotypic traits of offspring, then internal factors (genes) clearly had to have priority over external factors when attempting to explain both development and evolution.

As I will explain in more detail in subsequent chapters, this restricted view of the role of development in contributing to the appearance across life of human structural and functional attributes is found also in the views of contemporary psychologists forwarding genetic reductionist models, such as evolutionary developmental psychology (EDP) (e.g., Bjorklund, 2015, 2016; Bjorklund & Ellis, 2005; Del Giudice & Ellis, 2016; and see Lerner, 2016, and Witherington & Lickliter, 2016, for critiques of EDP ideas).

However, contemporary biological research that is consonant with RDS metatheory has called into question the diminished view of the importance



Robert Lickliter

David Witherington

of development held by proponents of both the modern synthesis and conceptions such as EDP. Simply, in contemporary evolutionary biology (e.g., Bateson, 2015, 2016; Bateson & Gluckman, 2011; Gissis & Jablonka, 2011; Jablonka & Lamb, 2005; Noble, 2015; West-Eberhard, 2003; Woese, 2004) the relational developmental system, involving coactions across all levels of organization involving the organism and its context, is the source of the structure and function of the individual. In addition, Lickliter (2016) explains that development not only influences the features of the individual that emerge across ontogeny. As well, development influences evolution (phylogeny). He notes that:

> development contributes to evolution in two important ways: (1) It generates the reliable reproduction of phenotypes across generations and (2) it introduces phenotypic variations and novelties of potential evolutionary significance . . . the regulatory function of development in evolution . . . collectively serve[s] to restrict the 'range of the possible' in phenotypic form and function . . . [and] the generative function of development, has significant implications for understanding the mechanisms of evolutionary change. In particular, the generative function of development provides a source of phenotypic variation upon which natural selection can act. Simply put, evolutionary novelties largely originate in the process of development.
>
> (Lickliter, 2016, p. 2)

Moreover, Lickliter (2016) notes that there is convincing evidence, derived from research on species as diverse as insects, fish, monkeys, and humans, that the development of all physical and behavioral attributes derives from coactions among genes, cells, and the physical and social contexts of the ecology of the organism. This evidence, Lickliter (2016, p. 2) concludes, "indicates that genes are not the only source of inheritance across generations."

Jablonka and Lamb (2005) agree. They presented evidence demonstrating that human evolution involves four interrelated dimensions: genes, epigenetics, behavior, and culture. They explained that contemporary research in molecular biology indicates clearly that neo-Darwinian assumptions about the role of genes in evolution (i.e., that genes are the unit of analyses and, as well, the driver of evolution) are mistaken. This research demonstrates that cells can transmit information to daughter cells through non-DNA, *epigenetic* means. Therefore, genetic and epigenetic processes constitute two dimensions of evolution. In addition, animals can transmit information across generations through their behavior, which constitutes a third dimension of evolution. A fourth dimension of evolution is constituted by culture, in that humans "inherit" from their parents symbols and, in particular, language. As such, Jablonka and Lamb (2005) concluded that "It is therefore quite wrong to think about heredity and evolution solely in terms of the genetic system. Epigenetic, behavioral, and symbolic inheritance also provide variation on which natural selection can act" (p. 1).

Thus, to Jablonka and Lamb (2005), as well as Lickliter (2016), there is a bidirectional relation between development and evolution—there is an evolution↔development relation, one involving the time and place within which development happens (Elder et al., 2015) and the epigenetic changes that involve the individual's biology and his or her behavioral, social, and cultural context changing across history.

EPIGENETICS: AN OVERVIEW

Deoxyribonucleic acid (DNA) is found within the nucleus of the cell, and is organized into units termed nucleosomes (Meaney, 2010). Each nucleosome is composed of about 150 pairs of amino acids—adenine is paired with thymine and guanine is paired with cytosine. These pairs of chemicals are the bases of DNA. The base pairs of each nucleosome are wrapped around another chemical structure—histone—and DNA and the histones, together, are termed chromatin (Meaney, 2010, p. 48).

Slavich and Cole (2013; see too Cole, 2014) note that DNA does not provide an absolute set of instructions for any human characteristic, either physical, psychological, or social. The *only* role of DNA in human development is to code for ribonucleic acid (RNA); that is, DNA must be transcribed into RNA which, in turn, then is involved in other chemical reactions from which specific human characteristics develop (Meaney, 2010, 2014; Moore, 2015a, 2016; Slavich & Cole, 2013). In other words, DNA does not produce—it does not represent a code for—any human psychological or social characteristic, such as intelligence, personality, character, or social relationships. Simply, there are no such things as genes "for" intelligence, depression, virtuous or antisocial behaviors, athletic ability, or any of the myriad attributes defining an individual's repertoire of characteristics (Meaney, 2010; Moore, 2015a, 2016). DNA contributes to the process of development only through being transcribed into RNA but, quite critically, this contribution involves coactions between DNA and the environment (the context) both within the cell and the context outside the cell, including the social world of the individual (Slavich & Cole, 2013).

The process of transcribing DNA into RNA involves the actions of many other chemicals (proteins), often more than 100 (Harper, 2005, p. 344). The activity of these other proteins involves epigenetics. Two instances of these other chemicals have been studied the most—chemicals termed *acetyl groups* and *methyl groups* (Meaney, 2010; Moore, 2015a, 2016)—and these chemicals may be used to illustrate epigenetic processes. Transcription of DNA into what is termed messenger RNA (mRNA) may involve either the process of acetylation or the process of methylation (Harper, 2005; Meaney, 2010; Moore, 2015a, 2016). For instance, DNA methylation involves a methyl group being added to a cytosine base (cytosine is one of the four bases of the DNA molecule—along with guanine, adenine, and thymine), and generally results in stable gene silencing (in no transcription). In turn, acetylation results in gene transcription.

Acetylation and methylation are components of the process of epigenetics. Epigenetics may be defined as "mitotically and meiotically heritable changes in gene expression that cannot be explained by changes in DNA sequence" (Lester et al., 2016, p. 29). Or, less formally, "epigenetics refers to how genetic material is . . . expressed in different contexts or situations" (Moore, 2015a, p. 14). Thus, epigenetics constitutes "changes in phenotype or gene expression brought about by processes other than changes in the underlying DNA sequence"

(Lester et al., 2016, p. 29). These modifications in gene expression occur through two main processes: histone modification and DNA methylation (Lester et al., 2016). DNA methylation is the most studied process in regard to human behavior and development. DNA methylation is more stable—extending even across generations—than histone modifications (e.g., Lester et al., 2016; Meaney, 2010; Moore, 2015a).

As I have noted, when acetyl groups link with one of the four base chemicals comprising DNA, that is, cytosine, this acetylation process allows DNA transcription but, when methyl groups are linked to cytosine, then this methylation process allows no transcription of DNA into mRNA. In short, acetylation processes allow DNA to be transcribed into mRNA (and therefore to play a role in producing proteins) and methylation processes silence DNA transcription.

If DNA is not transcribed into mRNA, then this DNA cannot play a role in the production of proteins for use by the cell. Because this silencing of gene transcription can persist (can remain stable) across generations (Meaney, 2010; Misteli, 2013; Moore, 2015a, 2016; Roth, 2012; Slavich & Cole, 2013), epigenetic influences constitute heritable changes caused by processes other than by DNA. Indeed, Gissis and Jablonka (2011) edited a book discussing the transformations of Lamarckian theory (basically, the idea that characteristics acquired over the life span of one organism can be transmitted to offspring) that have arisen in relation to the increasingly more active focus on epigenetic processes in the study of both evolution and development (Meaney, 2010; Moore, 2015a, 2016). Gissis and Jablonka noted that a form of inheritance of acquired characteristics does exist in the form of epigenetic inheritance systems.

This system of epigenetic effects involves chemicals within the cell, within the internal milieu of the body, and within the external ecology within which the body is embedded (Cole, 2014; Misteli, 2013; Roth, 2012; Slavich & Cole, 2013) or embodied, in the terms used by Overton (2015a). For instance, Roth (2012) noted that the genome of infants is modified by epigenetic changes involving experiential and environmental variables. She explained that parental stress, infant separation, or caregiver nurturance or maltreatment can alter methylation patterns that affect neurobiology and behavior across the life span. Similarly, Slavich and Cole (2013) discussed evidence that changes in the expression of hundreds of genes occur as a function of the physical and social environments inhabited by humans, and they noted that "external social conditions, especially our subjective perceptions of these conditions, can influence our most basic internal biological processes—namely, the expression of our genes" (p. 331)—a view that highlights the implications of embodied biological changes as a focus of actions aimed at enhancing positive human development or social justice.

Data presented in a 2016 special section of the journal *Child Development* indicate that "some behaviors may be affected by only slight changes in DNA methylation, while others may require a larger percent change in methylation; of course, the effects are also likely bidirectional, with behavior impacting changes in methylation" (Lester et al., 2016, p. 31). *This point is key*. It underscores the absurdity of genetic reductionist models (a point to which I shall return in this chapter and, as well, across this book): *Genes do not determine behavior!*

"Epigenetics controls the . . . [expression] of the gene or how genes function" (Lester et al., 2016, p. 30). Further, just as methylation influences behaviors, the bidirectionality to which Lester et al. (2016) point means that behaviors influence methylation (see too Cole, 2014; Slavich & Cole, 2013). In short, behavior↔methylation relations, and not a gene acting as if it were the command center for human behavior and development, constitute the basic role of biology across the course of development (Noble, 2015). As Lester et al. (2016, p. 31) emphasize, "Epigenetic . . . [processes] control *how* the gene is expressed. This is the fundamental importance of epigenetics."

In short, then, epigenetics is "nature and nurture" (Lester et al., 2016, p. 36). That is, with the recent advances in understanding the role of epigenetics and recent research findings supporting this role, it should no longer be possible for any scientist to undertake the procedure of splitting nature and nurture and, through reductionist procedures, come to conclusions that the one or the other plays a more important role in behavior and development. To the contrary, the embodiment of genes within the epigenetic, behavioral, and cultural dimensions of evolution (of phylogeny) that Jablonka and Lamb (2005) discussed has important implications for the course of human development (for ontogeny).

One key implication is noted by Moore (2015a), who explains that: "Because biological development is all about cellular differentiation, focusing on development is an excellent way to learn about processes that control gene expression. And because these processes are epigenetic, studying development is a great way to learn about epigenetics" (p. 38); and "a gene's context always matters" (p. 44). However, Moore (2015a) cautions against overinterpreting epigenetics as a new form of biological determinism. Indeed, he emphasizes that:

> Development is not a deterministic process, so our mature characteristics are no more determined by our epigenetics than they are by our genetics. Actually, any of the developmental resources that contribute to our phenotypes . . . can mistakenly be thought to determine developmental outcomes. Epigenetic determinism . . . is still a form of determinism and as such, it is only marginally less perilous than genetic determinism . . . It is probably a bad idea to apply a deterministic worldview to a human being.
>
> (Moore, 2015a, pp. 190–192)

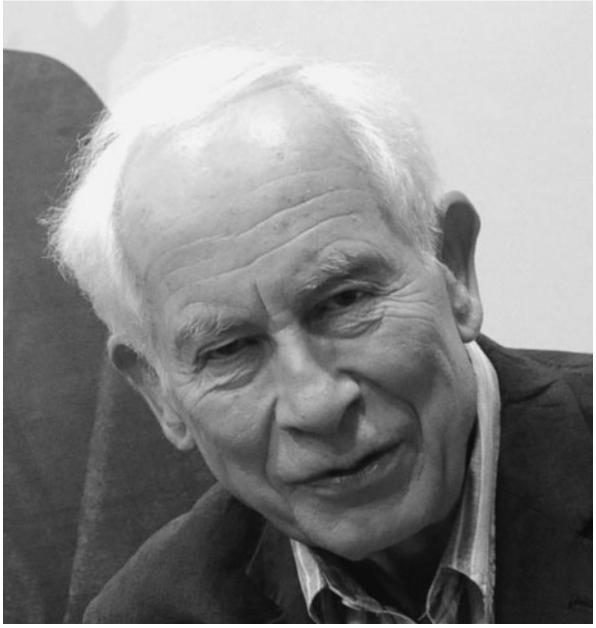
There are other key implications of the embodiment of genes within the relational developmental system for human development across life. One involves the idea that qualitative changes emerge across the life span through the integration of organism and contextual levels of organization (Lerner, 1984, 2012). A second key implication is the creation of relative plasticity in phylogeny and ontogeny occurring because of embodied actions resulting in (autopoietic) change in the developmental system (Witherington, 2014, 2015; Witherington & Lickliter, 2016). Relative plasticity characterizes the relations between organisms and contexts that, across time, create qualitative change in developmental processes within and across generations (Lerner, 1984). This qualitative discontinuity involves what developmental scientists have termed epigenetic (emergent) change (e.g., Gottlieb 1997, 1998; Werner, 1957) in ontogeny. In turn, the actions of gene↔context processes, that are instances of embodied change within the developmental system, are the focus of study in the field of epigenetics (e.g., Cole, 2014; Meaney, 2010; Misteli, 2013; Moore, 2015a; Slavich & Cole, 2013).

It is important to distinguish the differences in meaning for these two uses of the term epigenesis. Within the description of developmental change across the life span, the term epigenesis refers to the emergence of qualitatively discontinuous characteristics (e.g., developmental stages) across ontogeny (see Gottlieb, 1997, 1998; Lerner, 1984; Lerner & Benson, 2013a, 2013b; Werner, 1957). In turn, Misteli (2013), noting that the term "epi" comes from the Greek and means "over" or "above," indicated that

David Moore

epigenetic effects are effects that are ones "beyond" the effects of genes. Similarly, Moore (2016, p. 1) notes that Aristotle coined the word "epigenetics," and that he believed that all human characteristics arose from this process. In turn, in the contemporary literatures of evolutionary biology and of molecular biology, the term "epigenetics" refers to a process involving gene↔context relations resulting in the modification of information transmitted by DNA (through messenger RNA, or mRNA) across long, even multi-generational, time scales (e.g., Meaney, 2010; Misteli, 2013; Moore, 2015a; Slavich & Cole, 2013). In short, then, Blumberg (2016, p. 2) succinctly specifies that, "This process-oriented developmental perspective . . . has long been referred to as *epigenesis*. This term should not be confused with *epigenetics*, which refers specifically to the study of how non-genetic factors influence gene expression."

The two concepts, of epigenetic/emergent change across ontogeny and changes in the information transmitted by DNA through epigenetics, may pertain of course to interrelated phenomena. Emergent change across the life span is explained, within theories associated with the RDS metatheory, by systems changes involving mutually influential relations among levels of organization, which would include the gene↔context relations involved in epigenetics (e.g., Lerner & Benson, 2013a, 2013b; Moore, 2015a, 2016; Witherington, 2014). As well, contemporary scholarship about the features of epigenetics and evolution reflects the concept of embodied change (of fusion, or integration, of changes at all levels of organization within the developmental system). The embodiment of biological change within the developmental system means that the impact of an individual's biology on his or her developmental change can be altered (enhanced) through autopoietically occurring changes or through planned applications of developmental science in the service of promoting individual thriving or social justice.



Patrick Bateson

Bateson and Gluckman (2011) observed that gene expression is fundamentally shaped by variables external to the cell nucleus (where deoxyribonucleic acid, DNA, is located). They stressed therefore that "A willingness to move between different levels of analysis has become essential for an understanding of development and evolution" (Bateson & Gluckman, 2011, p. 5).

Similarly, Keller (2010) explained that it is erroneous either to conceptualize development as involving separate causal influences or to posit that attributes of the person develop as an outcome of the interaction of causal elements. Indeed, and in agreement with both Schneirla (1965) and, as well, points I made earlier in this chapter, she noted that the concept of interaction is itself flawed, in that its use is predicated on the idea that there exist attributes that are at least conceptually separate. Keller explained that the concept of developmental dynamics precludes such separation. She emphasized that:

> From its very beginning, development depends on the complex orchestration of multiple courses of action that involve interactions among many different kinds of elements—including not only preexisting elements (e.g., molecules) but also new elements (e.g., coding sequences) that are formed out of such interactions, temporal sequences of events, dynamical interactions, etc.
>
> (Keller, 2010, pp. 6–7)

In addition, Keller (2010), in discussing the elements of the epigenetic system, reflects the idea of

Evelyn Fox Keller

the research moment of the opposites of identity, discussed by Overton (2015a); as well, by pointing to the presence of dynamic coactions, her ideas reflect also the moment of the identity of opposites.

Moreover, Pigliucci and Müller (2010) noted that genes are not as much generators of evolutionary change as they are *followers* in the evolutionary process. They explained that "evolution progresses through the capture of emergent interactions into genetic-epigenetic circuits, which are passed to and elaborated on in subsequent generations" (Pigliucci & Müller, 2010, p. 14). Similarly, West-Eberhard (2003) connected evolution and the presence of relative plasticity across development. She explained that environmental variables are a major basis of adaptive evolutionary change. As also pointed out by Pigliucci and Müller (2010), West-Eberhard (2003) noted that genetic mutation does not provide either the origin or the evolution of novel adaptive characteristics because "genes are followers not leaders, in evolution" (p. 20). In addition, she explained that the relative plasticity of the phenotype can facilitate evolution by providing immediate changes in the organism (West-Eberhard, 2003). Similarly, Gissis and Jablonka (2011) noted that plasticity "is . . . a large topic, but, just as Lamarck anticipated, an understanding of plasticity is now recognized as being fundamental to an understanding of evolution" (p. xiii).

EPIGENETICS INVALIDATES IDEAS OF GENETIC REDUCTIONISM

Genetic reductionism is the idea that genes constitute the bed-rock (essential) causal agents accounting for a broad array of characteristics of living organisms. Simply, in this view, human behavior is regarded as being ultimately reducible to genes and, as such, genes are held to be the fundamental cause of human behavior and development. Genetic deterministic models do not ignore contextual factors, including the environment beyond the organism, but the base claim is that genes are the ultimate causal agents. I use the term "reductionist" to emphasize the fact that these models are epistemologically reductionist (i.e., their route to knowing is to reduce observed phenomena to their (purported) essential level), rather than holistic, along with being deterministic in nature (see Moore, 2015a).

Today, many scientists from diverse disciplinary backgrounds have criticized genetic reductionist models as being invalid (at this writing, some recent examples include Bateson, 2015, 2016; Bateson & Gluckman, 2011; Blumberg, 2016; Feldman, 2014; Joseph, 2015; Keller, 2010; Lickliter, 2016; Lickliter & Honeycutt, 2015; Moore, 2015a, 2016; Noble, 2015; Panofsky, 2014; Richardson, 2017; Witherington, 2014, 2015; see too Woese, 2004). This work has demonstrated that genetic reductionist arguments, when considered in the contexts of informed conceptual analyses, methodological rigor, and empirical evidence, become clearly exposed as a set of false propositions.

Indeed, Moore (2016) quite clearly notes the lack of usefulness of ideas that genes, independent of their coactions with their context, can contribute to behavioral development. He indicates that:

> What your genes are induced to *do* by their contexts—which include other genes, of course—is just as important as what genes you *have*; a gene you have inherited from your parents is of no consequence if that gene remains turned off by epigenetic processes, so you might as well not have the gene at all.
>
> (Moore, 2016, p. 2)

Nevertheless, genetic reductionists continue to assert their *absurd* (i.e., counterfactual and, even more, impossible) claims. For instance, one group clings to the gene-centric claim that intelligence is fundamentally determined by the genome, whereas environmental factors play, at best, a very minor role (e.g., Plomin et al., 2016; Rimfeld et al., 2016). As noted earlier in this chapter, another group contends that there are five, genetically-shaped personality traits that arise independent of any experiential contribution (e.g., Costa & McCrae, 1980, 2006; McCrea et al., 2000), and that these five traits explain all facets of human psychological individuality. Other groups (e.g., proponents of EDP and/or sociobiology) explicitly endorse genetic reductionism through the claim that the genotype actually contains information for the phenotype (e.g., Bjorklund, 2016). This group also maintains, among similar ideas, that girls of color have evolved to prefer a "reproductive strategy" that involves early and promiscuous sexuality and high fertility (see, e.g., Belsky, 2012; Belsky, Steinberg, & Draper, 1991; Draper & Harpending, 1982, 1988; Ellis, Schlomer, Tilley, & Butler, 2012). These groups propose neo-eugenicist ideas, contending that some children have genes that preclude their being able to develop positively through the application of interventions based on progressive policies (Belsky, 2014).

However, the evidence concerning epigenetics, embodied action, and plasticity that today is understood as accounting for the features of evolutionary and developmental change necessarily leads to deep skepticism about the "extreme nature" (e.g., Rose & Rose, 2000) of the claims of biological reductionists, for example, proponents of EDP (see Rose & Rose, 2000, and Saunders, 2013, for critiques), sociobiology (e.g., see Lerner, 1992a, Lerner & von Eye, 1992, for critiques), and behavior genetics (e.g., see Molenaar, 2014, and Moore & Shenk, 2016, for critiques). Clearly the claims of such reductionists are inconsistent with the now quite voluminous evidence in support of the role of epigenetics in the multiple, integrated dimensions of human evolution, discussed above (e.g., Bateson, 2015, 2016; Jablonka & Lamb, 2005; Lickliter, 2016, Lickliter & Honeycutt, 2015; Moore, 2015a; Noble, 2015). Moreover, these claims run counter to research that has importantly begun focusing on the role of the organism's active agency (McClelland, Geldhof, Cameron, & Wanless, 2015), and of culture (Mistry & Dutta, 2015), in creating change within and across generations.

The absurdity of the claims involved in genetic reductionist thinking is underscored by advances in the understanding of epigenetic processes in human development (Witherington & Lickliter, 2017; see too Jablonka & Lamb, 2005; Lester et al., 2016; Moore, 2015a, 2016) and by an appreciation of an alternative holistic approach to understanding developmental processes. This alternative is the RDS metatheory, which involves processes that create idiographic (individually distinct), emergent features of human development (Molenaar & Nesselroade, 2015). That is, from a non-gene-centric, holistic RDS-based view, development occurs through the person's (the system's) *embodied activities and actions* operating *coactively* in a lived world of physical and sociocultural objects, according to the principle of *probabilistic epigenesis*.

Conceptualized relationally, *probabilistic epigenesis* (e.g., Gottlieb, 1970, 1997, 1998, 2004; see too Chapter 9) designates a *holistic* approach to understanding developmental complexity. *Probabilistic epigenesis* (not epigenetics) is the principle that the role played by any part process of a relational developmental system—gene, cell, organ, organism, physical environment, culture—is a function of all of the interpenetrating and coacting part processes of the system. It is through complex relational, bidirectional, and multidirectional reciprocal interpenetrating actions among the coacting part processes that the system moves to levels of increasingly organized complexity.

Thus, probabilistic epigenesis identifies the system as being completely *contextualized* and *situated*; *time and place matter* (Elder et al., 2015). This relational development leads, through positive and negative feedback loops created by the system's organized action, to increasing system differentiation, integration, and complexity, directed towards adaptive ends (see Raeff, 2016).

EPIGENETICS WITHIN THE CONTEXT OF RDS METATHEORY

In contrast to the claims of biological reductionists, a process-relational paradigm and concepts associated with the RDS metatheory (Overton, 2015a) suggest that transmission across generations is accounted for by the plastic embodied processes of the individual functioning in a reciprocal, that is, bidirectional (↔), relation with his/her physical and cultural context. Thus, within an RDS-based approach to epigenetics, and in the context of contemporary evolutionary scholarship, (e.g., Gissis & Jablonka, 2011; Ho, 2010, 2013, 2014; Keller, 2010; Lickliter, 2016; Lickliter & Honeycutt, 2015; Meaney, 2010; Moore, 2015a, 2016; Noble, 2015; Saunders, 2014), the "Just So" stories (Gould, 1981) of genetic reductionist models, such as sociobiology, EDP, and behavior genetics, are conceptually and empirically flawed.

Furthermore, embodiment constitutes the basis for probabilistic epigenesis within the person's life span (Gottlieb, 1997, 1998), including qualitative discontinuity across ontogeny in relations among biological, psychological, behavioral, and social-cultural variables. Evidence for the relative plasticity of human development within the integrated levels of the ecology of human development makes biologically reductionist accounts (or, equally, completely sociogenic accounts) of features of human development, such as parenting, offspring development, or sexuality, implausible, at best, and entirely fanciful, at worst (Lerner, 1984, 2002, 2006b, 2015c, 2016).

The recognition that nature–nurture constitutes a non-dissolvable relational status enables the ideas involved in RDS metatheory to provide the conceptual grounding for understanding the role of epigenetics in human development. As such, it is this metatheory, and the process-relational paradigm from which it is derived (Overton, 2015a), that forms an alternative holistic framework within which the absurd claims of genetic reductionism become highlighted.

Theory-predicated research from multiple disciplines (e.g., evolutionary biology, human genetics, developmental science, sociology, and anthropology) provides empirical documentation of the logical shortcomings of biological reductionist (genetic or neuronal) models (e.g., sociobiology, EDP, Five Factor Theory, or behavioral genetics) and methods (e.g., adoption designs, monozygotic (MZ) and dizygotic (DZ) twin research, or heritability analyses; e.g., see Joseph, 2015; Lerner, 2015b, 2015c, 2015d; Overton, 2015a; Richardson, 2017; see Chapters 10 and 11). RDS-based theoretical models (e.g., Lerner & Callina, 2014a; Lerner et al., 2015) explain that any facet of individual structure or function (e.g., genes, the brain, personality, cognition, or intelligence) is embodied, and fused, with other features of the individual, and with the characteristics of his or her proximal and distal ecology, including culture and history. Embodiment indicates that biological, psychological, and behavioral attributes of the person, in fusion with history, have a temporal parameter. As I noted earlier in this chapter, this temporality provides the potential for at least relative plasticity in individuals' trajectories across the life span.

Embodiment provides a basis for the bidirectional relations between behavior and methylation—that is, behavior↔methylation relations—that characterize epigenetics. For instance, Bateson and Gluckman (2011, p. 5) observe that "gene expression is profoundly influenced by factors external to the cell nucleus in which reside the molecules making up the genes: the DNA. A willingness to move between different levels of analysis has become essential for an understanding of development and evolution." Similarly, Keller (2010, pp. 6–7) explains that:

> Not only is it a mistake to think of development in terms of separable causes, but it is also a mistake to think of development of traits as a product of causal elements interacting with one another. Indeed, the notion of interaction presupposes the existence of entities that are at least ideally separable—i.e., it presupposes an a priori space between component entities—and this is precisely what the character of developmental dynamics precludes. Everything we know about the processes of inheritance and development teaches us that the entanglement of developmental processes is not only immensely intricate, but it is there from the start. From its very beginning, development depends on the complex orchestration of multiple courses of action that involve coactions among many different kinds of . . . [components]—including not only preexisting . . . [components] (e.g., molecules) but also new . . . [components] (e.g., coding sequences) that are formed out of such . . . [coactions], temporal sequences of events, dynamical . . . [coactions], etc.

Moreover, Pigliucci and Müller (2010), in presenting what they term an "extended synthesis" of evolution, note that:

> Far from denying the importance of genes in organismal evolution, the extended theory gives less overall weight to genetic variation as a generative force. Rather, [there is a] view of "genes as followers" in the evolutionary process, ensuring the routinization of developmental . . . coactions, the faithfulness of their inheritance, and the progressive fixation of phenotypic traits that were initially mobilized through plastic responses of adaptive developmental systems to changing environmental conditions. In this way, evolution progresses through the capture of emergent . . . [coactions] into genetic-epigenetic circuits, which are passed to and elaborated on in subsequent generations.
>
> (p. 14)

In turn, West-Eberhard (2003) argues that "the universal environmental responsiveness of organisms, alongside genes, influences individual development and organic evolution, and this realization compels us to reexamine the major themes of evolutionary biology in a new light" (p. vii). Linking the presence of plasticity across development with evolution, she makes three major points:

> First, environmental induction is a major initiator of adaptive evolutionary change. The origin and evolution of adaptive novelty do not await mutation; on the contrary, genes are followers not leaders, in evolution. Second, evolutionary novelties result from the reorganization of preexisting phenotypes and the incorporation of environmental elements. Novel traits are not de novo constructions that depend on a series of genetic mutations. Third, phenotypic plasticity can facilitate evolution by the immediate accommodation and exaggeration of change. It should no longer be regarded as a source of noise in a system governed by genes, or as a "merely environmental" phenomenon without evolutionary importance.
>
> (West-Eberhard, 2003, p. 20)

Accordingly, propositions that reduce human development to genes that act independent of context, or which assert that genes contribute variance to human behavior (intelligence, personality, sexual relationships, etc.) that is not moderated by coactions with the context, are unwarranted and, in fact, absurd. Indeed, in the above-noted book discussing the transformations of Lamarckian theory that have arisen in relation to the increasingly more active focus on epigenetic processes in the study of both evolution and development, Gissis and Jablonka (2011, p. xiii) note the links between plasticity of embodied relations among an organism and the multiple biological through ecological levels of its ecology and epigenetic change. They point out that:

> Experimental work now shows that, contrary to the dogmatic assertions of many mid-twentieth-century biologists that it could not occur, even a form of "inheritance of acquired characteristics" does occur and might even be said to be ubiquitous. In particular, new variations induced by stress are sometimes inherited. The molecular mechanisms that underlie such inheritance—the epigenetic inheritance systems—are now partially understood, and . . . the existence of various types of [such] soft inheritance affects how we see adaptive evolution and speciation. It also has implications for human health.
>
> (Gissis and Jablonka, 2011, p. xiii)

In sum, the evidence about embodiment, plasticity, and epigenetics that accounts for the character of evolutionary and developmental change understandably elicits skepticism about, indeed the repudiation of, the "extreme nature" (Rose & Rose, 2000) of the claims of some biological reductionists. For instance, proponents of EP (evolutionary psychology) claim that "everything from children's alleged dislike of spinach to our supposed universal preferences for scenery featuring grassland and water derives from [the] mythic human origin in the African savannah" (Rose & Rose, 2000, p. 2). These claims are predicated on the basis of the assertion that one can explain:

> all aspects of human behaviors, and thence culture and society, on the basis of universal features of human nature that found their final evolutionary form during the infancy of our species

> some 100–600,000 years ago. Thus for EP, what its protagonists describe as the "architecture of the human mind" which evolved during the Pleistocene is fixed, and insufficient time has elapsed for any significant subsequent change. In this architecture there have been no major repairs, no extensions, no refurbishments, indeed nothing to suggest that micro or macro contextual changes since prehistory have been accompanied by evolutionary adaption.
>
> (Rose & Rose, 2000, p. 1)

Clearly such assertions within EP are inconsistent with the evidence in support of the epigenetic character of evolution and ontogeny, of the multiple, integrated dimensions of evolution, and of the role of the organism's own agency and of culture in creating change within and across generations (Overton, 2015a; see too the chapters in Overton & Molenaar, 2015). The embodiment of the individual and of his or her plastic developmental, biological, psychological, and behavioral processes within the relational developmental systems provides a basis for epigenetics across generations, that is, for changes in gene↔context relations within one generation being transmitted to succeeding generations.

In short, the burgeoning and convincing literature of epigenetics means that genetic function is a relatively plastic outcome of mutually influential relations among genes and the multiple levels of the context within which they are embedded (cellular and extracellular physiological processes, psychological functioning, and the physical, social, and cultural features of the changing ecology; e.g., Cole, 2014; Slavich & Cole, 2013). These relations create epigenetic change, for instance behavior↔methylation relations. As a consequence, the "Just So" stories (Gould, 1981) of EDP, as well as the assertions of behavior geneticists (Plomin et al., 2016), trait theorists (e.g., Costa & McCrae, 1980, 2006), and sociobiologists (e.g., Draper & Harpending, 1988; Freedman, 1979; Rushton, 2000), are conceptually flawed, ignore contemporary scholarship about evolutionary processes and their impact on ontogeny (e.g., Bateson, 2015, 2016; Gissis & Jablonka, 2011; Ho, 2010, 2013, 2014; Lickliter, 2016; Lickliter & Honeycutt, 2015; Meaney, 2010, 2014; Moore, 2015a, 2016; Saunders, 2014), and are more than empirically unsound. They are reduced to absurdities.

Conclusions

The presence of plasticity in human development, arising through embodied processes of individual↔context relations, including behavior↔methylation relations, means that combinations of individual↔context relations can be identified or designed to enhance the probability of positive development among all individuals, and to decrease disparities in opportunities for positive development. Indeed, Lester et al. (2016) "highlight the optimism in the epigenetics research community" (p. 34), and point out that the role of "evidence-based use of epigenetics for diagnostic purposes and intervention would be warranted, even perhaps ethically mandated, if they relieved human suffering" (p. 35). As such, the application of holistic RDS-framed epigenetics research can eventuate in the enhancement of social justice (Fisher et al., 2013).

In sum, the study of epigenetics illustrates that the genes received at conception (i.e., the genotype) are not a fixed blueprint for development. Genes are constantly getting turned on and off across the life span, and most of this activity is stochastic and short-term (and of largely unknown origin; Misteli, 2013). However, epigenetic changes are enduring, systematic, and even cross-generational (e.g., Meaney, 2010; Misteli, 2013; Moore, 2015a, 2016; Slavich & Cole, 2013). When conceptualized within a holistic, process-relational, RDS-based theoretical perspective, such changes are products of behavior↔methylation relations, and not of genes per se. It is neither conceptually nor empirically appropriate to claim, therefore, that intelligence can be reduced to genetic contributions (see Richardson, 2017, for a critique), that genes provide the basis of the five "big" components of human personality, that genes control the sexual behavior of girls of color, or that there are genes that preclude the efficacy of progressive interventions.

PERSON-CENTERED, IDIOGRAPHIC METHODS AND SYSTEMS SCIENCE METHODOLOGY

Developmental science is the study of change, within the individual, within the individual↔context relation, and within the autopoietic developmental system (Baltes et al., 1977; Witherington, 2014, 2015). Indeed, because of temporality, change is a constant within the developmental system. Developmental scientists do not ask, therefore, whether there is change but, rather, if and how one instance of a specific change matters for another specific instance of change (Bornstein, 2017). However, Molenaar (2014) explained that the standard approach to statistical analysis in the social and behavioral sciences is not focused on change but, instead, derived from mathematical assumptions regarding the constancy of phenomena across people and, critically, time. As I noted earlier in this chapter, these assumptions, *the ergodic theorems*, lead to statistical analyses placing prime interest on the population level. Interindividual variation, and not intraindividual change, is the source of this population information (Molenaar, 2014).

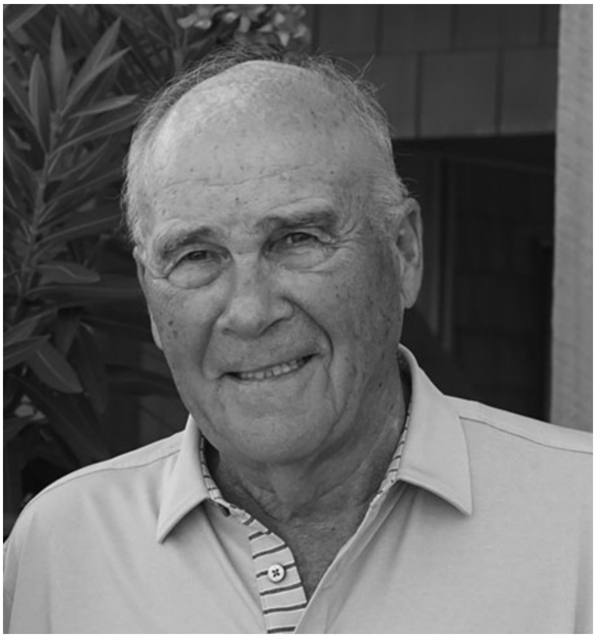
However, within the process-relational paradigm (Overton, 2015a), development is nonlinear and characterized by autopoietic (self-constructing) and hence idiographic (individually distinct) intraindividual change, features of human functioning that violate the ideas of ergodicity (Rose, 2016; Rose et al., 2013). Accordingly, use of the RDS metatheory as a frame for research requires a rejection of *primary* use of data-analytic tools predicated on the ergodic theorems, that is, the theorems that constitute the bases of traditional statistical procedures (Molenaar & Nesselroade, 2014, 2015; Nesselroade & Molenaar, 2010).

Molenaar (2014; Molenaar & Nesselroade, 2014, 2015) notes that, because developmental processes have time-varying means, variances, and/or time-varying sequential dependencies, the structure of interindividual variation at the population level is not equivalent to the structure of intraindividual variation at the level of the individual. *Developmental processes are therefore non-ergodic* (Rose, 2016). As a consequence, to obtain valid information about developmental processes it is necessary to study intraindividual variation within single individuals to both capture the non-ergodic nature of intraindividual change and, as well, produce generalities about groups that apply as well to the individuals within them. As noted earlier in the chapter, Molenaar and Nesselroade (e.g., 2014, 2015; Nesselroade & Molenaar, 2010) have begun to develop such methods. As well, other developmental scientists are also conducting such work (e.g., see Ram & Grimm, 2015; Velicer, Babbin, & Palumbo, 2014; von Eye et al., 2015).

The implications of such idiographic methods are that to properly conduct non-ergodic-based analyses researchers should ascertain *first* what are unique features of the developmental trajectories of the individuals they are studying and then ascertain if there is any commonality across individuals; if so, then researchers can make generalizations on that information (Molenaar & Nesselroade, 2015). This approach stands in marked contrast to initially aggregating the individual-level information and extracting generality from it in the form of average tendencies—the approach of traditional differential psychology. The idiographic, non-ergodic procedures suggested by Molenaar and Nesselroade replace static, trait conceptions with an approach that embraces development and complexity.

Given, then, the burgeoning presence of methodological tools enabling idiographic analysis, how, then, may research proceed? Consistent with the Bornstein (2006, 2017) specificity principle, I suggest,

Peter Molenaar



John R. Nesselroade

Marc H. Bornstein

therefore, that addressing a multi-part "what" question is the key to conducting programmatic research about the function, structure, and content of development across the life span. To test RDS-based ideas about the ontogenetically changing structure of development across the life span—to test empirically the process-relational conception of intraindividual change (Sokol, Hammond, Kuebli, & Sweetman 2015; Sokol et al., 2010; Overton, 2015a)—the task for developmental researchers is to undertake programs of research that use sets of interrelated questions associated with the specificity principle (Bornstein, 2017). For instance, developmental scientists should seek to ascertain answers to the following multi-part "what" question:

1. What structure–content relations emerge; that are linked to
2. What antecedent and consequent adaptive developmental regulations (to what trajectory of individual↔context relations); at
3. What points in development; for
4. What individuals; living in
5. What contexts; across
6. What historical periods?

I will return to the importance of the specificity principle for RDS-based research in later chapters. Here, however, I should note that the work of Molenaar and Nesselroade (e.g., 2014, 2015) is an example of the application of systems science methods to developmental science framed by RDS-based theories (see too Molenaar et al., 2014). For instance, the use by Molenaar and Nesselroade (2014, 2015) of dynamic factor analysis is an example of employing for developmental analysis a state–space model, in that it integrates a model of the dynamic evolution of the state process and another model linking the state process at each time point to the observed process at that time (see too Ram & Grimm, 2015).

Systems science "methods are designed to address complexity, that is, change . . . nonlinear relationships, bidirectional relationships (feedback loops), time-delayed effects, and emergent properties of the system—phenomena that are observed at the system level but cannot be linked to a specific individual component of the system" (Mabry & Kaplan 2013, p. 9S). As I noted earlier in this chapter, examples of systems science methods include such procedures as computational/mathematical modeling and simulation; micro simulation; agent-based modeling;

system dynamics modeling; network analysis; and discrete event simulation (Urban et al., 2011).

The use of systems science methods in developmental science is a sample case of the opening of the field to innovations in methodology, perhaps especially those associated with other disciplines. Scholars have recognized that new methodological tools are required for understanding the change processes involved in an epigenetic, agentic, and autopoietic system. New tools are required to appraise the qualitative changes marking this system and to model/test the revised understanding of causality within such a system. Accordingly, qualitative research and mixed-methods research are important and increasingly more prominent cases in point (Burton, Garrett-Peters, & Eaton, 2009; Tolan & Deutsch, 2015; Yoshikawa, Weisner, Kalil, & Way, 2008). In addition, there is burgeoning use of data-analytic methods derived from the work of econometricians.

ECONOMETRIC METHODS

The *sine qua non* of developmental analyses is the study of intraindividual change. As such, longitudinal designs continue to be the key approach to the study of such change (Molenaar & Nesselroade, 2015; von Eye et al., 2015). However, the problem of selection—of what economists term "endogeneity" (e.g., Heckman et al., 1997, 1998)—besets longitudinal studies, given that, even if representative samples are present at the beginning of a longitudinal study, selective attrition will increasingly bias the sample. People who stay in a study, perhaps especially a long-term one, may have "something about them" (something endogenous to them) that differs from participants who drop out of a study. Are changes seen in the remaining participants due, therefore, to something about the nature of the developmental process or to what may have been a pre-existing endogenous factor (e.g., the tenacity needed to stay at a task, obedience to authority, or trust in institutions)?

The problem of endogeneity is particularly problematic when longitudinal studies are used to assess whether particular experiences of one group (e.g., participation in a community-based, youth development program) are associated with developmental changes that differ from those seen within members of a group not participating in the experience (program). Here, the researcher may not be able to infer that the program was the cause of any differences between participating groups because it may be that there were pre-existing, endogenous factors that led some individuals to participate in (self-select into) the experience.

As a consequence, because of the problem of endogeneity, randomized control trials (RCTs) have been regarded as the "gold standard" design to test for causality (McCall & Green, 2004). As such, many potential funders of developmental science research have eschewed longitudinal studies because of the inability to demonstrate causality due to selection effects. However, the landscape of research aimed at causal analysis has changed. Econometric methods are being used in developmental science research to address endogeneity in longitudinal research. Among the important tools provided by econometricians are propensity score analyses, instrumental variable (IV) analyses, and regression discontinuity designs. These procedures are discussed in more detail in Chapter 13.

However, here it should be noted that the presence of these econometric tools indicates that randomized control trials are not the only means through which to assess causality in studies of the effects of programs on participants (Lerner & Callina, 2014a; Lerner, Lerner, Urban, & Zaff, 2016). In addition, as I will argue in Chapter 13, RCTs are not the "gold standard" for identifying causality, given limitations of three types of validity that I will discuss (internal, external, and ecological validity).

APPLICATIONS OF RDS-BASED RESEARCH: THE PROMOTION OF SOCIAL JUSTICE

Among the many split conceptions maintained by viewing the study of development through a Cartesian lens (Overton, 2015a) was the split between basic and applied research. However, within models of human development derived from the ideas of the RDS metatheory, this split joins other ones (e.g., nature–nurture or continuity–

discontinuity) in being rejected. When one studies the embodied individual within the developmental system, then explanations of how changes in the individual↔context relation (at Time 1) may eventuate in subsequent changes in this relation (at Time 2, Time 3, etc.) are tested by altering the Time 1 person↔context relation. When such alterations are conducted in the ecologically valid setting of the individual, these assessments constitute tests of the basic, relational process of human development *and, at the same time*, applications—interventions—into the course of human development (Lerner, 1995c). Indeed, depending on the level of analysis, aggregation, and time scale at which these interventions are implemented, such changes in the ecology of the individual↔context relation may involve relationships between individuals (e.g., mentoring relationships), community-based programs, or social policies (e.g., Bronfenbrenner, 2005).

As I have explained, the rationale for applying developmental science to enhance the lives of individuals or groups is predicated on the presence of relative plasticity in human development, a concept that is derived from RDS-based ideas, such as bidirectionally influential individual↔context relations and embodiment within time and place (including history; Elder, 1980). The relative plasticity of human development is a fundamental strength in, and the basis of optimism about, human development (Lerner, 1984). Developmental scientists can be hopeful that there are combinations of person and context that can be identified or created (through programs or policies) to enhance the lives of all individuals and groups.

In other words, developmental scientists may act to change the course of adaptive developmental regulations, of mutually beneficial individual↔context relations, in manners aimed at optimizing the opportunities for individual and group trajectories across life to reflect health and thriving. Given, then, the concepts associated with RDS-based theories and, as well, the methodological tools being devised in their scholarship (e.g., Molenaar et al., 2014), developmental scientists have in the repertoire of models and methods in their intellectual "toolbox" the means to work to promote a better life for all people. As I shall discuss in greater detail in Chapter 13, RDS-oriented developmental scientists may contribute effectively to providing to diverse individuals opportunities to maximize their positive aspirations. These scientists may enable individuals to more effectively act at being active producers of their positive development. When developmental scientists contribute to instantiating such aspirations and actions they are contributing as well to the promotion of a more socially just world (e.g., Fisher et al., 2013; Lerner, 2004; Lerner & Overton, 2008).

CONCLUSIONS

The epigenetic and embodied developmental changes that characterize individual↔context relations within the autopoietic developmental system provide a rationale for, and optimism about, applying developmental science in the service of promoting thriving and social justice for all people. Such applied developmental science requires "a theoretical framework more akin to current dynamic systems models than to traditional conceptions of either behavioral development or evolution" (Harper, 2005, p. 352). Derived from a process-relational paradigm, ideas associated with the RDS metatheory may be used to explain why "the Cartesian-split-mechanistic scientific paradigm that until recently functioned as the standard conceptual framework for subfields of developmental science (including inheritance, evolution, and organismic—prenatal, cognitive, emotional, motivational, sociocultural—development) has been progressively failing as a scientific research program" (Overton, 2013, p. 22). Overton noted:

> An alternative scientific paradigm composed of nested metatheories with relationism at the broadest level and relational developmental systems as a midrange metatheory is offered as a more progressive conceptual framework for developmental science. This framework accounts for the findings that are anomalies for the old paradigm; accounts for the emergence of new findings; and points the way to future scientific productivity.
>
> (Overton, 2013, p. 22)

And so where do we go from here? What is the future trajectory of developmental science framed by RDS metatheory?

THE FUTURE OF DEVELOPMENTAL SCIENCE

With a continued focus on RDS-based theoretical models framing methodologically rigorous, change-sensitive research, the future value of developmental science, as a useful frame for scholars in psychology, sociology, economics, biology, medicine, education, and other fields interested in describing, explaining, and optimizing the course of human life, seems assured. One bit of evidence in support of this prediction is the scholarship found in the seventh edition of the *Handbook of Child Psychology*, which had the expanded title of the *Handbook of Child Psychology and Developmental Science* (Lerner, 2015e). Across the four volumes of this publication, volume editors (Willis F. Overton and Peter C. M. Molenaar for Volume 1, on theory and methodology; Lynn S. Liben and Ulrich Müller for Volume 2, on processes of cognitive development; Michael E. Lamb for Volume 3, on processes of socioemotional development; and Marc H. Bornstein and Tama Leventhal for Volume 4, on ecological processes) emphasized the use of RDS-based perspectives and, as well, pointed to the implications of this metatheoretical frame for both understanding and enhancing (optimizing) individual↔context relations.

Lynn Liben

To the extent that the *Handbook* continues its history of marking the best work, and the key directions for scientific progress, in the areas of inquiry pertinent to developmental science, then the emphases on rigorous, theory-predicated research about the mutually influential relations among individual and ecological processes, and about the embodiment of human development within the rich and complex ecology of human life, will continue to be at the forefront of developmental science. Methodological innovations about how best to study the relational developmental system in manners maximally sensitive to time, place, and human diversity will therefore continue to be areas of active scholarship. Here I expect greater attention will be paid to tools for multi-method research, to creative ways to partition the x-axis in manners reflecting the non-ergodic character of developmental processes (Lerner, Schwartz, & Phelps, 2009; see too Chapter 13), to systems science methods (Urban, Osgood, & Mabry, 2011), and to the triangulation between micro and macro analyses of intraindividual change (Raeff, 2016; Werner, 1948, 1957).

Ulrich Müller

Michael E. Lamb

Tama Leventhal

In turn, the use, the application, of developmental science will continue to be a core and integrated concern of developmental scientists. The theoretical orientations and interests of new cohorts of developmental scientists, the requirements imposed by funders for producing scholarship that matters in the real world, and the needs for evidence-based means to address the challenges of the twenty-first century will coalesce to make Kurt Lewin's (1952, p. 169) quote, that "There is nothing so practical as a good theory," an oft-proven empirical reality. Indeed, I believe that the scientific and societal value on which the developmental science of the future will be judged will be whether its theoretical and methodological tools are productive at promoting positive human development across the life span for the diverse people of the world.

If past is prelude, then the realization of the future contributions of developmental science rests, at least in part, on the philosophical and scientific roots of the field. I discuss these historical foundations in Chapter 3.

CHAPTER THREE

Philosophical and Scientific Roots of Contemporary Developmental Science

Theories often have a life cycle. As described by Cairns and Cairns (2006), decades may elapse between the formulation of a new approach to human development and its becoming an organizing framework for research and/or its differentiation into a "family" of related theories (Damon, 2015; Lerner, 2015a; Reese & Overton, 1970). In turn, theories may lose their utility and cease being an influential frame for research and application. This diminution of influence can occur for many reasons.

There may be the identification of (a) fundamental conceptual flaws, including empirically counterfactual assertions (e.g., see Lerner, 2016; Lerner & Overton, 2017; Overton, 2015a, for examples of theories that split nature-variables from nurture-variables in attempting to account for human development); (b) irreparable problems with the methods associated with the empirical tests of ideas derived from the theory (e.g., see Chapters 11 and 12 for a discussion of such problems in behavior genetics and sociobiology); or (c) substantive "overreaching," that is, attempting to account for phenomena beyond the scope of the model (e.g., see Collins, Maccoby, Steinberg, Hetherington, & Bornstein, 2000; Elder et al., 2015; Horowitz, 2000; Shweder et al., 2006; Suomi, 2004a, 2004b; Overton, 2015a; Witherington & Lickliter, 2017, for discussions of this problem in genetic reductionist accounts, as occur in behavior genetics, sociobiology, and evolutionary developmental psychology (EDP); see Mascolo, 2013; Mascolo & Fischer, 2010; Thelen & Smith, 2006, for discussions of this problem in neo-nativist accounts of cognitive development; and see Bloom, 1998, for a discussion of this problem in behaviorist accounts of language development).

Cairns and Cairns (2006) note as well that a theoretical innovation in one period may actually constitute a return to ideas from an earlier era. When theoretical ideas are initially introduced, they may not become popular or even accepted for several reasons. There may be a lack of conceptual preparedness for the ideas or vocabulary used in a theory (e.g., see Flavell, 1963, for a discussion of why Piaget's early formulations, e.g., in 1923, were not embraced in the United States for almost 40 years). In addition, the ideas in a theory may not be able to be tested optimally because of methodological limitations (e.g., the absence of statistical procedures for modeling multilevel, hierarchically embedded, and reciprocal relations across time; e.g., see Molenaar, Lerner, & Newell, 2014; Nesselroade & Ram, 2004). Moreover, the "spirit of the times," the *zeitgeist* (Boring, 1950), may preclude acceptance of ideas that would require realigning the sociology of the science. Cairns and Cairns (2006) recount the challenges of instituting a truly multidisciplinary field of child development given the more than 50-year predominance of psychologists and of psychogenic (and reductionist) theories in that field.

Nevertheless, such conceptual, methodological, and sociological constraints on the acceptance of a theoretical orientation may be overcome (e.g., through the sort of evidentiary process involved in the paradigmatic revolutions discussed by Kuhn, 1962, 1970). As such, a theory introduced in one historical period may be rediscovered or a newer instantiation of it may be generated, albeit being "old wine in a new bottle." At this writing, the focus within the study of human development on concepts and models associated with relational developmental systems (RDS)-based theories (e.g., Lerner 2015a; Overton, 2015a) is a case in point, especially given that the roots of these models may be linked to ideas in developmental science that were presented at least as early as the 1930s and 1940s (e.g., Maier & Schneirla, 1935; Novikoff, 1945a, 1945b; von Bertalanffy, 1933), if not even significantly earlier (e.g., see Baltes, 1983).

Focusing on the Western world, we may note that many of the central questions and controversies about human development are quite old, with roots in ancient Greece and the traditions of Western philosophy. In both the 2,000 years of this philosophy and about 150 years of pertinent science, the ideas advanced to explain development have revolved around the same few issues. These issues represent the core concepts in any discussion of development, and differences among philosophers and scientists can be understood by looking at the stances they take in regard to such basic conceptual issues. These issues pertain most directly to one issue: the *nature–nurture controversy*. Although definable in several ways, this controversy pertains to a consideration of whether the sources of development lie in inborn (hereditary) processes or in acquired (or learned) processes. Thus, in order to organize and understand the evolution involved in the history of ideas of development, it is necessary first to introduce briefly some definition of the nature–nurture issue.

THE HISTORICAL ROLE OF THE NATURE–NURTURE ISSUE

The very first idea ever elaborated about human development, which I will note below was presented by Plato, involved what is still the most basic issue in development today: the *nature–nurture issue*. This issue pertains to the source of human behavior and development. Simply, a question is raised about where behavior and development come from.

In its most extreme form the issue pertains to whether behavior and development derive from *nature* (or in modern terms, *heredity, maturation*, or *genes*) or, at the other extreme, whether behavior and development derive from *nurture* (or in more modern terms, *environment, experience*, or *learning*). However, whatever terms are used, the issue raises questions about how inborn, intrinsic, native, or in short, nature characteristics (for example, genes) may contribute to development and/or, in turn, how acquired, socialized, environmental, experienced, or in short, nurture characteristics (for example, stimulus–response connections, education, or socialization) may play a role in development. Table 3.1 lists some terms used in regard to nature and nurture contributions, respectively.

The separation, or split, between nature and nurture illustrated in the table reflects a key distinction made by philosophers and scientists about the bases of human development. The separation between nature and nurture is an exemplar of the tendency in modern thought about human development to approach the study of people with concepts that reflect conceptual "splits." That is, realities about development are discussed or debated in either-or terms.

I noted in Chapters 1 and 2 that such bifurcation of concepts of development can be traced in modern philosophy at least to the ideas of the seventeenth-century philosopher, René Descartes. The study

Table 3.1 Terms associated with the nature or nurture conceptions of development

Nature terms	*Nurture terms*
Genetic	Acquired
Heredity	Education
Inborn	Empiricism
Innate	Environment
Instinct	Learning
Intrinsic	Socialization
Maturation	
Nativism	
Preformed	

of human development has been a field wherein fundamental conceptual issues have been framed traditionally as Cartesian splits (Overton, 2015a). That is, the conceptual issues that are regarded as foundational for the field are cast as controversies involving distinct conceptual entities—for instance, issues such as continuity versus discontinuity or stability versus instability.

However, these and other conceptual splits represent controversies because, and only because, they involve casting the fundamental nature of development into a split frame of reference. The split is illustrated by the categorical either/or form of the questions. With this split frame, all fundamental questions become "Which one?" Thus, this conceptual prejudice advances the argument that one or the other member of the pair necessarily constitutes the "real" or essential feature of development (Putnam, 1987; see also Descartes, 1969), and the opposite member is only apparently real or is derivative of secondary significance.

For example, Rowe's (1994) approach to understanding the role of the family, and of socialization agents more generally, in child development involves splitting nature from nurture, and results in a belief that the family and indeed all societal variables are really genetic in character. "Socialization" influences, to Rowe (1994) are, then, more apparent than real. To Rowe, what seem to be family or social influences are really inherited, genetic propensities to behave in particular ways in the presence of particular social settings. Adherents of other split, genetic reductionist ideas—for example, evolutionary developmental psychology (EDP) and sociobiology—explicitly endorse such reductionism through the claim that the genotype actually contains the phenotype. As I have noted, they state "Development is always modulated by the organized phenotype, which is initially provided by the parents in the form of a zygote" (Del Giudice & Ellis, 2016, p. 23). This group also maintains, among similar ideas, that girls of color have evolved to prefer a "reproductive strategy" that involves early and promiscuous sexuality and high fertility (see, e.g., Belsky, 2012; Belsky, Steinberg, & Draper, 1991; Draper & Harpending, 1982, 1988; Ellis, Schlomer, Tilley, & Butler, 2012). In addition, some members of this group propose a new version of eugenicist ideas, that is, ideas that suggest that humanity can be improved if people are bred to produce desired characteristics. These neo-eugenicists contend that investments of taxpayer money should be curtailed in regard to children who have genes that preclude their being able to develop positively through the application of interventions based on progressive policies (Belsky, 2014; and see the similar argument made by Binding & Hoche, 1920, in their racial hygiene book, *The Sanctioning of the Destruction of Lives Unworthy to Be Lived*; see too Chapter 11).

Once the analysis of development is framed in such a split fashion, it is generally further assumed that some set of empirical investigations will ultimately afford a definitive answer to the either/or question: Is development caused by nature or nurture variables? The simple observation that generations of empirical investigations have failed to resolve such a question demonstrates the inadequacy of the assumption on which this question is based. Nevertheless, the fundamental conceptual prejudice continues to hold the controversies in place as controversies (Overton, 2015a).

Split positions assign either/or explanatory values to the segregated individual elements. Traditionally, the elements are treated as "causes," and the two broad classes of elements used to explain change are "biological" causes or factors and "social-cultural" causes or factors. Thus, it is assumed within a split position that all change can be totally explained by one or the other, or by some *additive combination* of these two elementary foundational factors (Anastasi, 1958; Schneirla, 1956, 1957). In turn, in contrast to split positions, there exist relational conceptions. These concepts pertain to a set, or "family," of theories that I have labeled in Chapters 1 and 2 RDS-based models.

The explanatory categories generated by split positions stand in contrast to those generated by these relational positions (Overton, 2015a). Relational positions aim to "heal" the biological/social-cultural split (1) by offering categories that describe the biological and the social-cultural as alternative ways of viewing the same whole (see Gollin, 1981; Gottlieb, 1992; Lerner, 1986; Overton, 1973, 2015a; Tobach, 1981); and (2) by suggesting that action constitutes a broad-based process of development that itself differentiates into biological and social-cultural manifestations (Brandtstädter, 1998, 1999; Brandtstädter & Lerner, 1999; Eckensberger, 1989; Oppenheimer, 1991a, 1991b; Overton, 2015a).

As this chapter proceeds, I will discuss philosophers and scientists who have advanced ideas about development that pertain to nature, to nurture, or to some combination of the two. I will describe how, out of these debates, the RDS-based, and hence integrative and systems, perspective about nature and nurture evolved. I will argue that all ideas about development relate to the nature–nurture issue and, in turn, all other issues of development derive from this one. In fact, the history of developmental science can be regarded as involving the swinging of a pendulum. This pendulum moved from conceptions of human development stressing nature, to conceptions stressing the integration of the two ideas within RDS metatheory and, more specifically, the use of the three moments of analysis in programmatic developmental scholarship explained by Overton (2015a) and discussed in Chapter 2. Accordingly, I will point to the history of ideas about nature, nurture, and nature↔nurture coaction that evolved across the decades of the twentieth century and into the present one, for instance, in regard to the study of cognitive development, infant development, and life-span development.

PHILOSOPHICAL ROOTS

Prior to the nineteenth century, most efforts to understand human development were not associated with scientific theories of human development (Dixon & Lerner, 1999). Instead, many models of human development were derived primarily from philosophical, literary, or theological domains. It was not until the eighteenth and the early nineteenth century, however, that these theoretical perspectives were attached to systematic empirical investigations.

The beginnings of concern with the phenomena in the world can be traced to the first philosophers. Attempts to discuss the elements in the world constitute philosophical statements, and this sort of discussion first occurred more than 2,500 years ago. In about 600 B.C. a Greek named Thales of Miletus (640–546 B.C.) became the first philosopher through his attempts to speculate about the nature of the universe in order to predict a solar eclipse (Clark, 1957).

This event indicates that, when humans first turned their attention to the nature of phenomena in their world, they were concerned with the characteristics of the universe and not the characteristics of humans themselves. Philosophical concerns about the character of the universe pertain to cosmology, and this topic remained the predominant focus of thinkers for several hundred years. It was about 200 years later that the first major philosophical statement pertinent to the nature of humans was presented by Plato.

Plato (427–347 B.C.)

From Plato's ideas one can derive statements relevant to human development. Yet, many of these derivations are indirect. Plato's writings, and those of philosophers for centuries following him, do not reflect a primary concern with human development, although ideas about human change across life were apparent. The portion of Plato's writing from which one can derive his major ideas relevant to development deals essentially with the mind–body problem.

This problem—a major concern to philosophers for over 2,000 years—inquires into the relation between the physical, spatial, and temporal body and the non-physical, non-spatial, and non-temporal mind (or in Plato's term, "soul"). How does something that does not take up matter, space, or time (a soul) relate to something that does (a body)?

Plato reasoned that souls are eternal. He philosophized that there is a "realm of ideas," a spiritual place where souls reside. At birth, however, the body "traps" a particular soul. The soul remains in the body for the life of the person and returns to the realm of ideas when the person dies. Because the soul resides in the realm of ideas, it enters the body with these ideas at birth. That is, the person is born with *innate ideas*, with pre-existing, preformed knowledge.

Thus, Plato's idea about the relation between mind and body not only represents the first major statement about what humans are like but also represents a stance in regard to the nature–nurture issue. Humans are not the way they are primarily because of experience or education. They do not have to learn their knowledge. Rather, their knowledge is built into them; it is innate. Hence, this first major statement about human behavior was a nature one. That is, Plato said humans are the way they are (that is, having their innate ideas) because they have a soul, and this soul is a nature-based phenomenon.

In addition, Plato believed that the soul was divided into three layers, and these layers also have implications for a view of human development. The lowest layer of the soul involves humans' desires and appetites. Here passions, emotions, lusts, and physical needs are found (Muuss, 1975). A parallel can be seen between this layer of the soul and what Sigmund Freud (e.g., 1949), more than 2,000 years later, would describe as the *Id*, a purportedly innate structure of the personality proposed by Freud (1949).

Plato labels the second layer of the soul the spirit. Here courage, endurance, and aggressiveness originate (Muuss, 1975). Although humans and animals both have the first and second layers, only humans have the third layer. This is the true, or real, soul. It is, Plato said, reason. It is immortal and, as I already noted, only resides temporarily in the body.

What makes this layer idea relevant to a conception of development is that Plato did not believe that the attributes of each layer of the soul were immediately seen from birth. That is, people exercise the attributes of each layer successively, and Plato noted that although reason is certainly present in all humans, the exercise of reason is not achieved by all people (Muuss, 1975). Humans have to be trained in order to have their reasoning abilities drawn from them, and such training is what is involved in the Socratic method of education. This method involves existing knowledge being drawn from the person on the basis of questions by the teacher.

In sum, Plato's ideas provided the first major statement relevant to human development. This first conception of human development placed the basis of human functioning in the nature conceptual "camp." Moreover, some of Plato's ideas are compatible with ideas expressed in theories of human development devised thousands of years later (for example, Freud, 1949). Furthermore, although not an explicit theory of development, Plato's ideas of the layers of the soul did suggest that people differ across their lives in the attributes they manifest. Plato's ideas about the soul may have influenced others to speculate about the composition of the soul and about how its attributes were manifested. One person whose ideas were so influenced by Plato was his most famous student: Aristotle. Perhaps stimulated by his teacher's thinking, Aristotle revised Plato's ideas about the soul and about its relation to the body and, most important to this discussion, devised ideas explicitly relevant to understanding development.

Aristotle (384–322 B.C.)

Aristotle, who coined the term *epigenetics* (Moore, 2016) and who, in the *Nicomachean Ethics*, introduced moral virtues and character into the literature of philosophy, was also interested in the mind–body problem. His position differed from Plato's, however. Aristotle proposed the *hylomorphic doctrine*, which said that spirit (*hylo*) and matter (*morph*) were inseparable although distinct. The soul was present in all living organisms and gave life to matter. Aristotle believed that this influence occurred because there was a non-physical, non-spatial, non-temporal "force" that "breathed life" into matter. He called this force an *entelechy*. Aristotle's proposal reflected the idea of *vitalism*: There is a non-empirical, but vital (i.e., life-giving) entity present in any living organism that imparts life to that organism and directs its functioning.

Although an entelechy is present in all organisms, not all organisms have the same sort of entelechy. Like Plato, Aristotle postulated that layers of the soul existed. But in anticipation of Charles Darwin (1859), Aristotle conceived of these levels in a biological-evolutionary manner (Muuss, 1975). As did Plato, Aristotle believed there were three layers of the soul, but he identified them as a plant-like layer, an animal-like layer, and a human-like layer. The plant layer was associated with life functions related to reproduction and nourishment. Although animals and humans had this layer as part of their souls, plants had only this layer. Animals had an additional second layer, which was associated with functions such as locomotion, sensation, and perception; but animals did not have the third layer of the soul.

This additional layer was found only in humans, who of course had the other two layers as well. The human layer was associated with thinking and reasoning, and it was the possession of these attributes that Aristotle believed set humans apart from animals and plants. In essence, then, Aristotle believed that humans innately possess functions relating to three layers of the soul, and that the layer-related functions pertain to characteristics of life throughout the biological world. Accordingly, whereas Aristotle's postulation is a notion of development, it was a notion of changes in species, and this idea is then most akin to ideas of evolutionary change, of *phylogeny*. Ideas about changes within a species or within a member of a species across life are changes pertinent to *ontogeny*.

Simply, ontogeny is concerned with the development of an individual from conception to death, whereas phylogeny is also concerned with development, but here the concern is with how a particular species came to exist in the first place, or how it came to have the characteristics which it presently possesses. This latter concern, then, is with evolutionary or *phylogenetic* (or *phyletic*) roots (origins; Hodos & Campbell, 1969). In short, a developmental scientist may talk about either ontogeny or phylogeny and still be concerned with development. When the scientist talks about the latter, however, he or she is speaking about the history of the development of one or more species from their simpler ancestral to their more complex contemporary forms.

In that Aristotle's idea of the layers of the soul was related to the idea of phylogenetic development, it considered human attributes vis-à-vis the attributes of other (presumably less elaborated) forms of life. Later in this chapter, in my discussion of the ideas of Charles Darwin, Ernst Haeckel, and G. Stanley Hall, I will note that there are important distinctions between Aristotle's position and a view of phyletic development based on scientific understanding of evolution. Nevertheless, given this important qualification, Aristotle's ideas may be seen as the first statement directly pertinent to development, albeit less to ontogenetic development than to phyletic development.

However, Aristotle *did* offer ideas about ontogeny as well. First, like Plato, Aristotle believed that the functions associated with each layer of the soul emerged in a sequence from lower to higher. Aristotle was more explicit than Plato about this progression, and divided the maturation of the human being into three stages of seven years each.

The first seven years were labeled infancy, and Aristotle saw humans of this age and animals as alike. Both were ruled by their desires and emotions. Thus, in this first period, Aristotle saw phyletic consistency between humans and animals. The next period of development Aristotle labeled boyhood, whereas the last period of development was termed young manhood. After the end of this last stage was reached, development was presumably complete. The person of 21 was a mature adult.

One measure of Aristotle's continuing influence through history is that his belief that maturity was reached at Age 21 carried over to modern society. However, Aristotle's influence was even greater. Because his philosophy regarding the mind–body problem was adopted by Saint Thomas Aquinas (1225–1274), and then subsequently by the Catholic Church, Aristotle's views became almost canonized. They became the only acceptable dogma of the church (Misiak & Sexton, 1966; Misiak & Staudt, 1954).

Aristotle's philosophical hegemony resulted in an emphasis on the nature side of the nature–nurture debate. The levels of the soul were inherent in the human, as were the sequence of stages through which humans progressed until maturity was reached. Indeed, until the Protestant Reformation, begun in the sixteenth century by Martin Luther (1483–1546), Aristotle's philosophy remained largely unchallenged. Because the Catholic Church was, during these several centuries, a truly catholic (that is, universal) institution—and because of the prominence of religion in the lives of people during this period—challenging the dogma of the church was a dangerous act. A challenge to the dogma could lead to excommunication, and if expelled from the church there was no place else to go. Accordingly, because any one part of Aristotle's philosophy might be seen as related to another, no part was challenged until the Protestant Reformation provided an alternative to Catholicism. Until the sixteenth century, then, no view of development other than Aristotle's, regarding either ontogeny or phylogeny, was put forth. At this time, however, another idea relevant to development was advanced.

The Medieval Christian Era

As exemplified by John Calvin (1509–1564) and the American Puritans (e.g., the Pilgrims of the ship *Mayflower*), the medieval Christians had a religious philosophy that stressed the innate characteristics of humans. Based on portions of the Book of Genesis, this philosophy stressed the idea of original sin. Humans were said to be born with sin in them, or born basically evil. A second belief was that humans were basically depraved, and that their innate sin would be compounded by the inborn tendency to continue to commit sinful acts. In short, the medieval Christian view of human development was, like the others described thus far encountered, a nature one.

The nature orientation of this position is best illustrated by the reason given for the presence of innate sin and for innate tendencies toward continued badness. Medieval Christianity believed in the *homunculus* idea of creation. The reason for innate sin was that a homunculus—a fully-grown but miniature adult—was present from birth in the newborn's head. Instantly created with the child, this homunculus contained the sin and the basic depravity.

Of course, from this view, parents could apply harsh rules and stern punishments to their children. The children—having a preformed adult in them—were only different from other adults in terms of size. Hence when children were acting badly it was not because they did not know better; they acted badly because the "devil," the homunculus, made them do it.

Whereas this medieval Christian view does represent a conception of development different from Aristotle's, it still represents a nature view. In fact, in this concept children do not have to develop at all (except in size), since they have preformed adults within them. Thus, the ideas of Plato, Aristotle, and the medieval Christians provided a concept of human development that emphasized that the essential basis of development came from nature, from inborn tendencies. Furthermore, insofar as the medieval Christian view was concerned, there is no need for a theory of development, but only of addressing children's sinful behaviors. However, a philosophical position relevant to a concept of development did arise through the philosophy of Descartes and, in the span of another 150 years, led to a scientific view of development.

René Descartes (1596–1650)

Theological changes resulted in the loss of universal acceptance of Aristotelian philosophy and allowed philosophers to return to issues that had remained unaddressed for hundreds of years. A Frenchman, René Descartes, was a key figure in this movement. He reconsidered the mind–body problem, and his work marks the beginning of the era of modern philosophy.

In trying to formulate a proof for the existence of God, Descartes found it necessary to raise once again the issue of the relation between the physical body and the soul. He saw the two as separate, as dual, entities. As noted earlier, this view meant that modern philosophy was launched with a "split" view of reality (Overton, 2015a). That is, Descartes proposed that soul and body exist as two separate "lines" that cross at a particular location in the body, that is, the pineal gland (a small gland near the pituitary gland). Descartes termed this dualistic view of mind and body *interactionism*.

Moreover, in a manner similar to that of Plato, Descartes said that when the soul interacts with the body at the pineal gland, it gives the body knowledge. Thus, like Plato, Descartes believed in innate ideas. As such, although he was the first modern philosopher—by virtue of readdressing long-unconsidered issues—Descartes returned to a nativist (nature) conception of human functioning first put forth by Plato. Descartes' ideas split reality into body and soul, and indicated that the essential feature of humans—what made humans human—was the soul; without it humans would be automatons. Thus, by reducing humans to their *essential* characteristic, by splitting body and soul, Descartes could identify the fundamental, defining feature of human life. As such, his essentialism, reached through a process of reduction and of splitting the real or basic from the superfluous, ushered in an approach to understanding the world that would shape science through much of the ensuing several centuries (Overton, 2015a). In addition, Descartes' ideas had another important influence as well. They stimulated other philosophers to reconsider the "old" issue of the mind–body problem. Although accepting his dualism, other philosophers rejected his idea of mind–body interaction (Misiak & Sexton, 1966).

One major reason for this rejection was Descartes' attempt to "prove" statements about the mind on the basis of assertions that stressed innate characteristics, characteristics that were said to be "just there," that is, independent of any *empirical* (observable) proof. A group of philosophers who rejected Descartes' nativism argued that the only way to explain the existence of a phenomenon—of the soul or mind, for example—was through the formulation of ideas based on *empirical* events (that is, events capable of observation). Together, these philosophers formed a school of thought that evolved in Great Britain in the seventeenth century. One may understand the views of this group, and how they led to a concept of development, by focusing on the contributions of one leading thinker in what has been termed the British "school" (as in "school of thought") of empiricism.

John Locke (1632–1704)

Several British philosophers held similar ideas about the need to use empirical proof to discuss the mind (or soul) and the body. Examples are Thomas Hobbes, James Mill, John Stuart Mill, David Hume, David Hartley, Alexander Bain, and John Locke. I focus on Locke's ideas as an example of the British school's position, and also because of the influence of his ideas on later scientific thinking.

Locke rejected the idea that the mind is composed of innate ideas. Instead he said that, at birth, the mind is like a blank slate or, to use his (Latin) term, a *tabula rasa*. Any knowledge that the mind obtains is derived from experience. And experience makes its impression on the mind—it writes on the blank slate—by entering the body through the senses. Thus, because humans experience, or sense, certain observable events—for example, visual, auditory, and tactile stimulation—the mind changes from having no ideas to having knowledge. Accordingly, this philosophical statement is about ontogenetic development; it emphasizes nurture. Experiences from the environment provide the basis of development. The newborn is different from the adult because the newborn does not have knowledge and the adult does. Thus, there is development—change in knowledge in this case—and the development is based on nurture.

In emphasizing the role of nurture variables such as sensory stimulation in shaping behavior (or knowledge), Locke provided a philosophical view quite consistent with a major theory in the history of psychology: the behavioristic, learning approach to development. People like Skinner (1938), Bijou (1976), Bijou and Baer (1961), and Gewirtz and Stingle (1968) emphasized that behavioral changes can be understood in terms of environmentally based stimulus–response relations. In this regard, these learning theorists are quite like Locke.

However, Locke's influence extended beyond providing a philosophical and historical basis of at least some instances of learning theory. In fact, his ideas had two more general impacts. First, Locke's stress on the environment caused other philosophers to begin to consider the potential role of the environment. One major instance of such a philosopher was Jean-Jacques Rousseau (1712–1778). Rousseau combined both nativist and environmental ideas in his philosophy—one quite pertinent to a notion of development—and in so doing became the first philosopher explicitly to take the view that a nature–nurture relation provided the basis of human development. Rousseau said that all children are born innately good (a nature statement); however, through their relations with civilization (their experience, or nurture) they become corrupted. Hence, he argued for a "return to nature" in order to avoid the unfavorable effects of civilized experience.

Thus, the emphasis of Locke and other members of the British school of empiricism on the environment may have influenced other philosophers to consider nurture, with the fortunate additional result of leading them to devise other ideas of ontogenetic development. However, Locke's ideas may have had a second (more indirect but nevertheless more important) influence. A concern with empirical observation promotes a concern with science. All science rests on a basic characteristic, on observation. That is, science could not exist if the statements made by scientists could not be falsified or supported through observations. Accordingly, in promoting interest in empirical concerns among philosophers and other intellectuals, Locke and others in the British school of empiricism were in effect—albeit indirectly—promoting interest in scientific concerns.

During this time, the intellectuals in society were also the leaders of society (that is, the ones with the resources and power to get an education). Moreover, developments in such intellectual areas as philosophy, literature, and science were common and popular topics of social conversation. Knowledge of such developments was a mark of the status of being an educated and (usually) a rich and powerful person. As such, influencing the educated to consider empiricism may have promoted a general concern with science among them. Accordingly, when new events in science took place, news of them would not only reach other scientists but might get the attention of all educated people. Such information, then, if important enough, could not only influence scientists but might have implications for all areas of intellectual concern.

A century and a half after Locke's death, an event occurred in science that had such impact. It influenced not only the area of science it pertained to, but all areas of science and of intellectual concern (for example, education, theology, law, and medicine). The event was the publication of a book by a then relatively unknown British naturalist, Charles Darwin. The book—representing a theory derived from observations made while the author was on a trip to the Galapagos Islands (which are in the Pacific, on the equator, off the coast of Ecuador)—was *The Origin of Species by Means of Natural Selection*. Published in 1859, the book represents the transition from philosophical to scientific concern with the idea of development. As had been launched by Aristotle about 2,000 years earlier, the nature–nurture debate remained prominent across the history of scientific concern with the concept of development (Gottlieb, 1992).

SCIENTIFIC ROOTS OF DEVELOPMENT

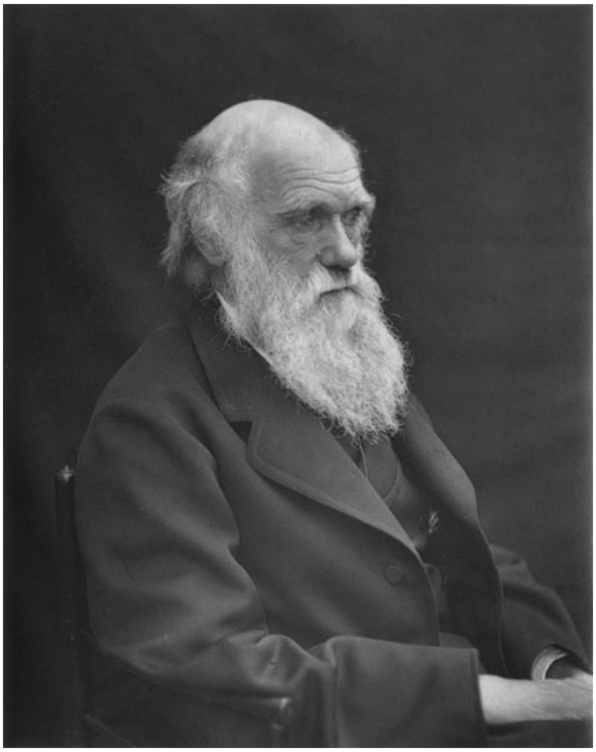
Interest in empiricism promotes a concern with science, and such a concern may be a basis for the impact of Darwin's ideas. Yet, there is a historical irony. The ideas of Locke emphasized a nurture view of ontogenetic development. However, the scientific view of development that Darwin devised emphasized a nature view of phylogenetic development. Accordingly, with the transition from philosophy to science, the nature–nurture pendulum swung back to nature. However, as in philosophy, the pendulum did not stay there.

Charles Darwin (1809–1882)

There are several key ideas in Darwin's theory of evolution. The environment in which a type of animal (a species) exists places demands on that animal. If the only food for an animal in a given environment is the leaves of tall trees, then the animal must be able to reach the leaves in order to survive. The environment "demands" that the animal possess some characteristic that will allow it to reach the high leaves. If the animal has that characteristic, it will fit in with its environment, get food, and survive. It will live and be able to reproduce, and thus pass on to its offspring those characteristics that enabled it to meet the demands for survival. If it is not fit, it will die. It will not live to reproduce. Its characteristics, which were not sufficient for meeting the demands of nature, will not be passed on. As other individuals of the species with such non-fit characteristics similarly fail to survive and reproduce, the species, and its characteristics, will become extinct.

Imagine, for example, that there were two species of giraffe, one with a long neck (as is the case) and the other with a short one. Because the long-neck giraffe has the characteristics that fit in with the demands of the particular environment, it would survive; the short-neck giraffe would not. Of course, if the setting changed—if, for example, only food very low on the ground were available—the characteristics of the short-neck giraffe might best fit the environment and the outcome could be reversed. The point Darwin made is that the characteristics of the natural setting determine which organism characteristics will lead to survival and which ones will not. Thus, it is the natural environment that selects organisms for survival. This process is termed *natural selection*.

Hence, Darwin used the idea of *survival of the fittest*, a concept introduced by Herbert Spencer



Charles Darwin

(Gottlieb, 1992). Organisms that possess characteristics that fit the survival requirements for a particular environmental setting will survive. In other words, specific characteristics in specific settings *have fundamental biological significance*—they allow the organism to survive (and hence to have the opportunity to reproduce and pass on the characteristics to offspring). Characteristics chosen by natural selection, and that meet the demands of the environment (and hence allow survival), are *adaptive* characteristics.

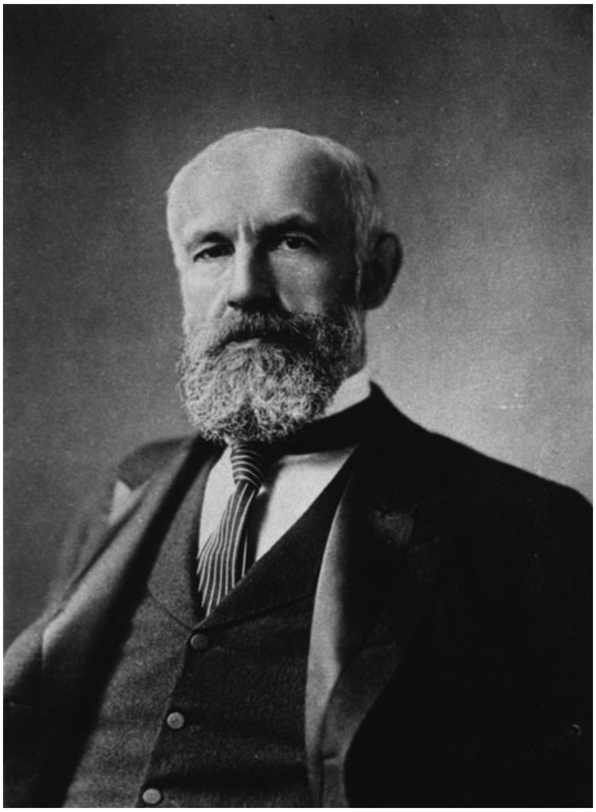
The giraffe example emphasizes that various physical characteristics of an organism may be *functional*. In an evolutionary sense, something is functional if it is adaptive, if it aids survival. In other words, "adaptive" means fit by virtue of natural selection (Gould & Vrba, 1982). Thus, the *structure* of an organism (its physical makeup, its constitution, and its morphological or bodily characteristics) may be functional. However, although Darwin in 1859 emphasized the function of physical structures of species, he later (1872) pointed out that behavior, too, had survival value. Showing fear when a dangerous bear approaches and being able to learn to avoid certain stimuli (snakes) and to approach others (food) are examples of behaviors that are adaptive; they aid survival.

The function of behavior became the focus of much social scientific concern. This concern was reflected not only in the ideas of those interested in the phylogeny (evolution) of behavior. In addition, the idea was promoted that the behavioral changes characterizing ontogeny (the life span) could be understood on the basis of adaptation. The adaptive role of behavior became an interest that provided a basis of all of American psychology (White, 1968). Indeed, this interest plays a major part in the ideas of theorists as diverse as Hall (1904), Freud (1949), Piaget (1950), Erikson (1959), and Skinner (1938, 1950). However, before the role of ontogenetic changes in adaptation—and hence in survival—can be completely discussed, it is useful to return to Darwin's ideas about survival and note how they reflect a concern not with ontogeny but with phylogeny.

Not all species survive. There are several reasons why this situation might happen. The natural environment might change, putting different demands on species. Species members that have adaptive characteristics will pass them on to their offspring and therefore the species will continue. Other species, lacking adaptive characteristics, will no longer be fit to survive and they will die out. After the modern synthesis of the mid-twentieth century (Lickliter, 2016), another reason that might be suggested about why one species might survive instead of another is that some change in the genetic material (for example, through mutation or cross-breeding) might give rise to new characteristics that favor survival. In either of these illustrations, however, evolution would proceed on the basis of the transmission of adaptive characteristics from parents to offspring. Species would evolve—change with history—as a consequence of natural selection, of survival of the fittest.

From a Darwinian perspective and, especially when Darwinism is seen through the lens of the modern synthesis (Lickliter, 2016), the basis of an organism's survival depends not primarily on what adaptive characteristics are acquired over the course of its ontogeny. Rather, the potential for adaptive functioning is transmitted to an individual by the parents (see Lerner, 2016; Witherington & Lickliter, 2016). From this perspective, development is not key for survival. Accordingly, adaptation is a hereditary, or nature, phenomenon. On the basis of evolution—the history of changes in a species, its phylogenetic development—a member of a species either will or will not be born with adaptive characteristics. Thus, Darwin's theory is a nature view of phylogenetic development.

In sum, based on his observations, Darwin presented the first major scientific theory of development. As noted, this view of species evolution had profound effects on areas of concern other than science. But, it is possible to remain within the scientific realm in order to gauge the impact of Darwin's ideas. Such impact occurred among scientists concerned not just with nature and phylogenetic issues, but also with issues pertinent to ontogeny and, finally, human development. Darwin's ideas were a major influence on G. Stanley Hall, the person who both founded the field of developmental psychology and devised the first scientific theory of human development.



G. Stanley Hall

G. Stanley Hall (1844–1924)

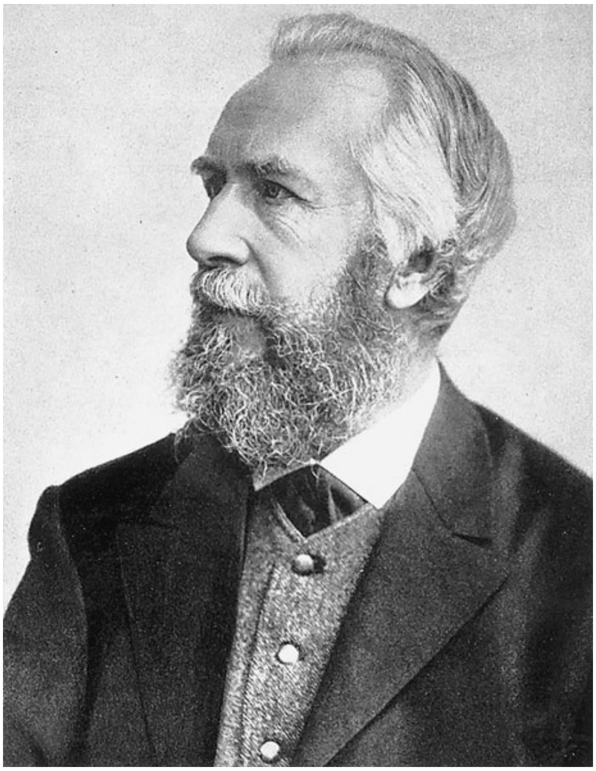
G. Stanley Hall (1844–1924) organized the American Psychological Association and became its first president. Hall and William James are the only two people ever elected twice to this post. Hall also started the first American journal of psychology, aptly called *The American Journal of Psychology*, as well as the first scientific journal devoted to human development (first entitled *Pedagogical Seminary*, and then given its present name, *The Journal of Genetic Psychology*). Moreover, Hall (1883) contributed one of the earliest papers on child psychology and also wrote the first text on adolescence (a two-volume work entitled *Adolescence*, 1904). His often-overlooked text on old age (*Senescence*, 1922) attests to the ground-breaking life-span perspective he brought to the study of human development.

One of the most prominent and influential psychologists at the turn of the century, Hall had his most specific influence on developmental psychology. Hall saw development from a nativist point of view. Although not many scholars adopted his specific nature-based theory of development, some of his students—including Arnold L. Gesell (1928, 1929, 1939, 1946) and Lewis M. Terman (1916, 1925; Terman & Tyler, 1954), who were among the most prominent developmental scientists during the first several decades of the twentieth century—did follow the general, nativist orientation to development that Hall espoused.

In devising his nature viewpoint, Hall was profoundly influenced by Darwin. In fact, fancying himself the "Darwin of the mind" (White, 1968), Hall attempted to translate Darwin's phylogenetic evolutionary principles into conceptions relevant to ontogeny. He did this by adapting ideas of the embryologist, Ernst Haeckel.

The Contributions of Ernst Haeckel

Ernst Haeckel (1834–1919) was a famed biologist, Darwinist, and theoretician. Haeckel's work was a major intellectual force in bringing Darwin's work into European and, particularly, German scholarship, and in creating the German *Social Darwinist* movement (Richards, 1987; Stein, 1987). Social Darwinism was an attempt to use Darwin's ideas of evolution to understand the organization of society *and* to create a new, or to legitimate an existing, social order (Tobach, Gianutsos, Topoff, & Gross, 1974). In other words, Haeckel's goal was to provide scientific legitimization for the romantic vision of the German people (*Volk*) as a group who have singularly met the test of succeeding in the struggle (*Kampf*) for survival imposed by nature and, as a consequence, have been selected for hegemony (i.e., domination, or rule) over other races, and indeed the world.


Ernst Haeckel

In this synthesis of his biologically determinist version of Darwinian evolutionary principles and volkish philosophy, Haeckel (1876, 1891) forged a viewpoint that was as much a political movement as it was science. Indeed, according to Stein (1987), Haeckel's views:

> combined an almost mystical, religious belief in the forces of nature (i.e., natural selection as the fundamental law of life) with a literal, and not analogical, transfer of the laws of biology to the social and political arena. It was, in essence, a romantic folkism synthesized with scientific evolutionism. It included the standard Darwinian ideas of struggle (*Kampf*) and competition as the foundation for natural, and therefore social law, with a curious "religion" of nature which implied a small place for rationalism, the lack of free will, and happiness as submission to the eternal laws of nature. *Blut und Boden* were the reality of human existence. (p. 259)

In 1906, joined by several prominent German scientists, theologians, literary critics, novelists, and politicians, Haeckel formed the Monist League, the aim of which was to organize both scientific and political support for Haeckel's Social Darwinist ideas. The belief uniting the members of the Monist League was that all of life—human and non-human—could be unified through use of Haeckel's Social Darwinist principles (Richards, 1987; Stein, 1987). Haeckel and his colleagues in the Monist League believed that one set of ideas could integrate not only the understanding of human evolution but politics, religion, morality, and ethics as well. The multiple disciplines and professions that were united within the perspective forwarded by Haeckel could therefore provide a compelling frame to a scholar, such as Hall, who was interested in integrating Darwinian biological ideas with the study of human development.

Haeckel's ideas regarding *recapitulation* were associated with Hall proposing just such an integration. Haeckel (1891) believed that an embryo's ontogenetic progression mirrored the phylogenetic history—the evolution—of its species. The embryo repeated adult instantiations of ancestral species but, of course, in a compressed timeline. Thus, when one looks at the changes characterizing an individual member of a species as it progresses across its embryological period, one sees a recapitulation of the evolutionary changes of the species.

By recapitulation, Haeckel meant that the process of evolution was a change in the timing of developmental events such that there occurred a universal acceleration of development that pushed ancestral (adult) forms into the juvenile stages of descendants (Gould, 1977). For example, Haeckel (1868) interpreted the gill slits of human embryos as characteristics of ancestral adult fish that had been compressed into the early stages of human ontogeny through a universal process of acceleration of development rates in evolving lines. In short, Haeckel was the author of the notion that "ontogeny recapitulates phylogeny."

Hall's Theory of Recapitulation

Hall applied to postnatal life the recapitulation idea that Haeckel used for prenatal, embryological development. Hall believed that the changes characterizing the human life cycle were a repetition of the sequence of changes human ancestors followed during their evolution. Arguing that during the years from birth to sexual maturity a person was repeating the history of the species, as had been done prenatally, Hall believed that the postnatal recapitulation was somewhat more limited than the prenatal (Gallatin, 1975). In fact, according to Gallatin (1975, pp. 26–27), Hall believed that:

> Rather than repeating the entire sweep of evolution, childhood was supposed to proceed in stages, each of which mirrored a primitive stage of the human species. Very early childhood might correspond, Hall speculated, to a monkey like ancestor of the human race that had reached sexual maturity around the age of six. The years between eight and twelve allegedly represented a reenactment of a more advanced, but still prehistoric form of mankind, possibly a species that had managed to survive by hunting and fishing.

Furthermore, Hall believed that adolescence represented a specific period in ontogeny after childhood. As such, Hall was the first person, within a scientific theory of development, to conceive of adolescence as a distinct portion of the life span (the term had, however, initially appeared in the first half of the fifteenth century; Muuss, 1975). Moreover, Hall's demarcation of adolescence as a distinct period of ontogeny was discussed in a manner consistent with a life-span view of human development. That is, Hall saw the capacities and changes of childhood continuing into adolescence, but at a more rapid and heightened pace.

In addition, he saw adolescence as a period of transition between childhood and adulthood. That is, the stages of life previous to adolescence emphasized the innate characteristics of humans held "in common with the animals" (Hall, 1904, I, p. 39). However, the stage of life following adolescence was said to raise a human "above them [i.e., animals] and make him most distinctively human" (Hall, 1904, I, p. 39). In short, adolescence was a period of transition from being essentially beast-like to being essentially human-like (i.e., civilized and mature).

The native endowment provided by human evolution, Hall believed, moved the person through the adolescent ontogenetic period, and thus put the person in the position of being able to contribute to humans' highest level of evolutionary attainment: civilization. Hence, Hall (1904, II, p. 71) said that "early adolescence is thus the infancy of man's higher nature, when he receives from the great all-mother his last capital of energy and evolutionary momentum." However, because of the acceleration and heightened capacities emerging in adolescence, and also because of the difficulty in casting off the characteristics of animal-like behavior and in acquiring at the same time the characteristics of civilization, the adolescent period was necessarily a stressful, difficult time of life. Adolescence was, to Hall, a universal period of storm and stress.

Criticisms of Recapitulation Theory

Hall extended the concept of human development beyond childhood and, in so doing, placed the period of adolescence within a perspective that encompassed the entire life span. However, the recapitulation theoretical frame within which he forwarded his view of human development was not generally accepted by either his students or his colleagues and, in fact, considerable conceptual and empirical criticism was leveled against Hall's, and of course therefore as well Haeckel's, recapitulation application of Darwinian evolutionary ideas (Gottlieb, 1992). It should be noted that Darwin too was an advocate of recapitulation, but just not as explicitly so as Haeckel (Gottlieb, 1992).

In light of the critiques of their respective views, it seems clear that neither Haeckel nor Hall appropriately represented the evolutionary process through their respective ideas about recapitulation. Even as an analogy, a description of human ontogeny as involving recapitulation is inappropriate. As initially pointed out by Thorndike (1904), and reemphasized by Gallatin (1975), by Age 2 to 3 years a human child has already exceeded the cognitive capacities of all other species—living (e.g., monkeys or apes) or extinct (e.g., humans' pre-hominid ancestors; Johanson & Edey, 1981). Sensorimotor, verbal, and social behaviors, for instance, are all more advanced in the 3-year-old human than in adults of any of these other species. In addition, there is no evidence that the developmental events of adolescence are a mirror of the history of civilization.

The Legacy of Hall's Recapitulation Conception: The Contributions of Terman and Gesell

Hall's most prominent students were Lewis Terman and Arnold Gesell. Their contributions illustrate much of the interest in ontogenetic development through the first three decades of the twentieth century. Terman was interested in mental measurement. The first intelligence test was constructed by Binet (Binet & Simon, 1905a, 1905b) in Paris. Terman was one of the first scholars to translate this test into English (H. H. Goddard in 1910 was the first). Terman, a professor at Stanford University, published the test as the Stanford–Binet (1916) and adopted the intelligence quotient (IQ), suggested by the German psychologist William Stern, to express people's performance on the test (IQ = mental age divided by chronological age, multiplied by 100 to remove the fraction).

Terman's interest in measuring intellectual ability was only in part based on a concern with describing how people differ (that is, interindividual differences). His interest was also a theoretical one. He believed that intelligence was mostly (if not exclusively) a nature-based characteristic. Accordingly, not only did he develop an instrument to describe interindividual differences in intelligence but he also carried out research to try to determine the genetic component of intelligence. One such project was his *Genetic Studies of Genius*, a longitudinal study of intellectually gifted children from 1921 onward (Terman, 1925; Terman & Oden, 1959). Terman's study was one of the first longitudinal investigations begun in the United States (Sears, 1975).

Perhaps needless to point out after the discussion of RDS metatheory and of epigenetics in Chapter 2, Terman's longitudinal study could not prove that intelligence is genetically determined (see too Richardson, 2017). Nevertheless, Terman's work,

Lewis Terman

involving nearly 50 years of study and reported in five published volumes over this span (see Terman & Oden, 1959), was quite important for several reasons. First, it encouraged several other longitudinal studies of human development. These studies provided data relevant to intraindividual changes, to development, across the life span. Second, Terman's findings did much to dispel myths about the psychological and social characteristics of intellectually gifted people. Although such people were sometimes stereotyped as weak, sickly, maladjusted, or socially inept, Terman provided data showing them to be healthy, physically fit, athletic, and personally and socially adjusted.

Third, Terman's work did much to make developmental science a descriptive, normative discipline. His work with the IQ test and his descriptions of the development of gifted people involved making *normative* statements. A *norm* is an average, typical, or modal characteristic for a particular group. If nature is the source of human development and environment plays no primary role, then all a scientist need do to depict the inevitable pattern of ontogeny is to describe the typical development of people (see Rose, 2016, for a critique of this view). As discussed briefly in Chapter 2 and as will be discussed in subsequent chapters as well, such a focus on norms, or on averages, is especially problematic for a developmental science focusing on the individually distinct (i.e., idiographic) character of human development (Molenaar & Nesselroade, 2014, 2015; Rose, 2016; Rose et al., 2013). However, Hall's other prominent student, Arnold Gesell, seemed to have based his work even more explicitly on this problematic reasoning than did Lewis Terman, and arguably did even more to make developmental psychology a normative, descriptive field.

To illustrate, Arnold Gesell was convinced of the importance of biological influences on development but was not an avid supporter of Hall's ideas about recapitulation (Dixon & Lerner, 1999; Kessen, 1965). For example, Gesell's positive regard for Darwin's impact on developmental science can be read in his article "Charles Darwin and Child Development" (Gesell, 1939, 1948). Here, Gesell acknowledged Darwin's "perception of the gradual genesis of all living things, including the genesis of the human mind" (1948, p. 44) and argued that Darwin's developmental perspective had a profound impact on the understanding of childhood. Gesell also pointed to the influence of Darwin's ideas on both Hall and him. But it was this common influence that Gesell emphasized, not Hall's specific "translation" of this influence into a (recapitulation) theory of human development (Dixon & Lerner, 1999).

Gesell proposed a theory that can be understood by his term *maturational readiness*. This nature-based theory said that maturational changes are independent of learning (Gesell's conception of what nurture amounted to). Gesell believed that sensorimotor behavior and even many cognitive abilities (for example, vocabulary development) were under the *primary* control of maturation. This control meant that their pattern of development was maturationally determined. Thus, an individual would develop when he or she was maturationally ready to, and attempts to teach a child before this time could not be helpful.

Arnold Gesell

Hence, in his writing and research (Gesell, 1929, 1931, 1934, 1946, 1954), Gesell emphasized the need for careful and systematic cataloging of growth norms. His work provided science with much useful knowledge about the expected sequence and times of emergence of numerous physical and mental developments of groups of children of particular demographic backgrounds. These descriptions would allow scientists, practitioners (e.g., pediatricians, educators), and parents to know, he believed, the nature-based sequence and timing of development and, as such, the point at which a person was maturationally ready for learning. Gesell's theory and research did much to make developmental science not only a nature-based discipline but also one whose major, if not exclusive, focus was descriptive. However, a nurture-based theory of behavior arose to counteract the predominant nature focus.

Behaviorism and Learning Theory

Just as the pendulum swung between nature and nurture in philosophy, it moved similarly in developmental science. In the second decade of the twentieth century and continuing through the 1950s, American psychology as well as other areas of social science (e.g., sociology; Homans, 1961) came to be quite strongly influenced by a particular conceptual-theoretical movement: a behaviorist, learning-theory view of behavior. Although this movement was not developed from a primary concern with children or human development, it was extensively applied to human development. In fact, no learning theory has ever been devised on the basis of information derived primarily from children (White, 1970). Nevertheless, philosophically consistent with Locke's empiricist views, this movement emphasized that in order for psychology to be an objective science, ideas about behavior had to be derived from empirically verifiable sources.

John B. Watson, emphasizing this orientation, developed his point of view under the label *behaviorism* (Watson 1913, 1918a, 1918b). He emphasized that stimuli and responses combined under specific lawful, empirical conditions—the laws of *classical and operant conditioning*. By focusing on how environmental stimuli gained control over the behavior of organisms, a developmental scientist could know how behavior was acquired and, by implication, developed. That is, development was seen as the cumulative acquisition of objective and empirical stimulus–response relations, and all a scientist had to understand to deal with human development was the way behavior was controlled by the laws of conditioning. Watson applied these ideas to children, both in his research (Watson & Raynor, 1920) and in his prescriptions for childcare (Watson, 1928).

The nurture view of behaviorism gave psychologists a position that allowed them to be viewed as objective scientists, like their colleagues in the natural sciences. As such, behaviorism and its variants and extensions (Hull, 1929; Skinner, 1938) became the predominant conceptual focus in American psychology. As with Watson's work, applications of ideas and principles were derived in the main from non-human organisms—usually rats (Beach, 1950; Herrnstein, 1977); but applications were made to human behavior, and ideas pertinent to human development arose. Thus, ideas about how humans acquire behavior consistent with the rules of society, that is, how they are *socialized*, were formulated. Such *social-learning* theories were not only reflective of a nurture view of development but also, at times, involved some attempt to reinterpret nature conceptions of development (e.g., those of Freud, 1949) in nurture terms (Dollard, Doob, Miller, Mowrer, & Sears, 1939; Miller & Dollard, 1941).

However, this nurture view of development had impacts that differed from integration with nature views of development. In fact, through the early 1940s, there was little integration of efforts by nature-oriented and nurture-oriented workers. The learning-oriented workers were doing *manipulative* studies—that is, they conducted experiments that varied stimuli to ascertain the effect on responses—and their work tended to concentrate on readily observable aspects of behavioral development (e.g., aggressive behaviors). This work constituted an elaborate and fairly precise compendium of how variations in specific stimulus characteristics were related to variations in the responses of specific groups of children—basically white, middle-class children of highly educated parents (Graham, 1992).

Thus, through the 1940s, proponents within the nature *or* nurture camps continued to work, but usually with little concern for integration with each other's endeavors. However, World War II served to alter this separation and to move developmental science from a primarily descriptive to a primarily theoretical, explanatory-oriented field.

World War II

The events surrounding World War II irrevocably altered the nature of American social science. First, the effects of events in Europe were felt even before the United States entered the war in December 1941. Nazi persecution led many Jewish intellectuals to flee Europe, and many sought refuge and a new start for their careers in the United States. Great pains were taken to find positions in American universities and associated institutions for the refugees, despite the fact that many of them held ideas counter to those predominating in the American academic scene (i.e., behaviorism and learning theory; Gengerelli, 1976).

For instance, although Freud himself settled in London (and died there in 1939), many psychoanalytically oriented people—some trained by Freud and/or his daughter Anna—immigrated to the United States. Some of them, for example, Peter Blos and most notably Erik Erikson, brought with them psychoanalytic ideas about human development.

In addition, once America entered the war, numerous soldiers had to be treated for psychological as well as physical trauma. The federal government gave universities large amounts of money to train clinical psychologists. This funding opened the door for many professionals with psychoanalytic orientations to become faculty members at universities previously dominated by behaviorists (Misiak & Sexton, 1966). These people had the backgrounds appropriate for teaching clinical skills to the large, new groups of future clinicians that were needed.

Thus, one impact of World War II was to encourage psychoanalytic thinking in many psychology departments. This orientation represented the introduction of nature-based thinking into departments where behaviorists previously resided in total control of the intellectual domain (Gengerelli, 1976). In addition, psychoanalysis represented just one of many different theoretical accounts of human functioning—accounts that emphasized either nature or both nature and nurture as sources of behavior and development—that were making inroads into American social and behavioral science thinking.

As such, nativist ideas about perception and learning—introduced by psychologists who believed in what were termed the holistic aspects of behavior—were juxtaposed with the learning ideas of the behaviorists. The *gestalt* (meaning "totality") views represented by these Europeans (people such as Max Wertheimer, Kurt Koffka, Wolfgang Kohler, and Kurt Lewin) were shown also to be pertinent to areas of concern such as brain function, group dynamics, and social problems (Henle, 1977; Sears, 1975).

Ideas explicitly relevant to development were also introduced. For example, Heinz Werner (1948) presented to Americans a view of development involving continual nature–nurture interactions and a concept—orthogenesis—that was held to be a general, regulative principle depicting the character of all developmental change (see too Raeff, 2016, and Chapter 8).

In addition, the developmental theory of the Russian psychologist Lev S. Vygotsky (e.g., 1927/1982, 1933/1966) was another example of the increasing influence of non-American ideas on the study of human development. Vygotsky, like his contemporary Jean Piaget (they were both born in 1896, but whereas Piaget lived until 1980 Vygotsky died in 1934 at age 37), saw development as progressing through stages of development. However, although both Piaget and Vygotsky presented theories that pertained to broad changes in the nature of a child's mental life, Vygotsky placed more emphasis than did Piaget on language and, especially, on culture in individual development (Cairns & Cairns, 2006; Keil, 1998; Valsiner, 1998).

Indeed, Vygotsky's theory drew on a broad range of ideas, from disciplines as varied as psychology, comparative ethology, art, cultural analysis, language, and neuroscience (Keil, 1998). Vygotsky's interest in these multiple disciplines, and the levels of organization within the ecology of human development to which they pertained, was associated with his devising a theory that emphasized the social and cultural origins of individual (e.g., cognitive or personality) development, and that emphasized that a person's instrumental activity (the actions a person takes to reach goals within a given situation) is enabled by social life (Cairns & Cairns, 2006; Overton, 2015a). Social relationships could *scaffold* a child's attainment of new knowledge or skills when the child was at a point in life when such *guided participation* could help the child attain these attributes. In other words, when the child was in the *zone of proximal development*, social support from a developmentally more advanced individual could help the child attain knowledge or a skill that would not be able to be obtained without such scaffolding.

Thus, Vygotsky's ideas about the zone of proximal development illustrate his emphasis on the importance of person–context relations in human development. This concept indicates that Vygotsky's theory integrated (a) the individual's actions; and (b) his or her thoughts and language (cognitive processes through which the person makes meaning in the world); with (c) his or her embeddedness in a specific "whole field," that is, the context or setting within which the person is acting (Valsiner, 1998). To Vygotsky, the person is an active agent in his or her development, selecting within the field the specific goals of his or her actions and identifying the means to reach them (Valsiner, 1998).

Lev S. Vygotsky

Some goals are available to the developing person through emitting actions already in his or her repertoire and by recruiting resources (means) available to him or her. For example, although a young child may not be able to reach a cookie on the kitchen counter by standing on his or her toes, he or she may be able to move a chair close to the counter, climb up and stand on it, and reach the cookie. However, some goals are not available to the child. A cookie placed on top of a refrigerator may not be able to be reached by him or her, even if standing on a chair. However, a child can be educated about the skills needed to recruit a taller person (e.g., an adult) to provide aid, and to get the cookie for him or her (e.g., "be polite," "say please," etc.). In addition, the child can be taught to either safely place a book on the chair, and then stand on it, or the child can be instructed in the use of a step-ladder.

Thus, the field within which the child exists can be divided into a "zone" within which the child can fend for himself or herself and a "zone" within which the child requires education or instruction. The zone of proximal development constitutes, then, "the difference between what a child can accomplish with guidance, and what he or she can achieve through individual effort and solo performance" (Valsiner, 1998, p. 207). By engaging in actions within this zone, Vygotsky believed that the child is developing through a process in which he or she "transcends his or her present level of development through constructive play" (Valsiner, 1998, p. 207).

In sum, whether due to interest in Werner's (1948) ideas about the format of developmental change, or in Vygotsky's (1927/1982, 1933/1966) theory about person–context developmental processes, the outcome of these changes in the range of intellectual ideas about development available to scientists, fostered in the United States by events relating to World War II, was a pluralism of ideas about human development. There were now numerous interpretations of behavior and development, and these interpretations were based on substantially different conceptions of the sources of development. Any given behavior, then, could be interpreted according to quite different alternatives, and these alternatives were advanced by respected advocates often working in the same academic settings. The simultaneous presentation of diverse interpretations promoted a move away from a focus on mere description and toward a primary concern with theoretical interpretations of development. This focus on explanation was heightened in the post-World War II era, throughout the 1950s and 1960s.

The 1950s and 1960s

Because of the pluralism of perspectives promoted by the events surrounding World War II, developmental scientists became less concerned with just collecting descriptive data. Rather, they focused more on the interpretation—the meaning—of development. As such, they became primarily concerned with the comparative use and evaluation of various theories in putting the facts of development together into an understandable whole. One index of this change of focus was the rediscovery of the theory of Jean Piaget.

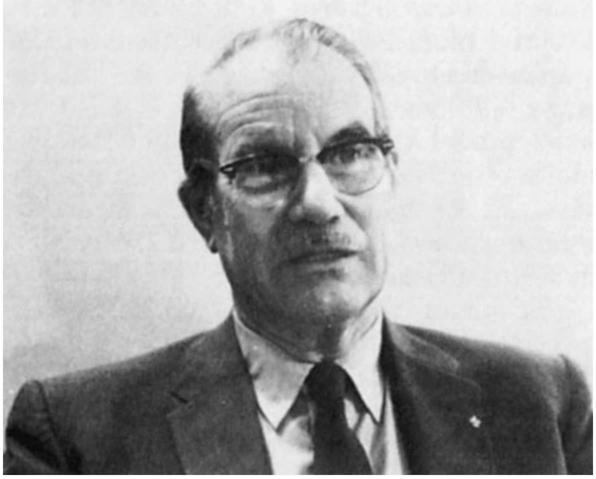
Piaget's theory of the development of cognition was known in America in the 1920s (Piaget, 1923). Yet, because of the "clinical" nature of his research methods, his non-statistical style of data analysis, the abstract constructs with which he was concerned, and his use of terms not then common in American psychological science (e.g., assimilation, operations)—all of which ran counter to predominant trends in the United States—his theory and research were not given much attention until the late 1950s (see Flavell, 1963). At that time, however, due to postwar European intellectual influences, Americans were turning greater attention to the intellectual resources in Europe. Thus, the Swiss scientist, Piaget, was rediscovered, and it can fairly be said that concern with the ideas and vocabulary involved in his theory came to dominate American developmental psychology throughout the 1960s.

Interest in adult development and aging also began to grow rapidly in the 1960s. As explained by Baltes (1979a), this interest provided a major impetus to the then renewed concern with development across the life span; studies of adult development and aging moved scientific interest beyond the childhood and, to some extent, the adolescent years that had been a major focus in the prior few decades. For instance, major research and theoretical contributions to the study of adult development and aging were provided by Bernice Neugarten (e.g., 1964, 1968; Neugarten & Gutmann, 1958; Neugarten, Havighurst, & Tobin, 1968) and Robert Havighurst (e.g., 1951, 1953, 1956; Havighurst, Neugarten, & Tobin, 1968) of the University of Chicago, who conducted longitudinal research beginning in the 1950s.


Jean Piaget

However, as Havighurst (1973) himself pointed out, this work had an intellectual debt to some earlier work done in the 1930s and 1940s. Except for one early work—an article by Sanford (in the *American Journal of Psychology*, 1902) called "Mental Growth and Decay"—interest in life-span changes and in researching the nature of life-span development did not really exist at all before the 1920s. In fact, except for Hall's (1922) text, *Senescence*, and a book by H. L. Hollingworth (1927), it was the 1930s that saw the growth of interests related to development across the entire life span. At this time Else Frenkel-Brunswik began a series of studies at the University of California (Berkeley) on the basis of an interest in life-span development; the work of Charlotte Bühler (1933) in Germany was published and began to become well known; and a book by Pressey, Janney, and Kuhlen (1939) was published. However, the scientists involved in these respective endeavors worked largely in isolation from one another, often unaware of (or at least not making reference to) the contributions of the others (see Baltes, 1979a).

Bernice L. Neugarten



Robert J. Havighurst

It was not until the 1950s, when the work of Neugarten and Havighurst really began, and the intellectual climate in the United States favored conceptual integration and pluralism, that these seeds of life-span interest really took hold. It was the fostering of research and theory in adult development and aging at that time that laid another portion of the foundation for the trends in human development seen in the decades following the 1950s and 1960s, trends that emphasized that multiple pathways of change exist across the life span (e.g., Lachman & James, 1997). Nevertheless, even before that period there was a long historical tradition behind the perspective that was labeled the life-span view of human development (Baltes 1979a, 1979b, 1987; Baltes et al., 1998; Baltes, Reese, & Lipsitt, 1980; Baltes et al., 1999).

Thus, by the 1960s, concern with development involved a focus on various theories of development, an interest in development into the adult and aged years, *and* a concern with internal and/or mental phenomena of development (e.g., the cognitive, or thinking, changes studied by Piaget and Vygotsky, or the emotional changes of interest to Sigmund Freud, Anna Freud, or Erik Erikson), and not only with overt, behavioral phenomena. Bronfenbrenner (1963), in a review of the history of developmental science, similarly notes that from the 1930s to the early 1960s there was a continuing shift from studies involving the mere collection of data toward research concerned with abstract processes and constructs. Some books and essays published during this period epitomized this trend by calling for the study of developmental processes or, using the parlance of the period, mechanisms (e.g., Harris, 1957; Spiker & McCandless, 1954). Accordingly, in depicting the status of the field in 1963, Bronfenbrenner said that, "first and foremost, the gathering of data for data's sake seems to have lost favor. The major concern in today's developmental research is clearly with inferred processes and constructs" (p. 257).

Similarly, in a review almost a decade later, Looft (1972) found a continuation of the trends noted by Bronfenbrenner. Looft's review, like Bronfenbrenner's, was based on an analysis of major handbooks of developmental psychology published from the 1930s through the time of his review. Each handbook represented a reflection of the current content, emphasis, and concerns of the field. Looft found that in the first handbook (Murchison, 1931) developmental science was largely descriptive. Consistent with my analysis and with Bronfenbrenner's conclusions, Looft saw workers devoting their time essentially to the collection of norms. However, a shift toward more general integrative concerns was seen by 1946, and this trend continued through 1963 (Bronfenbrenner, 1963) to 1972 (Looft, 1972). Indeed, as a case in point, I may note that the editor of the 1970, third edition of the *Handbook of Child Psychology*, Paul H. Mussen, pointed out that "the major contemporary empirical and theoretical emphases in the field of developmental psychology . . . seem to be on *explanations* of the psychological changes that occur, the mechanisms and processes accounting for growth and development" (Mussen, 1970, p. vii).

In commenting on Mussen's 1970 edition, William Damon (2006, pp. xiii–xiv), co-editor-in-chief of the sixth, 2006 edition of the *Handbook of Child Psychology* (with Richard M. Lerner), noted that:

> As for theory, Mussen's *Handbook* was thoroughly permeated with it. Much of the theorizing was organized around the approaches that, in 1970, were known as the "three grand systems": (1) Piaget's cognitive developmentalism, (2) psychoanalysis, and (3) learning theory. Piaget was given the most extensive treatment. He reappeared in the *Manual*, this time authoring a comprehensive (and, some say, definitive) statement of his entire theory, which now bore little resemblance to his 1931/1933 sortings of children's intriguing verbal expressions. In addition, chapters by John Flavell, by David Berlyne, by Martin Hoffman, and by William Kessen, Marshall Haith, and Philip Salapatek all gave major treatments to one or another aspect of Piaget's body of work. Other approaches were represented as well. Herbert and Anne Pick explicated Gibsonian theory in a chapter on sensation and perception, Jonas Langer wrote a chapter on Werner's organismic theory, David McNeill wrote a Chomskian account of language development, and Robert LeVine wrote an early version of what was soon to become "culture theory."
>
> With its increased emphasis on theory, the 1970 *Manual* explored in depth a matter that had been all but neglected in the book's previous versions: the mechanisms of change that could account for, to use Murchison's old phrase, "the problem of how the infant becomes an adult psychologically." In the process, old questions such as the relative importance of nature versus nurture were revisited, but with far more sophisticated conceptual and methodological tools.

In sum, a multiplicity of theories, and a concern with the explanation of the processes of development, came to be predominant foci by the beginning of the 1970s. Such concerns led to the recognition that there is not just one way (one theory) to follow in attempting to put together the facts (the descriptions) of development. Rather, a pluralistic approach to such integration was seen as needed. When followed, such integrations may indicate that more descriptions are necessary. Thus, although empirical observation is the basic feature of the *scientific method*, theoretical concerns guide descriptive endeavors. One gathers facts because one knows they will have a meaning within a particular theory. Moreover, since such theory-based research may proceed from any theoretical base, the data generated must be evaluated in terms of their use in advancing understanding of developmental change processes. These ideas burgeoned across the next three decades.

The 1970s, 1980s, and 1990s

The prominence of theory, the evaluation of theories by criteria of their usefulness in integrating the facts of development, and findings that developmental changes take many different forms at different points in time (and that such changes need to be understood from a diverse array of explanatory stances) led in the 1970s to an increasingly abstract concern with understanding the character of development. As a consequence, the decades of the 1970s, 1980s, and 1990s were characterized by the elaboration of numerous models of the association between the context of human life and the character of individual development.

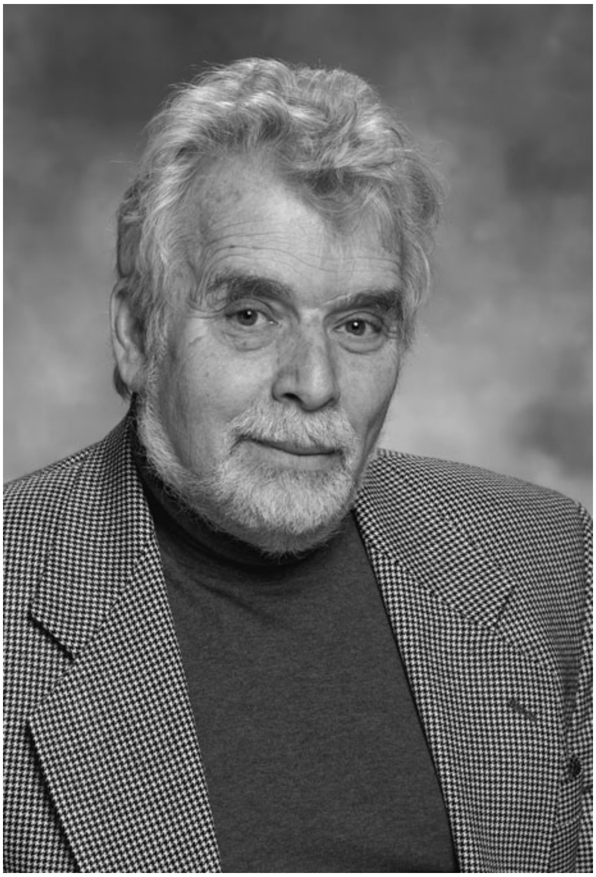
At the same time, these models of person–context relations were being developed as frames for actual research about the linkages between individuals and their complex, multi-tiered settings. This research served as both a product and a producer of the enhancement of theories of person↔context relations and of more nuanced understandings of the nature of the process through which human development was propelled by the associations individuals have with the ecology of human development.

Theory and Research about Infant Development

In the 1970s and 1980s, a major theoretical and empirical impetus for advances in the formulation of ideas pertinent to the growth of an RDS metatheoretical orientation to the study of life-span human development arose in the study of the first two years of life—infancy. The scholarship of Michael Lewis and of Michael Lamb provided key bases for these advances.

The work of Michael Lewis (e.g., 1972; Lewis & Feiring, 1978; Lewis & Lee-Painter, 1974; Lewis & Rosenblum, 1974; Pervin & Lewis, 1978) exemplifies the role that scholars of infant development played in devising models of person↔context relations and demonstrating their usefulness in research on human development. Building on the insights of Bell (1968) about the potential presence of bidirectional influences between parents and children in correlational data about socialization, Lewis and his colleagues launched a program of research that integrated model development with empirical research about infant–parent interaction.

For instance, in a book—*The Effect of the Infant on Its Caregiver* (Lewis & Rosenblum, 1974)—that represents a watershed event in the history of the study of human development through the use of person↔context RDS-based models—Lewis argued that "Not only is the infant or child influenced by its social, political, economic and biological world, but in fact the child itself influences its world in turn" (Lewis & Rosenblum, 1974, p. xv), and maintained that "only through interaction can we study, without distortion, human behavior" (Lewis & Lee-Painter, 1974, p. 21). In his research with Lee-Painter, Lewis provided data supporting the use of a flow model of interaction in understanding, for instance, sequences of exchanges


Michael Lewis

involving maternal and infant vocalizations as well as touch, looking, smiling, and play behaviors (e.g., Lewis & Lee-Painter, 1974, pp. 34–45).

Envisioning the relational, dynamic RDS-based models that would come to the fore in the study of human development by the end of the twentieth century, Lewis and Lee-Painter (1974) foresaw that:

> What we need to develop are models dealing with interaction . . . or with the interaction independent of the elements . . . This relational position not only requires that we deal with elements in interaction but also requires that we not consider the static quality of these interactions. Rather, it is necessary to study their flow with time . . . Exactly how this might be done is not at all clear. It may be necessary to consider a more metaphysical model, a circle in which there are neither elements nor beginnings/ends.
>
> (pp. 46–47)

Lewis himself continued across the ensuing quarter-century, and through this writing, to contribute theory and research that forwarded and empirically tested the dynamic models he envisioned in 1974 (e.g., Lewis, 1983, 1987, 1990, 1997, 2014, 2015). His scholarship fostered an intellectual climate among other infancy researchers to conceptualize phenomena of infant development within the sorts of dynamic person↔context relational models he championed.

One key instance of this influence arose in regard to the study of infant attachment. Here, the theory and research of Michael E. Lamb is a prime example of the use of person↔context relational models in the study of infant attachment. Lamb and his colleagues (e.g., Lamb, 1977a, 1977b, 1977c, 1978a, 1978b; Lamb, Thompson, Gardner, & Charnov, 1985; Thompson & Lamb, 1986) approached the study of infant attachment within the context of the assumptions that:

> 1. Children have an influence on their "socializers" and are not simply the receptive foci for socializing forces.
> 2. Early sociopersonality development occurs in the context of a complex family system rather than in the context of the mother–infant dyad.
> 3. Social and psychological development is not confined to infancy and childhood but is a process that continues from birth to death.
>
> (Lamb, 1978b, p. 137)

Within this conceptual framework, Lamb and his colleagues (e.g., Lamb et al., 1985) found that prior interpretations of infant attachment, which included "an emphasis on the formative significance of early experiences, a focus on unidirectional influences on the child, a tendency to view development within a narrow ecological context, and a search for universal processes of developmental change" (Thompson & Lamb, 1986, p. 1), were less powerful in accounting for the findings of attachment research than an interpretation associated with the sorts of person↔context relational models burgeoning during the 1970s and 1980s. Accordingly, in a review of attachment research conducted through the mid-1980s, Lamb and his colleagues concluded that "reciprocal organism–environment influences, developmental plasticity, individual patterns of developmental change and broader contextual influences on development can better help to integrate and interpret the attachment literature, and may also provide new directions for study" (Thompson & Lamb, 1986, p. 1).

Lamb's work challenged the field of infancy to study the early years of life, but not through the use of narrow conceptions of the exclusive influences of heredity or early experiences or through the use of simplistic views of proximal dyadic relationships acting in isolation from the fuller and richer ecology of human development. Instead, he provided a vision for the understanding of infancy as part of the entire life span of the individual and of all of the other people in the infant's world. His theoretical vision and empirical productivity provided developmental science with compelling bases for appreciating this complex set of social interactions as reciprocal exchanges in, and with, a multilevel and dynamic context (e.g., see Lamb, 1977a, 1977b, 1977c, 1978a, 1978b; Lamb et al., 1985).

In fact, and as was the case with Michael Lewis (e.g., 1997, 2014, 2015), Michael Lamb continued in the subsequent decades and through this writing to contribute research that studied infant development within the context of a dynamic, person↔context relational model (e.g., Campbell, Lamb, & Hwang, 2000; Lamb, 1998, 2000; Lamb, Malloy, Hershkowitz, & La Rooy, 2015). In essence, then, stimulated by scholars of infancy such as Michael Lewis and Michael Lamb, the study of human development during the 1970s and 1980s became increasingly focused on developing models, and conducting research, that enabled understanding of interactions, reciprocal influences, or bidirectional relations between individuals and the complex contexts within which they developed.

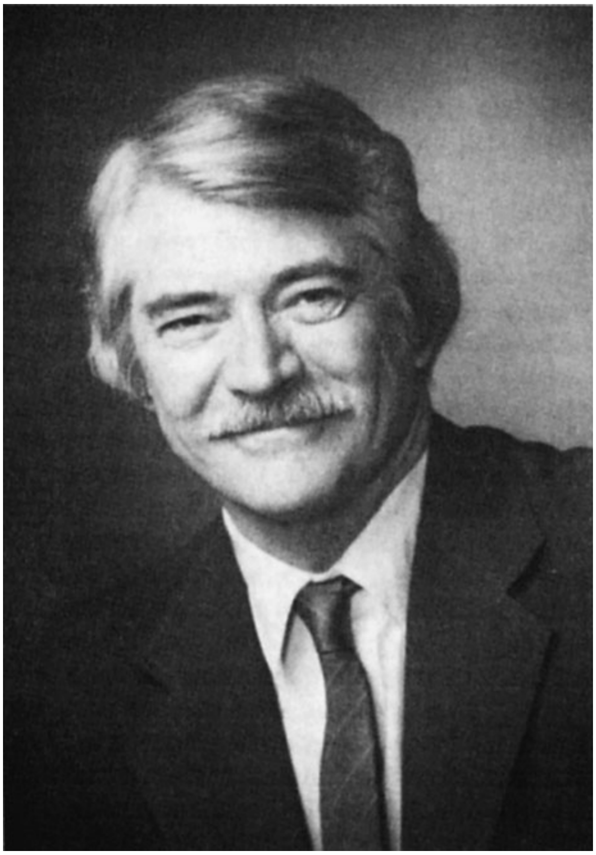
Damon (2006, p. xiv) characterized these trends, as they were represented in the 1983, fourth edition of the *Handbook of Child Psychology* (Mussen, 1983):

> The grand old theories were breaking down. Piaget was still represented by his 1970 piece, but his influence was on the wane throughout the other chapters. Learning theory and psychoanalysis were scarcely mentioned. Yet the early theorizing had left its mark, in vestiges that were apparent in new approaches, and in the evident conceptual sophistication with which authors treated their material. No return to dustbowl empiricism could be found anywhere in the set. Instead, a variety of classical and innovative ideas were coexisting: Ethology, neurobiology, information processing, attribution theory, cultural approaches, communications theory, behavioral genetics, sensory-perception models, psycholinguistics, sociolinguistics, discontinuous stage theories, and continuous memory theories all took their places, with none quite on center stage. Research topics now ranged from children's play to brain lateralization, from children's family life to the influences of school, day care, and disadvantageous risk factors. There also was coverage of the burgeoning attempts to use developmental theory as a basis for clinical and educational interventions. The interventions usually were described at the end of chapters that had discussed the research relevant to the particular intervention efforts, rather than in whole chapters dedicated specifically to issues of practice.

Accordingly, in order to understand the nature of changes in concepts and theories of human development that occurred from the 1970s through the 1990s, and how these changes resulted in a focus by the late 1990s and into the beginning of the twenty-first century on elaborating RDS-based theories of human development, it is important to focus on both the theoretical and empirical work conducted during these decades.

The Role of Philosophical Models

Reese and Overton (1970; Overton & Reese, 1973), among others (e.g., Lerner 1976, 1978; Lerner & Kauffman, 1985, 1986; Riegel, 1975), pointed out that just as the facts and methods of science are to be understood as shaped by theory, scientific theories, in turn, are shaped by superordinate philosophies. Throughout the 1970s repeated discussions



Hayne W. Reese

occurred about how two major philosophical positions, the mechanistic and the organismic ones, shaped developmental theories (e.g., Lerner, 1976, 1978, 1979; Overton, 1973; Overton & Reese, 1973; Reese & Overton, 1970; Riegel, 1975, 1976a, 1976b; Sameroff, 1975). Each of these philosophical positions led to a different set, or "family," of theories.

For example, many mechanistic-type theories emphasized that even quite complex levels of human behavior can be reduced to rather simple elements: basic stimulus–response (S–R) connections acquired through the "laws," or principles, of classical and operant conditioning (Baer, 1970, 1982; Bijou, 1976; Bijou and Baer, 1961; Skinner, 1938, 1950, 1971). Other mechanistic theories (e.g., Plomin, 1986; Rowe, 1994) sought to reduce social phenomena (e.g., parent–child relations, socialization) and psychological functioning (e.g., personality attributes, temperament style, or intelligence) to genetic inheritance (that is, to the complement of genes received at conception, the *genotype*).

In turn, many organismic-type theories emphasized that, as people develop, they pass through a universal and unchangeable sequence of qualitatively different phases, levels, or "stages," of development (e.g., Erikson, 1959, 1963, 1968; Freud, 1949, 1954; Piaget, 1950, 1970). Since each stage of development is different in kind from all others, organismically oriented developmental scientists disagreed with mechanistically oriented ones about the appropriateness of reducing different levels (e.g., society, the family, and the individual) or different stages (e.g., the sensorimotor, preoperational, concrete operational, and formal operation stages posited by Piaget, 1960, 1970) to either one level (e.g., that of biology or, more specifically, genes) or a common set of elements (e.g., stimulus–response connections formed through the "laws" of classical and operant conditioning), respectively.

The discussions prompted by the work of Reese and Overton (1970; Overton & Reese, 1973) involved, as well, consideration of the "family of theories" associated with each view. Although there are differences among family members (for example, Freud, in his organismic theory, emphasized emotional and personality development whereas Piaget, in his organismic theory, emphasized cognitive development), there is greater similarity among the theories within a family (e.g., the common stress on the qualitative, stage-like nature of development) than there is between theories associated with different families (e.g., mechanistically oriented behavioristic theorists, such as Bijou and Baer (1961), would deny the importance, indeed the reality, of qualitatively different stages in development).

Due to the philosophically-based differences between families of theories derived from the organismic and the mechanistic conceptions, the 1970s, 1980s, and the early 1990s involved several discussions about the different stances held by members of one or another theoretical "family" regarding an array of key conceptual issues of development. Examples are the nature and nurture bases of development (Lehrman, 1970; Lerner, 1978; Overton, 1973); the quality, openness, and continuity of change (Brim & Kagan, 1980b; Looft, 1973); appropriate methods for studying development (Baltes, Reese, & Nesselroade, 1977); and ultimately, the alternative truth criteria for establishing the "facts" of development (Dixon & Nesselroade, 1983; Reese & Overton, 1970).

This awareness of the philosophical bases of developmental theory, method, and data contributed to the consideration of additional models appropriate to the study of psychological development. In part, this consideration developed as a consequence of interest in integrating assumptions associated with theories derived from organismic and mechanistic models (Looft, 1973). For instance, Riegel (e.g., 1975, 1976a, 1976b) attempted to apply a historical model of development that seemed to include some features of organicism (e.g., the active organism) and some features of mechanism (e.g., the active environment). In turn, Riegel's interest in continual, reciprocal relations between an active organism and its active context (and not in either element per se), and the concern with these relations as they exist on all levels of analysis, formed a basis for his proposing a dialectical model of human development (Riegel, 1975, 1976a, 1976b).

Indeed, other developmental scientists, focusing too on the implications for theory of viewing distinct levels of analysis as reciprocally interactive, proposed related models, ones termed transactional (Sameroff, 1975, 1983), relational (Looft, 1973), or developmental contextual (Lerner, 1978, 1984, 1986).

Sowing the seeds of what would become, by the late 1990s, RDS-based models, this philosophically-driven interest in bidirectional organism–context relations led several theorists to explore the application of a change-oriented contextual model to the collection and interpretation of developmental (and other psychological) data (see especially the volumes on contextualism edited by Hayes, Hayes, Reese, & Sarbin, 1993, and by Rosnow & Georgoudi, 1986).

The discussions about the influence of the organismic and the mechanistic conceptions led developmental psychologists to recognize that the stances scientists took in regard to key issues of human development—such as whether, because of the appropriateness of reducing all behavior to common elements, there is a sameness, or continuity, across life *or* whether, because of the existence of new stages, there is change, or discontinuity, across life—depended ultimately on philosophical positions. That is, developmental scientists recognized that a main (if not the ultimate) reason scientists had different positions regarding concepts and theories of development was that they were committed to different philosophies (e.g., see Kuhn, 1962, 1970; Overton, 1998, 2006, 2015a). In other words, differences about these issues were underlain by non-empirical, philosophical differences and could not therefore be readily decided on the basis of data. Indeed, Reese and Overton (1970; Overton & Reese, 1973) pointed out that developmental scientists working from different philosophical positions would have different truth criteria for establishing the "facts" of development, because what is a fact to one scientist may not be accepted as a legitimate or relevant fact by another. As a consequence, because of basic philosophical disagreements, disputes *across* philosophical positions could not be settled by facts.

In short, the interest that arose in the 1970s and that developed across the next two decades in the philosophical bases of theories of development also led many developmental scientists to explore the potential use of philosophies other than the organismic and the mechanistic. The considerations of these ideas resulted in revised ways of thinking about the linkages between the developing individual and his or her changing context.

It is useful here to consider three significant instances of the theoretical models of human development that emerged from scholars' efforts to devise new, and more integrative, ways of thinking about individual–context relations. These instances of human development theory—the dialectical view championed by Klaus Riegel, the bioecological view developed by Urie Bronfenbrenner, and the life-span perspective developed by Paul B. Baltes, John R. Nesselroade, K. Warner Schaie, and their colleagues—continued to develop across the 1990s and, as well, into the first decade of the twenty-first century (e.g., Baltes, Lindenberger, & Staudinger, 1998, 2006; Bronfenbrenner, 2005; Bronfenbrenner & Morris, 1998, 2006). Together, the influence of their continued elaboration was to help crystallize the emphasis in developmental theory on understanding individual–context relations in relation to integrative, RDS-based models (Lerner, 1998a, 2006b).

The Dialectical Model

In many ways, Klaus F. Riegel (1975, 1976a, 1976b) was both the intellectual leader of and catalyst for the exploration in the 1970s of the use of alternative models for the study of human development. This influence was the case, first, because he was a prolific and passionate writer—his book, *Psychology Mon Amour: A Countertext* (Riegel, 1978), being an excellent case in point—and, second, because he was editor of the journal *Human Development*, the prime outlet for theoretical scholarship in the field of human development.

Of the many important contributions of Riegel's scholarship, two are particularly pertinent to the present discussion. First, his dialectical model emphasized that the primary goal of a developmental analysis was the study of change, not stasis. Second, his model emphasized that any level of organization—from inner-biological, through individual-psychological and physical-environmental, to the sociocultural—influences and is influenced by all other levels. Thus, Riegel (1975, 1976a, 1976b) "developmentalized" and "contextualized" the study of the person by embedding the individual within an integrated and changing matrix of influences derived from multiple levels of organization. The comparability between these ideas and

Klaus Riegel

Overton's (2015a) concept of embodiment, discussed in Chapter 2, seems evident.

Riegel (1973, 1975, 1976a, 1976b) proposed that dialectical philosophy could be used to devise a unique theory of development, one that did not focus on the organism (and, for instance, its genes or its maturationally-guided progression through stages) or just on the environment (as, for instance, the source of the stimulation that provided the basis of S–R connections). Instead, Riegel (1975, 1976a, 1976b) hoped to forge a dialectical psychology that focused on the *relations* between developing organisms and their changing environments. Riegel emphasized that such relations involved continual conflicts among variables from several levels of "being" (or levels of organization of life phenomena). For example, he assumed that development involved constant changes among the multiple, reciprocally related inner-biological, individual-psychological, physical-environmental, and sociocultural levels of analysis.

Riegel's model of dialectic development was an important instance of the growing interest during this period in the interactive role of the changing physical and social context for human behavior and development. Riegel's ideas, and those of Sameroff (1975), Looft (1973), Lerner (1978, 1979), and others (e.g., Bronfenbrenner, 1977, 1979), were similar in their emphasis on change and context—and, to this extent, may be interpreted as being part of a common "family" of models. At this writing, these ideas are clearly linked to RDS metatheory (see Chapter 2). However, in the context of the 1970s and early 1980s what was clear was perhaps only that, as scholarship about this family of theories advanced, there were important distinctions among family members.

For instance, Riegel's (1975, 1976a, 1976b) ideas on context and change differed from those of other family members with respect to the format of change. The nature of dialectical change, which is always in the same direction, that of a synthesis between two "conflicting" opposites (termed thesis and antithesis), may be more compatible with the view of change found in organicism than that of the philosophical position termed contextualism (Dixon, Lerner, & Hultsch, 1991a, 1991b; Pepper 1942). Contextualism promotes a view of change that is dispersive, that is, that can occur in innumerable directions (Pepper, 1942). On the other hand, organismic change is always unidirectional; it is directed to a single endpoint or goal (Pepper, 1942). Thus, when applied to the life span, Riegel's (1976b) dialectical view may have had more in common with organismic views (e.g., Alexander & Langer, 1990; Chapman, 1988a, 1988b; Piaget, 1970) than with contextual ones.

To counter this criticism, Riegel (1976b) tried to argue that dialecticism constituted a model of development distinct from organicism. In his view, the dialectical theory of cognitive development differed from the one of Piaget (1950, 1970). For example, whereas Piaget proposed that after the development of the last stage of development in his theory—a stage he termed "formal operations"—no new cognitive structure emerged, Riegel argued that the dialectic resulted in a fifth, open-ended stage of cognitive development. However, given that both the organismic model of Piaget and Riegel's dialectical model emphasized a single format and direction for developmental change, it was difficult for Riegel to maintain that at its core, in regard to the character of the main process of developmental change, the two positions were different.

Moreover, Riegel did not attend to the similarities and differences between his dialectical model and theories that emphasized the contextual philosophy or world view (Pepper, 1942), although both sets of ideas emphasized change through individual–context relations. Given its problem with discriminating itself from organicism and the availability of a model for theory building—contextualism—which afforded a different, and more plastic view of change, the dialectical model of Riegel did not remain a conception of prime focus among developmental scholars beyond the 1970s and early 1980s. Nevertheless, attention to Riegel's ideas did facilitate the interest of the community of developmental scholars in considering other theoretical models of change through individual–context relations. Thus, at least in this respect, his dialectical model can be seen as compatible with the attention paid during these decades to contextualism (Hultsch & Hickey, 1978; Lerner & Kauffman, 1985, 1986; Lerner, Skinner, & Sorell, 1980).

In contextualism, developmental changes occur as a consequence of reciprocal (bidirectional) relations between the active organism and the active context. Just as the context changes the individual, the individual changes the context. As such, by acting to change a source of their own development—by being both products and producers of their context—individuals affect their own development (Bell, 1968; Bell & Harper, 1977; Lerner, 1982; Lerner & Busch-Rossnagel, 1981b; Lewis & Rosenblum, 1974; Schneirla, 1957).

Contextualism found many adherents among developmental scientists across the 1970s to (at least the early) 1990s (Lerner, Hultsch, & Dixon, 1983), as well as many critics (e.g., see Capaldi & Proctor, 1999, for a review, and see Kendler, 1986, as an example). Nevertheless, because of the potential to provide ideas that could more usefully understand (e.g., account for more variance pertinent to) the dynamic (that is, the multilevel and bidirectional) relationships between the developing individual and variables associated with his or her biological, interpersonal, societal, cultural, and historical contexts, developmental scholars continued to explore the use of models of person–context relations associated with contextualism, if not specifically Riegel's dialecticism. Two major examples of such approaches were the bioecological model of human development (Bronfenbrenner, 1977, 1979) and the life-span developmental psychology perspective (e.g., Baltes et al., 1980). Both of these theoretical approaches to the relations between individuals and their contexts are instances of ideas that evolved from the contextualism of the 1970s and 1980s into the relationism associated by the late 1990s with concepts linked to RDS metatheory (see Chapter 10).

Bronfenbrenner's Bioecological Model of Human Development

The leading formulator of the bioecological approach to human development was Urie Bronfenbrenner (1977, 1979, 1983, 2005; Bronfenbrenner & Crouter, 1983; Bronfenbrenner & Morris, 1998, 2006). Bronfenbrenner argued that much of developmental research involved studying children under artificial "experimental" conditions. Thus, he argued, "Much of contemporary developmental psychology is the science of the strange behavior of children in strange situations with strange adults for the briefest possible periods of time" (1977, p. 513). Accordingly, Bronfenbrenner asserted that only "experiments created as real are real in their consequences" (1977, p. 529), and he emphasized that research should begin to focus on how children develop in settings representative of their actual world (i.e., in *ecologically valid* settings; Brunswik, 1955; Lerner & Callina, 2014b). For instance, instead of studying children only in the laboratory, a developmental scientist should study them in their homes, schools, and playgrounds.

Bronfenbrenner (1979; Bronfenbrenner & Morris, 2006) also argued that developmental scientists needed a much more precise and differentiated view of the actual ecology of human development. Viewing the features of a person's context as representing merely the "stimulus environment" was not sufficient. The context of human development was composed of different levels, or systems, of organization; although the systems were interrelated, often in a reciprocal manner, they were nevertheless sufficiently distinct to necessitate discrimination among them.

As I will discuss in greater detail in Chapter 10, Bronfenbrenner (1977, 1979) proposed four systems within the ecology of human development. The first system he labeled the *microsystem*, and he noted that this portion of the context is composed of "the complex of relations between the developing person and environment in an immediate setting containing the person" (Bronfenbrenner, 1977, p. 515). For example, the family is one major microsystem for infant and child development. It involves interactions between the child, his or her parents, and any siblings that are present in the home. Other microsystems of early life include the daycare, nursery, or school setting, involving both child–teacher and child–peer interactions, and the playground, most often involving child–peer interactions.

A child's microsystems may be interrelated. What occurs in the school may affect what happens in the family, and vice versa. Bronfenbrenner noted that such microsystem interrelations constitute a second ecological system. He termed this set of interrelations the *mesosystem*, and he defined it as "the interrelations among major settings containing the developing person at a particular point in his or her life" (Bronfenbrenner, 1977, p. 515).

Often, what happens in a microsystem (e.g., in a relationship between a child and a parent within the family context) may be influenced by events that occur in systems in which the child takes no part. Bronfenbrenner sees such influences as constituting a third system within the ecology of human development. He labels this system the exosystem, and he defines it as "an extension of the mesosystem embracing . . . specific social structures, both formal and informal, that do not themselves contain the developing person but impinge upon or encompass the immediate settings in which the person is found, and thereby delimit, influence, or even determine what goes on there" (Bronfenbrenner, 1977, p. 515).

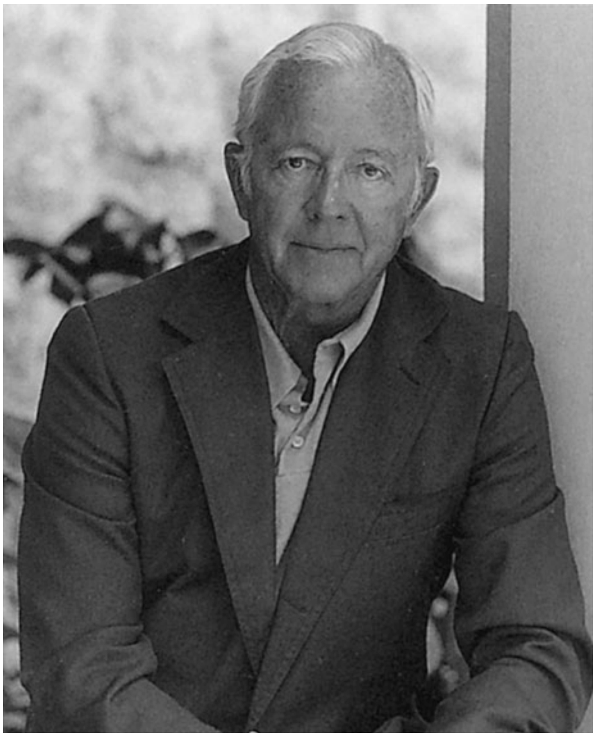
Finally, Bronfenbrenner notes that there exists a *macrosystem* within the ecology of human development. This system is composed of historical events (e.g., wars, floods, famines) that may affect the other ecological systems, as well as cultural values and beliefs that influence the other ecological systems. Natural disasters may destroy the homes, schools, or other microsystems of a person or a group of developing people, and/or they may make certain necessities of life (e.g., food, fresh water) less available. Cultural values can influence the developing child in many ways. For example, cultural beliefs about the appropriateness of breastfeeding, and about when weaning from the breast should occur, can affect not only the nutritional status of the child, but because mother's milk may make some children less likely to develop allergies later in life, it can also affect their health status. Values about childrearing, and indeed the value or role of children in society, can affect the behaviors developed by a child (e.g., see Baumrind, 1971, 1972) *and* can even have implications for whether the child survives.

Bronfenbrenner's (1977, 1979) bioecological model evolved across the subsequent three decades (e.g., Bronfenbrenner & Morris, 1998, 2006). I will return to this point in Chapter 10, and discuss ideas such as the chronosystem and the process–person–context–time model within bioecological theory (e.g., Bronfenbrenner & Morris, 2006). Here, however, it is useful to discuss briefly a second theoretical approach that burgeoned in the 1970s.

The Life-Span Developmental Perspective

A second major instance of a contextually-oriented perspective that became increasingly prominent during the 1970s was labeled the life-span developmental perspective or the life-span view of human development (Baltes, 1979b; Baltes et al., 1980; Lerner, Hultsch, & Dixon, 1983). The major formulators of this perspective were Paul B. Baltes, K. Warner Schaie, John R. Nesselroade, Hayne W. Reese, and Orville G. Brim, Jr. This perspective emphasizes the potential for systemic change across life (plasticity; Lerner, 1984) and sees this potential as deriving from reciprocal influences, of people on their contexts as well as of contexts on people, that is, individual↔context relations. As noted in Chapter 2, both of these ideas are linked to RDS metatheory. As such, it is not surprising that, in the conceptual evolution that took place in the 1990s and into the twenty-first century, the life-span approach was seen as an instance of an RDS-based model.

K. Warner Schaie



Orville G. Brim, Jr.

As Baltes (1979b, p. 2) indicated, there are two rationales for the emphasis in the life-span approach on person↔context relations as central to developmental change:

> One is, of course, evident also in current child development work. As development unfolds, it becomes more and more apparent that individuals act on the environment and produce novel behavior outcomes, thereby making the active and selective nature of human beings of paramount importance. Furthermore, the recognition of the interplay between age-graded, history-graded, and nonnormative life events suggests a contextualistic and dialectical conception of development. This dialectic is further accentuated by the fact that individual development is the reflection of multiple forces that are not always in synergism, or convergence, nor do they always permit the delineation of a specific set of endstates.

Based on this view of developmental change, the life-span developmental view (Baltes et al., 1998, 2006; Baltes, Reese, & Lipsitt, 1980) became crystallized as a set of interrelated ideas about the features of human development.

In their combination, these ideas present a set of implications for theory building, for methodology, and for scientific collaboration across disciplinary boundaries. Among the key ideas emanating from this perspective are that, across life, development involves the integration of gains–losses (e.g., humans gain facility in their native language but, with development, lose the capacity to be as fluent as a "native" speaker in other languages), embeddedness (individual development occurs within a social, physical, cultural, and historical context, with which it is reciprocally related), and plasticity (due to the relations between individuals and this complex context there are multiple pathways, or directions, that developmental change may take; Baltes, 1987). From these propositions an interrelated set of implications may be derived, and these propositions and implications constitute the key concepts in life-span thinking (Baltes et al., 1998, 2006).

Notably, the life-span perspective was often associated with a call for interdisciplinary research in human development (Dixon & Lerner, 1999). That is, attempts were made to integrate ideas from the many disciplines involved in the study of human lives (e.g., anthropology, biology, and sociology). This integration was important because change across life occurs on multiple levels (e.g., biological, psychological, social), and changes on one level may influence changes on other levels. Although the life-span perspective forwarded by Baltes, Nesselroade, and Schaie was often associated with scholarly publications appearing in the late 1960s (e.g., Baltes, 1968; Schaie, 1965; Schaie & Strother, 1968), several historical analyses (e.g., Baltes, 1979a, 1983; Dixon & Lerner, 1999; Dixon & Nesselroade, 1983; Havighurst, 1973; Müller-Brettel & Dixon, 1990) identified salient earlier contributions from both Europe and the United States. As I have noted, examples are Bühler (1933), Sanford (1902), Hall (1922), Havighurst (1948), Hollingworth (1927), Neugarten (1964), and Pressey, Janney, and Kuhlen (1939).

In turn, and as I indicated in Chapter 2, during the 1970s and 1980s the life-span perspective was advanced significantly by a series of conferences at West Virginia University on conceptual, methodological, and empirical issues (e.g., Baltes & Schaie, 1973; Datan & Ginsberg, 1975; Datan & Reese, 1977; McCluskey & Reese, 1984; Nesselroade & Reese, 1973). In addition, a series of volumes on life-span research (e.g., Baltes, 1978; Baltes & Brim, 1984; Baltes, Featherman, & Lerner, 1986; Featherman, Lerner, & Perlmutter, 1994) also contributed to advancing concepts and data pertinent to the life-span view of human development.

In sum, from the life-span perspective, the potential for developmental change is seen to be present across all of life; the human life course is held to be potentially multidirectional and necessarily multidimensional (Baltes et al., 1980, 2006). In addition, the sources of the potentially continual changes across life are held to involve both inner-biological and outer-ecological levels of the context within which the organism is reciprocally embedded. In short, the growth of the life-span developmental perspective in the 1970s and 1980s led, by the 1990s, to a view of human development that suggested that individual changes across life are both products and producers of the multiple contextual levels in which a person is embedded or, in Overton's (2015) terms, embodied.

Other Instances of Contextual Theorizing in the 1970s, 1980s, and 1990s

In addition to the bioecological and the life-span perspectives, other quite important instances of the influence of contextual thinking arose in the 1970s. Coming from a remarkably diverse array of intellectual traditions, these instances suggested that contextualism both offered a conceptual framework for asking ecologically meaningful questions and suggested methodological strategies for doing new and potentially more useful empirical research.

For example, in 1974 James J. Jenkins rejected the mechanistic model he had used to guide his associationist view of memory. He suggested that instead of this traditionally American approach to the study of memory, a contextual approach be adopted (Jenkins, 1974). He argued that "what memory is depends on context" (Jenkins 1974, p. 789) and defended this view by presenting the results of several empirical studies that demonstrated that:

> What is remembered in a given situation depends on the physical and the psychological context in which the event was experienced, the knowledge and skills that the subject brings to the context, the situation in which we ask for evidence for remembering, and the relation of what the subject remembers to what the experimenter demands.
>
> (Jenkins, 1974, p. 793)

Jenkins (1974, p. 787) noted that, to deal adequately with all these sources of variation, means that "being a psychologist is going to be much more difficult than we used to think it to be." In part, this difficulty arises because there is no one mode of analysis, or methodological strategy, that suggests itself as always useful for assessment of all the levels of analysis involved at all historical moments in the memory process. Thus, not only is methodological pluralism promoted from this contextual perspective, but the criterion of usefulness must also be employed when deciding if a particular methodological strategy is appropriate. That is, reflective of the specificity principle in Chapter 2 (Bornstein, 2006, 2017), a scientist must decide: "What kind of an analysis of memory will be useful to you in the kinds of problems you are facing. What kinds of events concern you?" (Jenkins, 1974, p. 794). In other words, Jenkins (1974, p. 794) believes that:

> The important thing is to pick the right kinds of events for your purposes. And it is true in this view that a whole theory of an experiment can be elaborated without contributing in an important way to the science because the situation is artificial and nonrepresentative in just the senses that determine its peculiar phenomena. In short, contextualism stresses relating one's laboratory problems to the ecologically valid problems of everyday life.

Thus Jenkins (1974) reaches a conclusion quite compatible with the one Bronfenbrenner (1977) reached. Clearly, the "spirit of the times" (the *zeitgeist*) in the 1970s set social and behavioral science on an intellectual course that prized the ecological validity of theory-predicated research.

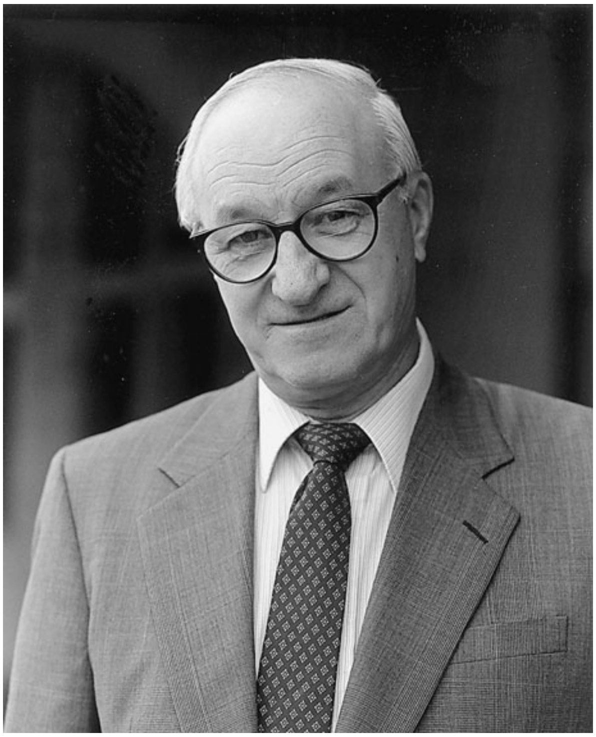
In addition, Sarbin's (1977) dramaturgical model of psychological functioning had marked similarity to Riegel's (1975, 1976a, 1976b) dialectical model, as well as to features of the life-span perspective. This model is a technique that, through use of the notion of emplotment, attempts to capture the sequence of reciprocal events between individuals and their changing social contexts. Sarbin (1977) applies his contextualism model to the analysis of data sets pertinent to the genesis of schizophrenia, to the nature of hypnosis, and to the characteristics of imagination, in order to illustrate the integrative utility of contextually derived ideas. His work illustrated that contextual ideas can be useful in understanding an array of psychological processes, ranging from those associated with cognition and affect to those traditionally labeled as personality and social ones. Moreover, Sarbin emphasized that the interrelation among processes cannot only be integrated by contextual thinking but, in fact, needs to be appreciated if both adaptive and non-adaptive outcomes of person–context relations are to be understood. For example, Sarbin suggested that in the understanding of the bases of schizophrenia, the contextualist will, as compared to the mechanist, take:

> as his unit, not schizophrenia, not improper conduct, not the rules of society, but as much of the total context as he can assimilate. His minimal unit of study would be the man who acted as if he believed he could travel unaided through space *and* the person or persons who passed judgment on such claims.
>
> (Sarbin, 1977, p. 25)

Thus, as in Riegel's (1976a) model of crises being generated by conflicts among different developmental levels, Sarbin (1977) searched for the bases of adaptive and maladaptive functioning *not* within the realm of individual ("personological") functioning, but rather within the domain of the conflicts and crises created by the degrees of "goodness of fit" (Thomas & Chess, 1977) a person experiences in his or her relations with the social context. Sarbin also sees the relevance of his ideas to those put forth in other calls for contextualist thinking. In fact, Jenkins (1974), as well as Cronbach (1975) and Gergen (1973), made consonant appeals.

Indeed, these latter two papers are not the only instances of appeals for contextualism in the 1970s; other prominent examples may be cited. The *American Psychologist* is the journal of the American Psychological Association, designed to publish articles of current and broad interest to psychologists. The articles by Jenkins (1974), Bronfenbrenner (1977), and Riegel (1976a) were published in the *American Psychologist*, and in the last three years of the 1970s three additional papers appeared in the *American Psychologist* that, in different ways, made an appeal for contextualism. Walter Mischel (1977), arguing for considering the role of context in understanding personality, suggested that, unless one considered the changing—and bidirectional—relations between people and their worlds, an adequate understanding of consistency and change in the person could not be attained. Petrinovich (1979) promoted "probabilistic functionalism"—an idea drawn from Egon Brunswik's (1955) notion of ecological validity—which called for an array of methodological strategies not dissimilar in intent to those suggested in calls for methodological pluralism put forth by contextual thinkers such as Bronfenbrenner (1977) and Jenkins (1974), among others (e.g., Lerner, Skinner, & Sorell, 1980; and see too Lerner & Callina, 2014b). Most interestingly, Albert Bandura (1978) reconceptualized his social-learning theory as involving causal processes that are based on reciprocal determinism. That is, consistent with key emphases in contextualism, Bandura asserted that, "from this perspective, psychological functioning involves a continuous reciprocal interaction between behavioral, cognitive, and environmental influences" (Bandura, 1978, p. 344).

Interestingly, as interest in contextualism grew in the 1970s and 1980s, the shortcomings of a completely contextual approach to human development became clearer as well (e.g., Overton, 1998). Based on Pepper's (1942) assertion that contextualism is a completely dispersive world hypothesis and, as such, provided no necessary systematicity or organization to successive changes across life (e.g., see Lerner & Kauffman, 1985), many developmental scientists began to seek ways to interrelate contextualism with organicism (e.g., Overton, 1984, 1991a, 1991b, 1994a). These conceptual explorations resulted in an intellectual movement from contextualism to RDS-based ideas (Overton, 1998, 2003, 2010). In many cases, the substantive focus for this work was individual↔context relations.



Albert Bandura

The Study of Individual↔Context Relations

As noted earlier, one basis of the development during the 1970s and 1980s of the interest in contextual ideas about human development was the empirical findings pertinent to person–context relations that were generated during this period. These findings were both products and producers of person↔context developmental models because they proved quite problematic to interpret when viewed from extant organismic- or mechanistic-derived theories. As a consequence, scholars sought to evaluate the use of dialectical and/or contextual philosophical, or metatheoretical, models for such interpretations.

For example, Brim and Kagan (1980b) reviewed evidence about the influence of early experience on the life course. To illustrate the sort of findings discussed by Brim and Kagan (1980b), I may note the research of Schaie (1979), which reported that the direction of age changes in intellectual aging is related to variables associated with birth cohort membership. Members of one birth cohort might show negatively accelerated changes in levels of cognitive abilities during their aged years; another cohort might show stability in these abilities during this period; and still another cohort might show positively accelerated growth in abilities during their aged years. The particular pattern depended on educational and pedagogical variables present in the context of a given cohort during the specific time in history when its members were educated.

Moreover, data were generated during this period that suggested that contextual variables exist which differentiate people born at given times in history (e.g., economic crises; Elder, 1974, 1999). As a consequence of this differentiation, these contextual

Jerome Kagan

variables might influence the particular direction of individuals' ontogenetic changes. In addition, there also may be contextual variables, present only at specific times of measurement, which may "cut across" cohorts and influence the direction of change of people from different cohorts. For instance, Nesselroade and Baltes (1974) studied about 1,800 West Virginia male and female adolescents in 1970, 1971, and 1972. These adolescents were from birth cohorts 1954 to 1957, and thus ranged in age at the time of first measurement from 13 to 16 years. Personality questionnaires and measures of intelligence were administered to these youth. Contrary to what is emphasized by those theorists who focused on personological components of adolescent development (for example, Anna Freud, 1969), Nesselroade and Baltes found that change at this time of life was quite responsive to sociocultural-historical influences. In fact, age by itself was not found to be a very influential contributor to change. Rather, for these groups of adolescents, developmental change was influenced more by cultural changes over the historical period than by age-related sequences. For instance, adolescents as a whole, despite their age or birth cohort, decreased in "superego strength," "social-emotional anxiety," and achievement during the 1970–1972 period. Moreover, most adolescents, regardless of age or cohort, increased in independence during this period.

Accordingly, the Nesselroade and Baltes (1974) data showed that it was the time at which all these differently-aged adolescents were measured that was most influential in their changes. Perhaps due to the events in society of that time—for example, events associated with the Vietnam War—all adolescents scored similarly in regard to these personality characteristics. Despite where they were (i.e., their age) upon "entering" the 1970–1972 historical era, members of different cohorts changed in similar directions, due presumably to events surrounding them at the times they were tested.

Perhaps the best example of how the changing social context provides a basis of individual development was derived from Elder's (1974, 1999) longitudinal study of the development of people who were children and adolescents during the Great Depression in the United States. Elder reported that among a group of 84 males and 83 females born in 1920–1921, characteristics of the historical era produced alterations in the influence of education on achievement, affected later adult psychological health for youth from working-class families suffering deprivation during this era, and enhanced the importance of children in later adult marriages for youth who suffered hardships during the depression (see too Elder et al., 2015).

Other components of a person's context which can influence individual development are the physical and social characteristics of the school environment. Indeed, Simmons, Rosenberg, and Rosenberg (1973) found that changes in the school context may influence personality. In a study of about 2,000 children and adolescents, they found that, in comparison to 8- to 11-year-old children, young adolescents—and particularly those 12 and 13 years of age—showed more self-consciousness, greater instability of self-image, and slightly lower self-esteem. However, they discovered that contextual- rather than age-associated effects seemed to account for these findings. Upon completion of the sixth grade, one portion of the young-adolescent group had moved to a new school—that is, a local junior high school—whereas the remaining portion of the adolescents stayed in the same school (which offered seventh- and eighth-grade classes). The group of young adolescents who changed their school setting showed a much greater incidence of the personality changes than did the group that remained in the same school. Corresponding findings have been reported by Simmons and Blyth (1987). Thus, variables related to changes in the school context may influence the personality development of young people.

In sum, empirical findings emerging throughout the 1970s and 1980s indicated that organism-centered models of developmental change could not account for the multidirectionality of ontogenetic change. Instead, the context of human development needed to be incorporated into any adequate analysis of the diversity of developmental trajectories which was seen to characterize the life course. However, this context was not the simplistic, S–R environment of learning theorists (see White, 1970) or of those taking a reductionist and mechanist behavior-analytic approach to development (e.g., Bijou, 1976; Bijou & Baer, 1961).

Indeed, the multiple levels of the context, which seem linked to the individual level over the course of the life span, cannot be reduced to the molecular elements of any extant mechanistic-behavioristic theory (Lerner & Kauffman, 1985). Instead, organism and context may be seen as two distinct, yet inextricably linked, components of the *system of relationships comprising the ecology of human life* (e.g., Bronfenbrenner, 1979, 2005; Bronfenbrenner & Morris, 1998; Ford, 1987; Ford & Lerner, 1992).

Thus, and in support of the idea that research and theory during the 1970s, 1980s, and 1990s were mutually influential, the empirical findings about individual–context relations meshed quite well with the view of organism and context being forwarded in the dialectical, bioecological, and life-span views of human development. The view of human development that emerged from this empirical–theoretical synergy was one wherein theoretical reductionism was eschewed in favor of models that depicted changing, synthetic, and systematic relations among qualitatively distinct levels of analysis. By the mid-1990s neither organism nor context alone was regarded as sufficient to account for the course of individual development. Intellectual excitement about contextualism per se, as a possible metatheory or world hypothesis (Pepper, 1942) that alone could frame developmental science, had evolved (transformed) into intellectual excitement about ideas that integrated individual and context into a developmental system, one that focused on individual↔context relations. Thus, the combined influence of research and theory during these decades was to set the stage for the elaboration, in the mid- to late 1990s and into the next century, of theories that viewed individual and context as integrated systemically across life.

Conclusions

In sum, as the decade of the 1980s ended and the 1990s progressed, Paul Mussen's (1970) view of developmental science at the beginning of the 1970s—that the field placed its emphasis on explanations of the process of development—was both validated and extended. Mussen alerted developmental scientists to the burgeoning interest not in either structure, function, or content per se but in change, to the processes through which change occurs, and thus the means through which structures transform and functions evolve over the course of human life. His vision of and for the field presaged what emerged by the late 1990s to be at the cutting-edge of developmental theory: a focus on the process through which the individual's engagement with his or her context constitutes the basic process of human development.

Into the Twenty-First Century

The Emergence of Relational Developmental Systems

The interest that had emerged by the end of the 1980s and the first years of the 1990s in understanding the dynamic relation between individual and context was, during the mid- to late 1990s and into the first decade of the twenty-first century, brought to a more abstract level, one concerned with understanding the character of the integration of the levels of organization comprising the context, or bioecology, of human development (Lerner, 1998a, 1998b, 2006a, 2006b). This concern was represented by reciprocal or dynamic conceptions of process, of how structures function and how functions are structured over time, and, interestingly, by the elaboration of theoretical models that were not tied necessarily to a particular content domain but rather were focused on understanding the broader developmental system within which all dimensions of individual development emerged (e.g., Ford & Lerner, 1992; Gottlieb, 1992, 1997; Sameroff, 1983; Thelen & Smith, 1994, 1998, 2006). In other words, although particular empirical issues or substantive foci (e.g., biological development; perceptual and motor development; personality, affective, and social development; successful aging; wisdom; extraordinary cognitive achievements; intentional behavior and goal pursuit; language acquisition; the development of diverse children; psychological complexity; spiritual and religious development; or positive human development) lent themselves readily as exemplary sample cases of the processes depicted in a given theory (Lerner, 1998a, 2006a), the theoretical models that were forwarded within the mid- to late 1990s and the early 2000s were superordinately concerned with elucidating the character of individual↔context (relational, integrative) developmental systems (Lerner, 1998b, 2006b).

For example, as illustrated by most of the chapters in Volume 1 of the sixth edition of the *Handbook of Child Psychology* (Damon & Lerner, 2006), a volume entitled "Theoretical Models of Human Development" (Lerner, 2006a), the theories forwarded by contributors illustrated that the interest and, arguably, the power of these instances of developmental theories lay in their ability to transcend a unidimensional portrayal of the developing person (e.g., the person seen from the vantage point of only cognitions, or emotions, or stimulus–response connections; e.g., see Piaget, 1970; Freud, 1949; and Bijou & Baer, 1961, respectively). That is, in these theories the person was neither biologized, psychologized, nor sociologized. Rather, the individual was "systemized," that is, his or her development was conceptualized as embedded within an integrated matrix of variables derived from multiple levels of organization. Across these theories, development was conceptualized as deriving from the dynamic relations among the variables within this multitiered matrix.

Moreover, the theories represented in Volume 1 of the 2006 edition of the *Handbook* (Lerner, 2006a) did not use the polarities, or splits, that engaged developmental theory in the past, most notably nature/nurture. That is, the theories did not employ split depictions of developmental processes along what were argued to be conceptually implausible and empirically counterfactual lines (Gollin, 1981; Overton, 2006); the theories did not force counterproductive choices between false opposites. Rather, the theories were united by a common interest in gaining insight into the integrations that exist among the multiple levels of organization involved in human development (e.g., see Baltes et al., 2006; Benson, Scales, Hamilton, & Sesma, 2006; Brandtstädter, 2006; Bronfenbrenner & Morris, 2006; Cairns & Cairns, 2006; Rathunde & Csikszentmihalyi, 2006; Elder & Shanahan, 2006; Fischer & Bidell, 2006; Gottlieb, Wahlsten, & Lickliter, 2006; Lerner, 2006b; Magnusson & Stattin, 2006; Oser, Scarlett, & Bucher, 2006; Overton, 2006; Shweder et al., 2006; Spencer, 2006; Thelen & Smith, 2006; Valsiner, 2006).

As noted by Cairns and Cairns (2006, p. 155) in their historical review of developmental psychology within this volume of the *Handbook*:

> Today, the split conceptions of nature and nurture, and of the reductionist formulations associated with either a nature (e.g., sociobiology or behavior genetics) or a nurture (e.g., Behaviorism or functional analysis approaches) [perspective] have passed from the main stream of theoretical and scientific interest (e.g., see Gottlieb et al., 2006; Overton, 2006) and—through the lens of various versions of developmental systems theories (e.g., see Fischer & Bidell, 2006; Lerner, 2006; Magnusson & Stattin, 2006; Thelen & Smith, 2006)—scientific attention has focused on models and methods that now promise to begin to

> address the question of how "both causes work together" at the level of biology, interactions, and social networks.

Integrative, RDS-based theories had come to the fore of developmental science by 2006, and provided the field with models more complex than their organismic or mechanistic predecessors. These theoretical models were also more nuanced, more flexible, more balanced, and less susceptible to extravagant, or even absurd, claims (for instance, that "nature," split from "nurture," can shape the course of human development; that there is a gene for altruism, militarism, intelligence, and even television watching; or that, when the social context is demonstrated to affect development, the influence can be reduced to a genetic one; e.g., Lorenz, 1966; Plomin, 1986; Plomin et al., 1990; Rowe, 1994; Rushton, 1987, 1988a, 1988b, 2000). RDS-based theories had become, by the early years of the twenty-first century, clear indicators of the mainstream and distinctive features of the field. Indeed, the centrality of systemic and multidisciplinary thinking, spanning and integrating basic and applied scholarship, has been associated with a change in the very label of the field during this time period.

To illustrate the further emergence of RDS-based theories as defining the cutting-edge of the field that Cairns and Cairns (2006) described, and to point to the change in the label of the field studying human development, I can point to Volume 1 of the 2015, seventh edition of the *Handbook*, which was renamed (at the suggestion of William Damon) the *Handbook of Child Psychology and Developmental Science* (Lerner, 2015e). Volume 1, edited by Willis F. Overton and Peter C. M. Molenaar (2015), was entitled "Theory and Method," and included theoretical chapters that used RDS-based ideas to discuss the characteristics of dynamic systems (Witherington, 2015), dynamic development of thinking, feeling, and action (Mascolo & Fischer, 2015), biological development, and human systems (Lickliter and Honeycutt, 2015), ethology and human development (Bateson, 2015), neuroscience, embodiment, and development (Marshall, 2015), the development of agency (Sokol et al., 2015), dialectical models of socialization (Kuczynski & De Mol, 2015), human development and culture (Mistry & Dutta, 2015), emotional development and consciousness (Lewis, 2015), moral development (Turiel, 2015), development and self-regulation (McClelland, Geldhof, Cameron, & Wanless, 2015), developmental psychopathology (Cummings & Valentino, 2015), positive youth development (Lerner et al., 2015), and systems methods for developmental research (Molenaar & Nesselroade, 2015). The substance of these chapters involved RDS-based models that ranged across the mid-level to topic-, or variable-, level theories depicted in the funnel model shown in Figure 1.1. In addition, Volume 1 included a chapter by Overton (2015a) on processes, relations, and relational-developmental systems.

In addition, across the other three volumes of the seventh edition of the *Handbook*, there were other important examples of the use of RDS-based models to depict a range of models corresponding to the range represented in Volume 1. For instance, in Volume 2, "Cognitive Processes," which was edited by Lynn S. Liben and Ulrich Müller, there were chapters on the development of social understanding (Carpendale and Lewis, 2015), the development of reasoning (Ricco, 2015), the development of executive function (Müller & Kerns, 2015), gender and social-cognitive development (Leaper, 2015), and cognitive development and culture (Gauvain & Perez, 2015) that involved the use of RDS-based ideas.

Similarly, Volume 3, "Socioemotional Processes," edited by Michael E. Lamb, included chapters on resilience and adversity (Luthar, Crossman, & Small, 2015), developmental implications of discrimination (Marks, Ejesi, McCullough, & García Coll, 2015), race, class, and ethnicity in young adulthood (McLoyd, Purtell, & Hardaway, 2015), children and the law (Lamb, Malloy, Hershkowitz, & La Rooy, 2015), development of achievement motivation and engagement (Wigfield, Eccles, Fredricks, Simkins, Roeser, & Schiefele, 2015), origins and development of morality (Killen & Smetana, 2015), development of the self (Spencer, Swanson, & Harpalani, 2015), gendered development (Hines, 2015), the development of sexuality (Diamond, Bonner, & Dickenson, 2015), and religious and spiritual development (King & Boyatzis, 2015) that also involved the use of RDS-based concepts.

Finally, Volume 4, "Ecological Settings and Processes," edited by Marc H. Bornstein and Tama

Leventhal, included chapters on human development in time and place (Elder et al., 2015), children's parents (Bornstein, 2015), children in diverse families (Ganong, Coleman, & Russell, 2015), children at school (Crosnoe & Benner, 2015), children's organized activities (Vandell, Larson, Mahoney, & Watts, 2015), children in diverse social contexts (Murry, Hill, Witherspoon, Berkel, & Bartz, 2015), children in neighborhoods (Leventhal, Dupéré, & Shuey, 2015), children in war and disaster (Masten, Narayan, Silverman, & Osofsky, 2015), and children in cultural context (Goodnow & Lawrence, 2015). These chapters again involved the use of RDS-based concepts comparable to those used in chapters in the other volumes of the seventh edition of the handbook.

In addition to the seventh edition of the *Handbook of Child Psychology and Developmental Science* (Lerner, 2015e), more and more scholars of human development referred to their field as developmental science (e.g., see Cairns & Cairns, 2006; Magnusson & Stattin, 2006). Moreover, at least one leading graduate textbook in the field has changed its title, from *Developmental Psychology: An Advanced Textbook* (Bornstein & Lamb, 1999) to *Developmental Science: An Advanced Textbook* (Bornstein & Lamb, 2005).

In sum, the change of name for the field studying the human life span reflects in large part key intellectual changes across the past two decades or so: (a) the certain demise of split conceptions of the nature–nurture issue, and of reductionist approaches to either nature formulations (sociobiology, EDP, behavior genetics) or nurture formulations (e.g., S–R (stimulus–response) models or functional analysis approaches; Overton, 2006, 2010, 2015a; Overton & Müller, 2013; Valsiner, 2006); (b) the ascendancy of a focus on RDS-based models, conceptions that seek to fuse systemically the levels of organization involved in the ecology of human development (from biology and physiology through culture and history; e.g., see Baltes et al., 2006; Elder et al., 2015; Gottlieb et al., 2006; Thelen & Smith, 2006; and the chapters across the four volumes of the seventh, 2015 edition of the *Handbook of Child Psychology and Developmental Science*; Lerner, 2015e); and (c) the emphasis on *relations* among levels, and not on the main effects of any level itself, as constituting the fundamental units of analysis of developmental analysis (e.g., see Bronfenbrenner & Morris, 2006; Brandtstädter, 2006; Fischer & Bidell, 2006; Magnusson & Stattin, 2006; Mascolo & Fischer, 2015; Noble, 2015; Rathunde & Csikszentmihalyi, 2006).

Implications of the RDS Metatheory for Developmental Science

The ascendancy of the process-relational paradigm and of the RDS metatheory frame for the conduct of developmental science has been a product and a producer of a shift in the philosophy of science framing discourse within the field (Overton, 2003, 2006, 2010, 2015a). RDS metatheory has served as a product and a producer of developmental systems thinking, that has rejected the idea derived from the positivist and reductionist notion that the universe is uniform and permanent—that the study of human behavior should be aimed at identifying nomothetic laws that pertain to the generic human being. This idea has been replaced by an emphasis on the individual, on the importance of attempting to identify both differential and potentially idiographic laws as involved in the course of human life (e.g., Block, 1971; Magnusson, 1999a, 1999b; Molenaar & Nesselroade, 2014, 2015; Rose, 2016; Rose et al., 2013), and on regarding the individual as an active producer of his or her own development (Brandtstädter, 1999, 2006; Lerner, 1982; Lerner & Busch-Rossnagel, 1981a, 1981b; Lerner, Lerner, Bowers, & Geldhof, 2015; Lerner, Theokas, & Jelicic, 2005; Mascolo & Fischer, 2015; Rathunde & Csikszentmihalyi, 2006). Similarly, the changed philosophical grounding of the field has altered developmental science from a field that enacted research as if time and place were irrelevant to the existence and operation of laws of behavioral development to a field that has sought to identify the role of contextual embeddedness and temporality in shaping the developmental trajectories of diverse individuals and groups (e.g., see Baltes et al., 2006; Bronfenbrenner & Morris, 2006; Elder, Modell, & Parke, 1993; Elder & Shanahan, 2006; Elder et al., 2015).

Arguably, the most profound impact of the RDS metatheory on the practice of developmental science has occurred in the conceptualization of diversity, of interindividual differences, in developmental trajectories (Bornstein, 2006, 2017; Lerner, 2004; Spencer, 2006). From the perspective of the uniformity and permanence assumptions, individual differences—diversity—were seen, at best, through a lens of error variance, as prima facie proof of a lack of experimental control or of inadequate measurement. At worst, diversity across time or place, or in the individual differences among people, was regarded as an indication that a deficit was present. Either the person doing the research was remiss for using a research design or measurement model that was replete with error (with a lack of experimental control sufficient to eliminate interindividual differences), *or* the people who varied from the norms associated with the generic human being—the relations among variables that were generalizable across time and place—were in some way deficient (see Gould, 1981, 1996; Rose, 2016). They were, to at least some observers, less than normatively human (e.g., see Belsky, 2014). Things have changed, however.

From Deficit to Diversity in Developmental Science

For colleagues trained in developmental science within the twenty-first century, the prior philosophical grounding and associated philosophical assumptions about science may seem either unbelievably naive or simply quaint vestiges from an unenlightened past. In what, for the history of science, is a very short period (Cairns & Cairns, 2006), participants in the field of human development have seen a sea change that perhaps qualifies as a true paradigm shift in what is thought of as the nature of human nature and in the appreciation of time, place, and individual diversity for understanding the laws of human behavior and development (Bronfenbrenner & Morris, 2006; Elder & Shanahan, 2006; Overton, 2006, 2010, 2015a; Shweder et al., 2006; Valsiner, 2006).

The publication in 1998 of the fifth edition of the *Handbook of Child Psychology*, edited by William Damon, may have been the first major reference work in developmental science that heralded that major contributions to the study of human development rejected the hegemony of positivism and reductionism. As evidenced by the chapters in all four volumes of the Damon (1998) *Handbook*, and arguably especially in Volume 1, "Theoretical Models of Human Development" (Damon & Lerner, 1998), the majority of the scholarship then defining the cutting edge of the field of human development was associated with the sorts of RDS-based models of human development that fill the pages of the 2006 and 2015 editions of this volume of the *Handbook* and that, as projected by Cairns (1998), were at the threshold of their time of ascendancy within developmental science. As I indicated in my discussion of the subsequent chapter by Cairns and Cairns (2006), within less than a decade the prediction by Cairns (1998) had been instantiated.

The view of the world that emerged from the chapters in the fifth edition of Volume 1 of the *Handbook* (Damon & Lerner, 1998), and that was confirmed across the corresponding chapters of the 2006 and 2015 editions of this work (including those chapters represented in earlier editions and those chapters new to an edition), was that the universe is dynamic and variegated. Time and place, therefore, are matters of substance, not error; and to understand human development, scholars must appreciate how variables associated with person, place, and time coalesce to shape the structure and function of behavior and its systematic and successive change.

Accordingly, diversity of person and context, and the idiographic and non-ergodic character of human development, have moved into the foreground of the analysis of human development (e.g., Lerner, 1991, 2004; Molenaar & Nesselroade, 2015; Rose, 2016). The dynamic, RDS-based perspective framing the study of human development at this writing does not reject the idea that there may be general laws of human development. Instead, there is an insistence on the presence of individual laws as well and a conviction that any generalizations about groups or humanity as a whole require empirical verification, not pre-empirical stipulation (Magnusson & Stattin, 2006; Molenaar & Nesselroade, 2015; Overton, 2006, 2015a; Rose, 2016).

To paraphrase the insight of Kluckhohn and Murray (1948), made more than a half-century ago, all people are like all other people, all people are like some other people, and each person is like no other person. Today, then, the science of human development recognizes that there are idiographic, differential, and nomothetic laws of human behavior and development (e.g., see Emmerich, 1968; see too Chapter 5). Each person and each group possesses unique and shared characteristics that need to be the core targets of developmental analysis.

Differences, then, among people or groups are not necessarily indicators of deficits in one and strengths in the other (Spencer, 2006; Spencer et al., 2015). Certainly, it is not useful to frame the study of human development through a model that a priori sets one group as the standard for positive or normative development and regards another group, when different from the group set as the normative one, as therefore defined as being in deficit. If there is any remaining place in developmental science for a deficit model of humans, it is useful only for understanding the thinking of those individuals who continue to treat diversity as either by definition indicative of error variance or as necessarily reflective of a deficiency of human development.

Vestiges of Reductionist Models

Despite the contemporary emphasis on RDS metatheory and on theories linked to it, the remnants of reductionism and deficit thinking still remain at the periphery of developmental science. These instances of genetic reductionism exist in behavior genetics (e.g., Rowe, 1994; Plomin, 2000; Plomin et al., 2016), in sociobiology (e.g., Rushton, 1999, 2000), and in EDP (e.g., Bjorklund, 2015, 2016; Del Giudice & Ellis, 2016). These approaches constitute today's version of the biologizing errors of the past, such as eugenics and racial hygiene (Lerner, 1992a, 1992b, 2015b, 2015c; Proctor, 1988).

As explained by Cairns and Cairns (2006) and by Collins et al. (2000), these ideas are no longer seen as part of the forefront of scientific theory. Nevertheless, their influence on scientific and public policy persists. Renowned biologists, working in the field of genetics and/or evolutionary biology, such as Bearer (2004), Edelman (1987, 1988), Feldman (e.g., 2014; Feldman & Laland, 1996), Ho (1984, 2010), Lewontin (2000), Müller-Hill (1988), and Venter (e.g., Venter et al., 2001); and eminent colleagues in comparative and biological psychology, such as Greenberg (e.g., Greenberg & Haraway, 2002; Greenberg & Tobach, 1984), Gottlieb (1997, 2004), Hirsch (1997, 2004), Lickliter (2016; Lickliter & Honeycutt, 2015); Michel (e.g., Michel & Moore, 1995), Moore (2015a, 2016) and Tobach (1981, 1994; Tobach, Gianutsos, Topoff, & Gross, 1974), alert us to the need for continued intellectual and social vigilance, lest such flawed ideas about genes and human development become the foci of public policies or social programs (see too Lerner, 2015b, 2015c).

Such applications of counterfactual ideas remain real possibilities, and in some cases unfortunate realities, due at least in part to what Horowitz (2000) described as the affinity of the "Person in the Street" to simplistic models of genetic effects on behavior. These simple and, I must emphasize, erroneous models are used by the Person in the Street to form opinions or to make decisions about human differences and potentials.

Genetic reductionism can, and has, led to views of diversity as a matter of the "haves" and the "have nots" (e.g., Belsky, 2014; Herrnstein & Murray, 1994; Rushton, 1999, 2000). There are, in this view, those people who manifest the normative characteristics of human behavior and development. These individuals are the "haves," the people who possess (innately, it is presumed; e.g., Belsky, 2014) the attributes that make them healthy, adaptive, or resilient. Given the diversity-insensitive assumptions and research that characterized much of the history of scholarship in human development even into the late 1990s and the first decades of the twenty-first century, these normative features of human development were associated with middle-class, European American samples (Graham, 1992; McLoyd, 1998; Shweder et al., 2006; Spencer, 1990, 2006; Spencer et al., 2015). In turn, there are those people who manifest other characteristics, and these individuals were generally non-European American and non-middle-class. These individuals were regarded as the "have-nots." As such, if the former group is regarded as normative, then the characteristics of the latter groups are regarded as non-normative (Gould, 1996). When such an interpretation is forwarded, entry has thus been made down the slippery slope of moving from a description of between-group differences to an attribution of deficits in the latter groups (Lerner, 2004, 2015b, 2015c).

Such an attribution is buttressed when seen through the lens of genetic reductionism because, in this conception, it must be genes that provide the final, material, and efficient cause of the characteristics of the latter groups (e.g., see Bjorklund, 2015, 2016; Rowe, 1994; Rushton, 2000; Plomin et al., 2016). Therefore, non-European American or non-middle-class groups are, in the fully tautological reasoning associated with genetic reductionism, behaviorally deficient because of the genes they possess, *and* because of the genes they possess, they have behavioral deficits (e.g., see Rushton, 2000). Simply, the ill-founded argument is that the genes that place one in a racial group are the genes that provide either deficits or assets in behavior, and one racial group possesses the genes that are assets and the other group possesses the genes that are deficits (e.g., see Belsky, 2014, for such an argument).

As shown in Table 3.2, these genetic reductionist ideas may have profound and dire effects on public policies and social programs (Lerner, 2004). The table presents "A," beliefs about whether genetic reductionist ideas are believed to be either (1) true or (2) false. The table presents also "B," public policy and social program implications that would be associated with genetic reductionism were it in fact (1) true or (2) false under either of the two belief conditions involved in "A." Moreover, the "A.2.B.2." quadrant of the table not only presents the policy and program implications of believing that the genetic reductionist conception is believed to be false when it is in fact false. In addition, this quadrant illustrates the policy and program implications of believing RDS-based ideas to be true when they are in fact the case. Table 3.2 demonstrates that if genetic reductionism is believed to be true, then

Table 3.2 Policy and program implications that arise if the hereditarian (genetic reductionist) "split" conception of genes (A) were believed to be true or false and (B) were in fact true or false

		B. Public policy and social program implications if hereditarian "split" position were in fact	
		1. True	2. False
A. Hereditarian "split" conception is believed to be:	1. True	• Repair inferior genotypes, making them equal to superior genotypes • Miscegenation laws • Restrictions of personal liberties of carriers of inferior genotypes (separation, discrimination, distinct social tracts) • Sterilization • Elimination of inferior genotypes from genetic pool	Same as A.1, B.1
	2. False	• Wasteful and futile humanitarian policies • Wasteful and futile programs of equal opportunity, affirmative action, equity, and social justice • Policies and programs to quell social unrest because of unrequited aspirations of genetically constrained people • Deterioration of culture and destruction of civil society	• Equity, social justice, equal opportunity, affirmative action • Celebration of diversity • Universal participation in civic life • Democracy • Systems assessment and engagement • Civil society

irrespective of whether it is in fact true (and, it must be emphasized that it is incontrovertibly *not* true), a range of actions may be promoted that constrain people's freedom of association, reproductive rights, and even survival.

In contrast, Table 3.3 presents the different implications for policies and programs of strict environmental (radical behaviorist) reductionist theories. As emphasized by Lewontin (1992) and by Overton (2003, 2006, 2010, 2015a), split and reductionist, radical behaviorist conceptions are equally philosophically problematic and empirically flawed. Both of these split conceptions thus can be expected to result in problems for the conduct of science and for the application of science to policies and programs. This comparability of problems between genetic and environmental reductionist approaches can be seen in the A.2.B.1. quadrant of Table 3.3. In

Table 3.3 Policy and program implications that arise if the strict environmentalist (radical contextual) "split" conception of context (A) were believed to be true or false and (B) were in fact true or false

		B. Public policy and social program implications if strict environmentalist "split" position were in fact	
		1. True	2. False
A. Strict environmental "split" conception is believed to be:	1. True	• Provide all children with same educational or experiential regimen to maximize their common potential/aptitude • Eliminate all individualized educational or training programs • Standardized assessments for all children • Penalties for parents, schools, and communities when children manifest individual differences in achievement • Educate all parents, caregivers, and teachers to act in a standard way in the treatment of all children	• Same as A.1, B.1
	2. False	• Wasteful and counterproductive diversity-sensitive policies	• Programs that are sensitive to individual differences and that seek to promote a goodness of fit between individually different people and contexts
		• Wasteful and counterproductive programs based on individual differences	• Affirmative actions to correct ontogenetic or historical inequities in person–context fit
		• Policies and programs to quell social unrest because of unrequited aspirations of people promised that the program they received would make them equal to all other people	• Celebration of diversity
		• Deterioration of culture and destruction of civil society	• Universal participation in civic life • Democracy • Systems assessment and engagement • Social justice • Civil society

turn, and as was also the case for the A.2.B.2 quadrant in Table 3.2, this quadrant of Table 3.3 presents the policy and program implications of believing that the split, environmentalist conception is (correctly) believed to be false and is in fact false. As in Table 3.2, then, this quadrant illustrates the policy and program implications of believing RDS-based ideas to be true when, in fact, they are the case.

As shown in Table 3.2, if the hereditarian conception were correctly regarded as false (and conversely RDS-based conceptions were correctly seen as true), then policies and programs aimed at social justice and civil society for the diverse families and children of the United States and world would be promoted. Similarly, Table 3.3 shows that if the developmental systems perspective is correctly seen as true and if the strict environmentalist conception is correctly regarded as false, corresponding results for social justice and civil society are promoted. This result obtains although the strict environmentalist perspective is associated with a set of problematic policy and program implications that differ from those problems linked to the hereditarian perspective.

Despite the theory and research that lend support to a dynamic conception of gene↔experience coaction, some proponents of genetic reductionism maintain that concepts and methods regarding genes as separable from context are valid and overwhelmingly, or irrefutably, evident (e.g., Belsky, 2014; Bjorklund, 2015; Plomin et al., 2016). The media continue to tell this story and, perhaps more often than not, the Person in the Street is persuaded by it (Horowitz, 2000).

The challenge that such language use and public discourse represent is not merely one of meeting our scientific responsibility to amend incorrect dissemination of research evidence. Horowitz (2000) reminds us that an additional, and ethical, responsibility is to support social justice (see too Fisher et al., 2013; Fisher & Lerner, 2013; Lerner, 2015b, 2015c; Lerner & Overton, 2008). Horowitz emphasizes that such action is critical in the face of the simplistically seductive ideas and language of genetic reductionism, especially when coupled with the deficit model. Overton (2006) points also to the need to appreciate the subtlety of language to avoid loading our scientific language with phrases that, on a manifest level, may seem to reject the split thinking of genetic reductionism but, on a deeper, structural level, employ terms that legitimate the language of such thinking remaining part of scientific discourse. He notes:

> In its current split form no one actually asserts that matter, body, brain, genes or society, culture, and environment provide *the* cause of behavior or development: The background idea of one or the other being the privileged determinant remains the silent subtext that continues to shape discussions. The most frequently voiced claim is that behavior and development are the products of the *interactions* of nature and nurture. But interaction itself is generally conceptualized as two split-off pure entities that function *independently* in cooperative and/or competitive ways (e.g., Collins et al., 2000). As a consequence, the debate simply becomes displaced to another level of discourse. At this new level, the contestants agree that behavior and development are determined by *both* nature *and* nurture, but they remain embattled over the relative merits of each entity's essential contribution.
>
> (Overton, 2006, p. 33)

Similarly, he explains:

> Moving beyond behavior genetics to the broader issue of biology and culture, conclusions such as "contemporary evidence confirms that the expression of heritable traits depends, often strongly, on experience" (Collins et al., 2000, p. 228) are brought into question for the same reason. Within a relational metatheory, such conclusions fail because they begin from the premise that there are pure forms of genetic inheritance termed "heritable traits" and within relational metatheory such a premise is unacceptable.
>
> (Overton, 2006, p. 36)

Whereas contemporary developmental science rejects the philosophical, theoretical, and (in large part) methodological features of the split thinking associated with genetic reductionist approaches to human development, found in behavior genetics, EDP, and sociobiology, subtle and nuanced problems of language continue to suggest that these split approaches to human development remain legitimate. I have noted the potentially enormous negative consequences of such problematic language in our scientific discourse—especially if the Person in the Street believes that employing such terms means that the genetic reductionist ideas about social policy should be countenanced. As a consequence, developmental scientists must be assiduous and exact in the terms they use to explain why split conceptions in general, and genetic reductionist ones in particular, fail as useful frames for scientific discourse about human development.

Conclusions

By the end of the twentieth century, and in the first two decades of the twenty-first century, the conceptually-split, mechanistic, essentialist views, that had been involved in so much of the history of concepts and theories of human development, had been replaced by theoretical models that emphasized relationism and integration across all the distinct but fused levels of organization involved in human life. This dynamic synthesis of multiple levels of analysis is a perspective having its roots in systems theories of biological development (Cairns & Cairns, 2006; Gottlieb, 1992; Kuo, 1930, 1967, 1976; Novikoff, 1945a, 1945b; Schneirla, 1956, 1957; von Bertalanffy, 1933), and allows development to be understood as a property of systemic change in the multiple and integrated levels of organization (ranging from biology to culture and history) comprising human life and its ecology (Overton, 2015a).

Moreover, as noted by Cairns and Cairns (2006), the interest in understanding person–context relations within an integrative, or systems, perspective has a rich history within developmental psychology as well as in developmental biology. For example, James Mark Baldwin (1897a, 1897b) expressed interest in studying development-in-context, and thus in understanding integrated, multilevel, and hence interdisciplinary scholarship (Cairns & Cairns, 2006). These interests were shared as well by Lightner Witmer, the founder in 1896 of the first psychological clinic in the United States (Cairns & Cairns, 2006; Lerner, 1977).

As well, Cairns and Cairns (2006) described the conception of developmental processes—as involving reciprocal interaction, bidirectionality, plasticity, and biobehavioral organization (all quite modern emphases)—as integral in the thinking of the founders of the field of human development. For instance, Wilhelm Stern (1914; see Kreppner, 1994) emphasized the holism that is associated with a developmental systems perspective about these features of developmental processes. In addition, other contributors to the foundations and early progress of the field of human development (e.g., John Dewey, 1916; Kurt Lewin, 1935, 1954; and even John B. Watson, 1928) emphasized the importance of linking child development research with application and child advocacy—a theme of contemporary relevance (Fisher et al., 2013; Lerner, 2012, 2015b, 2015c, 2015d; Lerner, Fisher, & Weinberg, 1997, 2000a, 2000b; Lerner & Overton, 2008; Zigler, 1998). This orientation toward the application of developmental science is a contemporary view as well, derived from the emphasis on plasticity and temporal embeddedness within RDS-based theories.

In short, there has been a history of visionary scholars interested in exploring the use of ideas associated with RDS-based theories for understanding the basic process of human development and for applying this knowledge within the actual (ecologically valid) contexts of people to enhance their paths across life (Lerner & Callina, 2014b). RDS-based theories have emerged from their historical roots to become, at this writing, the key conceptual frame associated with concepts and theories of human development.

In subsequent chapters, I will discuss and evaluate the ideas of both RDS-based and non-RDS-based theories in regard to the core conceptual issues involved in the study of human development (e.g., nature–nurture and continuity–discontinuity). As emphasized in the present chapter, this presentation will rest, however, on an understanding of the key philosophical issues involved in the understanding of concepts and theories of human development. Throughout prior chapters I have provided brief synopses of these philosophical issues in order to describe prior or current theoretical ideas. However, in Chapter 4, I discuss these philosophical issues in more detail.

CHAPTER FOUR

Metatheoretical Models of Development

Scientists may use a variety of concepts or theoretical formulations to guide their selection of hypotheses, methods, and data analysis procedures. However, scientists vary in regard to the concepts and theoretical ideas they believe are appropriate or useful for their empirical work. A key basis of this variation may be the use by scientists of different philosophical ideas about the nature of the world (Kuhn, 1962, 1970; Overton, 2015b; Pepper, 1942). That is, scientists hold *pre-empirical beliefs or presuppositions*, and these are often beliefs that are tacit or go unquestioned and are thus not open to empirical test (Kagan, 1980).

That is, philosophical assumptions and their associated beliefs may be explicit or implicit (Watson, 1977) and, if implicit, they may take the form of a presupposition about the nature of a specific feature of life, for example, that there is an inevitable connection between early experience and behavior in later life (Kagan, 1980, 1983). In addition, these beliefs may take the form of a more general paradigm (Kuhn, 1962, 1970), model (Overton & Reese, 1973; Reese & Overton, 1970), world view (Kuhn, 1962, 1970), or world hypothesis (Pepper, 1942). All of these terms pertain to a philosophical system of ideas that serves to organize a set, or a "family" (Reese & Overton, 1970), of scientific concepts, theories, and associated scientific methods.

PHILOSOPHICAL MODELS AND DEVELOPMENTAL SCIENCE

Philosophical models of the world may have quite a pervasive effect on scientific ideas about human development. They may specify the basic characteristics of humans, and of reality itself, and thus function either to include or exclude particular features of humans and/or of the world's events in the realm of scientific discourse. Hence, given the ways in which philosophical assumptions may shape a scholar's empirical work, science is relative rather than absolute. That is, facts are not purely naturally occurring events awaiting discovery. Instead, to Kuhn (1962), science

> seems an attempt to force nature into [a] preformed and relatively inflexible box . . . No part of the aim of normal science is to call forth new sorts of . . . phenomena; indeed those that will not fit the box are often not seen at all. Nor do scientists normally aim to invent new theories, and they are often intolerant of those invented by others.
>
> (p. 24)

Accordingly, a full understanding of human development cannot be obtained from any one theory or methodology, nor can it be obtained from a cataloging of empirical "facts" (von Bertalanffy, 1933). The integration of philosophy, theory, method, and research results is required to attain a complete understanding of an area of scientific scholarship

(Overton, 2015b). Within such an integration, theory and research are given meaning. Theory and research are developed and interpreted within the context of a given philosophical perspective. Thus, it is necessary to understand the different philosophical models, theories of theories, or metatheories (Overton, 2006, 2015a), on which the study of development can be based.

However, scholars of human development have a history of *refraining* from such an examination (Overton, 2006):

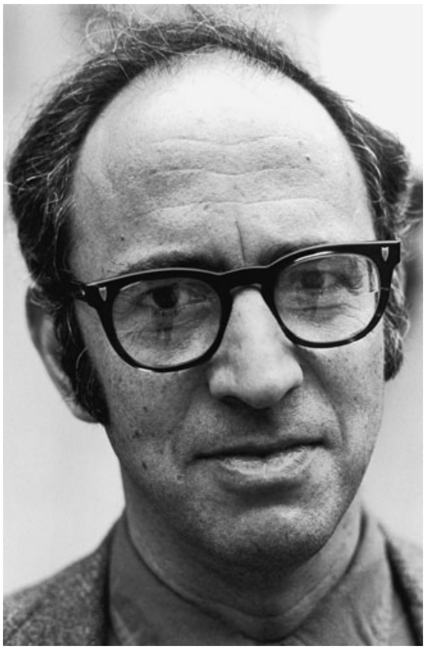
> Conceptual clarification and the exploration of conceptual foundations have traditionally been the principal provinces of philosophy, and therein lies the rub. Within the psychological community, philosophical thought—and, as a consequence, any focus on conceptual clarification—has tended to be assigned the role of the anti-science.
>
> (p. 19)

Ironically, this deep skepticism of philosophy within psychology is, itself, the consequence of certain philosophical assumptions (Overton, 2015b). For instance:

> Broadly, the marginalization of all things philosophical, and, hence, the marginalization of any extended examination of conceptual foundations, has rested on a forced dichotomy, which locates philosophy in a space of reason and reflection split off from observation and experimentation, and psychology in a space of observation and experimentation split off from reason and reflection . . . These assumptions begin with the idea of splitting reason from observation, and follow with the epistemological notion that knowledge and, indeed, reason itself originates in observation and only observation.
>
> (Overton, 2006, p. 19)

Yet, as noted by Valsiner (2006, p. 173), "*data are always constructed*—or better—*derived from phenomena*, on the basis of the investigator's reasoning."

Despite this historical skepticism about philosophy, since the 1970s some developmental scientists have worked to deepen the field's awareness of the influence of philosophical assumptions on theory and method. Notably, since the early 1970s Willis F. Overton and Hayne W. Reese wrote a series of essays (Overton, 1984, 1991a, 1991b, 1991c, 1991d, 1994a, 1994b, 1994c, 2015a, 2015b; Overton & Reese, 1973, 1981; Reese, 1982, 1993, 1995; Reese & Overton, 1970) that explain the ways in which theory and method are influenced by philosophical issues pertinent to the study of human development.


Thomas Kuhn

For instance, in earlier chapters, I discussed Overton's (2015a) discussion of "split" versus relational issues in philosophy. I noted that the former type of concept was linked to theories which involved the separation of nature- and nurture-related processes; within such split conceptions one process (nature or nurture) is regarded as the essential or the ultimate "real" (Putnam, 1987), or at least of primary influence, and the other type of process is seen as epiphenomenal or derivative (in regard to influencing development) (Overton, 2003, 2006, 2015a). In turn, Overton (2015a) explained how theories associated with relational philosophical ideas (e.g., relational developmental systems (RDS)-based theories; see Chapter 10) take an integrated view of nature and nurture processes, and see them as fused and reciprocally integrated (coactive) over the course of life.

Prior to Overton's (2015a) discussion of split-versus-relational ideas in philosophy, Overton and Reese (1973, 1981; Reese & Overton, 1970) focused their attention on the import for theory and method in human development of two world views—the mechanist and the organismic—which, historically, were central in influencing theories of development. Although many theories of development associated with mechanism and organicism were *similar* in adopting split views of nature and nurture, Overton and Reese advanced significantly the understanding of human development by describing the different "families" (related, or consonant, groups) of theories and methodological traditions associated with mechanist- and organismic-related theories.

Moreover, the work of Overton and Reese was seminal in promoting among developmental psychologists an interest in exploring the potential role of other world hypotheses in shaping theories of development. For instance, as noted in Chapter 3, Riegel (1975, 1977a, 1977b) discussed the potential use of a "dialectical" model of development, and Lerner (1984, 1985; Lerner, Hultsch, & Dixon, 1983; Lerner & Kauffman, 1985, 1986) as well as Reese and Overton (Reese, 1982; Overton, 1984; Overton & Reese, 1981) discussed the ways in which a "contextual" world hypothesis (Pepper, 1942) could be used to devise a theory of development. As noted also in Chapter 3, the dialectical model emphasizes syntheses among the conflicts arising from the coactions among variables from different levels of analysis, such as the inner-biological, individual-psychological, physical-environmental, and sociocultural (Riegel, 1975, 1976a, 1976b). In turn, contextualism stresses the continually changing context of life, the bidirectional relations among individuals and the context, and that the timing of these relationships shapes the direction and outcome of development (Pepper, 1942).

As Overton (1984) has made clear, however, these latter models (i.e., the dialectical and the contextual ones) do not readily provide a useful set of ideas for the derivation of scientifically adequate theories of development *unless* they are integrated into mechanist or organismic conceptions. Indeed, as both Overton (1984) and Lerner and Kauffman (1985) argued, the dispersive nature of the contextual world hypothesis does not provide a useful frame for understanding the systematic, organized, and successive (or progressive) character of change that is the defining feature of development (Ford & Lerner, 1992).

Thus, although Pepper (1942) claimed that it was not philosophically permissible to "mix metaphors" and combine mechanist, organismic, and contextual world views, I believe one may do just this. Arguing on the basis of criteria of usefulness (e.g., in regard to developing statements that, in comparison to those of other positions, account for more variance in developmental data sets; lead to more novel discoveries than do ideas associated with other positions; or integrate a broader range of phenomena pertinent to development than is the case with other positions), Overton (1984) and Lerner and Kauffman (1985) advanced the notion of combining organicism and contextualism to frame a new approach to developmental theory.

Overton (1984) pointed out the possibility of integrating contextualism with either mechanism *or* organicism to produce such a new theory. Similarly, Reese (1993; Hayes, Hayes, Reese, & Sarbin, 1993) discussed ways in which mechanism and contextualism are related. However, I believe that the mechanist view has too many conceptual limitations for use as a model for development. In fact, I will explain that a key conceptual problem is that mechanist theories of development inevitably follow a split view of reality, and thus involve a false division between nature and nurture processes (Overton, 1973, 1984, 2015a). In turn, I will argue for a synthesis of organicism and contextualism (as does Overton, 1984, 2003, 2010, 2015a), and will suggest that, just as contextualism needs organicism to enhance its use, so does organicism need contextualism. As already suggested in prior chapters, the result of this integration is RDS metatheory. To begin to develop this argument, it is useful to discuss in more detail the mechanist, the organismic, and the contextual models.

I begin this discussion by describing the defining attributes of the mechanist model and indicate how a mechanist model may be translated into a theory of development. I then present an example of a mechanist theory of development and present key problems of this model for theory in developmental science. I present a corresponding discussion of the organismic model. I then discuss the relation between the ideas in a mechanist and organismic model and issues of development. This presentation is organized around the concepts of elementarism versus holism, antecedent–consequent versus structure–function relations, behavioral versus structural change, continuity versus discontinuity, stages of development, and sources of development. My discussion of the issues pertinent to these concepts points to problems with both models and, as such, leads me to a discussion of the contextual model. I discuss the connection between contextualism and the concept of probabilistic epigenesis, and describe the concept of development involved in probabilistic epigenesis. I discuss probabilistic epigenesis as a concept that integrates other world views and its link to RDS-based theories of development. I also introduce the levels-of-organization hypothesis and the general-and-specific-laws compromise as ideas illustrating the ways that probabilistic epigenesis integrates ideas from other metatheories and, as I do in regard to the other metatheoretical models I discuss in this chapter, I also discuss limitations of the concept of probabilistic epigenesis. However, I also point to issues of the application of developmental science, in this case, to interventions, that are linked to probabilistic epigenesis. Finally, I end the chapter with a discussion of the implications of philosophical models for the future development of developmental science.

THE MECHANIST MODEL

Pepper (1942) noted that each of the "world hypotheses," or philosophical models, he described (formism, mechanism, organicism, and contextualism) could be associated with a core, or root, metaphor, a concept which captured the essence of the philosophy. In mechanism the root metaphor is a machine. As explained by Reese and Overton (1970), the mechanist philosophical model

> represents the universe as a machine, composed of discrete pieces operating in a spatiotemporal field. The pieces—elementary particles in motion—and their relations form the basic reality to which all other more complex phenomena are ultimately reducible. In the operation of the machine, forces are applied and there results a discrete chain-like sequence of events. These forces are the only efficient or immediate causes; purpose is seen as a mediate or derived cause. Given this, it is only a short trip to the recognition that complete prediction is in principle possible, since complete knowledge of the state of the machine at one point in time allows inference of the state at the next, given a knowledge of the forces to be applied.
>
> (p. 131)

In turn, as summarized by Anderson, the mechanist position states that "the workings of our minds and bodies, and of all the animate or inanimate matter of which we have any detailed knowledge, are assumed to be controlled by the same set of fundamental laws, which except under certain extreme conditions we feel we know pretty well" (1972, p. 393).

The mechanist position, then, rests on the principles of *foundationalism* and *atomism* (Overton, 2006), that is on: "the metatheoretical axioms that there is ultimately a rock bottom unchanging nature to reality (the foundation of foundationalism), and that this rock bottom is composed of elements—pure forms—(the atoms of atomism) that preserve their identity regardless of context" (p. 31).

Foundationalism and atomism are principles of decomposition, or reductionism, as they are concerned with reducing the composite to its core, *essential* elements. The reductionism of the mechanist model is, then, closely aligned with the concept of *essentialism*. Essentialism may be defined as a doctrine holding that there are necessary properties of things, and that these properties are logically prior to the existence of the individuals that instantiate them; as the doctrine that essence is prior to existence; and/or as the practice of regarding something as having innate existence. Reductionism also allows reconstruction or recomposition of the whole, which of course is *only* the sum of the (essential) parts. That is, recomposition is afforded by "*unidirectional and linear (additive) associative or causal sequences*" (Overton, 2006, p. 31).

Science has a long history of being committed to "identifying ever more fundamental things" and to the complementary goal of "understanding their 'thingness'" (Sameroff, 2009, p. 3). As noted by Sameroff (2009):

> From Aristotle's earth, air, fire and water, to Mendeleyev's periodical table of the elements, to the quarks and leptons of the contemporary standard physical model, the identification of fundamental particles defined as containing nothing smaller has moved to increasingly infinitesimal units. Since the Greeks, there has been a Platonic idealization that these newer, ever smaller elements represented things in themselves, independent of their context, and that simple combinations of these fundamental units would explain everything more complex.
>
> (p. 3)

Arnold J. Sameroff

Proponents of this viewpoint often hold that physics and chemistry are the basic natural sciences; they thus often believe that the laws of these two disciplines are the one set of fundamental laws alluded to by Anderson (1972). That is, although several different meanings of the term *mechanism* have been used by philosophers and scientists, one major version of the mechanist position is an interpretation of biological (or psychological) phenomena in physical and chemical terms (von Bertalanffy, 1933). This interpretation provides an apt illustration for my discussion, and I shall focus on it to explain the position.

In this interpretation it is the laws of chemistry and physics—the rules that depict the mechanisms by which atoms and molecules function—that are the fundamental laws of the real world. Everything involves atoms and molecules; nothing exists in the natural world that is not basically made up of these essential things. If one understands the mechanisms by which atoms and molecules combine and function, then one understands the basic laws of everything. The mechanics of chemistry and physics then become the ultimate laws of all events.

Thus, these basic laws that govern all natural events and phenomena, whether organic or inorganic, are held to apply to all levels of phenomenal analysis. Consistent with Nagel (1957), I define a *level* as a state of organization of matter, or life, phenomena. For example, chemistry, with its particular set of concepts and principles, represents one level of organization, whereas psychology, with its own set of terms, represents another. A scientist can describe behavior at its own level or in terms of the principles of another level. Developmental scientists can study how children at various age levels develop the ability to perform in certain situations (e.g., on classroom tests) by attempting to discern the social relationships (between teachers and students) and the psychological factors (e.g., cognitive ability, motivation) involved in such behavior. Alternatively, these very same behaviors may be described and studied at another level. The children's performance certainly involves the functioning of their physiological systems (e.g., their central nervous system, their endocrine system), a lower level (in the sense of underlying the behavioral level) of analysis. Ultimately, of course, the functioning of their physiological systems involves the functioning of the atoms and molecules that form the basic matter of living, organic material.

Accordingly, these other levels—psychology or physiology—are only "derivative." That is, *in reality*, they are nothing more than levels derived from combinations of the constituent, essential elements, the atoms and molecules governed by the laws of chemistry and physics. Hence, the split in mechanism between the essential or the real and the pseudophenomena of existence is apparent.

By splitting existence into the "real," or essential, level versus the "apparent but derivative" levels, mechanists can thus seek to understand psychological and social functioning by reference to the laws of physics and chemistry. These mechanisms represent the most fundamental level of analysis that can be reached, and because this level is invariably involved in any other level, scientists can certainly seek to understand psychology by reference to chemistry and physics. These basic physical laws are just as applicable to human development as they are to physiology, or for that matter to any other event or phenomenon in the natural world. Everything living or nonliving is made up of atoms and molecules. Ultimately, then, if scientists understand the rules by which atoms and molecules function, they can understand the components of all things in the natural world. All scientists must do to understand biology, psychology, sociology, or the movement of the stars is to bring each down to its most basic constituent elements, to the most fundamental level of analysis: the physical–chemical level. The events and phenomena of all sciences—of everything in the natural world—may be uniformly understood through the mechanisms involved in atoms and molecules.

Hence, proponents of the mechanist philosophical viewpoint would not seek to explain the phenomena of human development per se; this level is not the appropriate level of analysis. Rather, they would attempt to reduce these phenomena of physiological, psychological, and social functioning to the fundamental, essential level of analysis—the laws of chemistry and physics. The basic epistemological point of this mechanist position, then, is *reductionism*. That is, to gain knowledge of a level of phenomenal organization, the route to take is to reduce the phenomena of a given (higher) level to the elemental, fundamental (lower, or molecular), or essential units that comprise it.

Thus, adherents of mechanist thinking believe that there is nothing special about the complex pattern of events labeled physiological, psychological, or social functioning. In the final analysis these events involve the functioning of the very same atoms and molecules that are involved in the workings of a liver, a kidney, or a shooting star. Thus, like everything else, physiological, psychological, or social phenomena are governed by the laws of chemistry and physics and, upon appropriate reduction, may be understood in terms of those laws. From this standpoint, then, if scientists knew enough about chemistry and physics they could eliminate the sciences of human development, physiology, psychology, or sociology completely. For example, Homans (1961) proposed a stimulus–response view of social functioning that attempted to reduce the phenomena studied by sociology to the elemental units of classical and, especially, operant conditioning. In turn, Wilson (1975a) proposed a genetic reductionist view of social functioning (termed "sociobiology"; discussed in more detail in Chapter 12) which was aimed at eliminating the need for the sciences of psychology, sociology, and even anthropology. Thus, as pointed out by Bolles (1967, p. 5), this reductionist assumption involves "the doctrine that all natural events have physical causes, and that if we knew enough about physical and mechanical systems we would then be able to explain, at least in principle, all natural phenomena."

Reductionism directly implies a *continuity* position. No new laws are needed to explain the phenomena of a given level of study; rather, the same exact laws apply at all levels. Since natural phenomena at any and all levels can be reduced to the phenomena of the fundamental physical–chemical level, these same laws are continuously applicable to all levels of phenomena. Since no new, additional, or different laws are needed to account for or to understand the phenomena that may be thought to characterize any particular level, continuity by definition exists. Psychological or social functioning may be reduced to the level of chemistry and physics *because* the latter level is invariably present in anything that exists.

What this reductionism and continuity mean, then, is that the "real" laws governing any and all events in the world are really the laws of chemistry and physics. There is again, then, a split: The only real laws are those that pertain to the "to-be-reduced-to-level" (i.e., chemistry/physics). "Laws" about other levels do not reflect the reality of the true, causal, or essential phenomena of the natural world. In essence, the mechanist position holds that in the final analysis one must inevitably deal with specific fundamental laws in order to completely, accurately, and ultimately understand any and all living and nonliving matter in the natural world. And, as Anderson has commented, once this concept is accepted:

> It seems inevitable to go on uncritically to what appears at first sight to be an obvious corollary of reductionism: that if everything obeys the same fundamental laws, then the only scientists who are studying anything really fundamental are those who are working on those laws. In practice, that amounts to some astrophysicists, some elementary particle physicists, some logicians and other mathematicians, and few others.
>
> (Anderson, 1972, p. 393)

Because of the belief that reductionism will lead to fundamental knowledge, and because of the associated postulation of continuity in the laws and mechanisms that are involved in an appropriate consideration of natural phenomena, two events may ultimately occur. First, the phenomena in the world labeled psychological or social would no longer be a focus of scientific concern; these phenomena are not fundamental—they must be reduced to be appropriately understood. Second, the people in the world labeled physiologists, psychologists, sociologists, or human developmental scientists would no longer be necessary; these people are not studying the fundamental or essential phenomena of the natural world.

What would replace psychology, sociology, and in fact all sciences other than the "fundamental" ones, would be a consideration of the basic mechanisms of the physical–chemical level of analysis. From this perspective, to understand every event and phenomenon in the natural world a scientist must understand the mechanisms of physics and chemistry. This statement highlights another major, indeed defining, attribute of the mechanist position. Adherents of this position conceptualize the functioning of the components (the atoms and molecules) of the most fundamental level of analysis within the framework of a machine. As I have noted, according to this model, biological or psychological phenomena are only seemingly complicated constellations of physical and chemical processes. In principle, once scientists know the mechanisms of physical and chemical functioning, they know all they have to know about the world. In other words, because the fundamental level of analysis functions mechanistically, all the world is seen as functioning mechanistically.

Since physics and chemistry are machinelike sciences, all that must be done in order to move from one level of analysis to another is to specify the mechanism by which the basic elements of physics and chemistry combine. Since the molecular (physical–chemical) laws apply at the higher (physiological, psychological, or social) level, it is necessary only to discern the mechanisms by which these molecular elements are quantitatively added. In other words, to go to a higher level, all one must do is add these elements to what was present at the lower level.

If, by analogy, the nervous system was made up of 10 oranges, the circulatory system of 18 oranges, the respiratory system of 6 oranges, etc., all that

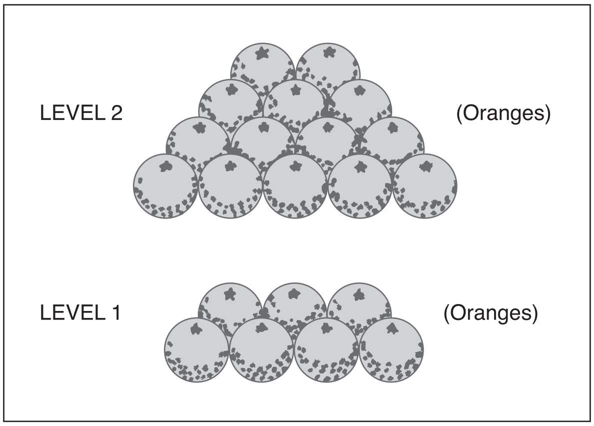


Figure 4.1 A representation of two levels of analysis within a mechanist model.

would be necessary for moving up to the psychological/behavioral level would be to add all oranges together. Thus the only difference between levels is a quantitative one, a difference in amount, size, magnitude, and so on.

The mechanist position is illustrated in Figure 4.1. Two levels of analysis are represented. Level 1, for example, could be the biological level and Level 2 the psychological level. Both are comprised of the same basic thing, in this case oranges. To move from one level to another, all one must do is add more oranges. Thus, between the two levels there is continuity in the basic elements that make up each level; put another way, each level can be reduced to the same basic elements.

In sum, when the mechanist philosophy of science is used as a framework from which to devise a theory of development, adherents of the mechanist model would view psychology, sociology, or human development as branches of natural science (e.g., see Bijou, 1976; Bijou & Baer, 1961). They would seek to reduce the phenomena of psychological or social functioning to basic mechanical laws (e.g., stimulus–response relations, as in Bijou & Bare, 1961, or Homans, 1961, or to the combinations of chemicals involved in the activity of genes, as in Plomin, 1986, Plomin et al., 2016, and Rowe, 1994). They would argue for such reductions because of their belief that these laws continuously apply to all phenomenal levels.

Hence, in this view, the phenomena of psychology or sociology are not unique in nature but are, rather, controlled by the laws that govern all events and phenomena in the natural world. The position thus holds that there are basic and common laws that govern all things in the universe. Neither biology, psychology, sociology, nor any science (other than physics or chemistry), for that matter, really has its own special laws; rather, in a basic sense, all sciences—and more important, all events and phenomena in the real world—are controlled by a common set of principles. It is believed that the phenomena, or events, that all sciences study can be uniformly subsumed (unified) and understood by one common set of natural-science principles (see Harris, 1957).

Thus, the basic characteristics of the mechanist position are:

1. It is a *natural-science* viewpoint.
2. It is a *reductionist* viewpoint.
3. It is a *continuity* viewpoint.
4. It is a *unity-of-science* viewpoint.
5. It is a *quantitative* viewpoint.
6. It is an *additive* viewpoint.

Translating the Mechanist Model into a Theory of Development

As I have illustrated briefly above, when the mechanist model is transformed, or translated, into a set of ideas pertinent to human development, a reactive, passive, or "empty-organism" model of humans results (Reese & Overton, 1970). From this perspective, the human is inherently passive; his or her activity results from the action of external forces, ones placed *on* the person through environmental stimulation or *in* the person through genetic inheritance. In either case, it is not the individual's own action that is the basis of his or her development. Rather, it is the force of nature (genes) *or* nurture (the stimulus environment) that is the real basis of the behavior of the developing person.

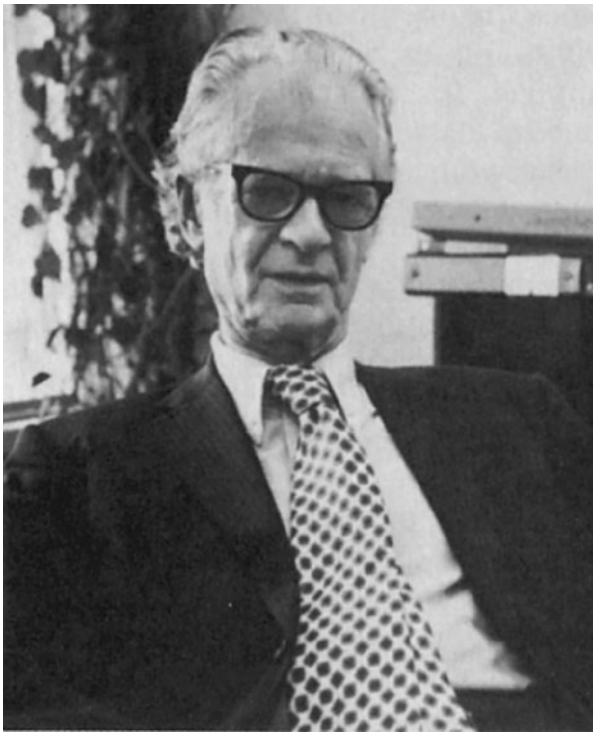
For example, in one behavioristic instance of this mechanist position, stimuli are held to evoke a response from a passive organism. The works of Skinner (1938), Bijou (1976), Baer (1982), Bijou and Baer (1961), and Gewirtz (1961) are representative of this position. These authors try to formulate the determining mechanisms of human behavioral development according to a natural-science model (Bijou & Baer, 1961). They attempt to discern the empirical (observable) and quantifiable parameters of environmental stimulation that fit this model (Gewirtz & Stingle, 1968).

Similarly, the nature, mechanist developmental theory of Rowe (1994) claimed to eliminate the need for "socialization science" by reducing all psychological and social phenomena to genes. Viewing behavior as a quantitative addition of discrete elements that combine, analogously, in the mechanical manner of chemistry and physics, nature or nurture mechanist theorists look to elements other than the individual as the source of human development. A machine is passive until extrinsic energy activates it. Human beings, viewed as machines, are also passive until environmental stimulation (nurture) or the genes inherited at conception (nature) cause them to act. Thus, human development becomes just the historical, "mechanical mirror" (Langer, 1969) of environmental stimulation or genetic determination. Moreover, as Reese and Overton (1970) explain, *changes* in the "products of the machine," that is, changes in the behavior of organisms, do *not* result from phenomena intrinsic to their "own" (individual) level (that is, their psychological, behavioral, or social relationship level); rather, again, changes result from (and can ultimately be reduced to) alterations in the stimuli impinging on them or the genes placed in them.

Thus, those scientists committed to such a mechanist position would, in their psychological theorizing, try to explain behavioral development in relation to the principles of classical and operant conditioning (e.g., Baer, 1982; Bijou, 1976; Bijou & Baer, 1961; Gewirtz & Stingle, 1968); or the principles of genetic inheritance (e.g., Plomin, 1986; Plomin et al., 2016); or the purported principles of genetic reproduction (such as gametic potential and inclusive fitness; Dawkins, 1976). Given that much of Chapters 11 and 12 is devoted to a discussion of these latter, nature, mechanist positions, it is useful to illustrate the "translation" of mechanist philosophy into a theory of human development by focusing on instances of nurture, mechanist theories.

A Nurture, Mechanist Theory of Development

All mechanist theories have a split ontology. They must split nature from nurture and hold that only one of these domains of potential influence is actually real or essential. Since the epistemology of mechanists is to reduce all phenomena to one common constituent level, it cannot be logically maintained that there are two *different* and real sources of influence on the person's development, nature *and* nurture. Either one source has to be reduced to another (as Rowe, 1994, does when he reduces family and other social influences to the activity of genes) or the other level has to be accepted as materially real but not functionally (efficiently) real (or relevant) in the determination of behavior and development. This latter split is the type typically adopted within nurture, mechanist theories of human development.


B. F. Skinner

In such viewpoints, humans are, at their core and/or initially in their ontogeny, basically passive entities, awaiting stimulation from the environment in order to act, or more accurately, to respond (e.g., Skinner, 1971). How does such stimulation bring human behavior under control?

Many mechanist-behavioristic theorists would suggest that the principles of classical (respondent) conditioning and of operant (instrumental) conditioning can explain it. The former set of principles can account for stimulation-produced responding (S → R) whereas the latter can account for response-produced stimulation (R → S). Given the broad applicability of these types of conditioning in the natural world, they should be able to account for the acquisition of the responses of organisms (Bijou, 1976; Bijou & Baer, 1961). Such mechanist–behavioristic theorists deal, then, with the generic human being—the general case of humanity. The laws of conditioning are ubiquitous in their applicability to all human behavior and, for that matter, to the behavior of all organisms (see Skinner, 1938). Thus external stimulation provides the material and efficient cause of behavior and development.

However, for the nurture-reductionism of Skinner (e.g., 1971) to work as a comprehensive explanation of the behavior of organisms (Skinner, 1938), there must be an *S* for every *R*. However, as pointed out by Bowers (1973), one of the key reasons that Skinner's approach fails is the problem of the missing *S*. For instance, as discussed in more detail in Chapter 11, when the male three-spined stickleback fish encounters another male three-spined stickleback with a red belly, the fish displays a set of behaviors indicative of threat (Lorenz, 1965; Richards, 1987). The other male fish was held to be *the* S that elicited the threat response behaviors (Lorenz, 1965). However, Richards (1987) notes that if another male fish has not been encountered for some period of time, the threat response behaviors appear without the purportedly necessary stimulus!

Simply, research has failed to identify an *S* (i.e., a discriminative stimulus, which has the status of a secondary reinforcing stimulus) for every *R* that exists. Yet, such an *S* is stipulated by Skinner to be needed to elicit operant behavior. If such stimuli are *the* cause of operant behavior in any given situation, then how can empiricists hold that the *S*–*R* formulation (S^D–R–S^R) is useful when there are so many *R*s for which there are no *S*s to be seen? They cannot. As such, the radical behaviorism of Skinner (e.g., 1938, 1971) is reduced to a view that must be accepted on the basis of faith (that there must have been an *S* somewhere) and not on empirical evidence.

Nevertheless, despite the presence of this key conceptual problem, from a mechanist, behavioristic perspective, organisms differ across their life span only in the quantitative presence of qualitatively identical behavioral units, such as elements of the behavioral repertoire acquired by the causally efficient laws of conditioning (e.g., Bijou, 1976; Bijou & Baer, 1961). As such, the organism is seen as a host (Baer, 1976) of these elements, and even the most complex human behavior is believed reducible to these identically constituted units (Bijou, 1976). The only constraint on behavioral change in a "consequent" period of life is imposed by past (i.e., antecedent) reinforcement history; that is, the repertoire of behaviors present in the organism at any point in time may moderate the efficiency by which current stimuli can extinguish or otherwise modify any particular behavior in the repertoire. As I will explain, however, the meaning of "past reinforcement history" may be such as to preclude any strong view of the potential for developmental change beyond the earliest periods of life.

Indeed, from the mechanist, behavioral perspective, no strong (i.e., idealized) view of development is present. Instead, the concept of development is reduced to a concept of change in the elements of the behavioral repertoire; therefore, the processes by which change is brought about are by addition to or subtraction from the behavioral repertoire via conditioning. Consequently, change at any point in life becomes largely a technological matter always occurring with regard to past reinforcement history, and pertaining to such issues as management of stimulus contingencies and of reinforcement schedules (e.g., in regard to building up, reducing, or rearranging a behavioral "chain" of stimulus–response connections).

However, interindividual differences in response to a stimulus or interindividual differences in intraindividual change may become particularly problematic from this perspective. The only way in which such differences may be accounted for is by reference to differences in past reinforcement history, a history that may be typically uncharted among humans. Indeed, two organisms exposed to the same stimulus history who nevertheless react differently to the same immediate stimulus would present a formidable interpretative problem for this perspective (since an internal organizing structure independent of past stimulus history is not part of this model). Thus, because humans even from quite similar backgrounds (e.g., identical twins reared together; Joseph, 2015) may not behave in exactly the same way, scientists functioning from this perspective are forced to account for such differences by postulating some unseen but efficiently causal difference in stimulus history or by arguing that such behavioral differences arise merely as a consequence of errors of measurement. Alternatively, such differences may be ignored.

Sidney Bijou

Donald Baer

That is, given the belief in the continuous and exclusive applicability of, and only of, functional (which in this perspective means efficient, and in some cases material; Skinner, 1966) stimulus–behavior relations, *only* the most simplistic view of the context is found in this perspective (e.g., Bijou, 1976). I do not use the term "simplistic" in any pejorative sense; rather, it serves to indicate that in the behavioristic tradition a scientist can use only those features of the context—that is, the stimulus environment in the terms of this perspective—that can be translated into stimulus–response units. Features of the context that cannot be translated (i.e., reduced) into such units are invisible in this approach.

For instance, sociopolitical historical events or emergent qualitative changes in social structures must either be reduced to elementaristic, behavioral terms or ignored. Moreover, because of a necessarily unequivocal commitment to reduce to efficiently causal antecedents, a strict mechanist–behavioral position (e.g., the functional-analysis position of Baer, 1982) must be committed to the views (1) that early (indeed the earliest) stimulus–response experience is prepotent in shaping the rest of life; and (2) that, therefore, there can be no true novelty or qualitative change in life. Taken literally, a belief that any current behavior or event can be explained by or reduced to an antecedent efficient cause or a stimulus, means in behavioral terms that all of life must ultimately be explainable by the earliest experience of such antecedent–consequent relations. That is, any portion of "later" life must be explained by efficiently causal prior events. Thus nothing new or qualitatively distinct can in actuality emerge consequent to these initial events.

Zukav (1979) explains this feature of mechanist thinking (in regard to Newtonian physics) by noting that:

> If the laws of nature determine the future of an event, then, given enough information, we could have predicted our present at some time in the

> past. That time in the past also could have been predicted at a time still earlier. In short, if we are to accept the mechanistic determination of Newtonian physics—if the universe really is a great machine—then from the moment that the universe was created and set in motion, everything that was to happen in it already was determined. According to this philosophy, we may seem to have a will of our own and the ability to alter the course of events in our lives, but we do not. Everything, from the beginning of time has been predetermined, including our illusion of having a free will. The universe is a prerecorded tape playing itself out in the only way it can.
>
> (Zukav, 1979, p. 26)

In short, the nurture, mechanist–behavioral position represents a "translation" into psychological theory of the natural science, efficiently causal philosophy that Zukav (1979) describes in regard to Newtonian physics; that is, the first physical antecedent–consequent relation is transformed into the first, or at least quite an early, stimulus–response connection. In addition, although it is not emphasized in many discussions of mechanist–behavioral views, for example the functional-analysis perspective (Baer, 1982; Reese, 1982), the early proponents of this view were quite clear in their belief that early experience was prepotent in shaping all of life (see Kagan, 1983).

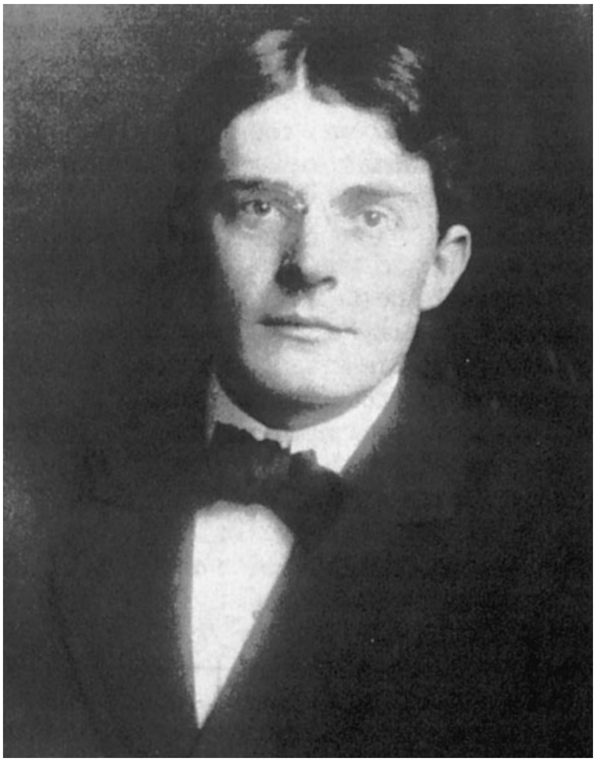
For instance, John B. Watson (1928) argued that "at three years of age the child's whole emotional life plan has been laid down, his emotional disposition set" (p. 45). Moreover, Watson (1924) boasted that:

> Give me a dozen healthy infants, wellformed, and my own specified world to bring them up in and I'll guarantee to take any one at random and train him to become any type of specialist I might select—doctor, lawyer, artist, merchant-chief and yes, even beggarman and thief, regardless of his talents, penchants, tendencies, abilities, vocations, race of his ancestors.
>
> (p. 82)

However, what often goes unrecognized is that Watson (1924) knew that this assertion about the efficacy of the application of radical Behavioristic ideas in shaping behavior was quite overstated. That is, he admitted that: "I am going beyond my facts and I admit it, but so have the advocates of the contrary and they have been doing it for many thousands of years" (p. 82).

Nevertheless, Watson, and other Behaviorists, in seeking to counter what they regarded as the similarly overstated claims of nativists, continued to insist on the primacy of early experience. For example, Edward Thorndike (1905) contended that:

> Though we seem to forget what we learn, each mental acquisition really leaves its mark and makes future judgment more sagacious . . . nothing of good or evil is ever lost; we may forget and forgive, but the neurones never forget or forgive . . . It is certain that every worthy deed represents a modification of the neurones of which nothing can ever rob us. Every event of a man's mental life is written indelibly in the brain's archives, to be counted for or against him.
>
> (pp. 330–331)



John B. Watson

Such views constitute a belief that the potential changes able to be induced in the person by later experience are quite limited, and that the potential for plasticity in later childhood, adolescence, and in the adult and aged years is markedly constrained by "early experience," by "past reinforcement history."

In sum, the point of the present discussion is to make clear the general nature of the translation of the mechanist, reductionist philosophical position into the psychological theoretical position of such nurture, mechanist, behaviorist psychologists as Bijou (1976), Bijou and Baer (1961), and Baer (1982). To such psychologists all behavioral functioning is a consequence of stimulation. To understand behavior at any and all points in development, all one must do is understand the laws by which a person's responses come to be under the control of environmental stimulation.

As I have noted, scientists functioning from this viewpoint often contend that there are two sets of laws that describe and explain how responses come under environmental stimulation: those of classical and of operant conditioning. Since all behavior is ultimately controlled by the stimulus world, and since this world exerts its control through the functioning of a fundamental set of laws of conditioning, then all behavior may be understood by reducing it to these same basic laws of stimulus–response relations. All behavior—whether of two different species of animals (rats and humans) or of two different age-groups of children (5- and 15-year-olds)—is composed of the same basic stimulus–response elements, and these same basic elements are also always associated on the basis of the same laws. Hence, seemingly complex behavior may be understood by reducing it to the same basic constituent elements that make up any and all behavior. And since all behavior may be so reduced, the same laws must therefore be applicable to explain behavior at any (animal or human age) level at which it occurs. Continuity in the laws of conditioning, in the rules that account for behavioral functioning, is another aspect of this approach.

Thus, all that one must know in order to completely understand behavioral functioning and development is the mechanisms by which stimuli in a person's world come to control that person's behavior at all points in the life span. Once these mechanisms are known, one can reduce behavior at different points in life to common constituent elements. In turn, since the same elements comprise behavior at each level, a scientist adopting the mechanist position could account for any differences in behavior between points in the life span merely by reference to the quantitative difference in the stimulus–response relations in the person's behavior repertoire. If behavior is composed totally of the stimulus–response relations a person has acquired over the course of life as a function of conditioning, then the difference between behavior at any two points in life could only be a quantitative one involving the number of associations acquired. A scientist could move from lower to higher levels of behavior analysis simply by adding on the similarly acquired stimulus–response associations.

By this point, then, the way in which the nurture, mechanist model becomes translated into a theoretical view of human development should be clear. This position has been influential in science in general and in the field of human development in particular. One basis for this influence in human development is that behavioral phenomena traditionally associated with Behaviorism, for example, classical and operant conditioning, desensitization, and behavioral shaping, depict important features of behavioral *change*, if not behavioral *development*. Thus, the mechanist position frames what are, descriptively, important means through which person–context relations (typically reduced of course to stimulus–response connections) may change.

Accordingly, in providing the philosophical basis of the empirical behaviorist approach, or the functional-analysis (Baer, 1982; Bijou, 1976; Reese, 1982) approach, to human development, the mechanist position presents what has been an influential philosophical/psychological view of the nature of humanity. In addition, the mechanist metatheory continues through this writing to be a major part of developmental science.

For instance, information processing theory uses a computer as an analog for cognitive development, focusing on quantitative variation in "mechanisms" of encoding, storage, and retrieval (Siegler, 2003, 2009). Eschewing qualitative changes in cognitive development (as is stipulated to occur in transformative models, such as stage theories of cognitive development; e.g., Turiel, 2015; Turiel & Nucci, 2018; Overton, 2015a), the information processing approach emphasizes variational (quantitative) changes pertinent to facets of cognition such as memory (e.g., Dixon, 2000), theory of mind (e.g., Harris, 2006), and problem solving, for instance, selective attention (e.g., Huanh-Pollock, Nigg, & Carr, 2005), and automatization (e.g., Bjorklund, 1990; Moores, Nicolson, & Fawcett, 2003).

Shoda, Cervone, and Downey (2007) noted that the cognitive "revolution" that occurred in the second half of the twentieth century (e.g., that was linked to the rediscovery of the work of Piaget, 1950, 1970) resulted in the formulation of more recent mechanist formulations of cognitive development, such as information processing theory. However, they also pointed out that these "new" mechanist formulations were criticized by one of the primary founders of the cognitive revolution, Jerome Bruner (1990), "as being so enamored with the information-processing capacities of intelligent machines that it failed to confront the meaning-making capacities of agentic persons" (p. 4).

Yong (2017) has summarized similar criticisms of the reductionism that exists in neuroscience. Yong noted that many neuroscientists focus their work on studying individual neurons or networks of neurons. This focus is based on the assumption that by understanding these parts of the organism, the explanation of its behavior will be identified. That is, the idea is that, by ascertaining the chemicals present at synapses, the course of electrical transmission across a specific neuron, or even a network of neurons, the mechanisms of cognition and emotion will be apparent. However, Yong (2017) notes that there are some neuroscientists who understand that behavior is an emergent property of the organism, and that such reductionism fails to identify emergent properties of the organism.

For instance, Greenberg and Lambdin (2007) note that "what the insertion of microelectrodes into neurons can tell us is the sensitivity of that neuron to certain stimuli and conditions. Nothing from such information follows about how consciousness is produced" (p. 466). In addition, they go on to note that:

> Psychology has amassed a great many facts in debates between cognitive nativists and behaviorist environmentalists since the days of Wundt. It became clear in the latter half of the 20th century that these debates were intractable because, in part, proponents of these various schools of thought were examining their own small parts of the same integrated biopsychosocial system. The central task of scientific psychology in the 21st century should be to articulate a set of principles that can fruitfully organize the set of factors ranging from genetic to neural, to intrapersonal, to micro- and macroecological. Our take on the evidence at hand is that development is that organizing principle, specifically the metatheoretical perspective of developmental systems. In sharp contrast to our colleagues, made weak by finding and fact, yet failing to yield and clinging to the search for an organizing "executive agent," be it the brain or the gene, the developmental systems perspective builds on the empirical properties of dynamic complex adaptive systems.
>
> (Greenberg & Lambdin, 2007, p. 472)

In short, the mechanism and reductionism associated with information processing theory, with some facets of neuroscience, and with other contemporary instances of mechanist theories, for example, the neo-nativist approaches of Spelke and Newport (e.g., 1998) and Keil (e.g., 1998, 2006), have been critiqued by advocates of dynamic systems approaches to human development (e.g., Feldman, 1994; Fischer & Bidell, 2006; Mascolo & Fischer, 2015). I discuss the features of the theoretical differences between mechanist and RDS-based approaches to cognitive development in subsequent chapters and, in particular, in Chapter 10.

In sum, as indicated by Bruner (1990) and Yong (2017), mechanist formulations (of either the nurture or nature variety) have had important criticisms leveled at them. In fact, it is possible to view the organismic position as a culmination of the objections raised about the assumptions and assertions of the mechanist position (von Bertalanffy, 1933). Hence, as a means of transition to my discussion of the organismic position, I will first note some of the important problems of the mechanist position.

Problems of the Mechanist Model

I have noted that the core conceptual basis of the mechanist model is reductionism. I have also indicated that the belief in reductionism is predicated on the assertion that, because all matter is made up of basic (e.g., physical–chemical) components, the only essential, necessary, and sufficient approach to investigating the fundamental laws of the natural world is to study these basic components. Hence, the adherent of the mechanist model asserts that to understand any and all levels of phenomena in the real world, these higher levels must be reduced to the laws of the fundamental constituent level. However, Anderson (1972), in describing the reductionistic component of the mechanist position, also sees the viewpoint as advancing an argument containing a logical error:

> The main fallacy in this kind of thinking is that the reductionist hypothesis does not by any means imply a "constructionist" one: The ability to reduce everything to simple fundamental laws does not imply the ability to start from those laws and reconstruct the universe. In fact, the more the elementary particle physicists tell us about the nature of the fundamental laws, the less relevance they seem to have to the very real problems of the rest of science, much less to those of society. (p. 393)

But why does the ability to reduce from a higher, seemingly more complex, level of analysis to the lower level not necessarily imply the reverse? Why does such reductionist ability not imply that a scientist can move from the lower to the higher levels—and thereby construct the universe—by simply adding more of the same constituent elements onto what already exists at a lower level? Why, when scientists attempt to do this additive analysis, and when scientists concomitantly learn more and more about the fundamental level, do they seem to be missing an understanding of the important problems and phenomena of the higher levels? Why does the reductionist fail when attempting to also be a constructionist? Again I may turn to Anderson (1972):

> The constructionist hypothesis breaks down when confronted with the twin difficulties of scale and complexity. The behavior of large and complex aggregates of elementary particles, it turns out, is not to be understood in terms of a simple extrapolation of the properties of a few particles. Instead, *at each level of complexity entirely new properties appear*; and the understanding of the new behaviors requires research which I think is as fundamental in its nature as any other. That is, it seems to me that one may array the sciences roughly linearly in a hierarchy, according to the idea: The elementary entities of science X obey the laws of science Y.
>
> | X | Y |
> |---|---|
> | Solid-state or many-body physics | Elementary particle physics |
> | Chemistry | Many-body physics |
> | Molecular biology | Chemistry |
> | Cell biology | Molecular biology |
> | . | . |
> | . | . |
> | Psychology | Physiology |
> | Social sciences | Psychology |
>
> But this hierarchy does not imply that science X is "just applied Y." *At each stage entirely new laws, concepts, and generalizations are necessary*, requiring inspiration and creativity to just as great a degree as in the previous one. Psychology is not applied biology, nor is biology applied chemistry. (p. 393, italics added)

What Anderson is saying, therefore, is that the constructionist hypothesis fails because, simply, "more is different." In other words, as one studies levels of higher and higher complexity, one concomitantly sees that new, qualitatively different characteristics come about—or emerge—at each of these levels. The new characteristics are not present at the lower, fundamental level and are therefore not understandable by reduction to it. One cannot move from higher to lower levels (and back again) merely by adding or subtracting more of the same.

Attempts at such movement fail because, as one combines more of the same into a higher level of complexity, this combination has a quality that is not present in the less complex constituent elements as they exist in isolation. Thus, the reductionist, mechanist position fails because reductionism does not mean constructionism, and in turn, constructionism fails because of the presence of qualitatively new properties emerging and characterizing each higher level of analysis.

However, reductionism fails for other reasons as well. Reductionism is predicated on the belief that reference to the constituent elements comprising all matter can suffice in accounting for the nature of phenomena at all levels of analysis. However, I have noted that this continuity assumption is weak. If new, qualitatively different phenomena characterize each higher level of analysis, then, by definition, continuity does not exist. If something new does exist, this existence clearly means that just the same things that existed before do not exist.

In other words, the shortcomings of the reductionist, mechanist position—whether it is a nature or a nurture version of such a view—also include the inadequacy of its continuity assumption. The mechanist philosophical position is unable to explain all natural phenomena through reduction to one set of fundamental laws because:

> This conception appears to ignore the additional fact that once the behavior has been explained physiologically, the physiology still remains to be explained (cf. Skinner, 1950). Furthermore, if physiology in turn is to be explained by biochemistry and it by physics, how physics is to be explained poses an enduring problem because there are no sciences left. In short, this type of explanation leads to a finite regression with one science left unexplained, unless, of course, it is self-explanatory; no one is likely to admit that of physics.
>
> (Eacker, 1972, p. 559)

Thus, there are many problems with the mechanist model. It fails to suffice in accounting for the nature of the phenomena present at all levels of analysis because at each level of analysis there exist qualitatively new, and hence discontinuous, phenomena. Hence, developmental scientists should perhaps turn to a point of view that emphasizes these phenomena. What is being alluded to, then, is the fact that the very objections raised about the mechanist position seem, in their explication, to suggest the necessary characteristics for a point of view that would successfully counter the position. Specifically, mechanist constructionism fails because new phenomena emerge to characterize higher levels of analysis; therefore, the first component of a successful alternative position would be one positing the emergence of qualitatively discontinuous changes as characterizing development.

This notion of emergence would be introduced to counter the problems of reductionism, and the idea of qualitative discontinuity would be raised to address the inability of a mechanist constructionist position to account for all phenomena present at all levels. In essence, a developing organism would be viewed as an individual passing through qualitatively different levels (e.g., phases or stages) of development, periods made different because of the presence of new (and hence lawfully distinct) phenomena emerging to characterize that portion of the life span. These alternative views of the nature of differences between levels of analysis, or between portions of the ontogeny of an organism, are represented in the organismic philosophy of science, which offers a view of the world that contrasts with the mechanist view.

THE ORGANISMIC MODEL

As explained by Reese and Overton (1970), the organismic model has as its basic metaphor "the organism, the living, organized system presented to experience in multiple forms" (p. 132). Moreover, Reese and Overton (1970) go on to note that:

> The essence of substance is activity rather than the static elementary particle proposed by the mechanist model . . . In this representation, then, the whole is organic rather than mechanical in nature. The nature of the whole, rather than being the sum of its parts, is presupposed by the parts and the whole constitutes the condition of the meaning and existence of the parts . . . the

> important point here is that efficient cause is replaced by formal cause (i.e., cause by the essential nature of a form).
>
> (p. 133)

Adherents of one or another version or instance of the organismic philosophy of science (e.g., Bertalanffy, 1933; Schneirla, 1957; Tobach, 1981) reject the reductionism of mechanism and maintain that at each new level of phenomenal organization there is an emergence of new phenomena that cannot be reduced to lower levels of organization. They hold that one cannot appropriately make a quantitative reduction to a lower organizational level and hope to understand all phenomena at the higher organizational level. This inability to reduce occurs because at each higher organizational level there is "the *emergence of novelty*" (Overton, 2006, p. 26; see also Novikoff, 1945a, 1945b). Thus, a change in quality and not merely in quantity characterizes the differences between one level of analysis and another. If one reduces to the lower level, one eliminates the opportunity of dealing with the new characteristic (which is actually the defining characteristic) of the higher level, the very attribute that represents the difference between the lower and higher levels.

For example, going from one animal level to another, or from one period of human life to another, would be analogous to changing from an orange into a motorcycle. How many oranges comprise a motorcycle? Obviously, this is a ludicrous question, because here we have a change in kind, type, or quality, rather than merely in amount, magnitude, or quantity.

As Overton (2006) explained, examples of qualitative, transformational change may be seen across human development and across human developmental domains:

> Embryological changes constitute some of the clearest and most concrete examples of transformational or morphological change (Edelman, 1992; Gottlieb, 1992). Through processes of differentiation and reintegration, movement occurs from the single celled zygote to the highly organized functioning systems of the 9-month fetus. Some cognitive and social-emotional phenomena of human ontogenesis have also been conceptualized as reflecting transformational change. For example, overt action may undergo a sequence of transformations to become symbolic thought, and further transformations lead to a reflective symbolic thought exhibiting novel logical characteristics . . . Memory may reflect transformational changes moving from recognition memory to recall memory. The sense of self and identity (Chandler, Lalonde, Sokol, & Hallett, 2003; Damon & Hart, 1988; Nucci, 1996) have been portrayed by some as moving through a sequence of transformations. Emotions have been understood as differentiations from an initial relatively global affective matrix (Lewis, 1993; Sroufe, 1979). Physical changes, such as changes in locomotion, have also been conceptualized as transformational changes (Thelen & Ulrich, 1991, p. 27).
>
> (p. 111)

The above argument—the irreducibility of a later form to an earlier one—is the essence of the *epigenetic viewpoint* (Van Speybroeck, Van de Vijver, & De Waele, 2002), the view that human development is characterized by transformational change (Overton, 2006), the emergence of new forms across ontogeny (see Chapter 2). One cannot reduce a qualitative change, something new, to a precursory form. Epigenesis denotes that at each higher level of complexity there emerges a new characteristic, one that simply was not present at the lower organizational level and thus whose presence is what establishes a new level as just that—a phase or period of organization qualitatively different from a preceding one. Thus, according to Gottlieb (1970, p. 111), epigenesis connotes that patterns of behavioral activity and sensitivity are not immediately evidenced in the initial stages of development. Since development is characterized by these qualitative emergences, then, by definition, the various new behavioral capacities that develop are not actually present until they do in fact emerge (Witherington & Lickliter, 2016).

The doctrine of epigenesis thus asserts that development is characterized by qualitative "emergences" (Raeff, 2016; Werner, 1957). Simply, new things come about in development. Newness means just that: Something now exists that was not present before, either in smaller or even in precursory form. Simply, then, epigenesis asserts that development is represented by the emergence of characteristics at each new period of development that were not present in any precursory form before their time of appearance.

The presence of qualitatively new characteristics at each higher period indicates that reduction to lower levels is inappropriate—if full understanding of the new period of life is sought. For instance, specific newborn behaviors may *perhaps* be understood by reference to relatively simple stimulus–response, reflex-like associations; yet, when the child is between 1 and 2 years of age a new symbolic function will typically emerge—language (as an example of the ability to represent physical reality through use of nonphysical symbols). Thus, as one consequence of this representational ability, the child may now show behaviors (e.g., being able to imitate some person or event long after the time of actual viewing) that can best be understood by reference to this emergent symbolic ability (e.g., Piaget, 1970). Trying to reduce a 2-year-old's behavior to the functioning shown at an earlier age would be inappropriate because the representational ability that may account for the 2-year-old's behavior was simply not present at the earlier age.

Thus, an antireductionist view is maintained because the qualitative change that depicts a higher period (level) of development cannot be understood, since it does not exist, at the lower level. Because the nature of what exists changes from level to level, and thus because there is qualitative change from level to level, there cannot be complete continuity across levels or periods. New things—variables, processes, and/or laws—represent the differences between levels; hence such qualitative change means that discontinuity (at least in part) characterizes differences between periods of life. Such differences are in *what* exists and not just in *how much* exists. Thus, to the organismic thinker, laws of the psychological level of analysis are unique in nature—they are not merely reducible to the laws of physics and chemistry. Similarly, each different phyletic or ontogenetic level is viewed as having features qualitatively discontinuous from every other.

This aspect of the organismic position is represented in Figure 4.2, which shows qualitative discontinuity between the two levels represented. Because something new has emerged at the higher level, one cannot reduce one level to another. Although Level 1 is comprised of oranges, Level 2 is a motorcycle. One cannot hope to understand the functioning of a motorcycle even through an intensive study of oranges!

But, on what basis do proponents of the organismic viewpoint assert that qualitative discontinuity characterizes development? How do organismic thinkers explain their assertion that epigenesis—qualitative discontinuity (Werner, 1957)—represents differences between levels?

Organismic thinkers would be in agreement with an idea borrowed from Gestalt psychology, that the whole (the organism) is more than the sum of its parts. That is, a human organism is more than a liver added to two kidneys, added to one spinal cord, and to one brain, and to one heart, and so on. Similarly, a human organism is more than the sum of carbon, plus hydrogen, plus oxygen, plus nitrogen, plus calcium, etc. They would reject the linear, additive, and implicitly split assumption underlying the mechanist position, and instead maintain that a fused, integrative, and holistic type of combination would be more accurate. They would argue that organisms, as organized and relational systems, show in the integrations among their constituent elements (their

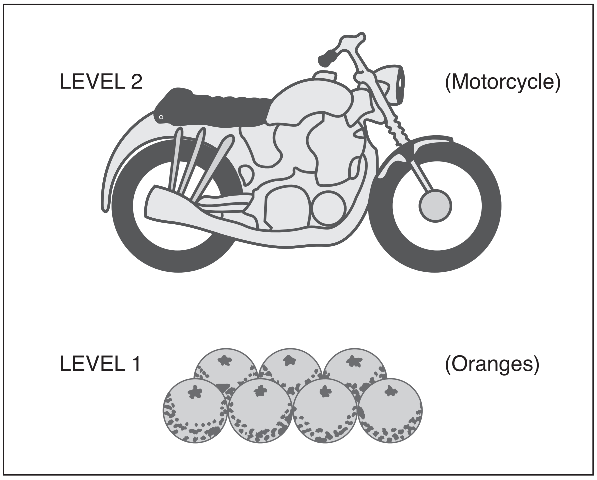


Figure 4.2 A representation of qualitative discontinuity across levels within the organismic model.

parts) properties that cannot be reduced to physical and chemical terms (Overton, 2015a; Raeff, 2016).

A developmental scientist may reach a physical and chemical understanding of a kidney, a brain, and a liver. However, properties will be seen in the organism considered as a whole that derive not from the separate organ systems per se but from their relations (integrations) with each other. When parts combine they produce a property that did not exist in the parts in isolation. The parts do not merely add up (e.g., 2 + 3 = 5) but multiplicatively interrelate (e.g., 2 x 3 = 6; in this example one more unit is present than with the additive combination), and this interrelation brings about the emergence of a new property.

In essence, the organismic viewpoint asserts that the basis of the epigenetic (qualitatively discontinuous) emergences that characterize development lies in the multiplicative interrelations of the constituent parts of the organism. When the parts combine they produce a new complexity, a characteristic existing only as a product of the coaction of these parts. This new property does not exist in any of the constituent parts—or in any of the lower organizational levels—even in precursory form.

Ludwig von Bertalanffy

Ludwig von Bertalanffy (1933), a leading formulator of the organismic viewpoint, suggested that nothing can be learned about the organism as such from a study of its parts in isolation. This inability exists because an organism in its natural state, viewed as a whole being (and not just as a bunch of constituent parts), shows phenomena that are so different from physical, mechanist ones that entirely new concepts are needed to understand them.

Thus, if one accepts the epigenetic, organismic point of view, a mechanist, reductionist view of organisms is entirely inappropriate. The characteristics of a whole living organism have nothing in common with the characteristics or structures of a machine (or a brain, kidney, liver, or other organ split off from the whole organism) (Greenberg, 1983, 2011). Such distinctions occur because a machine can be fully explained through an examination of its parts. This approach is not the case with living organisms. With living organisms, at each new level of analysis an emergence takes place; with every step building up to the whole living organism, from an atom to a molecule to a cell to a tissue to an organ, new phenomena occur that cannot be derived from the lower, subordinate levels.

Thus, knowledge of the functioning of the various subsystems that make up an organism does not lead to an understanding of the whole organism. For example, water has an emergent quality (its liquidness) that cannot be understood by reducing water to its constituent (and gaseous) elements (hydrogen and oxygen). Similarly, human beings have unique characteristics (or qualities), such as being able to love, being governed by abstract principles of moral and ethical conduct, or showing high levels of purposive, goal-directed behavior (Damon, 1998, 2008), that emerge as ontogenetically distinct (qualitatively discontinuous) features and cannot be understood by mere reduction to underlying neural, hormonal, and muscular processes. As I have already noted, a basis for this position put forward by organismic theorists is a belief in epigenetic processes—that is, a belief that at each new level of behavioral organization there emerge qualitatively new (discontinuous) phenomena that cannot be reduced to lower levels.

In summary, the basic characteristics of the organismic position are:

1. It is an *epigenetic* viewpoint.
2. It is an *antireductionist* viewpoint.
3. It is a *qualitative* viewpoint.
4. It is a *discontinuity* viewpoint.
5. It is an *integrative, relational*, or *multiplicative* interrelational viewpoint.

Translating the Organismic Model into a Theory of Development

When the organismic model is translated into a set of ideas pertinent to human development, an active organism model of humans results. From this perspective the human is inherently active; that is, it is the human who provides a source of his or her behaviors in the world, rather than the world providing the source of the human's behaviors (Brandtstädter, 1998, 2006). Humans, by virtue of their structure, give meaning to their behavior; that is, they provide it with organization—with form—by virtue of integrating any given behavior into the whole. Thus, the actions of a human are *constructors* of his or her world (Lerner, 1982; Lerner & Busch-Rossnagel, 1981b; Lerner & Walls, 1999). Moreover, as a consequence of the inherent activity of humans, change, or development, is accepted as given (Reese & Overton, 1970). In other words, change may not be reduced to efficient or material causes, although such causes may impede or facilitate change. Rather, the structure or configuration of mental or behavioral life, the integrated lattice of relationships, or simply the form of the whole, is the basis of the individual's development (Raeff, 2016). In short, formal cause is basic in the organismic perspective (Reese & Overton, 1970).

Accordingly, from the organismic perspective, development of a given process (e.g., cognition) is an idealized and goal-directed intraorganism phenomenon. As explained by Pepper (1942):

> With organicism, no ordinary common-sense term offers a safe reference to the root metaphor of the theory. The common term "organism" is too much loaded with biological connotations, too static and cellular, and "integration" is only a little better. Yet there are no preferable terms. With a warning we shall accordingly adopt these [p. 280] . . . The categories of organicism consist, on the one hand, in noting the steps involved in the organic process, and, on the other hand, in noting the principal features in the organic structure ultimately achieved or realized. The structure achieved or realized is always the ideal aimed at by the progressive steps of the process [p. 281] . . . The pivotal point in the system . . . is the goal and final stage of the progressive categories and it is the field for the specification of the ideal categories.
>
> (p. 283)

Qualitative change, forged by the inevitable synthesis of contradictions—as for example represented by emergent structural reorganization (e.g., Piaget, 1970) or focal reorientation in the mode of dealing with the world or with gratifying one's emotions (e.g., Erikson, 1959; Freud, 1954)—is seen as the key feature of development. Thus, the organismic approach is a holistic one, one wherein *formal* cause is present.

In addition, in its "purest" philosophical formulation organicism also includes *final* cause, also termed teleological, or goal-directed cause. Final cause, the telos, also provides the basis of developmental explanation (Nagel, 1957; Pepper, 1942). That is, in organicism there is a goal for development: to achieve the form the organism is inherently destined to take. This goal serves to direct the development of the organism, literally pulling the individual toward his or her final end state. By analogy, the ideal of the full flower in bloom—for instance, of the rose—pulls the seed, the bud, etc. in the direction of this future form. Such teleology, or goal-directedness, means that future idealizations of the organism—its final, fully developed form—direct change within the individual during the present. The telos shapes the organism in manners that enable it to attain the final form (e.g., the formal operational stage; Piaget, 1970) that is the goal of development.

However, given this formal and final explanatory orientation, especially when it is cast within a teleological, idealized view of developmental progression, material and efficient causative agents—for instance, as derived from the context enveloping the organism—are seen as irrelevant to the sequence of development, and as such to the form the organism takes at any point in this sequence. Said in another way, the inherent and goal-directed form of the organism is the basis of development, and any other potential source of influence on change across ontogeny is, at best, of only secondary importance. Thus, the context can facilitate or inhibit (i.e., speed up or slow down) developmental progression, but it cannot alter the quality of the process or its sequential universality. If a contextual variable does alter the quality or sequence of an organism's progression, then by definition that feature of functioning was not a component of development.

Ironically, then, although constituting an alternative to the nature–nurture split conceptions of mechanism, the "classic" (Reese & Overton, 1970) developmental version of organicism becomes also a split position! Although not involving as complete a split as in mechanism, wherein only one domain—nature *or* nurture—can be real, classic organicism (e.g., as in the developmental psychologies of Jean Piaget or psychoanalysis; Wolff, 1960) sees nature as of primary importance in life, as more *real* than nurture (Putnam, 1987). This inequality exists because nature provides the formal cause of developmental change. Nurture variables exist as well, however, in the view of classic organicists. The contribution of nurture variables to development is only secondary, in that their only influence is to create some variation in the rate of appearance of features of primarily intrinsic trends (e.g., such as pace of progression to the teleologically directed final form of development).

Gottlieb (1970) has labeled this version of organicism as *predetermined epigenesis*. An early version of Victor Hamburger's (1957) organismic position epitomizes this view:

> The architecture of the nervous system and the concomitant behavior patterns result from self-generating growth and maturation processes that are determined entirely by inherited, intrinsic factors, to the exclusion of functional adjustment, exercise, or anything else akin to learning.
>
> (p. 56)

It should be noted, however, that Hamburger (1973) later repudiated this view of epigenesis and adopted a position akin to an alternative view, one labeled as *probabilistic epigenesis* (Gottlieb, 1970, 1983, 2003; Gottlieb, Wahlsten, & Lickliter, 2006).

The features of probabilistic epigenesis are associated with RDS-based models (Ford & Lerner, 1992; Gottlieb, 1991, 1992, 1997; Sameroff, 2009; Thelen & Smith, 1994, 2006). This correspondence will be raised again, in the context of a discussion of how ideas associated with the mechanist and the organismic models pertain to several key issues of development. The rationale for this presentation is that, as was the case with the mechanist position, there are several problems that may be identified with the organismic position. These problems come to the fore when comparing the mechanist and the organismic models' positions in regard to several key developmental issues. However, I will argue that many of these problems can be usefully addressed by adopting a probabilistic-epigenetic, rather than a predetermined-epigenetic, view of organicism.

But, as I shall argue too, the adoption of such a view actually constitutes a divorce from "pure" organicism. In fact, what such an adoption does is to create a "marriage" (an integration) between organicism and another model useful in devising an approach to development. As I shall point out in various portions of the next section, organicism and contextualism are often intimately related philosophically (Overton, 1984; Pepper, 1942). Thus, the "marriage" I propose is one between quite compatible models. I will introduce some of the compatibilities between organicism and contextualism in the next section. In turn, by developing the argument for the usefulness of probabilistic epigenesis, in the context of a comparative discussion of mechanism and organicism, and of the uses and problems with each metatheory, I shall be setting the stage for both a direct treatment of the contextual model and a discussion of its uses and limitations. Therefore, I shall be setting the stage for a proposal about an integration of the organismic and the contextual models—an integration that is labeled in Chapter 9 as developmental contextualism, an instance of a relational developmental systems theory.

MECHANIST AND ORGANISMIC MODELS AND ISSUES OF DEVELOPMENT

It is useful to begin this section by reiterating some of the key features of the mechanist and organismic models. The mechanist model emphasizes the continuous applicability of a common set of laws or principles. Continuity exists because even quite complex behavior may be reduced to common, essential elements (e.g., stimulus–response connections in nurture-oriented, S–R, behavioristic theories, or genes in nature-oriented, behavioral genetics, evolutionary developmental psychology, or sociobiological theories). In these theories, elements are controlled by forces external to or placed into (through inheritance) the essentially passive, reactive organism. Thus, the task of developmental scientists, from these perspectives, is to identify the efficient antecedents (e.g., the environmental stimuli or genes) controlling consequent behaviors.

The organismic model emphasizes the integrated structural features of the organism. If the parts making up the whole become reorganized as a consequence of the organism's active construction of its own functioning, the structure of the organism may take on new meaning; thus qualitatively distinct principles may be involved in human functioning at different points in life. These distinct, or new, levels of organization are typically termed *stages* in this perspective (Reese & Overton, 1970). The task of developmental scientists within organicism is to assess the different functions of the organism that are associated with its changing structure.

From these general distinctions between the two models, there arise several other issues pertinent to understanding development. Reese and Overton (1970; Overton & Reese, 1973) and Lerner (1978, 1985) have identified several of these issues. These ideas serve to highlight the distinctions I have drawn already. In addition, their discussion will lead me to a presentation of a third model, contextualism.

Elementarism versus Holism

The mechanist model is an elementaristic one. Human functioning is reduced to its core, essential constituent elements (e.g., S–R connections, genes) and, in turn, the laws that govern the functioning of these elements are applicable continuously across life. As a consequence, there is no true qualitative discontinuity, no newness, no emergence, or no epigenesis within this perspective. Only quantitative differences may exist.

The organismic model is a holistic conception. As Reese and Overton (1970) explain,

> The assumption of holism derives from the active organism model. More particularly, it derives from the representation of the organism as an *organized* totality, a system of parts in interaction with each other, such that the part derives its meaning from the whole.
>
> (p. 136)

Reese and Overton note also that the idea of holism within organicism has been most clearly articulated by Werner and Kaplan (1963), who indicated that the idea:

> maintains that any local organ or activity is dependent upon the context, field or whole of which it is a constitutive part: its properties and functional significance [meaning] are, in larger measure, determined by the larger whole or context.
>
> (p. 3)

As I will note when I discuss the world hypothesis of contextualism (Pepper, 1942), a similar emphasis is placed on the role of the context in providing meaning for the parts of which it is constituted. Thus, as I have already noted (see Chapter 3), and as Pepper (1942) and Overton (1984) have explained, there is considerable similarity between the organismic and the contextual models. However, as I will argue below, the two models are distinct in significant ways—ways that lead to finding contextualism of use in surmounting some of the limitations that exist in the traditional (classic) organismic model. These limitations will be explained as I discuss further some of the issues that divide organicism and mechanism. To anticipate, however, these discussions will lead toward an integration of organicism and contextualism as a means to formulate a concept of development which adequately remedies problems found in exclusively organismic or mechanist (or contextual, for that matter) views (see too Raeff, 2016; Overton, 2015a).

Antecedent–Consequent versus Structure–Function Relations

The mechanist model stresses efficient (and material) causes and, as I have noted, is thereby concerned with identifying the necessary and sufficient antecedents of a behavior. Behavior is reduced, then, to an analysis of a qualitatively unchanging, continuous, and unbroken chain of cause–effect (e.g., S–R, gene–behavior) relations. In organicism, however, the emphasis is on determining the functions associated with the actively constructed structures of the organism. Qualitative changes in structures can occur as the active organism constructs—or better, reconstructs—its organization. Thus novelty, newness, qualitative discontinuity, or epigenesis occurs as a consequence of changing structure–function relations.

But if structure leads to function, what accounts for structure? One answer is simply function. That is, the active organism shapes its structure—which in turn influences the organism's function, and so on, in a continuous and bidirectional (reciprocal) manner (e.g., Gottlieb, 1976a, 1976b, 1983; Kuo, 1967; Overton, 2015a; Raeff, 2016; Tobach, 1981). That is, the activity of the holistic, integrated system is the source of the developmental course of the structure and the function of the system (Witherington, 2015; Witherington & Lickliter, 2016).

However, this answer is only one of several possible replies and is in fact quite controversial. Kohlberg (1968), Reese and Overton (1970), Overton (1973), and Gottlieb (1976a, 1976b) have noted that there exist several formulations about the source of an organism's structure. These formulations divide on the basis of their relative emphases on nature-based processes (e.g., nativist, preformed, innate variables) and nurture-based processes (e.g., conditioning, the physical ecology of one's context, the social events of one's context) in accounting for structure. As such, these formulations divide in respect to what I have suggested is perhaps the key issue of human development—the nature–nurture issue.

For instance, nurture-based, mechanist formulations about the character of psychological structure have tended to emphasize the role of environmentally-based processes (e.g., the laws of classical and operant conditioning) in building up a response repertoire (and/or mediation processes) within the organism (e.g., Bijou, 1976; Bijou & Baer, 1961). Thus, from this perspective, structure is imposed from outside the organism.

In turn, there exist several formulations associated with the organismic model. Several nature-based views stress the role of nativist variables and indicate that such variables exert a predetermined influence on an organism's structure—an influence independent of any role of nurture variables. Examples here are Chomsky (1965, 1966) and McNeill (1966), who maintain that psychological (linguistic) structures are completely present at birth, and Hamburger (1957), who was cited earlier as maintaining that the inherent structure of the nervous system directly determines various behavioral functions. Given, then, that the character of, and course of changes in, such structures is believed to be so thoroughly shaped by inborn variables, it may be apparent why Gottlieb (1970) has labeled such views predetermined-epigenetic.

Some formulations associated with the organismic view have emphasized that an *interaction* between nature and nurture variables provides the basis of structure. However, as I will discuss in this chapter and, as well, in subsequent ones (e.g., Chapter 10), the concept of interaction is itself a complex, controversial one. Indeed, one's concept of interaction—the components one sees as interrelating within the organismic whole—determines whether one remains committed to an exclusively organismic model or to a position that integrates organicism and contextualism (Lerner, 1985, 2015a; Lerner & Kauffman, 1985). The conclusion I shall reach is that, in integrative models of development, specifically ones derived from RDS metatheory, the term "interaction" should be used only in regard to statistical statements about connections among the variables in methods such as analyses of variance or regression analyses. In substantive discussions of the relations among variables within the developmental process, terms such as coaction, fusion, or simply relations, as in individual↔context relations, should be used.

However, prior to the elaboration of the RDS metatheory, scholars used the term interaction to describe substantive relations among variables. For instance, Piaget (1968, 1970) maintained that, although there existed an innate (congenital) structure, or organization, structures consequently develop through an interaction between the innate organization and the ongoing activity of the person (Reese & Overton, 1970). Note, however, that this concept of interaction sees the focal point, the locus, of interaction *within* the organism. The interaction is between the existing internal organization and the active organism's constructionist functions with that organization.

Whereas this organismic, internal version of interaction stands as the converse of the nurture, mechanist, more extrinsic notion of interaction (as a relation between past reinforcement history and present stimulus conditions), the Piagetian (1968, 1970) notion of interaction is distinct from those I will discuss as being associated with the contextual model. That is, to preview that discussion, a *strong* concept of organism–environment interaction (Lerner & Spanier, 1978b, 1980; Overton, 1973), transaction (Sameroff, 1975, 2009), or dynamic interaction (Lerner, 1978, 1979, 1985) is associated with a contextual perspective. This concept leads to the RDS-based idea of coaction and, as such, involves a rejection of a split between nature and nurture, or even between organism and environment; coaction emphasizes that a fused relationship exists among all components of the developmental system (Schneirla, 1956, 1957). As such, organism and context are always embedded each in the other (Lerner, Hultsch, & Dixon, 1983). The context is composed of multiple levels changing interdependently and, because organisms influence the context that influences them, they are efficacious in playing an active role in their own development (Lerner & Busch-Rossnagel, 1981b).

But, as also emphasized in the organismic-developmental view of Werner and Kaplan (1963; see too Raeff, 2016, and Chapter 10), because of the mutual embeddedness of organism and context, a given organismic attribute will have different implications for developmental outcomes in the milieu of different contextual conditions; this relation exists because the organismic attribute is only given its functional meaning by virtue of its relation to a specific context. If the context changes, as it may over time, the same organismic attribute will have a different import for development. In turn, the same contextual condition will lead to alternative developments in that different organisms coact with it. Thus, to draw quite a subtle distinction in somewhat strong terms, in the type of relations emphasized in contextualism, a given organismic attribute only has meaning for psychological development by virtue of its timing of coaction—that is, its relation to a specific set of time-bound contextual conditions. In turn, the import of any set of contextual conditions for psychosocial behavior and development can only be understood by specifying relations of the context to the specific, developmental features of the organisms within it. This central role of the timing of organism↔context relations in the determination of the nature and outcomes of development provides a time- (or timing-) dependent, probabilistic component of epigenesis (Gottlieb, 1970; Lickliter, 2016; Lickliter & Honeycutt, 2015; Scarr, 1982; Scarr & McCartney, 1983). As such, a distinctive feature of an approach to development that draws on contextual philosophical ideas is the treatment of the concepts of time and timing.

Although this probabilistic-epigenetic perspective gains its potential for providing an approach to developmental theory distinct from organicism (by drawing from issues associated with the contextual treatment of the concepts of time and timing), it can only do so by building on organicism. This relation is highlighted in the next developmental issue I discuss.

Behavioral versus Structural Change

What is it that develops, that changes, with development? Does this development have any necessary direction? As Reese and Overton (1970) explain, the answers to these questions provide human development with perhaps the most important distinctions between the mechanist and the organismic (and, I may note here too, the contextual) positions.

Within the mechanist model, qualitatively identical elements may be added to or subtracted from the machine. Such changes are just variational in character (Overton, 2015a). For instance, in the nurture, behavioristic translation of the model, lawfully identical S–R connections may be added to or subtracted from the response repertoire. Development is thus a matter of quantitative constancy or change (variation), with elements being added to or subtracted from the organism's repertoire in accordance with, for instance, the laws of conditioning (Bijou & Baer, 1961).

With decreases or increases possible in the number of S–R connections in the repertoire, development may be said to be multidirectional within this perspective. In short, in this exemplar of the mechanist model in human development, what changes in development is the number of S–R connections in the organism's repertoire, and there is no a priori necessary direction to such change.

Quite a different set of ideas exists within the organismic model. Reese and Overton (1970) note that this model emphasizes changes in structures and functions, and they stress that these changes are specified a priori to move toward a final goal or end state. That is, as noted earlier in this chapter, development is *teleological* within this view; it is goal-directed. Indeed, Reese and Overton (1970) indicate that, within the organismic model, the definition of development is "changes in the form, structure, or organization of a system, such changes being directed towards end states or goals" (p. 139). Such changes are transformational (Overton, 2015a).

Reese and Overton (1970) explain that development within this view is an a priori concept; that is, the general function of development—the end state or goal (e.g., "maturity," "ego integrity," "genital sexuality," or "formal operations")—is postulated in advance and acts as a principle for ordering change (see too Raeff, 2011, 2016). In short, in the organismic perspective, structure–function relations develop and these changes are, in a final sense, unidirectional—they move toward a final end state.

But, although development is thus seen to be an a priori, idealized ordering of structure–function relations, and development is therefore continuous in the sense of always being directed by the final end state, there may be—and typically are—qualitative changes in structure–function relations over the course of development. The possibility of such transformational structure–function changes of a qualitative character raises two other key developmental issues on which the models provide divergent perspectives.

Continuity versus Discontinuity

Continuity means constancy or a lack of change in some feature of development. For example, a given personality attribute (e.g., dependency) may be continuously present within a person across his or her life, or a child's growth rate (e.g., two inches a year) may remain constant across the childhood years. Discontinuity means change. Dependency may be altered or transformed into independence (e.g., by fostering secure attachment; Bowlby, 1969), and with puberty and the adolescent growth spurt, an individual's growth rate may increase dramatically.

Both the mechanist and the organismic models speak of continuity and discontinuity. In the nurture, mechanist model, the number of S–R connections (elements) in the response repertoire may be continuous; and in organicism, a given structure–function relation may be continuous for a specific period of the person's life. Thus, ideas of continuity may be derived from both models.

However, the models divide clearly when the issue of discontinuity is raised. As I have already noted, only quantitative discontinuity is possible within the translations of mechanism present in human development; only variational change is possible. However, within organicism, the active organism may construct—or better, revise—its structure and, in so doing, a new (transformed) structure–function relation will exist. Thus, qualitative discontinuity is possible within organicism. Such a change constitutes not just more of a previously or already existing structure; rather, it constitutes something new or novel, something that cannot be reduced to a prior state or status of the organism. As noted earlier, such changes are said to be transformational in character and emergent ones, and such qualitative discontinuity is termed epigenesis.

The possibility that life is characterized by qualitatively distinct phases of structure–function relations raises another key developmental issue: the issue of stages.

Stages of Development

As is the case with many of the other concepts I have been discussing, the concept of stage is a complex and controversial one (e.g., Brainerd, 1978; Flavell, 1980; Kessen, 1962; Lerner, 1980; McHale & Lerner, 1985; Overton & Reese, 1973; Reese & Overton, 1970; Wohlwill, 1973). Here, I need to note only that the models clearly divide on the basis of the way the term *stage* is used as a theoretical construct. In nurture, mechanist, behavioristic positions (e.g., Bijou, 1976), a stage summarizes the presence of some set, or some quantity, of S–R connections. However, there is nothing qualitatively different about organisms at one or another stage of life.

In organismic-based theories, however, a stage denotes a qualitatively distinct level of organization (e.g., Reese & Overton, 1970; Schneirla, 1957), that is, it is an organizational structure qualitatively discontinuous with those of prior or later periods. As Reese and Overton (1970) explain:

> Within the active organism model, change is in structure–function relationships or in organization. As organization changes to the extent that new system properties emerge (new structures and functions) and become operational, we speak of a new level of organization which exhibits a basic discontinuity with the previous level.
>
> (p. 143)

Sources of Development

The mechanist position, when translated into a developmental theory, will typically take the form of either a nature or nurture position. Resting on an additive and a mechanist assumption, the mechanist position tries to explain behavioral development in terms of a single set of source, or essential, determinants. As I have already noted above, because they are committed to a continuity position, mechanist thinkers would, by definition, be committed to the view that the same set of laws can always account for behavior. If continuity is asserted, it is then a difficult feat to draw one's explanations of behavioral development from different sources of development. Of course, it may be possible to argue that nature and nurture laws may be reduced to the same laws, and are thus not different sources after all; but this type of appeal really begs the question since, once again, a proponent of such an idea is back to arguing for one common set of laws.

As also already noted, mechanist-behavioristic theorists view the environment (nurture) as the source of the determinants of behavior. Human beings are seen as machines; they are energized to respond by stimulation that derives solely from the environment. Hence, humans are seen as essentially passive. They must await energizing stimulation that evokes behavior. Thus, human behavior is seen as at least initially amorphous, as having no (initial) shape or form. It is held that all human behavior is derived from a stimulus environment that exists independent of human beings. Hence, processes or variables not involved with such environmental stimulation really do not contribute at all to the shaping of behavior. Thus, heredity (nature) is really never systematically incorporated into these theorists' ideas. Heredity "just" delivers an organism ready to be shaped. The environment is considered the material and efficient source of the shaping of human behavior.

In turn, in mechanist, nature theories (e.g., Bjorklund, 2015, 2016; Bjorklund & Ellis, 2005; Freedman, 1979; Plomin, 1986; Plomin et al., 2016; Rowe, 1994) behavioral development is seen as deriving from a single source; but in this case the source would be nature. Behavioral development would thus be the continuous unfolding of preformed genetic givens. Prior to the behavior genetic theories of Plomin (1986) and Rowe (1994) or the sociobiological theories of Dawkins (1976), Freedman (1979), and Rushton (1987, 1999), William Sheldon's (1940, 1942) constitutional psychology position was a view consistent with a mechanist nature formulation, as was the work of some of the European animal behaviorists (ethologists such as Lorenz, 1965; see Chapter 11). Sheldon viewed body type as the essential determinant of personality or temperament. He maintained that body type—whether essentially fat, muscular, or thin—is primarily genetically determined; hence he viewed personality as derived essentially from a single source—genetic inheritance. Lorenz (1965) may also be seen as a mechanist-nature theorist. He believed that, in some animals, there exist behavior patterns called instincts, entities whose structures are inherited. Hence, such instincts are totally unavailable to any environmental influence. The validity of such ideas and, specifically, of those of Lorenz will be evaluated in Chapter 11.

The present point is that mechanist theorists typically emphasize either a nature or nurture viewpoint. Although some (if not most) mechanist-nurture theorists do explicitly admit, for example, that nature may provide an important contributory source of human functioning (for instance, see Bijou & Baer, 1961), this admission never seems to lead to any systematic consideration of the role of this other source in the development of behavior. Because changes in behavior are held to be continuous and additive instead of multiplicative, only one source of behavior (nature or nurture) is systematically taken into account. The combination of influences from nature and nurture occurs, however, as one of the predominant points of view within the organismic philosophy of science.

Although all organismic-epigenetic positions have the basic characteristics listed earlier, I have noted that the precise basis of the determinants of epigenesis is itself a controversial issue among organismic thinkers. What determines when and how the constituent parts comprising the whole organism combine to produce qualitative discontinuity? The basic issue involved in this question is the nature–nurture problem, and relates to the concept of interaction or, in the parlance of RDS-based ideas, coaction or fusion. The question becomes simply, "Does the source of epigenesis lie in nature, nurture, or a combination of the two?"

On the one hand, there are those scholars who maintain that epigenesis is predetermined through genetic inheritance (e.g., Erikson, 1959). Maturation, for instance, is held to play the key role in the order and timing of the qualitative emergences that define epigenesis (Erikson, 1959). In other words, development is seen as going through qualitative changes, and some proponents of epigenesis argue that these changes are completely determined by genes; the environment in which these genes exist is seen to play no role in producing the qualitative changes that characterize development (in this regard such proponents of epigenesis are indistinguishable from the mechanist-nature theorists such as Lorenz, 1965). Thus, the epigenetic changes envisioned by these scholars are predetermined by invariantly ordered maturational factors, such as growth and tissue differentiation, which are held simply to unfold in a fixed sequence—a sequence that arises independent of any experiential contribution. As noted before, this predetermined-epigenetic viewpoint is well illustrated by the early views of Hamburger (1957). Thus, according to Gottlieb (1970), this version of epigenesis, as it is expressed in the early views of Hamburger (1957) and others, means that:

> the development of behavior in larvae, embryos, fetuses, and neonates can be explained entirely in terms of neuromotor and neurosensory maturation (i.e., in terms of proliferation, migration, differentiation, and growth of neurons and their axonal and dendritic processes). In this view, factors such as the use or exercise of muscles, sensory stimulation, mechanical agitation, environmental heat, gravity, and so on, play only a passive role in the development of the nervous system. Thus, according to predetermined epigenesis, the nervous system matures in an encapsulated fashion so that a sufficiently comprehensive account of the maturation of the nervous system will suffice for an explanation of embryonic and neonatal behavior, the key idea being that structural maturation determines function, and not vice versa.
>
> (Gottlieb, 1983, p. 11)

This nature-epigenetic viewpoint has rather severe conceptual limitations (akin to those involved in the type of view represented by Lorenz, 1965). In my view, the alternative conception of the source of epigenesis—*probabilistic epigenesis*—appropriately deals with the conceptual issues inherent in a consideration of human development (see too Lickliter, 2016; Lickliter & Honeycutt, 2015; Witherington & Lickliter, 2017). Moreover, this view represents, in opposition to both the mechanist and the predetermined-epigenetic views, the notion that developmental changes are determined by a multiplicative interaction—or, in the terms used by proponents of RDS-based ideas, a fusion, a complete, systemic integration—of two sources of development, nature and nurture. Because the probabilistic-epigenetic position views development as qualitatively discontinuous and further views this discontinuity as arising from such a coaction or fusion, it is understandable that two different sources of development (hereditary and environmental sources) can be seen to provide the basis of the relation that defines and brings about the qualitative discontinuity.

T. C. Schneirla (1957), the eminent comparative psychologist, argued that no behavior is predetermined or preformed (see Chapter 7). The role of the environment must always be taken into account in trying to understand the qualitative changes that characterize epigenesis. Specifically, one must consider the experience of various stimulative events acting on the organism throughout the course of its life span (Schneirla, 1957). These stimulative events may occur in the environment outside the organism (exogenous stimulation) or in the environment within the organism's own body (endogenous stimulation). No matter where they occur, however, the influence of patterns of environmental stimulation upon the contribution that genes make toward behavior must always be considered.

Genes must exist in an environment. They do not just float in nothingness. Changes in the environment may help or hinder the contribution of the genes (as discussed in Chapter 2, this relation involves the process of epigenetics; Moore, 2015a, 2016). In other words, the experiences that take place in the environment will play a role in what contribution genes can make. If X-rays invade the environment of the genes, or if oxygen is lacking, or if poisonous chemicals enter this environment, then the role of the genes in contributing to behavior will certainly be different from their role if such environmental stimulative events did not occur. Different patterns of epigenetic changes will likely result (Cole, 2014; Slavich & Cole, 2013). In addition to toxic or noxious influences, the internal and external environments provide essential signals for gene expression during the course of normal development (Gottlieb, 1991, 1992, 1997; Slavich & Cole, 2013).

To illustrate the connection between genes and external environment (i.e., the environment outside the organism), it is useful to consider the results of experiments testing how exposure to enriched, as opposed to impoverished, environments alters DNA. Uphouse and Bonner (1975) assessed the transcription of RNA from DNA in the brains or livers of rats exposed to (1) high environmental enrichment (i.e., living in a cage with 11 other rats and having "toys" and mazes available for exploration); (2) low environmental enrichment (i.e., living in a cage with one other rat but no exploration materials); or (3) isolation (i.e., living in a cage alone and with no exploration materials). The RNA from the brains of the environmentally enriched rats showed a level of transcription of DNA significantly greater than that of the other groups. No significant differences were found with liver RNA.

Grouse et al. (1978) also found significant differences between the brain RNA of rats reared in an environmentally rich versus environmentally impoverished context. In addition, Grouse, Schrier, and Nelson (1979) found that the total complexity of brain RNA was greater for normally sighted kittens than for kittens that had both eyelids sutured at birth. However, the RNAs from the nonvisual cortices and from subcortical structures were not different for the two groups. Grouse et al. (1978) concluded that the normal development of the visual cortex, which is dependent on visual experience, involves a greater amount of genetic expression than occurs in the absence of visual experience. Given such findings about the contextual modifiability of genetic material, it is possible to assert that genes are appropriate targets of environmental influence (Blaze & Roth, 2015, 2017; Doherty & Roth, 2016; Meaney, 2010; Moore, 2015a, 2016; Roth, Lubin, Funk, & Sweatt, 2009; Slavich & Cole, 2013).

Moreover, developmental scientists cannot say with total certainty what type of environmental stimulative influences will always occur or whether the environment will coact with genes to help or hinder development. Rather, a scientist may say only that specific types of environmental influences will *probably* occur (as they do with the average organism of a specific species) and/or that a given emergence will *probably* take place if the gene–experience relation proceeds as it usually does.

Thus, in order for development to proceed normatively (that is, in the sequence typical for the species), environmental stimulative events must operate on the maturing organism at specific times in the organism's development. That is, since epigenesis is determined by the coaction of hereditary (genetic and epigenetic) and experiential sources, experience must coact with hereditary-linked processes (e.g., maturation) at specific times in the organism's development in order for specific emergences to occur. If the emergence of a specific behavioral development is influenced by a specific maturation–experience relation, and if for a specific species this relation usually occurs at a specific time in the life span (e.g., at about 6 months of age), then if the specific experience involved in this coaction occurs either earlier or later for a given member of the species, there will be a change in the emergent behavioral capacity (see the specificity principle discussed in Chapter 2; Bornstein, 2017). Thus, the species-typical timing of maturational–experiential coactions is essential in order for the emergences that characterize development to occur normatively.

However, the timing of these coactions is not invariant (Rose, 2016). One can never expect with complete certainty that these relations will occur at their typical times for all members of a species. As suggested above, some individuals in a given species may experience these coactions earlier than others, whereas others may undergo them at a later-than-average time. For instance, in humans, adolescents differ in the timing of their pubertal maturation, with some youth attaining a particular point in their maturation (e.g., menarche) earlier than their age-mates, and other youth reaching this point later than average (Brooks-Gunn & Petersen, 1983). These differences may or may not lead to significantly different, or substantially altered, characteristics in the resulting behavioral capacity. For instance, early-maturing boys enjoy greater peer popularity than do late-maturing boys, whereas the reverse set of associations occurs during early and mid-adolescence for early- and late-maturing girls (Brooks-Gunn & Petersen, 1983; Petersen, 1987, 1988).

The point is that, although emergent behavioral developments find their source in the coaction of maturation and experience, a developmental scientist cannot expect the timing or functional significance of these relations always to be the same for all individuals in a species. Thus, as illustrated by early- and late-maturing adolescents, alterations in the timing of these relations, if extreme enough, could lead to changes in the behavioral characteristics that develop as a consequence of the relations. Thus, one can only say that specific emergences will *probably* occur, given fairly typical timing of pubertal maturation (Rose, 2016). Hence, the probabilistic-epigenetic position recognizes that:

1. Both experience and maturation are invariably involved in determining the qualitative changes that characterize development;
2. The timing of the coactions between maturation and experience is a factor of critical importance in the determination of behavioral development; and
3. Since these relations cannot be expected to occur at exactly the same time for every organism within a given species, one can say, within a given level (or interval) of confidence, only that specific emergences will probably occur. Individuality, and not homogeneity across all members of a group, is the case in human development (Rose, 2016).

The probabilistic formulation of epigenesis should appear more complicated than its predeterminist counterpart, because it is! Development is an exceedingly complex phenomenon and any accurate conceptualization of it would have to take this complexity into account (Lerner, 2016). Thus, Schneirla (1957), recognizing both the complexity of behavioral development and the failure of predetermined-developmental notions to acknowledge that complexity, illustrates the probabilistic-epigenetic viewpoint by stating:

> The critical problem of behavioral development should be stated as follows: (1) to study the organization of behavior in terms of its properties at each stage from the time of egg formation and fertilization through individual life history; and (2) to work out the changing relationships of the organic mechanisms underlying behavior; (3) always in terms of the contributions of earlier stages in the developmental sequence; (4) and in consideration of the properties of the prevailing developmental context at each stage.
>
> (1957, p. 80)

As I have indicated, I believe that this probabilistic-epigenetic viewpoint offers the most appropriate conceptualization of development. As already noted, this perspective reflects the RDS-based ideas of human development (e.g., Ford & Lerner, 1992; Gottlieb, 1991, 1992, 1997; Lickliter, 2016; Lickliter & Honeycutt, 2015; Raeff, 2016; Overton, 2015a; Witherington, 2015; Witherington & Lickliter, 2017).

Conclusions

The mechanist model stresses a passive organism in an active world; it emphasizes reductionism, continuity of laws governing development, only quantitative behavioral change across life, potential multidirectionality of change, essentialism, and causality through antecedent–consequent relations; it also eschews the idea of stages as qualitatively distinct periods of life. The organismic model stresses an active organism in a relatively passive world, and it emphasizes emergence; qualitative change in structure–function relations across life; unidirectional, teleological, goal-directed change; holism; and the appropriateness of the idea of stages as qualitatively distinct levels of organization.

Each of these two models has led to a set of theories—sets that we have noted that Reese and Overton (1970) term a "family of theories." These families are of use in the study of all or part of the life span. For instance, the behavioristic, functional-analysis approach of Bijou and Baer (1961; Bijou, 1976) exemplifies the translation of a nurture, mechanist model into a theory of development. However, other family members include the social-learning theories of Miller and Dollard (1941), Davis (1944), and McCandless (1970). The theories of Werner (1948), Piaget (1950, 1968, 1970), Freud (1954), and Erikson (1959, 1963, 1968) exemplify the translation of versions of the organismic model into developmental theories.

Both mechanistic- and organismic-based orientations encounter problems when attempting to formulate a useful concept of development. Mechanistically derived conceptions cannot deal directly with novelty or with qualitatively distinct levels of being. In the former case, novelty must be interpreted as reducible to common constituent elements; in the latter case, the influence of cultural, sociological, and physical ecological variables, for instance, must also be reduced to common (e.g., behaviorist) principles in order for their influence to have a place in (that is, an efficient causal influence on) the continuity perspective of mechanist models. Often, such reduction is quite forced and/or artificial and, as such, variables from distinct levels of analysis may end up being ignored.

Moreover, despite the possibility of multidirectionality in development, in practice, mechanistically derived conceptions often adopt a position involving the continuous applicability of early experience to later life (e.g., Thorndike, 1905; Watson, 1928). In fact, in nature, mechanist theories that stress the role of genes as the material and efficient causes of behavioral development (e.g., Plomin, 1986; Plomin et al., 2016; Rowe, 1994; Rushton, 1987, 1999), *the very first experience—conception*—is the key one in human life. It is at conception that the genotype is received, and it is held that this inheritance is the major source of structure and function across the life span (e.g., see Bjorklund, 2015, 2016; Bjorklund & Ellis, 2015). Moreover, I have noted that Zukav (1979) argues that the view of antecedent–consequent relations held by mechanists logically requires such a proscription against discontinuity or change in later life. Finally, there is the insistence in mechanist ideas on a passive model of the organism. Such a conception, especially when translated into a theory of human development, is unable to account for the evidence that organisms have characteristics that as much shape their world as their world shapes them (Bell, 1968; Bell & Harper, 1977; Lerner, 1982; Lerner & Busch-Rossnagel, 1981a, 1981b; Lewis & Rosenblum, 1974) *and* that these organismic characteristics cannot be adequately interpreted as merely derivative of the organism's conditioning history or experience-independent genetic inheritance (Gottlieb, 1992, 1997, 2004; Gottlieb et al., 2006; Schneirla, 1957; Tobach, 1981; Tobach & Schneirla, 1968).

For these reasons, I am oriented more to formulating an organismic-derived concept of development than a mechanist-derived one. However, as I have also noted, there are major conceptual problems with organicism that diminish its usefulness for the derivation of a concept of development. Among these is the need in organicism to "deal mainly with historic processes even while it consistently explains time away" (Pepper, 1942, p. 280). That is, as I will explain in regard to my discussion of contextualism, "organicism takes time lightly or disparagingly" (Pepper, 1942, p. 281). In addition, another problematic feature of organicism is its teleological features. Here, for the "fragments" of an organic whole, there is "inevitability of connections among fragments . . . [an] implication of wholeness contained in them" (Pepper, 1942, p. 292), "an internal drive toward the integrations which complete them" (Pepper, 1942, p. 291); although the particular path to a goal is not predetermined it is nevertheless the case that "the goal was predetermined in the structure of the facts" (Pepper, 1942, p. 295).

These key features of "pure" organicism fail to deal with the point that the timing of coaction of causal developmental variables is probabilistic (Gollin, 1981; Gottlieb, 1970, 1976a, 1976b, 1991, 1992, 1997; Scarr, 1982; Scarr & McCartney, 1983; Schneirla, 1956, 1957; Tobach, 1981; Tobach & Schneirla, 1968). As a consequence, there is a lack of concern with the implication that such differences in time may mean that, whereas the process of development may remain invariant across history (e.g., an orthogenetic progression in structure–function relations may exist; Raeff, 2016), the ongoing features of developmental trajectories may show considerable interindividual variability (e.g., Molenaar & Nesselroade, 2015; Rose, 2016), *and* there may be no universally inevitable end state for a developmental progression. In other words, there may be a probabilistic, rather than a predetermined, pattern to developmental change.

Moreover, as with mechanistically derived conceptions, the use of "pure" organismic conceptions of development is diminished in light of several sets of findings for which extant organismic views cannot devise adequate interpretations. That is, as compared to mechanist conceptions, which encounter difficulty as a consequence of failures to treat adequately organismic features of the person, organismic conceptions have encountered difficulty as a consequence of not being able to test effects on the person associated with variables derived (ultimately) from the context enveloping the person (e.g., Baltes et al., 1999, 2006; Elder & Shanahan, 2006; Elder et al., 2015; Shweder et al., 1998).

To illustrate, attempts to use a biological model of growth, one based on an organismic conception of development (e.g., Cumming & Henry, 1961) to account for data sets pertinent to the adult and aged years, have not been completely successful (Baltes, Reese, & Lipsitt, 1980; Baltes & Schaie, 1973). Viewed from the perspective of this organismic conception, the adult and aged years were necessarily seen as periods of decline (Cumming & Henry, 1961). However, all data sets pertinent to age changes, for example, in regard to intellectual performance, during these periods were not consistent with such a unidirectional format of change (e.g., Schaie & Strother, 1968). For example, increasingly greater between-people differences in within-person change were evident in such data sets (Baltes, 1983; Baltes & Schaie, 1974, 1976; Schaie, Labouvie, & Buech, 1973). Simply put, as people developed into the adult and aged years, differences between them increased.

On the basis of such data, Brim and Kagan (1980b, p. 13) concluded that "growth is more individualistic than was thought, and it is difficult to find general patterns" (see too Rose, 2016). Factors associated with the historical time within which people were born (i.e., with membership in particular birth cohorts) and/or with events occurring at particular historical times appeared to account for more of these changes, particularly with respect to adult intellectual development, than did age-associated influences (Baltes et al., 1980). Data sets pertinent to the child (Baltes, Baltes, & Reinert, 1970) and the adolescent (Elder, 1974, 1980, 1988, 1999; Nesselroade & Baltes, 1974) that considered these birth-cohort and time-of-measurement effects also supported their saliency in developmental change. These findings led scientists to induce conceptualizations useful for understanding the role of these non-age-related variables in development (e.g., Baltes, Cornelius, & Nesselroade, 1978; Baltes et al., 1999, 2006; Brim & Ryff, 1980), and these conceptualizations may be interpreted as being consistent with an RDS-based view of development (e.g., Baltes, 1979b; Lerner, 1982, 2006b; Lerner, Hultsch, & Dixon, 1983; see too Chapter 10). Brim and Kagan (1980) have summarized this RDS-based view by noting that this:

> conception of human development . . . differs from most Western contemporary thought on the subject. The view that emerges . . . is that humans have a capacity for change across the entire life span. It questions the traditional idea that the experiences of the early years, which have a demonstrated contemporaneous effect, necessarily constrain the characteristics of adolescence and adulthood . . . there are important growth changes across the life span from birth to death, many individuals retain a great capacity for change, and the consequences of the events of early childhood are continually transformed by later experiences, making the course of human development more open than many have believed.
>
> (p. 1)

Given the interest in and importance attached to ideas linked to contextualism in the scholarship summarized by Brim and Kagan (1980), it is appropriate to evaluate the usefulness of this model for the derivation of an adequate concept of development. Whereas I will argue that contextualism does have many attractive conceptual features, I will also point out that—as is the case with mechanism and organicism—it has important problems. Indeed, the problems of contextualism are of sufficient scope to obviate the use of “pure” contextualism in deriving an adequate concept of development.

However, contextual views may be combined with organismic ones. I will argue that such a synthesis provides a useful basis for deriving a concept of development, one that eliminates many of the problems found in the two models taken separately. The synthesis I will discuss involves ideas reflecting the process-relational paradigm described by Overton (2015a) and, as such, the RDS metatheory.

THE CONTEXTUAL MODEL

According to Pepper (1942), the main metaphor of contextualism is neither the machine nor the whole organism. It is the historic event. “The real historic event, the event in its actuality, is when it’s going on now, the dynamic dramatic active event” (Pepper, 1942, p. 232). In contextualism, every behavior and incident in the world is a historic event, and thus change and novelty are accepted as fundamental. A contextual model assumes (1) *constant change* of all levels of analysis; and (2) *embeddedness* of each level with all others—that changes in one promote changes in all. The assumption of constant change denotes that there is no complete uniformity or constancy. Rather than change being a phenomenon to be explained, a perturbation in a stable system, change is a given (Overton, 1978); thus the task of the developmental scientist is to describe, explain, and optimize the parameters and trajectories of *processes* (i.e., variables that reflect the *relations* among the levels of the system and that show time-related changes in their quantity and/or quality).

The second assumption of contextualism is thus raised. It stresses the interrelation of all levels of analysis. Because phenomena are not seen as static but rather as change processes, and because any change process occurs within a similarly (i.e., constantly) changing world (of processes), any target change must be conceptualized in the context of the other changes within which it is embedded. Thus, change will constantly continue as a consequence of this embeddedness.

There is an organism in the contextual perspective, but it is conceived of as an “organism in relation” (Looft, 1973), or an “organism in transaction” with its context (Dewey & Bentley, 1948; Lerner, 1991, 1996; Pervin, 1968; Sameroff, 2009). These relations are the focus of developmental analysis. They (relations) constitute the *basic process* of human development. As such, the timing of the relation between organism and context is critical in contextualism. Indeed, as implied earlier, the fact that the timing of a relation plays a central role in contextualism serves to provide a key distinction between it and organicism. As Pepper (1942) explains:

> Organicism takes time lightly or disparagingly; contextualism takes it seriously . . . The root metaphor of organicism always does appear as a process, but it is the integration appearing in the process that the organicist works from and not the duration of the process. When the root metaphor reaches its ultimate refinement the organicist believes the temporal factor disappears.
>
> (p. 281)

However, major problems arise with the use of "pure" contextualism as a world hypothesis from which to derive a concept of development. Contextualism is at its core a dispersive paradigm (Overton, 1984). That is, there is no necessary connection or relation among the parts of the whole, either within or across time. Relations at one point in time (e.g., among thoughts, feelings, personality, and behavior) may or may not exist at another point in time. In pure contextualism there is simply no prediction possible from one point in life (or history) to the next. In other words, a purely contextual approach sees the components of life as completely dispersive (Pepper, 1942)—as lacking any necessary across-time organization, systemic connection, or successive patterning. Indeed, Pepper (1942) believes that it is the dispersive character of contextualism that is the key idea making it a world view distinct from the organismic one, a world view marked by integration (Pepper, 1942). However, as I have argued in prior chapters, if the term development is to have meaning beyond that of mere change it must imply, at the very least, systematic and successive changes in the organization of an organism or, more generally, a system. Thus, a world view that stressed only the dispersive, chaotic, and disorganized character of life would not readily lend itself to the derivation of a theory of development.

But, whereas contextualism may not suffice in and of itself as a model from which an adequate concept of development may be derived, it may be possible to combine features of this model with organicism—with which, Pepper (1942) notes, it is closely aligned—to forge such a concept. As I suggested earlier, this "marriage" is possible by reference to the ideas associated with the probabilistic-epigenetic view of organicism. Probabilistic epigenesis, therefore, is not actually an instance of organismic thinking. As explained by Gottlieb (1992, 1997; Gottlieb et al., 1998, 2006; see too Lickliter, 2016; Lickliter & Honeycutt, 2015), probabilistic epigenesis involves ideas linked to RDS metatheory.

Contextualism and Probabilistic Epigenesis

Building on the work of Schneirla (e.g., 1956, 1957) and Tobach and Schneirla (1968), Gilbert Gottlieb (1970) introduced the term probabilistic epigenesis and, over the next 30-plus years, developed the conceptual and empirical bases of this conception (e.g., Gottlieb, 1976a, 1976b, 1992, 1998, 1997, 2004; Gottlieb et al., 1998, 2006). Emphasizing the greater plasticity of development that is rationalized in this version of epigenesis than in the predetermined-epigenetic conception, Gottlieb (1970) noted that probabilistic epigenesis involves:

> The view that the behavioral development of individuals within a species does not follow an invariant or inevitable course, and, more specifically, that the sequence or outcome of individual behavioral development is probable (with respect to norms) rather than certain.
>
> (Gottlieb, 1970, p. 123)

Moreover, reflecting the key conception of developmental process involved in RDS-based theoretical models, that is of a ↔ linking components of the developmental system, Gottlieb explains that this probable, and not certain, character of individual development arises because:

> Probabilistic epigenesis necessitates a bidirectional structure–function hypothesis. The conventional version of the structure–function hypothesis is unidirectional in the sense that structure is supposed to determine function in an essentially nonreciprocal relationship. The unidirectionality of the structure–function relationship is one of the main assumptions of predetermined epigenesis. The bidirectional version of the structure–function relationship is a logical consequence of the view that the course and outcome of behavioral epigenesis is probabilistic: it entails the assumption of reciprocal effects in the relationship between structure and function whereby function (exposure to stimulation and/or movement of musculoskeletal activity) can significantly modify the development

> of the peripheral and central structures that are involved in these events.
>
> (Gottlieb, 1970, p. 123)

In essence, then, as compared to predetermined epigenesis, where the key assumption

> holds that there is a unidirectional relationship between structure and function whereby structural maturation determines function (structural maturation → function) but not the reverse, probabilistic epigenesis assumes a bidirectional or reciprocal relationship between structural maturation and function whereby structural maturation determines function and function alters structural maturation (structural maturation↔function).
>
> (Gottlieb, 1983, p. 12)

Most important for the formulation of a useful concept of development, the changes depicted in this probabilistic-epigenetic formulation of development are not completely dispersive. As does Overton (1984), I believe that, when features of organicism, for instance, its regulative ideas about integrative change across ontogeny (e.g., as illustrated through the concept of orthogenesis; Raeff, 2016; Siegel, Bisanz, & Bisanz, 1983; Wapner & Demick, 1998; Werner, 1957), are synthesized with the probabilistic nature of contextual change, a useful *developmental* conception is created, one that reflects the ideas described in Chapter 3 as linked to RDS metatheory. It is useful to discuss how ideas associated with the concept of probabilistic epigenesis are examples of RDS metatheory translated into theoretical models of development. This discussion of this instance of such translation will also pertain to other instances of such translations, for example, Overton's (1984) *contextual organicism* and the *developmental contextual* model of Lerner (e.g., 1978, 2004).

The Concept of Development in Probabilistic Epigenesis

Probabilistic epigenesis (e.g., Gottlieb, 1970, 1997) is an instance of a theoretical model that was informed by scholarship about mechanist and organismic metatheories but went beyond them. Drawing on ideas pertinent to systems thinking (e.g., von Bertalanffy, 1933, 1968), Gottlieb (1997) and others (e.g., Greenberg & Tobach, 1984; Kuo, 1976; Schneirla, 1957; Tobach & Schneirla, 1968) formulated a concept of development that envisioned the key process of development as involving individual↔context relations. As such, this concept of development may be seen as one linked to RDS metatheory as compared to other metatheories. To understand both this concept of development and its link to RDS metatheory, it is useful to discuss first the probabilistic feature of probabilistic epigenesis.

Gollin (1981) explains that probabilistic developmental change is not dispersive because the living system—the organism—has organization and internal coherence, and these features constrain the potentials of the *developmental context* to affect the system. He says:

> The determination of the successive qualities of living systems, given the web of relationships involved, is probabilistic. This is so because the number of factors operating conjointly in living systems is very great. Additionally, each factor and subsystem is capable of a greater or lesser degree of variability. Hence, the influence subsystems have upon each other, and upon the system as a whole, varies as a function of the varying states of the several concurrently operating subsystems. Thus, the very nature of living systems, both individual and collective, and of environments, assure the presumptive character of organic change. Living systems are organized systems with internal coherence. The properties of the parts are essentially dependent on relations between the parts and the whole (Waddington, 1957). The quality of the organization provides opportunities for change as well as constraints upon the extent and direction of change. Thus, while the determination of change is probabilistic, it is not chaotic.
>
> (Gollin, 1981, p. 232)

Gollin's position illustrates that one needs to understand that development occurs in a multilevel context, and that the nature of the changes in this context leads to the probabilistic character of development. However, one also needs to appreciate that the organism shapes the context as much as the context shapes the organism (Lerner & Busch-Rossnagel, 1981b; Lerner & Walls, 1999). Tobach (1981) made similar points. She indicated that:

> Three processes (contradictions) intercept in time to bring about qualitative changes in the individual (development, which includes growth and maturation): (a) the inner contradiction of the organism; (b) the inner contradiction of the environment; and (c) the outer contradiction between the organism and the environment. Some of the inner contradictions would be the metabolic cycle, and neurohormonal cycles; these have characteristics of negative and positive feedback that bring about continuous change with more or less stability in the organism. The environment expresses its own contradictions in diurnal and seasonal variations, faunal and floral interrelations, and so on. Given different lighting conditions (environmental contradictions), the effects on the hormonal function (intraorganismic contradictions) bring about changes in the organism's activity that bring it into changing relationships with the abiota or biota, and particularly with conspecifics (contradiction between organism and environment). The intersect of these three processes (contradictions) brings about developmental change in the organism. The organism may act on the environment (the social aspect), resulting in copulation, bringing about a new developmental stage.
>
> (Tobach, 1981, pp. 60–61)

Moreover, expanding on Tobach's (1981) discussion of the individual↔context relations comprising the developmental process, Gollin (1981) underscores a point I made earlier in this chapter, about why the term "interaction" is not appropriate for describing the relations or coactions between the individual and the context that constitute the basic relational "unit of analyses" in RDS-based models. He noted that:

Ethel Tobach

> The relationships between organisms and environments are not interactionist, as interaction implies that organism and environment are separate entities that come together at an interface. Organism and environment constitute a single life process . . . For analytic convenience, we may treat various aspects of a living system and various external environmental and biological features as independently definable properties. Analytical excursions are an essential aspect of scientific inquiry, but they are hazardous if they are primarily reductive. An account of the *collective behavior* of the parts as an organized entity is a necessary complement to a reductive analytic program, and serves to restore the information content lost in the course of the reductive excursion . . . In any event, the relationships that contain the sources of change are those between organized systems and environments, not between heredity and environment.
>
> (Gollin, 1981, pp. 231–232)

In a related vein, Tobach (1981) noted:

> Gene function is expressed in enzymes and proteins that are fundamental and ubiquitous

> to all aspects of molecular function and derivatively in physiological integration. However, the preeminence of societal factors in human development in determining the significance of these biochemical processes is also never lost. If the child is discovered to have an enzyme deficiency that is corrected through dietary supplementation, the outcome will depend on whether the child is in a society in which such knowledge is not available, or if the knowledge is available, whether the treatment is available to the individual child. Extremes in chromosomal structures and function such as trisomy-21, despite their demonstrated molecular base, are also variably vulnerable to societal processes.
>
> (p. 50)

A final point about the probabilistic-epigenetic view needs to be highlighted. Although both contextual and mechanist-behavioral perspectives make use of the context enveloping an organism in attempts to explain development, it is clear that they do so in *distinctly* different ways. Contextually-oriented theorists do not adopt a reflexively reductionist approach to conceptualizing the impact of the context. Instead, because of a focus on organism↔context transactions, and thus a commitment to using an interlevel, or relational, unit of analysis (Lerner, Skinner, & Sorell, 1980), the context may be conceptualized as composed of multiple, qualitatively different levels, for example, the inner-biological, the individual-psychological, the outer-physical, and the sociocultural (Riegel, 1975, 1976a, 1976b).

Moreover, although both the mechanist and the probabilistic-epigenetic perspectives hold that changes in the context become part of the organism's intraindividually changing constitution, the concept of "organism" found in the two perspectives is also quite distinct. In probabilistic epigenesis the organism is not merely the host of the elements of a simplistic environment (Baer, 1976). Instead, the organism is itself a qualitatively distinct level within the multiple dynamically coacting levels forming the context of life. As such, the organism has a distinct influence on that multilevel context that is influencing the organism. As a consequence, the organism is, in short, not a host of S–R connections but an active contributor to its own development (Lerner, 1982; Lerner & Busch-Rossnagel, 1981b; Lerner & Walls, 1999; see too Brandtstädter, 1998, 1999, 2006).

How may such organism↔context coactions occur? In other words, how may an organism make an active contribution to its own development? One answer to this question is found in the "goodness of fit" model of person–context relations (e.g., Chess & Thomas, 1999; Lerner & Lerner, 1983, 1989; Lerner et al., 1995). Just as a person brings his or her characteristics of physical, emotional, and behavioral individuality to a particular social setting, there are demands placed on the person by virtue of the social and physical components of the setting. These demands may take the form of (1) attitudes, values, or stereotypes held by others regarding the person's attributes; (2) the attributes (usually behavioral) of others with whom the person must coordinate, or fit, for adaptive relationships to exist; or (3) the physical characteristics of a setting (e.g., the presence or absence of access ramps for the handicapped) that require the person to possess certain attributes (again, usually behavioral) for the occurrence of efficient coaction.

The person's characteristics of individuality, in differentially meeting these demands, provide a basis for the feedback he or she gets from the socializing environment. For example, considering the second type of contextual demands that exist—those that arise as a consequence of the behavioral characteristics of others in the setting—problems of fit might occur when a child who is highly irregular in his or her biological function (e.g., eating, sleep–wake cycles, toileting behaviors) develops in a family setting composed of highly regular and behaviorally scheduled parents and siblings.

Lerner and Lerner (1983, 1989) and Thomas and Chess (1977; Chess & Thomas, 1984, 1999) believed that adaptive psychological and social functioning do not derive directly from either the nature of a person's characteristics of individuality per se or the nature of the demands of the contexts within which the person functions. Rather, if a person's characteristics of individuality match (or "fit") the demands of a particular setting, adaptive outcomes in that setting will accrue. Those people whose characteristics match most of the settings within which they exist should receive supportive or positive feedback from the contexts and should show evidence of the most adaptive behavioral development. In turn, of course, mismatched people, whose characteristics are incongruent with one or most settings, should show alternative developmental outcomes.

In sum, the present point is that, to developmental scientists using the concept of probabilistic epigenesis, behavioral development becomes, at least in part, a matter of self-activated generation. By focusing on the contributions that the organism's own characteristics (e.g., its type of behavioral style, its physical appearance) make toward its own further development (e.g., see Schneirla, 1957), developmental scientists using the idea of probabilistic epigenesis are essentially studying the continual accumulations of the fusion of nature and nurture.

Probabilistic Epigenesis as a "Compromise" Conception

Probabilistic epigenesis may be understood as constituting an integration of, or a "compromise" position derived from, different philosophical models or world views. Overton (1984) suggested that organicism may be integrated with either contextualism or mechanism in order to formulate a synthetic position which capitalizes on the useful features of the mechanist and the organismic positions. However, I have argued that mechanism in and of itself is not useful for forging a true developmental theory. Nevertheless, following Overton (1984), I suggest that within probabilistic epigenesis there are at least two ways of synthesizing some of the potentially useful features of mechanism and, of course, organicism.

The Levels-of-Organization Hypothesis

The first of these means of synthesis has been implied in much of what I have discussed above (e.g., regarding the ideas of Anderson, 1972). It is termed the *levels-of-organization hypothesis* and is illustrated by the work of Schneirla (1957; see too Chapter 7). The compromise notes that there are different levels of organic and/or phenomenal organization and that the laws of the lower levels (e.g., physics and chemistry) are implied in the laws of the higher (e.g., the psychological) level. Yet, the laws of the higher level cannot be reduced to or predicted from the laws of the lower level. This assertion is appropriate because such reduction will not lead to an understanding of the emergent quality of the higher level (Anderson, 1972). Clearly, this assertion has been presented as a basic part of the probabilistic-epigenetic viewpoint. The water example provided earlier in this chapter is an illustration of this compromise. Another illustration is that, although specific neural, hormonal, and muscular processes certainly underlie (are implied in) a person's being in love, reduction of love to these lower levels—or to the still-lower levels of chemistry and physics—is unlikely to result in an (ecologically valid) understanding of this phenomenon (e.g., see Brent, 1978; Witherington, 2014, 2015).

An example of the application of the levels-of-organization compromise may be seen by reference to some classic findings in the literature on children's problem-solving behavior. Kendler and Kendler (1962) devised a way to study problem-solving behavior in various species of organisms (e.g., rats and humans), as well as in humans of various ages (e.g., nursery-school children and college students). For instance, children are presented first with two large squares and two small squares. One of each type of square is black and one of each type is white. Thus, there is a large black and a large white square, and a small white and a small black square. The children's task is to learn to respond either to the color dimension (thus ignoring the size) or to the size dimension (thus ignoring the color). For example, a child may be presented with a large black and a small white square on one trial and then perhaps a large white square and a small black square on another. If size is the aspect of the stimuli that should be responded to and, further, if a response toward the bigger of the two squares will always lead to a reward, the child should choose the large stimulus in each trial, no matter what the color. In other words, the child first learns that size is the relevant aspect of the stimuli; therefore, the child learns to respond to the difference in size and to ignore (not respond to) differences in color of the squares.

Rats, nursery-school children, and college students can all learn this first problem-solving task. The interesting thing about this type of problem solving is what happens when the rules about the relevant aspect of the stimuli are changed. In the first problem-solving task, size was the relevant dimension (the big squares were rewarded and the small squares were not). Without directly cueing the child that this rule has changed, it is possible still to keep the size of the stimuli as the relevant dimension (and the color as the non-relevant), but to make choice of the *small* squares the response that will be rewarded. Thus, the same dimension of the stimuli (size) is still relevant, but there has been a reversal as to which *aspect* of size (from large to small) will lead to a reward. Kendler and Kendler termed this type of alteration a *reversal shift*; the same stimulus dimension is still related to reward, but which of the two stimuli within this same dimension is positive and which is negative is reversed.

A second type of shift may occur, however, in the second problem-solving task. Instead of size being the reward-relevant dimension, color can be. Now response to the black squares (regardless of their size) will lead to a reward, and response to the white squares (regardless of their size) will not. This type of change involves a shift to the other dimension of the stimuli and is not within the same dimension. Hence the Kendlers term this second type of possible change a *nonreversal shift*. Figure 4.3 illustrates the reversal and the nonreversal shifts. In all cases the stimuli toward which a response will lead to a reward are marked "**+**," while the stimuli toward which a response will not be rewarded are marked "**-**."

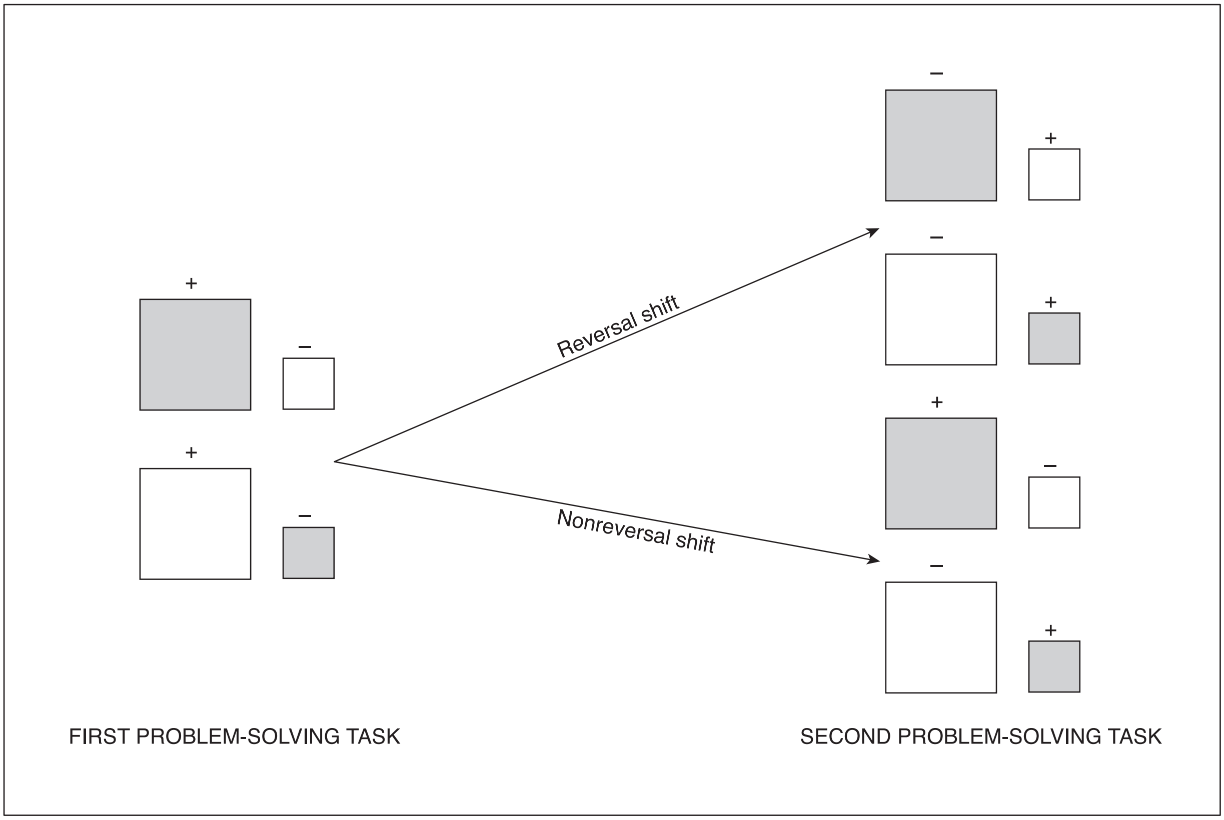


Figure 4.3 An illustration of the reversal shift and of the nonreversal shift.

H. H. Kendler and T. S. Kendler (1962). Vertical and horizontal processes in human concept learning. *Psychological Review, 69*. Copyright © 1962 by the American Psychological Association. Reprinted by permission.

Kendler and Kendler (1962) reviewed the studies of reversal and nonreversal problem solving done with rats, nursery-school children, and college students. After learning the first problem (for example, after making 10 correct responses to the large-size stimuli), would it then be easier to learn a reversal shift or a nonreversal shift (again using the learning criterion of 10 consecutive correct responses)? The Kendlers' review indicated that rats learn a nonreversal shift more easily than a reversal shift. Moreover, so do most nursery-school children. As do rats, these human children reach the criterion for making a nonreversal shift faster than they reach the criterion for making a reversal shift. However, somewhat older children, as well as college students, find a reversal shift easier.

The Kendlers interpreted these age changes by suggesting that, in development, there emerges a new mental process in children such that they move from rat-like responses to college-student-like responses; this new mental process, not present at earlier ages (e.g., efficient language processes), alters children's problem-solving behavior so that a reversal shift becomes easier than a nonreversal shift. Hence, whereas children's problem-solving behavior at the nursery-school level can be accounted for by reference to processes apparently also identifiable in rats, their later behavior may be explained by the emergence of a new mental process.

Certainly the processes present in the nursery-school children provided a developmental basis for the processes seen among the older children. That is, it would be unlikely to find older children who now functioned like college students but never functioned like younger children (or rats, too, in this case). Yet, these former processes are not sufficient to account for the behavior of the older children. The type of problem-solving behavior changes, and this alteration appears related to the emergence of a new mental function. Any attempt to reduce the laws of the later level to those of the earlier level will avoid dealing with the important emergent processes that apparently characterize the older age level. Thus, although other interpretations of these findings have been offered (see Esposito, 1975), the present point is that the work reported by the Kendlers (1962) illustrates the level-of-organization compromise. The laws of the lower level may be involved in those of the higher one but, because those of the higher level involve emergent qualities, the former laws will not suffice to account for the phenomena of the higher level if any attempt at reduction is made.

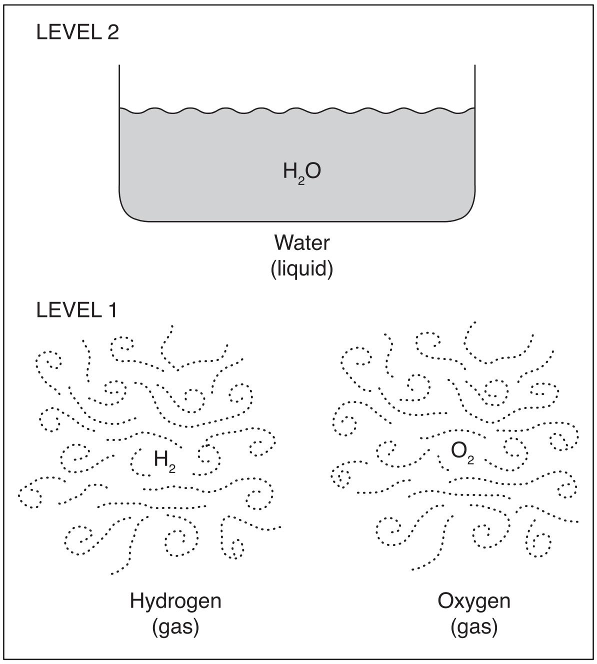


Figure 4.4 An illustration of the levels-of-organization compromise.

The levels-of-organization compromise is presented diagrammatically in Figure 4.4. At Level 1, two gases, hydrogen and oxygen, are present; at Level 2, however, the two gases combine to produce a substance (water) that has a property (liquidness) that did not exist in either of the Level 1 elements in isolation. Although the presence of the lower level's phenomena is certainly implied in the phenomena of the higher level, the latter level still has phenomena (e.g., liquidness) that cannot be understood through reduction to those of the lower level.

The General-and-Specific-Laws Compromise

The second compromise between the mechanist and the organismic positions maintains that there are general and specific laws that govern development: Certain general laws apply to any and all levels of psychological functioning. Yet, each specific level of development is also governed by specific laws. Such a compromise is often found in the work of organismic theorists, for example Jean Piaget (1950, 1970). Like other organismic-developmental theorists who use the concepts of stage in their ideas (see Chapter 5), Piaget views development as involving two processes: first, a general, continuous process (the "equilibration" process) that is present at all levels, and in fact is used to account for the continual development of children through the various stages of cognitive development. Second, there are specific qualitatively distinct phenomena (e.g., preoperational thinking), which actually serve as the definitional basis of the various stages of development at which they occur.

Sigmund Freud, also an organismic theorist, similarly made use of a compromise between general and specific laws of development. Freud (1949) viewed sexual functioning as passing through various "psychosexual stages of development". However, he saw this development as being energized by a finite amount of mental energy ("libido") present in every individual at birth. This mental energy passed through the body of a person in a prescribed sequence, and became concentrated at specific locations of the body at specific periods of the person's life (e.g., the mouth during the "oral stage"). Although this same mental energy was seen as always being involved in emotional ("psychosexual") functioning at all times in a person's life—and as such represents a general law of development—the manner in which emotional functioning was expressed was dependent on exactly where the mental energy was centered. Thus, to Freud (1949, 1954), psychosexual functioning involved the combined contribution of a continuously applicable mental energy and a specific area (or zone) of the body where this mental energy happened to be located at a specific time in development; this specific characteristic of psychosexual functioning determined the mode of expression of one's emotions. Hence Freud's view of psychosexual development is an example of how organismic-oriented developmental scientists may utilize the general-and-specific-laws compromise in their theories.

The general-and-specific-laws compromise is represented in Figure 4.5. At both Level 1 and Level 2, a general law, G, exists. However, at Level 1, there is also a specific law, S_1, present, whereas at Level 2 there is a different specific law, S_2, present. In short, at each new developmental level of organization there are both laws that exist at all other levels and the emergence of new phenomena that cannot be reduced to lower organizational levels.

In sum, then, whether concerned with the general and specific laws that may be pertinent within and across portions of ontogeny, and/or with the descriptions of the laws associated with different levels of organization, there is still the need to depict the process through which the laws of development function to instantiate an individual's trajectory across life. Although I believe that a developmental scientist's search for a conceptual frame useful for addressing this need will result in consideration of RDS-based ideas (see too Overton, 2015a, 2015b), ideas such as probabilistic epigenesis, I should note

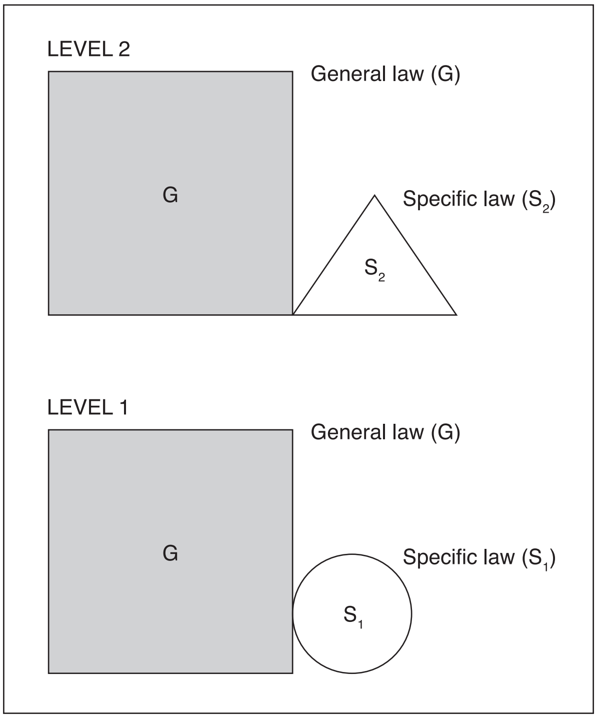


Figure 4.5 An illustration of the general-and-specific-laws compromise.

that, as with organicism, mechanism, and contextualism, the idea of probabilistic epigenesis also has limitations.

Limitations of the Concept of Probabilistic Epigenesis

The concepts of organism, of context, and of the relations between the two found in the probabilistic-epigenetic conception are, as a set, quite distinct from those associated with organismic and mechanist conceptions. Such a probabilistic-epigenetic perspective leads to a multilevel concept of development, one in which the focus of inquiry is the dynamic organism–environment relation or transaction. Further, such an orientation places an emphasis on the potential for systematic intraindividual change in structure and function—for plasticity—across the life span. Yet, several conceptual and derivative methodological problems must be confronted in order: (1) To make probabilistic epigenesis a useful developmental model (Baltes, 1979b); and (2) To usefully employ this model as a framework within which to study dynamic individual↔context relations.

First, substantively, developmental scientists recognize that despite the great amount of evidence that exists for human plasticity (e.g., Baltes et al., 1999, 2006; Gollin, 1981; Gottlieb, 1997; Lerner, 1984; Lickliter, 2016; Lickliter & Honeycutt, 2015), several fundamental questions about the dynamically coactive parameters of plasticity remain to be answered. Are different levels of analysis and/or different targets within levels differentially plastic at different ontogenetic or historical times? For example, it may be the case that selected features of the human genotype (e.g., the number of chromosomes we possess) cannot be altered (without, at least, severely damaging our organismic integrity) no matter what the nature of our organism↔context relations may be. On the other hand, more molar, behavioral features of functioning may not be subject to such restrictions.

For instance, are there limits to the number of random digits a person can learn to recall or to the number of locations a person may recall? Current evidence indicates that such limits are quite variable, and that even among very old people—for example, those in their ninth or tenth decade of life—there are training techniques that can capitalize on their still-available (albeit diminished, relative to earlier age periods) "reserve" of plasticity in order to enhance performance on such tasks (e.g., see Baltes, 1987, 1997; Baltes et al., 1999, 2006).

In addition to not fully knowing the limits of plasticity that currently characterize levels of analysis, developmental scientists do not know what further substantive and technological advances may imply for the future character of these limits. If developmental scientists take the idea of probabilistic epigenesis seriously, and if they recognize that science and technology represent natural parts of the human ecology, then they cannot anticipate where future scientific advances may lead. The arrow of time is not simply a straight line. Non-normative events can change the intercept of the line for individuals and groups (Bronfenbrenner & Morris, 2006; Elder et al., 2015).

For example, the geneticist Brown (1981) noted that in the 1970s scientists could not *imagine* how a gene could ever be isolated. Yet, Nobel laureate Paul Berg (1981) indicated that by the 1980s such identification was quite routine, and that in just a few years the growth in the application of recombinant DNA methods had been truly explosive. For instance, he indicated that

> molecular cloning provides the means to solve the organization and detailed molecular structure of extended regions of chromosomes and eventually the entire genome, including man. Already, investigators have isolated a number of mammalian and human genes, and in some instances determined their chromosomal arrangement and even their detailed nucleotide sequence.
>
> (Berg, 1981, p. 302)

Thus, if developmental scientists take the idea of probabilistic epigenesis seriously, and if they recognize that science and technology represent natural parts of the human ecology, they cannot completely anticipate where future scientific advances may lead. As a consequence, the current limits of plasticity are not necessarily future ones. These limits are themselves plastic and will probably change in a broader and broader direction in ways that, for some developmental scientists, are beyond imagination. As Toulmin (1981) put the issue:

> And as for the possibilities open to future, more complex cultures, there too we must be prepared to speculate open-mindedly. There, perhaps, people generally will take pride in having overcome the "illusions" of material conservation and Euclidean space alike, and may come to talk about everyday material objects with the same conceptual sophistication we ourselves display toward such un-everyday things as electrons.
>
> (p. 261)

But recognition that the limits of plasticity can change across history raises a developmental issue. The actualization of plasticity of course involves change, and change can only be identified across time. Numerous questions exist about the rates of change of plastic processes at the several levels of analysis that are integrated to provide the bases of behavior. First, it is clear that there is a "non-equivalent temporal metric" across the various levels of analysis (Lerner, Schwartz, & Phelps, 2009) involved in person↔context transactions (see Chapter 13). That is, all levels of the context change, but time may not have an identical meaning at all the levels (Elder et al., 2015; Lerner et al., 2009).

One way to understand this point is to note that the smallest meaningful division of time to detect change differs among levels. If time is the X axis, with the Y axis reflecting levels of a target process, then sensible X-axis divisions to detect some facets of infant development may be as small as weeks. However, the smallest sensible division to detect changes in society brought about by new public health recommendations, for example, regarding the benefits of exclusive breastfeeding for the first six months of life (Gartner et al., 2005), may be a year. As such, the effects of such a policy statement on infant immunological development might need to be assessed not by studying changes *within* a group of infants but, instead, by comparing differences *across* different infant birth cohorts.

For example, through such a comparison a developmental scientist could understand if a group of, say, 36-week-old infants experienced fewer occurrences of otitis media one or two years after the policy than was the case for a group studied one or two years before the policy. In other words, because it may take a year or more to detect changes due to macro-level alterations, within-person changes (which may occur over weeks or months) may "fall between the cracks," that is, between the year-by-year (or larger) divisions, of the X axis. Indeed, if an attempt is made to verify the existence of such macro (e.g., policy) influences, it may be that a long-term, perhaps intergenerational, perspective needs to be taken; or in a within-cohort analysis, it may be that only interindividual differences in intraindividual change, and not intraindividual change itself, can be assessed.

In addition, even within a given level, time may not have an equivalent meaning at different points in development (Bronfenbrenner & Morris, 2006; Elder et al., 2015). For example, on the level of the individual, a one-year separation between birthdays may seem a vast length of time to a 5-year-old; to someone experiencing his or her thirty-ninth birthday the one-year period until the fortieth birthday may seem quite short; and to an 85-year-old, a one-year wait for some important event may again seem quite long.

Complicating this issue is that, although the effects of a biological intervention on society may take a long time to detect, there is not necessarily symmetry of influence. That is, "upper level" societal alteration and social change may impact quite visibly and relatively rapidly on "lower level" individual and biological processes. For example, changes in federal government funding programs for school lunch programs for poor children; for welfare support to working, single mothers; or for Medicare and Medicaid for the elderly can influence relatively quickly an individual's health, cognitive, and familial functioning variables (Lerner, Sparks, & McCubbin, 1999, 2000).

The issues of the non-equivalent temporal metric, and of the asymmetry of interlevel influences, can be seen to lead to other ones. First, given the rates of change of different levels, one needs to know how processes at different levels connect to one another: How do interlevel influences occur? One answer to this question may be to explore the use of a "goodness of fit" model of person↔context relations I noted earlier in this chapter (e.g., Chess & Thomas, 1999; Eccles, 1991; Eccles & Midgley, 1989; Lerner & Lerner, 1983, 1999). Here, individual behavioral characteristics that are congruent with pertinent behavioral presses are studied for their import for adaptive person–immediate social context (e.g., peer group) exchanges.

Of course, the goodness-of-fit model is not the only conception of person↔context relations that may be derived from a probabilistic-epigenetic orientation. Indeed, an infinity of interlevel relations may perhaps occur, and there exists a potentially similarly large array of ways to model them. Scholars need to devote more thought and empirical energies to their investigation and, in turn, to providing the evidence base to apply developmental science in order to capitalize positively on the individual↔context relations of focal concern in RDS-based concepts such as probabilistic epigenesis.

Issues for Intervention

Interventions represent attempts to (a) ameliorate or prevent undesired or problematic features of individual and/or group behavior, and/or (b) enhance or optimize an individual's or a group's behavior or social situation in the direction of some desired or valued end (e.g., better health, improved self-concept, or social justice promotion; Fisher et al., 2013; Lerner et al., 2014; Lerner & Overton, 2008). A probabilistic-epigenetic view of individual↔context relations and of plasticity raises several issues pertinent to intervention.

First, the issue of asymmetry of interlevel influences raises largely unaddressed concerns about efficiency and about cost–benefit ratios. For instance, with an intervention targeted at the cognitive-behavioral level, for example the modification of academic achievement, is it more efficient to institute a "bottom-up strategy" (e.g., intervening at the biological level, e.g., through enhancing prenatal care and maternal nutrition and reducing the likelihood of encountering teratogens), a "parallel-level strategy" (e.g., intervening by cognitive-behavioral means), or a "top-down" strategy (intervening by instituting or changing social programs)? In turn, is it feasible to answer "all of the above," and attempt a thorough systems change strategy? Which strategy leads to the most benefits, relative to economic, social, and personal costs? Developmental scientists simply do not know the answers to these questions for many of the potential targets of intervention.

Moreover, a decision about the level of analysis on which to focus one's intervention efforts is complicated by the fact that all levels of analysis are developing or changing. Whereas this feature of the human condition permits both *concurrent* (same time, immediate) and *historical* (long-term, delayed) interventions, it again raises questions of efficiency and cost–benefit ratios. For example, when during the life span is it best to intervene to optimize a particular target process (and, of course, on what level is it best to focus one's efforts)? Are periods of developmental transition (e.g., puberty, retirement), or are periods of *relatively* more stability (e.g., midlife; Lachman & James, 1997), better times within which to focus one's efforts? Moreover, do some intervention goals, for example the elimination of fetal alcohol syndrome, or FAS (Streissguth et al., 1980), require an intergenerational-developmental rather than an ontogenetic-developmental approach? In the case of FAS, for instance, might it be of more benefit to intervene with women who are at risk for excessive alcohol use during pregnancy *before* they become pregnant? Again, for most potential targets, intervention issues such as these have remained relatively unaddressed.

A final relatively unaddressed issue relates to direct and indirect intervention effects and to planned and unplanned effects. If an individual's plasticity both derives from and contributes to changes within the other levels of analysis with which he or she transacts, actualizing the potential for plasticity at any one level of analysis will influence changes among other variables, both at that level and at others. From this perspective, any direct and/or intended effect of intervention will have indirect and often unintended consequences (Willems, 1973).

This recognition leads to two points. First, interventions should not be initiated without some conceptual or theoretical analysis of potential indirect and unintended consequences. Simply, developmental scientists need to consider the developmental system in planning interventions.

For instance, changing a spouse's assertiveness may be the direct intended effect of a cognitive-behavior therapist's efforts. However, the changed assertiveness might lead to a diminution of marital quality and, in addition, to a divorce. Such indirect effects might have been unintended by the therapist and undesired by either therapist or client. Thus, developmental scientists framing their work within models derived from RDS metatheory must think quite seriously about the broader, contextual effects of their intervention efforts. Developmental scientists should be sensitive to the general possibility, and perhaps some specific instances, of the indirect effects of their intervention efforts. Such reflection will be useful in several ways, a major instance of which is that some undesirable indirect effects may be anticipated. If so, the issue of cost–benefit ratios can be addressed before intervention begins.

Of course, the fact that undesired effects may arise from intervention efforts raises the point that plasticity is a double-edged sword: A relational developmental system is always open to enhancement; but it is also always open to deterioration. That is, plasticity permits interventions to be planned in order to improve the human condition, but indirect effects may also cause a deterioration in a target person's life condition and/or the condition of his or her context. Moreover, this problem is complicated by recognizing that as a consequence of people being transactionally related to their multilevel contexts, a failure to intervene, to alter the context of life, is *itself* an intervention; that is, it keeps the context on a trajectory from which it might have been shifted if a developmental scientist had acted. Thus, a scientist must assess the cost–benefit ratio not only of acting but also of failing to act.

Conclusions

I have pointed to some of the key conceptual and methodological issues that remain to be resolved if an RDS-based conception such as probabilistic epigenesis is to be successfully used to study individual↔context relations and also to intervene to enhance such relations. Pessimism because of the presence of these problems is unwarranted. Every approach to human development has limitations, as I hope I have made clear in this chapter. Thus, there are problems to be resolved about specific RDS-based ideas and about RDS-based theories more generally. However, the future work that is needed does not single these views out from scholarly tasks associated with other developmental metatheories. Indeed, given that it was only in the latter decades of the twentieth century that RDS-based models came to the fore (see Chapter 3), the clarity with which the problems have been articulated, the methodological advances that have already been made (e.g., see Molenaar et al., 2014; Molenaar & Nesselroade, 2014, 2015; Nesselroade & Molenaar, 2010), and the several data sets that speak to the empirical use of this contextual perspective (e.g., see the four volumes of the seventh edition of the *Handbook of Child Psychology and Developmental Science*; Lerner, 2015e) are reasons for great optimism for the future.

RDS-based theoretical models have come to the forefront of developmental science at the time of this writing, and I anticipate that they will continue to influence scientific activity across the twenty-first century (Lerner et al., 2014). Given the potential impact of this particular instance of a philosophical view's impact on science, it is useful to discuss briefly the nature and implications of philosophy–science relations in developmental science.

IMPLICATIONS OF PHILOSOPHICAL MODELS OF DEVELOPMENT FOR DEVELOPMENTAL SCIENCE

Philosophical models should not be evaluated in regard to whether they are correct (Overton, 2006, 2015a, 2015b; Reese & Overton, 1970). Nevertheless, as illustrated in this chapter about mechanist, organismic, contextual, and RDS metatheories, they shape the theories that scientists use to interpret the facts they derive from their studies of the "real" world. Moreover, in shaping theories, metatheories shape as well the very questions scientists ask in their study of the real world. The questions that follow from different theories are likely to be quite different, and in turn, the data generated to answer these contrasting questions are unlikely to provide comparable answers. In this regard, every metatheory gives rise to an associated *metamethod*—"a vision of the tools that will be most adequate to explore the world described by the metatheory" (Overton, 2006, p. 21).

For instance, a nurture-mechanist-oriented developmental scientist may try to reduce behavior to learning principles common to people of all ages. Thus, he or she might seek to discover those environmental–behavioral reactions that remain identical from infancy through adolescence and adulthood. Alternatively, an organismic-oriented developmental scientist might attempt to find those phenomena that are unique to and representative of particular age periods. In turn, developmental scientists using RDS metatheory might assess the relation of a specific event to others at earlier times in the life cycle, as well as to current cultural, environmental, and long-term historical influences; the scientist might then appraise the reciprocal nature of these relations.

Whereas each of the above described developmental scientists who are using different theoretical models are engaged in the same scientific-knowledge-building enterprise, they differ in "*the route* [they take] *from common sense to science*," in their "*methodology of science*" (Overton, 2006, p. 71; see Overton, 1998, for a historical review of these differing routes). Scientific activity derived from alternative world views asks different questions about development. Consequently, scientists committed to alternative world views may collect data on different topics. One scientist is not necessarily functioning correctly and another incorrectly. The issue is not one of deciding which theory is best, or which leads to truth and which does not. Theories from different world views ask different questions because the very nature of reality is conceived of differently. Thus, what is a true depiction of reality for one world view may be irrelevant for another (e.g., see Kuhn, 1962, 1970).

One major implication of the nature of this philosophy–science relationship is that a criterion other than truth must be used to evaluate interpretations of development. In this chapter and prior ones, I forwarded "usefulness" as one such criterion (e.g., in regard to accounting for more variance in developmental data sets, leading to more novel discoveries, or integrating a broader range of phenomena pertinent to development than is the case with other positions); such dimensions of utility could be summarized by the concepts of precision, scope, and deployability. When theories have precision, scope, and deployability they are useful for the description of developmental phenomena, for the explanation of development, and for devising ways to optimize human behavior and development.

Throughout its history, the study of human development has been the captive of numerous fundamental antinomies (Overton, 2015a). Whereas the original Cartesian splits were between mind and body or subject and object, the most prominent of contemporary split conceptions has been, of course, between nature and nurture or variants of this split, such as maturation versus experience or innate versus acquired (e.g., Garcia Coll, Bearer, & Lerner, 2004; Lerner et al., 2014). Other splits that have marked the field include, for example, continuity and discontinuity, stability and instability, constancy versus change, qualitative change versus quantitative change, individual and context, and basic science versus applied science.

At the time of this writing, however, as a consequence of the acceptance across virtually all of developmental science (e.g., Damon, 2015; Lerner, 2015a, 2015e) of relational philosophical ideas and the associated RDS metatheory of human development (Overton, 2015a), these fundamental antinomies inherent in pure mechanist, organismic, and contextual philosophical models are regarded as theoretically less "useful" for the description, explanation, and optimization of human development. As a result, the crude and counterfactual reduction of causal or fundamental processes in human development to either genetic inheritance or stimulus–response connections has been eliminated from the mainstream of developmental science (see the chapters in Overton & Molenaar, 2015, for examples). Such conceptual splitting and the reductionism implicated by splitting are regarded as, at best, *passé* historical phases in a historical progression to the central emphasis in contemporary developmental science on mutually influential, individual↔context relations. These relations are the basic focus of developmental analysis (e.g., see Fischer & Bidell, 2006; Lickliter, 2016; Overton, 2015a; Sameroff, 2009; Witherington, 2015; Witherington & Lickliter, 2016).

Within a developmental science informed by RDS-based theories, the goals of science change from the traditional tripartite conception of description, explanation, and manipulation (or control) of phenomena to a tripartite interest in description, explanation, and optimization of change across the life span (Baltes, 1987; Baltes et al., 2006). As I have noted, because of plasticity, a system that can be changed for the better can also be changed for the worse. However, for obvious ethical reasons the full range of potential variation that could be produced through one's assessments of explanations of human development cannot be empirically tested. Instead, the developmental scientist seeks to specify and test individual↔context relations that are linked developmentally to health and positive functioning.

The conduct of such scholarship illuminates the character of the basic relational process of human development and, as well, provides information about how to promote positive human development in real-world settings, in the ecology of everyday life (Bronfenbrenner, 2005). Depending on the levels of analysis involved in the contexts being studied in relation to the developing individuals involved in a given research project, the work of providing information about the promotion of positive development may be termed "intervention research"; such research may be targeted at either the level of community programs or of social policies (Lerner, 2004). Yet, such "applied" work is at the same time the very work that is required to understand the character of (adaptive) developmental changes. As such, within an RDS-based approach to developmental science, there is no split between theoretically predicated research about basic processes and practically important research elucidating how knowledge may be applied to foster programs or policies better able to promote positive development (Lerner, 1995c, 2002, 2004, 2005). For instance, Jensen, Hoagwood, and Trickett (1999) describe an instance of such research in the arena of community-based programs aimed at enhancing mental health. Termed an "outreach scholarship" model, Jensen et al. (1999) explain how researchers and their universities may collaborate with community members to go beyond demonstrating what programs could work in the abstract. They describe how a collaboration can identify the specific, mutually beneficial relations between universities and their community and how such identification can produce useful programs. Jensen et al. define such programs as being effective in fostering mental health and, as well, palatable, feasible, durable, affordable, and hence ultimately sustainable in communities.

As discussed again in later chapters, and in Chapter 13 in particular, the outcome of such synthetic basic↔applied scholarship is twofold: positive human development and social justice! At the individual level, developmental scientists learn how to identify and align the developmental assets of contexts to promote positive human development among diverse individuals. For instance, in regard to youth development, developmental scientists can answer an optimization question structured on the basis of the specificity principle (Bornstein, 2017). For example, they can ask "What specific contextual resources, for what specific youth, at what specific points in their adolescence, result in what specific features of positive development?" In answering this question, developmental scientists may learn the sectors and features of the context that are needed to maximize positive development among diverse youth. For instance, Theokas and Lerner (2006) found that greater access in schools to high-quality teachers (e.g., as operationalized through lower teacher–student ratios) is linked to positive youth development; however, the opportunity for a youth to be in such a relation with a teacher obviously varies in relation to socioeconomic issues pertaining to a given school or school district (e.g., involving teacher salaries).

Accordingly, the optimization component of the tripartite scientific agenda of developmental scientists, as well as the synthesis between basic and applied (program and policy) work, means that theoretically predicated changes in the developmental system (i.e., changes in the contextual component of the individual↔context relation within the school or school system in this example) need to be evaluated in regard to whether positive development can be equally promoted among individuals whose socioeconomic circumstances lower the probability of positive development. The developmental scientist should strive to identify the means to change the individual↔context relation in order to enhance the probability that all individuals, no matter their individual characteristics or contextual circumstances, move toward an equivalent chance to experience positive human development (Bronfenbrenner, 2005). Such scholarship is aimed at promoting social justice, that is, the opportunity within a society for all individuals to have the chance to maximize their chances to develop in healthy and positive ways (Fisher et al., 2013; Lerner & Overton, 2008).

In short, then, enhancing the presence of social justice in society is a necessary goal of a developmental science that is framed by a process-relational paradigm and RDS metatheory and that is concerned, therefore, with learning how to foster adaptive developmental connections among all individuals and all contexts. Such a developmental science is committed to the tripartite scientific mission of description, explanation, and optimization. Consistent with the integration of basic and applied science inherent in the RDS-based perspective, the developmental scientist, through her or his research, needs to be as much an agent of social change in the direction of social justice as a scholar seeking to understand the nomothetic and idiographic laws of human development. Indeed, without theory-predicated tests of how to foster social justice for all individuals, the research of developmental science will be inevitably limited in its potential generalizability and ecological validity. Without the promotion of social justice as a key scholarly goal, developmental science is critically incomplete (Lerner et al., 2014; Lerner & Overton, 2008).

Through developmental science research predicated on RDS metatheory, developmental scientists have a historically unique opportunity to conduct scholarship that will fruitfully address what may be argued to be the “really big” question for science and society. Cast in regard to the specificity principle (Bornstein, 2017) derived from RDS-based ideas, this question asks, “What actions, of what duration, with what individuals, in what communities, at what points in ontogenetic and historical time, will result in what features of positive development and contributions to self, family, community, and civil society?” Or, more simply, developmental scientists may answer the question, “How may developmental science contribute to promoting mutually beneficial relations between healthy individuals and a world marked by social justice, democracy, and liberty?”

CONCLUSIONS

Since any theory might be used to pursue understanding of human development or to influence public policy, I have suggested that theories should be evaluated on the basis of their usefulness and indicated that an RDS-based perspective may be particularly useful in regard to description, explanation, and optimization. Of course, these uses depend on the meaning attached to the concepts of description, explanation, and optimization.

Theories differ in regard to the features of behavioral or mental life they deem important to describe. Nevertheless, there is consensus that description per se pertains to the depiction or representation of the phenomena of interest in a given theory. However, considerably less consensus exists in regard to the explanation of development. For instance, as noted earlier in this chapter, mechanist-, organismic-, and contextual-oriented theorists differ in respect to whether cause–effect, formal, or configural information is regarded as essential for explanation. In turn, when theories differ in regard to how development is explained, they vary also in their ideas for what variables need to be engaged in interventions aimed at optimizing development. In the next chapter, I focus on key dimensions of difference among theories that have been used to frame the work of developmental scientists.

CHAPTER FIVE

Theoretical Roots of Contemporary Developmental Science: Nomothetic (Stage), Differential, and Idiographic (Ipsative) Approaches

Walter Emmerich

In previous chapters, I discussed the historical bases and philosophical underpinnings of the key conceptual issues of human development (e.g., the nature–nurture and the continuity–discontinuity controversies). In relation to these conceptual issues, I also noted various theoretical approaches to human development. The approaches I cited included various mechanistic-nurture positions, for example, the behaviorist ideas of Skinner (1971) and of Bijou and Baer (1961); mechanistic-nature positions, for example, the hereditarian ideas associated with behavior genetics (e.g., Plomin, 2000; Plomin et al., 2016) or evolutionary developmental psychology (e.g., Bjorklund, 2015, 2016; Bjorklund & Ellis, 2005); and relational developmental systems (RDS)-based conceptions (e.g., Gottlieb, 1997; Lerner, 2004; Lickliter, 2016; Schneirla, 1957; Overton, 1984, 2015a). These groups of theories will be discussed in more detail in Chapters 10, 11, and 12. However, these theories do not exhaust the range of theoretical approaches that can and have been used to understand the course of human development.

Walter Emmerich (1968) explained that these additional approaches—the stage theory approach, the differential approach, and the idiographic (ipsative) approach—also relate to the core conceptual issues of human development and, as well, to the ideas associated with the various theoretical models I have already discussed. Thus, these additional approaches merit separate discussion, and I will frame much of my discussion of them by drawing on Emmerich's (1968) scholarship.

The three approaches, and especially the stage theory approach, have been major conceptual orientations to theory building in human development (e.g., Emmerich, 1968; Muuss, 1996). Stage theories have attempted to depict universal features of development, features applicable to all humans. In turn, differential approaches have sought to identify features of development common to specific groups of people. Researchers using idiographic (ipsative) approaches begin their analysis of human ontogeny by seeking to find characteristics of development that might be unique to individuals and, then, and only then (Molenaar & Nesselroade, 2015; Rose, 2016), will they seek to aggregate data across individuals.

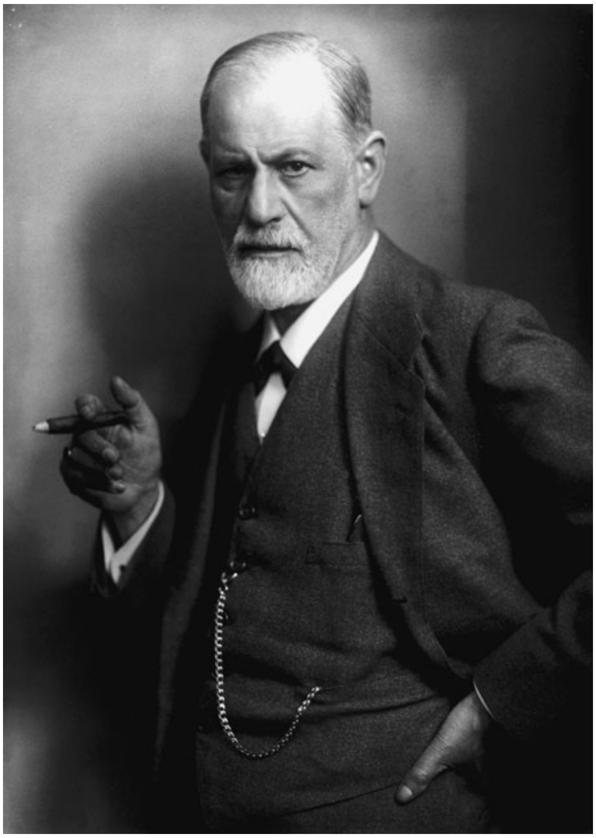
In essence, a discussion of these three approaches to theory reflects the often-cited observation of Kluckhohn and Murray (1948, p. 35) that, in certain respects, every person is like all other people (as in stage theory), like some other people (as in the differential approach), and like no other person (as in the idiographic/ipsative approach). I focus first on the stage approach and then consider, respectively, the differential and idiographic (ipsative) approaches.

THE STAGE THEORY APPROACH TO DEVELOPMENT

The stage approach to developmental theory may also be simply termed the developmental approach or the classical approach, perhaps because it was systematized first historically. Accordingly, I will use the terms stage theory, classical theory, and classical developmental theory interchangeably.

Various theorists who have used this approach have considered different aspects of development (e.g., the development of cognition, morality, and personality). Nevertheless, all classical developmental theories share some specific characteristics. These theories hold that, if they develop, all people pass through a series of qualitatively different levels (stages) of organization and that the ordering of these stages is invariant across all humans. To such a theorist, then, there are universal stages of development. If people develop, they will pass through all these stages, and they will do so in a fixed order. The ordering of the stages is held to be invariant; this stipulation means that people cannot skip stages or reorder them.

Freud's (1949) theory is an example. Freud postulated that there are five stages in psychosexual development: the oral, anal, phallic, latency, and genital stages. Freud held that if a person develops, he or she will pass through all these stages; he believed that all of the stages apply to any given person's development and, in fact, to all people's development. Moreover, Freud contended that the order of these stages is the same for all people. Thus, it would be theoretically impossible for someone to skip a stage; one could not go right from the oral



Sigmund Freud

stage to the phallic stage; instead, one would have to develop through the intermediary stage, the anal stage. Similarly, one cannot reorder the sequence; thus, one could not go from the oral to the phallic stage and then to the anal stage. In essence, according to Freud, all people who develop must pass through each stage in the specified, and fixed, invariant sequence.

The Definition of a Developmental Stage

But what are these entities that develop in an invariant sequence? Answering this question—and arriving, therefore, at a definition of a developmental stage—is a far from uncomplicated and uncontroversial issue. Indeed, several distinct and quite diverse theoretical stage or stage-related formulations have been forwarded in attempts to characterize human development across the life span. Runyan (1980) commented on the breadth of the formulations that have been forwarded. He noted:

> The search for useful ways of conceptualizing the course of human lives has been a long and difficult one, approached from many different theoretical perspectives, each with distinct assets and limitations. To provide a partial list, the life course has been conceptualized as a sequence of episodes and proceedings (Murray, 1938, 1959); a sequence of tasks or issues (Erikson, 1963); a sequence of stages (Levinson et al., 1978; Loevinger, 1976); a sequence of transitions (Lowenthal, Thurnher, & Chiriboga, 1975); a sequence of personality organizations (Block, 1971); a sequence of changing environments and organismic responses (Skinner, 1953); a sequence of dialectical operations (Riegel, 1975); a sequence of person–situation interactions (Baltes & Schaie, 1973); and a sequence of behavior-determining, person-determining, and situation-determining processes (Runyan, 1978). The life course has also been conceptualized from sociological and social-structural perspectives that focus more on roles, life-long socialization, age norms, and the flow of populations through socially and historically structured pathways (e.g., Clausen, 1972; Elder, 1975, 1977; Neugarten & Datan, 1973; Riley, Johnson, & Foner, 1972).
>
> (p. 951)

Still other dimensions of diversity exist in the stage-related formulations that have been applied to understanding human development across life. For instance, many developmental stage theorists describe changes across much, and in some cases all, of the life span, and they focus on broad-based changes—for example, on the nature of individuals' psychosocial conflicts (e.g., Erikson, 1950) or on individuals' cognitive structuring of the world (e.g., Bruner, 1964; Piaget, 1954). In turn, other theorists have offered stage-like descriptions of more circumscribed domains of development (e.g., Case, 1984, 1992a, 1992b; Davison et al., 1980; Feldman, 1994, 1995, 2000; Fischer, 1980; Fischer & Bidell, 1998; Gardner, Kornhaber, & Wake, 1996; Kohlberg, 1968; Selman, 1976; Siegler, 1978, 1981; von den Daele, 1975).

Some theorists have opted to investigate relatively specific areas of ability, such as problem-solving skills (e.g., Siegler, 1981) and social-cognitive development (e.g., Selman, 1976; Turiel, 1978). In addition, these theorists have tried to define patterns of change more precisely by limiting their focus of study, by delineating smaller and more circumscribed increments of developmental change, and by identifying procedures for measuring developmental change. Some of these theorists, for instance, have described specific sequences of development and have argued against the existence of pervasive underlying structures and homogeneous functioning across different domains of behavior. For instance, Fischer (1980; Fischer & Bidell, 1998) portrayed development as the acquisition of sequences of skills in different domains of functioning.

A final complication is that there exist several terms in the developmental literature that may relate to the stage concept. However, theorists differ in regard to the way terms are used in relation to stage theories. As Glasersfeld and Kelley (1982) observed: "In the field of developmental psychology we find ambiguity and occasional confusion with regard to the use of the terms stage and level. The confusion is compounded by the terms period and phase which some authors freely interchange"

(p. 152). Similar problems have been identified by Campbell and Richie (1983) and by Wohlwill (1973), who noted the confusion that exists between the concepts of stage and sequence.

One key point to be derived from these discussions is that, although all developmental stages involve a sequence (of invariantly ordered qualitative changes in an organism's structures), not all sequences involve developmental stages. For example, the sequence of changes in motor behaviors that has been described by Shirley (1933) describes "steps" along a path of physical maturation; such "steps" do not involve the theoretical specification of qualitative structural changes (Wohlwill, 1973). A second key point to abstract from these discussions is that whatever is meant by stage is not merely an increase in the quantity of behaviors or skills; rather, a conception encompassing other, more abstract and transformational, changes is involved in the use of this term.

In sum, there is breadth and depth in the diverse developmental science literature that attempts to use the idea of stage and/or some stage-like notions—(i.e., "period," "phase," or "level") (Campbell & Richie, 1983; Glasersfeld & Kelley, 1982; Wohlwill, 1973). The task facing scholars seeking to understand this literature is to extract from this literature the key features of, and/or issues involved in formulating, a definition of a developmental stage. Fortunately, being able to draw on the scholarship of other developmental scientists who have taken on this task (e.g., Feldman, 2000; Flavell, 1971, 1972; Flavell & Wohlwill, 1969; Kessen, 1962; Wohlwill, 1963, 1973) somewhat simplifies this work.

Based on the analyses in this literature, I believe it is useful to start a discussion of this definitional issue by noting that, in the most general sense, developmental stages may be regarded as portions of the life span that are qualitatively different from each other. That is, each stage in a given theoretically specified sequence represents a qualitatively different organization—or, more precisely, a qualitatively different structure—from every other stage. In fact, the existence of qualitative, structural differences among portions of life is the basis of the stage formulation. That is, the reason why one portion of time in ontogenetic development is labeled as one stage and another portion of time is labeled as another stage, is that developmental scientists believe that within each of the two periods something qualitatively different exists. If different portions of development were not qualitatively different, there would seem to be no reason to maintain that they were, in actuality, different portions of development. Thus, it is necessary for the classical theorist to posit the existence of qualitatively distinct stages. Simply, from this vantage point, a stage involves transformational change and never *just* variational change (Overton, 2015a).

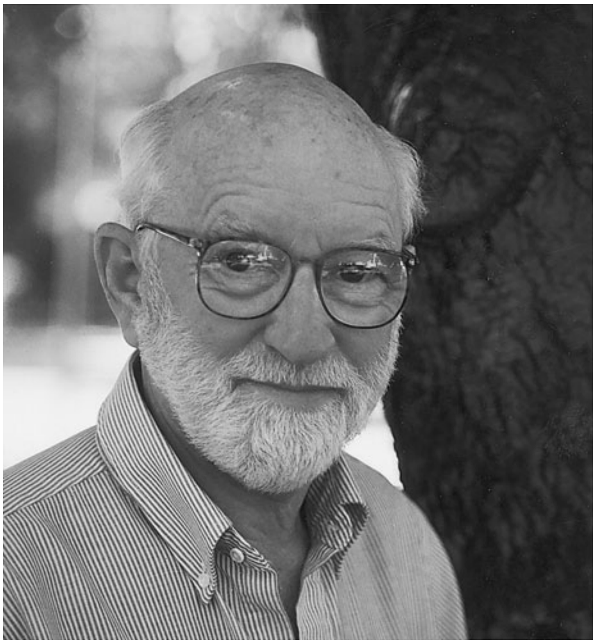
Joachim Wohlwill (1973) underscored this view by noting that the concept of stage "is most profitably reserved for modal interrelationships among two or more qualitatively defined variables, variables developing apace" (p. 192). He added that "conceptual links among these behavioral dimensions allow each stage to be defined in terms of a set of behaviors sharing some feature in common. In other words, 'stage' is taken as a construct within a structurally defined system, having the property of unifying a set of behaviors" (Wohlwill, 1973,

Joachim Wohlwill

p. 192). Thus, in Wohlwill's view, the presence of qualitatively distinct and integrative structures differentiates one period of life from another.

In discussing the stage-related properties of cognitive development, John Flavell (1971) offered a compatible conception of stage; but he also added more elements to the definition. Flavell asked what would be revealed if one could take a psychological X-ray in order to evaluate all the cognitive items present in a child who is said to be at a given stage of development ("item" is used here to refer to such things as concepts, rules, or, in fact, any cognitive "element"). Flavell's (1971) conception of stage led him to say four things about these items. First, he claimed that the items do not exist in an unrelated manner, as elements isolated one from the other. Rather, they interrelate with each other and can, as such, be said to be organized into cognitive structures. Second, Flavell (1971) contended that "the items and their structural organizations are qualitatively rather than just quantitatively different from those defining previous stages of the child's cognitive evolution; they are genuine developmental novelties, not merely more efficient or otherwise improved versions of what had already been achieved" (pp. 422–423).

Thus, in stressing that qualitative structural distinctiveness is a key defining attribute of stages, Flavell (1971) took a position consonant with that of Wohlwill (1973). His ideas are also consistent with Overton's (2015a) ideas of transformational change discussed in Chapter 4. As noted, Flavell added two other statements to these first two in order to present what he saw as the key attributes of stages. However, even in regard to his first two statements, he introduced some qualifications that complicate the conception of stage he put forward.


John Flavell

First, as seen in his second statement quoted above, Flavell raised the issue of the role of quantitative changes in development and their relation to the qualitative structural changes that define a stage as an ontogenetically novel period in life. The presence of qualitative change does not deny the presence of quantitative change (variational change; Overton, 2015a), and vice versa. Both exist in development; and, in fact, it may be that if one focused on how people develop from one stage to the next (i.e., if one focused on stage transitions), one would see "that processes which either remain the same or only change quantitatively could directly or indirectly facilitate the qualitative changes we observe" (Flavell, 1971, p. 425; see also Flavell & Wohlwill, 1969). Indeed, in major examples of stage theories—that is, the theories of Piaget (e.g., 1970), Kohlberg (e.g., 1978), and Freud (e.g., 1949)—a role for invariance is specified. All theorists posit that the functioning of a constant qualitatively unchanging process is the basis of a person's movement from one stage to the next; that is, from the continual application of a qualitatively constant functional invariant (i.e., a process that always functions in the same way) qualitative changes occur, that is, stage transitions take place. In Chapter 4, I discussed this possibility in regard to what I termed the "general and specific processes" compromise. In Piaget's (1970) and in Kohlberg's (1978) theories the "equilibration process" is the functional invariant accounting for stage transition. In Freud's (1949) theory, the libido model plays this role. In addition, in the stage theory of Erikson (1959), his idea of the "maturational time table" has this function.

Thus, it seems that to specify what is changing in development and, more basically, how this change comes about, one must posit the existence of a constant. Indeed, in a more general sense, how could change be detected unless there were some constancy against which to appraise it (Lerner, 1984)?

A second complication Flavell (1971) introduced into his first two definitional statements concerns the idea that a stage involves the organization and interrelation of specific (i.e., qualitatively distinct) items into a structure. Flavell noted that, to use the term structure correctly, there must be at least two items or elements linked by at least one relationship. But Flavell contended that there exist two other properties of a structure. He claimed that a structure provides a "common underlying basis of a variety of superficially distinct, possibly even unrelated-looking behavioral acts" (Flavell, 1971, p. 443). This view is also taken by Wohlwill (1973), who added that stages are "systematic forms of interpatterning among sets of developmental responses" (p. 191). However, Flavell (1971) also contended that structures involve organizations of items that are "relatively stable, enduring affairs, rather than merely temporary arrangements" (p. 443). This property of a structure is likely to generate more controversy than the others that Flavell suggested. There are at least two reasons for such controversy.

One is that "relatively stable," "enduring," and "temporary" are not fixed or standardly agreed-on time spans. Different theorists are free to attach time spans to these terms in almost any manner they wish or, at the very least, with enough of a range that what is seen as non-enduring by one theorist may be viewed by another theorist as exhibiting a high rate of change. For example, a structure prototypic of an infant's early cognitive functioning for three to six months may be seen by a scholar who focused on theorizing only about the early years of life as a relatively continuous organization (and, given this theorist's frame of reference, it is relatively continuous). However, a theorist who is concerned with the scope of the entire life span (e.g., Erikson, 1959, 1963) might contend that such a structure was short-lived and, at best, only transitory. Moreover, even short-lived structures, such as those that are studied by comparative psychologists concerned with "transitory ontogenetic adaptations" (Gottlieb, 1983), may be of great importance for the development and, indeed, the survival of an organism.

A second reason why controversy may exist in regard to using the length of time a given organization exists as a criterion of a structure is that stage theorists do not see "time spent" within a given stage as a key property of a stage or of development in general. Although developmental stage theorists typically do not pay a great deal of attention to the topic of individual differences in development, one way (of the two) in which people are held to differ is in their rate of development through stages. This variation in rate implies, then, that the relative duration of the existence of a stage-specific structure is largely irrelevant in defining a stage.

Further controversy about how to define a stage is seen when the last two statements Flavell (1971) offered in regard to his view of the properties of a stage of development are considered. The third feature of a stage Flavell noted is that, as soon as a stage is said to exist, this existence means that any given item involved in that stage functions at its "peak level" of efficiency—that is, it shows an "adult-level" state of proficiency. Flavell noted that this proficiency means, for example, that as soon as a child could perform the mathematical operation of multiplication in respect to a given set of objects, then the child "was capable of performing this particular concrete operation on all sets of classes and in all the task settings that he would ever be capable of" (Flavell, 1971, p. 423). Moreover, the fourth statement Flavell made in regard to his conception of stages is that all the items involved in a given stage make this abrupt transition—from not being present or functional to being present and immediately functional at an adult level—simultaneously; that is, Flavell said that a fourth feature of stages is that all items involved in a stage become linked to it as soon as a person enters that stage.

Obviously Flavell's last two statements (1971) about what he believed to be a prototypical conception of stage are ideas that bring the issue of stage transition into the definition of stage per se. That is, Flavell (1971) noted that his last two statements assert that a person cannot be in a stage in a partial, ambiguous, or qualified way, "either in the sense of having only a rudimentary command of some given operation (third assertion) or in the sense of possessing only some of those operations at a given time (fourth assertion)" (p. 423). Although Flavell was clear that he proposed his statements in an admittedly overdrawn fashion (particularly in respect to the third and fourth statements), it is, nevertheless, the case that many developmental scientists have subscribed to such a rather strict view of stage (e.g., Gibson, 1969; Pinard & Laurendeau, 1969). As such, issues of transitions, or developments, between and within stages are issues that must be dealt with in attempting to define a stage.

The third statement that Flavell (1971) forwarded may be understood as a concern with the issue of "abruptness," that is, in this context the term means that "the development of individual stage-specific items is characteristically abrupt rather than gradual; that is, there is a zero-order transition period between the initial appearance of each item and its state of functional maturity" (Flavell, 1971, p. 425). In turn, the fourth statement that Flavell (1971) forwarded may be understood as a concern with "concurrence"; that is, "The various items which define a given stage develop concurrently (i.e., in synchrony with one another)" (Flavell, 1971, p. 435). I consider these two issues separately.

The Issue of "Abruptness": What Is the Nature of Stage Transition?

What happens to people as they progress through the various stages within a particular sequence? Specifically, what happens to the qualitatively distinct characteristics of a first stage when the person passes into a qualitatively different second stage?

Flavell (1971) noted that there are several ways of answering such a question. One may envision, or formulate models of, or types of, transitions from one stage to the next. These models may vary along a dimension anchored, at one extreme, by complete abruptness of change and, at the other, by complete gradualness of change (an extreme wherein, if it existed for a given theory, the idea of stage—as a novel period of life, one having structures special only to it—would probably lose all meaning). However, whereas a model of stage transition located at the extreme-gradualism end of the abruptness–gradualism dimension would, in effect, be a non-stage model of development, a model of stage transition located at the extreme-abruptness end of this dimension would be a non-developmental model of stage.

Such an extreme-abruptness model of development is depicted in Figure 5.1a, an illustration adapted from Flavell (1971). Flavell explained why the extreme-abruptness model illustrated in Figure 5.1a is, in actuality, a non-developmental and, indeed, a quite static model of stage. Flavell (1971) noted that, in this model, a person is characterized as being "in" a particular stage of development because and just so long as, he or she continues to behave in some particular fashion; developmental changes in behavior are largely relegated to the:

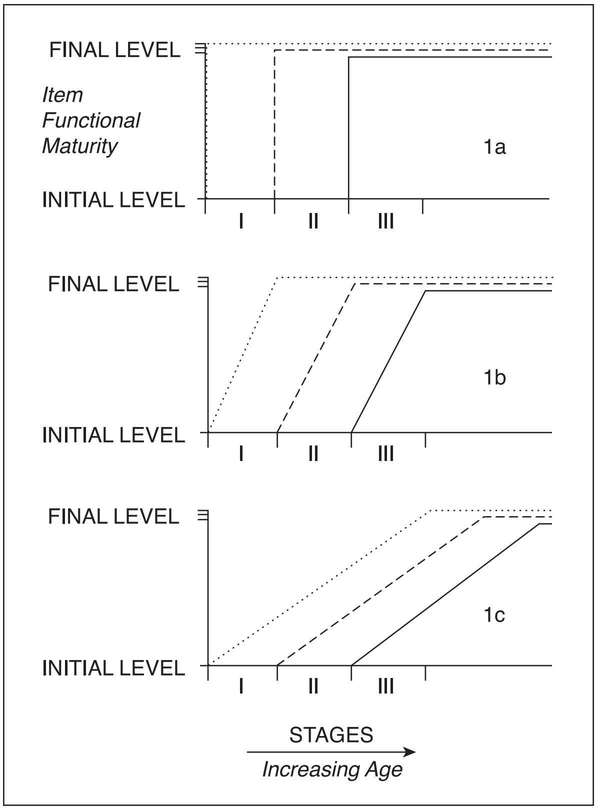


Figure 5.1 Three models of stage transitions. The developmental course of individual stage-specific items varies in relation to the model's location along a dimension of "abruptness–gradualism" of transition.

Source: Adapted from Flavell (1971, p. 426).

> "period of transition" from one stage to the next. If these "periods of transition" are taken to be of essentially null duration . . . the view that stages emerge abruptly rather than gradually, leads logically to the rather paradoxical conclusion that the individual spends virtually all of his childhood years "being" rather than "becoming" . . . the termination of any stage is defined not by the cessation of developmental change in the stage-specific item (this change having both commenced and ceased at the beginning of the stage), but simply by the abrupt emergence of the succeeding stage.
>
> (pp. 426–427)

Flavell (1971) went on to note that this model:

> has much to commend it on formal grounds. It lends a meaning to "stage" that is conceptually clear, theoretically strong, operationally useful, and quite congruent with the ordinary language meaning of that term. Unfortunately, that developing system we call the child just does not seem to conform to it [therefore this model] can immediately be ruled out of contention.
>
> (p. 428)

I agree that this model of stage is out of contention. No major developmental stage theorist takes such an abrupt view of stage transition—although it is the case that such abruptness is, as noted, part of the "stereotyped" view of stages (Flavell, 1971) and that some critics of stage theory (e.g., Bandura, 1977; Mischel & Mischel, 1976) have attributed such a conception to stage theories. For example, Davison and colleagues (1980), in discussing the stage concept in theories of cognitive development, noted:

> Such critics have assumed that stage theories imply that there will be a patterning to a subject's stage score. All of them will be zero except one, the stage at which the subject reasons. Actually the stage theorists . . . do not say that people reason at only one stage. Their theories are more complicated than is assumed by the critics. These more complicated stage theories, however, can imply that there will be a patterning to subjects' stage scores, but not the simple patterning that would occur if subjects reason at only one stage.
>
> (pp. 121–122)

The patterning to which Davison and colleagues (1980) referred is explicitly recognized by stage theorists, for example in their use of concepts such as "stage mixture" (Turiel, 1969, 1974)—a notion indicating that people function simultaneously at several different stages. I return later to this notion. Here, however, I should note that in the extant major developmental stage theories, a model of abrupt stage progression such as that in the first model of Figure 5.1 is not used; stage progression is never held to be an all-or-none event. That is, people do not progress from one stage to another instantaneously, or even overnight. It is not the case that one day a person goes to sleep in Stage 1 and the next day awakens in qualitatively different Stage 2. Development is not held to be a series of qualitative leaps, of saltatory, step-like functions. Rather, transitions from one stage to the next are gradual; they take place across time.

For example, one way of determining if a person is at a particular stage in development is to see if the person shows behaviors consistent with what would be expected from knowledge of a specific stage. If the person does not show such behaviors, a developmental scientist could say that the person has not developed into that stage. On the other hand, however, just because a person does show responses representative of a particular stage of development does not mean that the person has fully developed into that stage, that the stage is completely and comprehensively associated with his or her behavior. Because people progress from one stage to another gradually, they will, therefore, show behaviors that are representative of more than one stage at the same time (e.g., Turiel, 1969). In other words, because stage progression is not an all-or-none process, but rather it is a process that takes place gradually across ontogeny, developmental scientists would expect a person to show behaviors representative of more than one stage of development at the same time.

How, then, may a development scientist determine what stage a person is in? Clearly, one behavior or even a few would not be a sufficient sample to allow a development scientist to determine unequivocally a person's representative stage of development. Rather, it is necessary to get a large sample of the person's behaviors. Once the scientist knows what behaviors are representative of specific stages of development, he or she will have to observe many instances of the person's behavior. Only then can the developmental scientist make a stage determination. With such data, the development scientist may know within which stage the majority of the person's behaviors fall. By determining the most frequently occurring (i.e., the modal) behavior, the scientist is determining what stage best represents the person's level of development.

Hence, whenever a development scientist says that a person is in a particular stage of development, he or she should be making the statement on the basis of the person's most frequently occurring (modal) behaviors. The scientist would not say that a person at a particular stage of development is functioning *only* at that one stage; in fact, he or she should expect quite the opposite to be the case. If development scientists are judicious enough to obtain a large sample of a person's behaviors, then they should be able to ascertain the stage that is most representative of a given individual.

In essence, then, whenever development scientists speak of a person as being at a particular stage of development, they should be making a relative, not an absolute, statement—a statement that should ideally be based on that person's modal response pattern. Developmental scientists should be stating that, relative to other stages of development, the person's modal behavior is representative of a particular stage. In other words, people may function at more than one developmental level at a time, and attributing the status of a particular stage to a person should be based on a large sample of behaviors and then a determination of the person's modal behaviors. However, if people also simultaneously possess attributes of more than one stage as a consequence of less than completely abrupt transitions between stages, then developmental scientists need to inquire into the form that may be taken by these more gradual stage changes. Thus, it is useful to refer to the other models illustrated in Figure 5.1.

Figure 5.1b represents a model of stage transition wherein the functional maturity of a stage-specific item increases gradually throughout the stage. Development of this item continues until the very end of the stage, when full maturity is reached. However, at the endpoint of the stage, the items specific to the next stage are beginning their development. Therefore, Flavell (1971) noted that, in this model, the endpoint of a stage is defined by the completion of development of its own items and by the initiation of development of the items of the next stage. Thus, in this model a stage is not a static state of being. Rather it is a state of constant "becoming." The stage's own items are becoming more developed (i.e., functionally mature) throughout the stage and, while this achievement is being attained, the stage is also a period of preparation for the development of the items of the next stage.

This view of stage transition is found in Piaget's (1955) writings. As translated by Flavell (1971), Piaget indicated that "a stage thus comprises both a level of preparation, on the one hand, and of achievement, on the other" (Piaget, 1955, p. 35). Flavell (1971, p. 427) himself noted that "a stage here is not a state but a process—it is itself the 'period of transition.'"

However, models can be formulated that are even more extreme in their emphasis on the gradualness of development. One such model, formulated by Flavell (1971), is represented in Figure 5.1c. In this model, a stage's items do not reach complete functional maturity within the stage with which they are modally associated or, in other words, within the stage within which the major proportion of their development occurs. In this third model, an item's development can continue into subsequent stages. Thus, a feature of development present in this model, but not in the other two suggested by Flavell (1971), is that items from two different stages can be developing at the same time. An item from a former stage can be completing its development in a subsequent stage while, at the same time, items from that subsequent stage can also be developing.

Flavell (1971) observed that because the three models illustrated in Figure 5.1 lie on a continuum of abruptness–gradualness, it is possible to formulate models representative of other points along this continuum. A general implication of this observation is that there may be several different ways to conceptualize the characteristics of stage transition.

Indeed, scholars other than Flavell (1971) have formulated different sets of models, or schemes, of stage transition (e.g., Emmerich, 1968; Turiel, 1969; van den Daele, 1969, 1974; Wohlwill, 1973). Despite starting from perhaps different conceptual bases, because these scholars deal with the same issues addressed by Flavell (1971), their schemes of transition are often substantially compatible.

To illustrate another approach to the topic of the ways in which stage transitions may occur, and to indicate this compatibility among scholars, the views of Walter Emmerich (1968) are useful to consider. Emmerich is a scholar whose work clarified issues pertinent not only to the stage approach but also to the differential and the idiographic (ipsative) approaches.

Emmerich claimed that one of three things may happen to the characteristics (or "items," in Flavell's, 1971, terms) of a previous stage when a person develops into the next stage. He pointed out that the first thing that could happen when a person completes a transition from one stage of development into the next is that the characteristics of the first stage become completely displaced. This possibility is the most extreme view of what may happen when transition from one stage to the next is complete. This component of Emmerich's first alternative is compatible with the outcome of abrupt change depicted in Flavell's (1971) first model. That is, both views hold that when transition is complete, the person will be completely newly organized and the characteristics of the previous stage will be lost. However, there is no requirement in Emmerich's first alternative that the change from Stage 1 to 2 be totally abrupt. That is, even this radical transition may take place gradually; accordingly, even in this case the person will show evidence of characteristics of two developmental stages while the transition between them is still occurring.

In the second type of transition, the later stage becomes the dominant level of functioning, but the behavioral characteristics of the previous stage are still seen. This possibility is consonant with features of the second and third models illustrated by Flavell (1971). However, this second alternative suggested by Emmerich (1968) places greater emphasis on stage development as a modal phenomenon (cf. Turiel, 1969, and the notion of stage mixture). This alternative, then, stresses the notion that current stages are dominant in that behaviors representative of that stage are most frequent. However, although they do occur at a lower frequency than the modal behaviors, the behavioral characteristics of earlier stages are not lost. In fact, it is sometimes held that under some circumstances the lower-frequency behaviors can, for a time, become dominant in frequency (Emmerich, 1968, p. 674).

The third possibility is similar to the second. Here, however, when the new stage has fully emerged, the behavioral characteristics of the earlier stage do not typically occur. That is, the characteristics of the new stage will be the only characteristics that are typically seen. The characteristics of the earlier stage lie dormant, or are latent, and are not typically seen. In specific special circumstances, however, the earlier characteristics may emerge (Emmerich, 1968, p. 674).

David Henry Feldman (e.g., 2000, 2007) is another scholar who has advanced understanding of the character of stage transitions, writing about transitions both within stages and between stages. He presented his ideas by focusing on cognitive developmental stages and by contrasting his views with the approach to stage development taken by Piaget (e.g., 1950, 1970). For instance, Feldman (2000) noted that Piaget's equilibration model:

> or something very much like equilibration is involved in the extension, adjustment, elaboration and transformation of structures . . . it is not at all clear how such processes could account for the appearance of a set of structures as broad, interconnected, qualitatively advanced, and dramatically improved as is true of each of the stages succeeding Sensorimotor behavior.
>
> (p. 6)

Thus, Feldman (2000) identified a key problem with the emergence of novelty within Piaget's theory: the "miraculous transition" problem. That is, there is no adequate means through which to explain the emergence of the integrated, qualitative features that characterize the structures as a whole of a given stage. Furthermore, Feldman (2000) noted that the miraculous transition problem between stages is compounded by a lack of clarity about within-stage

David Henry Feldman

changes, about how progressions within a stage lead the individual through a stage and into a succeeding one. In addition, Feldman believes that the depiction of the characteristics of within-stage functioning are too general to account for, or even recognize, the presence of individual differences. In this regard, Feldman (2000) noted that:

> Piaget's theory is of course notorious for being oblivious to individual differences . . . In Piaget's rarified theoretical space, there exists only "epistemic subjects," minds disembodied from the rough and tumble of day to day existence, unmarked and uninfluenced by ambient variations in experience. The developing mind was supposed to make its way ineluctably toward mature thought regardless of gender, cultural context, historical period, nutritional input, training, or even biological variations unless they proved to be very extreme indeed (e.g., Down syndrome).
>
> (pp. 7–8)

Given, then, the problems of within-stage developmental change and the lack of sensitivity to individual differences in the stages described by Piaget, Feldman (2000) concluded that:

> Only the sensori-motor period has a carefully delineated within-stage sequence of six levels from the innate reflexes of looking, listening, grasping and sucking to the beginnings of symbolic thought that appear near the end of the second year of life [p. 10] . . . The stages are simply too broad and are intended to cover too large an age span . . . to be plausible in the absence of more fine grained internal roadmaps through them. Indeed, the persistently difficult problems of structures as a whole and where they come from (the miraculous transition problem) has been exacerbated by the scale of the stages and the fact that they seem to appear out of nowhere in most accounts.
>
> (p. 19)

In turn, Feldman (2000) provided an alternative to the Piagetian approach to stage transition. He drew on RDS-based concepts (e.g., Ford & Lerner, 1992; Thelen & Smith, 1998). For instance, Feldman (2000) proposed a concept of situated activity, wherein the relation of the activity of the person and the activity of the context becomes the focus of cognitive functioning, as compared to a focus on within-person symbolic processing. Similarly, Feldman (2000) used the concept of recursive sequences (i.e., "a repeating sequence that can be found in more than one place, a kind of loop where the last event in the previous set becomes the first event in the succeeding set, often with a parameter or parameters shifting as the pattern repeats itself over and over again"; Feldman, 2000, p. 16) to depict the changes that occur within the system as a consequence of situated activity.

Feldman (2000) noted, then, that his approach to reconceptualizing Piagetian stages reoriented stage theory more generally from an individualistic, within-the-person formulation to a relational and integrative perspective, one that overcame the "split" between internal and external reality (cf. Overton, 1998). Accordingly, Feldman (2000) pointed out that:

> Instead of focusing on the processing of information where action is inside the head and where there is a clear demarcation between what is inside and what is outside the mind of the epistemic subject, the situated approach focuses on the ontology of its interactions. The situated paradigm does not decompose the system under study by function but rather by activity. It uses the activity patterns as the categories under study.

> The situationalist approach is called "enactivism" because it studies the subject as enacting activity patterns that have been differentiated through the history of structural couplings between the subject and its environment.
>
> (p. 26)

As such, Feldman (2000) conceives of stage development as being propelled through the integration between the actions of the individual and the situational, or contextual, levels of the relational developmental system.

Thus, through these individual↔context relations stages are "constructed," that is, stages are dynamic structures that arise within the holistic, integrated, and autopoietic relational developmental system. That is, Feldman (2000) noted that:

> The main recursive pattern involves dividing each of the four stages roughly into two halves. An "active construction phase" is followed by an "active extension and application phase" for each of the Piagetian stages. The turning point from construction to application is marked by a "taking of consciousness" process, indicating that the system is generally complete and available to the child, who in turn is aware of the added power of the new system.
>
> (p. 31)

Accordingly,

> The basic approach to revising the stages of Piaget is to set them as a sequence of recurring efforts to construct overall systems for understanding the world, punctuated by achieving a satisfactory system at about the halfway point of each stage, a "taking of consciousness" recognizing that the system is fully operational, a period devoted to extending and applying the system as widely as possible, followed by increasingly confronting the system's limitations during the latter part of the stage, and finally reaching the point where a new system is apprehended as an acceptable alternative to the prevailing system. Then, starting the basic construction process begins again to build a new, more advanced system; this is where the last phase of the current stage and the first phase of the succeeding stage are simultaneously occurring.
>
> (Feldman, 2000, p. 31)

In sum, scholars such as Flavell (1971), Emmerich (1968), and Feldman (2000), among others (e.g., Turiel, 1969, 1974; van den Daele, 1969, 1974; Wohlwill, 1973), suggested several types of transitions that may occur between and within the stages proposed within different developmental stage theories. Different stage theories may opt for any one of these alternatives. Of course, the difficulty for the researcher who wants to test these different alternatives lies in measuring the differences that each alternative predicts. It would be difficult to discriminate among the different types of transitions proposed in different analyses of stages because, in any event, all the transitions take place gradually. Hence, by the time a given stage has almost completely displaced a previous stage as a person's dominant level of functioning, another stage may be beginning to displace this now-dominant stage.

Using his concept of *stage mixture*, Elliot Turiel (e.g., 1969) has explained that stage development is very complex, and it is most difficult to ascertain which model or scheme of stage transition best fits the data (i.e., best characterizes development). However, this very complexity is the major point of the present discussion. Because of the gradual nature of stage transition, a person functions at more than one qualitatively different stage at the same time. Thus, stage mixture is an essential component of any adequate stage theory of development and is a key feature of an appropriate conceptualization of a developmental stage.

I have noted, however, that at least one other concept—that of concurrence—needs to be evaluated in respect to its role as a feature of the definition of stage. The concept of abruptness pertains to the issue of the development—the transitions—(largely) between stages. In turn, the concept of concurrence pertains to the issue of development within a stage.

The Issue of "Concurrence:" Is There Synchrony in the Development of the Items within a Stage?

Is the time course of the development of the items that define a stage common across all these items? "Being in" a stage means possessing specific stage-specific attributes. But does this idea mean, too, that all items begin and end their development at the same time? These questions are involved in considering the concept of concurrence.

As already indicated by my discussion of transitions between stages—which, inevitably, raised issues pertinent to what is occurring within stages (e.g., Emmerich, 1968; Feldman, 2000; Flavell, 1971)—the best answer to these questions seems to be "no." Time differences are typically, indeed almost invariably, found in the attainment of the different attributes (or items) that are specific to a stage (Flavell, 1971). In fact, such lacks of concurrence are quite specifically included in the ideas of some stage theorists. For instance, consider Piaget's (1950, 1970) notions of vertical and horizontal *décalage* (Piaget, 1950, 1970). *Vertical décalage* involves enacting a specific task using approaches that reflect increasingly more abstract approaches. For instance, as described by Feldman (2007), across his or her development a child may enact a seriation task (that is, a task that involves putting stimuli into some sort of series, e.g., from smaller to larger) in a manner reflecting vertical *décalage*. The child may initially approach the task using sensorimotor exploration, trial and error, a concrete plan, an abstract plan, or through the use of a theory (Feldman, 2007). In turn, *horizontal décalage* involves successive ontogenetic (time) differences in the achievement of different tasks that involve the presence of the same cognitive structures (Feldman, 2007). For instance, the point in development when a child may conserve number, mass, and volume may differ (Feldman, 2007).

Accordingly, and although the point is not held without some exceptions (e.g., Pinard & Laurendeau, 1969), I agree with Flavell (1971) that complete concurrence is not a requirement of a developmental stage theory, be it Piaget's (1950, 1970) or any other. Wohlwill (1973) appears to agree with this point as well, but adds some important qualifications, noting "That despite the undeniable fact of asynchrony, a considerable degree of order and regularity—or, to put it another way, of constraints on the forms which the interrelationships of developing elements of a structure may take—still obtains" (p. 239). In other words, despite a lack of complete concurrence, the elements or items of a stage do not develop in a completely haphazard fashion. For instance, just as one may model the nature of developments, or transitions, between stages as varying along an abruptness–gradualness continuum (Flavell, 1971), one may model the nature of developments, or concurrences, within a stage as involving differing degrees of concurrence (or synchrony). Wohlwill (1973) formulated some models representing different degrees of concurrence, and they are summarized in Table 5.1.

Thus, Wohlwill's (1973) position is akin to Flavell's (1971): A concept of stage necessarily involves relative concurrence. Absolute concurrence is neither a theoretical requirement of developmental stage theories nor is it empirically ubiquitous. Wohlwill (1973) summarized his position by noting that his:

> underlying assumption is that in certain areas of development, particularly in the cognitive realm, but not necessarily confined to it, there exist regulating mechanisms that modulate the course of the individual's development so as to ensure a degree of harmony and integration in his functioning over a variety of related behavioral dimensions. The mechanism might be thought of in part as a mediational generalization process, permitting acquisitions in one area, for example number conservation, to spread both to equivalent aspects of different concepts (e.g., conservation of length) and to different aspects of the same concept (e.g., cardinal–ordinal correspondence). The result is the formation of a broad structural network of interrelated concepts appearing, not all at once to be sure, but within a fairly narrowly delimited period, with further progress along any component concept or dimension being assumed to be deferred till the consolidation of this network—that is, the attainment of the "stage." Stage development thus provides for relative consistency of behavior, economy in the acquisition of new responses, and harmony and

Table 5.1 Degrees of within-stage concurrence or synchrony: Wohlwill's models of developmental stages, arranged in order of complexity of interrelationship among component sequences

Model	Major hypothesis	Implications for concepts of stages
IA: Synchronous progression	Changes in level for all sequences occur in synchrony	Structural network tying together ordered sequences of responses at equivalent levels, with developmental progression occuring in unison in all sequences, linked in one rigid system
IIA: Horizontal décalage, convergent	Changes in level occur in synchrony, with exceptions for certain sequences, taking the form of staggered progression	Structural network integrating ordered sequences of responses at equivalent levels, with sequence-specific or extraneous factors resulting in temporary lags between systems at intermediary levels
IIB: Horizontal décalage, divergent	As in IIA above	As in IIA above, except that sequence-specific or extraneous factors have cumulative effect, with progressively widening gaps between sequences
III: Reciprocal interaction	Changes in level occur in synchrony, with exceptions for certain sequences, taking the form of intersecting developmental functions	Structural network integrating ordered sequences of responses at equivalent levels, with interdependence among particular sequences resulting in temporary perturbations in developmental timetable
IV: Disequilibration-stabilization	Attainment of levels of stage consolidation occurs synchronously for all sequences, separated by intermediary levels marked by behavior oscillation; irregular relationships among sequences	Structural network representing nodes at which ordered sequences of response become functionally integrated, with developmental progression occurring in fluid fashion between these nodes

Note. From *The Study of Behavioral Development* (p. 206), by J. F. Wohlwill, 1973, New York: Academic Press. Copyright © 1973 by Academic Press. Reprinted with permission.

> interrelatedness in the development of diverse concepts or skills across successive levels. (p. 192)

In sum, I may abstract from the controversy surrounding the conception of developmental stages a definition of a developmental stage as a component of a sequence of qualitative structural reorganizations. Between-stage developments are never completely abrupt, and there is no complete concurrence or synchrony in the within-stage development of the elements or items comprising a specific stage.

Finally, I may note that in positing the universal applicability of the stages they describe—that is, the invariant applicability of the stages to all people—stage theorists are proposing features of development that are common to all people. Thus, such theories describe the development of the *generic human being*, the general case of humanity, and accordingly the processes of development proposed by stage theorists are processes that apply to all individuals. Such processes are termed *nomothetic processes*. That is, the stage-theory approach is concerned with the postulation of general (nomothetic) processes of development—processes that apply to the generic human being. Such processes stand in contrast to idiographic processes, that is, processes that pertain to an individual (Molenaar & Nesselroade, 2015; Rose, 2016). Later in this chapter, I discuss an approach to developmental theory predicated on such processes. Here I turn to considering the role of individuality within nomothetically-oriented stage theory.

Individual Differences within Stage Theories

Despite their overriding attention to processes that characterize all people, stage theorists do recognize that people differ. However, as noted by Feldman (2000), stage theorists hold these individual differences to be relatively minimal. That is, stage theorists maintain that there are only two ways in which people may differ (Emmerich, 1968). First, as I noted earlier, people may differ in their rate of progression through the stages, in how fast they develop. It may take one individual one year and another individual two years to pass through the same stage; but all people pass through the same stages in the same order.

The second way that people may differ within developmental stage theories is in the final level of development they reach. Not all people go through all the stages—for example, because of illness, traumatic events, or death, the development of such people stops; for example, their development may become arrested or fixated (Freud, 1949) at a particular stage. The point is, however, that as far as the development of such people does proceed, it will necessarily be in accord with the specified stage progression; if these people had developed, they would have progressed through the stages in accordance with the specified sequence. In sum, according to stage theory, people may differ in how fast they develop (rate of stage progression) and in how far they develop (final level of development reached).

Relation of Concepts of Development to Stage Theories

The stage concept is used to denote an ordered, qualitative structural change in development. It should be clear that such an approach to development contrasts fundamentally with perspectives that describe developmental change as variational, quantitative, or incremental—that is, as occurring only continually and gradually, and involving only the addition of "molecular" (e.g., stimulus–response) and qualitatively invariant units to the behavioral repertoire (e.g., Bijou, 1976; Bijou & Baer, 1961). Such mechanistic approaches typically take a nurture-oriented, *empirical*-behaviorist or a nurture-oriented, *theoretical*-behaviorist approach to conceptualizing behavioral changes. In the empirical-behaviorist approach, for instance, the processes through which behaviors are shaped, and through which an increasing number of skills are acquired, are seen to involve an individual's response to contingencies in the external environment (e.g., Bijou & Baer, 1961).

A key basis of this difference between developmental stage theorists and such nurture-mechanistic theorists is, as noted in Chapter 4, that stage theories of development are predicated on a commitment to an organismic philosophy of science (see Reese & Overton, 1970). Within this tradition, the characterization of the nature of development is an idealized one, and it provides a formal conceptual metric against which observed behavioral changes are compared in order to ascertain whether a given change constitutes development (e.g., see Kaplan, 1966, 1983; Raeff, 2016).

From this organismic perspective, there are two key components of a developmental analysis. First, a stage theory must provide descriptions of the stages themselves—that is, descriptions of the structural properties of each stage in the sequence. Second, a stage theory must posit processes by which the individual progresses through these stages. However, the difference between organismic and mechanistic positions arises here. Organismic theories account for stages—and the progression through them—through the use of concepts different than those associated with explanations used in nurture-mechanistic views of development (e.g., Bijou, 1976; Brainerd, 1978, 1979). Specifically, within organismic formulations stage development is explained from the perspective of formal causality (e.g., see Berndt, 1978; Buss, 1979; Ford & Lerner, 1992; Neimark, 1978; Olson, 1978; Overton, 1998). The role of formal causality in developmental stages has, however, not often been understood or appreciated by mechanistically- and nurture-oriented developmental scientists—who either prefer to focus solely on notions of efficient causality (Bijou, 1976; Bijou & Baer, 1961) or who cannot appreciate the idea that there may be a useful notion of causality other than efficient cause (Brainerd, 1978, 1979). In short, the concern with formal cause within stage theory stands in contrast to the focus on efficient cause in nurture-mechanistic approaches to development. This difference stands as a key contrast between the two types of formulations.

Stage theorists take stands on developmental issues other than those pertinent to causality, however, and it should be clear at this point where stage theorists stand in regard to at least some of the concepts I have discussed in earlier chapters (e.g., the continuity–discontinuity issue; see too Chapter 8). By definition, stage theorists consider development to include qualitatively discontinuous phenomena. In specifying that the sequential emergence of qualitatively different levels of functioning characterizes development, stage theorists are defining development as being qualitatively discontinuous.

On the other hand, I have noted that stage theorists also recognize that there are specific processes that function invariantly across a person's life span. The equilibration model of Piaget (1950, 1970) is an exemplary case (Feldman, 2000). Hence, the postulation of such functional invariants indicates that most stage theorists recognize that development is characterized by continuity as well as discontinuity. In short, although stage theorists define development as being qualitatively discontinuous, continuous processes that exist throughout development are also recognized. Consistent with the ideas of Werner (1957), development involves a synthesis of processes making a person the same across life with processes making a person different across life (see Chapter 8).

Second, stage theorists—committed to an organismic philosophy of science—to differing extents take a relational viewpoint in respect to the nature–nurture controversy. Thus, to some extent, all stage theorists look at the relation between intrinsic (nature) and extrinsic (nurture) variables in accounting for development. However, different theorists put differing degrees of emphasis on nature and nurture factors. Thus, Piaget (1950, 1970) put greater emphasis on an interrelation between nature and nurture factors than did Freud (1949) and Erikson (1963, 1964), who placed greater emphasis on nature variables and who viewed the nurture variables as either facilitators or inhibitors of primarily intrinsic emergences (Emmerich, 1968; Kohlberg, 1963a). For example, Erikson (1959, 1963) placed a good deal of emphasis on the "maturational ground plan" that he claimed exists in all people. Thus, to Erikson, although a child must interact within society in order to develop normally, the stage emergences that characterize a child's development are primarily maturational in origin; they will emerge and exert a particular influence on development independent of the character of the child's relationships within society.

Moreover, just as stage theorists differ to some extent on the specifics of the nature–nurture relation, they also differ about the issue of critical periods. This issue raises the idea of whether there are particular periods in life when specific developments *must* occur if development is to proceed normally. The critical periods hypothesis is an issue reflecting a strong view of qualitative discontinuity. In essence, proponents of the critical periods hypothesis assert that a person must develop what he or she is supposed to develop within a specific time frame if he or she is ever to (adequately) develop it (e.g., Scott, 1962; but compare with Schneirla & Rosenblatt, 1963). It may be said that, in one sense, all stage theorists support a critical periods notion, in that in each qualitatively different stage something unique is developing. This unique development, which gives the stage its qualitative distinctiveness, is by definition supposed to be developing at this particular point. Because stage theorists define development as comprising qualitatively distinct phenomena that arise in a universal, invariant sequence, they, therefore, maintain that not all periods in development have equal potentiality for any particular development. Thus, each specific stage has its own specific emergence, which by its very existence serves to define that period in ontogeny as a stage. In this sense, each stage has its own "critical" (or, at least, defining) development.

Yet, different stage theorists have different ideas about "how critical is critical." Given the different views about stage transition that I have discussed, and, therefore, the different models of transition to which stage theorists may adhere, it is understandable that they disagree about the implications for later development of inappropriate development within a given stage. For some stage theorists, if one does not develop what one should develop in a given stage, one will never have another chance for such development (e.g., Erikson, 1959, 1968). Thus, each given stage of development is truly critical, in that, if one does not develop appropriately within a given period, irreversible unfavorable implications will be inevitable.

In addition, as might be surmised from these differences of opinion, stage theorists also differ about the source of critical periods. Just as different stage theorists place contrasting emphases on nature and nurture factors in explaining the interactive basis of development, they correspondingly place different emphases on these factors in accounting for the critical nature of different stages. Those theorists who lay greater emphasis on nature (maturational) factors in accounting for stage development similarly place greater stress on maturation as the source of the criticality of critical periods.

Conclusions

Stage theories make important contributions to the set of ideas pertinent to understanding the character of human development, in that they foreground the transformational (qualitative) changes that are part of the relational developmental system (Overton, 2015a). Although, at this writing, the grand developmental stage theories of Piaget, Freud, and Kolhberg have waned in the attention given to them in developmental research, in focusing the interests of developmental scientists on qualitative change, these theories continue to influence developmental science research, at least insofar as orienting scholars to the need to triangulate qualitative and quantitative approaches to inquiry in order to fully understand the range of changes involved in human development. This impact on developmental methodology is seen in the discussion in Chapter 13 about the implications of RDS-based models for developmental methodology.

In turn, stage theories offer a clear set of expectations about the character of human development. That is, all stage theorists present theories that speak to the various core conceptual issues of development. Although these theories deal with different aspects of the developing person, they have certain similarities. Whether talking about the development of cognition (Piaget, 1970), moral reasoning (Kohlberg, 1978), or psychosexual development (Freud, 1949), these theorists all hold that all people who develop pass through the stages specified in the theory in an invariant sequence. These stages represent universal sequences of development—that is, qualitatively different developmental levels through which all people must pass in the same order if they are to develop. As noted earlier, the essential ways in which people are thought to differ, from a classical stage point of view, are in the amount of time it takes them to move from one stage to the next (how fast they develop) and in the final level of development they reach (how far they eventually develop).

In turn, stage theories vary in the conceptualization of nature–nurture relations and in regard to the continuity or the discontinuity of development—although all stage theories posit that development is characterized, in part, by qualitatively different phenomena across ontogeny. In addition, stage theorists also maintain that there are continuous elements in development. Consistent with the organismic notions advanced by Heinz Werner (1957), stage theorists more or less explicitly view development as an organismic synthesis of the discontinuous and continuous variables affecting development.

As I turn now to a discussion of the differential approach to developmental theory, I will illustrate that developmental scientists who frame their ideas within this approach also adopt specific ideas about the key issues of human development.

THE DIFFERENTIAL APPROACH

The differential approach to development begins by posing what is basically an empirical question: "How in the course of development do groups of people become assorted into subgroups, subgroups which are differentiated on the basis of status and behavior attributes?" (Emmerich, 1968, p. 671). In its most basic form, the differential approach to development is primarily empirical rather than theoretical; it uses particular research methods (e.g., factor analysis, cluster analysis, latent class analysis, latent trajectory analysis, or growth mixture modeling; Jung & Wickrama, 2008; Ram & Grimm, 2009) to study differences among groups of people and individuals within these groups. Thus, the differential approach does not necessarily connote any given theoretical point of view; it can be used by people with various theoretical perspectives.

In short, then, the main focus of the differential approach within developmental science is to discover how people become sorted into subgroups over the course of their development. Subgroups are formed, or differentiated, on the basis of one of two types of attributes. The first type is status attributes. Status attributes are characteristics that place people in particular demographic categories or groups, such as those based on age, sex, race, religion, and socioeconomic status (SES). A differentiation of people into subgroups on the basis of age, sex, and race is illustrated in Figure 5.2.

Obviously, however, there is nothing really developmental about differentiating a group of people on the basis of their status attributes. A similar situation exists when people are differentiated on the basis of the second type of attribute of interest in the differential approach, behavioral attributes.

Behavioral attributes may be considered as behavioral, or psychological, dimensions. For example, behavioral attributes might involve dimensions such as:

Extraversion–introversion.
Dominance–submission.
Aggression–passivity.
High activity level–low activity level.
Independence–dependence.
Basic trust–mistrust.

A behavioral attribute is really a continuum that has opposite characteristics at either end. A differential researcher using the term behavioral attribute, then, is referring to behavioral or psychological attributes conceptualized along a continuum. For instance, behavioral attributes such as independence–dependence or high activity level–low activity level are seen as attributes running along a continuum, and people grouped toward one end of these continua might be termed independent or high-active, whereas people grouped toward the other end of these continua might be termed dependent or low-active, respectively.

How does the study of development enter into the differential approach? The goal of a developmental scientist using the differential approach for the study of development would be to discover the subgroups into which people become assorted *across ontogeny* on the basis of both their behavioral and status attributes. The differential developmental researcher would choose some behavioral attributes (e.g., aggression–passivity and independence–dependence), as well as some selected status attributes (e.g., age groups—5-year-olds and 10-year-olds—and gender groups), for study and then try to discover how people in these groups become differentiated in the course of development.

For instance, the researcher would ask questions to see whether the 5-year-old boys as a subgroup

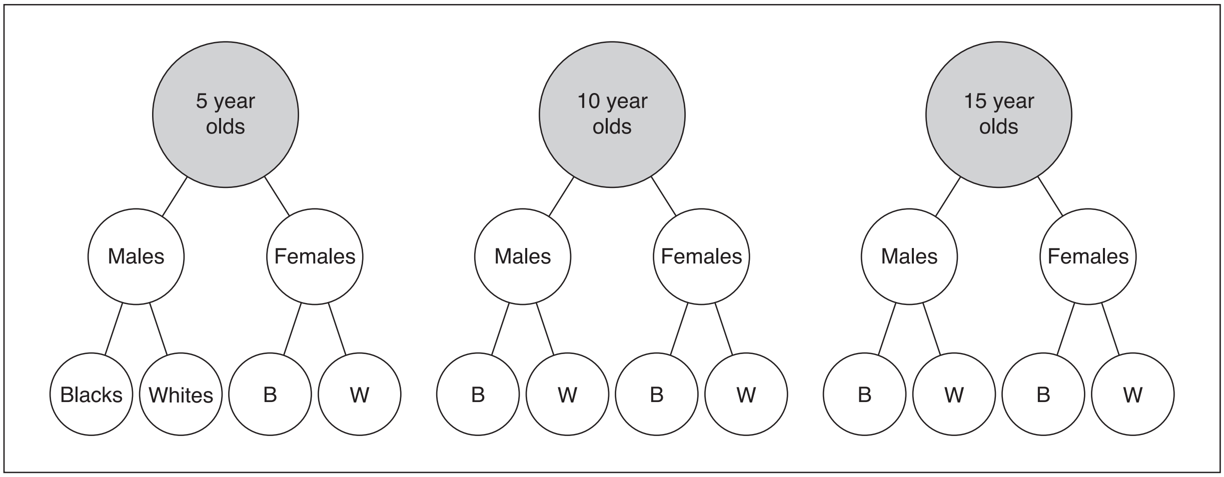


Figure 5.2 A group of people differentiated into subgroups on the basis of the status attributes of age, sex, and race.

are located at different points along the aggression–passivity and the independence–dependence continua than are the 5-year-old girls. The researcher would also ask these same questions of the 10-year-old male and female subgroups. Thus, in relation to the status attributes of age and gender, the researcher would be able to determine whether these people form subgroups located at different points along the behavioral dimensions. The researcher would be able to see, for instance, if 5-year-old girls as a subgroup are more or less aggressive than 5-year-old boys; for that matter, the researcher would be able to see how each subgroup compares with every other subgroup in terms of relative location along each of the bipolar dimensions studied.

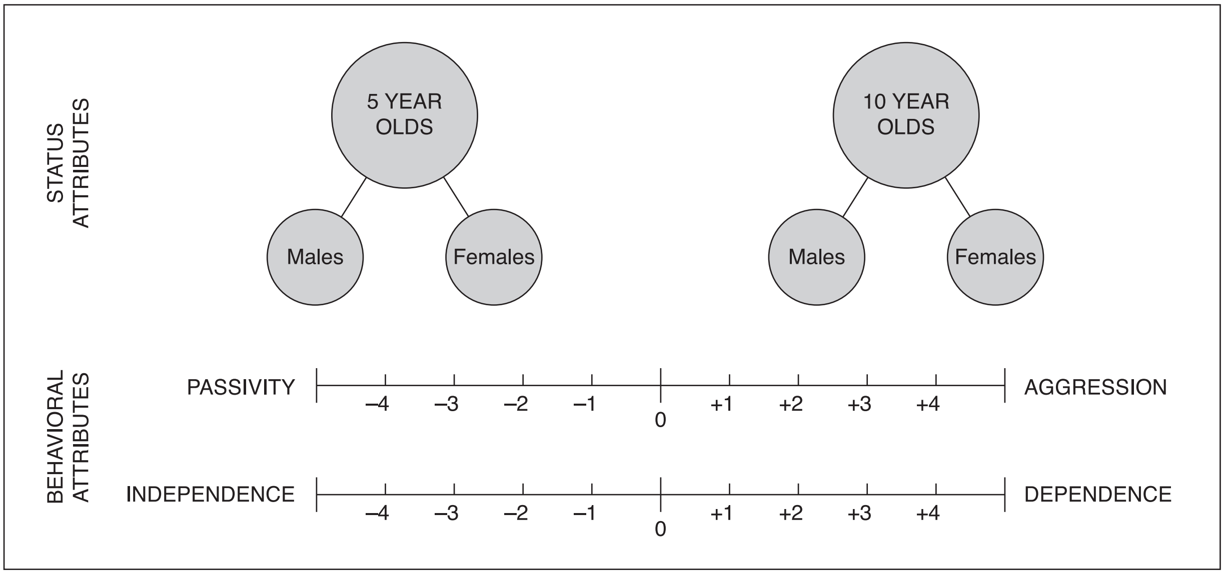


Figure 5.3a Design of a differential study of the relation of two status attributes to two behavioral attributes.

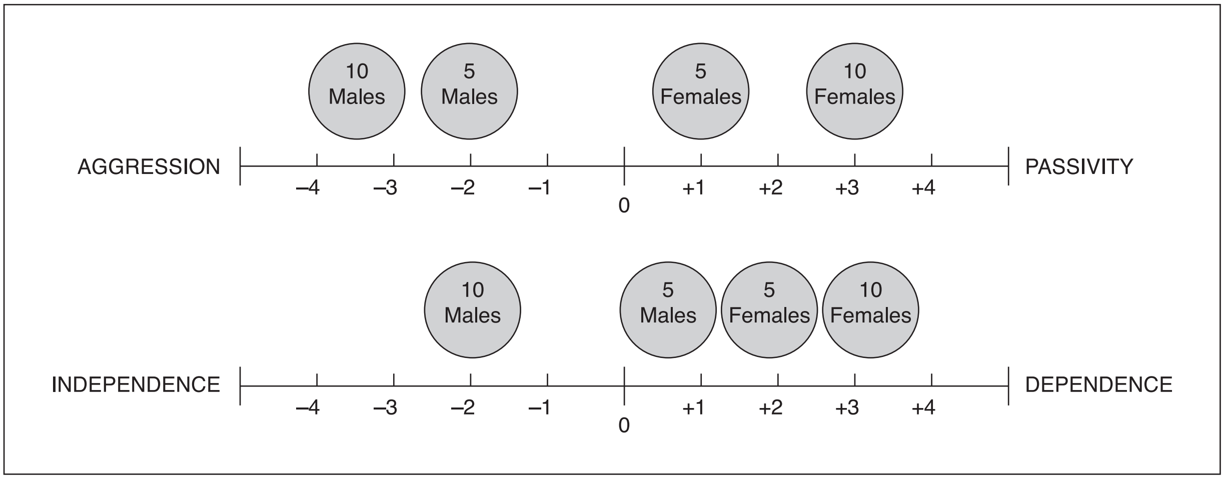


Figure 5.3b Some imaginary findings of the study illustrated in Figure 5.3a.

The design of such an inquiry is illustrated in Figures 5.3(a) and 5.3(b); some imaginary results are depicted in order to illustrate the aforementioned points. In this figure, the four subgroups that are depicted are differentiated on the basis of status attributes and are also differentiated on the basis of their location along the behavioral dimensions. That is, the subgroups occupy different spaces on these dimensions.

Individual Differences within the Differential Approach

The differential approach is primarily concerned with groups—or better, with subgroups—of people. Accordingly, in attempting to ascertain how such subgroups become differentiated with development, the differential approach is concerned with discovering nomothetic processes, albeit processes that, although pertinent to a group of people, may not apply to *all* people. That is, those scientists taking the differential approach do not necessary posit universal group processes of development, and they are thus different from the nomothetically-oriented stage theorists. Yet, differential researchers are concerned with general processes insofar as they are concerned with ascertaining the variables that predict how groups are differentiated into subgroups over the course of development.

However, differential researchers are more interested than are stage theorists in ascertaining the dimensions of individual differences in development. Consistent with how they conceptualize subgroup differences, they define individual differences in regard to people's different locations along various dimensions. Just as subgroups have different locations along each of these dimensions, so too do individuals. In turn, each individual may also have his or her own location in multidimensional space. This situation is illustrated in Figure 5.4 for two individuals, each of whom takes up a different space along each of the four dimensions shown. In sum, within the differential approach individuality is defined as one's location in multidimensional space (Emmerich, 1968, p. 678).

The Study of Development within the Differential Approach

Although the differential approach can be used simply as an empirical approach within which to

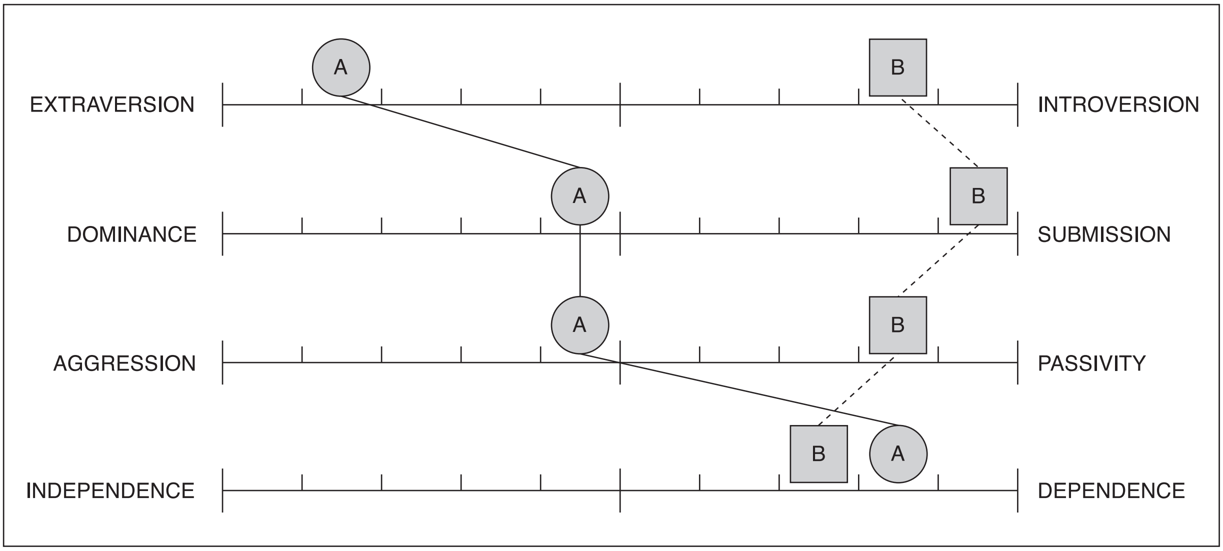
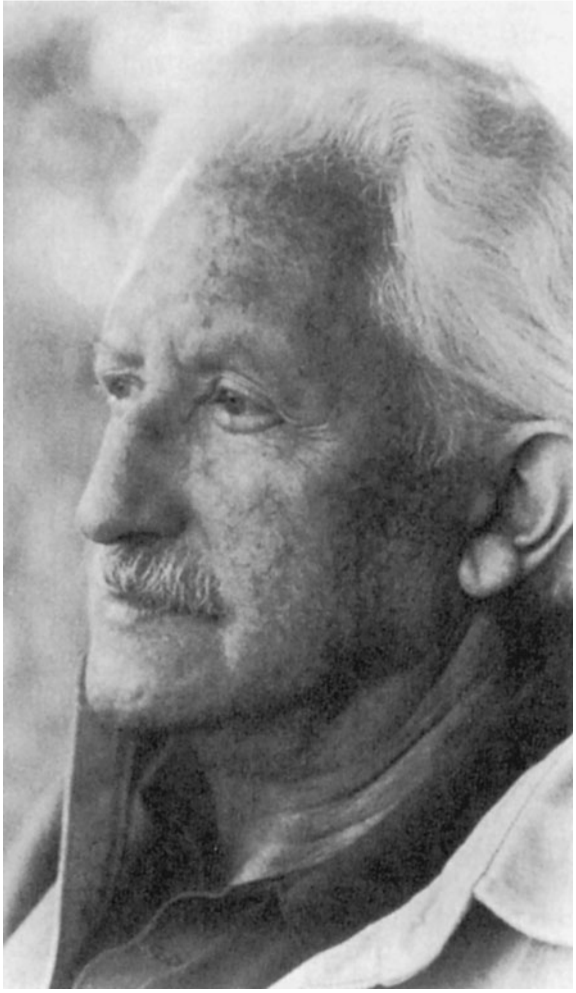


Figure 5.4 Individuality within the differential approach. Individuals A and B have different locations on each of the four dimensions.

consider development, it can also be combined with specific theoretical formulations. For example, Erik H. Erikson, primarily a stage theorist, employed a differential formulation within each of his "eight stages of man" (Erikson, 1959, 1963). As shown in Table 5.2, within each of the eight stages of development in this theory, there is what Erikson (1959) conceptualized as a core (or nuclear) emotional crisis. These crises are conceived of as differential dimensions. Similarly, just as stage theorists may use the differential approach within their qualitatively discontinuous theoretical point of view, other theorists may use differential ideas within theoretical approaches that stress continuity throughout development (e.g., Cattell, 1957). The point here is that the differential approach does not constitute a perspective mutually exclusive from other approaches.

In regard to the empirical use of the differential approach, a researcher may have no previously formulated ideas about whether development is characterized by, for instance, developmental stages and/or by continuity or discontinuity. The researcher may thus adopt the differential approach in order to see which of these concepts best describes the development of specific subgroups of people.

For example, do the same groups of behavioral attributes characterize girls and women when they are 5, 10, 15, and 20 years of age (e.g., Block, 1971)? Alternatively, do the dimensions that must be used to group girls and women of different ages vary over the course of development? Such questions are addressed by various statistical procedures such as those previously mentioned. For instance, growth



Erik H. Erikson

Table 5.2 Erikson's stages

	Bipolar emotional crisis		
Psychosocial stage	A sense of	Versus	A sense of
1. Oral-sensory	basic trust	______________	mistrust
2. Anal-musculature	autonomy	______________	shame, doubt
3. Genital-locomotor	initiative	______________	guilt
4. Latency	industry	______________	inferiority
5. Puberty and adolescence	identity	______________	role confusion
6. Young adulthood	intimacy	______________	isolation
7. Adulthood	generativity	______________	stagnation
8. Maturity	ego integrity	______________	despair

mixture modeling (GMM) procedures (e.g., Ram & Grimm, 2009) might be used to discern whether the number of dimensions that differentiated the groups stayed the same or changed, and whether the group of behavioral attributes that related to each other at Time 1 in development remained in the same relation at Time 2. It is beyond the purposes of this discussion to elucidate the details of statistical methods such as GMM. However, it is useful to summarize the value for developmental scientists of such tools by noting that:

> Growth curve modeling has provided a set of tools that are useful for modeling within-person change and between-person differences in change . . . Conventional growth modeling applications usually assume that the sample is drawn from a single population characterized by a single set of parameters (e.g., means, variances, covariances). Substantively, though, we are often interested in and deal with samples from multiple populations (e.g., we collect data from males and females, adults with pre-clinical dementia, and adults without any signs of dementia). Simultaneous modeling of change for multiple observed populations can be accommodated using multiple group growth models, wherein parameters describing growth patterns are examined to determine whether they are invariant over group (i.e., sub-sample). The multiple-group framework allows for a description of how (and possible reasons why) the groups differ in their prototypical pattern of change through formal statistical comparisons. Application of multiple-group growth models requires a priori knowledge of individuals' group membership. In contrast, growth mixture modeling (GMM) is a method for identifying multiple unobserved subpopulations, describing longitudinal change within each unobserved sub-population, and examining differences in change among unobserved sub-populations.
>
> (Ram & Grimm, 2009, p. 565)

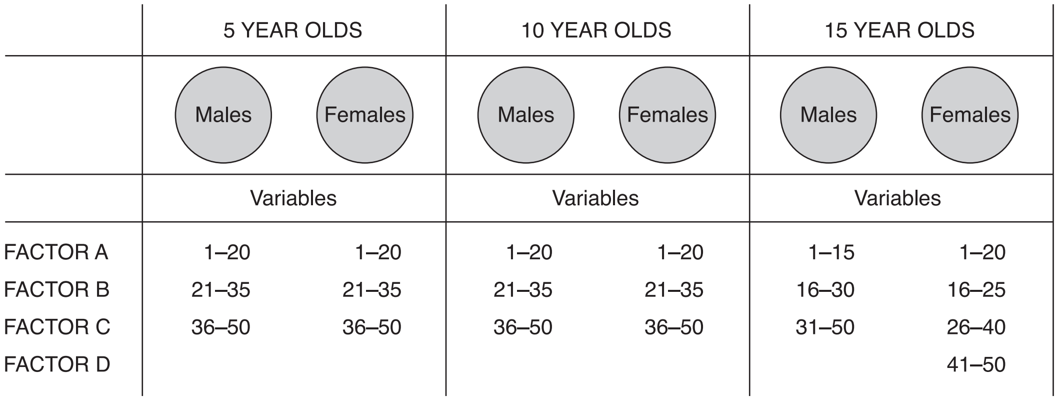
Continuity–Discontinuity

When looking at results across age levels, the differential researcher will be primarily concerned with whether the subgroup differentiations found at earlier age levels (e.g., with 5-year-olds) remain the same or change at older age levels. If the same variables seem to relate to each other in the same way at all age levels, then continuity is in evidence. If, however, differences from earlier patterns are found, then there is evidence for discontinuity. Specifically, differential researchers using, for instance, the method of factor analysis may find continuity if, for example, the same number of factors, comprised by the same variables, exist in the younger and older subgroups (Baltes & Nesselroade, 1973). Alternatively, discontinuity may be discovered if, for example, a different number of factors exist within the older subgroups. In addition, discontinuity may be found even if the same factors exist but different behavioral attributes comprise the factors. That is, dimensions not included in earlier age-level factors may be related to older age-level factors or vice versa.

In Figure 5.5, I present an imaginary example of such differential research, illustrating the discovery of both continuity and discontinuity using this methodological approach. Continuity exists between both the 5-year-old male and female subgroups and both the 10-year-old male and female subgroups. The same number of factors exist in each subgroup and, in addition, the variables comprised by each factor remain the same. However, discontinuity exists between the 10- and 15-year-old subgroups. With the males, the same number of factors still exist at both age levels, but the meaning of the factors is different because different variables make up the factors of the 15-year-old males as compared to the 10-year-old males. With the females, discontinuity also exists. Here, however, the reason is primarily the emergence of a new factor among the 15-year-old females.

Stability–Instability

In addition to being able to determine whether the same variables account for differentiation through-



	5 YEAR OLDS		10 YEAR OLDS		15 YEAR OLDS	
	Males	Females	Males	Females	Males	Females
	Variables		Variables		Variables	
FACTOR A	1–20	1–20	1–20	1–20	1–15	1–20
FACTOR B	21–35	21–35	21–35	21–35	16–30	16–25
FACTOR C	36–50	36–50	36–50	36–50	31–50	26–40
FACTOR D						41–50

Figure 5.5 An example of hypothetical findings of differential research, illustrating both continuity (between 5 and 10 years of age) and discontinuity (between 10 and 15 years of age).

out development (continuity–discontinuity), the differential researcher is able to determine whether a person's rank on a variable, and on a factor within his or her subgroup, remains the same or changes. Such a determination pertains to the issue of stability–instability (see Chapter 8). If a person's position relative to a reference group remains the same across time, then stability is present. If the person's position changes, then instability is present.

Any subgroup is composed, of course, of individuals who have scores on some set of measured variables. Although these scores may be similar, it would still be possible to rank-order all of the individuals in a subgroup, from high to low. Thus, a person's rank for a variable may change with development; when such a change relative to a reference group occurs, then instability is present. If a person's rank on a variable remains the same across time, then stability is in evidence.

Emmerich (1968, pp. 676–677) pointed out that any thorough analysis of development from the differential point of view must consider the continuity–discontinuity and stability–instability issues at the same time, and that any combination of continuity–discontinuity and stability–instability may occur. In reference specifically to the differential approach, Emmerich (1968, p. 677) pointed out that:

1. Continuity and stability may occur when the factors (and the variables within them) remain the same for subgroups from Time 1 to Time 2 and, accordingly, individuals' rankings within their respective subgroups remain unaltered.
2. Continuity and instability may occur when the factors (and the variables within them) remain the same for subgroups from Time 1 to Time 2 but, despite this consistency, individuals' rankings within their respective subgroups change.
3. Discontinuity and stability may occur when factors (and/or the variables within them) are altered for subgroups from Time 1 to Time 2 but, despite these changes, individuals are ranked in similar ways within these new subgroupings.
4. Discontinuity and instability may occur when the factors (and/or the variables within them) change for subgroups from Time 1 to Time 2 and individuals' rankings are accordingly altered.

A developmental researcher involved in differential research should address these possible interrelations in order to understand the results of a specific instance of research done with this approach. In addition, the developmental scientist should be concerned with how the differential approach pertains to key conceptual issues of development.

Relation of Concepts of Development to the Differential Approach

Researchers employing a differential approach may deal primarily with the continuity–discontinuity and stability–instability issues. However, because the differential approach may be interrelated with other theory-based approaches to understanding development, other conceptual issues may become relevant. For instance, when this approach is interrelated with stage theories of development, as in the case of Erikson's (1959, 1963) theory of psychosocial development, the stage formulation, in a sense, takes theoretical precedence. That is, when Erikson (1959) uses differential formulations within the context of his stage theory, the continuity–discontinuity of behavioral development does not remain an empirical question; rather, development is held to proceed through eight qualitatively different stages.

The interrelation by Erikson of the stage and differential approaches does not alter the substance of the differential approach. As more of an approach to the study of development, rather than a theoretical view of development, the differential approach does not maintain an a priori position relative to the continuity–discontinuity issue. Moreover, it in no way addresses the nature–nurture issue or related issues. That is, the differential approach in no way offers formulations that specify the sources of differential developmental subgroupings; rather, in its use within the context of contrasting theoretical perspectives, it can be integrated with virtually any position on a nature-oriented to a nurture-oriented conceptual continuum.

Conclusions

The differential approach to the study of behavioral development considers how people become sorted into various subgroups over the course of their development. Researchers taking such an approach are concerned with the developmental interrelations among selected status and behavioral attributes. This concern may be expressed either in primarily theoretical terms or as a primarily empirical interest. Researchers employing differential concepts as components of their theoretical views may specify how specific status attributes will be interrelated with specific behavioral attributes. Such theoretical attempts may first posit particular status attributes and then specify, along with each status attribute, characteristics that are thought of in behavioral-attribute terms, for example, characteristics thought of as dimensions (such as activity–passivity).

Differential researchers whose orientation is primarily empirical do not a priori specify the exact interrelation of these attributes. They certainly may have theoretical orientations that affect their choices of particular status and behavioral attributes for study, and they certainly may make predictions about how status and behavioral attributes will interrelate. However, they are primarily concerned with empirically discovering or verifying these interrelations. Thus, this approach attempts to ascertain empirically how people become differentiated into subgroups over the course of their development.

Finally, although proponents of the differential approach pay greater attention to individual differences than do proponents of classic stage theories, it is the subgroup of individuals, and not the individual per se, that is of primary interest among differential researchers. However, quite a different interest in individuality is part of the idiographic (ipsative) approach.

THE IDIOGRAPHIC (IPSATIVE) APPROACH TO DEVELOPMENT

In an idiographic approach to development, the individual, and his or her individuality, are of primary interest (Molenaar & Nesselroade, 2015; Rose, 2016). Developmental scientists taking an idiographic approach thus focus primarily on intraindividual (within-person) change. As such, the analytic approach to the individual is to compare him or her to himself or herself, respectively, across time. Such an approach involves *ipsative* analysis, that is, a comparison of a single person across two or more time points. As compared with the stage and differential approaches to developmental science, the idiographic (ipsative) approach seeks to identify what might be completely idiographic processes, or regularities, associated with an individual instead of a group.

In other words, the goal of idiographic, ipsative analyses is to identify individual processes or regularities of development if and when they exist. Those opting for such an approach might argue that the nomothetic processes of individual behavioral development, which apply only to groups and not to the individuals within them, are meaningless (Rose, 2016); they would, thus, try to ascertain the variables involved in an individual's development. If these findings could then be applied to larger groups of people (e.g., to better understand any qualifications in the application of group processes to individuals), so much the better for the science of human development (Molenaar & Nesselroade, 2015). However, if the findings of idiographic research indicated that group processes were too general to be useful for understanding the character of an individual's life course, then again so much the better for science. Here, the contribution would be, however, that scientists would not be misled by relatively vacuous general principles of human functioning (Rose, 2016).

In short, the rationale for an idiographic, ipsative analysis of development is that the variables providing the bases of human functioning may coalesce in each person in a unique way. As such, processes of behavioral and psychological functioning that apply only to groups may have no direct meaning for a given individual's functioning, although they may constrain that individual's social interpersonal behaviors; such an impact is possible if a person is evaluated unfavorably if or when he or she deviates from the average for his or her group (Rose, 2016).

Accordingly, development at the individual level must be understood, and ipsative analysis of development (e.g., through the method of P-technique factor analysis; Molenaar & Nesselroade, 2014, 2015; Nesselroade & Molenaar, 2010) considers intraindividual consistencies and changes in the development of the person (Emmerich, 1968). The approach asks whether the variables that comprise the individual remain the same or change throughout the individual's ontogeny.

It should be noted that scholars from diverse theoretical perspectives have argued for the need for an idiographic approach to, and ipsative analyses of, human behavior. For example, the need for such analyses may derive from an individual's unique genotype and genotype–environment relation (Hirsch, 1970), from the person's individual reinforcement history (Bijou, 1976; Bijou & Baer, 1961), from the person's unique interrelation of his or her temperament attributes (Chess & Thomas, 1984, 1996, 1999; Thomas & Chess, 1977, 1980), or from his or her personality organization (Allport, 1937; Block, 1971). It should be noted, however, that, although all such theorists would agree that ipsative analyses are necessary to describe an individual's functioning, not all would agree that idiographic processes need to be used to account for intraindividual uniqueness. For example, Bijou and Baer (1961) might argue that although each person would have a unique reinforcement history and would, therefore, have a unique response repertoire, the processes governing the acquisition of any of the responses (e.g., processes of associative learning or conditioning) are applicable to all organisms.

When ipsative analyses are used in developmental research, however, the scientist seeks to understand the makeup of the individual in two ways. First, an attempt is made to ascertain the specific attributes (e.g., behavioral or psychological variables) that make up the person (Emmerich, 1968) over the course of development. These attributes may be characteristics such as features of personality (e.g., dependency or aggression), temperamental styles (e.g., high activity level or low threshold of responsivity), or, in fact, any set of psychological–behavioral variables. Moreover, these attributes may be unique to the person or common among many people (e.g., personality attributes or cognitive characteristics such as language or memory). In any event, the first task of an idiographic approach is to determine what attributes comprise the individual, to discover the individual's attribute repertoire across development.

For example, a researcher may be interested in discovering a person's values. Accordingly, the researcher might discover that, at Age 15, a given person was comprised of four values (e.g., values about the health and fitness one's body, about sexuality, about education, and about religion).

However, at Age 25, an additional two values may have become a part of this person's value attribute repertoire (e.g., values about a career and about raising a family).

Second, when there is more than one attribute characterizing a person, there must be some organization of attributes (Flavell, 1971). Some attributes may be central in that they serve to organize other attributes, whereas others may be subordinate. Alternatively, the organization of attributes may exist as clusters. For instance, some attributes may be grouped together whereas others may not. By analogy, this attribute organization might be conceptualized in regard to intraindividual attribute factors. In fact, one form of factor analysis—the P-technique factor analysis method I have already noted—is aimed at identifying intraindividual factors (Nesselroade, 1983; Nesselroade & Molenaar, 2010; see too Ram & Grimm, 2015); in other words, P-technique factor analysis provides a means to conduct ipsative analysis of the structure of attributes that exists within a single person over the course of his or her development. Other methods have also been used to conduct ipsative analyses of the development of intraindividual structures, for example, the Q-sort methodology used in Block's (1971) longitudinal study of the lives of males and females from adolescence into young adulthood.

However, no matter which particular data analysis technique is used, a person may be found to have several personality attributes clustered together, and these attributes may be independent of, for example, the person's cluster of value attributes and temperamental attributes. Moreover, within a particular cluster, a specific attribute may be superordinate. Thus, the sexuality value may be superordinate to a person at a particular time in life, with all other values subordinate, at least if they are viewed in terms of the overriding importance of the sexual value. In any event, the second task of an idiographic approach is to attempt to understand a person's attribute interrelation across development, that is, how the attributes comprising the person are related to each other over the course of the person's life. To address these issues, an ipsative developmental analysis would be undertaken.

To illustrate, suppose an individual has three types of value attributes—relating to religiosity, sexuality, and economic resource attainment. Although it is possible that these same three values may comprise the person's attribute repertoire at different times in life, the values may be interrelated differently over time. For instance, at Age 17, the person's sexuality value may be most important (superordinate), with the others subordinate. This attribute interrelation may stay the same over time, but it might also change. For instance, at Age 38, the economic resources value may be superordinate and the sexuality value not as important—it may now have fallen to second-order importance; however, the religiosity value may maintain its previous intraindividual position. Still later, however, perhaps at Age 67, these same three values may still make up the person's repertoire, but once again they are interrelated differently. Thus, at this age, the person's religious value may be most important, whereas the economic resources value has fallen to second place and the sexuality value to third place.

In sum, those taking an idiographic approach to the study of development seek to discover the regularities involved in an individual's development by attempting to conduct ipsative analysis elucidating the person's attribute repertoire—those characteristics comprising the person—and attribute interrelation—the intraindividual organization of these attributes across the person's ontogeny. Thus, the idiographic problem in development is to discern intraindividual consistencies and changes in attributes and their organization over the course of an individual's development.

It is useful to illustrate how, in an ipsative analysis, one may identify how a person may change over the course of development as a function of new attributes existing in his or her repertoire. Consider the attribute repertoire depicted in Figure 5.6. Here, at Time 1 the person was comprised of seven attributes (a–g), whereas at Time 2, three new variables (h–j) are in the repertoire.

Even if the attributes in a person's repertoire remain the same in development, the person may change if the attributes are interrelated differently across time. This second type of change that can be identified by an ipsative analysis of intraindividual development is depicted in Figure 5.7. Here, although the same number of attributes exist in the person's repertoire at Times 1 and 2 in development,

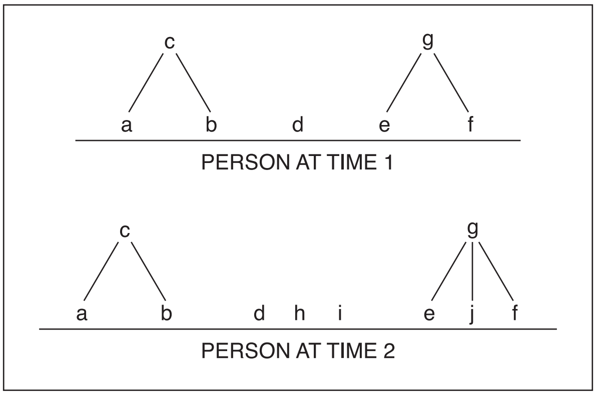


Figure 5.6 An example of ipsative change. The person's attribute repertoire changes from Times 1 to 2.

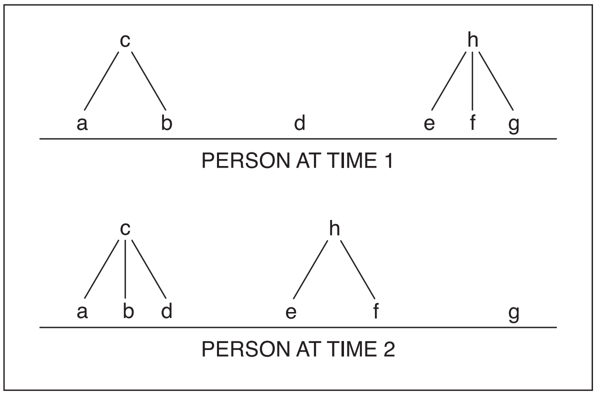


Figure 5.7 An example of ipsative change. The attributes in a person's repertoire are interrelated differently at Times 1 and 2.

the interrelation of the attributes is different at these two times. At Time 1, Attributes a, b (subordinate), and c (superordinate) cluster together, as do Attributes e, f, g, and h. Attribute d does not cluster with either of these two groups. At Time 2, however, the organization of the attributes is different. Here, we see that Attribute d now clusters along with Attributes a, b, and c, whereas Attribute g has now become independent of the attribute cluster composed of e, f, and h.

Of course, both the number of attributes in the person's repertoire and the attribute interrelation may change over the course of development. If the attribute repertoire changes, then of course the person will have changed; but even if the attribute repertoire remains the same, the person can still change through a change in the attribute interrelation. Either or both of these changes may constitute an individual's development.

Individual Differences within the Ipsative Approach

It should be clear that individual differences are the essence of the idiographic approach. Simply, the goal of this approach is to ascertain processes or regularities lying at the idiographic end and not at the nomothetic end of an idiographic–nomothetic continuum (Block, 1971). Thus, the concern in this approach is to identify processes applying to an individual's development and then, and only then, assess if it is appropriate to aggregate across individuals, and hence if regularities closer to the nomothetic end of the above-noted continuum might exist (Molenaar & Nesselroade, 2014, 2015; Rose, 2016). Accordingly, a result of an ipsative analysis may be the formulation of highly specific generalizations, ones about the course of an individual's development. Analysis begins at the level of the individual because, it is held, general processes of development may not apply equally or at all to an individual. The average score for a group may not characterize any individual in the group (Rose, 2016). Hence, within this perspective, one should first understand how the individual develops before one tries to understand how large groups of individuals develop. In short, in a nomothetic approach the analytic steps are aggregate and then analyze; however, in an idiographic approach the (ipsative) analytic steps are analyze and then (if possible) aggregate (Molenaar & Nesselroade, 2014, 2015; Rose, 2016).

In other words, those taking an idiographic approach are not denying either the validity or the necessity for studying general processes of development. Rather, they are emphasizing a different aspect of the problem of developmental analysis. They are trying to first understand the role of the individual in his or her own development (Schneirla, 1957; see too Chapter 7). Accordingly, they would suggest that particular attributes of a person may be unique, but they would not disregard the possibility that other attributes of the individual may be similar to those of other individuals. Thus, the emphasis in the idiographic (ipsative) approach is not that all people are completely different, but rather that, in order to understand all of the phenomena of development, one must deal with the particular intraindividual features of development. Indeed, Molenaar and Nesselroade (2014, 2015) have developed data analytic procedures—involving dynamic factor analysis and a tool termed the Idiographic Filter—enabling the formulation of either group differential or fully nomothetic generalizations after initial, ipsative analyses are conducted.

There are other issues that may be raised in respect to the focus of the idiographic approach to the individual and to intraindividual differences across ontogenetic points—instead of a focus on interindividual differences within time, as is more often the focus with nomothetically-oriented approaches. First, theorists differ in regard to their beliefs about the ability of idiographic processes to account for substantial proportions of the variance in development, both absolutely and especially in comparison to nomothetic processes.

A second scientific issue is that the interpretation of the individual differences described in ipsative analyses is open to debate. Do such individual differences reflect qualitatively unique, individual processes or only quantitative (and perhaps "error") variance around some more general (group or universal) process? If the former alternative of this second issue is the case, then both basic research and more applied endeavors of assessment and intervention need to focus primarily on the individual, deferring for secondary analysis any focus on the group or general processes. If the latter alternative is the case, then given the practical problems (of cost and time) of designing and implementing plans for separate assessment of every individual, it may be that the appropriate role of basic research is to remain focused on designing research to assess general processes. Similarly, if the latter is the case, the same practical problems of cost and time would suggest that those interested in applied issues should be primarily concerned with assessments and interventions aimed at the more general components of human functioning.

These issues are not settled at this writing. Nevertheless, the research of Molenaar and Nesselroade (2014, 2015), and the data analytic tools they have developed (such as the Idiographic Filter), as well as the idiographic ideas and associated ipsative analyses of other researchers (e.g., Rose et al., 2013; Velicer, Babbin, & Palumbo, 2014) document the presence of idiographic regularities in human development *and* the feasibility and effectiveness of ideographically-oriented applications of developmental science.

Developmental Changes within the Ipsative Approach

Within the ipsative approach, people may change throughout the course of their development on the basis of changes in their attribute repertoire and/or their attribute interrelation. But are such intraindividual changes systematic? Do they follow a predictable pattern or are they unique to each and every individual? In other words, are there any principles that may be used to understand the nature of the intraindividual changes constituting development?

The scholarship of Emmerich (1968, pp. 679–681) allows the answer to all these questions to be "yes." Although Emmerich pointed out that, traditionally, there was little evidence of systematic, idiographic developmental theorizing, he suggested that a principle exists that allows such conceptualization to proceed. As noted in prior chapters, Werner's (1957) orthogenetic principle (see too Raeff, 2016, and Chapter 8 for a fuller discussion) is a general, regulative principle of development that describes the course of developmental changes whenever development occurs.

The orthogenetic principle holds that, whenever development occurs, it proceeds from a state of globality and lack of differentiation to a state of differentiation, integration, and hierarchical organization (Werner, 1957). Individuals, of course, develop. Therefore, developmental scientists would expect the orthogenetic principle to hold for the intraindividual development of a person (Raeff, 2016). The principle would imply that, no matter what the specific attribute repertoire of a person may be, the developmental changes in this attribute repertoire follow a specific, systematic course. All changes in a person's intraindividual attributes would proceed in accordance with the orthogenetic principle, from more globality at earlier ontogenetic points to less globality and more differentiation and hierarchic integration at later ontogenetic points. Raeff (2016) has provided numerous examples, across diverse substantive areas of developmental science, of precisely this pattern of change. Her examples involve the emergence of object permanence in infancy, action development in early life, patterns of childrearing and socialization, agency and goal-directed behaviors, play, peer relations, self/identify development, narratives and storytelling, cultural practices, and macrosystem changes involving social/cultural institutions and economies.

Hence, even if all individuals were completely unique in the repertoires of attributes they possessed, the development of the interrelations of their attributes could still be expected to change in accord with the descriptions provided by the orthogenetic principle. Therefore, developmental scientists might expect that, as an individual develops from Times 1 to 2, his or her attribute repertoire and attribute interrelation would develop along specific, systematic lines. Specifically, based on the orthogenetic principle, some expectations about intraindividual change might be that:

1. An individual's attribute repertoire would be relatively global and undifferentiated at Time 1 in development but more differentiated at Time 2. In other words, as an individual develops, new and more differentiated attributes should emerge in his or her attribute repertoire. Thus, in regard to the attribute repertoire, the orthogenetic principle suggests that discontinuity should characterize development. Differentiated attributes should emerge from global attributes.
2. An individual's attribute interrelation would change in the direction of greater hierarchical integration with development. At Time 1, a person's attribute interrelation would be less integrated, less hierarchically organized than at Time 2. Thus, in regard to the attribute interrelation, the orthogenetic principle suggests that continuity should characterize development. The attribute interrelation should become increasingly more hierarchically organized over the course of an individual's development.

In sum, when the orthogenetic principle is applied to intraindividual development, idiographic development may be held to follow specific systematic changes. There will be discontinuous changes when the person develops from Times 1 to 2 in his or her ontogeny because the attribute repertoire will go from a state of globality to a state of differentiation. For instance, when a child is about 15 months old, she may say "doggie" when pointing to any furry, four-legged creature she encounters. However, when she is 24 months old she may say "doggie," "kitty," and "bunny" when pointing to these animals. If a developmental scientist compared these two ontogenetic points (15 months and 24 months), he or she could say that there was evidence of discontinuity in animal concepts across these two points. In addition, there will be continuous changes when the person develops from Times 1 to 2, because the attribute interrelation will become increasingly more hierarchically organized. Using the same example of the animal concepts of the young child at two points in her ontogeny, the child at 24 months might be able to state that dogs, cats, and rabbits were all animals. If so, the developmental scientist could point to this statement as evidence that the child's knowledge of these animals existed in a hierarchical structure.

Relation of Concepts of Development to the Ipsative Approach

When the idiographic (ipsative) approach is interrelated with the orthogenetic principle, the ipsative approach takes a clear position on the continuity–discontinuity issue. The orthogenetic principle implies the existence of both continuity and discontinuity in development and, accordingly, when this principle is applied to the idiographic (ipsative) approach, this approach also characterizes development as having both continuous and discontinuous components. Thus, when this interrelation is achieved, the continuity–discontinuity issue does not remain an empirical issue for those taking the idiographic (ipsative) approach but, instead, becomes a theoretical issue; those taking this approach would now maintain that development is both continuous and discontinuous in character.

The idiographic (ipsative) approach also has specific applicability to the nature–nurture issue. An essential consideration of the idiographic (ipsative) approach is the role of the processes governing the individual. This focus leads, in my view, to a concern with the contribution the individual makes to his or her own development. This point may be explained by reference to the ideas of Schneirla (1957). As will be discussed more fully in Chapter 7, Schneirla (1957) suggested that the nature↔nurture relations that provide the source of all individuals' development give each person a lawfully singular set of behavioral characteristics; this behavioral individuality provides, then, a third source of the individual's development through the establishment of circular functions and self-stimulation in ontogeny: An individually distinct organism elicits reactions from others that differ from the reactions associated with other individuals; these distinct individual↔context relations provide a source of the organism's specific experiences and, hence, its continued individual development.

Thus, idiographic-oriented researchers, in focusing on a person's individuality, can ascertain how the individual—in coaction with his or her environment—provides a source of his or her own development. Thus, such researchers would necessarily be taking a relational stance in respect to the nature–nurture controversy; and, by attempting to discover the contributions of the individual to his or her own development, they would be ascertaining important evidence bearing on this aspect of the nature–nurture issue.

I should note here that another notion involved in the idiographic approach is in accord with an emphasis on individual↔context relations. The idiographic (ipsative) approach recognizes that, although it is possible for people to have completely unique attribute repertoires, it is also possible for people to have attribute repertoires that are very similar. Accordingly, developmental scientists may identify people who have similar attribute repertoires at Time 1 in their development and study them longitudinally. Some of these people will remain similar at Time 2, whereas others will be different. Thus, by focusing on the different types of individual↔context relations these people experienced, developmental scientists may discover how specific coactions provide a source of an individual's development. Such work may be especially important in identifying why monozygotic (MZ) twins become different over the course of their respective ontogenies (Joseph, 2015; Lickliter & Honeycutt, 2015; see too Chapters 11 and 12). By discovering the processes that function to change people who were similar at Time 1 into people who either remained similar or were different at Time 2, developmental scientists may learn about the specific characteristics of individual↔context relations that enable the person to provide a third source of his or her behavioral development.

Alternatively, of course, developmental scientists could also focus on people who had different attribute repertoires and/or interrelations at Time 1 and study those people who remained different or became similar at Time 2. By assessing the individual↔context relations of these groups, developmental scientists might further discover how the characteristics of the individual in coaction with his or her environment provide a source of the individual's own development. In short, the idiographic (ipsative) approach takes a stand on the continuity–discontinuity issue and can be potentially useful in providing information about nature–nurture coactions as a source of development. As well, this approach holds great promise for elucidating the developmental course of individual↔context relations that are brought to the fore of attention in RDS-based models of human development.

Conclusions

The idiographic (ipsative) approach to human development assesses intraindividual consistencies and changes in the attribute repertoire and the attribute interrelation of a person over the course of development. As compared to the more nomothetically-oriented stage and differential approaches, the idiographic (ipsative) approach seeks to understand the processes that govern an individual's behavior; it attempts to formulate highly specific generalizations, those potentially applicable to the development of a single individual.

However, in seeking to understand the variables involved in an individual's development, those taking an idiographic (ipsative) point of view are not necessarily formulating specific processes of development applicable only to that given person (Block, 1971). Rather, these developmental scientists stress that an understanding of the individual is a necessary basis for any more general understanding of human development (Molenaar & Nesselroade, 2015; Rose, 2016). Although the field of human development must be concerned with ascertaining nomothetic (or group) processes as well as idiographic processes, those developmental scientists taking an idiographic approach suggest that the science would suffer if the former were emphasized to the exclusion of the latter. General processes of development, averages, may not apply equally (or at all) to all the individuals in a group (Rose, 2016). Hence, intraindividual processes must also be understood to get a full account of development. In other words, the contributions that an organism's own individuality makes toward its own development must be understood in order to comprehend development more fully (Schneirla, 1957).

Accordingly, a basic orientation of the idiographic (ipsative) approach is an assessment of the role of the organism's own characteristics in its own development. An organism's lawful, systematic characteristics of individuality provide an important source of that organism's own development. This idea is a key reason why those taking an idiographic (ipsative) point of view seek to assess an individual's attribute repertoire and the concomitant interrelation of this repertoire over the course of the individual's development. Although not necessarily denying the validity of other approaches to the study of human development (e.g., the stage approach), the idiographic (ipsative) approach suggests that these other orientations are incomplete because they do not pay sufficient attention to the organism's lawful (and potentially unique) characteristics of individuality and the contributions of this individuality to the organism's own development.

The idiographic (ipsative) approach lends itself, then, to an RDS-based analysis of human development, at least insofar as, following Schneirla (1957), it involves the idea that the organism's own characteristics play an active role in its own development. Yet, despite the similarities between the idiographic (ipsative) approach and other RDS-oriented positions, relatively little systematic developmental research has been conducted from an essentially idiographic (ipsative) point of view. However, as previously noted, the research base for idiographic analysis and its use in the application of developmental science is growing impressively (in quantity and quality of work) at this writing (e.g., Molenaar & Nesselroade, 2014, 2015; Rose, 2016; Rose et al., 2013; Velicer et al., 2014).

FROM THEORY TO CORE CONCEPTUAL ISSUES

The stage, differential, and idiographic (ipsative) approaches represent contrasting orientations to the study of development. Yet, all approaches forward concepts that bear on the core conceptual issues of development. These approaches—along with the other theoretical views of human development discussed in previous chapters and, as well, in subsequent ones—provide different ideas about the bases of development and about the continuity of the influence of these bases across ontogeny.

Although the presentation in both this chapter and prior ones has discussed, at least briefly, these core conceptual issues, it is useful to understand more thoroughly how these issues have been treated previously in developmental science and, as well, currently in both RDS-based and non-RDS-based theories. Accordingly, the next several chapters provide these discussions.

CHAPTER SIX

Toward Resolving the Nature–Nurture Controversy

Contributions and Implications of the Scholarship of Anne Anastasi

In June 2000, a breakthrough announcement was made about the completion of the mapping of the human genome (International Human Genome Sequencing Consortium, 2001; Venter et al., 2001). Heralded as a major scientific accomplishment on its own, the implications of such knowledge for the creation of genetically based interventions for the amelioration and eradication of disease were immediately envisioned.

However, hopes that this scientific breakthrough would result in major advances in the treatment of diverse diseases have not been realized at this writing, and efforts to identify particular genes or set of genes implicated in human behavior and development have met with even less success (Charney, 2016; Richardson, 2017; Wahlsten, 2012, 2013; see too Chapters 11 and 12).

Perhaps more complicated are the implications of identifying genes that are associated with normal variation resulting in individual differences in, for example, temperament, aggression, shyness, intelligence, or activity level. Could scientists—and should scientists—eradicate from the human genome genetic material that purportedly predisposes some people to be too aggressive or too shy or less intelligent, or not active enough? Who has the political power and/or the moral authority to make such eugenic decisions? Who will have access to those interventions? Do such questions even make sense scientifically, given the burgeoning knowledge of gene↔context relations emerging from the study of epigenetics (e.g., Lester et al., 2016; Meaney, 2010, 2014; Moore, 2015a, 2016)?

The premise of this chapter is that the complexity of the coactions, the fusions, within the integrated relational developmental system involving nature and nurture makes answers to these questions incredibly complex. In fact, it fundamentally calls into question whether the eugenics ideas associated with the assumptions that genes *directly* provide a basis for any facet of human development have any legitimacy whatsoever (Lerner, 2015b, 2015c). As I will discuss in several chapters, oversimplification of rules of how biology and environment operate in human behavior and development can lead to radically different understanding and implications for public policy (e.g., contrast Belsky, 2014, with Feldman, 2014).

From simplistic ideas about the relations between biology and context arise questions such as: “Is this particular behavioral attribute biologically determined and, therefore, not amenable to environmental interventions?” or “How much does ‘genes versus environment’ explain individual differences in a particular behavior or developmental process?” (Anastasi, 1958; Gottlieb, 1997; Hirsch, 1997). Such questions involve splitting nature from nurture (Overton, 2015a), a split that is illogical and counterfactual when the character of the relational developmental system is considered. Such splitting can lead to the misconception that genes are destiny or that genetic programming is unresponsive to the environment.

This misconception is quite broad in society, as evidenced by the information presented by the late Harvard University biologist, Ruth Hubbard, and her son, Elijah Wald, in their 1999 book, *Exploding the Gene Myth: How Genetic Information Is Produced and Manipulated by Scientists, Physicians, Employers, Insurance Companies, Educators, and Law Enforcers*. This work builds on a distinguished history of Hubbard's theoretical and empirical contributions to biological science (e.g., Hubbard, 1951, 1956, 1958, 1990, 1995; Hubbard & Lewontin, 1996; Hubbard & St. George, 1958; Hubbard & Wald, 1952), and involves Hubbard's concern with explaining to scientists and the general public the dangers of misconstruing the role of genes in human development and, more specifically, health and disease.

For instance, in discussing some of the mistaken ideas of biologists who aspire to reduce health and disease to a genetically determined base, Hubbard explains that:

> Many of them imagine that in the not too distant future this science will provide each of us with a little plastic card the size of a credit card, imprinted with a bar code representing our DNA sequence. Our bar code will then be used to predict our personal health risks. It will suggest the situations we had best avoid in order to maximize our chances of staying healthy, and help physicians design therapies tailored to our individual "genetic profiles."
>
> As I make clear in this book, I do not believe that even the most detailed DNA sequences and the smartest computers can provide that kind of information. There is no way to anticipate the biological, physical, and social eventualities that affect how each of us develops and changes over the course of our lives. Such claims are based on completely invalid extrapolations from a few, quite rare, genetic conditions . . . When one goes on to look at much more complicated conditions such as diabetes, high blood pressure, cancer, or various behaviors, genetic components become just one factor of many in a process that is so complex that it makes little sense to look to genes for an answer. Of course, inherited characteristics—which is all we are talking about—play a part, but the minutiae in our genetic bar code would tell the vast majority of us no more about our future health than do the family histories already in our medical records.
>
> (Hubbard & Wald, 1999, pp. xx–xxi)

Thus, the conclusion that Hubbard and Wald (1999) reach is that if, for instance, a woman is found to have mutations or alterations in genes linked statistically to an above-average likelihood that she will develop breast cancer (e.g., genes labeled BRCA-1 and BRCA-2), one of two eventualities will definitely occur. She will either develop breast cancer or she will not develop breast cancer.

In addition, and as has occurred repeatedly across history, the danger of such oversimplification of the role of genes in human development is its potential deleterious impact on public policy (e.g., Gould, 1981; Hirsch, 1970; Kamin, 1974; Lerner, 1992a, 1992b, 2014, 2015b, 2015c, 2016; Lewontin, Rose, & Kamin, 1984; Sarason, 1973). For example, simplistic notions of how human beings' behavior and development


Ruth Hubbard

operate led in the past to eugenic laws that were designed to eradicate particular developmental deviations such as mental retardation or low IQ through selective breeding (Gould, 1981; Proctor, 1988; see too Chapters 11 and 12). Thus, public policy interventions promoting genetic manipulations in specific populations identified as having a particular gene for an undesirable attribute, or the lack of a public willingness to intervene if a purported genetic "predisposition," rather than environmental etiology, has been identified, are two of the possible scenarios derived from this misunderstanding of the complex coactions between genes and the environment within the relational developmental system.

Fortunately, current knowledge of biology, relational developmental systems, and the fused, or synthetic, coaction of environmental and biological influences on behavior and development can illuminate and guide understanding of the actual (factual) character of gene↔environment relational processes and such knowledge can and should influence the design of appropriate and useful public policies (Gottlieb, 1997; Lerner, 2015b, 2015c, 2016). Even at the cellular level, and detectable even *in utero*, genes are expressed within particular environmental circumstances, and alterations of those environments can lead to radically different phenotypic expressions from similar genetic material (e.g., Moore, 2015a, 2016; Slavich & Cole, 2013). Indeed, as illustrated by the RDS-based work of Gottlieb (2004; Gottlieb, Wahlsten, & Lickliter, 2006), Moore (2015a), Suomi (2004, 2006), and others (see Garcia Coll, Bearer, & Lerner, 2004; Richardson, 2017), genes and environments coact within the holistic and integrated relational developmental system. Consequently, theories of human development based entirely on either nature or nurture alone are counterfactual, and research predicated on such dichotomies will produce incomplete and possibly useless data.

As discussed in prior chapters, the basic issue in human development has been the nature–nurture controversy. Indeed, this controversy remains very much an issue. For example, whereas some writers (e.g., Bjorklund & Ellis, 2005; Lorenz, 1965; Plomin et al., 2016; Rushton, 2000) postulated preformed, innate "mechanisms" to account for various instances of behavioral development (e.g., altruism, aggression and militarism, cognition, morality, and even television watching), others have taken a probabilistic-epigenetic approach (e.g., Gottlieb, 1970, 1983, 1997, 2004; Kuo, 1967; Lehrman, 1953; Schneirla, 1957, 1966). In addition, some scientists interested in intelligence suggested hypotheses that emphasized the primacy of heredity factors (e.g., Herrnstein, 1971; Herrnstein & Murray, 1994; Jensen, 1969, 1974, 1980; Rowe, 1994; Rushton, 1999, 2000; Rushton & Jensen, 2005), whereas others pointed to the role of gene↔environment coactions (Griffiths & Tabery, 2008, 2013; Lewontin, 1976, 2000, 2011; Lewontin, Rose, & Kamin, 1984; Richardson, 2017; Richardson & Norgate, 2006).

Perhaps the best example of the multidisciplinary dimensions of this debate arose in 1975 with the publication of E. O. Wilson's *Sociobiology: The New Synthesis.* As noted more fully in Chapter 12, sociobiology, as promoted by Wilson (1975a; Lumsden & Wilson, 2006) and others (e.g., Alcock, 2001; Trivers, 1971, 1985; Rushton, 1999, 2000), attempts to integrate through genetic reductionism the biological sciences, the social sciences, and the humanities. As pointed out by the philosopher Caplan (1978, p. 2), this approach is the "most strident of a series of efforts in the biological sciences to direct scientific and humanistic attention toward the question of what is, fundamentally, the nature of human nature." Consistent with the metatheoretical assumptions discussed in Chapter 4 as associated with a predetermined-epigenetic position, many sociobiologists construe nature as making a predetermined, immutable contribution to behavior. That is, whatever the proportion of variance in human social behavior with a genetic basis, it is that proportion that is genetically constrained and generally unavailable to contextual influence.

The criticisms of sociobiology have come from the several disciplinary quarters that sociobiologists seek to digest (e.g., see Caplan, 1978; Hubbard, 1990; Lerner, 1992a, 1992b, 2006b, 2006c; Lerner & von Eye, 1992; Lewontin, 2000; Lips, 2001). Within the biological and social sciences, criticisms have generally been associated with the conceptualizations that emphasize that sociobiologists do not appreciate the plasticity of genes, organisms, or contexts; and that, just as genes influence their contexts, the reverse is also the case (e.g., see Gottlieb, 1991, 1992, 1997, 2004; Gottlieb et al., 1998; Grouse et al., 1978, 1979; Uphouse & Bonner, 1975).

In this debate about the usefulness of sociobiological thinking, a key influence on the differences of opinion expressed by participants involves philosophical issues (Griffiths & Tabery, 2013; Tabery & Griffiths, 2010). Those who favor sociobiological thinking are essentially arguing from a predetermined-epigenetic viewpoint (as compared to a probabilistic-epigenetic viewpoint; see Gottlieb, 1970, 1983, 1997). In turn, those who reject this conceptualization argue from a metatheoretical stance that emphasizes dynamic (mutually influential) coactions between heredity and environment (e.g., Gottlieb, 2004; Gould, 1976; Greenberg, 2007; Halpern, Hood, & Lerner, 2007; Hirsch, 2004; Hood, Halpern, Greenberg, & Lerner, 2010; Lerner, 1976, 1978, 1992a, 1992b, 2006b, 2006c, 2012, 2015b, 2015c, 2016; Lerner & von Eye, 1992; Lewontin, 1976, 2011; Lewontin et al., 1984; Overton, 1973, 2006, 2015a). Thus, this viewpoint is consonant with the ideas linked to relational developmental systems (RDS) metatheory.

The details of each of the above controversies need not be specified here in order to make the point that the field of human development in no way takes a place behind philosophy in the intensity of its debate over the nature–nurture issue. In all cases, the essence of each debate is always the same—the relative contributions of nature and nurture variables in providing a source of behavior.

By this point in the book, some readers may be wondering how a dispute that has engaged so many bright men and women for so many years can still remain an ongoing controversy. Can the issues not be detailed in such a way as to somehow diminish the seemingly endless division of opinion? I think they can. Rather than discuss here all the details of such controversies which often led to, what I believe were, conceptual dead ends, I will turn to a review of various scholars' formulations that were offered in an attempt to resolve the nature–nurture controversy. I begin with a review of the seminal ideas of a famous psychologist, a former president of the American Psychological Association, Anne Anastasi. I then use Anastasi's formulations as a general framework within which to begin to consider the issues necessary for our reconceptualization of the nature–nurture controversy.

That is, in this chapter and the next two ones I point to the pioneering intellectual contributions of scholars who provided foundational ideas about core concepts within developmental science. I also discuss contemporary (at this writing) implications of the work of these scholars—Anne Anastasi, in this chapter, and T. C. Schneirla and Heinz Werner, in Chapters 7 and 8, respectively.

Of course, each of these scientists wrote in the argot of their times, periods within the history of developmental science wherein split and reductionist approaches were at the forefront of the field. As such, they at times phrased ideas with terms different than ones more typically used at this writing. For instance, Anastasi (1958) discussed heredity–environment *interactions* whereas contemporary developmental scientists might write about gene↔context *coactions* (e.g., Moore, 2015a, 2016; Slavich & Cole, 2013). Despite this variation in terminology, I hope to make evident connections between such across-time discussions. I hope too that my discussion will help developmental scientists of the twenty-first century become more aware of the important work of developmental scientists of the twentieth century.

ANNE ANASTASI (1958): HEREDITY, ENVIRONMENT, AND THE QUESTION "HOW?"

Anne Anastasi's classic article, which first appeared in the *Psychological Review* in 1958, represents a key, and foundational, treatment of the nature–nurture controversy. Although it was one of a set of publications during the mid- to late 1950s that addressed the key issues in the controversy in similar ways (e.g., Lehrman, 1953; Schneirla, 1956, 1957), Anastasti's article became the most widely read and reprinted approach to resolving the controversy. Because of its foundational importance and, as well, its broad acceptance in the developmental science community—even among community members who were inclined to take a split, genetic reductionist position—it is historically important and conceptually useful to review the issues involved in the controversy through the lens of Anastasi's ideas.

Anne Anastasi

The essential problem in appropriately conceptualizing the nature–nurture controversy, as Anastasi saw it, was that psychologists were asking the wrong questions. Therefore, they obviously could not get the right answers. Anastasi attempted to show why previous inquiries led to dead ends and to identify the appropriate question.

The first way that philosophers—as well as psychologists—inquired into this problem was to ask, "Which one?" Based on a Cartesian split conception of reality (Overton, 2015a), the question was framed as "Does heredity or environment, nature or nurture, provide the determining source of behavior?" Those who posed the issue in this way were assuming the reality or essential character of only one source of behavior; that is, in splitting the world into the real versus the pseudo-phenomenal, the primary versus the secondary, or the essential versus the derivative, the assumption was made that the *independent, isolated action* of one or the other domain provided a source of a behavior. However, developmental scientists should reject this split way of posing the problem, because it is basically illogical. To explain, I will use the terms focused on by Anastasi: That is, nature is *heredity* and nurture is *environment.*

The "which one?" question assumes that heredity and environment are independent, separable sources of influence and, as such, that one can exert an influence in isolation from the other. But Anastasi pointed out that such a split assumption was illogical. This assertion was true because there would be no one in an environment without heredity, and there would be no place to see the effects of heredity without environment. Genes do not exist in a vacuum. They exert their influence on behavior in an environment. At the same time, however, if there were no genes (and consequently no heredity), the environment would not have an organism in it to influence. Accordingly, nature and nurture are inextricably tied together. In life, they never exist independent of the other. They are fused. As such, Anastasi argued that *any* theory of development, in order to be logical and to accurately reflect life situations (i.e., to have *ecological validity*), must stress that nature and nurture were always involved in all behavior, and it was simply not appropriate to ask "which one?" because they were both completely necessary for any organism's existence or for the existence of any behavior.

Some psychologists (e.g., Hebb, 1949; Lehrman, 1953; Schneirla, 1956, 1957), however, had recognized the inappropriateness of the "which one?" question even before Anastasi's (1958) article was published. Yet, others had asked another question that, according to Anastasi, was also inappropriate. It also led to a conceptual dead end. These psychologists put the issue this way: Granted that nature and nurture are always involved in any behavior, that both of them are always needed, *how much* of each is needed for a given behavior? For intelligence, do you need 90% heredity and 10% environment, or is intelligence perhaps only two parts heredity and eight parts environment? Or some might ask: For personality, can it be 50% of each, whereas for perception it is seven parts of one, three parts of the other? In essence, psychologists asking this question would attempt to ascertain how much of each source was needed for a given type of behavior. As discussed by Tabery (2014; and see Moore, 2015b), the "how much of each?" question is a question addressed through variance partitioning; as discussed in Chapters 11 and 12, this variance partitioning approach is exemplified by the work of behavior geneticists (e.g., Plomin, 2000; Plomin et al., 2016).

But the "how much of each?" question also leads to a fruitless end, because—like the "which one?" question—it is based on the same split and inappropriate underlying assumption. In the case of the "how much?" question, the instantiation of the split assumption may be termed the *independent, additive-action assumption.* It suggests that the way in which nature and nurture are related to each other is that the contribution of one source is added to the contribution of the other to provide the basis of a specific behavior. This solution puts the nature–nurture relation in the terms of a recipe: Add one part of X to some part of Y to get a specific behavior; that is, add some unknown part of nature to an unknown part of nurture to get a specific behavior.

However, such a question raises many others. For example, for the 80% of intelligence that might be thought to be nature, a developmental scientist may ask where that 80% exerts its influence if not in an environment. For the 20% of intelligence thought to be nurture, how is that 20% acted on if an organism does not first have genes involved in making it a material being? Does not the nature part play a role with the nurture part? If not, then what can that nurture possibly contribute to? And what of the (unknown) contributory part of nature? Can it contribute to behavior without any environmental support? Where does it contribute if not in an environment?

Thus, the "how much?" question soon leads to separating out (splitting) the independent, isolated effects of nature and nurture, a conceptual route just taken by means of the "which one?" question. I rejected the route, with its split notions of either heredity or environment, because nature and nurture are always inextricably bound. Thus, developmental scientists must also reject the "how much?" route because it really does not move beyond the "which one?" path. In fact, the "which one?" question can be seen to be just a special case of the "how much?" question. That is, the former question implies a 100%/0% (or a 0%/100%) split between nature and nurture, respectively, whereas the latter question implies some percentage split less than this 100% versus 0% alternative.

Thus, a conceptualization of the independent action of either source (in either an isolated or an additive manner) will lead to a conceptually vacuous dead end. Developmental scientists should conclude, then, that two assertions directly follow from the rejection of these vacuous arguments. First, nature and nurture are always completely involved in all behavior. Put another way, 100% of nature and 100% of nurture always make their contributions to all behavior. Any method of inquiry into the source of behavioral development that does not take cognizance of this statement, and that seeks to make artificial distinctions between nature and nurture, can lead only to conceptual confusion and to an empirical blind alley. Second, since independent-action conceptualizations of the contributions of nature and nurture similarly lead to conceptual dead ends, an alternative conceptualization of their contributions, that of (*dynamically*) *coacting*, or *fused*, *action*, seems more appropriate (Greenberg & Tobach, 1984; Overton, 2015a).

This alternative, which seems useful from the perspective of RDS-based concepts (Overton, 1973, 2015a), indicates that both nature and nurture coact dynamically (as components, or dimensions, of a fused, relational developmental system) to provide a source of behavioral development. Because both sources are necessarily completely present and because it is inappropriate to speak of their contributions as adding to each other, then it seems that developmental scientists should ask: *How* do nature and nurture dynamically interrelate to produce behavioral development? *How* do the effects of each multiply (or reciprocally interrelate within a fused, relational developmental system) to provide a source of development? Thus, the RDS-based probabilistic-epigenetic version of organicism, with its notion that dynamic coactions between nature and nurture provide the basis of development (Gottlieb, 2004), is the view associated with this question.

This third question—the question of "how?"—leads to what Anastasi (1958) regarded as the appropriate route of investigation into the contributions of nature and nurture. This formulation, Anastasi argued, is the appropriate way to conceptualize the issue, because it takes cognizance of the logical necessity of the material existence of both domains for a living organism (or living system; Ford & Lerner, 1992; Overton, 2015a). That is, this question denies a split between nature and nurture based on the contention that one domain is real (Putnam, 1987) and the other is pseudo-phenomenal (e.g., as in Rowe, 1994). Rather, the "how?" question is based on what I term the *integrated coaction approach*, which implies:

1. That nature and nurture are both fully involved in providing a source of any feature of human development.
2. That nature and nurture cannot, therefore, function in isolation from one another but must always be systemically fused in their contributions.
3. That fusion (which cannot be appropriately construed to mean addition) can be conceptualized as an integrated coaction—that is, a type of relation in which the full presence of each source is completely intertwined with the other.

In other words, from this view heredity and environment do not add together to contribute to behavior, but rather development is seen as a *product* of nature↔nurture coactions within the integrated, holistic, and autopoietic relational developmental system.

Some analogies may help illustrate this point. First, consider that the area of a rectangle is determined by a formula that multiplies the length by the width (area = length x width). To know the area of a given rectangle, one has to look at the product of a multiplicative relation. It is simply incorrect to ask which one, length or width, determines the area, because a rectangle would not exist unless it had both length and width. Similarly, it is incorrect to ask how much of each is necessary to have area, because the two dimensions cannot merely be added; they must be multiplied in order to produce a rectangle.

Of course, although length and width must always be completely present in order to have a rectangle, different values of each will lead to different products (or areas). Thus, in determining a particular product (a given area) of a length x width relation, one must ask *how* a specific value of length in relation (in multiplication in this analogy) with a specific value of width produces a rectangle of a specific area. More generally, the same width would lead to different areas in relation to varying lengths and, in turn, the same length would lead to different areas in relation to varying widths.

A second analogy involves viewing a baked cake. When the cake is done, an observer cannot tell how much of it is composed of sugar, how much is composed of eggs, and how much is composed of flour. These ingredients are different as a result of the baking process and the resulting cake cannot be deconstructed into the ingredients that compose it.

By moving from these analogies back to the question "how?" in regard to the nature–nurture issue, comparable statements may be made. There would be no product—no development—if nature and nurture were not 100% present (the area of a rectangle analogy) or if all of the components of the whole, all of nature and nurture, were not integrated in the developmental process (the baking a cake analogy). Thus, the assumption of a split that underlies the "which one?" and "how much of each?" questions is rejected, and it is the case that any development is the result of an integrative coaction (fusion) between specific hereditary and environmental variables. Moreover, this idea means that the same hereditary influence will lead to different developmental products in relation to varying environments; furthermore, the same environment will lead to varying products in relation to different hereditary variables.

Thus, heredity and environment *never* function independently of each other. Nature (e.g., genes) never affects behavior directly; it always acts in the context of internal and external environments. That is, genes are influenced by the inside-the-organism context (e.g., the cellular and the extra-cellular physiological environment within the body of the organism) and the outside-the-organism context, for example, social relationships and the conditions of the natural and designed physical ecology (e.g., Lester et al., 2016; Meaney, 2010, 2014; Moore, 2015a, 2016). In turn, environment (e.g., social stimulation) never directly influences behavior; it will show variation in its influence depending on the heredity-related attributes of the organism on which it acts.

These statements about the reciprocal interdependence of nature and nurture are not just casual matters. In Chapter 3, I noted that philosophers and scientists have tried to conceptualize behavior and development in terms that are inconsistent with the view reflected by the integrated coaction conception. Succeeding chapters consider theorists who emphasize that various components of development (e.g., cognition or personality) can be understood by ideas that stress *either* nature or nurture (i.e., the "which one?" question; see Chapters 11 and 12). Thus, it is important to point out that others do not necessarily agree that the formulation I favor is the best or most useful one. Nevertheless, I believe that the question "how?" leads the developmental scientist to a consideration of the coactions of nature and nurture that provide a basis of development. It seems that, of the questions discussed by Anastasi (1958), only this "how?" question casts aside fruitless and counterfactual polemics and allows developmental scientists to begin to unravel the decidedly complex relations of nature and nurture in the relational developmental system.

HEREDITY–ENVIRONMENT RELATIONS

Assuredly, if Anastasi (1958) had ended her article after making some of the above points, her contribution to the conceptual clarification of the nature–nurture controversy would have been considered substantial. Anastasi's article provided additional ideas, however. After indicating that the appropriate way to conceptualize the nature–nurture controversy is in terms of *how* these two sets of sources interrelate, Anastasi suggested how heredity and environment may provide a basis of development.

Nature Effects Are Indirect

Using Overton's (2015a) second moment of theory-predicated research that I discussed in Chapter 2, that is, the opposites of identity, I focus first on hereditary factors. Anastasi argued that the effects of heredity on behavior were diverse and always indirect. That is, no facet of human development is ever directly inherited (e.g., see Meaney, 2010, 2014; Moore, 2015a, 2016; Slavich & Cole, 2013); heredity always relates to behavior in an indirect way. This assertion is derived from the rationale that was used to object to the reality-splitting (Overton, 2015a) "which one?" question: Developmental scientists need to assess internal and external environments to see the effects of heredity; there would be no place to see the contribution of nature if there were no environmental context. Accordingly, the specific contribution of heredity to development will depend on the specific environment in which that contribution occurs. Consistent with the probabilistic-epigenetic position discussed in prior chapters, any hereditary contribution must occur in an environmental context, and the particular expression of the hereditary contribution that will eventually be seen will depend on the specific characteristics of the environment in which it occurs.

To illustrate, hereditary contributions may be represented by the letter "G" (for genes), environmental contributions by the letter "E," and behavioral outcomes by the letter "B." Genetic reductionist formulations might suggest that it is possible to conceptualize the contribution of heredity to behavior as being direct. In such a formulation, a particular combination of genes (G_1) will invariably lead to a particular behavioral outcome (B_1). However, I have indicated that this conceptualization is not appropriate. As such, I have argued for a coactive idea of nature and nurture, as illustrated in Figure 6.1. Here, the same hereditary contribution (G_1) can be linked with an infinity of behavioral outcomes (B_1 to B_n) as a consequence of coaction with the infinity of environments (E_1 to E_n) that could exist. Consider again the baking a cake analogy I used earlier in this chapter. The same kind and amount of ingredients (eggs, sugar, flour, etc.) can produce different results depending on a lot of other factors involved in baking (e.g., cooking time, cooking temperature, order in which the ingredients are added, etc.).

Consider as an example the case of a child born with Down syndrome. The genetic material—the

DNA—of genes is arranged on chromosomes. The typical cells of the human body have 46 chromosomes, divided into 23 pairs. The only cells in the body that do not have 46 chromosomes are the gametes—the sex cells (sperms in males and ova in females). These cells carry only 23 chromosomes, one of each pair. This arrangement assures that when a sperm fertilizes an ovum to form a zygote, the new human so created will have the number of chromosome pairs appropriate for the species. However, in a child born with Down syndrome, a genetic anomaly exists. There is an extra chromosome in the twenty-first-pair—three chromosomes instead of two.

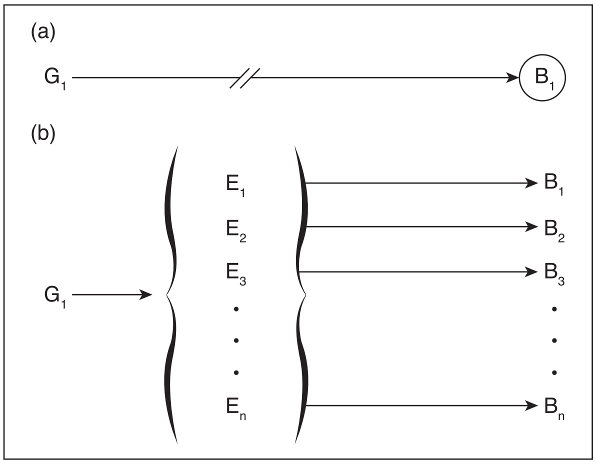


Figure 6.1 (a) Heredity (*G*) does not directly lead to behavior (*B*). (b) Rather, the effects of heredity on behavior will be different under different environmental (*E*) conditions. Since the early 1900s, this phenomenon has been known in biology as the "norm of reaction."

Source: Gottlieb, 1992; Hirsch 1970.

Thus, children with Down syndrome have a specific genetic inheritance. The complement of genes transmitted to people at conception by the union of the sperm and ovum is termed the *genotype*. This transmission is what constitutes humans' genetic inheritance. At least insofar as the extra chromosome is concerned, the child with Down syndrome has a specific genotype. Yet, although the genotype remains the same for any such child, the behavioral outcomes associated with this genotype differ.

In the middle decades of the twentieth century, children with Down syndrome, who are typically recognized by certain physical (particularly facial) characteristics, were expected to have life spans of no more than about 12 years. They were also expected to have quite low scores on tests of intelligence.

Figure 6.2 Shannon Woodward and Evan Snyder in the film, *Girlfriend*. Photo by Quyen Tran/courtesy of Strand Releasing.

They were typically classified into a group of people who, because of low intelligence, required custodial (usually institutional) care. At this writing, however, Down syndrome children often live well beyond adolescence. In addition, they lead more self-reliant lives. Their intelligence scores are now typically higher, often falling in the range allowing for education, training, gainful employment, and even accomplishments in the creative or performing arts. For instance, Down syndrome individuals may act in or even star in films. One instance of such a performance is illustrated in Figure 6.2. In the 2008 feature film, *Girlfriend*, by director and screenwriter, Justin Lerner, the lead actor, Evan Snyder, is a person with Down syndrome.

How did these vast differences come about? Certainly, the genotype did not change. Rather, what changed was the environment of these individuals. Instead of invariably being put into institutions, different and more advanced special education techniques were provided, often on an outpatient basis. These contrasts in environment led to variation in behavioral outcomes despite the same heredity, that is, despite *genotypic invariance.*

That heredity always exerts its effects indirectly through environment in the development of physical as well as behavioral characteristics may also be illustrated. First, consider the disease *phenylketonuria* (PKU). This disorder, involving an inability to metabolize fatty substances because of the absence of a particular digestive enzyme, led to the development of distorted physical features and severe disability in children. It was discovered that the lack of the necessary enzyme resulted from the absence of a particular gene and, as such, PKU is another instance of a disease associated with a specific genotype.

Today, however, many people—perhaps even some students reading this book—may have the PKU genotype without having either the physical or the behavioral deficits formerly associated with the disease. In the latter decades of the twentieth century, researchers discovered that, if the missing enzyme is put into the diets of newborns identified as having the disease, all negative effects could be avoided (Scriver & Clow, 1980a, 1980b). Again, change in the environment has changed the outcome linked to the gene. In fact, researchers also found that, at about 1 year of age, the child with PKU no longer needs the added enzyme, since the body either no longer needs it to metabolize fat or produces the enzyme in another way (Scriver & Clow, 1980a, 1980b). Here, again, the same genotype will lead to alternative outcomes, both physical and behavioral, when it occurs in contrasting environmental settings.

Another example illustrates this point still further and, more important, provides a basis for specifying the variety of environmental characteristics within which hereditary contributions are embedded. First, imagine that an experiment (improbable for ethical and technological reasons) was done—say a mother was pregnant with *monozygotic* (identical) twins. These are twins who develop from the same fertilized egg—the same zygote—which splits after conception. Hence, the two zygotes have the same genotype. But, importantly, because the zygotes may implant on somewhat different parts of the wall of the uterus and/or have different placentas, there may exist somewhat different environments. Imagine further that it was possible, immediately after the zygote split into two, to take one of them and implant it in another woman who would carry the organism through to birth. Finally, imagine that the first woman, "Mother A," has lived for the last several years on a diet of chocolate bars, potato chips, and diet soft drinks, smoked two packs of cigarettes a day, and consumed a pint of alcohol each evening. On the other hand, say the second woman, "Mother B," has consumed a well-balanced diet and neither smoked nor drank. In all other respects, the women are alike.

Here is a situation wherein two genotypically identical organisms are developing in quite different uterine environments. Such differences are known to relate to *prenatal*, *perinatal* (birth), and *postnatal* behavior on the part of the offspring, and even to have implications for the mother. Thus, despite possession of the same genotype, the offspring of Mother A would be more likely to be born anemic (because of the mother's poor diet) and to be smaller, less alert, and more hyperactive (because of the mother's smoking habit and alcohol intake) than the offspring of Mother B.

Although this study with Mother A and Mother B is imaginary, the influence of the uterine environment on the offspring is not at all fanciful. The imaginary example was used to illustrate that variations in the environment will cause significant physical and behavioral changes in an offspring despite the genotype. As I discuss later in this chapter, even physical characteristics, such as eye or skin color, may be influenced by environmental variations (albeit extreme ones) no matter what genes are inherited. If mothers are exposed to extreme radiation or dangerous chemicals (as in the case of mothers in the 1950s who took the drug *thalidomide*), pigmentation of the eyes or the skin can be radically altered and/or limbs can be severely deformed.

In sum, then, I believe that, in order to understand the contributions of heredity to development, developmental scientists need to recognize that genes influence physical and behavioral characteristics indirectly, by acting in a specific environment. If the same genetic contribution were to be expressed in an environment having other specific characteristics, the same genes might be associated with an alternative behavioral outcome. Accordingly, in order to completely specify the integrated coactions of nature with nurture, all the ways in which the environment can vary should be known (and, as is argued in the following section, it is also the case that developmental scientists must know how genes vary to specify the relations between nurture and nature).

There is an infinity of possible environmental variations; and today, developmental scientists cannot even begin to identify all the chemical, nutritional, psychological, social, and physical ecological variables that may vary in the environment, much less identify the ways in which they provide a significant context for development. Nevertheless, developmental scientists may note at this point that the environment may be thought of as existing at many levels. Developmental scientists can look at the environment in molecular terms—and talk of chemicals in the body of the mother. In addition, they can use molar terms—and talk of noise and air pollution levels in particular settings (e.g., urban ones) (e.g., Bronfenbrenner, 2005; Bronfenbrenner & Morris, 2006). Consequently, it is useful to specify levels of the environment because it allows discussion about where the variables that provide the context for nature–nurture coactions may lie.

Levels of the Environment

An organism does not exist independent of an environment, and as much as the organism is shaped by the environment, the organism *shapes* the environment (Halfon & Forrest, 2018; Lerner, 1982; Lerner & Busch-Rossnagel, 1981a, 1981b; Lerner, Theokas, & Jelicic, 2005; Lerner & Walls, 1999). As a consequence of this interdependency, both organism and environment may continually change, and this change involves multiple levels of analysis. These levels—for example, the inner-biological, individual-psychological, physical-environmental, and sociocultural-historical (Riegel, 1975, 1976a)—denote the types of nurture-related variables that may provide the context for coactions with nature.

The Inner-Biological Level

The genotype is first expressed *in utero*, in the mother's body. Hence, the chemical and physical makeup of the mother can affect the offspring (Zuckerman & Keder, 2015; Wang, Bartell, & Wang, 2018). For instance, chemicals in the mother's bloodstream can enter that offspring through the umbilical cord, the attachment between mother and offspring. As already noted, poor nutrition, excessive smoking, alcohol use, and other drug ingestion can affect the unborn child. In addition, diseases (e.g., rubella) can lead to malformations of the heart and limbs and can affect the development and function of sensory organs (the eyes or ears).

The work of Phelps, Davis, and Schartz (1997) illustrated that even among monozygotic (MZ) twins, internal biological influences occurring prenatally can affect development. For instance, MZ twins may differ in regard to whether they share a single placenta and chorion or have separate placentas and chorions. Phelps et al. (1997) report that MZ twins who share a chorion can be more dissimilar than MZ twins who do not share a chorion in regard to physical and medical variables (e.g., birth weights) and more similar in regard to some psychological characteristics (e.g., variables related to personality or intelligence).

The Individual-Psychological Level

Independent of her diet, smoking or drinking habits, and physical health status, the psychological functioning of the mother can affect the unborn child. For example, maternal stress and depression during pregnancy have been linked to adverse birth outcomes, including specific congenital anomalies (Allister, Lester, Carr, & Liu, 2001; Carmichael & Shaw, 2000; Halfon & Forrest, 2018; Zuckerman & Keder, 2015). To illustrate the interrelation among all the levels of the environment, it may be that maternal stress exerts an influence on the unborn child by altering the chemicals (e.g., adrenaline) in the blood—at the inner-biological level—at a time in the embryological period when specific organs are being formed.

In addition, previous childrearing experiences can play a part on the individual-psychological level. Experienced parents (those who already have a child) are not the same people they were before they had an offspring (Bornstein, 2015). Firstborns, in this sense, have different parents than latter-borns, although the parents involved may be biologically the same (Thomas, Chess, & Birch, 1968). Thus, a mother may be less likely to be stressed by a second pregnancy. Not only might this changed stress level affect the chemicals in her bloodstream but also, in being less "nervous," she might be less likely to engage in "nervous" behaviors (e.g., smoking).

Of course, as more information about prenatal care becomes available in society (e.g., about the dangers of a woman drinking or smoking during pregnancy), and as cultural values change (e.g., toward showing general approbation for drinking or smoking during pregnancy), effects on maternal stress and "nervous" behaviors will change. Thus, one level of environment is related to another, the individual-psychological to the sociocultural-historical. Before discussing the latter level, however, it is useful to consider the physical-environmental level.

The Physical-Environmental Level

Physical settings differ in such variables as air quality, water purity, noise levels, population density, and general pollution of the environment. Such variables can affect the inner-biological functioning of a person by producing variations in the likelihood of contracting certain diseases (Willems, 1973; Zuckerman & Keder, 2015), and can also affect the individual-psychological level by producing various levels of stress (Gump, 1975; Halfon, Forrest, Lerner, & Faustman, 2018). In turn, the quality of the physical setting may be seen as both a product of the values and behaviors of the culture of a society and a producer of changes in the sociocultural setting across time. If values regarding industrialization in the United States had not existed as they did in the early 1960s, and if high levels of industrial waste had not polluted air, land, water, and wildlife, there would have been no basis for the general emergence of countervailing values in the late 1960s and 1970s regarding environmentalism, ecology, and the reduction of pollution. The physical-environmental level is not independent of the sociocultural-historical level (Halfon et al., 2018).

The Sociocultural-Historical Level

Attitudes toward smoking, knowledge about prenatal health care, and values and public policies (e.g., prohibiting smoking in public buildings) about secondhand smoke may change to influence the unborn child (Boyce & Hertzman, 2018; Halfon & Forrest, 2018). Thus, with advances in education (recall the example of children with Down syndrome), medicine, and science (recall the example of children with PKU), and changes in attitudes (e.g., regarding breast versus formula feeding), values, mores, behaviors, and policies (e.g., regarding smoking, drinking, drug use, and pollution of the environment), the outcome of any given hereditary contribution to development will be altered (Halfon et al., 2018; Zuckerman & Keder, 2015).

In sum, a variety of behavioral outcomes may result from nature variables being integrated with a multilevel environment. Development is thus an outcome of hereditary contributions dynamically coacting with changes in the environment (Lerner, 2006b, 2012, 2015a, 2015d). Thus, a genotype is not a blueprint for a final behavioral outcome (Gottlieb, 1992, 1997; Gottlieb et al., 2006; Hirsch, 1970). There is no one-to-one relation between genotype (genetic inheritance) and *phenotype* (the observed outcome of development, the outcome of a specific genotype–environment interrelation). Rather, numerous phenotypes can result from the same genotype. The range of potential outcomes that could result from a given genotype's potentially infinite coactions with environments is termed the *norm of reaction* (Hirsch, 1970).

The indirectness of hereditary effects means that developmental scientists cannot a priori specify what behavioral effect a particular hereditary contribution will have. Hereditary contributions can express themselves only within the context of their integration with a complex (i.e., multilevel and systemically changing) environment (e.g., Cole, 2014; Meaney, 2010; Moore, 2015a, 2016; Slavich & Cole, 2013). Without knowing how this environment will influence the hereditary effects, no before-the-fact statement may be made about what specific behaviors will result from a particular hereditary contribution.

Thus, there can be no preformed, direct, or invariant hereditary contribution to behavior. As I indicated in Chapter 4, in my discussion of the probabilistic-epigenetic conception, the most accurate way of conceptualizing the contribution of nature factors to behavioral development is:

1. To recognize the necessary and crucial role that nurture factors play in providing an integratively coacting context for nature factors.
2. To recognize that the time at which these factors interrelate will play an important role in shaping development; that is, as suggested by the concept of probabilistic epigenesis and, indeed, all RDS-based conceptions (e.g., Bronfenbrenner & Morris, 2006; Elder et al., 2015; Lerner, 1978, 1979, 2015a, 2015e), the contribution of one factor to the other will not be the same at different points in development.

The characteristics of nature and nurture factors as well as their time of integration (coaction) cannot be expected to occur at exactly the same time or in exactly the same way for every organism. Therefore, in advance of a specific coaction, a researcher can say only that specific developments will probably occur. That is, the best a developmental scientist can do is to take an (informed) guess, with some degree of confidence in his or her chances of being correct (i.e., he or she can make a probabilistic statement), about what sort of specific development will eventually result from a specific hereditary contribution. Said another way, *behavioral development is a probabilistic outcome of the fusion of specific instances (e.g., states) of variables that exist at multiple, integrated levels of organization* (e.g., Gottlieb, 1997; Overton, 2015a; Tobach, 1981).

Any statement that in effect says that a given hereditary contribution will invariably (in all environmental contexts) result in a specific behavior (e.g., as asserted by Costa & McCrae, 1980; McCrae et al., 2000) is simply incorrect. Therefore, several statements can be made about the indirect effects of heredity on behavior. First, the following points should be clear:

1. The same hereditary attribute can be expected to have a different behavioral influence in different environmental conditions.
2. Alternatively, the reverse may also be true—different hereditary attributes can lead to the same behavioral development in varying environmental situations.

To be complete, I should also point out that:

3. The same environmental condition may be expected to lead to different behavioral outcomes under differing hereditary contributions.
4. Different environments can lead to the same outcome in the context of varying hereditary contributions.

Together, these points mean that the sources of behavior coact with each other in complex ways. Any analysis of behavioral development that attempts to be appropriate in its recognition of this complexity must always attempt to understand the varying status of the integration of both nature and nurture factors (Lerner, 2016; Witherington & Lickliter, 2016). RDS-based models embrace such complexity. Nature-reductionist ones (e.g., those associated with evolutionary developmental psychology; e.g., Bjorklund, 2015, 2016; Bjorklund & Ellis, 2005) seek to eliminate such complexity through genetic reductionism (Lerner, 2016).

The Continuum of Indirectness

Given heredity's indirect contribution to behavior, Anastasi (1958) conceptualized the contributions of heredity to behavior as varying along a "continuum of indirectness," a hypothetical line whose endpoints are "least indirect" and "most indirect." Such a hypothetical continuum is represented in Figure 6.3. The left end represents those hereditary contributions to a person that are *least indirect* (or most direct) in their influence. Such effects may be represented by such physical characteristics as eye color or eventual shape of the nose. However, it is important to be careful to remember that even these least indirect hereditary effects need, at the very least, the supportive, facilitative influence of the environment. That is, "least indirect" effects are those that will tend to arise despite a wide range of contextual variation. However, at the extremes of this variation (e.g., involving highly toxic chemical influences or high dosages of radiation occurring at particular times in ontogeny, such as during the embryonic period of prenatal development), even "least indirect" effects will show context-dependent variation (Gottlieb, 1997; Lickliter, 2016). Indeed, in such circumstances chickens can grow teeth (e.g., Lickliter, 2016)!

The right end of the continuum represents those hereditary contributions to a person that are *most indirect.* Here the possible number or types of interrelations with the environment increase and, accordingly, the range of resulting behavioral outcomes are much more numerous. Thus, as hereditary influences become more indirect, the range of possible behavioral outcomes of the coaction between heredity and environment similarly increases.

What are some possible illustrations of the range of indirect hereditary contributions to behavior? Anastasi suggested four points along the continuum of indirectness to illustrate this range of effects. These four hypothetical points are ordinal in nature; they are ordered consecutively from "least indirect" effect through "most indirect" effect, although no exact specification of the location of these points can be made. Thus, although these effects are ordered appropriately, neither their exact locations along the

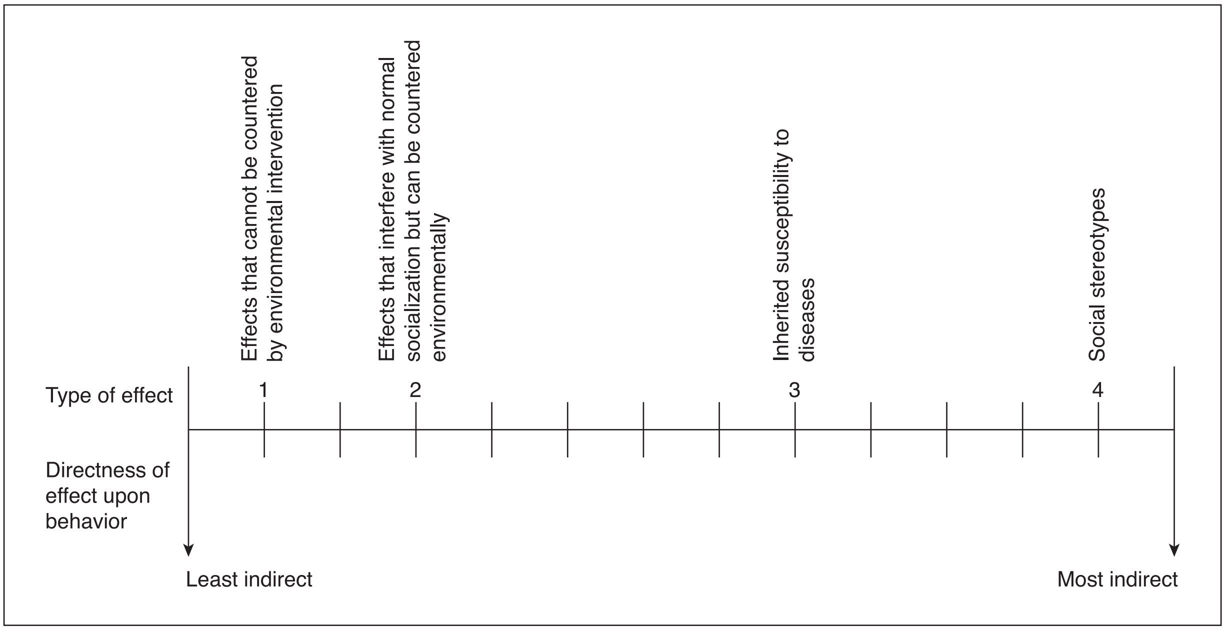


Figure 6.3 Contributions of heredity to behavioral development vary along a continuum of indirectness. Numbers 1 through 4 refer to some points along this continuum. See the text for an explanation of the uses and limits of this figure.

continuum nor the relative distances between them are certain.

Moreover, it is essential to note that Points 1 and 2 in the figure refer to genetic deficits (i.e., situations wherein the genes are not working either because they are absent; because they are anomalous, such as in Down syndrome, wherein extra genetic material is present; or because there is a lack of the appropriate signals for genetic expression, such as in the disease cystic fibrosis). These two points in the figure do not reflect the character of heredity–environment coaction within the normal complement of genes. Despite the limitations of Figure 6.3, Anastasi's specification of the points along the continuum serves to usefully illustrate the range of heredity's indirect effects on behavior.

Hereditary Effects That in No Way Can Be Countered Through Any Known Environmental Intervention

Hereditary contributions to behavior that cannot be ameliorated through the use of any *known* intervention or environmental manipulation are considered to make up the class of hereditary effects that are least indirect in relation to behavior. Although, as I noted, these effects need a supportive, facilitative environment in which to exert their contribution, once they make their contribution to behavior there is nothing that can be done at the time, through changing the environment, to alter that contribution.

For example, consider the inheritance of a chromosome trisomy, that is, the inheritance that is associated with Down syndrome. As I noted, a child with Down syndrome has rather distinctive physical (particularly facial) characteristics and may have moderate-to-severe learning disability. Thus, after the inheritance of such a chromosomal anomaly, there is nothing that can be done through environmental intervention, with current knowledge, to avert the inevitability of some level of mental disability. Although developmental scientists can certainly attempt to train the child to maximize his or her potential, at this writing they cannot raise that potential to the level it might have reached had the child not inherited a trisomy of Chromosome Pair 21. This inheritance thus represents the least indirect contribution of heredity to behavior, a contribution that cannot *presently* be countered through environmental manipulation.

Of course, future scientific advances, for instance in recombinant DNA technology (Sandhu, 2010), or epigenetics research (e.g., Moore, 2015a), may result in knowledge allowing prenatal or antenatal repair of a flawed human genome. Thus, as with PKU, the effect of a given genetic deficit on behavior can be altered, given appropriate scientific advances (Scriver & Clow, 1980a, 1980b). As such, an effect once classified as "least indirect"—such as was once the case with PKU—may not remain so for all time.

Hereditary Deficits That Interfere with Normal Socialization But Can Be Countered Through Environmental Intervention

Moving a little further along on the continuum of indirectness leads to a second class of indirect hereditary effects. These are more indirect in their contribution to behavior because their contribution can be somewhat ameliorated by changes in the environment. Although effects of this second class may interfere with the process by which a child acquires the behaviors that society may define as being necessary and appropriate, such interference may at least be somewhat counteracted by appropriate environmental modifications.

For example, imagine that, because of a genetic anomaly, a child is born blind or deaf. Certainly, such a condition would alter the development of the child's communication skills and, in this way, create an instance of socialization that would vary from a norm for such a process. Because the child cannot see (or hear), the process by which the child develops the behaviors that society designates as necessary or appropriate may differ in comparison to a child who does not have this condition. However, the fact that such an inheritance changes the specific course of the development of communication skills and socialization does not mean that these attributes do not develop effectively in other ways. As is dramatically illustrated in the play, *The Miracle Worker*, the story of Helen Keller and her teacher, the conditions of blindness and deafness need not obviate the development of effective communication skills. Thus, although a hereditary effect could alter the course of socialization, specific environmental modifications can be instituted to modify or possibly even eliminate the effects of that hereditary contribution to behavior.

Inherited Susceptibility to Disease

A third, still more indirect hereditary contribution to behavior is that type of inheritance that may predispose a person to contracting specific diseases. For example, as part of a person's inherited physical characteristics, he or she develops relatively weak musculature in one chamber of the heart. This hereditary contribution may or may not exert any influence on the person's behavior. But specific environmentally-based characteristics the person may possess (e.g., being overweight, lacking regular exercise, or having a poor diet) may affect the person's constitution and make him or her more likely to have a heart attack. Yet, in another person similarly predisposed but not having the same environmentally-based characteristics, heart disease may never develop.

Similarly, consider hay fever or other environmental allergy conditions. To the extent that this disease is hereditarily based, inherited susceptibility may or may not lead to a behavioral effect, depending on the specific environment in which a person lives. If a person lives in an area where the pollen count is extremely low, susceptibility to this disease may never significantly affect behavior. In fact, because there would be little if any pollen to precipitate an allergy attack, the person might never know that he or she has the disease. But if the person lives in an area in which the pollen count varies seasonally and, at specific times of the year, it reaches high levels, then behavior might be affected. The person might sneeze, have eyes that watered, and make attempts to seek the comfort and release provided by antihistamines and air-conditioned rooms. Behavior might be affected even at times of the year when no pollen was present in the air. The person might, for example, go to a physician all winter to get weekly or monthly desensitization injections that would act to diminish the effects of pollen during the late summer. This third point along the continuum of indirectness illustrates, then, a point made earlier: The same hereditary contribution will have a different effect on behavior under different environmental conditions.

Social Stereotypes

A final point along the hereditary continuum of indirectness, certainly representing the *most* indirect effect, is *social stereotypes.* This continuum point may seem somewhat puzzling: How can social stereotypes be a hereditary effect, albeit the most indirect one, on behavior? However, the reasoning underlying this classification demonstrates its tenability and, as well, illustrates the complex coactions between heredity and environment that provide the source of development.

As I previously noted, physical characteristics may be among the least indirect hereditary contributions to a person. Thus, specific physical characteristics such as sex, eye color, or skin pigmentation are to a great extent very directly hereditarily determined. The range of variation in these characteristics, despite environmental differences, is not as great as that of other types of characteristics located at more indirect points along the continuum.

How may such physical characteristics lead to social stereotypes? There is a vast literature providing answers to this question (e.g., Aboud, 2005; Allport, 1954; Bigler, 1995; Bigler & Liben, 2006; Hilliard & Liben, 2010; Park & Rothbart, 1982; Tajfel & Turner, 1986). A synopsis of ideas from this literature suggests that, in attempting to function efficiently, people find ways to reduce the complexity of the situations around them. In the real world, people are literally bombarded by stimulation coming from numerous, diverse sources. Obviously, people cannot respond to all these stimuli simultaneously or even successively. If they tried to do so they would never get anything done, and this outcome certainly would not make humans very adaptive organisms: People would devote the major share of their lives to processing all the nuances of all the dimensions of the world before they acted as a consequence of stimulation. Consequently, people attend to some stimuli in their environment and disregard others, depending in part on what information most richly, reliably, and validly tells people what they need or want at that time and in that situation. In this way, people can be economical and efficient in their social interactions.

A person is one type of stimulus object encountered in the ecology of human development (Bronfenbrenner, 1979, 2005; Bronfenbrenner & Morris, 2006). But a person is also a complex stimulus having many observable dimensions (sex, age, race, style of dress, apparent status, etc.), and people cannot respond to all characteristics of another person at once if they are to be efficient and economical in their actions in the world. In order to be economical and efficient, people are likely to attend to the fewest dimensions of a person needed to provide them with the information required for effective actions-in-context. Depending in part, for example, on the type of information they need in order to function efficiently at that moment, people may attend only to specific stimulus attributes, or cues. On the basis of these cues, people place other people in specific categories; that is, they associate people's specific stimulus attributes with specific categories of information, behavioral characteristics, or social attributes. By doing this categorization, people need only respond to a specific few dimensions and this reduction in complexity allows humans to function efficiently and economically.

This process of categorization is a very basic one, permeating all social relations. Of course, person perception and categorization is a developmental process (Leaper, 2015). However, to illustrate the role of social stereotypes along the continuum of indirectness, I will use examples based on adult individuals. For instance, consider a person lost in a big city. It might be a successful, but a relatively inefficient, strategy to stop and ask randomly-encountered people how to get to a specific location. But if the person saw another person wearing a uniform and a badge standing by an intersection, he or she might respond by placing that person in the category of police officer. The person would then attribute to that other person the possession of specific information (e.g., knowledge of directions); the person would ask for this information, likely accurately be given it, and then be on his or her way.

Thus, whenever people perceive other people:

1. They respond to specific stimulus attributes, or cues, they possess (in order to maintain economical interpersonal relations).
2. On the basis of these cues, people place these other people into specific categories.
3. On the basis of this categorization, people attribute to these other persons the possession of specific information or specific characteristics of behavior.

Anastasi suggested—and from much accumulated evidence (e.g., see Bigler & Liben, 2006; Hines, 2015; Hilliard & Liben, 2010; Lerner & Korn, 1972; Secord & Backman, 1964) it seems clear—that one major type of cue that people readily use in organizing their interpersonal perceptions and interactions is physical characteristics, such as sex, that are least indirectly hereditarily determined. Thus, it is probable that in some societies (and here I am intentionally understating my argument) people are categorized on the basis of specific inherited physical characteristics. If this stereotyping occurs, people will probably make specific invariant personality attributions and maintain specific invariant behavioral expectancies for all people placed in that category. People may be expected to show this invariance because, as I have argued, the reason that people categorize in the first place is to tell them efficiently what to expect about that class of people–stimuli. It would defeat the purpose of economical categorization processes to admit exceptions to these attributions.

What may be the effects of categorizing people on the basis of inherited physical characteristics? Answering this question indicates how such inherited characteristics provide the most indirect hereditary source of behavior: social stereotypes. To address this question, though, I offer a not-too-imaginary example.

Suppose that there is a society that has, as a most salient cue for the categorization of people, a specific inherited physical characteristic: skin color. Now, for argument's sake, imagine further that one of the two skin-color groups in this society is categorized unfavorably. That is, people in that group, when put into this physically cued category, receive negative behavioral expectations and personality attributions—for example, they are thought of as unable to profit very much from educational experiences or progressive interventions (e.g., as in Belsky, 2014). Certainly, at least some people in this imaginary category could probably benefit from education and are not lacking plasticity sufficient to gain from programs aimed at academic enhancement (e.g., Heckman, Humphries, & Kautz, 2014; Lerner, 2015b, 2015c, 2015d), but it is likely that such categorizations would be maintained despite experience of such exceptions. If this stereotype maintenance is the case—that categorization involves an overgeneralized belief or attitude—then such a categorization is a *stereotype.* Thus, it is possible that, in response to a physical attribute, people place other persons in a category and in so doing maintain stereotyped expectations about these other people.

If a skin-color group were stereotyped as uneducable, it would not make sense to put much effort into attempting to educate people of that group. Because people would not expect the stereotyped group to learn very much, they would not spend much money on their schooling. In fact, such a group might have a history of going to inferior schools where there were inadequate facilities and poorly qualified teachers. Thus, because of the stereotype, this skin-color group would experience inadequate, inferior, or substandard educational opportunities.

Finally, years later, a researcher might come along and decide to see if the categorization of these people involves an overgeneralization. He or she finds that this group does not seem to be doing very well educationally: Many people in this group do not score high scholastically, do not seem to have intellectual aptitudes as high as those of members of the other skin-color group, often do not go on to higher education, and accordingly do not often enter into the higher-prestige, higher-salary, and higher-socioeconomic-status professions. Thus, the person doing this study might conclude that the facts show that this skin-color group cannot profit to any great degree from educational experiences. Many of those in the favored skin-color group of this imaginary society, who may often have made such an attribution about those in the less-favored skin-color group, might say that they “knew it all along.”

But my analysis of the situation is certainly different. I believe that what occurred with this stereotyped skin-color group in my example was as follows:

1. On the basis of their relatively direct inheritance of a physical characteristic—their skin color—people in this group were placed in a specific unfavorable category.
2. In turn, on the basis of this categorization, specific negative behavioral expectations were invariably attributed to members of this category. (I might suggest here that a basis of both the initial categorization and the concomitant attributions and expectations might lie in the social and economic history of this group, e.g., see Sampson, 2016).
3. These attributions were associated with differential experiences and opportunities (different when compared to the society’s other skin-color group).
4. These differential situations delimited the range of possible behaviors that this group could develop. In other words, the group was channeled into a selected, limited number of behavioral alternatives—the very same behaviors they were stereotypically held to have.
5. Finally, many members of the group developed these behaviors because of the above channeling. That is, the end result of the physically cued social stereotype was a *self-fulfilling prophecy.*

In sum, on the basis of a physically cued categorization, people may make a stereotypic attribution and, accordingly, channel the people of that category into specific behavioral patterns by creating social situations within which they cannot do other than develop along the lines of the social stereotype. Social stereotypes about directly inherited physical cues may have a very profound effect on behavior, therefore. They may result in self-fulfilling prophecies.

Unfortunately, of course, the example that I have just presented is not imaginary at all (e.g., see Spencer, 2006; Spencer et al., 2015). Although this social-stereotype effect on behavior can obviously function either favorably or unfavorably for categorized people, this illustration reflects the most pernicious example of the effect of social stereotypes.

From the analysis of the example I provided and, as well, from empirical literature in support of it (e.g., Spencer, 2006; Spencer et al., 2015), a strong argument can be made that the people of color in the United States have perhaps experienced the most unfortunate effects of this most indirect type of hereditary contribution to behavior–social stereotypes. Thus, it may be that African Americans for many years have been involved in an educational and intellectual self-fulfilling prophecy in the United States. This possibility is an important concept in Chapters 11 and 12 when the controversy about the nature and nurture of racial differences in intelligence is discussed (e.g., Gould, 1980, 1981; Hebb, 1970; Herrnstein, 1971; Herrnstein & Murray, 1994; Jensen, 1969, 1973, 1980; Joseph, 2015; Layzer, 1974; Richardson, 2017).

At this point, however, it suffices to say that social stereotypes certainly may be a potent source of development. Although this source is the most indirect hereditary contribution to behavior, it may, nonetheless, play a ubiquitous role in development. I have used the example of skin color to illustrate the effects of physically cued social stereotypes, but other, more subtle or more obvious, examples could be mentioned—for example, sex, hair texture, shape of nose, breast size in women, or body build and physical attractiveness (e.g., Bigler & Liben, 2006; Graham, 1992; Hines, 2015; Hilliard & Liben, 2010).

In sum, up to this point, I have considered the implications of Anastasi's suggested four points along the theoretical continuum of indirectness, the continuum along which she believed that heredity contributes to behavior. But the major implication of the question "how?" is that nature coacts with nurture to affect developmental changes. As such, it is important to also consider the ways in which the environment contributes to behavioral development. In so doing, I can continue to use Anastasi's (1958) article as a model. Here, another continuum—the environmental continuum or, as Anastasi conceptualizes it, the "continuum of breadth"—is relevant.

Nurture: The Continuum of Breadth

Continuing my use of the moment of the opposites of integration discussed in Chapter 2 (Overton, 2015a), I consider environmental factors, and note that, just as the effects of heredity on behavior can best be understood in relation to the environment, the effects of the environment on behavior can best be understood in relation to the nature of the organism. Anastasi (1958) conceived of the environment as making its contribution to behavioral development along a continuum of breadth. In other words, environmental factors vary in terms of their pervasiveness of effect on behavior.

Some environmental factors, then, may be seen to have very broad, pervasive effects on a person, relating to many dimensions of functioning and enduring in their contributions for relatively long periods of time. Alternatively, other environmental factors may have narrow, minimal effects, making their contributions only to small or limited segments of a person's behavior and exerting their influence for relatively short, transitory periods. Such a continuum of breadth is illustrated in Figure 6.4. Those environmental effects that are derived from the left end of the continuum exert narrow, minimally pervasive effects on behavior, whereas those at the right end are broad and maximally pervasive in nature.

But what are examples of environmental effects? Just what sort of variables are there in the environment that contribute to behavioral development along such a continuum of breadth? Anastasi suggested two general categories of environmental effects.

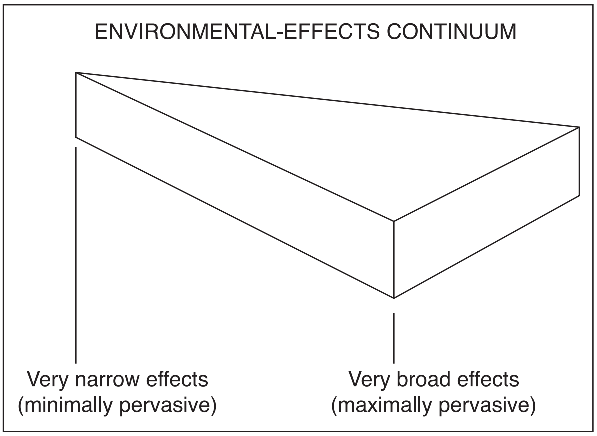


Figure 6.4 The contributions of environment to behavioral development vary along a continuum of breadth.

Organic Effects

The first category of environmental effects may be labeled *organic*. There are some environmental occurrences that lead to changes in the makeup, the constitution, of the organism; they affect what the organism has and how it functions. In short, these factors change the constitution of an organism's physical and/or physiological processes.

A person may encounter environmental variables that affect the organic makeup of the body either through contracting a disease or having an accident. However, the eventual behavioral outcomes of such organic changes may, in turn, be either broad or narrow in nature. For example, losing half of one's cerebral cortex in an auto crash, or an arm or a leg, or having permanent facial scars after a fire may all be considered environmentally mediated changes in a person's organic makeup that may have pervasive, enduring effects on behavior. Alternatively, loss of a single finger or toe, while being an organic change, would probably not have as great an effect. Moreover, accidents such as stubbing one's toe certainly affect behavior, but in an obviously more trivial, narrow, and transitory way.

Disease may also be broad or narrow in its behavioral contributions. Contracting a disease such as polio, sickle cell anemia, or muscular dystrophy would certainly have a very pervasive effect on a child's development and behavioral functioning. The range of behaviors in which children with such disorders can engage may differ greatly from that of children not so affected. On the other hand, some diseases, although affecting the makeup of the organism, do so only in limited or short-term ways. Thus, catching a cold or contracting a childhood disease such as chicken pox would affect behavior but, if adequately treated, only for a minimal amount of time and probably in a not too pervasive way.

Of course, the environment can contribute to development through organic changes in ways that do not have to be construed as negative. Environmentally based organic changes that facilitate or improve behavioral functioning, rather than deteriorating it, can be induced. Such factors as changes in diet, climate, physical regimen, or medical treatments can result in changes in organic makeup that may have positive effects on behavioral functioning and development (Halfon et al., 2018; Zuckerman & Keder, 2015).

Stimulative Effects

The second category of environmental effects on behavior may be termed *stimulative*. These effects are environmental events that act as direct stimulative influences on behavioral responses. Here, too, such variables may be broad or narrow in their contributions to behavior. Perhaps the broadest stimulative environmental variable is culture (Cole, 2006; Mistry & Dutta, 2015; Raeff, 2016; Rogoff, 1998, 2003, 2011). Differences among cultures may pertain to variation in values, modes of living, technology, language, religion, presence of material goods, and availability of educational opportunities. For example, a child growing up in an industrialized, Western nation is exposed to vastly different qualitative and quantitative experience stimuli than is a child living in a nation in the majority world or Global South (Lerner, 2015d; Lerner et al., in press). The differential resources and experiences associated with these contrasting cultural settings permeate all aspects of the developing child's world, and serve to shape his or her varying behavioral repertoires.

A somewhat narrower, less pervasive stimulative influence may be, for example, the college experience (Callina et al., 2017). Events in this specific environment certainly shape intellectual, attitudinal, and behavioral repertoires among students, and such cognitive and behavioral repertoires are, in turn, probably different in nature from those found among young people not exposed to the college experience (Callina et al., 2017; Hamilton, 1994; Hamilton & Hamilton, 2009; Sherrod, Haggerty, & Featherman, 1993). Finally, some stimulative influences are exceedingly narrow, trivial, and of short duration in their contributions to behavior. Such minimally pervasive effects are numerous, occurring daily in our interrelations in the real world. Thus, having a particularly rude, discourteous cab-driver or salesperson may affect a person momentarily, but probably not to any great, enduring extent.

In sum, then, Anastasi suggested that the effects of the environment on behavioral development vary in their pervasiveness. Whether these effects are organic or stimulative in type, they present a range of environmental influences that will coact with indirect hereditary contributions to provide the source of development. But, as was the case with hereditary influences on behavior, such environmental effects do not have direct impact on development. Rather, just as the effects of nature on development are influenced by nurture, environmental contributions to behavior are influenced by the nature of the organism.

Here, then, I need to return to the third moment of developmental research discussed by Overton (2015a), and focus on the synthesis of wholes. From this view, the same environmental event (e.g., contraction of a disease or exposure to a particular college course) or group of events (e.g., those associated with middle-class as opposed to upper-class membership) will lead to different behavioral outcomes depending on the nature of the organism.

Using the same symbols as in Figure 6.1, this view is illustrated in Figure 6.5. Environmental (nurture) determinists might argue that it is possible to conceptualize the contribution of environment to behavior as being direct. From this perspective, it would be possible to assert that a specific environmental event or set of events (E_1) is seen as directly leading to a specific behavioral outcome (B_1). However, as with the former argument regarding nature contributions, I believe that view is not tenable. I have argued for a dynamic, integrative view of nature and nurture, and the environmental contribution component of this view is illustrated in Figure 6.5. Here the same environmental contribution (E_1) can be associated with an array of behavioral outcomes (B_1 to B_n) as a consequence of coactions with organisms having different natures (G_1 to G_n). A basis of *plasticity* (the potential for systematic change) in development is thus promoted (Lerner, 1984, 2012, 2015a), and this plasticity may be illustrated in several ways.

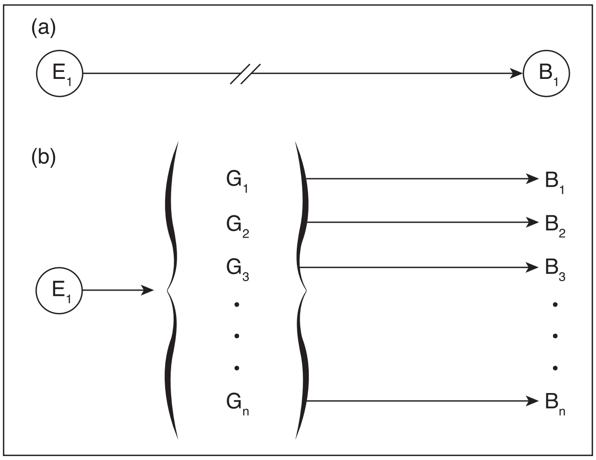


Figure 6.5 (a) Environment (*E*) does not directly lead to behavior (*B*). (b) Rather, the effects of environment on behavior (*B*) will be different in interaction with organisms having different heredities (*G*).

First, consider a very general set of experiential events associated with being a child of upper-middle-class parents. Imagine that such parents had two children who were *dizygotic twins*, also termed *fraternal twins.* Such siblings are born of the same pregnancy but are from two separate ova that are fertilized at the same time. Thus, although born together, these siblings have different genotypes (unlike monozygotic twins). If one of these twins was born with the genetic anomaly discussed earlier (Down syndrome) but the other was born with a normal complement of genes, a situation would result wherein two children, born of the same parents at the same time, would also potentially be exposed to the same environmental events.

However, regardless of the experiences encountered by the twin with Down syndrome, the effects of those experiences could not be expected to result in behaviors falling within a range identical to that of the sibling. Despite advances in special education noted earlier, one would not expect a child with Down syndrome to have the same potential level of intellectual aptitude or academic achievement as his or her sibling. For instance, one would not expect the child with Down syndrome to attain a vocation such as physicist, neurosurgeon, or professional basketball player. Such expectations might, however, be appropriately maintained in regard to the sibling born with the normal genotype. Thus, the hereditary nature of the organism imposes limits on the possible contributions of environment.

Other illustrations of this relation may be drawn from the information presented above about the prenatal maternal environment (e.g., Wang et al., 2018). I noted that if the mother contracted rubella during pregnancy, adverse physical and functional outcomes for the infant might follow. However, this same experience (contraction of rubella) may or may not lead to these outcomes depending on the maturational level of the organism. If the experience occurs during the embryological period, these negative effects are more likely to occur; if it happens in the late fetal period, problematic effects of the disease are less likely to happen. Similarly, maternal stress and depression will or will not be more likely to lead to specific congenital anomalies depending on the maturational level of the organism (Halfon & Forrest, 2018). Thus, here again, the nature of the organism moderates the influence of experience on development.

I conclude, then, that even if very narrow sorts of environmental experiences (e.g., meeting a specific person in a brief social encounter) or very broad types of experiences (e.g., those associated with membership in one culture versus another) are being considered, the effects of these environmental influences would not be the same if they interrelated with hereditarily (genotypically) different organisms. Similarly, the effects would not be the same even if it were possible to ensure that the different organisms had identical experiences. As long as the nature of the organism is different, the contributions of experience will vary.

It is important to note that there is enormous genetic diversity among humans. For instance, Hirsch (2004) estimated that the chances are one in 6.27 billion that the same genotype will arise across pairings of individuals. In other words, there are over six billion potential human genotypes. This estimate suggests that it is quite unlikely that any two humans share the same genotype, with the possible exception of identical (MZ) twins. However, even for MZs, the differences in the experiences they encountered—differences that began as soon as their respective zygotes were implanted at different points on their mother's uterine wall—contribute to their diversity (see Joseph, 2015). I should note, however, that overall genotype uniqueness exists simultaneously with a great deal of genetic commonality across *all* people. Any two people taken at random from the world's population will differ only in about three of 1,000 base pairs of DNA (Charney, 2016; Gottlieb, 1998; Gottlieb et al., 1998; Wahlsten, 2012, 2013). Accordingly, human heredity involves an integration of genetic commonality and genetic uniqueness.

This argument is underscored by noting that a genotype immediately becomes a phenotype at the moment of conception. The genotype is expressed in one and *only* one intrauterine environment. Hence, although a norm of reaction exists for the genotype, that is, for any given genotype, there is a range of possible phenotypes that may arise from it, once the genotype is expressed in one particular context, all the other alternative phenotypes that *could* have resulted from the given genotype are excluded. Thus, even identical twins become (at least slightly) phenotypically different from each other at the moment of implantation, and these differences become greater across ontogeny (Joseph, 2015; Lickliter & Honeycutt, 2015), especially given the distinct courses of individual↔context relations encountered by the twins and the differences in epigenetics that this variation might engender (Cole, 2014; Lester et al., 2016; Meaney, 2010, 2014; Moore, 2015a, 2016; Slavich & Cole, 2013).

Conclusions about the Implications of Anastasi's Contributions

Because of genotypic uniqueness, all individuals will have different relations with their environments (be they the same or different) in unique, specific ways. Thus, the environment always contributes to behavior, but the precise direction and outcome of this influence can only be completely understood in the context of an appreciation of the genetic (and epigenetic) individuality of the person. In turn, individual differences in genetic makeup do not, in and of themselves, directly shape behavior (e.g., Jablonka & Lamb, 2005; Slavich & Cole, 2013). Integrated coactions with an environment, itself having a host of distinctly individual features, have to be taken into account.

In other words, heredity and environment, nature and nurture, are always present and involved in providing a source of human development. The specific indirect contribution of nature can be understood only in the context of the particular broad-to-narrow contribution of nurture with which it is coacting.

In turn, an exact understanding of how a specific environmental contribution affects behavior can be reached only by understanding how it interrelates with the organism's nature. Thus, in trying to conceptualize how nature and nurture coact, Anastasi (1958) relied—as have I—on the *norm-of-reaction* concept as useful in conceptualizing the fused influences of nature and nurture.

THE NORM OF REACTION

The concept of norm of reaction has been a popular and useful one for geneticists since it was introduced in the early part of the twentieth century by Woltereck (Dunn, 1965). To understand the concept, recall that what humans inherit from their parents, what parents transmit to offspring when fertilization occurs, is a particular set of genes. This genetic endowment, or genotype (Hirsch, 1963, 2004), represents the hereditary-developmental potential for all the eventual physical, physiological, and behavioral characteristics of all individuals. However, there is not a one-to-one relation, an *isomorphism*, between genotype and the eventual characteristics individuals develop. That is, the genotype does not represent a genetic blueprint; indeed it is not possible to specify how a specific genetic contribution will manifest itself merely by knowledge of the genotype. As I have noted, no human characteristic is ever directly inherited. The eventual manifestation of an individual's genotype—how the genotype will express itself when specific attributes develop—depends on the integrated coaction (fusion) of that genotype with the environment. Of course, this expression will vary under different environmental conditions.

Thus, the attributes of the developing person are the product of the coaction of the environment with the person's genotype. As already noted, what is observable is the phenotype. Therefore, even despite genotypic invariance, because phenotypes can be expected to be different in varying environmental conditions, what genetic inheritance actually represents is not a predetermined, inevitable blueprint of our eventual characteristics. The phenotype is neither a mere replica of the genotype nor is it isomorphic with it. Rather, genetic inheritance represents *a range of potential outcomes*, and the developmental outcome that eventually manifests itself will occur due to coactions of the environment within this range of genetic potential.

This idea, then, is the *norm-of-reaction* concept: "The same genotype can give rise to a wide array of phenotypes depending upon the environment in which it develops" (Hirsch, 1970, p. 73). In other words, the genotype—heredity—*could* be part of a developmental process that results in a range of possible outcomes, or phenotypes. A *phenotype* is an outcome of a specific coaction between a specific genotype and a specific set of contextual conditions. Accordingly, the specific phenotype that does occur is an outcome of the features of the specific environment within which the specific genotype coacts across ontogeny. In short, the norm-of-reaction concept asserts that genetic inheritance constitutes a potential for a range of phenotypes, but the eventual phenotype that does develop will depend on the specifics of the environment coacting with heredity.

In a sense, then, this relation reflects Overton's (2015a) first research moment—the integration of opposites. It may be best illustrated by M. C. Escher's famous lithograph, *Drawing Hands*. There is both a right hand and a left hand depicted in the work. One hand is drawing the other and, therefore, although they are opposites, they are integratively coacting (Overton, 2015a). So too are genes and context in the relational developmental system, a relation specified by the norm-of-reaction concept.

Before assessing some of the implications of this concept, another example may be useful to illustrate the meaning of the concept. I have suggested that the genotype may be conceived of as a range of potential behavioral outcomes, as the hereditary upper and lower limits for the development of a specific structural or functional (physiological, psychological, or behavioral) attribute of an individual. For purposes only of illustration, I will suggest a counterfactual situation; that is, suppose some researcher claimed that he or she could estimate that a given child's genotype for intelligence had a range from a low of 70 to a high of 130 IQ points. As I shall explain, such a supposition is flawed for both conceptual and methodological reasons (e.g., Hirsch, 2004; Moore & Shenk, 2016; Richardson, 2017; see too Chapters 11 and 12). Simply, the claim I am using in this example is meaningless and devoid of empirically real referents. Nevertheless, I want to continue using the example to illustrate the functional significance of the concept of norm of reaction. Thus, returning to the example, suppose further that this researcher went on to ask: "What will the child's measured IQ be? In other words, what will the phenotypic IQ be?"

Any score for intellectual ability, such as an IQ score, will depend, of course, on the specifics of the environment in which the child is reared. If the child was reared for the first 12 years of his or her life in highly adverse circumstances, for example, a neighborhood marked by poverty, social disorganization, environmental degradation, crime, high rates of adult incarceration, and racial prejudice (e.g., see Sampson, 2016), then the phenotypic IQ would likely fall near the lower limit set by the child's genotype in this imaginary example, most likely near 70. However, a different phenotypic IQ might be expected if it was possible to take another child with the same exact genotype and rear him or her in a more stimulating environment, say in the home of a professional couple who provided facilitative general learning tools and excellent language models, and who fostered high achievement motivation (see Bloom, 1964). A child reared in such an environment would be likely to have a measured IQ near the upper limit of the norm-of-reaction range used in the imaginary example.

Although both children in the example had the same exact genotypic range for intelligence, their phenotypic intelligence would be quite different because of their markedly varied rearing environments. In essence, then, the phenotype for any observed characteristic does not depend solely on the person's genotype, or genetic endowment; rather, the phenotype is the end result of a complex coaction involving the environment within the genotypic range of potentials, represented by that person's norm of reaction. However, given the enormous diversity of human genotypes that exist (recall Hirsch's, 2004, estimate of more than six billion potential genotypes), the imaginary researcher could not conduct the comparison I described in the example. In fact, he or she could not even know the norm of reaction for *any individual child*.

Psychologists other than Anastasi (e.g., Hirsch, 1963, 1970; Schneirla, 1957) recognized the utility of the norm-of-reaction concept in conceptualizing the nature of heredity–environment relations. Hebb (1949), for instance, offered a conceptualization of intelligence consistent with the notions implicit in the norm-of-reaction concept. He imagined that humans were endowed with a range of intellectual potential, a genotypic intelligence. Although such a conception suggests a split between genes and environment in the foundation of intelligence, Hebb's concept actually illustrates the problems associated with such a split. He termed the "inherited" range of intellectual potential *Intelligence A*. However, as I just noted, researchers have not devised and, as will also be noted, cannot devise a means of assessing Intelligence A; that is, there is no technique to appraise a person's genotypic intelligence. Thus, even if Intelligence A existed—which it does not—there is no way to know it. Rather, what can be measured is only what Hebb termed *Intelligence B*—the outcome of a person's history of individual↔context relations within his or her norm of reaction. Thus, this phenotypic intelligence, Intelligence B, is a measurement of the result of the coactions between environment and heredity. However, how this result, which can be measured, reflects either the imagined *Intelligence A* or a specific point in an individual's norm of reaction cannot be measured and, as such, remains unknowable.

To the extent, then, that Hebb's (1949) notions are tenable, the norm-of-reaction concept as it applies to this conceptualization of intelligence suggests the following:

1. Even if there were an entity such as a genotypic intelligence (Intelligence A), researchers do not and cannot measure it; no means exist to measure this hypothetical construct (see Layzer, 1974).
2. Another type of intelligence does exist, however, and it can be measured. It is the product of coactions between the person's environmental history and genes. This second type of intelligence (*Intelligence B*, in Hebb's terms) represents the phenotypic intelligence of the person.
3. This phenotypic intelligence is what is measured by IQ tests.
4. However, the genotype–phenotype intelligence correspondence remains unknown; that is, even if the genotype represents a range of possible

intellectual outcomes, whether the phenotype represents a low, middle, or high point within this range remains unknown. The person has been endowed with a specific genotype and, through environmental coactions, this genotype has provided a basis for the person's phenotype; however, whether this environment↔heredity relation led to a phenotype that reflects the high or low part of the person's genotype cannot be assessed (again, see Layzer, 1974, for detailed mathematical reasons).

5. Finally, all this reasoning suggests that, given another environmental history, the same genotype could be expected to have led to a different phenotype (Hirsch, 2004). Still, however, the portion of the norm of reaction to which this new phenotype related would remain unknown.

A key implication of Hebb's ideas about Intelligences A and B is that, although researchers may expect the same genotype to lead to different phenotypes in different environments, what portion of the genotype is reflected by a specific phenotype remains unknown. This point suggests that the norm-of-reaction concept has limitations, and it is important that these limitations be made clear.

Limitations of the Norm-of-Reaction Concept

The relation between genetic endowment and behavior has been a continuing research and theoretical concern of Jerry Hirsch (e.g., Hirsch, 1970, 1981, 2004). He has argued that, although there was a norm of reaction associated with the observable outcomes of an individual's ontogenetic development (i.e., a person's phenotype), this range was not predictable in advance. In other words, before the person has developed, it is impossible to say that, because of his or her genotype, and given specific environmental manipulations, one type of phenotype will develop, whereas given other environmental circumstances another phenotype will result. In essence, at the human level there is really no way to directly assess the expected range of phenotypes that can be associated with a given genotype. At best, researchers can only make statements about particular genotype–environment coactions *after* they have occurred.

In fact, at any level of life organization (from fruit flies, *Drosophila*, to humans, for instance), the norm of reaction remains largely unknown in most cases (Hirsch, 1970). This situation is so because, in order to be able to exactly specify the norm of reaction for any living animal (or plant, for that matter), one must be able to reproduce exactly—to clone—an individual, specific genotype many times. In effect, one must be able to reproduce several genetically identical organisms. These replicated (cloned) genotypes must then be exposed to as diverse an array of environments as possible. The range of phenotypes that develop from these exposures would give an *estimate* of the norm of reaction for *that specific genotype* (only). Ideally, this exposure should be totally inclusive of all possible environmental conditions to which the genotype might be exposed. Of course, in reality such an infinite exposure could only at best be approximated, so the most that can be done is to offer an approximation of the norm of reaction for any one genotype. I agree, then, with Hirsch's (1970) conclusion:

> Even in the most favorable materials only an approximate estimate can be obtained for the norm of reaction, when, as in plants and some animals, an individual genotype can be replicated many times and its development studied over a range of environmental conditions. The more varied the conditions, the more diverse might be the phenotypes developed from any one genotype.
> (pp. 69–70)

Further clarifications of the norm-of-reaction concept need to be made. Hirsch (1970) pointed out that different genotypes should not be expected to have the same norm of reaction. The norm of reaction associated with each individual genotype can be expected to be differentially unique—that is, differentially broad or narrow. Therefore, the range of phenotypes that would develop from a specific genotype under varying environmental conditions can be expected to differ from individual to individual. In addition, it is likely that across all people walking this earth, including identical—monozygotic—twins (Joseph, 2015; Lickliter & Honeycutt, 2015; Richardson, 2017), no two people have the same history of individual↔context relations and thus the same phenotype.

To illustrate how genetic endowment provides a basis of the uniqueness of each human life and provides substance for the claim that all humans have a unique heredity↔environment coaction history (Hirsch, 1970, 2004; Lerner, 1978, 1979; McClearn, 1981; Richardson, 2017), consider that estimates of the number of structural genes (i.e., genes that code for proteins) in humans range between 50,000 and 100,000 (e.g., Bodmer & Cavalli-Sforza, 1976; Gottlieb, 1998; Stern, 1973). Although, as I have emphasized above, humans are much more genetically similar than dissimilar, it is also the case that if one considers how much genotypic variability can be produced by the reshuffling process of meiosis occurring with 100,000 genes, then the potential for variability is so enormous that "it is next to impossible that there have ever been two individuals with the same combination of genes" (McClearn, 1981, p. 19).

Indeed, I have noted already the estimate by Hirsch (1970) that there are over six billion potential human genotypes. Bodmer and Cavalli-Sforza (1976) provided further information about the genetic variability associated with each human by estimating that each human had the capacity to generate 10^{3000} different eggs or sperm. In comparison, Bodmer and Cavalli-Sforza estimated that the number of sperm of all men who had ever lived was *only* 10^{24}. Accordingly, McClearn (1981) noted:

> If we consider 10^{3000} possible eggs being generated by an individual woman and 10^{3000} possible sperms being generated by an individual man, the likelihood of anyone ever—in the past, present, or future having the same genotype as anyone else (excepting multiple identical births, of course) becomes dismissably small.
>
> (p. 19)

Moreover, the character of human genetic variability is highlighted if developmental scientists recognize that "genetic" does not mean "congenital"; that is, that the "total genome is not functioning at fertilization, at birth, or at any other time of life" (McClearn, 1981, p. 26). The expression of any individual human genotype is a developmental phenomenon, influenced in regard to the turning on and/or off of genes by the endogenous and exogenous components of the individual's genotype↔environment coaction history (Jacob & Monod, 1961; McClearn, 1970, 1981; Schaie et al., 1975), that is, by his or her history of epigenetic changes (e.g., Cole, 2014; Lester et al., 2016; Meaney, 2010, 2014; Moore, 2015a, 2016; Slavich & Cole, 2013). For instance, McClearn (1981) notes:

> Different genes are decoded and come into play at various times during the lifetime of a particular organism. One illustration of this phenomenon is the differential production of certain kinds of hemoglobin during various phases of development. For example, production of the beta chain accelerates at the time of birth and peaks after a few months, whereas production of the alpha chain rises prenatally and maintains a high level.
>
> (p. 26)

As Hirsch (1970, 1999, 2004) explained, across the life span, norms of reaction will individualize even identical genotypes (see too Lickliter & Honeycutt, 2015; Phelps et al., 1997), and, given the evidence I have just reviewed, even if one simplifies the situation enormously in order to make an estimate, few people will have the same genotype—much less identical norms of reaction across life (Hirsch, 1970, 1999, 2004).

Because of this uniqueness, all individuals will coact with their environments in unique, specific ways; there will be idiographic trajectories of individual↔context relations (Nesselroade & Molenaar, 2010; Molenaar & Nesselroade, 2014, 2015; Rose, 2016; Rose et al., 2013). This assertion points to the necessity of trying to determine individual (idiographic) laws of human behavior—laws that account for the individual's unique pattern of development within his or her environment. In turn, Hirsch's (1970, 2004) argument suggests the futility of attempting to specify general "laws of environmental influence" or of attempting to account for all the variations in human behavior merely by recourse to invariant, overt environmental stimuli and responses.

There are two important implications here: First, each individual is genotypically unique and will coact differently in a given environment than will other, genotypically unique people. Second, a complete focus on the environment in an attempt to account for all behavioral variation is both misguided and incorrect. These implications reflect my discussions in Chapter 4 of organismic and mechanistic theories, as well as my analysis of the ideographic (ipsative) approach to human development discussed in Chapter 5. I believe, then, that across the prior chapters of this book, evidence has been presented for the (inescapable) fact of human uniqueness. This fact is derived from an appropriate understanding of the genetic basis of (or contribution to) individuality.

At this point, then, I can summarize our discussion of the norm-of-reaction concept by stating what it does and does not say about how nature and nurture coact to produce human development.

1. Heredity alone does not determine behavior. An isomorphism does not exist between a genotype and a phenotype.
2. Rather, the way that genes function is to contribute to (that is, not determine) a range of possible outcomes of development. These outcomes will result from the varying specifics of the coactions between the environment and the genotype, and different phenotypes can be expected to result from different coactions. Epigenetics, and not just genetics alone, is involved in influencing the course of an individual's development.
3. The norm of reaction cannot be predicted in advance and, on the human level, it cannot even be well estimated or approximated.
4. Therefore (and this is a crucial point), in actuality, those limits set by our hereditary endowment, by our genotype, can never be specified (Hirsch, 1970, p. 70). Scientists cannot reproduce individual human genotypes and expose them to all possible environmental situations. Because of this fact, scientists cannot know any given individual's range of genetic potential.
5. But what scientists *can* do is to recognize that the norm of reaction is unique with each individual and, therefore, since it can be expected to vary from one individual to another, individuals will coact idiographically with their environments. This process will result in basic phenotypic uniqueness among people.

Thus, the norm-of-reaction concept highlights the necessity of focusing on the coaction of nature and nurture in order to understand development. This concept's implications illustrate that "extreme environmentalists were wrong to hope that one law or set of laws described universal features of modifiability. Extreme hereditarians were wrong to ignore the norm of reaction" (Hirsch, 1970, p. 70).

CONCLUSIONS

In this chapter, I have considered general concepts in the nature–nurture controversy and have dealt with some of their rather broad implications. At the beginning of this chapter, however, I indicated that the nature–nurture issue is still very much alive at this writing and still "rears its head" in many currently researched and contested content areas of human development (Lerner, 2015b, 2015c, 2016). In order to illustrate how the concepts discussed in this chapter and prior ones may be appropriately applied to specific topics, I turn in subsequent chapters to discussions of several of these topics—intelligence, the notion of instincts, and the ideas associated with the areas of sociobiology, behavior genetics, and evolutionary developmental psychology. The discussions in these chapters are preceded by—or framed within—a fuller explication of the approach to the nature–nurture issue that I have termed probabilistic-epigenetic, and by an account of how this approach fits within the broader sets of ideas associated with RDS-based theories.

However, prior to these illustrative discussions, I need to turn first, in Chapter 7, to a second set of key ideas pertinent to understanding how integrative coactions involving nature and nurture exist across all levels within the relational developmental system. I discuss contributions to developmental science made by the renowned comparative psychologist, T. C. Schneirla.

CHAPTER SEVEN

Toward Resolving the Nature–Nurture Controversy

Contributions and Implications of the Scholarship of T. C. Schneirla

T. C. Schneirla (1902–1968) was trained in comparative psychology, receiving a Doctor of Science degree from the Department of Psychology at the University of Michigan in Ann Arbor. He left Ann Arbor in 1927 to take a position at New York University, and remained on the faculty of that institution until his death. However, beginning in 1943, he also became a member of the staff (starting as an associate curator) of the American Museum of Natural History. His theoretical and empirical work at both NYU and the museum provided developmental science with important conceptual tools for understanding how multiple—biological through ecological—levels of organization are integrated within a developmental system that propels an organism across the course of life.

As such, Schneirla's theoretical ideas constituted a foundation for understanding the dynamic fusion of nature and nurture variables in development. Schneirla's ideas, therefore, provide a frame for a discussion of probabilistic-epigenetic (or developmental-contextual) ideas about human development. In addition, his ideas provide a means to critique theoretical ideas that split nature from nurture and/or stress the primacy of either biological or contextual influences in human development (Overton, 2015a).

To understand the contributions of Schneirla's ideas, it is useful to recall that, across history, there have been instances in which theorists have emphasized the independent, isolated action of either hereditary variables (Sheldon, 1940, 1942) or environmental variables (Skinner, 1938; Watson, 1913, 1918b) for some selected subset of an organism's behavioral repertoire. Although, at this writing, due to the impact of essays by Schneirla (1956, 1957, 1966; Tobach & Schneirla, 1968) and also by

T. C. Schneirla

Anastasi (1958), Kuo (1967, 1976), Gottlieb (1970, 1992, 1997, 2004; Gottlieb et al., 2006), Greenberg (e.g., 2011, 2013, 2014, 2015; Greenberg, Partridge, Weiss, & Haraway, 1999), Lehrman (1953, 1970), Tobach (1981), and Overton (1973, 2006, 2015a), most developmental scientists acknowledge that variables from both nature and nurture sources contribute to development.

Agreement about the contribution of both nature and nurture to development, however, may be more apparent than real. Differences of opinion exist about the modes of contribution among variables derived from each of these sources; about the meaning and constitution of the contributing sources; and about how these differences are related to the alternative philosophical models to which human developmental scientists may be committed (see Chapter 4). Indeed, although the concept of "interaction" continues to be invoked by many theorists to indicate how variables providing the source of development relate to each other, I have explained in previous chapters (and see too Chapter 10) that the concept is controversial and that, from a relational developmental systems (RDS)-based perspective, the term should only be used when referring to the statistical concept of interactions. Instead, terms such as coaction or fusion should be used in statements describing the relations among variables within the relational developmental system. However, the present point about the term interaction is that how one defines the concept also depends on one's philosophical and concomitant theoretical orientation. Understanding the various philosophical and theoretical stances one may take in regard to the concept of interaction enables appreciation of the importance of T. C. Schneirla's contributions to the resolution of the nature–nurture controversy.

MOVING BEYOND THE CONCEPT OF INTERACTION

As I noted in Chapter 6, the debate about the nature–nurture controversy that emerged in the developmental science literature in the 1950s (e.g., Anastasi, 1958) was a debate that, in essence, contrasted split, reductionist accounts of the roles of heredity and environment with views that emphasized that variables from both (indeed all) domains of the relational human developmental system were inextricably related. Anastasi (1958) was a champion of the latter position, of course, and her formulation of the resolution of the controversy involved her use of the term "interaction" to depict the inviolable links between nature and nurture variables. For much of the more than half-century following her influential 1958 publication, other scholars also used the term interaction to discuss nature–nurture relations.

However, as RDS metatheory became increasingly more thoroughly detailed in the first decade or so of the twenty-first century (e.g., Overton, 2003, 2010, 2013, 2015a; see too the chapters in Overton & Molenaar, 2015), conceptual problems with the term "interaction" began to become clear and, as I have noted, at this writing, the term *interaction* is reserved for discussions of statistical associations by scholars using ideas drawn from RDS metatheory (e.g., Lerner, 2012; Lerner, Lerner, Bowers, & Geldhof, 2015). Coaction, fusion, integration, or other such terms are used to depict the substantive connection among variables and processes within the relational developmental system. Schneirla, in fact, used the idea of *fusion* to depict his conception of the developmental process. In a critique of J. P. Scott's (1962) concept of *critical periods* in development (a concept I discuss later in this chapter), Schneirla, joined by his colleague Jay Rosenblatt, noted that their view of development places an emphasis "upon the fusion of maturation (growth-contributed) and experience (stimulation-contributed) processes at different stages in behavioral ontogeny" (Schneirla & Rosenblatt, 1963, pp. 1112–1113).

It is useful to explain the evolution in thinking and terminology regarding the term *interaction*. This discussion enables me to highlight an instance of the prescient thinking of Schneirla. In addition, it allows me to underscore the distinctions between, on the one hand, the process-relational paradigm and RDS-based models of development and, on the other hand, models that follow from a Cartesian-split paradigm.

Past Uses of the Term "Interaction" within Attempts to Resolve the Nature–Nurture Controversy

It is possible to discuss the range of concepts involved in the use of the term *interaction* by placing ideas about interaction along a continuum that ranges from "weak" to "strong." Points along the continuum reflect differences in the extent to which one source of development (nature *or* nurture) is accorded primacy as an influence on development. The greater the emphasis placed on one source of development as the "prime" mover of change across ontogeny, the weaker the concept of interaction is being invoked. It is useful to begin the discussion of concepts of interaction, then, by considering such weak interaction notions.

Weak Interactions

To understand weak interactions, it is useful to consider the concept of interaction found in the nurture, mechanistic-behavioral view. Some psychologists, for example, Bijou (1976), argued that a person's development derives from an interaction between past reinforcement history and the current reinforcement context. Because the organism is the "host" (Baer, 1976), or locus, of the past reinforcement history, Bijou construed his concept of interaction as pertaining to organism–environment relations. Nevertheless, Bijou's (1976) view is that the organism is a largely passive component in the swirl of past and present reinforcements surrounding it. The organism plays no primary role in shaping the context that influences it.

In essence, Bijou (1976) follows Skinner's (1971, p. 211) view that "a person does not act upon the world, the world acts upon him." Moreover, to underscore the idea that the stimulus environment is the cause of human behavior—in both a phylogenetic and an ontogenetic sense—Skinner (1971, p. 214) went on to say that "An experimental analysis shifts the determination of behavior from autonomous man to the environment—an environment responsible for both the evolution of the species and the repertoire acquired by each member." Thus, to Skinner (1971, p. 205), "A scientific analysis of behavior dispossesses autonomous man and turns the control he has been said to exert over to the environment."

The components of the environment that, within this perspective, are in total control of human behavior, interact only in the sense that past and present stimulus-contingencies additively combine to influence behavior. Because these environmental influences are not qualitatively distinct, and because of the restricted role delegated to the organism in this form of organism–environment interaction, some reviewers (Lerner, 1978, 1985; Overton, 1973) have characterized the type of interaction illustrated by Bijou's (1976) position as a *weak* interaction.

The type of interaction found in many predetermined organismic-stage theories (see Chapter 5) may, as with the mechanistic-behavioristic tradition, be characterized as being of the weak variety. This situation is somewhat ironic because organismic developmental theory has been termed a *strong* developmental position (Overton & Reese, 1973; Reese & Overton, 1970). This weak interaction is the opposite of the one forwarded by Bijou (1976), because nature—not nurture—is the main force in this interaction. Although variables associated with both organism and context are said to be involved in the interactions associated with developmental (i.e., stage) progression, environmental (contextual) variables are only seen to facilitate or inhibit trajectories of primarily intrinsic (i.e., maturational) origin (Emmerich, 1968). According to organismic-stage theories, contextual variables cannot alter the direction, sequence, or quality of developmental change.

Moreover, in the predetermined epigenetic version of the organismic perspective, the maturational timetable (Erikson, 1959) or other biological phenomena (e.g., the movement of libido to particular areas of the body; Freud, 1954), that are believed to control the nature of developmental progressions, are all construed to be impervious to environmental influence—insofar as their impact on the quality of development is concerned. The organism is no more an influence on such biological variables than it is a determinant of the array of genes it receives at its conception. Thus, although the prime locus of developmental change lies within the organism, the organism is no more of an active agent in the interaction of this internal basis of development with the external environment than it is in nurture, mechanistic-behavioral theories such as those of Bijou (1976; Bijou & Baer, 1961).

Moderate Interactions

Another concept of interaction found in the developmental literature can be labeled as moderate (Lerner & Spanier, 1978b, 1980). Here, both organism and environment are (conceptually) equally weighted as influences on developmental outcomes. But the nature of these sources' relation while interacting may be conceptualized as analogous to the interaction term in the analysis of variance. Although organism- and environment-associated variables combine (in the manner provided by the general linear model) to influence developmental outcomes, each is construed to exist independent of (uninfluenced by) the other before (and presumably after) their interaction, and to be unchanged by the other during their interaction.

The concept of moderate interaction is not typically articulated as a feature of a particular theory of human development. Instead, it is found in the perspective to studying behavior that Gollin (1965) labeled the *child psychology approach.* This perspective is characterized by an ahistorical "subjects [participants] x tasks" approach to the analysis of behavior, and it is contrasted by Gollin (1965) with the historical "subjects x tasks-levels" approach characteristic of what he termed the *child development perspective.* In the child psychology approach, the goal is to determine the empirical contribution to variation in a dependent variable of organism-related variables (often vaguely represented by using age or sex as a factor), and environment-related variables (typically represented operationally by a specific task or manipulation), separately and in additive combination (i.e., "interactively") with organism-related variables.

In other words, the concept of moderate interaction is typically expressed as a methodological component of what is also termed the *experimental child psychology approach* (Reese & Lipsitt, 1970). This approach views the treatment of subject and task, or of organism and environment (or of heredity and environment, in the analogous analysis of variance approach involved in determining what, in Chapter 10, is discussed as "heritability"), as necessarily separate, independent factors whose interaction effect or contribution is linear and additive. The interaction effect itself may combine two sources in a nonlinear, multiplicative way. That effect, however, adds linearly to the total variability.

Strong Interactions

Finally, a strong concept of organism–environment interaction (Lerner & Spanier, 1978b, 1980; Overton, 1973, 2015a), or a concept of dynamic interaction (Lerner, 1978, 1979), is associated with a probabilistic-epigenetic, RDS-based perspective (Ford & Lerner, 1992; Gottlieb, 1983, 1991, 1992, 1997, 2004; Kuo, 1967; Lickliter, 2016; Mascolo & Fischer, 2015; Thelen & Smith, 2006). As noted in my previous discussions of probabilistic epigenesis, this concept emphasizes that organism and context are always embedded each in the other (Lerner, Hultsch, & Dixon, 1983); the context is composed of multiple levels of organization, with variables associated with each level changing interdependently across time (i.e., historically). Because organisms influence the context that influences them, they are thus efficacious in playing an active role in their own development (Lerner, 1982; Lerner & Busch-Rossnagel, 1981a, 1981b; Lerner & Walls, 1999).

Moreover, as discussed in Chapter 6, the mutual embeddedness of organism and context means that any attribute of the individual (e.g., a physical characteristic such as body build or a behavioral attribute such as a rhythmic temperamental style) will have different implications for developmental outcomes under different contextual conditions (e.g., in regard to different cultural ideas of bodily attractiveness or in regard to the requirements placed by different parents on their children in regard to the regularity of their sleep–wake cycles; Lerner, 1976, 1986, 2002). The individual characteristic is given its functional meaning only by virtue of its relation to a specific context and, since contexts vary (among themselves and each across time), the same characteristic will have a different import for development. In turn, the same contextual condition will lead to alternative developments because different individuals coact with it. Thus, as noted earlier, a given characteristic of individuality only has meaning for human development by virtue of its timing of interaction; that is, its relation to a particular set of time-bound contextual conditions. In turn, the import of any set of contextual conditions for psychosocial behavior and development can only be understood by specifying the context's relations to the specific developmental features of the individuals within it. As discussed in prior chapters, this central role for the timing of individual↔context relations in the determination of the outcomes of development is, of course, the probabilistic component of probabilistic epigenesis (Gottlieb, 1970; Scarr, 1982; Scarr & McCartney, 1983).

To illustrate the attributes of organism↔context relations described in this view, consider the implications of a child's temperamental individuality for his or her personality development (e.g., Chess & Thomas, 1999; Lerner & Lerner, 1983, 1989), a topic that I will discuss again later in this chapter. The significance of this individuality lies in the level of congruence, match, or "goodness of fit" (Lerner & Lerner, 1983, 1989) between a particular aspect of temperament and the demands or presses of the psychosocial and physical contexts. For instance, some parents may desire or demand highly regular eating, sleeping, and toileting behaviors from their children, whereas for other parents such biological rhythmicity may be irrelevant (e.g., Bornstein, 2015; Super & Harkness, 1981). A child who is biologically arrhythmic would not match the former type of demands, and as such, the import of this feature of temperament might be to promote poor parent–child relations (e.g., Lewis & Rosenblum, 1974); a consequence of a history of such relations might be poor adjustment.

In short, developmental scientists taking an RDS-based approach contend that, as a consequence of person↔context interdependency, a potential for plasticity exists across the life span. That is, if intraindividual development is a synthesis of intraorganism and contextual variables, and if the context does and/or can be made to change, then the person's developmental trajectory can, at least in part, be altered. It follows that constraints on development—for example, those that *might be* imposed by genes (Hubbard & Wald, 1999) or by early experience—are not fixed or immutable, as advocates of genetic reductionist conceptions have argued (e.g., Belsky, 2014; Binding & Hoche, 1920).

In contemporary human development, there are several perspectives that are consistent with the probabilistic-epigenetic conception of coaction found in RDS-based models. For instance, in the seventh edition of the *Handbook of Child Psychology and Developmental Science* (Lerner, 2015e), Volume 1 was devoted to "Theory and Method" (Overton & Molenaar, 2015). All chapters in this volume advanced ideas consistent with RDS-based conceptions. As such, these positions are compatible with the view that nature and nurture, organism and context, heredity and environment, relate to each other in dynamically coactive manners; that is, the variables involved in development are related in a manner reflective of fused levels of organization, of integrative levels within a relational developmental system (Ford & Lerner, 1992; Overton, 2015a; Schneirla, 1956, 1957; Tobach, 1981).

In the remainder of this chapter, I explore in some detail the ideas involved in understanding the structure and function of the integrated levels that Schneirla described as providing the basis of the dynamic relations found in the relational developmental system. Schneirla's ideas provided a useful framework for understanding the role of this system in fostering continuity and discontinuity across the course of life. His ideas also afforded a basis for elaborating a life-span view of human development (Baltes, 1987, 1997; Baltes et al., 1999; Baltes, Lindenberger, & Staudinger, 2006) and other theoretical models that stress the reciprocity of person↔context relations across life (i.e., action theory; Brandtstädter, 1999, 2006; Brandtstädter & Lerner, 1999).

THE CONTRIBUTIONS OF T. C. SCHNEIRLA

The scientific work of T. C. Schneirla and his colleagues (e.g., Lester Aronson, Herbert G. Birch, Daniel Lehrman, Norman Maier, Howard Moltz, Jay Rosenblatt, and Ethel Tobach) represents an attempt to deal systematically with the problems of behavioral development without resorting to facile and/or reductionist solutions. That is, Schneirla rejected, as naive and overly simplistic, theoretical conceptions that emphasized the exclusive (split) role of either nature (hereditarily preformed or predetermined processes) or nurture (shaping of behavior solely by environmental stimulation). Thus, Schneirla focused on a dynamic coaction among nature and nurture variables in attempting to find the sources of behavioral development. Because he rejected the notion that development is a simple process, he also rejected the idea that methods used to study this process can be simple. Hence, in commenting on the relation between a nature-based variable, maturation, and a nurture-based variable, experience, Schneirla (1956) said:

> It would seem to be the prevalence of an intimate, dynamic relationship between the factors of maturation and experience that renders analytical study of behavioral ontogeny so difficult. Methods must be devised appropriate to the complexity and subtlety of these processes. In such work, little may be expected from attempts to estimate the specific or the proportionate contributions of the innate vs. the acquired in ontogeny.
>
> (p. 407)

Thus, Schneirla presented a theoretical position consistent with the probabilistic-epigenetic conception of development. A more detailed analysis of the ideas of Schneirla and his associates will provide some understanding of the extent to which his work provides a fruitful, integrative framework with which to consider concepts of development pertinent to the role of the relational developmental system in human development.

Structure–Function Relations

One of Schneirla's major concerns as a comparative psychologist was with the relation between an organism's functioning (e.g., its motor behavior in its ecological niche) and the structure underlying the function (e.g., the neural and hormonal processes involved in such behavior). This concern arises because, in comparative psychology, a scientist is interested in learning whether the relation between structure and function is similar or different in different species (i.e., across phylogenetic levels).

Schneirla (1957) argued that the relationship between structure and function is not always the same for organisms of different phylogenetic levels. The same functions (e.g., learning) may be present in both a rat and a human or in an infant human and an adult human. Moreover, this function may play an analogous role for each of these organisms. That is, the function may allow the organism to adapt to its environment, to survive. However, the presence of this analogous function in and of itself in no way indicates that the underlying structure of learning is the same for a rat versus a person, or for an infant versus an adult.

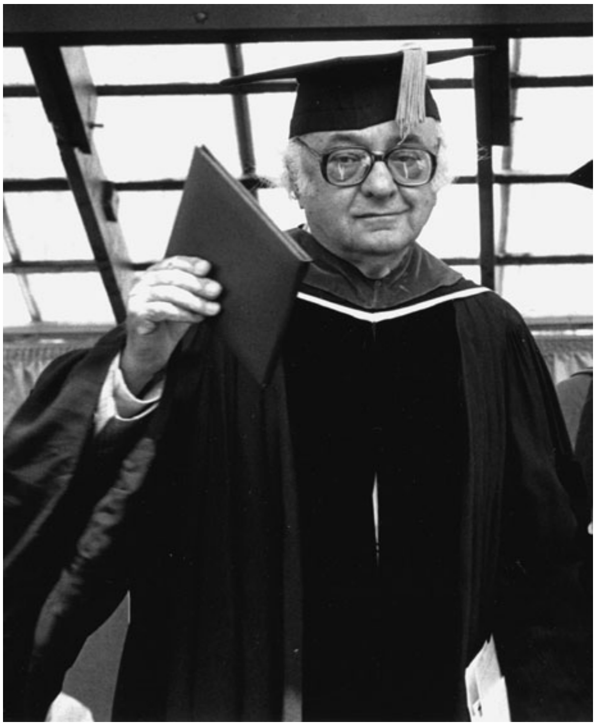
To the contrary, Schneirla suggested that the relation between structure and function is exceedingly complex and—more important—that it will occur with varying degrees of directness at different phylogenetic and ontogenetic levels (cf. Bitterman, 1965, 1975; and see Chapter 8). Thus, the degree of directness of relation between the two would be different for the ant, bird, rat, dog, ape, and human.

Schneirla noted (1956) that behavioral patterns often reached similar developments in different phylogenetic levels as a result of parallel adaptive, evolutionary processes. To some, such a similarity indicates equivalent underlying organization, or structure. But such an assumption is neither empirically universal nor logically necessary because the attainment of equivalently adaptive developments says nothing whatsoever about the antecedent developmental processes that brought about these adaptive functions. All mammals learn; but it is not necessarily correct to assert that, because both a rat and a human being develop this adaptive function, the laws, or structures, underlying their learning are the same (again, see the discussion in Chapter 8 of the work of Bitterman, 1965, 1975, in regard to phyletic differences in learning). The developmental processes by which learning comes about may be totally different for these two organisms. That is, antecedent developmental processes may be completely disparate for two different types of organisms, despite the fact that both demonstrate a similarly adaptive function. Thus, as Schneirla pointed out, these processes "may involve complex anticipations, as in a socialized human being, or may be reflex-like and automatic, as in a lower invertebrate" (1956, p. 392).

In essence, Schneirla suggested that the underlying structure of even evolutionarily similar behavioral developments is different for different phylogenetic levels. Although certainly not denying that structure underlies function, he emphasized, rather, that one must expect the relationship between structure and function to be differentially direct at different phyletic levels. Each level must be understood in and of itself, because the structure–function relationships of other phylogenetic levels will not hold for another level in question. In other words, the laws of one phyletic level will not apply to another, since the same structure–function relationships do not hold. Therefore, one cannot completely understand one phyletic level by merely reducing it to another. Schneirla also viewed ontogenetic development in a manner analogous to phylogenetic development. That is, ontogenetic processes proceed through levels just as phylogenetic processes do. Accordingly, structure–function relations between different ontogenetic levels can also be expected to be different.

It should be clear, then, that Schneirla took a now-familiar viewpoint. He advanced the probabilistic-epigenetic idea of qualitative discontinuity between levels: Each different phylogenetic level has its own structure–function relationship or, in other words, its own law (Novikoff, 1945a, 1945b). Within this perspective (Novikoff, 1945a), levels are conceived of as integrative organizations. That is:

> The concept of integrative levels recognizes as equally essential for the purpose of scientific analysis both the isolation of parts of a whole and their integration into the structure of the whole. It neither reduces phenomena of a higher level to those of a lower one, as in mechanism, nor describes the higher level in vague nonmaterial terms which are but substitutes for understanding, as in vitalism. Unlike other "holistic" theories, it never leaves the firm ground of material reality . . . The concept points to the need to study the organizational interrelationships of parts and whole.
>
> (Novikoff, 1945a, p. 209)



Alex B. Novikoff

Moreover, Tobach and Greenberg (1984) emphasized that:

> The interdependence among levels is of great significance. The dialectic nature of the relationship among levels is one in which lower levels are subsumed in higher levels so that any particular level is an integration of preceding levels . . . In the process of integration, or fusion, *new* levels with their own characteristics result.
>
> (p. 2)

Thus, because of its own laws, each different level is qualitatively different from the next. These differences across levels are the case, Schneirla asserted, for both different phylogenetic levels—which he termed *psychological levels*—and different ontogenetic levels—which he termed *functional orders*—because "on each further psychological level, the contribution of individual ontogeny is a characteristically different total behavior pattern arising in a different total context" (Schneirla, 1957, p. 82).

In addition to adopting the probabilistic-epigenetic viewpoint, Schneirla also adopted the levels-of-organization compromise discussed in Chapter 5. Schneirla asserted that knowledge of the structural basis of function is not sufficient for understanding behavioral developments at any given psychological level or functional order. Structure does not simply give you function because "something else" is needed, and as such, each different level must be studied in its own terms. That "something else" is, of course, the environmental, or experiential, context within which the organism develops. Structure–function relationships can be understood only in interrelation with their environmental context. Thus, Schneirla asserted that, to understand development, one must conceive of the nature–nurture relation in a dynamic way (Lerner, 1978, 2015a, 2015c).

Hence, the levels-of-organization compromise may be restated to align with Schneirla's position: For any given psychological level or functional order, the laws (variables) of the structural (lower) level are involved with (implied in) the laws of the functional, behavioral (higher) level, but function cannot be understood merely through an understanding of structure. Knowledge of structure alone is insufficient for understanding function. This situation is the case because function develops out of complex (dynamic) coactions between an organism's structure and the environmental variables impinging on it, coactions that produce a qualitatively different developmental context at each different level.

In sum, Schneirla saw phyletic and ontogenetic development as involving, at least in part, qualitative discontinuity. He saw different structure–function relationships at different levels. He maintained that the same psychological function may thus be underlain by different processes at different points in development. Thus, Schneirla took a position that is central in developmental theory and is shared by many other RDS-oriented theorists (e.g., Mascolo & Fischer, 2015; Raeff, 2016; Werner, 1957). The same behavior is often determined by different variables—by qualitatively different phenomena (e.g., by features of different periods of development—at different points in ontogeny (or phylogeny).

Behavioral Stereotypy versus Behavioral Plasticity

Schneirla (1957) suggested that psychological levels differ qualitatively from one another because different organisms have qualitatively different structure–function relationships. These relationships are based on different organismic structure↔experience relations. But what is the nature of these different interrelationships? What is the basis of the differences between different psychological levels? This question has been a critical one in comparative psychology and evolutionary biology, and it pertains to the nature of species' evolutionary changes, the character of interspecies differences in species' evolutionary changes, and the task of providing criteria for discriminating among species levels.

In addressing these issues, many evolutionary biologists and comparative psychologists have made use of the concept of *anagenesis* (Yarczower & Hazlett, 1977; Greenberg, Partridge, Weiss, & Haraway, 1999). Although it is not an uncontroversial idea (Capitanio & Leger, 1979; Yarczower & Yarczower, 1979), most scientists agree that "anagenesis refers to the evolution of increased complexity in some trait" (Capitanio & Leger, 1979, p. 876). For example, Dobzhansky et al. (1977) note that "Anagenetic episodes commonly create organisms with novel characters and abilities beyond those of their ancestors" (p. 236), or simply, that anagenesis is an "evolutionary advance or change" (p. 236). Similarly, Jerison (1978) noted that an evolutionary analysis of progress from earlier to later species "is called 'anagenetic' and is about progressive evolution," and indicated that in such an analysis "the objective is to identify grades in evolution" (pp. 1–2). Thus, an anagenetic (evolutionary) advance would place a species at a different evolutionary grade (Gould, 1976), and location of a species at a different grade would mark interspecies differences in evolutionary changes (i.e., anagenesis; Dobzhansky et al., 1977; Jerison, 1978).

However, an advance in complexity is often difficult to identify; for example, what specific structural and behavioral criteria need to be met (cf. Capitanio & Leger, 1979)? This difficulty is especially problematic when human social behavior is involved (Yarczower & Hazlett, 1977; Yarczower &

Yarczower, 1979; see also Sampson, 1977). However, Schneirla (1957, 1959; Tobach & Schneirla, 1968), among others (e.g., Birch & Lefford, 1963; Sherrington, 1951; Tobach, 1978, 1981), provided a useful framework.

Schneirla (1957) proposed the use of a behavioral *stereotypy–plasticity continuum* to differentiate the levels of complexity representative of different species. If an organism's behavioral development is stereotyped, there is a relatively fixed relation between the stimulation the organism receives and the concomitant responses it emits; that is, an almost unchanging relation exists between what goes in (stimulation) and what goes out (response). There is little, if any, variability in response to stimulation. Thus, if a researcher deprives a normal frog of food for some time and then presents a fly to the frog in its immediate field of vision, the researcher will inevitably see the frog flick out its tongue to catch the fly. Assuming that no steps are taken to intervene in this situation, and that the frog continues to exist in its natural habitat, little variation in the response to this stimulation will be seen. Plasticity, on the other hand, refers to the ability to show varying responses to the same stimulus input (Lerner, 1984). That is, a more variable relation exists between what goes in and what will come out.

To illustrate, suppose a person is in the habit of "flicking out" his tongue whenever his favorite food goes by (e.g., dessert). However, at times a reprimand from a spouse (e.g., "If you get any fatter, you won't fit through the door") will result in the person varying his response to the dessert stimulus; he may take a smaller helping than usual, or perhaps none at all. Although it would be relatively easy to train a rat to find its way in a maze to get to a desired food (e.g., cheese), it would be more difficult to train it to develop a large and flexible repertoire of alternate routes that it could efficiently introduce when more habitual routes to the cheese were blocked. Humans, however, develop this alternate-route repertoire quite readily. Hence, as illustrated in Figure 7.1, if a person's most direct—and, thus, habitual—route for driving from home to the market is suddenly blocked one day, he or she can quite efficiently adopt an alternate route.

An organism that shows stereotypy in its ontogeny develops little behavioral variability in response to stimulation. Alternatively, an organism that shows plasticity develops a relatively considerable degree

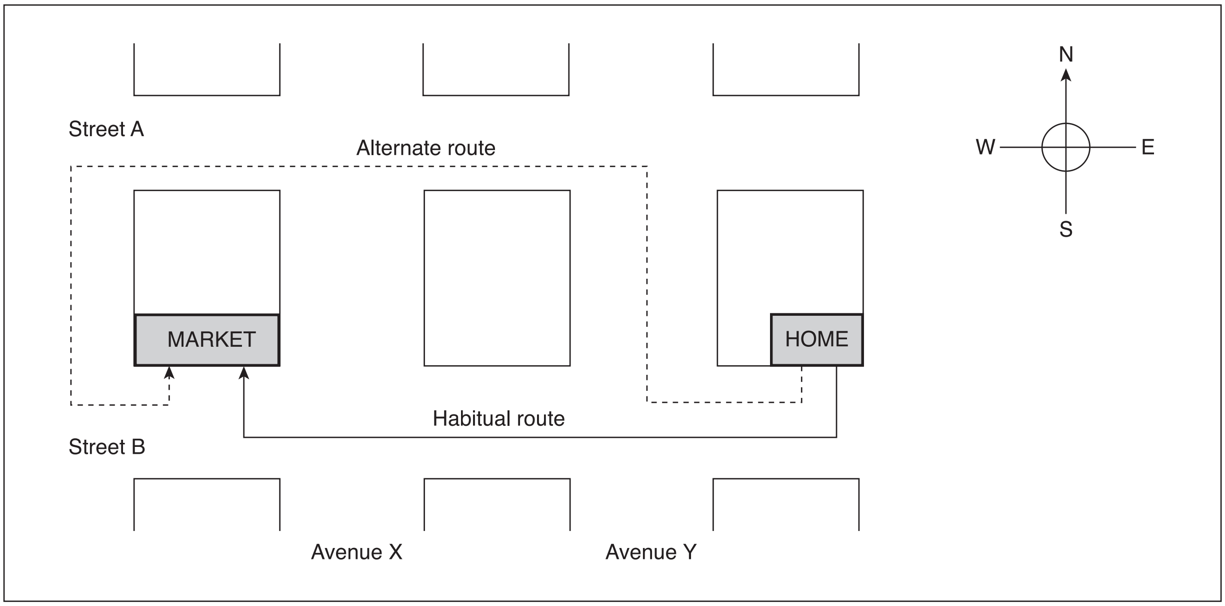


Figure 7.1 An illustration of human plasticity.

of variability in response to stimulation. Moreover, organisms with differing degrees of plasticity or stereotypy are on different psychological levels; the more plasticity shown in an organism's development, the higher the organism's psychological level. Thus, as Schneirla (1956) stated, "The appearance of behavioral stereotypy through ontogeny, if found characteristic of a species, indicates a lower psychological level, whereas the systematic plasticity through experiences indicates a high level" (p. 83).

Of course, neither plasticity nor stereotypy is an all-or-none phenomenon. It is therefore difficult to assert that the behavior of a particular species is either all stereotyped or all plastic. Rather, it is possible to think of stereotypy and plasticity as forming a continuum, with stereotyped and plastic behavior at opposite ends. Different psychological levels will fall at different points along this continuum, and the closer any species is to the plasticity end, the higher its psychological level. A hypothetical example of the ordering of species of different psychological levels along this continuum is presented in Figure 7.2. Ants are at the lower end because their behavior is less plastic than that of any other represented species. Human beings are closest to the plasticity end because human behavior is more plastic than that of the other represented species, and, accordingly, the human psychological level is higher than that of any of the other species.

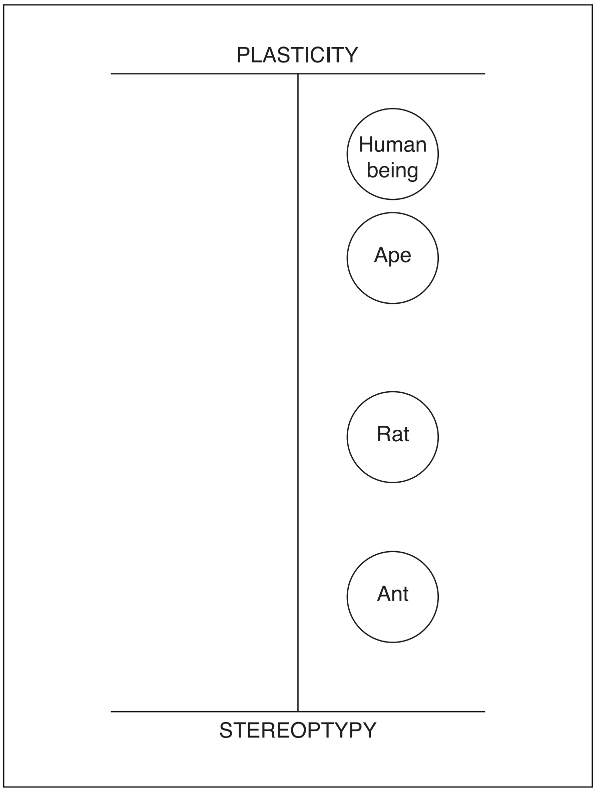


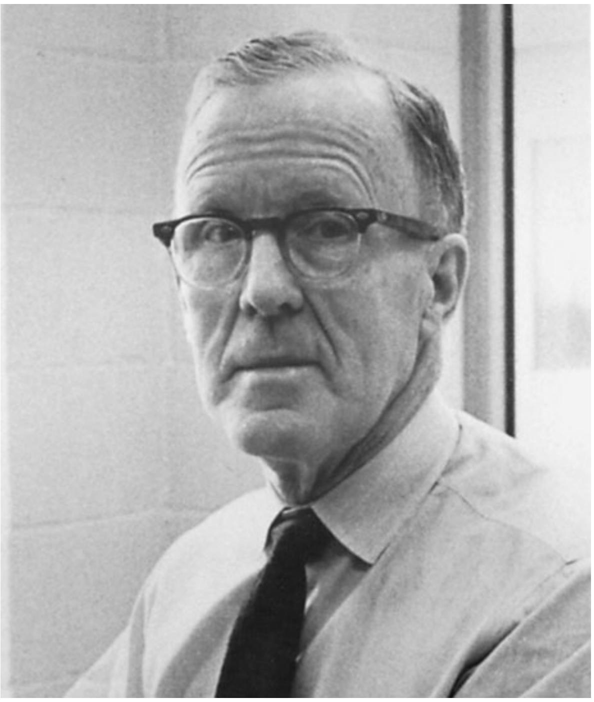
Figure 7.2 An illustration of a hypothetical stereotypy–plasticity continuum.

Donald Hebb's A/S Ratio

What is the structural contribution to these contrasting functional capabilities? For example, what nervous system structures may contribute to the plasticity or stereotypy of different psychological levels? One answer to this question might lie in a concept found in Donald O. Hebb's (1949) writings.

The cerebral cortex of the brain of mammals (e.g., rats, monkeys, dogs, or human beings) has various sections. One section is comprised of nerve cells (neurons) that constitute the cerebral centers for sensory information—information that comes from the outside world through our receptors (e.g., the rods and cones of the retina of the eye) and into our bodies. Another area of the cerebrum is comprised of neurons that constitute our motor cortex—that part of our cerebral cortex that sends messages to our muscles and thus allows us to act. Still another section of our cortex is comprised of association neurons, cells that integrate and associate information from various parts of the brain. For example, one role of the association cortex is to integrate information from the sensory cortex—pertaining to what is stimulating us—with information sent to the motor cortex, relating to our (motor, or muscular) actions, or our behavior.

The more association cortex an organism has, the more connections it can have between a given stimulus input and a behavioral output. That is, the more association fibers that exist, the more variable should associations be to any stimulus, and, accordingly, behavior should be more variable in relation to stimulus input. In 1949, Hebb proposed to express the relation between the amount of sensory cortex



Donald O. Hebb

and the amount of association cortex in a species in terms of a ratio. This ratio was termed *the association cortex/sensory cortex ratio*, or simply the A/S ratio.

Some organisms have a low A/S ratio, expressed as A/S ratio <1.0; simply, they have more sensory cortex than association cortex. For such organisms, sensory input will be more directly related to response than it will for organisms with higher A/S ratios. These organisms may be termed *sense-dominated*. Because such organisms have fewer association fibers with which to integrate their sensory input, their behavior in response to sensory input will be less variable. It will be more stereotyped. It will be, relatively, directly controlled by environmental stimulation.

Animals with higher A/S ratios will, however, show relatively less sense domination. Animals with more association fibers relative to sensory fibers (animals whose A/S ratio ≥ 1.0) will integrate their sensory input with the information provided by their association fibers and thus demonstrate more variable behavior in response to stimulation. The behavior of such organisms will be more a product of an interrelation between their association cortex and their sensory cortex than would be the case with organisms having a low A/S ratio. Accordingly, their behavior will be more variable in response to stimulus (sensory) input. It will be more plastic.

Thus, differences in A/S ratios may account for different degrees of plasticity and stereotypy among different psychological levels. Consistent with Schneirla's views, Hebb (1949, p. 125) suggested that, for widely differing phylogenetic levels, a hierarchy of psychological complexity can be assumed that corresponds to gross differences in the proportion of sensory to association neurons. Similarly, Schneirla (1956, p. 411) pointed out that a deficiency in the brain's association capacity seems to be a prime condition for specific fixed responses to specific stimuli, because stereotyped response tendencies are strongest in animals with the lowest supply of association neurons.

In sum, Hebb's notions promote a hypothesis about a structural basis for the functional differences in stereotypy–plasticity seen on different psychological levels. Animals with more sensory cortex than association cortex are more stereotyped in their behavioral development than are animals with more association cortex relative to their sensory cortex. These latter animals are more plastic in their behavioral development than are the former. Thus, the higher an animal's A/S ratio, the more functionally plastic its behavioral development should be. Conversely, the lower an animal's A/S ratio, the more functionally stereotyped its behavioral development should be. To make an analogy, then, low A/S ratios are to stereotypy (and low psychological levels) as high A/S ratios are to plasticity (and high psychological levels).

Ontogenetic Implications of Stereotypy–Plasticity and of the A/S Ratio

As might be surmised, Hebb (as well as Schneirla) maintained an active interest in the developmental implications of his ideas. Accordingly, Hebb qualified his notions about the A/S ratio by pointing out differences in the ontogeny of animals with different

A/S ratios. Schneirla reached conclusions similar to those of Hebb in regard to stereotypy–plasticity.

Animals with low A/S ratios are more stereotyped in their eventual behavioral development and, accordingly, are on low psychological levels. Yet, such animals reach their final level of functional organization—of behavioral functioning—much sooner in their development than do animals with high A/S ratios. Animals with few association fibers compared to sensory fibers progress through their ontogeny to full maturity relatively rapidly; they reach their final, albeit stereotyped, level in a relatively short time in their development.

One way of understanding this idea is to realize that such animals have comparatively few association-area cortex fibers that have to be organized through their development; they have relatively few associations that can be developed. Thus, they organize their association cortex comparatively rapidly. But at the same time, because of their comparatively limited association capacity, their behavior can never develop much variability, and hence it will be relatively stereotyped. Although these organisms have seemingly impressive adaptive automatisms and organism↔context regulations, they are not capable of responding to a nonstationary context with creative action (Brandtstädter, 2006). That is, their high specialization—and thus their limited latitude for variation—is an adaptive deficit. Fortunately for these organisms, the strict regulation of their behavior by a context to which they are adapted solves their inability for flexible organism↔context regulations (Hebb, 1949; Schneirla, 1957), until of course this context changes in significant ways.

On the other hand, animals with high A/S ratios are comparatively more plastic in their eventual behavioral development and are, therefore, on higher psychological levels. However, such animals develop toward their final level of development relatively slowly. These high A/S ratio animals reach their final level of functional capacity—of fully mature behavioral organization—much later in their ontogeny than do low A/S ratio animals. High A/S ratio animals have more association cortex compared to their sensory cortex, and they progress through their ontogeny relatively slowly. These animals reach a higher, more plastic psychological level, but it takes them a longer time to do so.

In sum, lower A/S ratio animals develop more rapidly, but their behavior remains relatively stereotyped; it is sense-dominated and shows little variability. On the other hand, higher A/S ratio animals develop more slowly, but their eventual behavioral development will be relatively plastic; it will show considerable variability. For example, a rat is on a lower psychological level than is a human being. Similarly, the rat has a lower A/S ratio than does a human being. But, in the time span of just a few weeks, a rat may be considered to be fully developed, whereas a human infant after only a few weeks of life is not at all, of course, like an adult human in regard to behavioral or cognitive functioning. The human infant will take years to reach a level analogous to the one that the rat reaches in just a few weeks. Yet, the human, when an adult, will be capable of considerably more complex, plastic behavior than any adult rat will ever be able to produce. In fact, this contrast will be true of the not-yet-fully-developed human; the human will surpass the adult rat while still a child.

An empirical instance of the point can be found in the results of a classic study by Kellogg and Kellogg (1933). The Kelloggs reared a newborn ape in their home and attempted to treat it like their own newborn child, who, by the way, also happened to be living there at the time. They diapered both infants and prompted their behavioral development, including language, in the ways that parents typically do. At first, the ape was ahead of their child in regard to behavioral development. Soon, however, the child overtook the ape and was never bested again.

Other lines of research support Schneirla's ideas about ontogenetic changes in stereotypy and plasticity. In order to illustrate this support, I discuss some issues in the development of perception.

Intersensory Integration: An Illustration

All species of animals have available processes that are adaptive; that is, every living species, by virtue of its existence, has processes that allow it to adapt to its environment. All species have ways of taking in food and eliminating waste products. I have noted that this similarity does not mean that all species have the same processes available to them. Although all species take in food and eliminate wastes, they may manifest these functions in different ways. Thus, organisms at different psychological levels may use different processes to serve the same function. Both the one-celled amoeba and human beings take in food and eliminate wastes, but they certainly perform these adaptive functions in different ways. This difference exists because there are new processes available at different psychological levels; there is qualitative discontinuity across psychological levels.

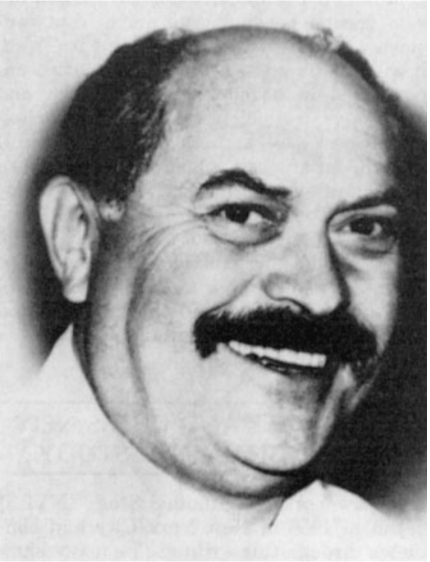
Accordingly, although all psychological levels have the capacity to react to stimulation, it is not appropriate to attribute the capacity of perception to all psychological levels. All psychological levels must have the ability to react to stimulation in order to survive, what Schneirla (1957) termed the capacity for *sensation.* Even one-celled protozoa have this capacity. Yet, it is not until a much higher psychological level is reached that the capacity for *perception*—that is, the ability to sense with meaning—is seen. Thus, at higher psychological levels a qualitatively discontinuous capacity emerges—perception—which allows the organisms of that level to adapt to their environment. These organisms have the ability, for example, to make associations with their sensations, that is, to integrate their purely sensory information with other information available to them. Such organisms can show different responses to the same stimulus; they can associate a different output with the same input. Thus, through their association capacity (e.g., underlain by their A/S ratio), they sense with meaning.

If the capacity of higher psychological levels is qualitatively different from that of lower psychological levels, it follows that these differences should be reflected not in the degree to which different psychological levels can organize sensory information, but in the kind of organization they achieve (Schneirla, 1957, p. 96). That is, these differences should be represented not only in how much sensory information can be handled but also in what is done with that information. Higher psychological levels should show greater associative variability—greater plasticity—than should lower levels.

Accordingly, Schneirla suggested that, as scientists assess differences between lower and higher psychological levels, they will see that, at increasingly higher psychological levels, sensory experiences result in organisms being more likely to develop a "trace effect" of a particular sensory input–reaction experience. However, at lower psychological levels there is more likely to be only the *fixation* of the effects of experience. In addition, at the higher levels sensory experience results also in the *correlation* of these trace effects. In other words, organisms at higher psychological levels have the capacity for a kind of organization of sensory information that is different from the capacity of phylogenetically lower organisms. They have the ability to correlate or associate information coming from one sense modality (e.g., vision) with information coming from another sense modality (e.g., touch).

Thus, at higher psychological levels, organisms have the capacity of *intersensory integration*, the ability to transduce (i.e., transfer or transform) information from one sense modality to another. With this capacity, sensory input from vision, for example, may be "equated" with sensory input from touch; thus, the sensory input from the two different modalities (modes of sensing) can come to mean the same thing to the organism. For example, adult humans can recognize a specific coin by feeling or seeing the coin. They also can recognize an ice cube by touching or seeing it. The sensations from either of these objects can mean the same thing to humans although they are delivered through different modalities.

Hence, as movement up the phylogenetic scale from lower psychological levels to higher psychological levels is considered, scientists will see perceptual ability emerging not because of new senses being present—not because higher psychological levels have more senses with which to fixate the trace effects of sensory experience than do lower levels—but because at higher psychological levels better liaison emerges among existing senses. Advances are seen in the capacity to correlate information among the senses. Such correlation is evidence of intersensory integration. Thus, as Birch and Lefford (1963) pointed out, "In the emergence of the mammalian nervous system from lower forms, the essential evolutionary strategy has been the development of mechanisms for improved interaction among the separate sensory modalities" (p. 3). Similarly, Sherrington (1951, p. 289) stated,



Herbert G. Birch

"Not new senses, but better liaison between old senses is what the developing nervous system has in this respect stood for."

Accordingly, an organism of a high psychological level—for instance, a human—has the capacity to develop considerable intersensory integrative ability, to make considerable gains through sensory experiences. However, humans, with their high psychological level, correspondingly have a high A/S ratio. This relation means that, although human beings are capable of high levels of behavioral development, it takes them a long period of time, relative to other animals, to reach this developmental level—the highest point of their functional order. In essence, human beings, although capable of considerable intersensory integrative ability, can be expected to develop this ability over several years in the course of their ontogeny. Simply, it is reasonable to hypothesize that human intersensory integrative ability is a developmental phenomenon.

This hypothesis was tested in a classic study by Birch and Lefford (1963) of the ability of children of different ages (ranging from 5 to 11 years of age) to integrate information from three different sense modalities—vision, active touch (or the haptic sense), and passive touch (or the kinesthetic sense). Birch and Lefford used geometric forms such as blocks in the shapes of circles, squares, triangles, stars, and crosses as stimuli for the children. Two blocks were presented at a time. Sometimes, the same object was presented for each child to see and touch; at other times, different objects were presented. In either case, each child was asked to judge whether the two blocks were the same or different.

Figure 7.3 depicts some of the results of this study. In support of the hypothesis that intersensory integrative ability increases with age (that it is a developmental phenomenon), Birch and Lefford (1963) found that "the ability to make the various intersensory judgments clearly improved with age" and concluded that "the findings strongly indicate that information received by young children through one avenue of sense is not directly transduced to another sensory modality . . . In fact, it may perhaps be argued that the emergence of such equivalence is developmental" (p. 45).

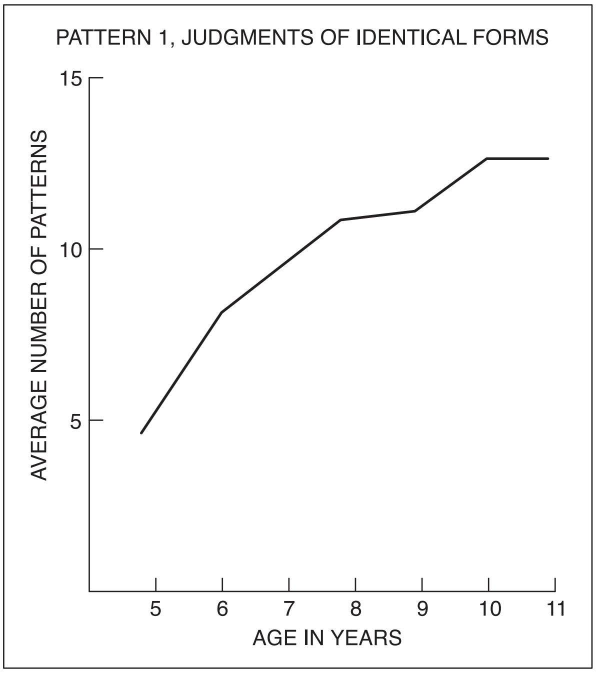


Figure 7.3 Some of the results of the Birch and Lefford study: correct judgments for all intersensory pairings made when judging identical forms at different ages.

Source: Intersensory Development in Children. Copyright © 1963 by The Society for Research in Child Development. Reprinted with permission.

In essence, Birch and Lefford (1963) provided strong evidence in support of the notion that the intersensory integrative ability of human beings reaches its eventual high level only after years of development. However, they also found that even their youngest research participants—the 5-year-olds—had relatively well-developed intersensory integrative ability. They suggested, however, that at younger ages (at about 3 years of age), this ability would be markedly inefficient but would rapidly improve.

In a similar experiment, Abravanel (1968) studied intersensory integrative development in children ranging in age from 3.3 to 14.2 years. He found that the base level (the lowest level) for performing the various intersensory equivalencies occurred at about 3 years of age. After this time, however, integrative ability improved greatly through 7 years of age, when it reached a high level of efficiency. Thus, consistent with the notions derived from Schneirla's ideas (1957) about perceptual development, both the Birch and Lefford (1963) study and the Abravanel (1968) study provided findings that support the hypothesis that humans' ability to transduce information from one modality to another increases with age—that it is a developmental phenomenon.

Abravanel (1968) provided further findings that support some of Schneirla's other concepts about development, those pertaining to the role of the organism's own activity as a source of its own development (see too Chapter 10, where action theories of human development are discussed in greater detail; e.g., Brandtstädter, 1999, 2006; Brandtstädter & Lerner, 1999; Heckhausen, 1999). Abravanel found that increases in intersensory integrative ability were associated with changes in the type of exploration activity the children showed when actively touching the stimulus. Specifically, younger, less accurate children explored the stimuli by either gross or passive movements. Alternatively, older, more accurate children used finer and more articulated movements, exploring with the fingertips, for instance, rather than with the palms.

The role of the organism's activity in the development of its own plasticity has been identified in other human data sets reported by Piaget (1961; Piaget & Inhelder, 1956) and by Birch and Lefford (1967). In addition, experimental research with animals (Held & Hein, 1963) confirmed this role of the organism's activity. Littermate kittens were or were not allowed to make motor adjustments as they traversed a circular route. Those animals making the active motor adjustments later performed better on a test of depth perception—that is, on a visual cliff apparatus (Gibson & Walk, 1960)—than did the restricted animals.

Thus, the idea that human plasticity is a developmental phenomenon, advanced by Schneirla (1957) as well as by Hebb (1949), Piaget (1961; Piaget & Inhelder, 1956), Bühler (1928), and Baldwin (1897a, 1897b), finds empirical support. In addition, support for the notion that the organism itself actively provides a basis of this progression is evident.

Conclusions

Animals of low psychological levels will develop much more rapidly than will animals of high psychological levels. However, the gains that these two levels of animals will make through their ontogeny will be quite different. Animals on a low psychological level will be able to gain little behavioral variability through their ontogeny because the nature of their development is restricted by their structural limitations, by their low A/S ratio. Animals on a high psychological level will be able to gain considerable behavioral variability through their ontogeny because their development occurs within the context of broader structural capabilities, their high A/S ratio.

Schneirla's (1957) ideas have relevance, then, for both the phylogenetic and the ontogenetic changes of humans. Human evolution should be able to be characterized by progressively greater potentials for plasticity (Gould, 1977). In turn, however, although evolution has led to the presence of this potential, its basis in structure requires organization over the course of ontogeny (Gould, 1977; Johanson & Edey, 1981). As such, normative patterns of human ontogeny should be able to be characterized by the progressively greater presence of plasticity. There are data supporting these ideas.

In evolutionary biology, Lewontin and Levins (1978) provided evidence for the link among anagenesis, complexity, plasticity, and what they term *coupling–uncoupling* phenomena. Lewontin and Levins (1978) cite Hegel's warning "that the organism is made up of arms, legs, head, and trunk only as it passes under the knife of the anatomist," and note that "the intricate interdependence of the parts of the body . . . permit[s] survival when they function well, but in pathological conditions produce[s] pervasive disaster" (p. 79). However, such interdependence of parts is neither phylogenetically nor ontogenetically static. Relations among parts change over the course of evolution; often this change involves the rapid evolution of some characteristics and the relative constancy of others. In other words, whereas various aspects of an organism may be bound together, if these attributes are either units of development or selection they may lose their cohesion and evolve independently if the direction of selection is altered (Lewontin & Levins, 1978).

Indeed, there are several aspects of adaptation that suggest that tight integration of attributes—or in Lewontin and Levins's terms, coupling—is disadvantageous. In considering this point, imagine that muscular strength, running speed, visual acuity, intellectual ability, eye color, handedness (whether a person is right- or left-handed), and ability to digest specific foods (e.g., milk) were attributes that were highly bound together in evolution. That is, imagine that these attributes were coupled in the sense that selection pressures on one attribute (e.g., decreasing the fitness, and, hence, likelihood of survival, of individuals who could not digest milk) would influence all the other characteristics equally (i.e., that the lack of survival of individuals who could not digest milk would mean that the attributes of muscular strength, running speed, intellectual ability, etc. would be lost as well). Clearly, in such a situation a high degree of coupling would be a disadvantage.

Lewontin and Levins pointed out that if there is not high coupling a given characteristic may be subject to alternative selection pressures. If the optimal states of the characteristic under the separate pressures are not vastly different, then adaptation would be best served by a "compromise in which the part in question is determined by" all the separate presses. However, as illustrated by the imaginary example involving the digestion of milk, the uncoupling of attributes may be advantageous "as the number of interacting variables and the intensity of their interaction increases" (Lewontin & Levins, 1978, pp. 83–84). This difference is so because, in the face of increases in the number of coacting variables, it becomes increasingly difficult for selective pressures to increase fitness. Thus, species with very tight coupling will be unable to adapt as readily as those in which the different components that increase fitness are more autonomous. Indeed, the more that the attributes of an organism are strongly coupled and interdependent, the more pervasive the damage done to an organism when some stressor overwhelms one particular attribute.

Accordingly, over the course of evolution the advantages of coordinated functioning and mutual regulation have come to oppose the disadvantages of excessive constraint and, hence, vulnerability; at least at the human level, individuals may have the capacity to couple and uncouple attributes successively. Ontogenetically, then, it may be that the most effective organisms (those most likely to function well across diverse contexts and circumstances) are those that have the potential to develop the capacity to couple and uncouple attributes as the context demands.

Therefore, a reasonable hypothesis may be that the direction of evolution at the human level has involved moving toward providing the substrate for the coupling–uncoupling of attributes. This increasing capacity for coupling and uncoupling may be the changes involved in anagenesis. That is, if higher evolutionary grades are defined as being more complex, and if greater complexity means greater plasticity, a key instance of plasticity would be the capability to couple, uncouple, and couple anew—either through recoupling or with ontogenetically unique couplings. This facility should become progressively established across ontogeny, as the physiological substrate of the psychological level of analysis becomes organized. Thus, again, this reasoning is consistent with the view that evolutionary and ontogenetic progression involves successive change toward greater plasticity of functioning.

CONCEPTS REPRESENTING DEVELOPMENT

From this consideration of stereotypy–plasticity, it is clear that Schneirla (1956, 1957) was just as concerned with the problems of ontogeny as he was with those of phylogeny. He viewed both as progressing through a series of qualitatively different levels; he drew a distinction between the progression from one phylogenetic level to another, and the progression from one ontogenetic level to another, by his concepts of *psychological levels* and *functional orders*, respectively. Using Piaget's (1950, 1970) stage theory as an example (see Chapter 5), the first two years of life may correspond to the first part of the functional order of a human (the sensorimotor stage); the next five years to another, separate portion of the functional order (the preoperational stage); and the following five years to still another part of this functional order (the stage of concrete operations).

The concepts of stereotypy and plasticity serve to differentiate between different psychological levels, and the relative degree of stereotypy–plasticity may serve to characterize the psychological level of a particular animal species. With these ideas about psychological levels as background, it is useful to turn to a consideration of Schneirla's concepts characterizing the functional order of a species.

A Definition of Development

To Schneirla (1957), *development* referred to successive changes in the organization of an organism, an organism that was viewed as a functional and adaptive system throughout its life. Providing, then, a forerunner of contemporary RDS-based theories (e.g., see the chapters in Overton & Molenaar, 2015; see too Raeff, 2016), Schneirla's definition denoted that development involves successive changes within a living, functioning, adaptive, individual system. By continually functioning in an adaptive manner, this system develops through successive changes throughout the life span.

But what are the characteristics of this system? What are the processes that comprise the determinants of the organism's development? Schneirla (1957, p. 86) suggested that two broad concepts represent the complex factors that make up the successive changes of development.

Maturation

The first of these two concepts is maturation. To Schneirla, maturation meant growth and differentiation of the physical and physiological systems of an organism. *Growth* refers to changes in these systems by way of tissue accretion, that is, tissue enlargement. *Differentiation* refers to changes in the structural aspects of tissues with age, that is, alterations in the interrelationship among tissues, organs, or parts of either of these.

For example, at specific points in the development of the embryo, different layers of cells exist. These cells mature not only via accretion (growth) but also through differentiation. Thus, when the embryo is in its blastula stage of development, it is divided into three layers of cells. One of these layers is termed the *mesoderm.* Eventually, as the embryo goes through changes and the cells of the mesoderm grow larger and differentiate, these cells will come to form the muscles and bones of the body. Hence, maturation refers to changes in the organism that result from the growth and differentiation of its tissues and organs.

However, as I noted earlier in this chapter in regard to Schneirla's use of the term "fusion" to depict the relation between biological and experiential contributions to the process of development (Schneirla & Rosenblatt, 1963), Schneirla cautioned against thinking that maturation could occur in any way independent of environmental contribution. Consistent with discussions of probabilistic epigenesis in earlier chapters (e.g., Gollin, 1981; Gottlieb, 1970, 1992, 1997, 2004; Tobach, 1981), Schneirla emphasized that maturational processes must *always* occur within the context of a supportive, facilitative environment; because of this interdependence, the exact path that maturation will take will be affected by what is happening in the environmental context of the organism. Just as maturation is not independent of environment, structure is not independent of function. Hence, as Schneirla (1957) stated:

> Maturation is neither the direct, specific representative of genetic determination in development, nor is it synonymous with structural growth. Much as an environmental context is now recognized as indispensable to any development, students of behavioral development . . . emphasize the roles of structure and function as inseparable in development.
>
> (p. 86)

Experience

The second concept needed to represent the complexity of the factors comprising developmental changes is therefore *experience*. To Schneirla, experience referred to all stimulus influences that act on the organism throughout the course of its life. Consistent with the ideas of Anastasi (1958) regarding the continuum of breadth involved in understanding the impact of the environment on human development (see Chapter 6), Schneirla also believed that experience is a very broad, all-encompassing concept. Any stimulative influence, any stimulus that acts on the organism in any way, is part of experience; and this stimulative influence can occur at any time in the organism's journey from conception to death. Experience affects the organism across all of ontogeny.

Thus, experience can affect the organism before it is born. For example, stimulative influences may act on the fetus in the form of chemicals, drugs, or disease entities. Thus, a baby whose mother contracts German measles (rubella) during the early part of her pregnancy will encounter an adverse experience. The effects of such an experience may be a deformed heart or blindness. Intrauterine experience may also take the form of chronic maternal stress, which has been shown to affect long-term metabolic, immune, endocrine, and cognitive function (e.g., Entringer, Buss, & Wadhwa, 2010; Wang et al., 2018; Zuckerman & Keder, 2015). Of course, as well, experience will affect the organism after it is born. This impact may also take the form of diseases or accidents, as well as the type of care the infant receives and the support he or she has for positive, healthy development (cf. Anastasi, 1958).

In sum, experience is a term representing any and all stimulative influences acting on the organism as it develops. These influences may result from events taking place within the organism's body (endogenous stimulative influences) or outside the organism's body (exogenous stimulative influences, that is, influences of the context or the ecology of human development; Bronfenbrenner, 1979, 2005). In either case, experience acting on the organism provides one of the two coacting factors determining development.

The Role of Maturation↔Experience Relations in Development

Experience is necessary for any and all developments throughout ontogeny. Experience always has an effect on the organism, and in a specific way. As I noted, to Schneirla (1957), experience results in *trace effects*. To Schneirla (1957), trace effects are organismic changes that result from experience and that, in turn, influence future experience. Experience effects changes in the organism, and these changes—these trace effects—influence how future experience will act on the organism. In other words, when experience acts on the organism, it will leave a trace of its action, and this trace effect becomes part of the organism and, thus, changes the organism's character. Hence, any later experience that acts on the organism will act on a *different* organism—an organism that now has a residual effect, a trace effect, of its previous experience. The second experience will result in an effect different from what would have happened had the previous experience not occurred.

For example, a young child may have an experience that results in a physical disability (e.g., lack of mobility in his or her limbs). Because of this condition, which changes the character of the organism, future experiences (e.g., exposure to a physical education program) will influence the child differently than if the child had not had that previous experience. However, the possible effects of such an experience are limited by the maturational status of the organism. That is, the same experience will have different meanings and generate different associations depending on the organism's level of growth and differentiation. Thus, because the organism's sensory, association, and motor portions of the nervous system change developmentally, the effects of experience are limited, or framed, by the developmental level of the individual.

For example, an infant is capable of perception and can form trace effects resulting from some types of perceptual experience (e.g., listening to the spoken language of his or her parents) (e.g., MacWhinney, 2015). At later points of development, the same child will be capable of developing trace effects as a result of the perceptual experiences involved in reading (e.g., Lonigan, 2015). However, these later experiences would not have resulted in the same trace effects had they been presented to the relatively physiologically immature infant. Alternatively, the trace effects that obtain as a result of the perceptual experiences involved in reading could never have occurred when they did had the child not had a particular series of perceptual experiences since infancy (e.g., becoming familiar with the spoken language of his or her conspecifics), resulting in trace effects. In sum, the nature of the behavioral gains that can result from experience is limited by the relative physiological maturation of the organism (Schneirla, 1957, p. 90).

However, maturation also has limits. These limits are imposed by experience. Consistent with the probabilistic-epigenetic view he espouses, Schneirla (1957, p. 90) pointed out that the limitation of experience imposed by maturation is in turn limited by the developmental level of the organism—by the attained functional order the organism has reached in its ontogeny. The growth and differentiation of maturation do not occur without the supportive, facilitative effects of experience. This experience, leaving its trace effects on the organism, provides the milieu within which maturation occurs. Inappropriate experiences—such as loss of oxygen supply during the perinatal period—will not allow maturation to proceed as it would have had the inappropriate experience not occurred. Thus, maturation must coact with experience in order for development to proceed, and, in turn, the effects of experience are constrained or framed by their interaction within the limits imposed by the organism's maturational status.

Hence, a complex coaction between experience and maturation provides the basis of behavioral development. Experience results in trace effects, but the nature of the trace effects is limited by the maturational status of the organism. In addition, this maturation↔experience relation determines what behavior the organism can develop at any particular time in its ontogeny. As well, the experience↔maturation relation provides the basis for the developmental level reached at a particular time in an organism's ontogeny. In turn, this developmental level, comprising the result of the experience↔maturation relation provides (a) the milieu within which further maturation proceeds, does not proceed, or proceeds at a different rate; and (b) the milieu that determines what trace effects will result from further experiences. In sum, Schneirla said: "The nature of the gains made through experience is both canalized and limited by the relative maturity of species-typical afferent, neural, and efferent mechanisms, in dependence upon the developmental stage attained" (1957, p. 90).

Thus, to Schneirla, behavior emerges through the course of development as a function of coactions between experience and maturation. If appropriate experiences do not occur or, conversely, if inappropriate experiences occur, maturation will not proceed as it otherwise would have; accordingly, the behavior that would have developed will not, therefore, develop at that time. In other words, if inappropriate experiences occur (such as disease, loss of oxygen supply, or loss of a mother's nurturance), or if appropriate experiences occur but do so at a time too late for typical development to proceed, then maturation will not develop typically. It follows that the behavior that would usually have developed will be altered.

Conversely, if maturation does not proceed as it typically does (because of a lack of supportive, facilitative experiences), then the effects of experience—the trace effects—will be altered; in turn, the behavior that emerges at a particular point in time will be different. Thus, the ordered emergence of behavior in development depends centrally on the nature and timing of experience↔maturation relations. The attainment of developmental levels is dependent on the quality and timing of the variables involved in these dynamic coactions.

Conclusions

Schneirla's ideas reflect the concept of probabilistic epigenesis. Schneirla emphasized that the nature and timing of the coactions between maturation and experience are central in determining development. Moreover, the nature and timing of experience↔maturation relations cannot necessarily be predicted in advance for every organism within a species. At best, developmental scientists can say only that if the nature and timing of the maturation↔experience relations occur in specific ways, then behavior will probably develop along specific lines.

Accordingly, norms for development, which are statements about when in people's lives a specific behavior is typically seen, can be used only as general guidelines for considering development (Rose, 2016). Statements such as "babies will sit up at 6 months of age," "babies will say their first word at 11 months of age," or "babies will walk at 14 months of age" can be considered only as statements that apply in general terms. They may apply to a given group as a whole, but they may not necessarily apply to any individual in that group. That is, such norms do not mean that babies must do these things at these times in order to be considered normal. Rather, they mean only that for a given large group of babies, an average time exists for the emergence of a particular behavior. At the same time that this norm exists, however, differences among individuals (i.e., interindividual differences) also *necessarily* exist. Different people show a behavior either before or after the norm for their group. In fact, developmental scientists should expect that all people should *not* reach the same level of development at the same time (Rose, 2016).

In sum, to Schneirla any behavioral development that occurs is obtained through a bidirectional (reciprocal) coaction between maturational and experiential factors. Thus, the emergence of any behavior at any time in an organism's development is dependent on the nature and timing of this relation. In other words, behavioral development is not dependent on maturation or experience alone. This situation is the case because "factors of maturation may differ significantly in their influence upon ontogeny, both in the nature and in the timing of their effects, according to what relations to the effects of experience are possible under existing conditions" (Schneirla & Rosenblatt, 1963, p. 288). Hence, from Schneirla's theoretical point of view, any notion of behavioral development that emphasizes the exclusive contribution of either maturational or experiential factors (or biological or contextual, or nature or nurture factors) is incorrect.

Of course, Schneirla's model of development, reflecting an emphasis on the developmental system as causal in human development, and linked to philosophical notions that integrate probabilistically the views of organicism and contextualism, is not the only theoretical conception that is used to understand human development. Although Schneirla's ideas are conceptually compelling, and have a strong empirical foundation in biology, and comparative and developmental psychology (Aronson, Tobach, Lehrman, & Rosenblatt, 1970, 1972; Gottlieb, 1997; Gottlieb et al., 2006; Hood, Halpern, Greenberg, & Lerner, 2010; Lerner & Benson, 2013a; Tobach & Greenberg, 1984; Overton, 2015a), I emphasized in prior chapters that there exist theories that derive from other philosophical positions and that do not emphasize nature↔nurture fusion but, instead, emphasize either nature, nurture, or weak or moderate nature–nurture interactions.

Schneirla's ideas constitute historical and conceptual bases for other theories that emphasize how the integration of levels of biological-through-contextual/ecological organization provides a basis of human development. Accordingly, in several following chapters, I consider such theories. That is, I discuss the roots and, at this writing, contemporary instantiations of RDS-based theories.

Schneirla's theory—and the RDS-based theories elaborated after it—constitute a counterpoint to theories that emphasize nature, nurture, or weak or moderate interaction views. Historically, it is arguably the case that Schneirla's ideas provided the strongest voice countering nature theories across much of the twentieth century (Aronson et al., 1970, 1972; Gottlieb, 1992, 1997). For instance, during this period, Schneirla and his colleagues (Lehrman, 1953; Tobach, Gianutsos, Topoff, & Gross, 1974; Schneirla, 1966) stood as the clearest alternative to nature theories of phylogenetic and ontogenetic change that were forwarded (e.g., Lorenz, 1965). Schneirla (e.g., 1966) argued against Lorenz's (1965) notion of instinct and, as well, Scott's (1962) formulation of the concept of critical periods.

Regardless of how convincing one may find RDS-based theories, I have noted that nature-oriented conceptions remain on the scholarly scene through this writing (e.g., Bjorklund & Ellis, 2005; Plomin et al., 2016). After reviewing instances of RDS-based theories, I discuss such nature-oriented theories in Chapters 11 and 12. I use ideas that have their foundation in Schneirla's probabilistic-epigenetic conceptions to critique the nature-oriented positions.

However, to help prepare for this "point–counterpoint" approach to the presentation and analysis of RDS-based models versus nature-based, genetic reductionist models, it is useful to consider two concepts that provide nature explanations of behavioral development—the notion of critical periods and the concept of *instinct* (or innate behavior). The critique of these concepts engaged much of Schneirla's attention during his scholarly career. Discussion of these concepts illustrates how Schneirla's views allow developmental scientists to effectively counter views that suggest that nature variables alone can account for human development. I turn first to the critical-periods hypothesis.

THE CRITICAL-PERIODS HYPOTHESIS

The notion of critical periods in development was formulated in embryology. Within that area of science, the idea was advanced that the various parts of the whole organism (e.g., various organs or organ systems) emerge in a fixed sequence; more importantly, it was held that the parts that develop in a fixed sequence do so with just a certain amount of time allowed for each part to develop. It was believed that there was an overall timetable of development, and each part of the whole organism had its own fixed time of emergence, set by maturation. Each part had a critical period in which to develop (e.g., see Scott, 1962).

This perspective holds that a part of the organism that is in its critical period can easily be stimulated. Such a part is highly responsive to both facilitating and disruptive influences. Thus, if the part does not develop normally or appropriately during its critical period, it will never have a second chance. Because the time limits of development are invariably fixed by maturation, even if the part does not develop, the focus of development will change. It will shift to another organ system, in accordance with the predetermined timetable of development, and that different organ system will then be in its critical period of development. Hence, any part that does not develop appropriately during its own critical period will not have another chance.

Similarly, in human development such a critical period idea refers to a time in the ontogeny of a species during which it is crucial for a particular feature of development to emerge. The period is crucial because specific maturational processes then occurring would allegedly place time limits on the development (Rosenblatt, Turkewitz, & Schneirla, 1961; Schneirla & Rosenblatt, 1961, 1963). For example, within his psychosocial theory of development, Erik Erikson (e.g., 1950, 1959, 1968) divided the human life span into eight stages, each of which may be interpreted as consistent with the definition of a maturation-based critical period. In my view, the eight stages in Erikson's theory may be regarded as critical periods because each emerges in accordance with a maturational "ground plan," a developmental scheme that is built into the person (Erikson, 1959). Thus, Erikson maintains that, in the first year of life, the infant must develop a specific degree of a "sense of trust." If the infant does not develop this feeling at the time when it is supposed to develop, not only will there never be another chance but also the rest of that person's development will be unfavorably altered.

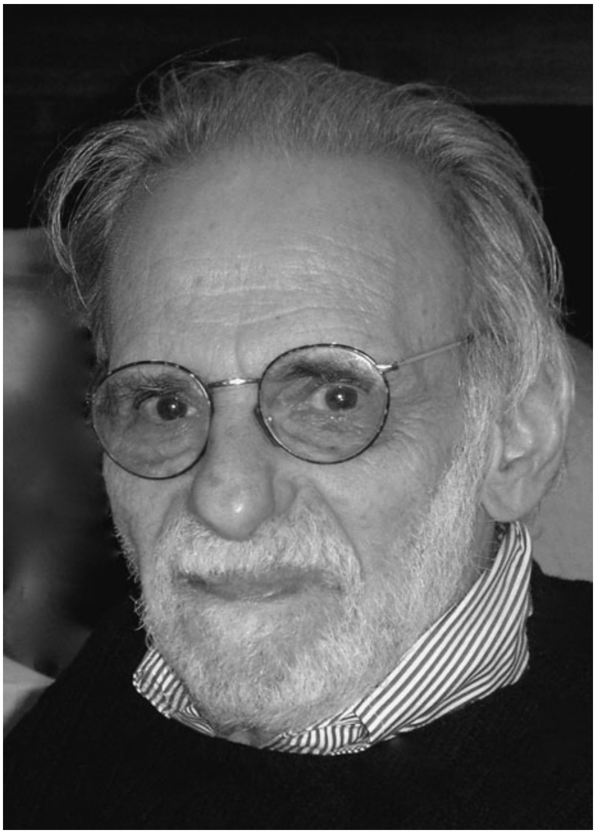
Clearly, the critical-periods hypothesis places primary dependence for healthy development on an intrinsic, maturation-determined timetable. What this formulation clearly indicates, then, is that maturation in and of itself sets critical time limits for development; there are periods in an organism's development that are circumscribed by maturation, and the time limits of these periods are somehow not related to experiential factors. However, from Schneirla's (1956, 1957) and others' probabilistic-epigenetic position (Gottlieb, 1970, 1983, 1991, 1992, 1997, 2004; Gottlieb et al., 1998; Tobach, 1981; Tobach & Greenberg, 1984), such a conception of critical periods is untenable. Rather than emphasizing the independent contribution of maturation, Schneirla would opt to investigate the process by which maturation and experience coact to enable a specific development to take place at a specific time in ontogeny. For instance, in rejecting the idea of critical periods, Schneirla and Rosenblatt (1963) noted that, in their theory of behavior development, "we conclude that factors of maturation may differ significantly in their influence upon ontogeny, both in the nature and in the timing of their effects, according to what relations to the effects of experience are possible under the existing conditions" (p. 1113).

Schneirla did not say that specific developments were not critical for some later developments. He would agree to some extent with other researchers concerned with the critical-periods notion (e.g., Scott, 1962) that there are critical phases of life, for instance for the development of learning. He would agree that what is learned at a specific time in an organism's ontogeny may indeed be important or even essential for whatever follows (Schneirla & Rosenblatt, 1963; Scott, 1962). That is, Schneirla and Rosenblatt (1963) noted that:

> In discussing his concept of critical periods, Scott [1962] reports us as having "suggested that there are critical stages of learning—that what has been learned at a particular time in development may be critical for whatever follows."
>
> Although we are not disposed to dispute this broad statement, it is not ours. In our view, any such sentence should have a more comprehensive context, to the effect that what the young animal may attain in behavior at any phase of ontogeny depends upon the outcome of earlier development in its every aspect.
>
> (p. 1110)

Accordingly, Schneirla's critique of the critical-periods hypothesis is based on his contention that the use of the term "critical" means only that what happens at "Time 1" in an organism's life may be very important—in fact, foundational or even essential—for what can or will happen at "Time 2." Such an assertion merely describes a relation between events that occur at two different times in ontogeny; it makes no statement about whether the first event was determined by maturation alone or by coaction between maturation and experience (Bateson, 1983).

It is the source of the "criticalness" in development about which Schneirla argued. Simply, time limits for development that are fixed by maturation, arising without the contribution of experience, are inconsistent with his probabilistic-epigenetic position. Rather, Schneirla proposed a theory that placed "emphasis upon the *fusion* of maturation (growth-contributed) and experience (stimulation-contributed) processes at different stages in behavior ontogeny, together with the . . . contributions both of maturation and experience . . . as well as the interrelations of these contributions . . ." (Schneirla & Rosenblatt, 1963, p. 288, italics added). Indeed, Howard Moltz (e.g., 1973), a leading student



Howard Moltz

of Schneirla's, found experimentally that the time limits of specific purportedly critical periods (e.g., involving the immediately-after-hatching "following" behaviors of some species of birds) *could* be altered through specific manipulations of the birds' early visual experiences (e.g., Moltz & Stettner, 1961).

Weak and Strong Versions of the Hypothesis

Of course, Schneirla's view is not the only one that exists in regard to the meaning and bases of the concept of critical periods in development. Indeed, over the course of numerous reviews of the concept (e.g., Bateson, 1979, 1983; Colombo, 1982; Connolly, 1972; Hess, 1973; Hinde, 1962; Nash, 1978; Scott, 1962; Thorpe, 1961), several definitions of critical period were forwarded. These definitions may be divided in several ways. For instance, Krashen (1975) distinguished between strong and weak versions of the critical-periods hypothesis. Consistent with McGraw (1943), the hypothesis by Colombo (1982) states in its weak form that:

> A critical period is a time during the life span of an organism in which the organism may be affected by some exogenous influence to an extent beyond that observed at other times. Simply, the organization is more sensitive to environmental stimulation during a critical period than at other times in its life.
>
> (p. 261)

Similarly, in Krashen's (1975) view, the weak version of the hypothesis states that there are periods in life when the development of a system can best be furthered by particular stimulation but that the system's development can, nevertheless, still occur after such a period.

In essence, then, in the weak form of the hypothesis, the critical period is really only a *sensitive period*, one wherein particular experiences may most readily or efficiently promote development of a system (e.g., cognition, vision, or language); nevertheless, similar or perhaps distinct experiences can foster the system's development after such a period, albeit with the requirement that the experience (the stimulus) be more intense in order to result in comparable development (MacDonald, 1985). Thus, this form of the hypothesis indicates that critical periods are not so critical after all, and that they are little more than labels applied to the well-known and hardly controversial observations that:

1. When a system is developing it needs stimulation to allow it to adequately do so (e.g., if humans were totally deprived of light stimulation their visual system would not develop; Hebb, 1949), or simply that, as Schneirla (1957) explained, the human development system needs to be active to function adequately.
2. It is easier to influence a system—for better or worse—when it is in a state of development than after it has been fully organized (MacDonald, 1985).

In sum, in the weak form of the critical-periods hypothesis, particular experiences play a noncontingent (Moltz, 1973) role in development; although they are not absolutely necessary for adequate or healthy development to occur, specific experiences can enhance development due to the greater efficiency of their influence at a specific time. Thus, with such not-quite-critical critical periods, developmental deficits produced by the lack of a particular experience (e.g., language deficits due to the absence of an adequate language model) may be overcome by experiences in later life. If recovery of function can occur, this recovery means that whereas a given period may be *optimal* (Moltz, 1973) for the development of a particular function, it is not a critical time for this development. As Bateson (1983, p. 8) puts it, "Once the mechanisms protecting behavior from change are stripped away by suitable treatment, change resulting from renewed plasticity is once again possible" (see too Bateson, 2015, 2016).

In turn, in the strong form of the critical-periods hypothesis, particular stimulation is needed at a particular time in order for normal development to proceed; in other words, if the appropriate stimulation does not occur when it is supposed to in life, then what will occur is "an irrevocable result not modifiable in subsequent development" (Scott, 1962, p. 957). Thus, in such a formulation the organism *needs* specific stimulation for its continued normal development, and given inappropriate experience, it is *vulnerable to*, or *at risk for*, abnormal development during such a period (Colombo, 1982). Simply, for such a period, no recovery of function by later experience is possible (Krashen, 1975) and, as such, experience during this period has a "contingent" role (Moltz, 1973); that is, it is absolutely necessary for normal development.

The original instance of the strong version of the critical-periods hypothesis derives from the work of Konrad Lorenz (1937a, 1937b, 1937c; see too Chapter 11). Lorenz introduced the concept of "imprinting" to describe what he believed to be an irrevocable social bond, or attachment, formed by newly hatched precocial birds (e.g., birds such as ducks or geese, that can move sufficiently to follow other animals immediately after birth). These birds followed the first moving object they saw (usually their mothers) during the first hours after birth. Although Moltz and Stettner (1961) were able to manipulate (i.e., extend) the time period for imprinting through altering the visual experience of such birds (by placing a hood over their eyes that, although allowing light to come through, did not enable them to see any patterns or shapes), Lorenz (1937a, 1937b, 1937c, 1965) claimed, nevertheless, that these first few hours were the critical period for imprinting to occur.

The evidence that exists to support the reality of such strong critical periods may be questioned, however. Colombo (1982) summarized data pertinent to the existence of strongly defined critical periods in regard to four areas of research: imprinting in birds, social development in rhesus monkeys, language acquisition in humans, and binocular vision development in mammals. Colombo (1982) noted:

> Nearly every demonstration of a critical period in behavioral development during the past 50 years has been followed by a demonstration of some behavioral recovery from the effects of critical period exposure or deprivation. The first example was with avian imprinting, in which Lorenz's (1937a, 1937b, 1937c) claims of a tightly bounded period during which a permanent parent–offspring relationship was formed were rigorously tested. Subsequent evidence suggested that the critical period was not as temporally distinct (Brown, 1974) nor were the effects of stimulation within it as irreversible (e.g., Ratner & Hoffman, 1974; Salzen & Meyer, 1968) as Lorenz had originally thought (Bateson, 1966).

After observing the results of social isolation during the first year of life, Harlow (1959, 1965) suggested the existence of a critical period for the development of social behavior in the rhesus monkey lasting (in one version) from birth to 250 days. The critical stimulus was apparently what he called "contact comfort," the absence of which during this early period resulted in permanent social/psychological maladjustment. Later, however, a series of experiments (Mason & Kenney, 1974; Novak & Harlow, 1975; Suomi & Harlow, 1972) demonstrated that with special interventions and patience, the adverse effects of deprivation during this period could be overcome.

Language acquisition was another major developmental process to which critical period theory was applied, only to have that application subsequently questioned. Elaborating on a suggestion by Penfield and Roberts (1959) and through the use of data on early and late unilateral brain damage (e.g., Basser, 1962; Landsell, 1969) and the development of language in retardates (Lenneberg, Nichols, & Rosenberger, 1964), Lenneberg (1967, 1969) hypothesized a period of receptiveness to language lasting from ages 2 to 12. Language could be most easily acquired during this period; after this period, acquisition of a first language would be extremely difficult, if not impossible. The absolute irreversibility of the period's effects has been somewhat disconfirmed by subsequent investigation of acquisition after linguistic deprivation (Curtiss, 1977; Curtiss et al., 1975; Fromkin et al., 1974) and of second language learning (McLaughlin, 1977).

In initial studies of the critical period for the development of binocular vision, during which monocular deprivation resulted in anatomical degeneration of the deprived eye's pathways, complete domination of cortical physiology by the deprived eye, and apparent blindness of the deprived eye (e.g., Hubel & Wiesel, 1970) no recovery of function was reported (Blakemore & Van Sluyters, 1974; Hubel & Wiesel, 1970; Wiesel &

> Hubel, 1965). Subsequent studies, however, demonstrated that recovery in at least the behavioral aspects of visual function could be obtained after the end of the period (e.g., Baxter, 1966; Chow & Stewart, 1972; Cynader, Berman, & Hein, 1976; Mitchell, Cynader, & Movshon, 1977; Timney, Mitchell, & Griffin, 1978; Mitchell, 1978). It is worth noting, however, that this recovery has yet to be demonstrated in primates (Crawford et al., 1975; von Noorden, Dowling, & Ferguson, 1970). (pp. 268–269)

Moreover, Colombo (1982) reviewed additional evidence that both the presumed onsets and terminations of critical periods—that is, the times in life when these periods are believed to begin and end—are influenced by variables both endogenous and exogenous to the organism. Thus, the time limits of these periods are not as fixed and sudden as Lorenz (1937a, 1937b, 1937c, 1965) maintained or as impervious to contextual influences as Lorenz also believed. Colombo (1982) indicated that rather than a sudden and dramatic onset of sensitivity to a specific stimulus, sensitivity rises gradually to a peak and then gradually declines. These changes can be manipulated; for example, manipulation in regard to binocular visual development can occur by altering the amount of light in the rearing environment. I noted that similar perceptual stimulation manipulations can alter the imprinting period in birds (e.g., Moltz & Stettner, 1961). In addition, pharmacological manipulations can extend the imprinting period of birds or even prevent it from occurring at all (Colombo, 1982).

Conclusions

There is no good evidence to support the strong version of the critical-periods hypothesis, as for instance advanced by Lorenz (1937a, 1937b, 1937c, 1965). Nature variables (e.g., an instinct, DNA) *do not* prescribe fixed time limits within the life span, wherein specific stimulation must occur for normal development to proceed. Rather, when a specific portion of the relational developmental system is in a period of marked development it is especially responsive to influences by variables "outside" the specific portion of the system but of course endogenous to the overall, holistic, integrated system. Indeed, Colombo's (1982) conclusions regarding the character of critical periods in development are comparable to those that reflect Schneirla's (1956, 1957) perspective. Colombo noted that "the emergence of a critical period . . . is based on, and may be predicted by . . . the interaction of dynamic, developing systems, and as much effort should be directed toward identifying those systems and their interactions as toward identifying the period itself" (Colombo, 1982, p. 270).

Subsequent discussions of the critical-periods hypothesis support Columbo's (1982) views (e.g., Michel, 2010; Michel & Tyler, 2005). For instance, Michel and Tyler (2005) note that the history of research on critical periods indicates that the questions posed by researchers have changed. They have altered from the question of whether there *is* a critical period to ones about what controls the developmental processes that shape criticality for specific facets of development (e.g., language, vision, neural development, etc.). Michel and Tyler (2005) emphasize that this focus on process has enabled researchers to move to a more sophisticated approach to research, one that seeks to understand specific ways in which past and current experiences shape ensuing experiences. This developmental focus moves the study of criticality in development to an assessment of the trajectory of experiences in the lives of organisms, and how these pathways frame the importance of prior features of the trajectory for subsequent development.

As I turn now to a discussion of instinct, or innate behavior, I will emphasize that this concept is also predicated on the idea that nature is the sole determinant of features of development. As I discuss in greater detail in Chapter 11, this concept is centrally related to Lorenz's notions regarding the critical period for imprinting and, in fact, it is Lorenz who made the most prominent contribution to the concept of instinct.

INSTINCT: INNATE BEHAVIOR

The concept of instinct is perplexing. The concept may seem to both describe and explain the appearance of complex behaviors in different species but, when a scientist considers the concept with more than passing scrutiny, the explanatory use of the term evaporates and, as well, even the descriptive use of the term becomes problematic. For instance, Mark Blumberg (2016) asks:

> How do birds know to migrate south for the winter? How do border collies know to herd sheep? How do sea turtles find their way back home to the beach on which they hatched? As a shorthand—as an aid to communication—we might talk about a migratory instinct, a herding instinct, or a homing instinct.
>
> (p. 1)

In answering the questions he posed, Blumberg (2016) notes:

> Such labels may seem gratifying, but it is an illusory gratification. Scratch the surface of any complex, adaptive behavior and one is confronted with a seemingly endless array of hard questions spanning evolutionary and developmental time, the intricacies of ecological and social experience, and the machinations of the nervous system with its billions of neurons. The more we dive into these matters, the harder it is to settle on any clear notion of what an instinct actually is. As Patrick Bateson (2002) has pointed out, this conceptual confusion about *instinct* is reflected in the many meanings that are routinely ascribed to it, including:
>
> - present at birth,
> - not learned,
> - developed before it is used,
> - unchanged once developed,
> - shared by all members of a species,
> - adapted during evolution,
> - served by a distinct module in the brain,
> - attributable to genes.
>
> Scientists often unknowingly invoke more than one of these meanings at any given time, and may even unwittingly switch between meanings in a single article. This is not just a matter of lazy thinking. The murkiness of the term reflects actual confusion about the subject. No one doubts the existence of species-typical behaviors, and we can all agree that any science of behavior must endeavor to make sense of them. But there is an unsettling gulf between widely accepted assumptions surrounding *instinct* and the actual science available to explain it.
>
> (p. 1)

How did the murkiness of the concept of instinct arise? What can be done to bring light (clarity) into the discussion of this concept? To address these questions, it is useful to consider the initial use of the term.

The notion of instinct, or instinctive behavior, is perhaps most often associated with the work of Konrad Lorenz. Beginning in the 1930s, Lorenz, an Austrian-born zoologist and physician, studied specific types of behavior he termed *instinctive behavior.* By this term, Lorenz seemed to mean behavior that is preformed in the genotype. Lorenz contended that humans inherit a genotype, and built into this genotype is a "limited range of possible forms in which an identical genetic blueprint can find its expression in phylogeny" (Lorenz, 1965, p. 1).

In essence, then, Lorenz contended that there is a fixed and invariant relation between specific genetic inheritance and specific behaviors, and this correspondence is what he meant by instinctive behavior. Specific behaviors are preformed, or at least predetermined, and, thus, they are innate; they are built into the organism through genetic inheritance (the genotype) and, thus, these behaviors are simply unavailable to any environmental influence.

More specifically, Lorenz saw specific inherited properties of nervous system structures as innate. Specific groups of neurons, he claimed, had built into them specific, distinctive properties (Lehrman, 1970). They obtained these properties directly from the genotype, with experience having no influence. For example, as Lehrman (1970, p. 24) pointed out, one such innate property of a given neural structure is "its ability to select, from the range of available possible stimuli, the one which specifically elicits its activity, and thus the response seen by the observer." That is, in the view of Lorenz, specific nervous system structures come with the innate ability to select out specific stimuli from the environment; these are the stimuli that elicit (bring forth) the built-in (predetermined) functional component of the structure, that is, the response (Lorenz, 1965).

Because, as Lorenz (1965) contended, experience plays no role in the presence of this instinctive behavior, a scientist does not have to bother with the issue of how the relation between the stimuli and the responses comes to be established. All one has to say is that the behavior is there because it is innate. Then, one simply "explains" that innate behavior comes this way. Thus, to Lorenz, no further analysis was needed. In advancing this argument, Lorenz "solved" the problems of behavioral development by simply avoiding them—by defining them away.

In essence, then, Lorenz (1940a, 1965) argued that genetic inheritance represented a "blueprint" for the development and final level and form of behavior; that is, it represented a set of directives that were unalterable by environment, experience, learning, socialization, and so on (cf. Lehrman, 1953, 1970). This genetic inheritance was believed to be able to circumscribe behavior so severely because it led directly to the formation of an instinct—a predetermined, innate, and unmodifiable pattern of behavior specific to the species within which it exists. The behaviors associated with this instinct are then not capable of environmental, experiential modification.

Thus, to Lorenz, behavior is constrained by instincts; variation in behavior beyond the limits imposed by the genetically fixed instinct is not possible. Such a conception of genetic influence precludes, then, a process analysis of the ways in which genetic and environmental variables contribute coactively to behavioral development. In other words, Lorenz's (1940a, 1965) conception of instinct precludes a consideration of how organismic and/or contextual processes may contribute to the development and organization of behavior. His conception eliminates any use for studying how behavior may be altered or enhanced.

From Schneirla's (1956, 1957, 1966) perspective, there are several problems inherent in Lorenz's ideas about instincts. By making a distinction between what is innate and what comes about through the environment and by implying that there exists a genetic blueprint that imposes fixed constraints on development, Lorenz opted for the "which one?" (nature or nurture) question, which Anastasi (1958) rejected as inadequate (see Chapter 6). Thus, given Schneirla's probabilistic-epigenetic position and the gene↔context relations pointed to by the norm-of-reaction concept (see Chapter 6), the notion of innate, or instinctive, behavior as formulated by Lorenz (1965) is not tenable for the following reasons:

1. Nature and nurture are inextricably bound; it is inappropriate to assert that genes can directly produce human behavior (e.g., Gottlieb, 1997, 1998, 2004; Lickliter, 2016; Moore, 2015a, 2016). Nature variables need the supportive, facilitative influence of experiential factors in order to contribute to behavior. In turn, of course, experience needs nature variables with which to coact; as such behavior development occurs through gene↔context relations within the integrated, relational developmental system (Overton, 2015a).
2. Because of this interdependency, it is inappropriate to speak of "innate" as meaning developmentally fixed—that is, to speak of specific behavior as being unavailable to environmental influence or to say that an organism must develop specific behaviors because it inherited a certain genotype (Lehrman, 1970, p. 23). The nature↔nurture relation is more complex. Genes play a role in human development only through these coactions. The role of genes in human development will be different under different environmental (experiential) conditions (as I noted in my discussion of the norm-of-reaction concept); therefore, it is incorrect to speak of a genetic blueprint. Simply, there is no isomorphism between genotype and eventual behavior.

Conclusions

Lorenz used the terms *innate* or *instinctive* to refer to behavior that is genetically fixed and, therefore, unavailable to environmental influence. However, from the perspective of adherents to the concept of probabilistic epigenesis the notion of instinct can be rejected as being overly simplistic, as being based on faulty logic and, most important, as ignoring the problems and issues of behavioral development. To study the problems of behavioral development, developmental scientists must avoid terms such as *innate* (as employed by Lorenz, 1940a, 1965, 1966). Such terms end scientific investigation by simply saying that a behavior develops in a specific way because the organism is built that way. Thus, use of the terms *innate* or *instinctive* avoids assessing the processes by which behavior develops and, hence, is of little, if any, scientific use.

Although I will return in Chapter 11 to the work of Lorenz, and I will raise additional issues regarding his use of the term instinct, it is useful here to point to perhaps the most succinct summary of the criticisms that can be leveled against Lorenz's use of the term instinct. This criticism was made by one of Schneirla's former students, Daniel Lehrman. In a classic paper, published in 1953, Lehrman noted:


Daniel Lehrman

> The "instinct" is obviously not present in the zygote. Just as obviously it is present in the behavior of the animal after the appropriate age. The problem for the investigator who wishes to make a closer analysis of behavior is: How did the behavior come about? The use of "explanatory" categories such as "innate" and "genetically fixed" obscures the necessity of investigating developmental *processes* in order to gain insight into the actual mechanisms of behavior and their interrelations. The problem of development is the problem of the development of new structures and activity patterns from the resolution of the interaction of *existing* structures and patterns, within the organism and its internal environment, and between the organism and its outer environment. At any stage of development, the new features emerge from the interactions within the *current* stage and between the *current* stage and the environment. The interaction out of which the organism develops is *not* one, as is often said, between heredity and environment. It is between *organism* and environment! And the organism is different at each stage of its development.
>
> (p. 345)

Although the theoretical position of Lorenz is egregiously flawed conceptually and is, as well, empirically counterfactual, his ideas had great influence (e.g., he was awarded the Nobel Prize in Medicine or Physiology in 1973), especially among nativist-oriented theorists. For instance, his ideas provided a major basis for sociobiological interpretations of human development (see Chapter 12). In that such nativist interpretations are still forwarded at this writing (e.g., Bjorklund, 2015; Bjorklund & Ellis, 2005; Del Giudice & Ellis, 2016), the ideas of Lorenz merit greater scrutiny. This analysis is warranted also because, arguably, more so than any other nature-oriented theorist about human development, Lorenz's views were used (intentionally by Lorenz) to further the political agenda of the Nazi regime in Germany during the 1930s and 1940s (see Eisenberg, 1972; Kalikow, 1978a, 1978b, 1983; Lerner, 1992).

Accordingly, there are important reasons involving human development theory and its application to turn (in Chapter 11) to a fuller presentation and critique of the ideas of Lorenz. In presenting a useful, RDS-based alternative to the nature-reductionist ideas of Lorenz (e.g., 1965, 1966), I consider another key concept associated with Schneirla's (1957) ideas. This concept enables developmental scientists to go beyond the simplistic split of nature and nurture, on which ideas such as those of Lorenz rely, and, instead, to elaborate an action-oriented, dynamic view of the individual. In the view I now present, the individual—by being the organismic focus (or locus) of the integration of nature and nurture—is a key agent in its own development.

CIRCULAR FUNCTIONS AND SELF-STIMULATION IN DEVELOPMENT

From my previous discussion, Schneirla's (1956, 1957) theoretical position may be seen as a viewpoint that provides a way to conceptualize the dynamically coactive influences of nature and nurture in behavioral development. However, Schneirla (1957) suggested that there was a "third source" of development. In addition to the coaction of nature and nurture, Schneirla said, there was another source of an organism's development: the organism itself.

A "Third Source" of Development

As the organism develops, it attains specific behavioral characteristics through the effects of the maturation↔experience relation. These individual behavioral characteristics of the organism stimulate aspects of its environment (e.g., the organism's parents or its conspecifics more generally). This stimulated aspect of the context then responds to the organism and this response, in turn, again stimulates the organism. This organism↔context relation is a *circular function.* The organism acts on its environment in a specific manner and, because of this specific action, the environment acts on the organism in a specific way.

In other words, the organism develops distinct individual behavioral characteristics as a result of the specific maturational↔experience coactions with which it is involved, and as a function of these specific behavioral attributes the organism behaves in an individually distinct manner in its environment. This action on the organism's environment constitutes, then, a specific, differential stimulus for reactions *from* the environment. The feedback to the organism that is represented by these reactions changes the organism in a manner that makes it increasingly distinct and, as a consequence, the circular function continues. Thus, the circular function is a basis of the idiographic development of the individual organism (see Rose, 2016, for a similar analysis of the basis of idiographic development).

For example, the organism's individual characteristics of behavior (e.g., its temperamental characteristics such as activity level, intensity of reaction to a stimulus, threshold of responsivity to a stimulus, or rhythmicity or predictability of its patterns of behaviors, for instance, regarding sleep–wake cycles or feeding behaviors) may differentially stimulate other similarly aged organisms, the organism's parents, or even itself. For instance, humans may find it more difficult to care for an arrhythmic infant than a rhythmic one (Chess & Thomas, 1999; Thomas, Chess, Birch, Hertzig, & Korn, 1963). The individualistic stimulation (e.g., unpredictable sleep–wake or hunger cycles) will evoke differential responses (e.g., caregivers may become more frustrated or weary trying to care for an unpredictable infant than for a predictable one), which in turn will serve to stimulate the organism in a manner specific to it. For instance, Brazelton, Koslowski, and Main (1974) found that mothers of unpredictable infants developed poorer parenting behaviors than mothers of predictable infants. The different, individualized pattern of stimulation (i.e., the specific feedback) given to infants in this example (poorer versus better parenting of him or her) becomes part of the experience that shapes the further individually distinct development of the infant (and, thus, there is a continuation of the circular function). In this way, the circular stimulative process, initiated by the organism's own individually distinct characteristics and actions, creates a source of the organism's own further, individual development.

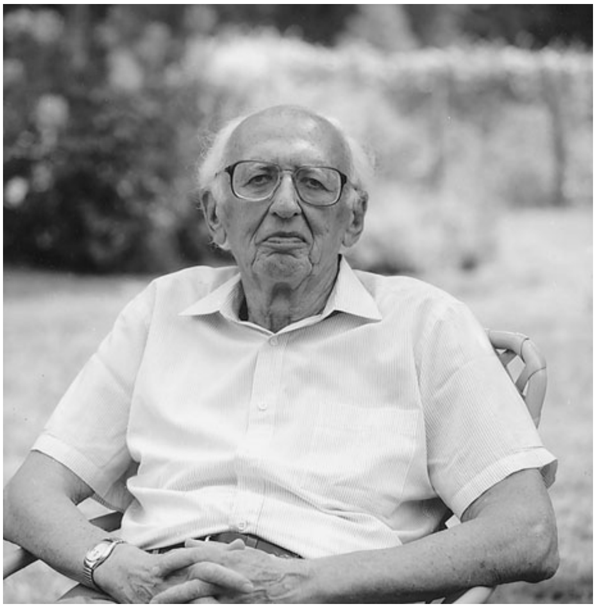
Hence, the organism provides an important, ever-present source of its own development. This source must be considered as important as the other sources of the organism's behavior—those that influenced the behavior that originally initiated these circular functions. In commenting on the importance of this third source of development, Schneirla (1957) said:

> An indispensable feature of development is that of circular relationships of self-stimulation in the organism. The individual seems to be interactive with itself throughout development, as the processes of each stage open the way for further stimulus–reaction relationships depending on the scope of the intrinsic and extrinsic conditions then prevalent.
>
> (p. 86)

The example of circular functions that I provided by discussing possible infant↔mother coactions involving arrhythmic and rhythmic infants was not fanciful. I drew this example from the longitudinal research of Alexander Thomas and Stella Chess (e.g., Thomas et al., 1963). This study in fact assessed individuals' temperament and, in this context, identified different types of constellations of temperament that might make for difficult or easy infant↔caregiver relations. Arrhythmicity and rhythmicity differentiate difficult and easy infant↔caregiver relations, respectively. However, additional variables were part of the different constellations of temperamental attributes that created variation in relationships between infants and their parents.

That is, Thomas and Chess and their colleagues (e.g., Thomas, Chess, & Birch, 1968; Thomas et al., 1963) noted that, because of the specifics of an infant's maturation↔experience relations, some children developed a style of behavior consisting of:

1. A lack of regularity. For example, the child might sleep for two hours, wake for five, sleep for three hours, and then wake for seven. Sometimes, the baby might eliminate almost immediately after feeding, whereas at other times there might be a considerable length of time between feeding and elimination.
2. A quite high activity level when awake.
3. A relatively negative mood when awake; the child cries or screams quite often.



Alexander Thomas

Stella Chess

4. High intensity of crying and screaming.
5. A low threshold for the manifestation of all of this high activity and loud crying and screaming. That is, the child has a low threshold for responding.

However, Thomas and Chess found that other children developed quite a different style of behavior. That is, in contrast to the first group of children, this second group of children were characterized by the following behaviors:

1. Rhythmic behaviors; they woke, slept, and eliminated in predictable cycles.
2. When they were awake, their activity levels were of moderate magnitude.
3. These children generally had a positive mood; they smiled and laughed a lot.
4. Their behaviors were of moderate intensity.
5. Finally, the children maintained a moderate threshold for responding.

Thus, the children in these two groups showed different sets of temperament characteristics. Whereas both sets resulted from the specifics of each child's maturation↔experience relations, the implications of each set of characteristics for the development of the respective children were quite disparate (Chess & Thomas, 1984, 1999; Lerner & Lerner, 1983, 1989; Thomas & Chess, 1977). The former type of child would present obvious difficulties for his or her parents. The child would stimulate reactions that would be quite different from those stimulated in the parents of the latter type of child. Certainly, compared with the former type of child, the latter type of child would be easier to interact with.

If parents could choose, before the fact, either of the above sets of characteristics for an expected child, they would almost without exception choose the second set. Parents would rather have a predictable, smiling, moderately active baby than an unpredictable, loudly crying, and highly active one. But parents cannot choose their baby's behavioral characteristics. The first type of baby's temperament attributes would create specific reactions in his or her parents that might involve difficulty in handling the baby for long lengths of time, and so they might make their social exchanges with the child relatively short and abrupt (cf. Brazelton, Koslowski, & Main, 1974). Babies who are rarely in arms tend to cry more (Hunziker & Barr, 1986), which in turn may make them less desirable to hold—and thus a problematic circular function might become exacerbated.

Alternatively, however, if a baby's temperament attributes were like those of the second group of children identified by Thomas and Chess, then different reactions might be evoked in parents. For instance, babies in this second group would be easier to handle, and extended exchanges with the baby might be more likely. Moreover, the exchanges might have a different quality, for instance, they may involve warmer and more positive relations (Chess & Thomas, 1999). As such, more positive circular functions might ensue.

CONCLUSIONS

Schneirla highlighted the necessity of focusing on the organism and its own actions in trying to understand the sources of behavioral development. His notions of circular functions and self-stimulation illustrate a central idea in the probabilistic-epigenetic conception of development: The organism is central in its own development (Brandtstädter, 1999, 2006; Brandtstädter & Lerner, 1999; Gottlieb, 1983, 1997; Lerner, 1982, 1984; Lerner & Busch-Rossnagel, 1981a, 1981b; Lerner & Walls, 1999; Scarr & McCartney, 1983). An organism does not just sit passively waiting for the world to act on it, as Skinner (1971) contended. As well, an organism does not just wait for maturation to unfold so that it can emit behaviors and have bidirectional exchanges with its context. Rather, the organism is always active, and its own actions provide an important source of its own development. Thus, development is in part a self-generated phenomenon.

Simply, then, the idea of an active organism dynamically relating to an active context is a central idea in Schneirla's view of development *and* in the RDS-based approach to human development for which it provides both a historical and a conceptual foundation. The foundation Schneirla (1956, 1967) provided for resolving split approaches to nature and nurture within the context of RDS-based ideas, such as probabilistic epigenesis and, as well, the comparable foundation for this resolution that was provided at about the same time by Anastasi (1958) (see Chapter 6), occurred more than a half-century ago, at this writing.

To a student or early career developmental scientist reading this chapter and the preceding one, then, it may seem either odd or even unnecessary to have reviewed such "old" information. If such reactions to the content of these chapters occur, it may be because—as I hope and predict (Lerner, 2015c; Lerner et al., 2014)—readers will have developed their scholarly orientation to developmental science within an era wherein RDS-based concepts and theories are, and have been, the key intellectual frames for the field. However, to paraphrase the famous statement of historian George Santayana, if developmental scientists do not understand the history of challenges faced in attempting to eliminate egregiously flawed, but persistently used, essentialist and, in particular, genetic reductionist conceptions from their field, they will inevitably continue to be beset by such counterfactual and problematic ideas.

The contributions of Anastasi, Schneirla, and others—for instance, Lehrman and Hebb—were made at least a half-century before some of the readers of this book not only began graduate school but, perhaps, were even born. Therefore, it is understandable to me that such readers may not appreciate the *zeitgeist* of the 1950s, the spirit of the times, within which Anastasi, Schneirla, and others, were trying to change the frame of developmental science. They were writing at a time within which the dominant paradigm of science was a Cartesian, split one (Overton, 2015a).

Their challenge, then, was to try to promote a view of how developmental science should approach its subject matter in a manner that fell outside the boundaries of then normal science (Kuhn, 1962). However, scientific revolutions—paradigm shifts—do not happen overnight (Kuhn, 1962, 1970). For the edifice of one paradigm to crumble and be replaced by another paradigm, anomalous theory-predicated facts need to accumulate; such facts—for instance, findings derived from concepts associated with nature↔nurture processes—are anomalous because they cannot be accommodated by models within the dominant paradigm, for instance, models involving split, nature or nurture processes. For anomalous findings to result in the emergence of a new paradigm to frame normal science, such findings have to be repeatedly generated and brought to the attention of normal scientists, that is, scientists working within the extant paradigm.

However, to communicate with scientists working within the Cartesian, split paradigm, Anastasi, Schneirla, and other advocates of what would become the process-relational paradigm (Overton, 2015a) had to use terms that were comprehensible to these other colleagues. I noted in Chapter 5 that Piaget's work was known in the United States in the 1920s but, because his vocabulary was so different from the one used by U.S. developmental scientists, his work was largely ignored, and it was about 40 years before his work was "rediscovered" in the United States (Flavell, 1963).

Accordingly, I regard the use of seemingly split conceptions such as heredity and environment (Anastasi, 1958), maturation and experience (Schneirla, 1957), or Intelligence A and Intelligence B (Hebb, 1949) to be conceptually strategic, albeit perhaps not explicitly intended to be so by the respective scholars who employed these terms to show the shortcomings of split approaches. Anastasi, Schneirla, Lehrman, and Hebb changed the paradigm by seeming to work within it and by demonstrating that the antimonies prevalent in the Cartesian, split paradigm failed to account for the evidence that was generated when these split ideas were replaced by integrative, coaction conceptions.

In the next chapter, I discuss the contributions of Heinz Werner, another developmental scientist working within the historical era, the 1950s, within which Anastasi and Schneirla worked (e.g., Werner, 1957). His contributions were to also replace another split conception used to study human development—continuity versus discontinuity—with an integrative model of human development. The model he advanced is another key instance of RDS-based approaches to human development (Raeff, 2016).

CHAPTER EIGHT

The Orthogenetic Principle and the Resolution of the Continuity–Discontinuity Issue

Contributions and Implications of the Work of Heinz Werner

The conceptual factors that influence the continuity–discontinuity issue were usefully specified by Heinz Werner (1948, 1957). Like T. C. Schneirla, and other major contributors to developmental psychology, such as Piaget, in the mid- to latter half of the twentieth century, Werner conceptualized development from an organismic-developmental point of view. He emphasized that orthogenesis constituted a general regulative principle for all developmental changes (e.g., Raeff, 2016; Werner, 1948, 1957). Werner's writings, and those of his colleagues (e.g., Seymour Wapner, Bernard Kaplan, and Jonas Langer), have contributed immeasurably to the advancement of organismic-developmental theory as well as to the appropriate conceptualization of the continuity–discontinuity issue (see too Raeff, 2011, 2016). I discuss Wapner's specific theoretical ideas (e.g., Wapner, 1969, 1995; Wapner & Demick, 1998; Werner & Wapner, 1949, 1952) in Chapter 10.

Werner (1957) saw that considerable confusion existed among developmental scientists over the continuity–discontinuity issue. At the crux of this confusion was a lack of understanding about two different aspects of change, that is, two features of change that could be *integratively* understood by the orthogenetic principle (e.g., see Chapter 5). This integration reflects the use of the relational developmental systems (RDS) idea of the integration of opposites (discussed by Overton, 2015a, as the first moment of RDS-based research; see Chapter 2).

To briefly reiterate my previous discussion of the orthogenetic principle, the principle suggests that discontinuity should characterize development when differentiated attributes emerge from global attributes. In turn, the principle suggests that continuity should characterize development when the interrelation among attributes becomes increasingly more hierarchically organized over the course of an individual's development. Given the integrated character of change specified within the orthogenetic principle, developmental processes can change quantitatively (i.e., there can be variational change, or changes in the amount, amplitude, frequency, or magnitude of a variable) or qualitatively (i.e., there can be transformational change, or changes in what exists within the developmental system) (Overton, 2015a). Understanding the developmental links between variational and transformational change is important for both descriptive and explanatory reasons. Indeed, understanding the bases of continuity and change in human development is a task fundamentally linked to the nature–nurture issue. Granted that there are processes involved in the course of development, and that these processes lie within the province of nature and nurture or, in the view articulated in the genre of theories of which Werner's conception (organismic-developmental theory, an instance of RDS-based models; Raeff, 2016; see too Chapter 10) is an example, in nature↔nurture relations, developmental scientists explore how these processes function across the life span of a species. Do the processes involved in determining development remain the same or do they change in their functioning across ontogeny? In turn, do the psychological functions of the person and/or the variables involved in his or her behavior stay the same or change across life?

If the same processes and/or variables are involved in development at different times in the ontogeny of a species, then *continuity* is present. Alternatively, if different processes account for, and/or if there are different psychological functions involved in development at different times in the ontogeny of a species, then *discontinuity* is present.

DEFINING THE ISSUE

In a general way, if things across the life span (e.g., variables, processes) stay the same, *continuity* exists, and if things across life change, *discontinuity* exists. However, greater precision and clarification of the continuity–discontinuity issue are necessary. Within human development, the continuity–discontinuity issue pertains to issues of the description and explanation of within-person change.

Descriptions of Intraindividual Change

In seeking to systematically represent the changes a person goes through across time, that is, in trying to describe intraindividual change, a developmental scientist may ask whether the behavior being described takes the same form across time. Simply, does the behavior look the same? When engaging in peer-group relations, does a person, when a child, an adolescent, and an adult do the same things? If behavior seen at one point in the life span can be represented or depicted in the same way as behavior at another point, then *descriptive continuity* exists. If behavior seen at one point in the life span cannot be represented or depicted in the same way as behavior at another point, then *descriptive discontinuity* exists.

The former situation would exist if what a person did with his or her peers in order to "have

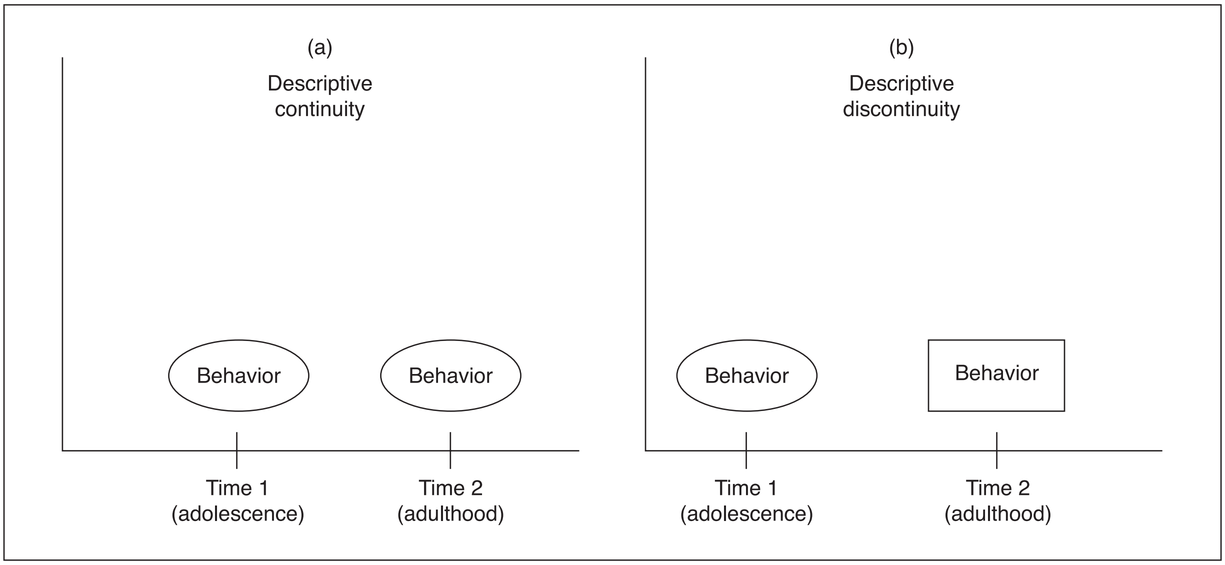


Figure 8.1 (a) If behavior can be represented in the same way at two times in the individual's life span, then descriptive continuity exists between these two points. (b) If behavior cannot be represented in the same way at two times in the individual's life span, then descriptive discontinuity exists between these two points.

fun" were the same in adolescence and adulthood, whereas the latter situation would exist if the person engaged in different activities during these two times. Further illustration of descriptive continuity and discontinuity is seen in Figure 8.1. Part (a) of the figure illustrates no change in intraindividual status (continuity), whereas Part (b) shows change in intraindividual status (discontinuity).

Explanations of Intraindividual Change

Changes in the description of behavior across a person's life can occur for many reasons. In fact, even the *same* change, regardless of whether it is descriptively continuous *or* discontinuous, can be explained by different reasons. If the same explanations are used to account for behavior across a person's life, then this constancy means that behavior is interpreted as involving unchanging processes. In this case, there is *explanatory continuity.* If, however, different explanations are used to account for behavior across a person's life, then there is *explanatory discontinuity.* In other words, if the variables used to account for developmental processes do not vary from Time 1 to Time 2 in a person's life, explanatory continuity exists; if the variables used to account for developmental processes do vary from Time 1 to Time 2 in a person's life, explanatory discontinuity exists.

Simply, then, if the explanations of behavior (e.g., processes invoked to account for behavior) remain the same across time, continuity exists; if these explanations change, discontinuity exists. These relations are illustrated in Figure 8.2.

Descriptive and Explanatory Combinations

It is possible to have any combination of descriptive continuity–discontinuity and explanatory continuity–discontinuity. For instance, suppose a developmental scientist were interested in accounting for a person's recreational behavior at different

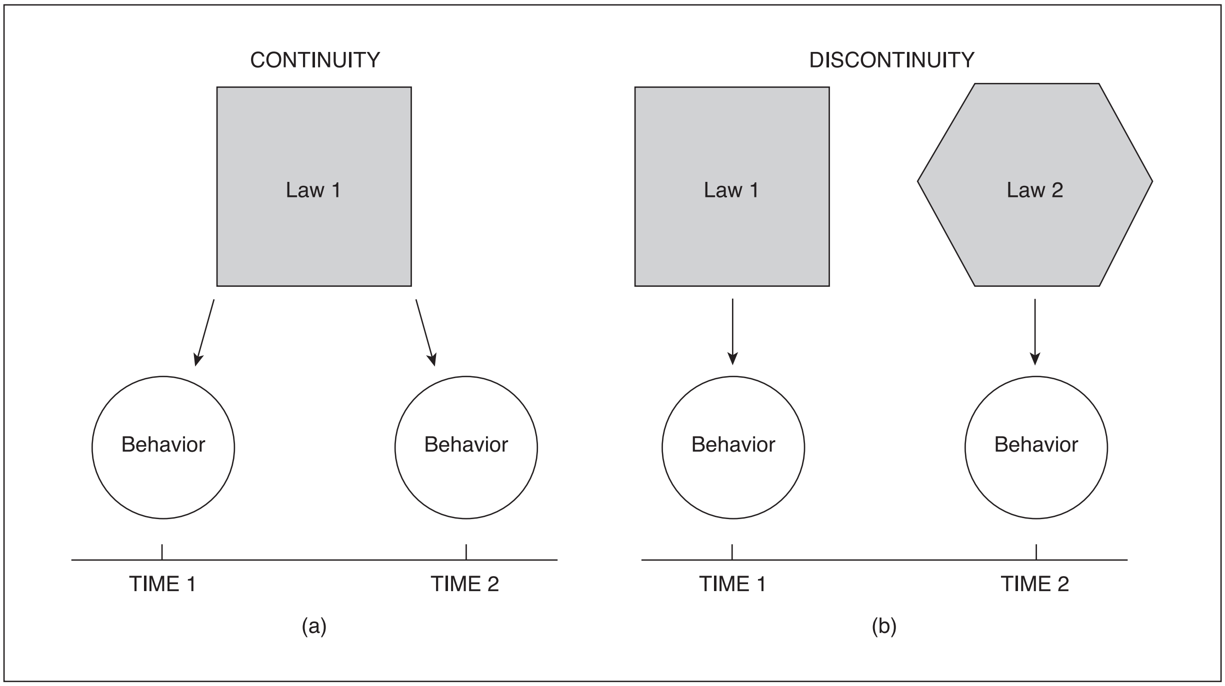


Figure 8.2 An illustration of (a) continuity and (b) discontinuity in development.

times in the individual's life and tried to explain this behavior through the use of motivational ideas. There might or might not be changes in the main recreational behaviors (e.g., bicycle riding or aerobic exercise) from childhood to adolescence. There might be descriptive continuity or discontinuity. In either case, however, the developmental scientist might suggest a continuous or a discontinuous explanation.

For instance, it might be argued that recreational behavior—whatever specific form it may take—is always motivated by curiosity. Bike riding in childhood and adolescence, or bike riding in the former period and aerobic exercise in the latter, may just be determined by the person's curiosity about seeing where the bike ride can take him or her (in the former case) or about learning about the benefits of new exercise regimens (in the latter case). Thus, the developmental scientist could account for behavior based on an explanatory continuous interpretation.

Alternatively, it might be argued that recreational behavior in adolescence is determined not by curiosity motivation but rather by sexual motivation. That is, although curiosity led to bike riding in childhood, the adolescent goes to aerobic exercise classes to meet possible dating partners. Here, then, the developmental scientist could account for behavior based on an explanatory discontinuous interpretation.

Further illustration of explanatory continuity and explanatory discontinuity is presented in Figure 8.3. Part (a) is an illustration of no intraindividual change in the explanations for behavior (continuity). Part (b) shows intraindividual change in the explanations for behavior (discontinuity). In both portions of the figure, the behavior being described is continuous; as previously indicated, however, descriptive continuity or discontinuity and explanatory continuity or discontinuity can occur. Intraindividual change may take a form fitting into any of the quadrants shown in Figure 8.4.

With Change Form 1, descriptions of behavior would remain the same (e.g., the person engages in recreational activities by doing the same thing); similarly, the reasons used to explain why the behavior did not change would also remain the same (e.g., the same motive is present). Change Form 2 would involve the same descriptions of behavior (e.g., bike riding as the major form of recreation) across time, but the explanation for the identical behavior would change from Time 1 (e.g., the person rides to master a motor skill) to Time 2 (e.g., the person rides

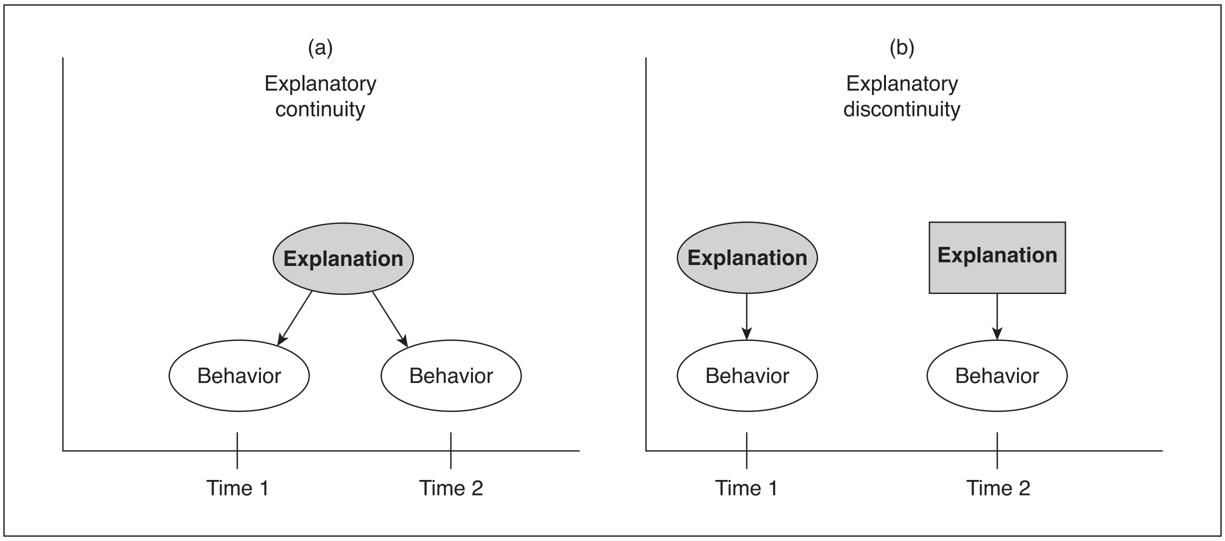
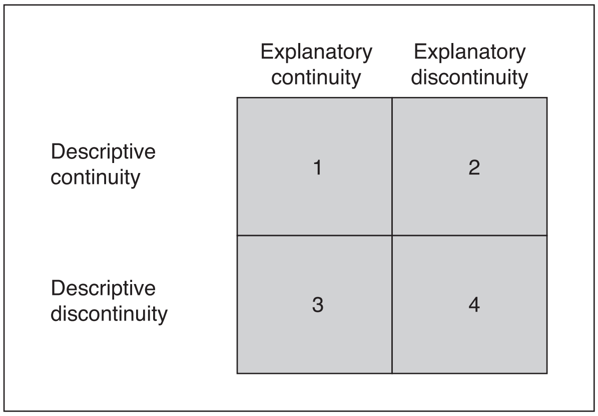


Figure 8.3 (a) If behavior can be accounted for in the same way at two times in the person's life span, then explanatory continuity exists between these two points. (b) If behavior cannot be accounted for in the same way at two times in the individual's life span, then explanatory discontinuity exists between these two points.



	Explanatory continuity	Explanatory discontinuity
Descriptive continuity	1	2
Descriptive discontinuity	3	4

Figure 8.4 Intraindividual change may take a form reflecting any combination of descriptive and explanatory continuity or discontinuity.

to meet possible dating partners). Change Form 3 would involve the behavior changing from Time 1 (e.g., bike riding) to Time 2 (e.g., aerobic exercise), but the explanation for behavior would remain the same (e.g., motivation to master motor skills). Finally, Change Form 4 involves the behavior being understood on the basis of different reasons (e.g., Time 1 behavior involves a motor-skill motive and Time 2 involves an interest in dating).

Quantitative versus Qualitative Changes

Descriptions or explanations of development can involve quantitative (variational) or qualitative (transformational) changes. Descriptively, quantitative changes involve differences in how much (or how many) of something exists. For example, in adolescence, quantitative changes occur in such areas as height and weight as a consequence of adolescent growth that involves these attributes; these changes are often interpreted as resulting from quantitative increases in the production of growth-stimulating hormone.

In turn, descriptive qualitative changes involve differences in what exists, in what sort of phenomenon is present. The emergence in adolescence of a purported drive-state never before present in life—that is, a reproductively mature sexual drive (Freud, 1969)—and the theorized emergence in adolescence of new and abstract thought capabilities not present in younger people—that is, formal operations (Piaget, 1950, 1970)—are instances of changes *interpreted* as arising from qualitative alterations in the person. The person is not just "more of the same"; rather, the person has a *new* quality or characteristic.

Explanations of development can vary also in regard to whether a developmental scientist *accounts* for change by positing quantitative changes (e.g., increases in the amounts of growth hormone present in the bloodstream), or by positing new, qualitatively different (emergent) reasons for behaviors. For example, Erikson (1959) proposed that an infant's behaviors in his or her social world are predicated on the need to establish a sense of basic trust in the world, whereas an adolescent's social behaviors involve the need to establish a sense of identity, or a self-definition, and an adult's social behaviors involve efforts to create a sense of generativity.

In other words, it is possible to offer an explanatory discontinuous interpretation of development involving *either* quantitative or qualitative change. For instance, when particular types of explanatory discontinuous qualitative changes are said to be involved in development (those linked to biological or, more specifically, genetic, reductionist accounts; e.g., Lorenz, 1965), the critical-periods hypothesis discussed in Chapter 7 is often raised (e.g., Erikson, 1959, 1968). Here, development is alleged to be qualitatively different than in prior or subsequent ontogenetic periods because biologically-shaped maturational timetables create—independent of any influence of the context within which the individual is embedded—specific times in life when specific phenomena (e.g., imprinting or attachment; Lorenz, 1965) must develop, or else they may never adequately develop. Although no credible evidence exists for the presence of biologically-determined critical periods in human development (e.g., Colombo, 1982; see Chapter 7), the point of noting this concept is to illustrate the idea that, on the basis of adherence to a particular theory of development (e.g., a predetermined-epigenetic model or a nature-reductionist model), qualitative changes may be believed to characterize ontogeny and, if so, then discontinuous explanations of change are needed.

Accordingly, virtually any statement about the character of intraindividual development involves, explicitly or implicitly, taking a position in regard to three dimensions of change: (1) descriptive continuity–discontinuity; (2) explanatory continuity–discontinuity; and (3) the quantitative versus the qualitative character of the descriptions and explanations—that is, the quantitative–qualitative dimension pertains to both description and explanation. This situation is illustrated in Figure 8.5. As suggested by this figure, a developmental scientist may have descriptive quantitative discontinuity coupled with explanatory qualitative continuity, or descriptive qualitative continuity coupled with explanatory quantitative discontinuity, and so forth.

For example, a feature of personality (e.g., a component of temperament such as quality of mood; Thomas et al., 1963) may remain descriptively the same across life. It may be represented or depicted isomorphically at two different temporal points (e.g., positive mood may be represented by the percentage of facial expressions per unit time which are scored as indicative of smiling). Such cases, therefore, may be an instance of descriptive qualitative continuity. However, *more* of this qualitatively invariant phenomenon may exist at Time 2 (e.g., there may be more smiles per unit time), and thus descriptive quantitative discontinuity may be coupled with descriptive qualitative continuity.

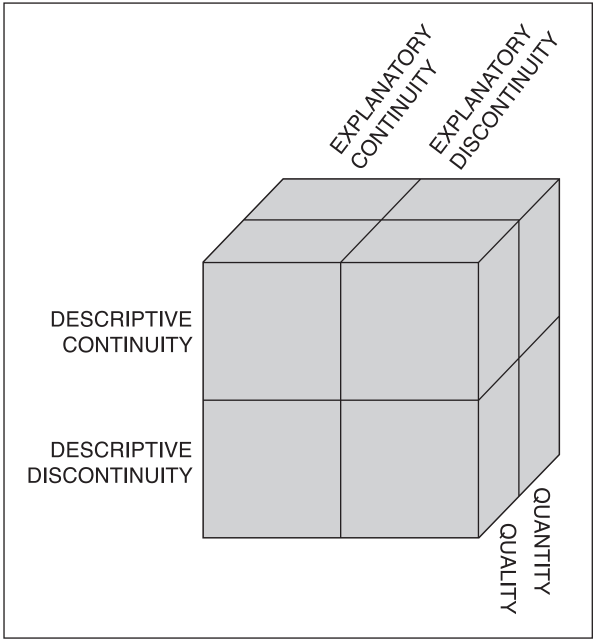


Figure 8.5 The intraindividual change box. Intraindividual development involves change along three dimensions—descriptive continuity–discontinuity; explanatory continuity–discontinuity; and a quantitative–qualitative dimension.

Moreover, both descriptive quantitative discontinuity and descriptive qualitative continuity may be explained by the same ideas, such as by continuous explanatory principles. Smiling may be assumed to be released across life by biogenetically-based physiological mechanisms. Alternatively, descriptive continuity or descriptive discontinuity may be explained by different ideas, such as by discontinuous explanatory principles. For instance, smiling may be assumed to be biogenetically released in early infancy and mediated by cognitively and socially textured processes across subsequent developmental periods. Indeed, if different explanations are invoked, they may involve statements that constitute either quantitatively or qualitatively altered processes.

In short, the particular couplings that a developmental scientist posits to be involved in human life will depend on the substantive domain of development he or she is studying (e.g., intelligence, motivation, personality, or peer-group relations) and, primarily, on the theory that he or she uses to frame this domain of development. That is, any particular description or explanation of intraindividual change is the result of a particular theoretical view of development. This idea implies that commitment to a theory that focuses only on specific variables or processes will restrict a developmental scientist's view of the variety of changes that may characterize development. Indeed, theory, not data, is the *primary* lens through which one "observes" continuity or discontinuity in development.

CONTINUITY–DISCONTINUITY AS A THEORETICAL ISSUE

For a long time, many developmental scientists held continuity–discontinuity to be an empirical issue

(Lerner & Nesselroade, 1991). Developmental scientists contended that the existence of continuity or discontinuity for the development of a given psychological process could be determined only from the results of research. Of course, this position has a degree of validity. Whether a developmental scientist sees continuity or discontinuity in behavioral development is partially dependent on research data. However, the point is that, because theory determines what variables are studied within a selected domain of human development, the results of research about these variables are not the only determining factor for the existence of continuity or discontinuity. There are other, more important factors, and these are primarily theoretical.

The Role of Theory

A change can take any one of several forms, and even the same descriptive change can be interpreted (explained) in different ways. The primary reason that developmental scientists interpret a given change in contrasting ways is that theoretical differences exist among them. For instance, if a developmental scientist adopts a theoretical position stressing the progressive, hierarchical integration of the organism (e.g., Gagné, 1968), he or she will necessarily view development as essentially continuous. On the other hand, if a developmental scientist emphasizes the progressive differentiation of the organism, he or she will view development as essentially discontinuous.

Accordingly, a given theoretical position might lead one developmental scientist to interpret a given piece of empirical evidence in one way (e.g., as consistent with a continuity position), whereas a developmental scientist with a different theoretical position might interpret that same empirical fact in another way (e.g., as consistent with a discontinuity position). To illustrate, whether a developmental scientist views infant babbling as continuous or discontinuous with speech depends on his or her particular theoretical perspective. Similarly, the events of adolescence may be interpreted as continuations of processes present in earlier ontogenetic periods or as results of processes present only in adolescence. Thus, Davis (1944) explained storm-and-stress behavior in adolescence (behavior that was regarded as descriptively discontinuous) by proposing social-learning principles applicable to earlier ontogenetic periods. That is, he used an explanatory continuous idea to account for descriptive changes in the behaviors of children versus adolescents. Hall (1904), however, coupled descriptive discontinuity with explanatory discontinuity, and argued that the adolescent period recapitulated a distinct portion of phylogeny.

In turn, within the adult and aged portions of the life span, there are decreases, or "losses," in some facets of perceptual-motor and cognitive processing skills (Baltes, Lindenberger, & Staudinger, 2006). A developmental scientist may perceive such losses as involving evidence for the emergence of a new developmental process, one summarized by the term "aging," and thus regard such changes as evidence for qualitative discontinuity. Alternatively, as posited by Baltes (1997; Baltes et al., 2006), and as agreed on by some developmental scientists (Overton, 2015a), a developmental scientist may propose that all of development across the life span involves a common developmental process. Accordingly, as is described by Baltes (1987; Baltes et al., 2006), an integration of processes of gain and loss is present across the life span. As such, there is no new process termed "aging," but only a quantitative change in the gain–loss balance (see too Riley, 1979).

The point of recasting the ideas of Davis, Hall, and Baltes into continuity–discontinuity terms is to indicate that whether a given behavior is seen as continuous or discontinuous is not primarily an empirical issue. It is a theoretical issue (Langer, 1970; Raeff, 2011, 2016; Werner, 1957). Furthermore, since theoretical differences affect the ways in which one collects and analyzes data, even descriptions of behavior as continuous or discontinuous are primarily matters of theoretical interpretation and not of empirical "reality."

Suppose that a researcher wants to study the level of aggression in the play situations of children from the ages of 6 through 11 years. The researcher develops a measure of aggression that is applicable to children throughout this age range, and he or she studies groups of children at each age level and obtains scores for each child. Imagine that the researcher has a theory about the development of

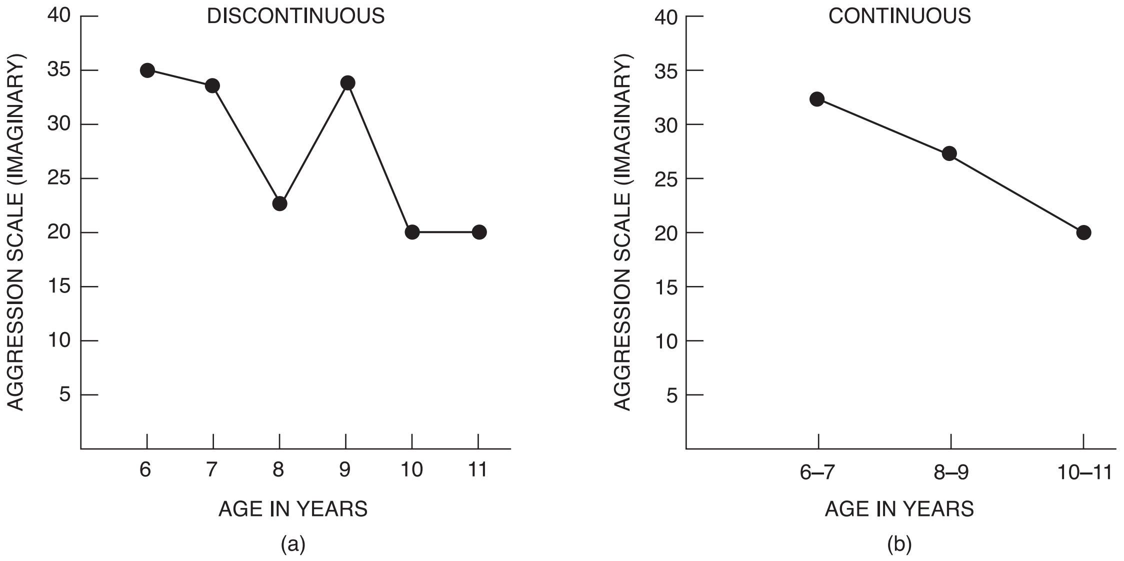


Figure 8.6 How one handles data may contribute to whether one views development as being discontinuous (a) or continuous (b).

aggression that predicts that aggression in children of this age range should be discontinuous. Thus, the researcher might specifically expect to see abrupt changes in the levels of aggression, and he or she accordingly graphs the results of the study so that any year-by-year fluctuations in aggression levels would be evident. Such a graph is seen in Figure 8.6a. The graphed results would reveal abrupt fluctuations in measured aggression levels in play situations within the age range studied. The researcher could use these results to support the notion that aggression in play situations is a discontinuous phenomenon in children.

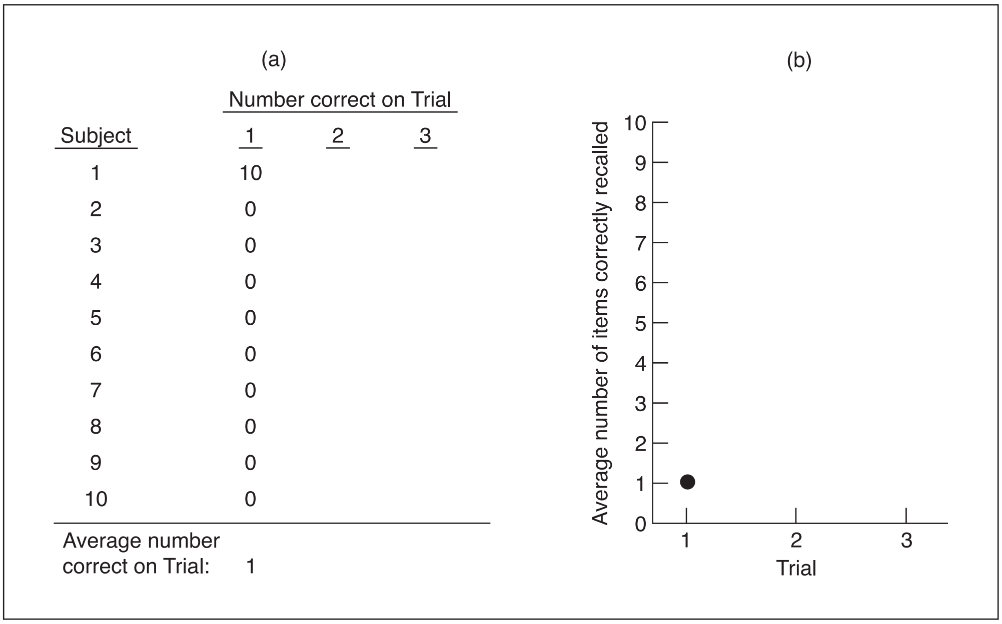
On the other hand, another researcher's theory might hold that aggression is a continuous phenomenon in children of this age range. Accordingly, this researcher might not expect any abrupt changes in levels of aggression with age; instead, he or she might expect such development to involve a gradual process of gain (for instance, in children with problems of impulse control) or loss (for example, in children who are becoming increasingly socialized into conforming to social norms against interpersonal violence). Thus, for ease and clarity in the analysis and presentation of the results of the study (and/or because the aggregation of data may lead to a more reliable estimation of data points), the researcher might use the average scores for a combination of the 6- and 7-year-olds as one data point on the graph, the average scores for a combination of the 8- and 9-year-olds as another data point, and so on (see Figure 8.6b). The researcher could now use these results to support the contention that aggression in play situations decreases gradually as children age and that aggression is therefore a continuous phenomenon.

As another example, suppose one researcher believed in a theory that specified that learning in childhood involved general processes leading to smooth, continuous, incremental learning. Suppose, in turn, that another scholar used a theory that suggested that childhood learning was discontinuous—that it involved jumps or spurts in knowledge and that different children spurted ahead at different times. Both researchers might do the same experiment to test their respective views, but the way they handled their data—and what their data purportedly proved—would reflect more about their theoretical biases than the actual "facts" about learning.

Suppose that, to study learning in children, 10 elementary-school students were selected on the basis of those factors that might influence their ability to learn (e.g., their cognitive abilities, ages, educational levels, etc.). Each student would be given a list of 10 nonsense syllables—two consonants and a vowel—for which no previous knowledge existed. Syllables like "guz," "weg," or "zek" might be used. After seeing the list, the students would be asked to recall the items. The number of words correctly recalled on each of the trials would be the score the researcher would record for each student.

A researcher who believed in general (nomothetic) processes and continuity might decide to pool the responses across students because of the researcher's belief that learning was generally the same for all children (and because of the relation between aggregation and reliability of estimation noted earlier). Thus, in graphing the results, the researcher might use the group average for the number of items correct on Trial 1 (see Figure 8.7a). Suppose that on Trial 1, Participant 1 recalled all items correctly but all other participants recalled no items. The total number of items recalled for this trial would be 10, and the average number for the 10 participants would be 1. Thus, the point on the graph of Figure 8.7b would be entered. Now suppose that, on Trial 2, Participant 1 continued to recall all 10 items correctly, and that Participant 2 did the same—whereas all other participants continued to score zero; if so, then the situation in Figure 8.8 would occur. The total number of correct items would be 20, the average would be 2, and the second point on the graph (see Figure 8.8b) would be entered. Similarly, if on the third trial Participant 3 recalled all 10 items—as Participants 1 and 2 continued to do—but all others still scored zero, a situation like that in Figure 8.9 would occur.

If such patterns continued and the researcher connected the points in the figure, he or she would see evidence that learning was smooth and continuous. Because of the belief in general processes of learning (i.e., that all people learn in the same manner), the researcher might not look at the individual differences in the participants' learning, and the data graphed would be group scores. Thus, in this example (which is intentionally extreme to make a point about the theoretical basis of


(a)

	Number correct on Trial		
Subject	1	2	3
1	10		
2	0		
3	0		
4	0		
5	0		
6	0		
7	0		
8	0		
9	0		
10	0		
Average number correct on Trial:	1		

Figure 8.7 Results of a study of learning in childhood—the data collected and graphed for the group for Trial 1.

Figure 8.8 Results of a study of learning in childhood—the data collected and graphed for the group for Trial 2.

(a)

Subject	Number correct on Trial 1	2	3
1	10	10	
2	0	10	
3	0	0	
4	0	0	
5	0	0	
6	0	0	
7	0	0	
8	0	0	
9	0	0	
10	0	0	
Average number correct on Trial:	1	2	

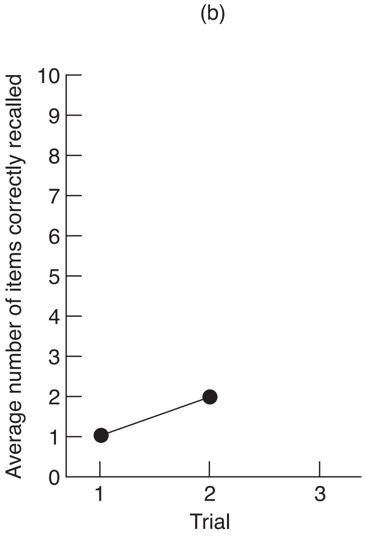
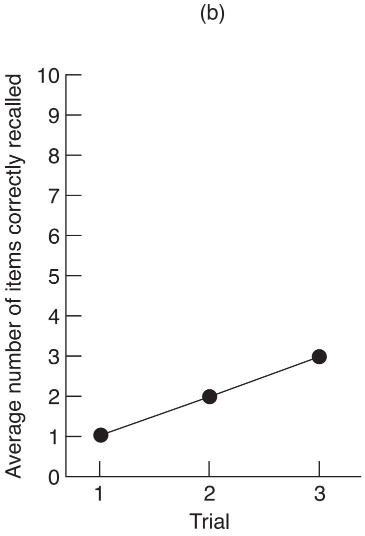


Figure 8.9 Results of a study of learning in childhood—the data collected and graphed for the group for Trial 3.

(a)

Subject	Number correct on Trial 1	2	3
1	10	10	10
2	0	10	10
3	0	0	10
4	0	0	0
5	0	0	0
6	0	0	0
7	0	0	0
8	0	0	0
9	0	0	0
10	0	0	0
Average number correct on Trial:	1	2	3



continuity–discontinuity decisions), the group data would support a continuity view of learning. Yet, if analyzed differently, the very same data could support a discontinuity interpretation (see Rose, 2016, for other examples).

If a researcher who believed in discontinuity graphed the data shown in Figures 8.7, 8.8, and 8.9, he or she might emphasize the individuality of learning processes—that children show discontinuous spurts in learning after varying lengths of time in which no learning is evidenced. Thus, from the same data, a graph like that of Figure 8.10 could be drawn and, as such, the individual data would now support the discontinuity view of learning.

I have already stated that this example is extreme. Experienced, competent researchers would be sensitive to such major trends in their data. However, this point is precisely the one I wish to make. Most often, trends in data are *considerably* more subtle than those in Figures 8.7 through 8.10. As such, the impact of a theoretical orientation on the collection and handling of data is not as readily obvious. This situation not only requires vigilance about how researchers—because of their biases—may affect the nature of the "realities" they "discover;" it also highlights the need to be aware of how depictions of data relate primarily to theoretical issues and not to empirical ones.

In sum, both in explaining and describing intraindividual change as continuous or discontinuous, a developmental scientist's theoretical perspective is a major determinant of what particular change format (see Figure 8.5) is advanced as representative of development. Furthermore, it is important to note that even among those developmental scientists who agree that development must be explained by discontinuous terms, there are important differences in the discontinuities they specify as being involved in change. But here, too, the basis of these differences involves theoretical contrasts. With theoretical issues so central, then, in the continuity–discontinuity issue, it may be of considerable use to have a means to organize these issues systematically. To do so, I turn again to the ideas of Heinz Werner.

WERNER AND THE CONCEPTUALIZATION OF QUANTITATIVE AND QUALITATIVE CHANGE

Continuity–discontinuity across the life span involves description of changes or constancies in human functioning. Werner (1948, 1957) explained the superordinate conceptual importance of the qualitative–quantitative dimension of change through discussing the features of orthogenetic development and by embedding this discussion

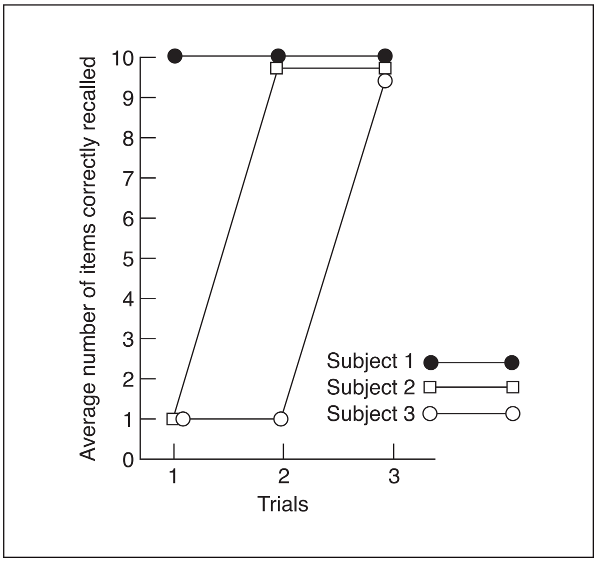


Figure 8.10 Results of a study of learning in childhood—the data collected are the same as those of Figures 8.7, 8.8, and 8.9, but here they are graphed to show individual (as opposed to group) performances across trials.

Heinz Werner

within organismic-developmental ideas, ideas that reflect RDS-based conceptions. As also emphasized by Witherington and Lickliter (2016; see too Lickliter & Honeycutt, 2015; Mascolo, 2013; Mascolo & Fischer, 2015; Overton, 2015a; van Geert & Fischer, 2009; Witherington, 2011, 2015), Raeff (2016, pp. 12–13) explains that in the RDS-based view:

> behavior emerges out of interrelations among "ongoing processes intrinsic to the system" (Lewis, 2000, p. 38). Claiming that human functioning emerges through interrelations among intrinsic constituent processes means that one does not have to involve external, antecedent, or independent factors to explain what people do. In addition, the concept of emergence stands in explicit contrast to any conceptualization of behavior and development as predesigned or predestined by, for example, genetics or how the brain is "hardwired." Rather, what a person does emerges, or is always coming into being, through the ongoing dynamics of constituent processes.

Quantitative Change

In regard to the quantitative aspect of development, there may be change in a feature of development in regard to how much of something exists. Quantitative (variational) change is an alteration in the amount, frequency, magnitude, or amplitude of a developmental variable or process. For example, imagine that a person's weight had been measured at the same time during each of his eighth through thirteenth years. He weighed 125 pounds when he was measured at Ages 8, 9, 10, 11, and 12; but he weighed 150 pounds when he was measured at Age 13. Thus, a quantitative change occurred in how much weight existed between the times of measurement occurring at 12 and 13 years of age.

This example is illustrated in Figure 8.11. Here, quantitative change is abrupt. There are no intermediate steps by which the person's weight gradually moved from one level (amount) to the next. In measuring this change, there is a gap between one point in the measurement curve and another; that is, the curve representing the different measurements is not smooth (as in Figure 8.12) but, rather, it has an abrupt change in its direction (as in Figure

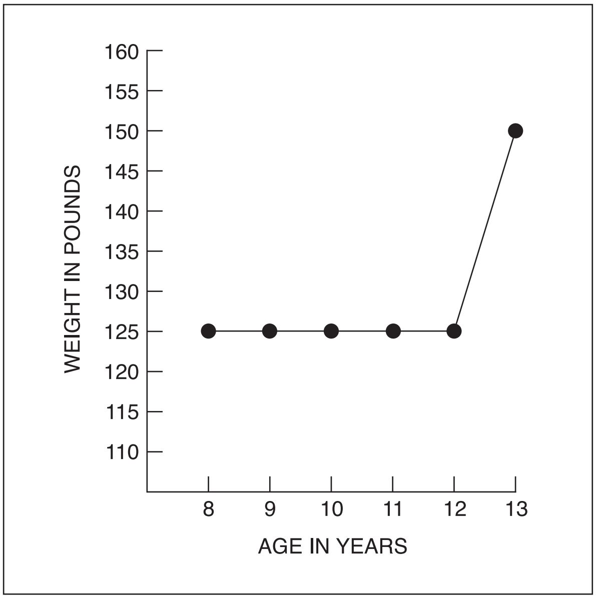


Figure 8.11 An example of an abrupt change (quantitative discontinuity).

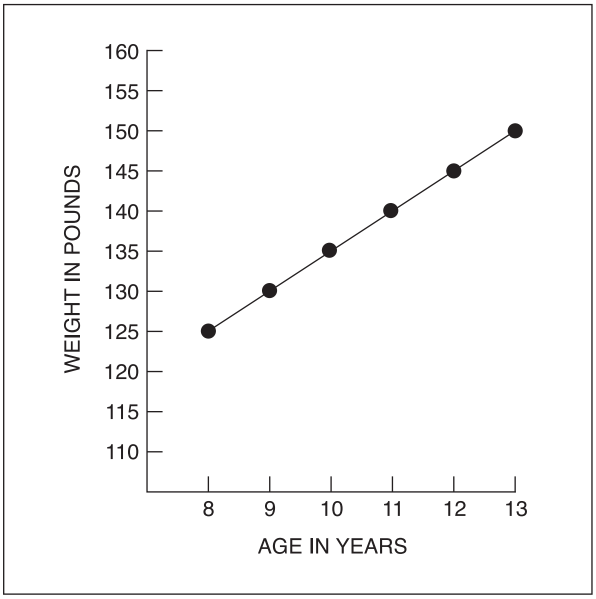


Figure 8.12 An example of a gradual quantitative change.

8.11). There is a "gappiness" in the curve—a lack of an intermediate stage between the earlier and later levels of a variable (Werner, 1957, p. 133). The occurrence of an abrupt change is *quantitative discontinuity.*

Alternatively, the child's change in weight could have been gradual, as is illustrated in Figure 8.12. By gaining 5 pounds a year, the child gradually goes from 125 to 150 pounds between his eighth and thirteenth years. With gradual quantitative changes the rate of change stays the same—it is continuous—from one measurement time to the next. This is *quantitative continuity.*

Qualitative Change

The second aspect of change that Werner specified is the qualitative one. Here, developmental scientists are primarily concerned not with *how much* of something exists but with *what* exists—what kind or type of thing exists. Developmental scientists are concerned with whether a new quality has come to characterize an individual, whether something new has emerged in development. When developmental scientists are considering qualitative change, they are dealing with *epigenesis*, or emergence (Blumberg, 2016).

In previous chapters, I discussed the central role of epigenesis in organismic conceptions of development. In distinguishing between quantitative and qualitative aspects of change, Werner (1957) highlighted a core conception of the organismic position. Some of the types of changes that comprise development are emergent changes. These are changes in what exists rather than in how much of something exists. Something new comes about in development, and because it is new—because it is qualitatively different from what went before—it cannot be reduced to what went before. Hence, consistent with the analogy I presented in Chapter 4, if at Time 1 a person can be represented by 10 oranges and at Time 2 he or she can be represented by a motorcycle, the Time 2 motorcycle status cannot be reduced to the Time 1 orange status.

To take another example, Anna Freud (1969) theorized that, before puberty, a person may be characterized as being (in part) comprised of several drives (e.g., a hunger drive, a thirst drive, a drive to avoid pain, and, perhaps, a curiosity drive). With puberty, however, she suggested that a new drive emerges (or, at least, emerges in a mature form)—the sex drive (Freud, 1969). With this *emergence*—that is, with the development of a structure or function that cannot be (completely) reduced to a prior form—the adolescent begins to have new feelings, thoughts, and behaviors, which may be interpreted as being a consequence of this new drive (Freud, 1969). The emergence of this new drive is an instance of qualitative discontinuity. The sex drive cannot be reduced to hunger and thirst drives, for instance.

Qualitative changes are by their very nature discontinuous. A qualitative, emergent, epigenetic change is *always* an instance of discontinuity. Moreover, not only is an emergent change an irreducible change (in that it cannot be construed as a derivative of a prior form), but it is a change characterized by gappiness. Developmental gappiness occurs when there is a lack of an intermediate level between earlier and later levels of development, as seen in the emergence of the sex drive. Gappiness must also be a part of an emergent change. The presence of an intermediate step between what exists at Time 1 and the new quality that emerges at Time 2 would suggest that the new quality at Time 2 could be reduced through reference to the intermediate step. Because an emergent change is defined in regard to its developmental irreducibility to what went before, gappiness must also be a characteristic of any emergence.

In sum, the characteristics of emergence and gappiness are necessary to describe qualitatively discontinuous changes in development. On the other hand, the characteristic of gappiness (abruptness of change) alone seems to be sufficient for characterizing quantitatively discontinuous changes. Thus, as Werner (1957) stated:

> It seems that discontinuity in terms of qualitative changes can be best defined by two characteristics: "emergence," i.e., the irreducibility of a later stage to an earlier; and "gappiness," i.e., the lack of intermediate stages between earlier and later forms. Quantitative discontinuity on the other hand, appears to be sufficiently defined by the

> second characteristic . . . To facilitate distinction and alleviate confusion, I would suggest substituting "abruptness" for quantitative discontinuity, reserving the term "discontinuity" only for the qualitative aspect of change.
>
> (p. 133)

Through his elaboration of the implications of the orthogenetic principle, Werner provided developmental science with an integrated means to consider the continuity–discontinuity issue. He gave developmental science the conceptual means by which to discriminate between quantitative continuity–discontinuity and qualitative continuity–discontinuity.

Which of these two concepts (continuity or discontinuity) best characterizes the changes involved in human development? Werner's answer to this question would be that *both* concepts characterize developmental changes. That is, Werner provided developmental science with a concept that allows scholars to see the interrelation of continuity and discontinuity in development and to see, again, that the continuity–discontinuity issue is primarily theoretical. Werner's ideas allow developmental scientists to understand that whether one posits continuity or discontinuity as characterizing development rests primarily on the implicit theoretical assumptions and the explicit theoretical positions one maintains. As I have explained (e.g., earlier in this chapter and in Chapter 5), the concept that allows developmental scientists to see this state of affairs clearly is the orthogenetic principle. Accordingly, I consider the use of the orthogenetic principle in organizing the key conceptual concerns involved in the continuity–discontinuity issue.

THE ORTHOGENETIC PRINCIPLE

Werner postulated that developmental psychology had one general regulative principle of development. This principle, which he termed the *orthogenetic principle*, states that "whenever development occurs it proceeds from a state of relative globality and lack of differentiation to a state of increasing differentiation, articulation, and hierarchic integration" (Werner, 1957, p. 126).

Thus, whenever development occurs, the changes that characterize it follow a specific *structural* course. At Time 1 in development, a particular psychological process, or variable, would be relatively more global—general, or undifferentiated—than at subsequent times. At Time 2 in development, however, this same psychological process would have become relatively differentiated—more specific. In addition, the differentiated status of the process would exist in the form of a hierarchy.

Figure 8.13 A young child might use the same word, for example, puppy, to name different animals. Photos by Kristina Schmid Callina.

An illustration of the orthogenetic principle elucidates its meaning. Consider a relatively young child, for example, a child of about 14 months of age. Suppose an uncle or aunt of the child spent a day with him or her and decided to take a short walk. While doing so the adult sees a dog. The child points and says "puppy." The adult smiles, perhaps, and says, "Yes, that's a puppy." But soon the adult and child encounter a cat and the child also points and says "puppy." Similarly, when the child sees a picture of a raccoon in a magazine, he or she also says "puppy." This situation is depicted in Figure 8.13, which shows a young girl involved in just such verbal behavior.

A developmental scientist might conclude, then, that this child has a relatively global (undifferentiated) concept of animals. The child calls any furry creature with four legs and a tail a puppy. In other words, this child's conceptual development, at least insofar as animals are concerned, is in a state of globality, or lack of differentiation. Now, suppose that the uncle or aunt visited this same child about a year or so later. On the basis of Werner's orthogenetic principle, developmental scientists would expect that if the child's animal concepts had developed, they would be relatively less global—they would be more differentiated. The child might now say "puppy" or even "dog" only when a dog is, in fact, in view, and "cat," "raccoon," and so on when the appropriate animal was in sight.

On another, still later visit the adult might notice some other things. The child might show evidence of knowing that all dogs, cats, horses, and so on are animals and, in turn, that animals are different from trees. The child's animal concepts had not only become more differentiated but had also formed into a hierarchy—that is, cats, dogs, and raccoons had all become instances of the class "animals." Still later, perhaps, the visitor would see that increasing differentiation and hierarchical organization had occurred. The child would have developed a concept not only of dog but also of different breeds of dogs and, in addition, might be able to show evidence of knowing that within each breed there are puppies and adults and/or males and females of that breed. Moreover, the child might be able to differentiate among types of plants (e.g., trees, flowers, and vegetables). Furthermore, the child might know that both plants and animals are in a similar, higher-order class (living things) and are different from nonliving things.

Thus, what might be seen in regard to the development of the child's animal concepts is a change from having relatively global, undifferentiated concepts to having concepts organized into a hierarchical structure. This development is illustrated in Figure 8.14, which shows that the orthogenetic principle can be used to describe the nature of developmental change: all developmental changes should proceed from globality to differentiation and hierarchical organization. Thus, Werner (1957), in asserting that the orthogenetic principle was a general, regulative principle for all developmental change, depicted what he believed to be the ubiquitous nature of developmental change. In so doing, he provided developmental science with a framework within which to consider the continuities and discontinuities that may be involved in any specific instance of human development.

The Orthogenetic Principle and the Continuity–Discontinuity Issue

Jonas Langer (1970), an eminent former student of Heinz Werner, and Catherine Raeff (2011, 2016), another distinguished scholar trained in the organismic-developmental tradition of Werner, contributed to clarifying how the orthogenetic principle helps developmental scientists understand the continuity–discontinuity issue. Langer pointed out that *both* continuity and discontinuity may be considered to characterize development. Discontinuity occurs as the relatively global organization of earlier times in development becomes differentiated. On the other hand, continuity occurs as the differentiated organism is hierarchically integrated. Developmental continuity might be identified by pointing out that earlier developments will become subsumed under later ones—that what went before will be subordinated to later, superordinate developments.

Hence, development is characterized by a *synthesis*, an interweaving, of two opposing tendencies. First, there is the tendency to become more differentiated. This type of change involves the tendency for new characteristics to emerge from previous global characteristics, that is, the tendency for global

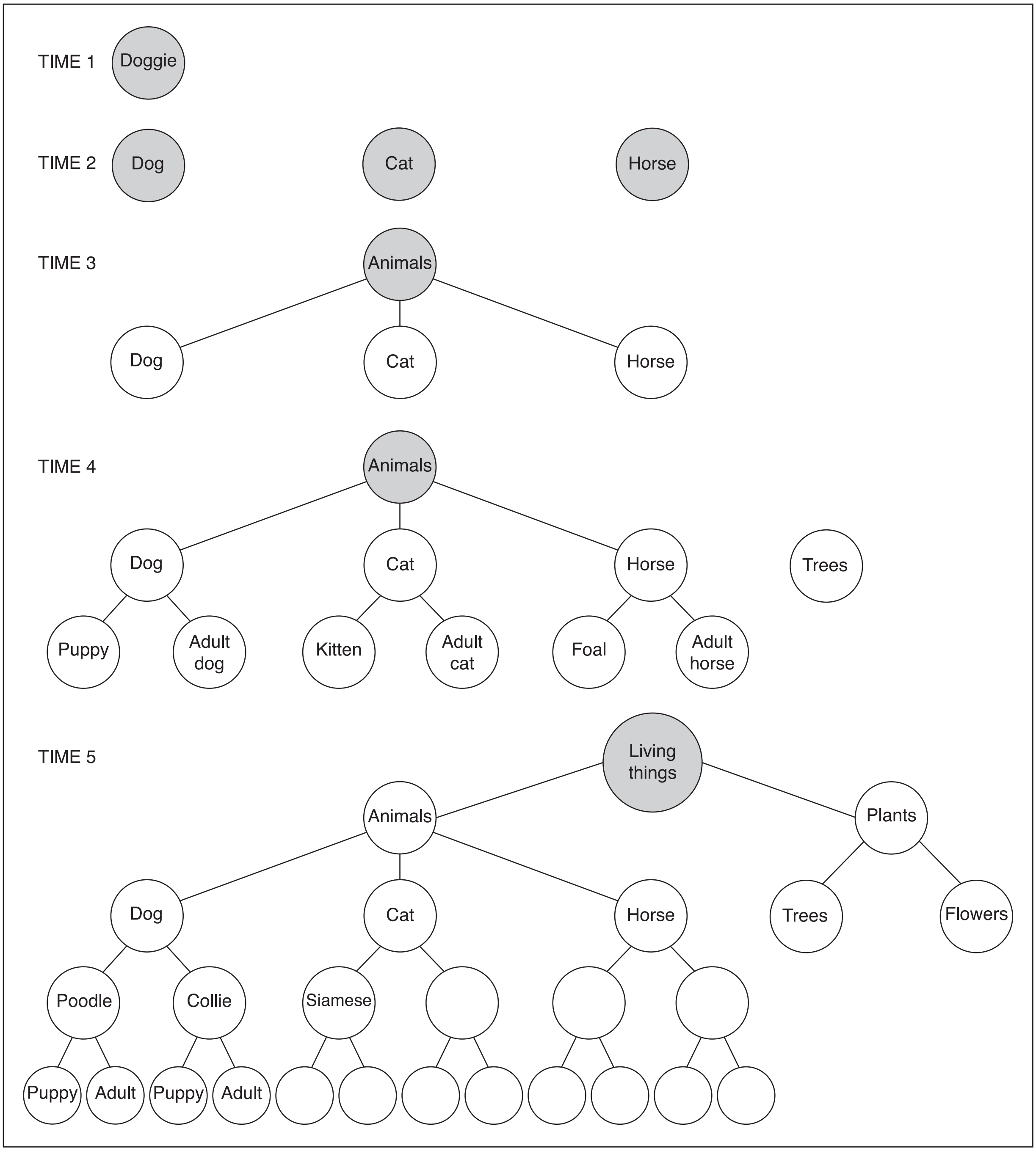


Figure 8.14 An illustration of the orthogenetic principle. The child's concepts of animals develop from a state of globality and lack of differentiation (Time 1) to a state of differentiation and hierarchical organization (Time 5).

characteristics to become different, specific characteristics. This differentiation is thus discontinuity. Second, there is the tendency to become hierarchically organized, that is, the tendency for earlier developments to be continuously subsumed under later ones. This hierarchical organization is thus continuity.

In short, what Langer (1969, 1970), Werner (1957), and Raeff (2011, 2016) suggest is that there are *both* continuous and discontinuous processes integrated into all aspects of development. To maintain an appropriate perspective about development, therefore, a developmental scientist must recognize that the organism develops in accord with both of these perhaps seemingly opposed processes. If a developmental scientist focuses exclusively, however, on one or the other of these two different processes, he or she will miss the nature of the synthesis that characterizes human development, and, accordingly, he or she will have an incomplete view.

Thus, if a developmental scientist focuses exclusively on discontinuity, he or she might incorrectly view development as quite a disorderly process. Alternatively, if the focus is exclusively on continuity, he or she will not understand the qualitative changes of the coacting, developing organism (Langer, 1970, p. 733). Langer, Raeff, and Werner opt for a view of development that recognizes the existence of both general (continuous) and specific (discontinuous) processes of development. This general-and-specific-processes position was outlined in Chapter 5; Piaget, too, opted for this position and, thus, took a theoretical stance quite similar to Werner's organismic-developmental position.

The orthogenetic principle highlights the fact that a developmental scientist must consider both the continuous and the discontinuous aspects of development. Both can be seen to characterize developmental changes, in that development proceeds from a state of globality and lack of differentiation to a state of differentiation (hence, discontinuity) and integrated, hierarchical organization (hence, continuity). In other words, development is actually a sort of *dialectical* process, a synthesis between thesis and antithesis (Hegel, 1830). Throughout the life span there is a dialectical integration—a synthesis—between discontinuous differentiation (thesis) and continuous hierarchic organization (antithesis).

Catherine Raeff

However, despite the apparent tenability of these assertions, arguments over whether continuity or discontinuity characterizes phylogenetic and/or ontogenetic development still occur. One such debate has centered on whether the processes governing the phylogenetic development of learning are continuous or discontinuous. It is useful to consider this controversy. First, the specifics of this debate serve to illustrate a particular and important instance of the continuity–discontinuity issue. Second, the information is essential for consideration of the continuity–discontinuity issue as it is applied to ontogenetic development.

THE PHYLOGENY OF LEARNING: CONTINUITY OR DISCONTINUITY?

Learning is a complex phenomenon. Although psychologists have spent a considerable amount of time and energy studying the learning process (e.g., see Fredricks, Alfeld, & Eccles, 2010; Hilgard, 1956; Kimble, 1961; Kuhn, 1995, 2009; Kuhn & Franklin, 2006; Lerner, 1995b), there is no consensus about the nature of learning. Different theorists define learning in different ways and advance different notions about what processes make up learning (see Kuhn, 1995, 2009; Kuhn & Franklin, 2006). For the purposes of the present discussion, I regard learning to be the acquisition of relations between environmental stimulation and behavioral responses, or simply, the acquisition of specific instances of stimulus–response relations. If an animal acquires a bar-pressing response in the presence of a red light or a response of turning to the right at various points in a maze in order to obtain food, it is possible to say that learning has occurred. Although this definition certainly does not address all the complexities involved in a consideration of learning (Kuhn, 1995, 2009), it is not my goal here to deal with all these issues. Rather, my focus is on a particular aspect of the controversies involved in the study of learning, that is, the issue of whether learning is a phylogenetically continuous or discontinuous phenomenon. Are the processes governing learning the same for all species? Or must new processes be posited to account for the learning of animals of different phylogenetic levels?

In the history of this controversy, M. E. Bitterman played a central, clarifying role. In three important papers (1960, 1965, 1975), Bitterman presented arguments and empirical evidence that served to clarify the continuity–discontinuity issue in learning. Bitterman (1960) noted that many psychologists interested in studying learning in different animals adopted a working assumption that learning processes are essentially the same in all animals. This assumption, he pointed out, had its basis in the ideas of no less eminent a figure than Charles Darwin. Darwin (1872) believed that differences among species in capacities such as learning were differences in amount (degree) and not in type (kind). Thus, relying perhaps on Darwin, many researchers assumed that the processes governing the learning of one phyletic level were qualitatively identical to those governing the learning of other phylogenetic levels.

This working hypothesis was extremely useful. Its adoption facilitated the experimental analysis of the learning process. Once continuity was assumed, researchers could study one species and then apply the resulting data to other species. Hence, because it was easier to manipulate the stimulus–response relationships of laboratory rats (e.g., as compared with children), rats virtually came to be the exclusive organism studied. Any processes found with rats could be *assumed* to apply to humans, because the only difference between these species was in quantity, not quality, of learning. The processes of rat learning could be used to understand how humans learn (e.g., Skinner, 1950, 1956, 1966). In other words, by focusing on how the rat learned, a researcher could readily discover the universal processes of learning, that is, the processes that applied to all organisms (Skinner, 1938).

As Bitterman (1960, p. 485) pointed out, however, many "learning psychologists" (those psychologists interested in the study of learning) soon lost sight of the fact that their working assumption was only just that—an assumption—and that it needed to be put to empirical test. Researchers needed to see if the processes of learning for one species were in fact applicable to all species. This assessment, of course, could not be done if learning psychologists continued to focus research on learning almost exclusively on the laboratory rat.

Unfortunately, many learning psychologists never did put this assumption to the test, and soon many transformed the working assumption into an article of faith, an untested belief (Bitterman, 1960). Accordingly, an early, renowned learning psychologist, John B. Watson, said that "in passing from the unicellular organisms to man no new principle is needed" (1914, p. 318). Similarly, later learning theorists, such as Dollard and Miller (1950), maintained that "any general phenomena of learning found in rats will also be found in people" (p. 63). Indeed, one of the most prominent psychologists ever identified with the psychology of learning, B. F. Skinner (e.g., 1938, 1971), espoused an identical position. He, too, turned the working assumption that began with Darwin into an article of faith.

In 1956, Skinner published an article that contained the graph seen in Figure 8.15, which shows what are termed *learning curves* (i.e., the cumulative records of changes in responses as a function of learning trials or time), obtained by Skinner from the responses of a pigeon, a rat, and a monkey. But which curve belongs to which one of these three quite different animals? Skinner's answer to this question was, "It doesn't matter" (1956, p. 230). As Bitterman (1960) pointed out, Skinner did not present these curves to show that the learning processes of these animals were identical; rather, he assumed this correspondence was the case. However, although the behavioral products of these animals—their learning curves—are markedly similar, this correspondence

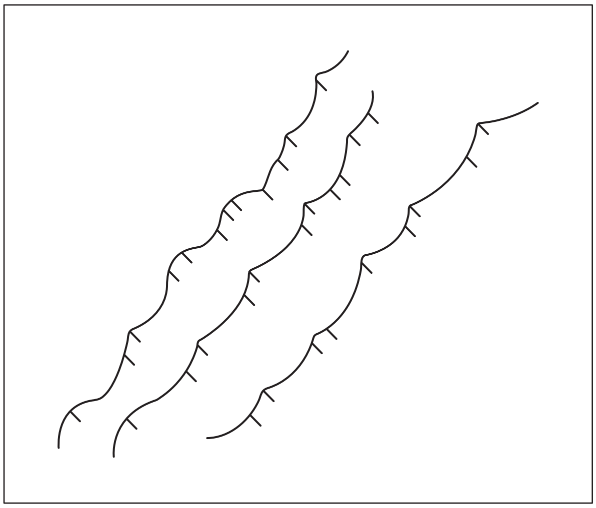


Figure 8.15 Learning curves for a pigeon, a rat, and a monkey.

Source: B. F. Skinner (1956). A case history in scientific method. *American Psychologist, 11*.

does not necessarily mean that it does not matter which curve belongs to which animal. By asserting this position on the basis of functional (response) similarity, Skinner was assuming that the processes, or structures, underlying these learning functions in diverse animals are identical.

Skinner asserted that it did not really matter what processes underlie an animal's behavioral capability so long as a researcher could demonstrate that the animal's behavior could be shaped in specific ways in specific situations. If a researcher could make an animal emit a specific response (e.g., learn to press a bar in a given pattern), and make another species of animal emit an identical response, then it was believed to be irrelevant if the processes by which these animals came to develop their response capabilities were different. As long as a researcher could control the stimulus–response relations of animals and thereby demonstrate that different organisms could be made to respond in identical ways in these situations, other differences among the animals were held to be irrelevant. They were regarded to be irrelevant because, in demonstrating that different animals could be made to do the same things, the researcher believed that he or she had demonstrated that these animals were essentially the same.

In Chapter 7, I noted that such an argument is inconsistent with Schneirla's (1957) probabilistic-epigenetic view of behavioral development and of the changing character of the relations between structure and function. This discussion points to some of the pitfalls of the position Skinner espoused. Just because there exist techniques with which to manipulate the behavior of two different animals so as to make them emit markedly similar responses in similar situations, it is not necessarily appropriate to conclude that the developmental processes governing the acquisition of their response capabilities are the same, or that the different animals will typically show identical responses in all other situations. Whereas a researcher can make an animal do something through the use of a specific experimental manipulation, this demonstration does not mean that this method reflects the way the animal comes to behave in its natural environment (cf. McCall, 1981).

Thus, to summarize the essential difference between Skinner's continuity position and Schneirla's probabilistic-epigenetic position, I may offer an anecdote told to me by my Ph.D. dissertation mentor, Sam J. Korn (personal communication, 1967), about one of Schneirla's most eminent students, Daniel Lehrman. Once, at a symposium held at the New York Academy of Sciences, Lehrman was called on to summarize the essential differences between the positions of Skinner and Schneirla, who had just presented lectures to the assembled group. He did so in one sentence: "Professor Skinner is interested in finding out how animals come to do what he wants them to do, whereas Professor Schneirla is interested in finding out how animals come to do what *they* want to do!"

In addition, according to Bitterman (1960), Skinner's reasoning is unwarranted. First, demonstrating that different animals can be made to do the same things does not necessarily prove that they learn in the same way. Again, the assumption that even identical behaviors are underlain by identical processes is not logically necessary. Second, as previously implied, demonstrating that animals can be made to acquire *specific* stimulus–response relations in specific situations does not prove that they acquire *all* their stimulus–response relations in *all* of their life situations in that same way. Third, demonstrating that *some* animals can be made to perform the same way in a specific situation does not prove that *all* animals can be made to perform identically. For example, rats, pigeons, and some apes can be made to perform identically in some situations, but what about fish, elephants, pigs, 3-year-old humans, and 70-year-old humans? In fact, when some researchers *have* compared such other animals (e.g., pigs, raccoons, and chickens) on similar learning tasks, they have found that similar behaviors cannot necessarily be made to take place (Breland & Breland, 1961).

A simple demonstration of similar learning curves among different species does not demonstrate any universal processes of learning. The applicability of any process of learning to all species needs empirical verification, and Bitterman (1960) emphasized that such testing had by no means been provided by Skinner or any other learning psychologist. Thus, the assumption of phylogenetic continuity remained just an assumption.

Bitterman did not merely point to the need for testing this assumption. He also began a series of important experiments designed to determine whether the processes of learning were continuous across the phylogenetic scale. Accordingly, he chose as research participants species of animals other than laboratory rats. In a paper published in 1965, Bitterman reported on some of the results of his studies, as well as studies by other researchers.

In some of the studies, the learning capabilities of a specific species of fish were compared with rat learning. When Bitterman compared the learning of these two types of animals in four different learning situations, he found that the processes governing the learning of this type of fish appeared to be different from the processes governing rat learning. For example, Figure 8.16 (adapted from Bitterman, 1965) shows that the performance of rats on one of these four learning tasks clearly improved across trials. As the rats were trained, they made fewer and fewer errors. On the other hand, the performance of the fish clearly did not improve. In fact, the curve presented in the figure indicates that their performance seemed to get worse. The more practice they had on the task, the more errors they seemed to make. Thus, for a specific type of learning task, one species improved with practice and the other species seemed to get worse. Clearly, the processes

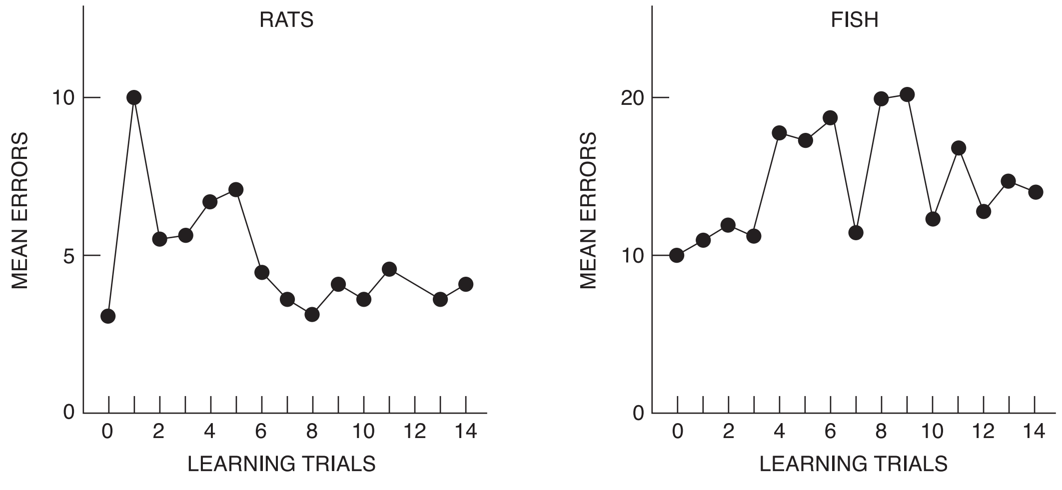


Figure 8.16 Performance of (a) rats and (b) fish on a specific type of learning task.

Source: Adapted from M. E. Bitterman (1965). Phyletic differences in learning. *American Psychologist, 20*. Copyright © 1965 by the American Psychological Association. Reprinted with permission.

Table 8.1 Behavior of a variety of animals in four types of learning problems expressed in terms of whether their learning was similar to that of the rat or of the fish

	Learning problem			
Animal	1	2	3	4
Monkey	Rat	Rat	Rat	Rat
Rat	Rat	Rat	Rat	Rat
Pigeon	Rat	Rat	Fish	Fish
Turtle	Fish	Fish	Fish	Fish
Fish	Fish	Fish	Fish	Fish
Cockroach	Fish	Fish	—	—
Earthworm	Fish	—	—	—

Note. Adapted from M. E. Bitterman (1965). Phyletic differences in learning. *American Psychologist*, 20, 396–410.Copyright © 1965 by the American Psychological Association. Adapted with permission.

governing the learning for these two species are not the same.

In addition, Bitterman compared the performance of the rats and the fish on the four types of learning tasks with the performances of other species on these four types of tasks. Not only did he again find evidence for phyletic discontinuity in the processes of learning, but he also found that, on some tasks, some species learned like rats, and on other tasks these same species learned in a manner similar to fish. Thus, whereas some species (e.g., monkeys) always learned in the way that the rats learned and others seemed to learn in the way fish learned, some animals learned some problems the way rats did and other problems the way fish did. These findings by Bitterman are summarized in Table 8.1, which is adapted from his 1965 article.

In summary, Bitterman argued against the seemingly well-ingrained assumption that there were universal processes of learning and only these universal processes, and thus that the processes of learning were necessarily continuous along the phylogenetic scale. He believed that the processes of learning of one species could not necessarily be assumed to apply to all species. His assessments of different species of animals on different types of learning tasks afford the simple conclusion that the same processes of learning do not seem to apply to all species. Thus, the importance of Bitterman's work is to demonstrate the necessity of testing crucial developmental issues, and not simply assuming that a specific position on the issue is correct.

Moreover, Bitterman provided developmental science with evidence against the notion that all phylogenetic levels are the same in regard to psychological or behavioral functioning. Bitterman indicated that one common set of processes may not suffice to account for all the behavior of all species, and that instead there were qualitative differences among species. That is, there were differences in kind as well as in degree. A key implication of Bitterman's work was that discontinuity as well as continuity may characterize the phylogeny of learning.

If there are differences among animals in the processes governing learning, may it also be possible that there are differences *within* a given species? That is, may ontogenetic development also be characterized by discontinuity in the processes governing learning?

ONTOGENETIC IMPLICATIONS OF THE CONTINUITY–DISCONTINUITY ISSUE

In 1980, Orville G. Brim, Jr., and Jerome Kagan edited a book (*Constancy and Change in Human Development*) that reviewed evidence from several disciplines about whether early experience provided a virtually immutable shaper of the entire life course—in other words, about whether events in early life necessarily constrained developments later on. With the publication of the Brim and Kagan

(1980a) volume, the issue of continuity–discontinuity across life came to the forefront of concern in the study of human development.

The authors brought together in the Brim and Kagan volume reviewed studies that indicated that features of the person's historical setting often shaped personality, social, and intellectual functioning to a much greater extent than maturational- or age-associated changes (Elder, 1974; Nesselroade & Baltes, 1974; Schaie, 1979; see also Elder & Shanahan, 2006, and Elder et al., 2015). General historical events such as wars, economic privations, or political upheavals, as well as personal events such as marriage, divorce, illness, death, or career change, were often seen to provide potent shapers of the quantity of life changes and of the quality of the life course (e.g., Elder, 1974, 1979, 1980; Elder et al., 2015; Sampson, 2016). These studies also indicated that there were multiple paths through life. As people age they become increasingly different from each other, and these different life paths are again linked to general historical or personal events (Baltes, 1987, 1997; Baltes et al., 1980, 1999, 2006; Brim & Ryff, 1980).

On the basis of such findings, Brim and Kagan (1980b) concluded that the potential for change exists across life; that as a consequence of active people reciprocally coacting in a changing world, the life course was always characterized by the potential for *plasticity*—that is, systematic changes within the person in his or her structure and/or function. While not denying that constancies and continuities could and did characterize much of many people's life courses, and that plasticity is therefore not limitless, Brim and Kagan (1980b) suggested that change and the potential for change characterized life because of the plasticity of the processes involved in people's lives.

These conclusions were controversial. One key reason for the controversy was a consequence of the fact that many of the scientific disciplines devoted to the study of human behavior, its evolution, and its development across the life span have historically been influenced by a "presupposition of limits" (Gould, 1981).

The Presupposition of Limits and the Presupposition of Plasticity

A presupposition is a culturally deep-rooted, pre-empirical idea about the nature of reality. It is an idea held even before data are collected pertinent to the idea. Presuppositions function in a manner comparable to the ideas I discussed earlier in regard to theories; that is, a presupposition, like a theory, may lead to differential treatment of data, and such treatment may result in the "discovery" in the data of evidence for the truth of the presupposition.

The term *presupposition of limits* is meant to summarize a general position or class of arguments in philosophy, one that has many instances (see Toulmin, 1981). This presupposition involves the view that human functioning is unalterably constrained by one factor or by a circumscribed set of factors (e.g., genes and early experience); that is, the view is that there is a necessary "connection" (Kagan, 1980, 1983) between what is given by these causal variables and a consequent form or function, and that this connection is unavailable for manipulation or alteration (Lehrman, 1970). In other words, this view implies that there is one (or a limited few) developmental pathway(s), and that an individual's trajectory along a path is determined by causal factors that permit no deviation.

Although most current conceptions of development do not manifest this presupposition in terms as strong as those I have outlined, there are, nevertheless, several influential theoretical statements consistent with the presupposition of limits that have appeared within the twenty-first century (e.g., Belsky, 2014; Bjorklund, 2015; Bjorklund & Ellis, 2005), as well as in the years in the twentieth century surrounding Kagan's (1980, 1983) writing about this issue (e.g., Eysenck & Kamin, 1981a, 1981b; Herrnstein & Murray, 1994; Lorenz, 1965; Rowe, 1994). In Chapters 11 and 12, I discuss these statements in greater detail. However, it is useful here to discuss an influential set of ideas framed by the presupposition of limits that was presented around the time of Kagan's (1980, 1983) work.

About 40 years ago, at this writing, Klaus and Kennell (1976) introduced a notion of maternal–infant bonding that stressed that the quality of the bond established between the mother and the infant in the first moments after the infant's birth imposed a potent constraint on the rest of the newborn's social and affective development. Klaus and Kennell (1976) indicated: "We strongly believe that an essential principle of attachment is that there is a *sensitive period* in the first minutes and hours after an infant's birth which is optimal for the parent–infant attachment" (pp. 65–66). Klaus and Kennell (1976) explained that one of their principles of attachment was that "early events have long-lasting effects. Anxieties a mother has about her baby in the first few days after birth, even about a problem that is easily resolved, may affect her relationship with the child long afterward" (p. 52). Klaus and Kennell (1976) concluded:

> This original mother–infant bond is the wellspring for all the infant's subsequent attachments and is the formative relationship in the course of which the child develops a sense of himself. Throughout his lifetime the strength and character of this attachment will influence the quality of all future bonds to other individuals.
>
> (pp. 1–2)

A theoretical position consistent with the implicit stance taken by Klaus and Kennell (1976) in regard to the presupposition of limits was presented by Fraiberg (1977). She contended that, throughout the life span, every instance of and/or type of expression of the emotion of love was necessarily connected to a bond that originated in the first year of life. Fraiberg (1977) argued that:

> Love of a partner and sensual pleasure experienced with that partner begin in infancy, and progress to a culminating experience, "falling in love," the finding of a permanent partner, the achievement of sexual fulfillment. In every act of love in mature life, there is a prologue which originated in the first year of life.
>
> (pp. 31–32)

In addition to these formulations, Bruer (1999) reviewed other arguments that perpetuated what he termed the *myth of the first three years*, that is, the belief that brain development in the first three years of life determines, in an all-or-none, once-in-a-lifetime manner, the success of a child's development across the entire life span. Bruer (1999) noted that, as a consequence of this myth, parents might have believed that by the time their children entered kindergarten all the brain developments crucial to their success in life had already transpired. Although, as discussed in Chapter 7, such critical period ideas have no convincing empirical support, Bruer (1999) added to the evidence base refuting the critical period idea by providing considerable neurobiological and behavioral evidence indicating that the brain remains an instrument for learning and development across life; indeed, there are data pertinent to very old age (e.g., from the Berlin Study of Aging; Baltes et al., 1999, 2006) that indicate that cognitive development and learning can occur in the ninth and tenth decades of life. On the basis of such data, Nelson (1999) concluded that:

> First, given the protracted nature of synapse formation, given that the cultivation of some synaptic circuits depends heavily on experience, and given the multitude of experiences a child has in his or her lifetime, we should be telling parents that no single experience, good or bad, will likely have much influence on their child's development (although there will be caveats to this suggestion, e.g., whether the child has been spared perinatal or genetic injury). Similarly, given the long evolutionary history our species enjoys, many of the so-called enriched experiences some parents seem so intent on providing their children with will likely not matter later in life. Thus, whether the child has the "right" mobile positioned above the crib or the right music or foreign language tape playing in the background will likely prove inconsequential in the long run; that is, the child might have a facility for languages or music (although even this is uncertain), but these experiences will not impact development broadly defined. This, in turn, should take the pressure off parents to be perfect. Third, our species would not have survived as long as it has if all of our development depended heavily on specific experiences occurring at precise points in time. Moreover, even those systems whose development is tied to sensitive or critical periods (e.g., our sensory system) provide for some

> flexibility both in the quality and the timing of certain experiences. Thus, so long as our visual system receives general patterned information, we will develop pattern vision. The lesson here, of course, is that we, as parents, teachers, and role models, can make a few mistakes. Lastly, as dramatic as brain development is in the first few years, we should think of these years as analogous to building a foundation for a house. However, unless construction continues, the house will be incomplete, and its owners may never be satisfied with the final product—nor may its neighbors. Based on my perspective as a neuroscientist and developmental psychologist, I argue that our responsibility to our children must be distributed throughout the course of their lives, not focused on just the first 3 years. To do otherwise would be not only short-sighted, it would not be good science.
>
> (p. 237)

Despite the good sense and the good science that are reflected in the views of Nelson (1999) and Bruer (1999), the fact that they had to make their arguments two decades after Brim and Kagan (1980a) had also presented evidence supporting just such ideas about plasticity across the life span suggests that notions about the lack of plasticity in human development—or, in other words, notions predicated on the presupposition of limits—are difficult to eliminate, even when they are shown to be counterfactual. Indeed, across the history of the study of human development there have been formulations that were fairly explicitly associated with the presupposition of limits.

For example, the nineteenth-century craniology of Broca (1861, 1862a, 1862b, 1862c) involved the assumption that the size of the human skull was the factor limiting an individual's or a social group's intellectual capacity (Gould, 1981). Similarly, genetic deterministic theories of both the nineteenth and twentieth centuries assumed that one's biology—received at conception and represented by the genotype—constrained one's moral (Lorenz, 1940a), cognitive-intellectual (Goddard, 1912, 1914; Herrnstein & Murray, 1994), or vocational (Terman, 1916) developments (cf. Gould, 1981). In short, the presupposition of limits is a pre-empirical—and in my view unduly pessimistic (Lerner, 1984)—belief in the irremediable character of human nature. It holds that for better or for worse, humans are a direct, unalterable product of their evolution, biology, genes, and early experiences. Such a view holds that there can be no intervention to prevent, ameliorate, or enhance this "natural order." Simply, this "biology is destiny" argument leaves little room for ontogenetic adaptation.

However, a presupposition of limits may be contrasted with one of plasticity. Such a presupposition involves the belief that there may be systematic and relatively enduring changes within a person, changes in his or her physical, psychological, and social structures and functions. This potential for change is thought to exist because of the constant irrevocable relation that exists between a person and his/her world. In other words, the presupposition of plasticity rests on the idea that the person always exists in a world that he or she both influences and is influenced by (Lerner, 1982; Lerner & Busch-Rossnagel, 1981b; Lerner & Walls, 1999). Consistent with Schneirla's (1957) conception of circular functions in ontogeny, changes in individuals influence changes in their physical and social worlds—worlds that, as they are thus altered, promote further changes in people. Because of the reciprocal relations between people and their worlds, because of individual↔context relations, developmental scientists may be optimistic that there now are (or may eventually be) ways to better the human condition. Developmental scientists may, therefore, maintain the hope that experiences at one time in life need not constrain possibilities later or that at least some early problems, deficits, or insults to the integrity of the organism may be ameliorated (Lerner, 1984; Lerner et al., 2014; Sigman, 1982).

To contrast the implications of the presupposition of limits versus the presupposition of plasticity in regard to optimism versus pessimism about changing human functioning, I may note that beliefs in (or the presence of) fixity in human functioning and development suggest that humans are resistant to change, that they are static, immutable organisms (Dweck, 2006, 2016). Beliefs in (or evidence for) plasticity suggest by definition that there is some potential for within-person (intraindividual) change, and these beliefs promote a scientific emphasis on studying processes fostering or constraining change. In addition, the existence of plasticity in the functioning and development of humans permits an optimistic orientation to intervention (e.g., Lerner, 2012, 2015d; Lerner et al., 2015). In addition to preventative strategies, techniques aimed at ameliorating, or even enhancing, the human condition may appropriately be instituted (Clarke & Clarke, 1976; Lerner, 1995a, 2012). Without plasticity, humans who possess undesired or undesirable characteristics would be, simply, without remediation (Hunt, 1961; Lerner, 1984; Lerner et al., 2014).

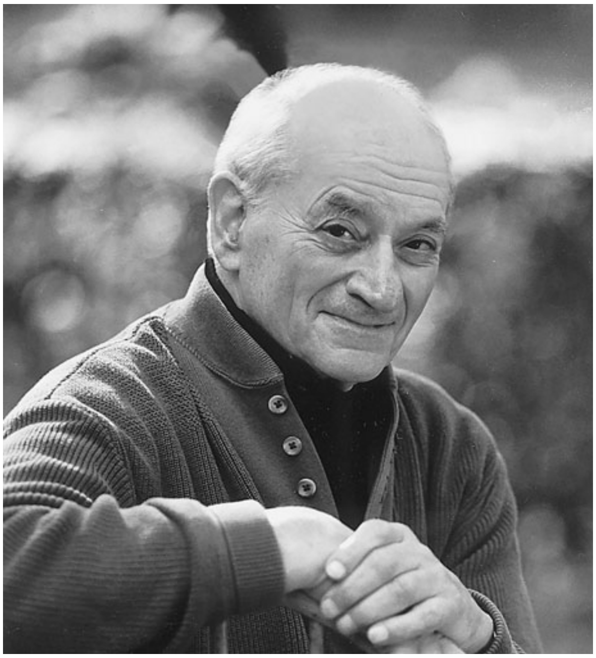
What could be done with such people? If there is a belief that personal and social behaviors and health are fixed by genes or, in turn, by experiences in very early life, experiences that are presumed to have unmodifiable connections to functioning in later life (Kagan, 1980, 1983), then rather severe treatment policies can be instituted (Lerner, Wiatrowski et al., 2011). Brim and Kagan (1980b) depicted such perspectives by noting:

> The belief that early experiences create lasting characteristics, like the belief in biological and genetic determinism, makes it possible to assume that attempts to improve the course of human development after early childhood are wasted and without consequence. If society believes that it is all over by the third year of life, it can deal harshly with many people in later life because nothing more can be done, and social programs designed to educate, redirect, reverse, or eliminate unwanted human characteristics cannot be justified. Policies of racial, ethnic, and sex discrimination, incarceration rather than rehabilitation of criminals, ignoring urban and rural poverty, and isolation of the elderly have found shelter in the belief in the determinism of the early years of life.
>
> (p. 21)

The presence of plasticity holds the promise of potentially enhancing human life and the presence of fixity or immutability does not. The empirical existence of plasticity is, therefore, not a point of minor practical significance. If all levels of life are available to be changed, then there is great reason to be optimistic about the ability of intervention programs to enhance human development. However, as Brim and Kagan (1980b) indicated, optimism about plasticity must be tempered in light of the need to understand the presence of both continuity and discontinuity—of both constancy and change—in ontogeny. Werner's (1957) ideas again help us to understand this point.

Plasticity and Probabilistic Epigenesis

If discontinuity in development exists along with continuity, then any plasticity that arises as a consequence of discontinuity must be understood as a *relativistic* phenomenon. As a consequence of this relativity, the issue for the study of human development is to learn the individual and contextual conditions that promote and/or constrain systematic change in structure and/or function. A similar call for the need to understand how the processes that promote plasticity also promote constraints on change was made by Gollin (1981), who also adopted a relativistic view of the bases of an organism's plasticity


Jack Block

across life, one based on the probabilistic-epigenetic character of development.

That is, an emphasis on probabilistic-epigenetic development indicates that the processes that give humans their individuality and their plasticity are the same ones that provide for human commonality and constancies (cf. McClearn, 1981). Indeed, Jack Block (1982) made this point eloquently, cautioning that, when using the term plasticity, one must not also imply that, within the malleable system, there is not a structure or structures. He explained that if individuals are self-initiating, self-organizing systems, responsive in dynamic ways to changing contexts, these characteristics exist because individuals have within them various ego structures, cognitive structures, perceptual structures, and action or knowledge structures through which experience is apprehended, processed, and behavior is forged.

In essence, processes of development are plastic in that they continually involve probabilistic-epigenetic transactions between organism and context. The outcomes (ontogenetic products) of these individual↔context relations are internal and behavioral structures creating a person with the ability to change self and/or context to meet the demands of life, the ability to attain a good fit or match with the context.

However, as I have noted, plasticity is not limitless. Human behavior is always influenced by past events, by current conditions, and by the specific features of the individual's organismic constitution. As noted in Chapter 4, a notion of complete or limitless plasticity is antithetical to any useful concept of development (Baltes, Dittmann-Kohli, & Dixon, 1984; Kaplan, 1983; Lerner & Busch-Rossnagel, 1981b; Lerner & Walls, 1999; Sroufe, 1979; Sroufe & Waters, 1977), and is, therefore, unwarranted on philosophical, theoretical, and methodological—as well as on empirical—grounds (e.g., see Block, 1982).

On the other hand, any view that stresses complete constraints, necessary connectivity across life periods, or irremediable limits placed on later behavioral organization by antecedent experiences is similarly unwarranted. Such a view would ignore the demonstrations that at least some behavioral flexibility can be shown across all of life (Baltes, 1987, 1997; Baltes & Baltes, 1980; Baltes et al., 1999, 2006; Baltes & Willis, 1982; Bateson, 2015, 2016; Brim & Kagan, 1980b; Bruer, 1999; Greenough & Green, 1981; Lerner, 1984, 2006b; Nelson, 1999; Willis, 1982; Willis & Baltes, 1980) and that there is evidence for the plasticity of the processes producing such capability.

The point I want to emphasize here is that the intellectual agenda promoted by an analysis of the plasticity concept, at least insofar as one follows the probabilistic-epigenetic model, is not one of determining whether constancy or change, or whether stereotypy or plasticity, characterize development. *Both do.*

It is useful to reiterate that a key feature of Werner's (1948, 1957) orthogenetic principle is that a developmental change is *defined* as one wherein processes promoting discontinuity (i.e., those promoting differentiation) are synthesized with those promoting continuity (i.e., those promoting hierarchic integration). From this orthogenetic perspective, developmental change is not only lawful and a synthesis of constancy and change, but developmental change is also thereby consistent with the features of ontogeny that are highlighted by a probabilistic-epigenetic conception of development. Thus, the task for developmental analysis is one of determining the individual and contextual conditions under which one will see constancy or change (cf. Block, 1982; Lerner, 1979).

For instance, what developmental processes lead to a child developing a specific level of "ego resiliency" (Block & Block, 1980), and what conditions constrain the development of such a level of flexibility? In order to address such questions, developmental scientists must be concerned with the life-span character of the relation between constancy and change, of plasticity, and of constraints on plasticity. Some of these features of plasticity are discussed in the next section.

Parameters of Plasticity

An organism's plasticity does not remain at a constant level across its life span. There are several lines of work pertinent to this point. MacDonald (1985), in an essay integrating the concept of sensitive period (discussed in Chapter 7) with the literature pertinent to early experience effects, noted that plasticity is a ubiquitous but declining phenomenon across the life span (see too Baltes et al., 2006; Lerner, 1984). He argued that:

> We have come a long way from supposing that behavior is absolutely fixed at an early age by genetic factors or that after a sensitive period it is impossible to change behavior. Nevertheless, there are too many data showing otherwise to reject the idea that there are important constraints on plasticity for human or animal behavior. This fact does not, of course, prevent us from finding ways to intervene with individuals who have suffered early environmental insults. Indeed, the theory of sensitive periods suggests that the intensity of an ecologically appropriate stimulus can, at least up to a point, overcome the organism's declining plasticity . . . The fact of declining plasticity merely indicates what we already know, that successful interventions are not at present easily come by.
>
> (p. 116)

Of course, the fact that one does not see a change in behavior cannot be taken as proof of the absence of plasticity (MacDonald, 1985). Constancy in the individual can result from consistency in the demands and/or constraints of the environment within which the individual is functioning and to which the individual must adapt (cf. Wohlwill, 1980). In addition, and especially among humans, the developing individual's progressive ability to be competent in self-regulation means that the individual becomes better able to self-select and shape the context within which he or she coacts, and thereby can produce, maintain, or alter the continuity of his or her behavioral repertoire (Baltes et al., 2006; Brandtstädter, 1999, 2006; Brandtstädter & Lerner, 1999; Freund, Li, & Baltes, 1999; Gestsdóttir et al., 2009, 2010; Gestsdóttir & Lerner, 2008; Heckhausen, 1999; Lerner, 1982; Lerner & Busch-Rossnagel, 1981b). Given that the contextual pressures could be changing while such individual production processes are occurring, the maintenance of individual constancy would be evidence of both considerable plasticity and the use of a discontinuous set of strategies for behavioral management on the part of the individual.

Cairns and Hood (1983) discussed five factors that may give rise to individual continuity in development. They noted that, first, individually specific biological variables may contribute to continuity in an individual's behavior. Such variables included genetic processes that might endure over several developmental periods, hormonal processes, and morphology (Cairns & Hood, 1983). However, Cairns and Hood (1983) cautioned that:

> *Biological factors are rarely translated directly into differences in social interaction patterns.* The linkages between psychobiological processes and social behavior patterns *need to be examined at each of the several points in ontogeny.* It cannot be safely assumed that biological or genetic-based differences will persist, unmodified by social encounters or interchanges in which the individual engages.
>
> (p. 309, italics added)

The second factor that Cairns and Hood (1983) identified as potentially contributing to the continuity of behavior included the social network in which development occurs. They believed that, if all other factors were equal, similarities in behavior from one time to the next would be greatest when the social network in which development occurred remained constant. The third factor Cairns and Hood (1983) identified was behavioral consolidation. Here, based on social learning of interactional learning experiences, diverse behaviors became part of an integrated behavioral repertoire or sequence. For example, one may learn how to "put together" efficiently all the diverse behaviors involved in hosting a dinner party.

The fourth and fifth factors noted by Cairns and Hood (1983) were social evocation and mutual control; these factors allow individuals to contribute to the continuity of their own behavior by virtue of their being involved in a circular function (Schneirla, 1957). That is, by virtue of their individual physical and behavioral characteristics, people evoke different reactions in others, reactions that involve (a) classification of the person–stimulus into categories (e.g., attractive, overweight, male, and black); and

(b) category-specific feedback to the person (e.g., Kendall, Lerner, & Craighead, 1984). Cairns and Hood (1983) noted: "To the extent that some stimulus properties of the individual remain relatively constant over time, the social actions contingent upon the actions of others may themselves remain relatively similar" (p. 310).

Finally, Cairns and Hood (1983) noted that individuals may actively promote their own continuity. Especially as self-regulatory competency increases, individuals show choices and preferences, take actions that preserve their social network and their social relations, and maintain their environmental setting (cf. Brandtstädter, 1998, 1999, 2006; Freund et al., 1999; Heckhausen, 1999; Kendall, Lerner, & Craighead, 1984; Mischel, 1977; Snyder, 1981).

The point involved in the Cairns and Hood (1983) presentation is that there are several processes that may maintain constancy in an individual's behavior, and that none of these processes pertains to the lasting or constraining effects of early experience or speaks directly to the level of plasticity prototypic of individuals across their development. The level of plasticity of the individual↔context relations that exists across the life span serves, then, as the moderator of whether, and under what individual and contextual conditions, continuity or discontinuity characterizes the features of a person's development. Indeed, because of plasticity and the role of the individual as an active agent in constructing his or her own developmental trajectory, the empirical presence of continuity may not serve in and of itself as evidence against (or for) constancy or change in features of development (Cairns & Hood, 1983; Wohlwill, 1980).

For instance, Baltes (1987; Baltes et al., 2006) and Freund et al. (1999) explain that changes in "molecular" intentional self-regulatory processes may compensate for functional losses in aging and may maintain more "molar" behaviors. For instance, an aging professional pianist can still have well-received performances despite slower psycho-motor speed if he or she compensates by playing pieces that have slower tempos, or by playing pieces that can be played at slower tempos.

Conclusions about Plasticity in Development

Plasticity not only represents a ubiquitous but declining phenomenon across life but also, because an instance of plasticity may involve the individual actively and creatively maintaining a context within which it can continue to function consistently, the presence of plasticity may be difficult to verify. Indeed, in this view, the presence of constancy may be an index of plasticity. Thus, the outcomes of the effects of plasticity may be difficult to disentangle from other phenomena leading to constancy or change, to continuity or discontinuity, in development.

When the continuity–discontinuity issue is raised in regard to development across the human life span, it raises a concern with descriptions, explanations, and the identification of quantitative versus qualitative constancy or change. In addition, issues of plasticity, of constraints on development, and of the nature–nurture controversy are raised. However, just as the continuity–discontinuity issue is related to the nature–nurture issue, another key issue of development is closely linked to that of continuity–discontinuity: the issue of stability–instability. I consider this issue next.

THE STABILITY–INSTABILITY ISSUE

The study of continuity and discontinuity in an individual's development is really an appraisal of how descriptions and/or explanations of change may apply across ontogeny. Such appraisals necessarily involve consideration of what happens to a person as a function of the variables affecting his or her development. In other words, consideration of the continuity–discontinuity issue is, in effect, an assessment of how the character of the variables influencing development results in quantitative and/or qualitative differences within a person over the course of his or her life. Simply, the continuity–discontinuity issue is one of intraindividual (within-person) change.

However, not all people undergo intraindividual change in precisely the same way. There are differences *between* people in how they change intraindividually. Thus, in addition to asking questions about within-person change, a developmental scientist may also ask what happens to a person relative to other people, as the relations among the variables that affect development change or remain the same.

People may obviously be placed in reference groups such as sex, age, race, ethnicity, or religion (see Chapter 5, and the discussion of the differential approach). What happens to the person's position in a reference group as the variables affecting the person function?

For example, the most common reference group in developmental science is an age group. Suppose that a developmental scientist measured the IQ of every member of a 5-year-old age group. The developmental scientist would expect that different people would get different IQ scores. In fact, he or she could rank every member of the age group from high to low, and any given person would, therefore, have a position in the age group. What happens to this person when the variables that affect behavior function? The person's position could change, or it could remain the same, relative to the other people in the age group.

Thus, whenever the continuity–discontinuity issue is considered, a second, subsidiary issue is also raised—that of *stability–instability.* The stability–instability issue describes differences that arise between people within groups as a consequence of within-person change. Thus, two types of alterations involving people are occurring simultaneously. People may be changing, and because not all people change in the same way or at the same rate, people's locations relative to others may also alter. In order to understand all dimensions of a person's changes, both aspects of change should be considered simultaneously. Only through such a joint, simultaneous focus can development across the life span best be portrayed.

If a person's position relative to his or her reference group changes with development, then *instability* is present. Alternatively, if a person's position relative to his or her reference group remains the same with development, then *stability* is present. These terms describe a person's ranking relative to some reference group. These relations are illustrated in Figure 8.17.

In this figure in both examples the IQ of the person in question (the target person) *increased* from Time 1 to Time 2 in development. This point is quite important to recognize. Whether stability or instability occurs says nothing whatsoever about

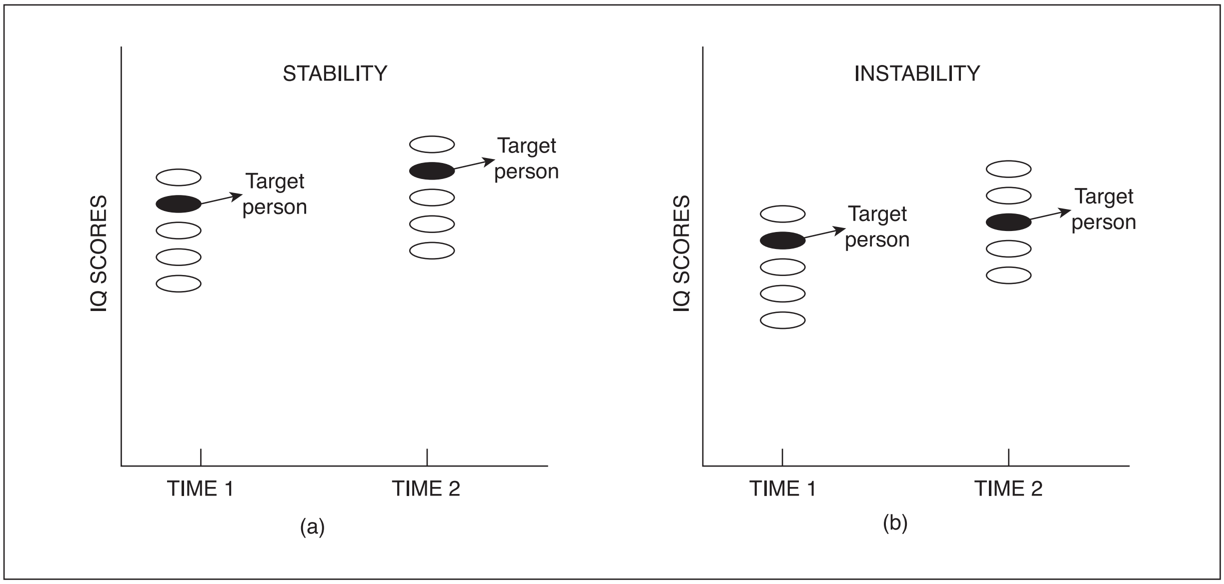


Figure 8.17 Examples of (a) stability and (b) instability in development.

whether any *absolute* change took place. A person can change, and this change may still be labeled stability. This instance of stability could occur if others in the reference group also changed and if the target person remained in the same relative position. On the other hand, a person could remain the same from Time 1 to Time 2 and yet his or her position relative to the reference group could be termed instable. This outcome could occur if others in the group changed whereas the target person did not. Hence, the terms *stability* and *instability* describe *relative*, not absolute, changes.

To illustrate, the concept of IQ, or intelligence quotient, is relative: it expresses a measure of a person's intelligence relative to his or her age group. For example, one way of expressing IQ is through use of the intelligence-quotient formula (i.e., IQ = MA/CA x 100, where MA = mental age, CA = chronological age, and 100 is used to avoid fractions). Thus, if a person is as bright as a 5-year-old (MA = 5 years), and is also 5 years of age (CA = 5 years), his or her IQ will equal 100. Similarly, if a person is 8 years old and he or she is as bright as an 8-year-old, his or her IQ will also equal 100. Thus, IQ is a relative concept because it expresses an individual's measured intelligence relative to his or her age (reference) group.

Thus, if a 5-year-old has an IQ of 120 and an 8-year-old has an IQ of 100, the 5-year-old is brighter than the 8-year-old because the 5-year-old knows more relative to his or her age group than the 8-year-old knows relative to his or her age group. Certainly, if researchers could construct some imaginary scale of absolute knowledge, the 8-year-old would probably have more absolute knowledge than the 5-year-old. Yet, the 5-year-old is regarded as brighter because IQ is a relative concept, and the younger child has a higher ranking in the 5-year-old reference group than does the older child in his or her reference group.

A person's absolute knowledge may change, but if the person's age group keeps pace, then his or her IQ would be stable. Conversely, even if a person's absolute knowledge remains the same from Time 1 to Time 2, his or her IQ could (a) remain the same if the age group did not change; (b) be instable and decrease if the age group increased in its level of absolute knowledge; or (c) be instable and increase if the age group decreased in its level of absolute knowledge.

As another example, consider the distribution of scores that would be obtained if people were measured on the characteristic of "height at puberty." Not all people would be the same height at puberty. Some would be shorter, some taller, and some of average height. People with different scores (in this case, heights) would have different positions (or locations) in the group. When the group is tested a second time (e.g., height measured at the end of the final growth spurt in adolescence), heights may have changed for most, if not all, people. However, each person's relative position in the group could have stayed the same. If Persons A, B, and C each grew four inches, and all the other people in their group did as well, then, despite the absolute increase in height, their relative positions in the group would have stayed the same. Despite intraindividual change, there were no interindividual differences in such change. This illustration is an example of stability. However, if a person's rate of change relative to the others in the group changes over time, if Person C grew eight inches in height whereas everyone else grew only four inches, then Person C would have changed more than those in his or her group, and instability (for this person) would have occurred. As with the IQ illustrations, notice again that, in the present illustrations of both stability and instability for height, the score of the person in question increased between Time 1 and Time 2.

In short, then, the terms *stability* and *instability* describe *relative*, not absolute, changes. Again, the terms relate to whether differences present among people in a group at Time 1 persisted at Time 2 (and, hence, stability occurred) or were altered, with the group distributed differently the second time (and, hence, instability occurred).

Therefore, developmental stability and/or instability can be obtained in several ways. Stability between two times in a person's development can occur when (a) the person remains the same and so does the reference group or (b) the person changes and so does the reference group to corresponding extents. On the other hand, instability between two times in a person's development can occur when (a) the person remains the same but the reference group changes or (b) the person changes but so do members of the reference group to extents not corresponding with the person's degree of change.

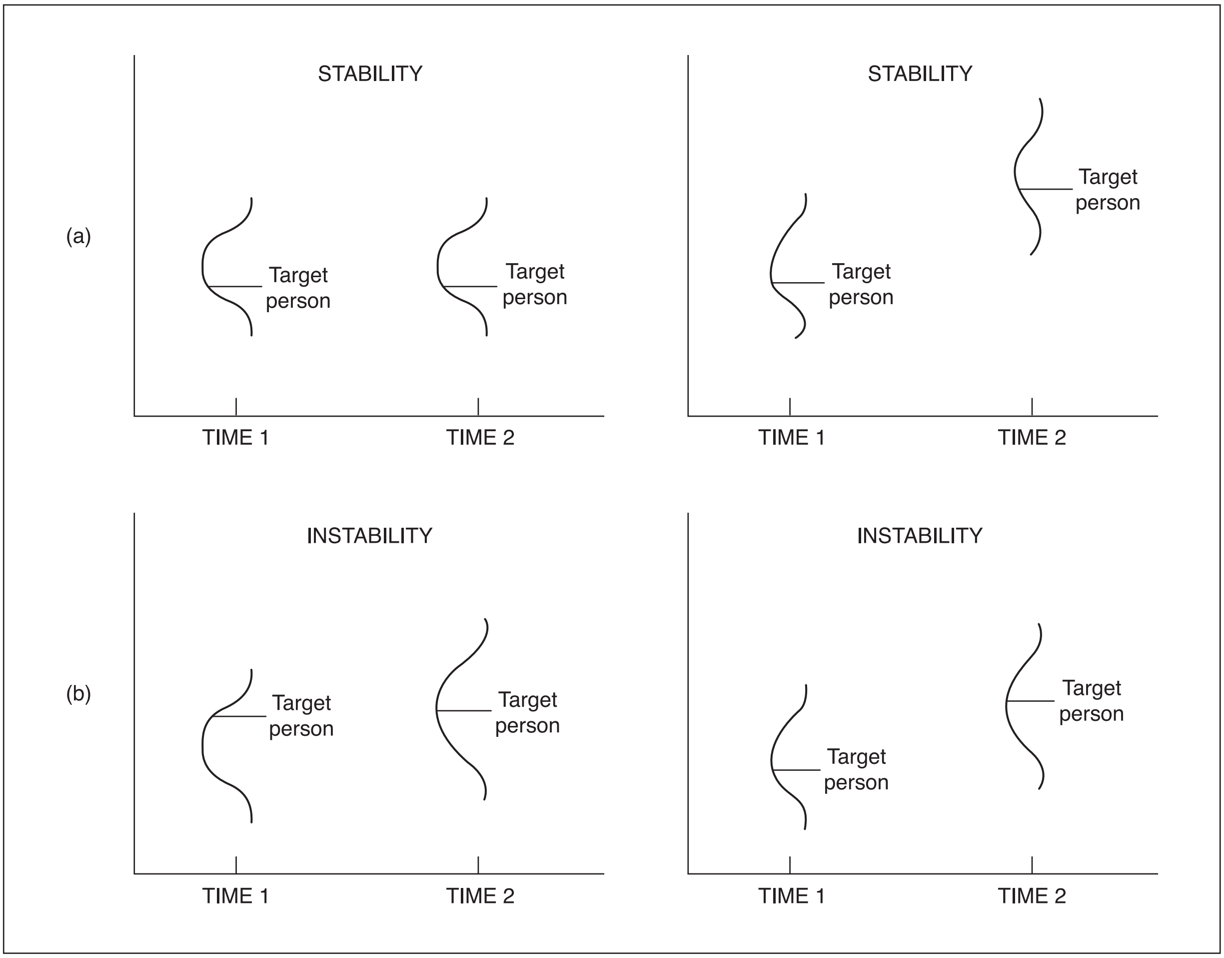


Figure 8.18 Two instances of the relative changes comprising (a) stability and (b) instability.

These instances of stability and instability are illustrated in Figure 8.18, where the relative changes that comprise stability and instability in reference to a given target person in each instance are illustrated.

Relation of Continuity and Discontinuity to Stability and Instability

The concepts of stability and instability describe the relative position of a developing person, whereas continuity and discontinuity pertain to the intraindividual manifestation of the functioning of the processes affecting development. In traditional (or classic) psychometric (or test) theory, stability refers to the maintenance across time of interindividual differences; instability refers to the alteration over time in these between-people differences. Whereas stability–instability is a methodological issue (pertinent to the reliability of measurement) in classic test theory, the issue is a substantive one in the study of human development. In order to understand and describe the types of changes that characterize human development, a developmental scientist can, and must, deal simultaneously with two issues—continuity–discontinuity and stability–instability (e.g., see Baltes & Nesselroade, 1973; Emmerich, 1968). The processes that determine a person's development may be either continuous or discontinuous (in regard to both description and explanation), and the functioning of these processes may result in a person's stability or instability relative to his or her reference group.

To illustrate in respect to explanation, a developmental change may be of one of four types: (1) continuity and stability; (2) continuity and instability; (3) discontinuity and stability; or (4) discontinuity and instability. These four types of changes are indicated in Figure 8.19. In Box 1, continuity and stability is referenced. A change that is both continuous and stable is a change in which the processes governing behavior remain the same between two points in development and the rank-ordering of people in a reference group affected by the continuous functioning of these processes remains the same. Thus, the variables involved in the determination of these people's behavior do not change, and the people's relative positions in the group also remain the same.

In Box 2, a second type of developmental change, continuity and instability, is referenced. In this case, although the processes affecting development remain the same across time points (continuity), people's relative positions in their reference group change with development. Changes of this sort would comprise no alterations in the variables affecting development but only changes in the ranking of people in a reference group.

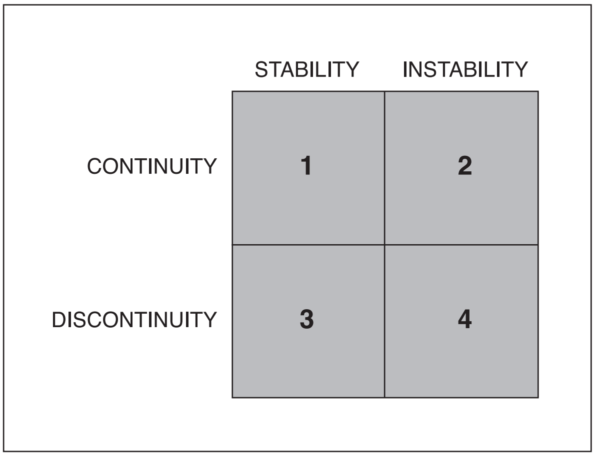


Figure 8.19 The interrelation of continuity–discontinuity and stability–instability.

In Box 3, discontinuity and stability is referenced. Here, the processes affecting development are altered across time points, but people's relative positions in their reference group remain the same. Such changes are constituted by the nature of the variables involved in development changing (discontinuity) but people's rank-ordering in their reference group remaining the same (stability).

Finally, in Box 4, a fourth type of developmental change is referenced, discontinuity and instability. In this instance, the processes governing behavioral development change, and so do the relative positions of people in a reference group affected by these changed processes. In this kind of change the variables involved in development are altered, and the rankings of people in a reference group affected by the discontinuous functioning of these variables are also changed.

An important conclusion to draw from Figure 8.19 is that phenomena of continuity–discontinuity are distinct from those of stability–instability. Continuity does not imply stability and discontinuity does not imply instability; continuity may just as readily be coupled with instability as with stability, and discontinuity may just as readily be coupled with stability. All these relations are possible because the concepts of continuity and discontinuity pertain to the description and explanation of intraindividual change, whereas the concepts of stability and instability refer to interindividual differences. These latter concepts pertain to whether interindividual differences—for example, the rank-order of people along some dimension—remain the same (stability) or change (instability) across time.

It is crucial that these distinctions be kept in mind in order to avoid making mistaken inferences about the absence or presence of intraindividual change on the basis of information about stability (Baltes & Nesselroade, 1973; Baltes, Cornelius, & Nesselroade, 1978). The scores of a group of individuals may show complete stability. For example, the correlation between scores on two occasions of measurement may be perfect; the rank-order of a group in regard to their scores on a dimension may not change from Time 1 to Time 2; or the average (mean) score for the group may remain the same from Time 1 to Time 2.

Nevertheless, considerable intraindividual change may exist in regard to most if not all of the

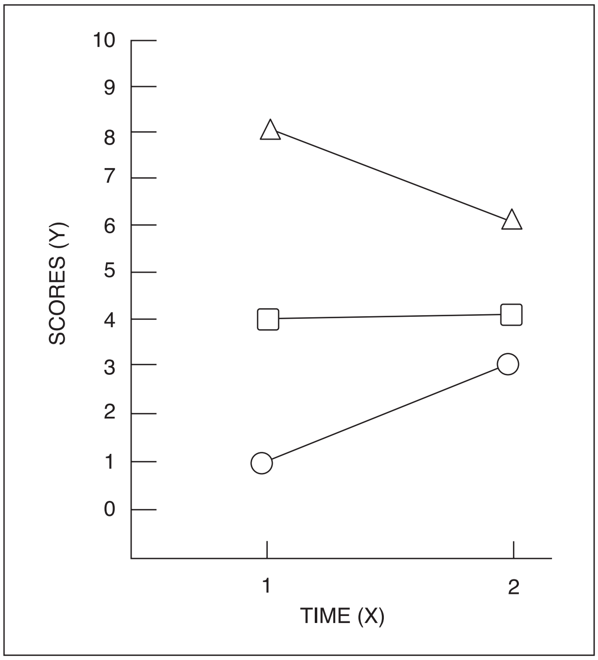


Figure 8.20 An illustration of why stability does not mean the absence of intraindividual change. The rank-order position along the Y axis of all people studied at Times 1 and 2 remains stable, as does the group mean; however, this stability says nothing about whether intraindividual change has occurred or about the directions (the trajectories) of intraindividual change, which in this illustration are all different.

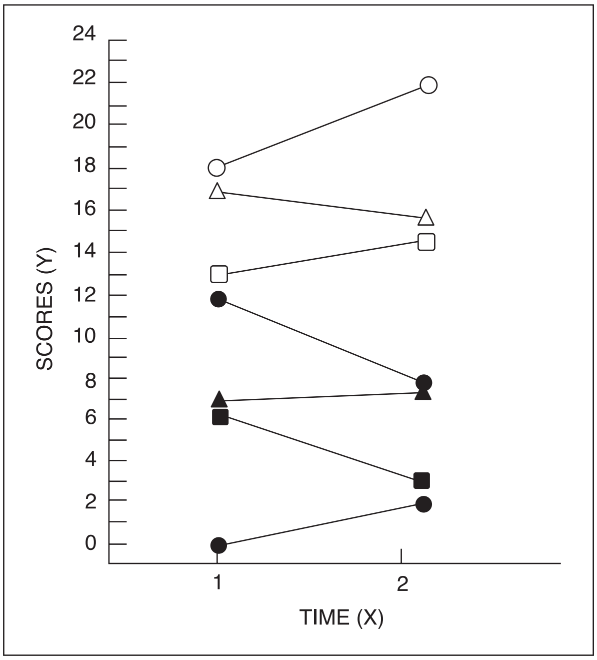


Figure 8.21 Another illustration of the distinction between stability and intraindividual change. The rank-order location of the group along the Y axis remains unchanged across time, indicative of complete stability, and yet all members of the group undergo intraindividual change across time.

people in the group. This possibility is illustrated in Figure 8.20, where, for a group of three people, there is complete stability in regard to rank-order and mean level, and yet considerable intraindividual change exists in regard to two of the three people in the group. Indeed, the directions of development (the trajectories of intraindividual change) are different for each of the people in this group. Another illustration of the distinction between stability and intraindividual change is presented in Figure 8.21, but here for a group larger than depicted in Figure 8.20; again, despite complete stability in regard to their rank-order placement at two times of measurement, each member of this group shows evidence of intraindividual change, and, in addition, several different change trajectories are present.

Changes Characteristic of Development

A person may show stability of location when in one group (e.g., distributions formed by measuring height at 17 years of age and at 21 years of age), but may show instability when considered in the context of another (e.g., a distribution formed by measuring knowledge of calculus at 17 years of age and at 21 years of age). Not only does this possible variation underscore the point that stability–instability is a group consideration and not an attribute of a person, but it also suggests that, when different measures of characteristics are taken, different statements about stability–instability may appropriately be made.

Within the same portion of the life span, people may show stability in regard to measures of some processes and instability in others. Any of these differences may, of course, involve either continuity or discontinuity. A developmental scientist cannot appropriately speak, then, of a given period of life as including just one particular type of change. Moreover, in the large and complex data sets typically analyzed by human development researchers, the conceptual distinctions between continuity–discontinuity and stability–instability must be kept in mind because the differences between these constructs may not be readily discernible just by an inspection of some aggregate scores, such as means and correlations (Molenaar & Nesselroade, 2014, 2015; Rose, 2016).

Any statements about the nature of change depend on the particular change process on which a developmental scientist focuses. More important, however, because the same change phenomenon (e.g., attachment in infancy or childhood, storm and stress in adolescence, or feelings of generativity in adulthood) may be understood and measured in different ways, depending on the theoretical orientation of the researcher, statements about the nature of change relate primarily to theoretical issues.

In order to fully describe the types of changes that may characterize any portion of the life span, a developmental scientist should pay attention to all the levels at which change can exist, and to the way in which concepts drawn from theories pertaining to processes at all these levels together may provide a comprehensive and integrated depiction of development across life. In turn, it is possible to see theory as a key "protection" against interpreting a given data set as indicative of one versus another form of continuity versus discontinuity (and/or stability versus instability). This "buffer" is the a priori metatheoretical presuppositions (Kagan, 1980, 1983) and theoretical assumptions (Overton, 2015a) that proscribe and prescribe particular formats, or instances, of continuity or discontinuity. To illustrate, it is useful to discuss the instances of continuity and/or discontinuity that are prototypically included or excluded in theories associated with organismic, mechanistic, and RDS-based models of development.

CONTINUITY AND DISCONTINUITY IN DEVELOPMENT: METATHEORETICAL AND THEORETICAL PROSCRIPTIONS AND PRESCRIPTIONS

Theories embedded within a given model of development are not all alike. For example, whereas the nature-oriented theories of Gesell (1946) and Erikson (1959) emphasized an ontogenetic, maturational "ground plan" as constituting the key process explaining developmental change, Hall's (1904) nature-oriented theory explained ontogenetic changes by positing a biogenetic recapitulation in ontogeny of phylogenetic changes (see Haeckel, 1868). In turn, the nurture-mechanistic theory of Bijou (1976) emphasized proximal stimuli as the material and efficient causes shaping behavior. This formulation is vague or mute, however, regarding how distal sociocultural institutional influences are translated into such stimulation. In contrast, some theorists with mechanistic, sociological views (e.g., Dannefer, 1984; Homans, 1961; Meyer, 1988) discussed the distal, age-graded channeling of behavior by societal institutions, but did not discuss the links between these entities and proximal stimulation.

However, despite such differences, theories within a metatheoretical "family" (Reese & Overton, 1970) are more similar to one another than they are to theories associated with other metatheories or world hypotheses (see Pepper, 1942). Consequently, it is possible to describe, at least for the general case, the prototypic views regarding continuity and discontinuity associated with theories from organismic, mechanistic, and RDS-based metatheoretical families (remembering of course that subtle individual differences may nevertheless exist among "family members").

Theories associated with any model can accommodate the presence, at a descriptive level, of both quantitative and qualitative continuity. This acceptance, however, is nothing more than admitting that things may stay the same across at least some portions of life. That is, there may be ontogenetic stasis. Quantitatively, this constancy may mean that identical scores for a construct are present across time or that rates of growth remain constant (e.g., throughout the middle-childhood years). Qualitatively, constancy can occur when there is no numerical or structural change in the components of the behavioral repertoire.

Differences among the models exist, however, for the remaining instances of continuity and discontinuity. Both organismic- and RDS-based theories recognize that, descriptively, both quantitative and qualitative discontinuities can occur in development. However, descriptive quantitative discontinuities are largely irrelevant to organismic theorists, whose interests focus almost exclusively on qualitative structural variation across ontogeny (e.g., Erikson, 1959; Piaget, 1950). In mechanistic theories of development, no true descriptive qualitative discontinuity can exist. No novelty can exist in development (Overton, 2015a; von Bertalanffy, 1933). Given the commitment in mechanist models to reduce all developmental phenomena to a common set of constituent elements (e.g., in nature, mechanistic theories to genes, and in nurture, mechanistic theories to S–R connections), lightly scratching the surface of any claim of newness or novelty in development will readily reveal that the operationalization of such terms is made via recourse to the quantitative combinations (additions) of identically constituted elements. The treatment by the nurture, mechanistic theorists Bijou and Baer (1961), of personality in childhood, which is interpreted as being reducible to chains of S–R connections, is a case in point. Similarly, the ideas of the nature, mechanist theorist David Rowe (1994), that all of socialization in human development can be reduced to the genes inherited at conception, are another example of such a perspective.

In regard to explanatory continuity and discontinuity, all models are distinct (see Overton, 1991a, 1991b, 2015a). With respect to continuity, organismic theories do not typically explain development by reference to quantitatively invariant processes. Instead, qualitative invariance is stressed (i.e., to explain within-stage consistency or across-stage *décalages*; e.g., Levinson, 1978; Neugarten & Guttmann, 1968; Piaget, 1950). The reverse of these emphases is found in mechanistic theories. That is, quantitative invariance is emphasized. In RDS-based models, development may be explained by either quantitative or qualitative continuity. In regard to quantitative invariance, Haan and Day (1974) accounted for the maintenance of adults' style of engagement with their context by reference to a quantitative invariance in scores for activity level. Schaie and Geiwitz (1982) accounted for the maintenance of adult personality structure by noting that adults select contexts within which to coact that provide a goodness of fit with their already established personality structure and, as such, do not provide demands for qualitative change in personality.

Finally, whereas organismic theories do not discuss quantitative discontinuity as an explanation for development, in mechanistic theories, qualitative discontinuity in the explanations of development is not possible. Within RDS-based models, both qualitatively and quantitatively discontinuous explanations may be used. For example, whereas Elder (1974, 1980, 1999; Elder et al., 2015) and Schaie (1984) both drew on the link between individuals and features of the historical epoch within which they lived to explain individual differences in personality development, they did so by positing different types of influences. Elder (1974) argued that individual differences in achievement, health, and degrees of commitments to family values were influenced by whether a person experienced a qualitatively distinct historical event (the Great Depression) in his or her childhood or adolescence. Thus, Elder (1974) used experience by members of one birth cohort of an individual↔context relation that is qualitatively discontinuous from that of another cohort to account for interindividual differences (across birth cohorts) in intraindividual change.

In turn, Schaie (1984) posited that such cohort differences in personality development across the adult and aged years may be explained by quantitative differences across historical eras in the accumulation of life events (e.g., epidemics, wars, unemployment, inflation, and technological innovations). To Schaie, it was not the nature (quality) of the events per se that explained interindividual differences in intraindividual change in personality. Instead, these differences occurred because of historical quantitative discontinuities in the cumulative number of events that comprised the context of a given cohort.

In sum, theories associated with the organismic, mechanistic, and developmental-contextual models differ in the instances of continuity–discontinuity they see as possible. These differences underscore, then, the idea that continuity–discontinuity is not only an empirical concern but also a metatheoretical issue. In other words, the issue of continuity–discontinuity in human development is an empirical question, but *only* within the constraints of the variables considered relevant by the researcher's theory or metatheory; adopting alternative metatheories may introduce new variables that show a different empirical pattern.

Moreover, this theoretical embeddedness of continuity–discontinuity leads to another key point: No instance of quantitative or qualitative continuity or discontinuity is necessarily excluded within RDS-based models. Indeed, any instance of continuity or discontinuity may exist within or across periods of the life span, depending on conditions pertinent to the dynamic (mutually influential) relations between individuals and the multilevel context (ecology) of human development, i.e., depending on the course of individual↔context relations. Accordingly, there are multiple directions that development can take. For example, a process may show increases, decreases, curvilinear change, or smooth or abrupt change, etc. All these formats of change in a developmental process are possible, and the specific shape or form of a developmental trajectory for a specific individual or group is a matter of theory-predicated empirical inquiry (Wohlwill, 1973).

Acceptance of all possibilities may seem like uncritical eclecticism, but I believe that such an appraisal is not correct. RDS-based theories do not maintain that all instances of continuity–discontinuity occur within and across developmental periods. Instead, the point is that several instances *may* happen. The empirical implication of this view is that developmental scientists using RDS-based theories to frame their research should seek to identify the individual and contextual conditions within which any specific instance of continuity–discontinuity occurs (Baltes, 1987; Lerner, 1984).

In the search for the individual and contextual conditions of continuity and discontinuity, RDS-oriented developmental scientists would tend to agree with one implication of the orthogenetic principle of Heinz Werner (1957). The human life course is a synthesis of processes which, simultaneously, make humans both (a) similar to themselves at other points in time, and to others as well (these are nomothetic processes, ones affording continuity at both descriptive and explanatory levels—global and hierarchically integrated processes to Werner); and (b) *different* from themselves at other points in time, and from others as well (these are idiographic processes, ones affording discontinuity at both descriptive and explanatory levels—differentiating processes to Werner, 1957).

In other words, to developmental scientists the search for the conditions of continuity and discontinuity may be translated into a search for nomothetic change processes (i.e., processes general to all of human development; processes that all people experience) and idiographic change processes (i.e., processes specific to the development of a particular individual). The integrative presence of both nomothetic and idiographic processes makes all people, at one and the same time, both similar to others and different from them. The search to identify both sorts of processes is admittedly quite complex, certainly more difficult than one involving the a priori theoretical exclusion of specific instances of continuity–discontinuity.

CONCLUSIONS

Any developmental change may be characterized as being either continuous or discontinuous *and* either stable or instable, and different theories of development proscribe and prescribe the character of the changes that may be involved in human development. Theories that vary in their commitment to nature, to nurture, or to nature↔nurture relational ideas may be contrasted, then, in regard to their inclusion of ideas pertinent to qualitative and quantitative, descriptive and explanatory continuity and discontinuity.

Thus, the two core issues involved in the study of human development—the nature–nurture and the continuity–discontinuity controversies—are both involved in different theories of development. The connection between these controversies in various theories of human development will be evident as I begin to consider the details of the different theoretical positions in human development. I focus first on models associated with RDS-based theories (Chapters 9 and 10) and then on models linked to genetic-reductionist conceptions (Chapters 11 and 12).

CHAPTER NINE

Relational Developmental Systems-Based Theories

Comparative, Evolutionary, and Ontogenetic Conceptions

The approaches to developmental explanation associated with the Cartesian-split paradigm stand in contrast to those explanations associated with the process-relational paradigm and relational developmental systems (RDS) metatheory (Overton, 2015a). RDS-based approaches to theory aim to heal the biological/social-cultural split both by offering ideas that describe the biological, individual-psychological, and the sociocultural and physical-ecological levels of organization as alternative ways of viewing the same whole (see Gollin, 1981; Gottlieb, 1992, 1997, 2004; Lerner, 1986; Overton, 1973, 2015a; Tobach, 1981), and by suggesting that the integration of *action*—of the individual on the context and of the multiple levels of the context on the individual—constitutes the process of development (Overton, 2015a).

RDS-based theories are understood as a family of conceptual models that promote a holistic, integrated view of human development (Lerner, 2015a; Overton, 2015a). There are many theoretical instances of these relational positions (see the chapters in Overton & Molenaar, 2015, for several examples). In Chapter 2, I introduced some key features of RDS-based theories, and in subsequent chapters I presented several examples of these family members. Across these discussions, I drew to a great extent on the ideas of past and current comparative psychologists, as well as evolutionary and developmental biologists (Lerner, 2015a, 2016; Overton, 2015a).

Accordingly, to build on these discussions of what are some of the key paradigmatic and metatheoretical foundations of RDS-based theories of human development, it is useful to focus in this chapter on other contributions made by key contributors to these theories, that is, T. C. Schneirla, Ethel Tobach, Daniel Lehrman, Howard Moltz, Patrick Bateson, Eva Jablonka, Marion Lamb, Gary Greenberg, David Moore, Michael Meaney, Robert Lickliter, and Gilbert Gottlieb. A key conception found in the work of these scholars is that ideas pertinent to evolution (phylogeny) have relevance for ideas pertinent to ontogeny.

As such, I will also discuss in this chapter how concepts found in evolutionary and developmental biology and comparative psychology that reflect RDS-based thinking are relevant to (or, even more, may be transformed into) concepts and theories pertinent to human development. Therefore, I will also include in this chapter examples of RDS-based ontogenetic models that reflect the influence of RDS-based ideas found in evolutionary and developmental biology and comparative psychology. This discussion will enable me to introduce a model, developmental contextualism, that has helped frame my own research about human, and in particular adolescent and youth, development. I will also illustrate some empirical work derived from this model, and I will discuss the work my colleagues and I have done regarding the topic of positive youth development.

To begin these discussions, I may note that, together, comparative and evolutionary/developmental biologists have contributed ideas that enable integrative, coactional models of individual development to be formulated. As well, these ideas account in large part for the insistence by adherents of RDS-based models on non-split, non-reductionist, and non-essentialist approaches to the place of biological variables and processes within the relational developmental system. It is useful to begin this discussion by returning to the ideas of Gilbert Gottlieb, and to consider his approach to conceptualizing the developmental system.

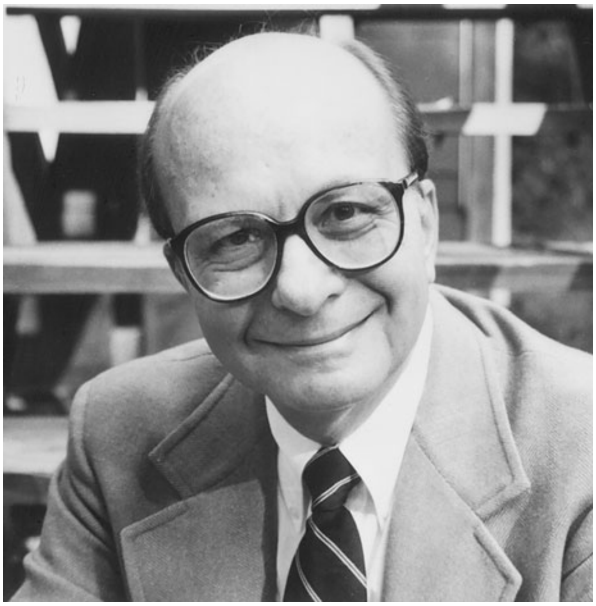
As already evident in prior chapters, I use Gottlieb's (e.g., 1970, 1997, 1998; Gottlieb et al., 2006) work to help frame discussions of RDS metatheory and, more specifically, of the probabilistic-epigenetic process involved in RDS-based theoretical models of human development (e.g., Ford & Lerner, 1992; Lerner, 1979, 2004). His ideas derived from his research involving analyses of biology↔context relations in ontogeny.

GILBERT GOTTLIEB'S THEORY OF DEVELOPMENTAL SYSTEMS

The histories of both developmental and comparative science during the twentieth century attest unequivocally to the fact that the theory and research of Gilbert Gottlieb, along with the work of such eminent colleagues as T. C. Schneirla (1956, 1957), Zing-Yang Kuo (1967; Greenberg & Partridge, 2000), Jay Rosenblatt (2010), Ethel Tobach (1971, 1981), Daniel Lehrman (1953, 1970), Howard Moltz (1965), George Michel (2010), Gary Greenberg (e.g., 2011, 2013, 2014, 2015), and Robert Lickliter (2016; Lickliter & Honeycutt, 2015), may be seen as the most creative, integrative, generative, and important scholarship in the field. For more than a third of a century, up until his death in 2006, Gilbert Gottlieb (e.g., 1970, 1997; Gottlieb, Wahlsten, & Lickliter, 2006; Lickliter & Honeycutt, 2015; Witherington & Lickliter, 2016, 2017) provided an insightful theoretical frame, and an ingenious empirical voice, involving the view that:

> an understanding of heredity and individual development will allow not only a clear picture of how an adult animal is formed but [also] that such an understanding is indispensable for an appreciation of the processes of evolution as well [and that] the persistence of the nature–nurture dichotomy reflects an inadequate understanding of the relations among heredity, development, and evolution, or, more specifically, the relationship of genetics to embryology.
>
> (Gottlieb, 1992, p. 137)

Gottlieb attempted to heal the Cartesian nature–nurture split between biological and social science (Overton, 2006, 2015a) by developing a theoretical conception of the dynamic and mutually influential relations, or coactions, among the levels of organization comprising the developmental system, that is, levels ranging from the genetic through the sociocultural and historical. In devising an RDS-based theoretical perspective about the sources of development, and bringing rigorous comparative developmental data to bear on the integrative


Gilbert Gottlieb

concepts involved in his model of mutually influential, organism↔context relations, Gottlieb's theory and research (e.g., Gottlieb, 1991, 1992, 1997, 1998, 2004; Gottlieb et al., 2006) became the exemplar in the last decades of the twentieth century and into the first portion of the initial decade of the twenty-first century of the RDS metatheory of developmental science (e.g., Overton, 2006, 2015a).

Gottlieb presented an integrative, RDS-based theory of evolution, ontogenetic development, and—ultimately—causality. Gottlieb argued that "The cause of development—what makes development happen—is the relationship of the components, not the components themselves. Genes in themselves cannot cause development any more than stimulation in itself can cause development" (Gottlieb, 1997, p. 91). Similarly, he noted that, "Because of the emergent nature of epigenetic development, another important feature of developmental systems is that causality is often not 'linear' or straightforward" (Gottlieb, 1997, p. 96).

As I discussed in previous chapters, Gottlieb offered, then, a probabilistic conception of epigenesis, one that constitutes a compelling alternative to views of development that rest on what he argued was a counterfactual, split, and reductionist nature–nurture conception (see Overton, 2015a). Gottlieb's developmental systems theory grew from decades of his research, which covered the range of emerging and continuing issues in understanding the dynamic fusion of biology and ecology that constitutes the fundamental feature of the developmental process (e.g., Gottlieb, 1997, 1998). In particular, he challenged the deterministic concepts of innateness and instinct, and offered instead his conception of probabilistic epigenesis as a basis for shaping behavioral development as well as evolutionary change.

According to Gottlieb, development proceeds in concert with influences from all levels of the organism and the context. "A probabilistic view of epigenesis holds that the sequence and outcomes of development are probabilistically determined by the critical operation of various endogenous and exogenous stimulative events" (Gottlieb, 2004, p. 94). Gottlieb's work has influenced several generations of comparative and developmental scientists to eschew simplistic, conceptually reductionist, and split (i.e., nature as separate from nurture) conceptions of the developmental process and to think, instead, systemically and, within the context of rigorous experimental and/or longitudinal studies, to attend to the dynamics of mutually influential organism↔context relations.

Gottlieb's impact on theory and research may be seen throughout *The Handbook of Developmental Science, Behavior, and Genetics* (Hood, Halpern, Greenberg, & Lerner, 2010), which commemorates the historically important and profound contributions he made across a scholarly career spanning more than four decades. Gottlieb was preparing this handbook when his untimely death in 2006 brought his work on this project to a halt. However, with the permission and support of the Gottlieb family, the editors of this work decided to complete Gottlieb's "last book," which was designed to bring together in one place the cutting-edge of contemporary theory and research underscoring the usefulness of an integrative, RDS-based theoretical approach to understanding the mutually influential relations among genes, behavior, and context that propel the development of organisms across their life spans (see the chapters in Hood et al., 2010). Accordingly, this handbook is a significant reference for documenting the cutting-edge ideas in comparative and developmental science and the scope and power of Gottlieb's scholarship.

RETURNING TO GOTTLIEB'S VIEW OF EPIGENESIS

In prior chapters, I discussed features of Gottlieb's (e.g., 1970, 1998) conception of probabilistic epigenesis. It is useful to elaborate on this prior discussion in order to both (a) point to some of the data Gottlieb drew from to derive his view of the probabilistic-epigenetic process; and (2) establish some key points about the place of genes within the relational developmental system. I will use the points I make about both of these facets of Gottlieb's work to frame my critique of past and current genetic reductionist theories in Chapters 11 and 12.

Gottlieb (1992) presented a developmental systems perspective within which changing gene↔context, or organism↔context, relations are the key foci of both developmental *and* evolutionary analysis. As such, he built on the work of Garstang (1922), de Beer (1930, 1958), and Goldschmidt (1933), and noted that “Phylogeny is thus not the cause but the product of a succession of different ontogenies” (Gottlieb, 1992, p. 90; see too Witherington & Lickliter, 2016, 2017). In other words, variation in development—for instance, behavioral novelty arising through the plasticity of dynamic, organism↔context relations (Lerner, 1984, 2006b, 2015a)—produces evolution; evolution does not produce development. “Ontogeny in each generation is a consequence of the coaction of hereditary or genetic factors and many different local environmental circumstances that determine the expression of the phenotype during the course of development” (Gottlieb, 1992, p. 95).

In essence, then, Gottlieb agreed with Goldschmidt (1933, p. 543) that “The nature and working of the developmental process of the individual then should, if known, permit us to form certain notions regarding the possibilities of evolutionary changes.” And what is the character of the developmental process as envisioned by Gottlieb?

Gottlieb’s conception of the developmental process “is one of a totally interrelated, fully coactional system in which the activity of genes themselves can be affected through the cytoplasm of the cell by events originating at any other level in the system, including the external environment” (Gottlieb, 1992, pp. 144–145). Based on the work of Schneirla (e.g., 1957), Kuo (1976), Lehrman (1970), and others (e.g., Tobach, 1981; and of course Gottlieb himself, 1970, 1976a, 1976b, 1983, 1991), Gottlieb (1992) provided a new definition of epigenesis:

> Individual development is characterized by an increase of complexity of organization—i.e., the emergence of new structural and functional properties and competencies—at all levels of analysis (molecular, subcellular, cellular, organismic) as a consequence of horizontal and vertical coactions among its parts, including organism–environment coactions.
>
> (pp. 159–160)

As I noted in prior chapters, the contemporary study of epigenetics provides abundant data in support of Gottlieb’s view (e.g., Cole, 2014; Harper, 2005, 2010, 2013; Lester et al., 2016; Meaney, 2010, 2014; Misteli, 2013; Moore, 2015a, 2016; Roth, 2012; Slavich & Cole, 2013).

Accordingly, horizontal coactions “are those that occur at the same level (gene–gene, cell–cell, tissue–tissue, organism–organism), whereas vertical coactions occur at different levels (gene–cytoplasm, cell–tissue, behavioral activity–nervous system) and are reciprocal, meaning that they can influence each other in either direction, from lower to higher, or from higher to lower, levels of the developing system” (Gottlieb, 1992, pp. 160–161).

In presenting his views of a developmental systems conception of development, Gottlieb noted that when one spoke of coaction between genes and the other levels of the system as being at the “heart of developmental analysis or causality what we mean is that we need to specify some relationship between at least two components of the developmental system” (Gottlieb, 1992, pp. 162–163). Indeed, Gottlieb (1992) contended that this systems view of individual development was the *only* “way to envisage the manner in which development must occur if a harmoniously functioning, fully integrated organism is to be its product” (pp. 165–166). “[G]enes are part of the developmental system in the same sense as other components (cell, tissue, organism), so genes must be susceptible to influence from other levels during the process of individual development” (Gottlieb, 1992, p. 167).

The theory and data Gottlieb (1997, 2004; Gottlieb et al., 2006) marshaled in support of this developmental systems view are compelling (see too Hood et al., 2010; Lerner & Benson, 2013a, 2013b; Lickliter & Honeycutt, 2015). Many of these examples involve integrated, multilevel exchanges of material (e.g., nutritional and hormonal) or energy (e.g., light) variables. Gottlieb explained that such evidence underscored that the action of genes (gene expression) was “affected by events at other levels of the [developmental] system” (Gottlieb, 1991, p. 5), that “all levels of the system may be considered potentially equal” (Gottlieb, 1991, p. 6), and, therefore, that “genetic activity does not by itself produce finished traits such as blue eyes, arms, legs, or neurons. The problem of anatomical and physiological differentiation remains unsolved, but it is unanimously recognized as requiring influences above the strictly cellular level" (Gottlieb, 1991, p. 5). Thus, intraorganism variables making up the proximal context of the gene, as well as extraorganism contextual variables, were shown by Gottlieb (1991), as well as in the literature he cited (e.g., Edelman, 1987, 1988; Grouse, Schrier, Letendre, & Nelson, 1980; Kollar & Fisher, 1980; Uphouse & Bonner, 1975), to exist in a reciprocally influential relation with genes.

Given this evidence, one conclusion is inescapable: The idea, that genes are impenetrable and fixed entities that direct a person's development in a manner independent of the supragenetic, organismic, and environmental (contextual) levels of organization within which the genes are embedded, is absurd (Ho, 1984, 2010, 2013, 2014; Krimsky, 2013; Lerner & Overton, 2017; Michel, 2010; Wahlsten, 2010, 2013; Strohman, 1993a, 1993b). No feature of biology is so encapsulated, so automated, and so invulnerable to moderation by the context that it can stand as an example of such an impenetrable entity. Simply, then, just as genes may influence supragenetic levels, both within and outside of the organism, these levels of organization influence genes (Cole, 2014; Slavich & Cole, 2013). It is these multilevel coactions that produce development, and that are embodied in Gottlieb's (1992) definition of epigenesis noted earlier.

Thus, the developmental systems framework of Gottlieb (1991, 1992, 1997) indicates that all organismic characteristics (e.g., genes, cells, tissues, and organs), as well as the whole organism itself, function in a bidirectional, reciprocal, or integrated, coactional, system with the contexts within which the organism is embedded. This system includes time (history), and the timing of coactions is critical in moderating developmental outcomes. For instance, processes obviously change temporally, and time-ordered changes in one component of the system will create changes in other components that will differ in relation to the status (e.g., phase or point) of change in this first component. This variation is the essence of the probabilistic facet of probabilistic epigenesis; thus, the dimension of time—the chronosystem within Bronfenbrenner's (2005; Bronfenbrenner & Morris, 2006; see too Elder, 1998; Elder et al., 2015) RDS-based conception—affects the course of individual development.

Within the human development literature, examples of dynamic coactions have most often involved integrated, multilevel exchanges (Ford & Lerner, 1992; Lerner, 1991, 2006b, 2012, 2015a). Although these types of examples refer to exchanges having contents that are qualitatively different, their structure and function can be integrated within a common RDS-based perspective, such as the one forwarded by Gottlieb (1991, 1992; see too Ford & Lerner, 1992; Lerner, 1979, 2004). Indeed, whether illustrated by data from the field of comparative psychology or from the field of human development, RDS-based models underscore the idea that the basic process of development is a relational one (Overton, 2015a). That is, the basic process of development is changing relations between the organism and the multilevel context comprising the ecology of the organism's development.

Moreover, the reciprocity between organism and context and, as noted in regard to the concept of timing, the temporality that derives from the embeddedness of all levels of this system in history, provides a change component to the organism, to the context, and to the relation between the two. In addition, the singularity of the array of variables from the multiple, integrated levels that characterize an organism across its life span, means that there may be systematic individual differences (i.e., individuality that is neither mere error variance nor substantively trivial) over the course of ontogeny. Thus, as Gottlieb (1992) pointed out: "Ontogeny in each generation is a consequence of the coaction of hereditary or genetic factors and many different local environmental circumstances that determine the expression of the phenotype during the course of development" (p. 95). Accordingly, the key features of Gottlieb's developmental systems perspective provide an intellectually important and societally timely frame for the study of human development. These features include:

- Changing organism–context coactions.
- A focus on the actual physical and social ecology within which the organism develops.
- Individual differences or diversity.
- A sensitivity to the entire life span as a legitimate frame within which to study coactions and individuality.

In short, within the context of Gottlieb's developmental systems view, epigenesis is a probabilistic process of individual development and "The most important feature of the developmental systems view is the explicit recognition that the genes are an integral part of the system and their activity (i.e., genetic expression) is affected by events at other levels of the system, including the environment of the organism" (1997, p. 82). Indeed, Gottlieb emphasized that "The principal ideas concern the epigenetic characterization of individual development as an emergent, coactional, hierarchical system" (Gottlieb, 1997, p. 89).

As I discussed in earlier chapters, Gottlieb (1997) drew a distinction between the probabilistic view of epigenesis and the predetermined version of epigenesis. The latter "viewpoint holds that behavioral epigenesis is predetermined by invariant organic factors of growth and differentiation (particularly neural maturation), and the . . . [former] . . . viewpoint holds that the sequence and outcome of prenatal behavior is probabilistically determined by the critical operation of various endogenous and exogenous stimulative events" (Gottlieb, 1970, p. 111). These intraindividual and extraindividual (contextual) events are parts of the organism's experience.

Within the RDS-based perspective forwarded by Gottlieb, experience represents, then, a concept that is central in attempts to distinguish between an integrative, developmental systems perspective and a split conception. Akin to the views advanced by Schneirla (1957), Gottlieb stated that:

> [E]xperience should be broadly defined to include activity produced within the organism itself (endogenous motor as well as sensory-system activity) . . . It is only by denying (or not acknowledging) the role of spontaneous endogenous activity within the nervous system as playing a formative role in neural and behavioral development that the outmoded nature versus nurture conception can be kept alive.
>
> (Gottlieb, 1997, p. 55)

Indeed, by explaining the various roles of experience in development, Gottlieb "forces us to think in a new way about the role of experience in the development of behavior that is thought of as instinctive" (1997, p. 76).

Modes of Experiential Contribution

Gottlieb (1997) presented a conception of experience that is rich and nuanced. He noted that:

> The invitation to search for nonobvious experiential bases of unlearned behavior, implicit and explicit in the writings of Kuo (1976), Schneirla (1956), and Lehrman (1953) has been largely ignored. As de Santillana has written, "But nothing is so easy to ignore as something that does not yield freely to understanding" (de Santillana & von Dechend, 1977, p. xii).
>
> (p. 76)

Yet, Gottlieb's scholarship does document the various roles of experience in individual development—involving inductive experiences, facilitating experiences, and maintenance experiences. He demonstrated that experience provides nonobvious, but compelling, bases of behavior. In the absence of often subtle analysis, these behaviors are naively attributed to predetermined or instinctive bases.

To illustrate, in the context of his discussion of the roles of experience, Gottlieb explained his conception of the place of canalization (i.e., the narrowing of the range of potential systematic intraindividual changes as a consequence of prior developmental changes), and of its "alternative" process—malleability or plasticity, in development. Gottlieb (1997) noted that canalization and malleability are subtypes of induction, in that they require inductive experiences to occur.

> Canalization is a narrowing of responsiveness as a consequence of experience. [pp. 57–58] . . . This experiential canalization process is very similar to what happens in the developing nervous system: The initially "exuberant" (i.e., very large) number of synaptic contacts is pruned by experience. This is a cardinal feature of Gerald Edelman's (1987) notion of "neural Darwinism" . . . [Canalization is thus] a fourth role of experience, with experience

> being defined broadly to signify the contribution of *functional activity* at the behavioral and neural levels of analysis, whether the activity arises from external or internal sources . . . [Experiences can also] lead to an enhanced malleability or plasticity (the opposite of the narrowing of responsiveness brought about by canalizing experiences).
>
> (p. 59)

Thus, in the discussion of his concept of canalization, Gottlieb noted that experiences (i.e., organism↔context coactions) can either increase or decrease the possibilities of subsequent systematic developmental changes, which is a very different view of experiential influences on development than is found in Waddington's (1942, 1957, 1971) concept of canalization. Indeed, Gottlieb's conceptualization of possible experiential influences on development could lead to a productive line of research framed by the specificity principle (Bornstein, 2017) discussed in earlier chapters. That is, Gottlieb's ideas could frame an empirical search for the specific trajectories of organism↔context coactions that result in either increases or decreases in the probabilities of specific systematic developmental changes for specific organisms within specific contexts.

Thus, Gottlieb's concept of canalization is testable, and thus falsifiable, and provides an instance of the inductive role of experience. Gottlieb (1997) criticized

> the developmental geneticist Waddington's (1942) notion that early normal or species-typical physiological and anatomical development can withstand great assaults or perturbations and still return to (or remain on) its usual developmental pathway, thus producing the usual or normal phenotype. Waddington's concept of canalization says that usual developmental pathways are so strongly buffered (by genes—Waddington, 1957, p. 36, Fig. 5) that normal or species-typical development can be only temporarily derailed. Waddington (1968) used the term *chreod* to express his ideas more succinctly: A chreod, according to Waddington, is a "fated" or predetermined developmental pathway.
>
> (p. 80)

Gottlieb (1997) noted that Waddington apparently relied on the seeming face validity of this version of the concept of canalization. That is, Waddington presented no empirical support for this concept. Instead, the process purportedly involved in this concept is depicted in figurative or metaphorical terms (Gottlieb, 1997), that is, as a ball rolling down the valleys of an "epigenetic landscape" that varies in shape and in the height of the valley walls (Waddington, 1942, 1953, 1957, 1968).

Given the lack of actual empirical referents for this concept, Waddington's notion is, therefore, both conceptually and operationally empty (Gottlieb, 1997). His concept corresponds directly with a nativist, genetic determinist view of development. For example, Waddington (1957) claimed that:

> The epigenetic feed-back mechanisms on which canalization depends can, of course, be regarded as examples of gene interactions [p. 131] . . . The degree to which each pathway is canalized or self-establishing is dependent on the particular alleles of the genes involved in it.
>
> (pp. 20–21)

In contrast to nativist notions, such as those forwarded by Waddington (1957, 1971), and to further illustrate the character of experiential influences within the developmental system, Gottlieb offered several examples of the nonobvious role of experience in individual development. For instance, he explained how experiential factors (such as social relationships, the introduction of particular gasses into the proximal atmosphere of the developing individual, or changing day length) may influence physiological functioning (e.g., hormone secretions) which, in turn, may result in the turning on of genes, that is, in the activation of DNA transcription in the cell nucleus (see Moore, 2015a, and Slavich & Cole, 2013, for examples). Outcomes of such experiences may involve effects as dramatic as the development of teeth-like structures in chickens (see too Lickliter, 2016), sex reversals in coral reef fish, and a second set of wings in otherwise normal fruit flies (Gottlieb, 1997).

These examples and the others provided by Gottlieb (1997) about the role of experience in individual development, derived from his and others' research, underscore that coactions, among the integrated levels of organization of the developmental system, provide the basis of ontogenetic change (Gottlieb et al., 2006). As such Gottlieb noted that "when certain scientists refer to behavior or any other aspect of organismic structure or function as being 'genetically determined,' they are not mindful of the fact that genes synthesize protein (not behavior) and that they do so in the context of a developmental system of higher influences" (1997, p. 93). Indeed, such genetic determinist (nature–nurture split) conceptions (e.g., Belsky, 2014; Belsky et al., 1991; Bjorklund & Ellis, 2005; Plomin, Defries, Knopik, & Neiderhiser, 2016; Rimfeld, Ayorech, Dale, Kovas, & Plomin, 2016) "have provided impediments to thinking clearly about the need for conceptual and empirical analysis at all levels of the developmental systems hierarchy" (Gottlieb, 1991, p. 7).

To document the character of the contribution of these higher influences, Gottlieb presented comparative data indicating that, first, there is no relationship between morphological complexity and genome size, a relation that he thought was a reasonable possibility to explore. For example, although the haploid genome size (nucleotide pairs) of mammals is *less* than that of frogs and toads, salamanders, angiosperms, gymnosperms, pterosids (ferns), and even algae, mammals exceed all of these species in their approximate number of cell types and in the approximate number of descriptive morphological types they possess. In turn, second, Gottlieb documented that there is no relationship between the number of genes coding for protein and the number of neurons in the nervous system. For instance, among chordates, both *Mus musculus* and *Homo sapiens* have approximately 70,000 genes. Yet, the former organisms have about 40 million neurons whereas the latter organisms have approximately 85 billion neurons; similarly, whereas, among nematodes, *Caenorhabdhitis elegans* have about 14,000 genes and, among arthropods, *Drosophila melanogaster* have about 12,000 genes, the former organisms have approximately 302 neurons and the latter about 250,000.

Given these data, it is clear that a developmental system involving coactions of genes with other levels of organization with which they are integrated within this system provides the basis for individual development. As such, Gottlieb concluded that "there is good reason to seek the answer to evolution above the level of the genes, in the total developmental system" (1997, p. 147).

What Maintains the Split in the Study of Human Development?

Gottlieb's scholarship has been a visible and influential force in the field of developmental science for some time (e.g., Gottlieb, 1970, 1976a, 1976b, 1983, 1991, 1992, 1997, 2004; Gottlieb et al., 2006). Moreover, his perspective converges with that found in the scholarship of other long-term leaders of developmental comparative science (e.g., Kuo, 1967, 1976; Greenberg, 2011, 2015; Lehrman, 1953; Schneirla, 1956; Tobach, 1971). The prominence of this work creates a puzzle: The conceptualization of genes and, superordinately, of nature as separable from nurture, found within disciplines such as behavior genetics, sociobiology, and evolutionary developmental psychology is known—at least among geneticists and developmental comparative scientists—to be counterfactual (e.g., Bateson, 2015, 2016; Ho, 1984, 2010, 2013, 2014; Lewontin, Rose, & Kamin, 1984; Lickliter, 2016; Moore, 2015a, 2016; Müller-Hill, 1988; Saunders, 2010, 2013, 2014; Strohman, 1993a, 1993b; Wahlsten, 2010, 2012, 2013).

Scholars in the field of human development must, therefore, confront several questions as a consequence of this curious situation: How did the biological, social, and behavioral sciences that attempt to contribute to the understanding of human behavior and development arrive at this point? Why do not these fields declare that the "emperor has no clothes?" Why, instead, are grants awarded and scientific journal space allocated to work having this fatal conceptual flaw? Most important, why do scholars allow such mistaken reductionist and mechanist thinking to influence both science policy and social policy (e.g., see also Fisher et al., 2013, and Joseph, 2015, on this question)? In turn, why is there still resistance among some scholars working in the field of human development and in the policy arena to the scientifically valid alternative models of the role of biology↔context relations, models associated with the compelling theoretical ideas and convincing empirical evidence of a group of scholars who synthesize animal comparative and human development research and forward an RDS-based, relational perspective in their scholarship?

I believe that the search for answers to these questions must encompass not only issues of the science of human development but also of the sociology of this science (e.g., see Panofsky, 2014). In addition, the "politics" of this science, that is, the use of the science to further extra-scientific ideological agendas, cannot be ignored (e.g., see Kamin, 1974; Lerner, 1992).

I believe as well that it is clear that these questions are not merely "academic" in character. Science and public policy continue to be influenced by biologically reductionist, split conceptions of nature and nurture. At this writing, some relatively recent instances of Op-Ed pieces in major newspapers (Belsky, 2014), or of books written by well-credentialed science writers (e.g., Wade, 2014), advocate without adequate evidence that biological reductionist ideas should be applied to programs and policies. These erroneous applications of fallacious ideas create social mischief, at best, and racial, ethnic, class, gender, etc. divisiveness and social turmoil, at worst (e.g., Feldman, 2014). These recommended uses of bad science reinforce fears of institutionalized racism in America and further the societal marginalization of groups of color and of low socioeconomic standing (e.g., see Sampson, 2016); these implications of their recommendations are never publicly considered by those who promulgate these flawed extensions of counterfactual genetic reductionism.

There is a long history of the reductionist and essentialist thinking involved in these flawed appeals to policy and social interventions (e.g., see Lerner, 1992, 2014, 2015c, for other examples). Indeed, given the long history of this thinking, it is important to note remarks made by Francis Galton more than 100 years earlier. Exemplifying the sharp division between nature and nurture that is his "dubious intellectual legacy" (Gottlieb, 1992, p. 49), *and* the racist overtones often historically associated with this dichotomy, Galton contended that "When nature and nurture compete for supremacy on equal terms . . . the former proves the stronger. It is needless to insist that neither is self-sufficient; the highest natural endowments may be starved by defective nurture, whereas no carefulness of nurture can overcome the evil tendencies of an intrinsically bad *physique,* weak brain, or brutal disposition" (Galton, 1975, pp. 9–10). "The Negro now born in the United States has much the same natural faculties as his distant cousin who is born in Africa; the effect of his transplantation being ineffective in changing his nature" (Galton, 1892, p. xxiv).

This position is not that discrepant from the one forwarded a century later by Rowe (1994), who argued

> that social class may capture not variation in rearing and environmental social background, but instead variation in genes. This idea returns genes to socialization science by a back door—by the very variable (social class) thought to have liberated social science from hereditarian thinking!
>
> (p. 135)

And how do genes create those environmental characteristics marked by the term *social class*? To Rowe (1994):

> The answer is that the genes may construct a nervous system—and that hormones and neurotransmitters may then motivate behaviors resulting in dramatic redesign of an environment. The way a beaver will restructure its environment is as genetically shaped as its flat tail and keen hearing.
>
> (p. 90)

Thus, Rowe's answer, which is his description of a process termed *niche picking*, illustrates the reductionism of the nature–nurture split position and, as well, the acontextual, asystemic, and non-relational thinking about developmental process that is associated with such a position. Simply, then, as illustrated by authors such as Belsky (2014) and Plomin et al. (2016), the intellectual debate between proponents of split and relational positions is very much a part of the landscape of the field of human development. It continues, as well, to play a role in the discussion of how developmental science may be applied to public policies affecting the social context of human life.

Implications of Gottlieb's Ideas for Developmental Methodology and Collaboration

Overton (2015a) explained that the casting of "our fundamental understanding of development into an inclusive relational frame has profound implications for the concepts and theories, as well as the methodology and methods, of developmental inquiry" (p. 114). I would, in addition, reiterate a point made in earlier chapters regarding the notion of plasticity, that is, the presence of relative plasticity in human development supports optimism about the potential efficacy of developmentally appropriate public policies and of preventive and optimizing developmental interventions. As well, the enactment and evaluation of such policies and programs serve as a way of testing or demonstrating an RDS-based perspective (cf. Brim & Kagan, 1980a, 1980b; Gottlieb, 1997, p. 138; Lerner, 1995). Accordingly, to test the limits of this RDS-based perspective for enhancing theory, methodology, research, and application, Gottlieb pointed science in the direction of pursuing methodological reductionism—as opposed to theoretical reductionism (cf. von Bertalanffy, 1933, 1962). Gottlieb (1997) explained that:

> Theoretical reductionism seeks to explain the behavior of the whole organism by reference to its component parts—a derivative of the older, additive physical concept of mechanism—whereas methodological reductionism holds that not only is a description of the various hierarchically organized levels of analysis of the whole organism necessary but also that a depiction of the bidirectional traffic between levels is crucial to a developmental understanding of the individual.
>
> (p. 132)

In short, Gottlieb expressed the hope that "the immense gap between molecular biology and developmental psychology will one day be filled with facts as well as valid concepts" (1997, p. 100).

I believe there is a strong basis for such hope, especially given the work of scientists such as Lester et al. (2016), Meaney (2010, 2014), Moore (2015a, 2016), and Slavich and Cole (2013; Cole, 2014). However, to realize this goal, it is necessary to understand that the major point in casting the study of development into an integrative, relational frame is that it has profound implications for the concepts and theories, as well as for the methodology of developmental inquiry (Overton, 2015a). As such, to understand "how things really work" in the process of development, developmental scientists will require knowledge far beyond that which could be gained from partitioning variance into genetic and environmental components. Scientists will need knowledge about all the levels of organization that comprise the ecology of human development and, as well, and perhaps most critically, about the dynamic system of developmental relations that comprise this ecology.

To obtain such knowledge, developmental scientists must go beyond the limits of any one area of scholarship. Indeed, I believe that developmental scientists will have to go beyond the limits of academe. How things really work in the real world involves people from all walks of life. In the end, then, the perspective of any one scientist or practitioner, or indeed any person, is limited. To create important and sustained social changes through the actions of individuals, communities of scholars in concert with communities of citizens will have to coalesce in order to learn how desired individual, family, and societal changes can be created.

A focus on relationism and on the relational developmental system, not on splits into counterfactual domains of isolated acting entities, is a frame for productive science and for applications that may serve to advance the human condition. Gottlieb's (1997, 2004; Gottlieb et al., 2006) scholarship sounds the "death knell" for the nature–nurture dichotomy. His work provides a scholarly and scientific legitimization of innovative, relational answers to key questions about science and policy. For those scholars or practitioners concerned with using the best of science to inform the policies and programs affecting the quality of human development within and across the nations of the world, Gottlieb's theory and research can serve as a key part of the template through which they may develop or extend other approaches to understanding and enhancing human development. These heuristic contributions of

Gottlieb's ideas (e.g., 1997, 1998; see too Hood et al., 2010) may be underscored when developmental scientists consider integratively all of the levels of the developmental system that Gottlieb believed to be involved in the probabilistic-epigenetic process.

THE MULTILEVEL CONTEXT OF PROBABILISTIC EPIGENESIS

The central idea in probabilistic epigenesis is that changing, reciprocal relations (or dynamic coactions) between individuals and the *multiple* contexts within which they live comprise the essential process of human development (e.g., Gottlieb, 1997, 1998, 2004). These bidirectional relations exist among the multiple levels of organization involved in human life (e.g., biology, psychology, social groups, and culture) (Bronfenbrenner, 1979; Bronfenbrenner & Morris, 2006; Lerner, 2006b), and they provide a framework for the structure of human behavior (Ford & Lerner, 1992). In addition, this system is itself dynamically interactive with historical changes; this temporality provides a change component to human life (Dixon, Lerner, & Hultsch, 1991a, 1991b; Elder, 1998; Elder et al., 2015). In other words, the probabilistic-epigenetic process involves a changing configuration of relationships across the life span (Ford & Lerner, 1992).

This conception of the developmental process emphasizes the probabilistic character of *both* the directions and outcomes of development, and, in so doing, admits of more plasticity in development than do predetermined-epigenetic conceptions (Gottlieb, 1970). As such, this plasticity necessitates a revised formulation of the continuity–discontinuity issue. The plasticity which derives from the probabilistic (yet, causal) integrative coactions among levels makes both continuity and/or discontinuity a probabilistic feature of developmental change across life periods.

Probabilism in continuity and discontinuity is stressed because, to reiterate the ideas of Gottlieb (1970, p. 123), "behavioral development of individuals within a species does not follow an invariant or inevitable course, and, more specifically . . . the sequence or outcome of individual behavioral development is probable (with respect to norms) rather than certain." Of course, it is possible to ask whether all instances of continuity and discontinuity have an equal probability of occurrence. As explained by Thelen and Smith (1998, 2006; see too Ford & Lerner, 1992), this situation is not the case across individuals. Within a relational developmental system, the integration between system-changing and system-constraining relations reduces the degrees of freedom available for change for any person; thus, the potentially infinite instances of change that could exist within a dynamic, open, and living system are reduced through the self- organizing actions of the system (Witherington, 2015; Witherington & Lickliter, 2016).

In short, development occurs because of the integrative coactions within a multilevel context (e.g., Bronfenbrenner & Morris, 2006; Elder et al., 2015; Gottlieb, 1997, 1998). The nature of the changes in this context contributes to the probabilistic character of development. However, developmental scientists also need to appreciate that the organism as much shapes the context as the context shapes the organism, and that—at the same time—both organism and context constrain (or limit) the other (e.g., Lerner, 1982; Lerner & Busch-Rossnagel, 1981b; Lerner & Walls, 1999). The processes that give humans their individuality and plasticity are the same ones that provide their commonality and constancy (Lerner, 1984, 1988).

Although there is some probability that any process or feature of development could show continuity or discontinuity, constraints on change, arising from both organism and context, make some instances of constancy and change more probable than others. This differential probability complicates the study of continuity and discontinuity because it requires not only an indication of "confidence intervals" around particular instances of continuity and discontinuity but also a specification of the likely systemic ordering of such instances.

For example, it is less likely that a large and complex social institution, such as a middle school or junior high school, will alter its overall curriculum or educational policies to accommodate one child's individuality than it is that a single classroom will show such change. Nevertheless, there is some possibility that a particular instance of a child's individuality (e.g., consider a child with a visual impairment) will evoke a general change in the middle school (e.g., the addition of Braille signage). Conversely, it is less likely that the experience of instruction within a single course will alter the lives of an entire cohort of adolescents than it is that the experience of an overall high school curriculum will have that influence. Yet, as the case of East Los Angeles Garfield High School mathematics teacher, Jaime Escalante, illustrates (in the 1988 film *Stand and Deliver*), a single class, or in this case teacher, can indeed alter the educational lives of an entire cohort of students. Thus, although a single child is more likely to influence one classroom than an entire school, and although a cohort of high school students is more likely to be influenced by an entire school curriculum than by a single course, there is, nevertheless, some probability in both cases that the less likely change will occur.

In turn, and as emphasized by Schneirla (1957), within the relational developmental system, the organism has a distinct influence on the multilevel context that is influencing it. That is, the organism is an active contributor to its own development (e.g., Lerner, 1982; Lerner, Theokas, & Jelicic, 2005; Schneirla, 1957). Clearly, then, probabilistic epigenesis involves the fact that, at least among mammals (in contrast to insects), and certainly among humans (Tobach & Schneirla, 1968), development is both biological and social. In fact, no form of life as we know it comes into existence independent of other life. No animal lives in total isolation from others of its species across its entire life span (Tobach, 1981; Tobach & Schneirla, 1968). In other words, Gottlieb's views about probabilistic epigenesis lead to the recognition that the change process in human development involves integrated coactions across the biological through social (and cultural and historical; Elder et al., 2015; Raeff, 2016) levels of organization comprising the ecology of human development (e.g., Bronfenbrenner, 2005). Moreover, Gottlieb (1970, 1992, 1997, 1998, 2004) explained that the probabilistic-epigenetic process of ontogenetic development is related to the emergence in phylogenetic history of this individual↔context process. It is important, then, to consider this link between evolution (phylogeny) and ontogeny—both to explicate the character of probabilistic epigenesis and to illustrate the difference between how RDS-based theories envision the link between ontogeny and phylogeny (e.g., Witherington & Lickliter, 2016) and how this link is approached by proponents of genetic reductionism, for instance, by proponents of evolutionary developmental psychology (e.g., Bjorklund, 2016; Bjorklund & Ellis, 2005).

Probabilistic Epigenesis and Human Evolution

Early humans were relatively defenseless, having neither sharp teeth nor claws. Coupled with the dangers of living in the open African savanna, where much of early human evolution occurred, group living was essential for survival (Masters, 1978; Washburn, 1961). Therefore, human beings were more likely to survive if they acted in concert with the group than if they acted in isolation. Human characteristics that support social relations (e.g., attachment and empathy) may have helped human survival over the course of human evolution (Hoffman, 1978; Hogan, Johnson, & Emler, 1978; Sahlins, 1978).

Biological survival requires meeting the demands of the environment or, as I noted in previous chapters, attaining a goodness-of-fit (Chess & Thomas, 1984, 1999; Lerner & Lerner, 1983, 1989; Thomas & Chess, 1977) with the context. Because this environment is populated by other members of one's species, adjustment to (or fit with) these other organisms is a requirement of survival (Tobach & Schneirla, 1968).

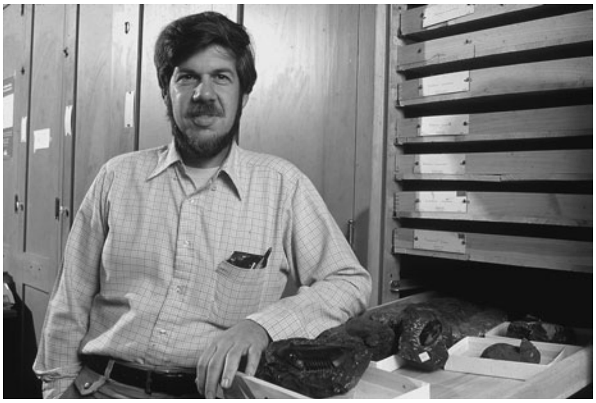
Given this biological contribution to, or, better, in the terms of RDS metatheory, fusion with the social ecology of human development, it is not surprising to learn that several scholars having ideas associated with RDS-based theories believe that human evolution has promoted the link between biological and social functioning (Featherman & Lerner, 1985; Gould, 1977). In other words, the ontogenetic integration of human biological and social levels of organization has been shaped by the evolutionary history of humans.

The scholarship of Stephen J. Gould (e.g., 1977) has provided singular contributions to the understanding of this linkage between ontogeny and phylogeny. A discussion of his ideas allows developmental scientists to understand the relevance of human evolution to individual↔context relations, relations that propel individual development across the life span.

GOULD'S VIEWS OF ONTOGENY AND PHYLOGENY: EVOLUTIONARY BASES OF INDIVIDUAL↔CONTEXT RELATIONS

As evident from the title of his book, *Ontogeny and Phylogeny* (1977), Stephen J. Gould had an abiding interest in detailing the relation between ontogeny and phylogeny. He contends "That some relationship exists . . . Evolutionary changes must be expressed in ontogeny, and phyletic information must, therefore, reside in the development of individuals" (Gould, 1977, p. 2). However, this point in itself is obvious and unenlightening for Gould. What makes the study of the relation between ontogeny and phylogeny interesting and important is that there are "changes in developmental timing that produce parallels between the stages of ontogeny and phylogeny" (Gould, 1977, p. 2).

Discussing the relation between ontogeny and phylogeny may raise the hackles (read: "Haeckels") of many scientists trained in human development. As I discussed in Chapter 3, the recapitulation ideas of Haeckel (e.g., 1868), especially as they were adopted by G. Stanley Hall (1904), have long been in disfavor. To recall this earlier discussion, Haeckel's theory of recapitulation involves the idea that the process of evolution occurred through a change in the timing of developmental events; this change in timing created a universal acceleration of development that pushed ancestral, adult forms into the juvenile stages of descendants. For example, Haeckel (1868) interpreted the gill slits of human embryos as characteristics of ancestral adult fish that had been compressed into the early stages of human ontogeny through this universal process of acceleration of developmental rates in evolving lines.



Stephen J. Gould

It is unfortunate for the scientific study of links between ontogeny and phylogeny that scientists came to regard Haeckel's concept of recapitulation and, even more, the entire topic of the connection between phylogeny and ontogeny as ideas that should not be addressed within evolutionary biology. As Gould (1977, p. 2) explained, "Haeckel's biogenetic law was so extreme, and its collapse so spectacular, that the entire subject became taboo." The absence of scientific attention was problematic because Gould (1977) noted that alternative formulations of the relation between ontogeny and phylogeny could avoid the shortcomings of Haeckel's (1968) formulation. Indeed, Gould (1977) offered such an alternative, one that provided a different conception of the evolutionary basis of individual↔context relations. According to Gould (1977), this alternative is the key to human evolution and to human plasticity. In order to understand this alternative, it is important to introduce three interrelated terms: heterochrony, neoteny, and paedomorphosis.

According to Gould (1977), evolution occurs when ontogeny is altered in one of two ways. First, evolution occurs when new characteristics are introduced, within any period of development, which then have varying influences on later developmental stages. The second way in which evolution occurs is when characteristics that are already present undergo changes in developmental timing. This second means by which phyletic change occurs is termed heterochrony. Specifically, heterochrony is changes in the relative time of appearance and rate of development of characteristics already present in ancestors.

In human evolution, a specific type of heterochrony has been predominant; as a consequence, the changes that were associated with human plasticity occurred. The type of heterochrony that has characterized human evolution is neoteny, which is a slowing down, a retardation, of development of selected somatic organs and parts. Heterochronic changes are regulatory effects; that is, they constitute "a change in rate for features already present" (Gould, 1977, p. 8). Gould (1977) maintained that neoteny has been a—and probably the—major determinant of human evolution.

For example, delayed growth has been found to be important in the evolution of complex and flexible social behavior and, interrelatedly, it has led to an increase in cerebralization by prolonging into later human life the rapid brain-growth characteristics of higher vertebrate fetuses. As such, this general evolutionary retardation of human development has resulted in adaptive features of ancestral juveniles being retained. That is, a key characteristic of human evolution is paedomorphosis, or phylogenetic change involving retention of ancestral juvenile characteristics by the adult. In other words, Gould (1977) noted:

> Our paedomorphic features are a set of adaptations coordinated by their common efficient cause of retarded development. We are not neotenous only because we possess an impressive set of paedomorphic characters; we are neotenous because these characters develop within a matrix of retarded development that coordinates their common appearance in human adults . . . [and these] temporal delays themselves are the most significant feature of human heterochrony.
>
> (pp. 397, 399)

But what are some of the paedomorphic-neotenous characteristics? How do they provide an evolutionary basis of human plasticity and individual↔context relations? Gould (1977) himself answered these questions, and, in so doing, indicated that humans' evolving plasticity both enabled and resulted from their embeddedness in a social and cultural context. Gould (1977) noted:

> In asserting the importance of delayed development . . . I assume that major human adaptations acted synergistically throughout their gradual development . . . *The interacting system of delayed development–upright posture–large brain is such a complex*: delayed development has produced a large brain by prolonging fetal growth rates and has supplied a set of cranial proportions adapted to upright posture. Upright posture freed the hand for tool use and set selection pressures for an expanded brain. A large brain may, itself, entail a longer life span.
>
> (p. 399, italics added)

And:

> Human evolution has emphasized one feature of . . . common primate heritage—delayed development, particularly as expressed in late instruction and extended childhood. This retardation has reacted synergistically with other hallmarks of hominization—with intelligence (by enlarging the brain through prolongation of fetal growth tendencies and by providing a longer period of childhood learning) and with socialization (by cementing family units through increased parental care of slowly developing offspring). It is hard to imagine how the distinctive suite of human characters could have emerged outside the context of delayed development.
>
> (p. 400)

Thus, in linking neoteny with reciprocal relations between brain development and sociocultural functioning, Gould (1977) made an argument of extreme importance for comparative-developmental and sociocultural-intergenerational analyses of human development. The role of the former type of analysis is raised in regard to species differences (heterochrony) in the ontogeny of brain organization and their import for levels of plasticity finally attained across life. In other portions of the evolutionary biology literature and in the anthropology literature, there is support for the link suggested by Gould (1977) between plastic brain development and human sociocultural functioning.

Individual↔Context Relations in Evolution: Paleoanthropological Perspectives

Several ideas in anthropology suggest that humans have evolved to manifest social dependency (e.g., Tobach, 1981; Tobach & Schneirla, 1968). The course and context of evolution was such that it was more adaptive to act in concert with the group than in isolation. For example, Masters (1978) noted that early hominids were hunters. These ancestors evolved from herbivorous primates under the pressure of climatic changes that caused the African forest to be replaced with savanna. Masters speculated that the large brains of humans (1978, p. 98) may be the result of cooperation among early hominids and, hence, in an evolutionary sense, the human brain is a social organ. Indeed, he believed that, with such evolution, the "central problem" in anthropological analysis—that of the origin of society—may be solved. Washburn (1961) appeared to agree. He noted that the relative defenselessness of early humans (lack of fighting teeth, nails, or horns), coupled with the dangers of living on the open African savanna, made group living and cooperation essential for survival (Hogan, Johnson, & Emler, 1978; Washburn, 1961).

There is some dispute in anthropological theory as to whether material culture or specific features of social relations, such as intensified parenting, monogamous pair bonding, nuclear family formation, and, thus, specialized sexual-reproductive behavior, were superordinate in these brain–behavior evolutionary relations. For example, some paleoanthropologists have maintained the idea that there are five characteristics that separate human beings from other hominids: large neocortex, bipedality, reduced anterior dentation with molar dominance, material culture, and unique sexual and reproductive behavior (e.g., of all primates only the human female's sexual behavior is not confined to the middle of her monthly menstrual cycle; Fisher, 1982a). Some paleoanthropologists believe that early human evolution was a direct consequence of brain expansion and material culture. However, Lovejoy (1981), among others (e.g., Johanson & Edey, 1981), believes that:

> Both advanced material culture and the Pleistocene acceleration in brain development are sequelae to an already established hominid character system, which included intensified parenting and social relationships, monogamous pair bonding, specialized sexual-reproductive behavior, and bipedality.
>
> (p. 348)

Other debates also exist. For instance, the roles that continual sexual receptivity and loss of estrus played in the evolution of human pair bonding are controversial and complex (e.g., Belsky, Steinberg, & Draper, 1991; Bjorklund & Shackelford, 1999; Ellis et al., 2012; Fisher, 1982b; Harley, 1982; Isaac, 1982; Swartz, 1982; Washburn, 1982). Such debate, however, exists in the midst of the general consensus indicated earlier: that the social functioning of hominids (be it interpreted as dyadic, familial, or cultural) was reciprocally related to the evolution of the human brain. Many evolutionary biologists appear to reach a similar conclusion.

For example, summarizing a review of literature pertaining to the character of the environment to which organisms adapt, Lewontin and Levins (1978) stressed that reciprocal processes between organism and environment were involved in human evolution; as such, this idea leads to a view that human functioning is one source of its own evolutionary development. Lewontin and Levins (1978) stated that:

> The activity of the organism sets the stage for its own evolution . . . The labor process by which the human ancestors modified natural objects to make them suitable for human use was itself the unique feature of the way of life that directed selection on the hand, larynx, and brain in a positive feedback that transformed the species, its environment, and its mode of interaction with nature.
>
> (p. 78)

Moreover, not only did Lovejoy (1981) and Fisher (1982a) give graphic accounts of the history of the role of hominid social behavior in human evolution, but—in specific support of Gould's (1977) views—they also showed how the complex social and physical facets of this evolution led to human neoteny. Interestingly, whereas Fisher and (especially) Lovejoy viewed the ecological presses that led to the evolution of social behaviors as eventuating in bipedalism and then rapid brain development, they nevertheless both saw these links in more of a circular than a linear framework.

For instance, Lovejoy (1981) noted that it was not just that ecological changes led to social relationships, which in turn led to bipedalism, and, in turn, to brain evolution. Instead, social relationships that led to brain evolution were then themselves altered when larger-brained and more plastic organisms were involved in them; in turn, new social patterns may have extended humans' adaptive presses and opportunities into other arenas, ones fostering further changes in the brain, in social embeddedness, and so forth. Indeed, as Johanson and Edey (1981) described Lovejoy's (1981) position, it is one that requires the examination of the process

> of a complex feedback loop in which several elements interact for mutual reinforcement . . . If parental care is a good thing, it will be selected for by the likelihood that the better mothers will be more apt to bring up children, and thus intensify any genetic tendency that exists in the population toward being better mothers. But increased parental care requires other things along with it. It requires a greater IQ on the part

Owen Lovejoy

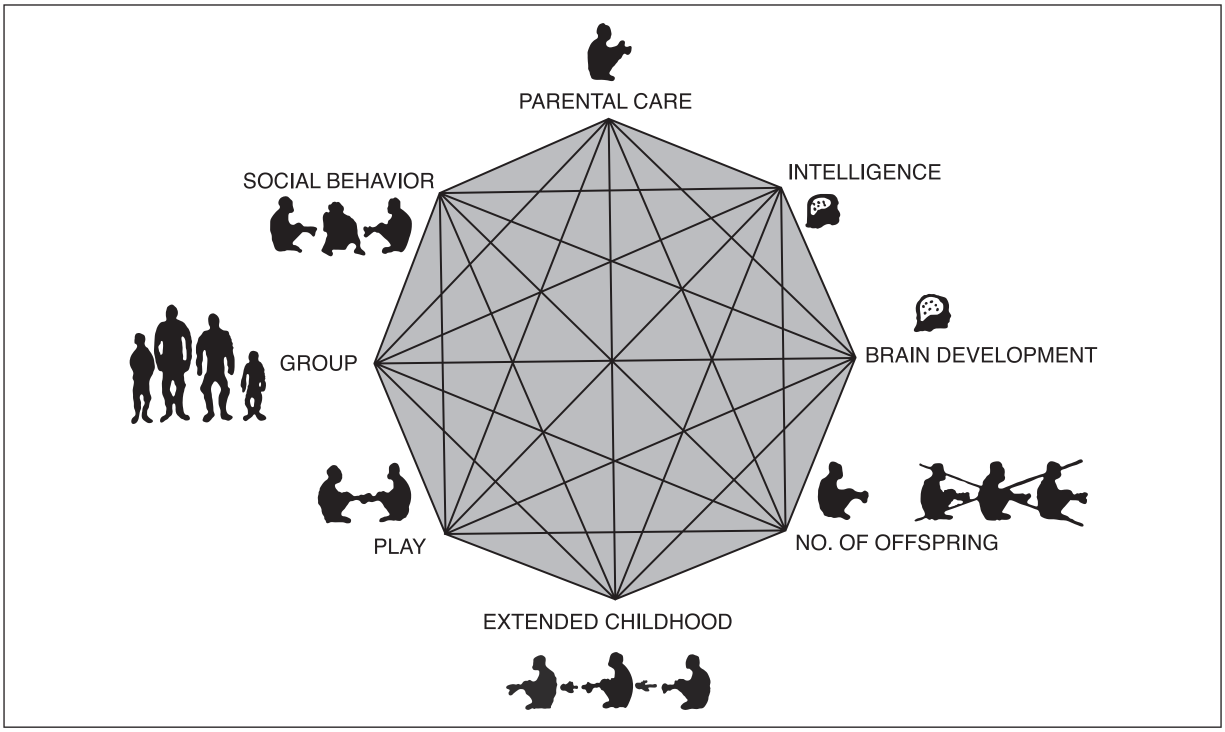


Figure 9.1 Components of the system of reciprocal influences that Lovejoy (1981; Johanson & Edey, 1981) believes was involved in the evolution of human neoteny and social embeddedness.

> of the mother; she cannot increase parental care if she is not intellectually up to it. That means brain development—not only for the mother, but for the infant daughter, too, for someday she will become a mother.
>
> In the case of primate evolution, the feedback is not just a simple A–B stimulus forward and backward between two poles. It is multipoled and circular, with many features to it instead of only two—all of them mutually reinforcing. For example, if an infant is to have a large brain, it must be given time to learn to use that brain before it has to face the world on its own. That means a long childhood. The best way to learn during childhood is to play. That means playmates, which, in turn, means a group social system that provides them. But if one is to function in such a group, one must learn acceptable social behavior. One can learn that properly only if one is intelligent. Therefore, social behavior ends up being linked with IQ (a loop back), with extended childhood (another loop), and finally with the energy investment and the parental care system which provide a brain capable of that IQ, and the entire feedback loop is complete.
>
> All parts of the feedback system are cross-connected. For example: if one is living in a group, the time spent finding food, being aware of predators and finding a mate can all be reduced by the very fact that one is in a group. As a consequence, more time can be spent on parental care (one loop), on play (another) and on social activity (another), all of which enhance intelligence (another) and result ultimately in fewer offspring (still another). The complete loop shows all poles connected to all others.
>
> (pp. 325–326)

An illustration of this "complete loop," or system of reciprocal influence, is presented in Figure 9.1. This figure illustrates that the foundations of humans' plasticity evolved in a complex system of bidirectional relationships among social, ontogenetic, and neuronal variables.

Conclusions

My discussion of the links between Gould's (1977) ideas pertinent to the role of neotenous heterochrony in the evolution of human plasticity has involved as well a discussion of the role of reciprocal relations between organisms and their contexts in human evolution. In other words, neoteny provides adaptive advantages for members of both older and younger generations. Considering children first, the neoteny of the human results in the newborn child being perhaps the most dependent organism found among placental mammalian infants (Gould, 1977). Moreover, their neoteny means that this dependency is extraordinarily prolonged, and this requires intense parental care for the child for several years.

The plasticity of childhood processes, which persists among humans for more than a decade, thus entails a history of necessarily close contact with adults and places an "adaptive premium . . . on learning (as opposed to innate response) . . . unmatched among organisms" (Gould, 1977, p. 401). Gould agrees with de Beer (1959) who stated that for the human:

> Delay in development enabled him to develop a larger and more complex brain, and the prolongation of childhood under conditions of parental care and instruction consequent upon memory-stored and speech-communicated experience, allowed him to benefit from a more efficient apprenticeship for his conditions of life.
>
> (p. 930)

The neoteny of humans, their prolonged childhood dependency on others, and their embeddedness in a social context composed of members of the older generation who both protect them and afford them the opportunity to actualize their potential plasticity allow members of a new birth cohort to adapt to the conditions and presses particular to their historical epoch.

Such development in a new cohort also has evolutionary significance for members of the older cohort. Gould (1977) pointed out that neoteny and the protracted period of dependent childhood may have led to the evolution of features of adult human behavior (e.g., parental behavior). The presence of young and dependent children requires adults to be organized in their adult↔adult and adult↔child relations in order to support and guide the children effectively. Furthermore, because the period of childhood dependency is so long, it is likely that human history tended to involve the appearance of later-born children before earlier-born children achieved full independence (Gould, 1977). Gould (1977, p. 403) saw such an occurrence as facilitating the emergence of pair bonding, and further saw "in delayed development a primary impetus for the origin of the human family."

In sum, several lines of evidence—from human development, evolutionary biology, sociology, and anthropology—converge to suggest that individuals and the other significant people in their lives (for instance, and perhaps most important for human development, children and their parents) engage in mutually influential, bidirectional relations. In so doing, they promote their own and each other's mutual development.

Whereas my discussion in this section has highlighted evolutionary bases of such person–social context reciprocity, there are also ontogenetic, historical, and contemporary contextual features of this relationship. To translate Gould's (1977) ideas about the phylogenetic bases of individual↔context relations into concepts useful in understanding the role of such relations in human ontogenetic changes, it is useful to draw again on the ideas from comparative psychology. In moving from phylogeny to ontogeny, I return to RDS-based ideas about the probabilistic-epigenetic process discussed by Gottlieb (e.g., 1970, 1997, 1998) in his developmental systems model and, as well, to Schneirla's (e.g., 1957) ideas about the plasticity present at the "psychological level" of humans. These ideas afford non-reductionist accounts of human development, ones that (1) differ from genetic reductionist accounts of human development that also draw on evolution and comparisons across the phyletic scale, e.g., evolutionary developmental psychology, discussed in Chapter 12 (see too Lerner, 2016, Lerner & Overton, 2017); and (2) provide sets of ideas pertinent to the RDS-based theories of human development.

One set of such translational ideas was presented by Ford and Lerner (1992). They, in effect, provided "design principles" for the formulation of developmental systems models pertinent to human development. I discuss the ideas of Ford and Lerner and then provide some examples of how their design principles may be translated into models of the development of specific, substantive processes of human development.

FORD AND LERNER (1992) ON DESIGN CRITERIA FOR DEVELOPMENTAL SYSTEMS THEORIES

Ford and Lerner (1992) provided both a general overview of the key design criteria for developmental systems theories and, as well, presented their specific ideas about the instantiation of such theories in regard to human development. Their ideas are consistent with Overton's (2015a) discussion of the process-relational paradigm and of the RDS metatheory derived from it. In addition, Ford and Lerner drew from the ideas of plasticity and circular functions involved in the ideas of Schneirla (e.g., 1957), Gottlieb (e.g., 1970, 1997, 1998), and others (e.g., Greenberg & Tobach, 1984; Gould, 1977; Tobach, 1981) regarding the probabilistic-epigenetic process.

Human Development as a Living System

Ford and Lerner (1992) characterized human development as an open, self-regulating, and self-constructing system. They conceptualized a system as an organization wherein the functioning of each component is at least in part influenced by the collective state of the entire organization. As such, they saw a system as an entity wherein the cooperation of components both preserves the configuration of its structures and functions and, as a consequence of dynamic equilibrium, restores structure and function after perturbations (across fluctuations). They indicated that all biological (living) entities possess such systemic organization (Ford & Lerner, 1992) and that, as well, such systems are open ones (i.e., as explained also by Thelen and Smith, 2006); they "exchange material, energy, and/or information with their contexts" (Ford & Lerner, 1992, p. 95).

Donald Ford

In the "natural world," the Second Law of Thermodynamics specifies that physical systems move in the direction of entropy, that is, of increasingly greater disorganization (Prigogine, 1978, 1980). However, human development systems are negentropic—they move in the direction of increasingly greater organization, in a pattern (structure) consistent with the orthogenetic principle (Brent, 1978; Prigogine, 1978, 1980; Zukav, 1979; see too Raeff, 2011, 2016). As such, and underscoring the negentropic character of open, living systems, Ford and Lerner (1992) noted that such systems "can become larger, more complex, and more elaborate because they can obtain and use additional resources from their contexts and transmit materials and information into their environments. Through such exchanges, moreover, they can alter the context and organization of their contexts" (p. 95).

As a consequence of these characteristics, open, living systems are self-constructing, self-regulating entities that, through their functioning, produce novelty—that is, the qualitative changes that characterize development (Raeff, 2016; Werner, 1948, 1957). A depiction of the self-regulation—or *control system*—involved in open, living systems is presented in Figure 9.2. The figure presents coactions of the system components and processes both among themselves and with the environment within which the system is embedded. The configurations of these exchanges at any point in time are termed *states*, and the figure illustrates that these states vary across time in relation to internal system dynamics and the coactions with the context.

Moreover, Ford and Lerner (1992) explained that several self-organization processes are involved in this control system and, together, these produce a dynamic equilibrium within the system. To illustrate, Ford and Lerner (1992) noted that:

> When a person reaches for an object, his or her mind establishes the goal (directive processes), the nervous system organizes the necessary movement pattern (control processes), and the body carries out the movements (action processes). The eye and muscle senses collect information about the accuracy of the action (information collection processes), the mind compares that with the desired result (regulatory processes), and initiates movement adjustments to ensure the desired result.
>
> (p. 98)

Based on this example, Ford and Lerner (1992) explained that:

> It is not necessary for the control system to anticipate, identify, and measure all the factors that might perturb its successful operation. That would be impossible. As long as the control system can compare current with desired states and make adjustments to reduce the discrepancy it doesn't need to know the nature of the perturbing influences. Because perturbations are frequently occurring and there is a time lag between the perturbation and the corrective action, the system will always oscillate around the exact values toward which it is directed. This kind of stable pattern of variability within boundaries is called

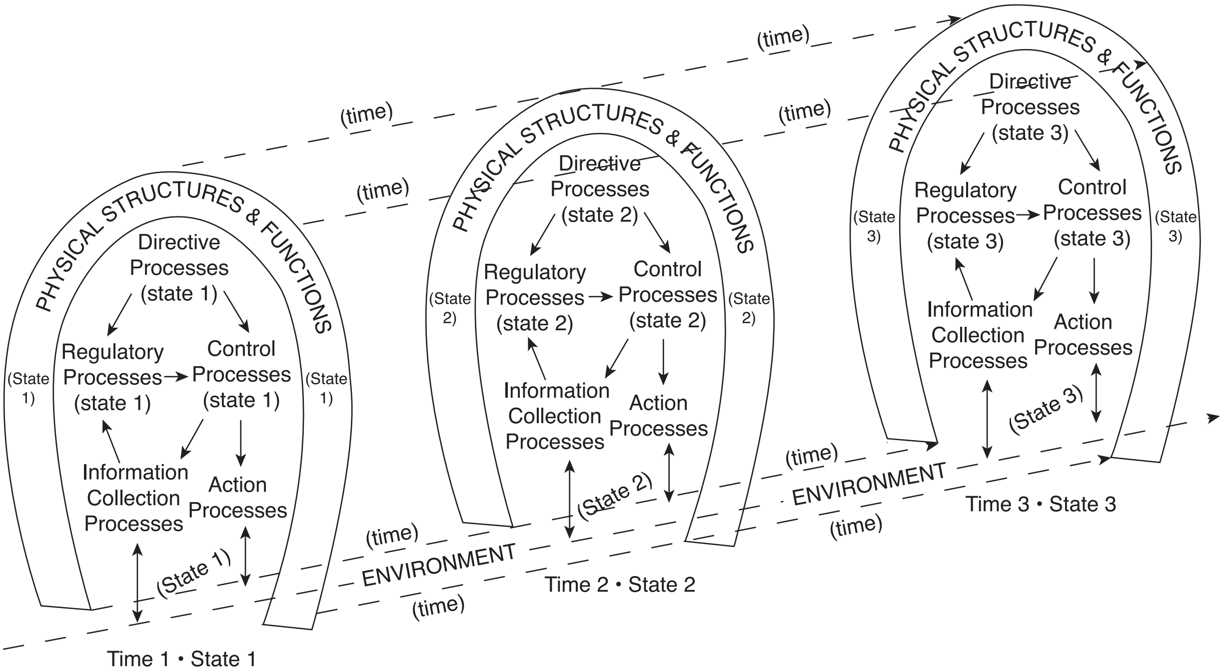


Figure 9.2 A prototypical representation of the dynamic nature of control systems. The component structure and processes are continually interacting among themselves and with their environment. The content of those activity configurations, called states, will vary and change across time as a function of their changing environment and of the internal dynamics of the system.

Source: D. H. Ford and R. M. Lerner (1992). Developmental systems theory: An integrative approach (pp. 88–90). Copyright © 1992 by Sage Publications. Reprinted with permission.

> a *dynamic equilibrium* or *steady state* . . . Thus a person's life is never a steady hum (equilibrium), but, rather, it is more like a symphony (a dynamic equilibrium).
>
> (pp. 98–99)

Moreover, as emphasized by Wapner (1986, 1987; Wapner & Demick, 1998), a person's psychological characteristics—his or her thoughts, feelings, values, goals, and actions with instrumentalities—play an essential part in creating this dynamic equilibrium and, through individual↔context relations involved in it, create a basis for the self-construction of developmental change (novelty) across life. Indeed, the self-constructing abilities of humans are the defining characteristic differentiating living systems from nonliving control systems (e.g., the computers involved in the "automatic pilot" of a modern jet plane). The biological, psychological, and sociocultural characteristics of humans are reciprocally involved in providing humans with their self-regulatory, self-constructing capacities.

Through the fusion of these levels of organization, each level both produces humans' self-constructing capabilities and, in turn, is a product of them. Ford and Lerner (1992) noted that these self-constructing properties of the human development system have been labeled *autopoiesis.* Moreover, and reflecting the ideas about the relational developmental system presented in earlier chapters, they agreed with the view that the living systems of humans "are autonomous, nonlinear, dissipative, active, open, thermodynamic systems that persist, adapt, evolve, reproduce, and construct themselves" (Yates & Iberall, 1973, p. 17).

Design Criteria for RDS-Based Theories

Ford and Lerner's (1992) conception of the living, human development system reflects their commitment to the ideas of dynamism, self-organization, novelty, and the integration of levels of organization that have been hallmarks of the other instances of RDS-based concepts I have discussed. In introducing their specific ideas about RDS-based theories, Ford and Lerner (1992) presented a summary of the guiding assumptions of any RDS-based theory and, then, used the assumptions as a template against which to judge the features of their own formulation. They contended that these assumptions reflected the internal, logical coherence of developmental systems theories and, as such, constituted design criteria for any such theory of development (and, in fact, they argued, for "any adequate theory of development" (Ford & Lerner, 1992, p. 87).

There were six design criteria presented by Ford and Lerner (1992, pp. 88–90). *Criterion 1* stipulated that in such theories variables from the multiple, qualitatively distinct levels of organization involved in human life and development (e.g., Gottlieb, 1998) must operate in organized patterns (e.g., Werner, 1957), and these patterns must result in the coherent functioning of the person in context (which is functioning on which both human life and development depend; e.g., Tobach & Schneirla, 1968). Ford and Lerner explained that this first design criterion necessitated that the individual be understood as a complex, multilevel organization of biological structures plus biological and psychological-behavioral processes embedded and fused in dynamic coaction with multilevel environments (e.g., Gottlieb, 1998; Johanson & Edey, 1981; Overton, 2015a).

Similarly, Ford and Lerner (1992) indicated that *Criterion 2* specified that each individual's functioning and development result from coactions within and between levels of organization of multiple, qualitatively different variables (e.g., Gottlieb, 1970). They explained that these coactions take the form of individual↔context relations, or mutually causal relationships, through which the functioning of and changes in any variable are influenced by the organization of the set of variables in which it is embedded (e.g., Gottlieb, 1998). Ford and Lerner noted that this probabilistic-epigenetic process occurred in what they termed a causal field of variables. They pointed out also that either constancy or change in this process, and the development that derives from this process, derive from the organization or configuration of variables and not from the action of single variables (e.g., genes alone do not account for development; gene↔context relations constitute a causal field). Therefore, human development involves mutually influential (dynamic) change in the organization of variables between and within levels.

Criterion 3 discussed by Ford and Lerner (1992) is about interindividual differences in intraindividual change (e.g., Baltes et al., 1977; Lerner, 2012). It is a criterion about the idiographic, non-ergodic character of human development (e.g., Molenaar & Nesselroade, 2014, 2015; Rose, 2016). Ford and Lerner indicated that every human differs from every other human, for example, in a combined set involving his or her genetic endowment, the contexts within which he or she is embedded during his or her life span, and the integrated coactions between genes and context that are fused in their influence on behavior and development (e.g., Anastasi, 1958; Schneirla, 1956, 1957). The idiographic character of these coactions means that, across the life span, each person has individually distinct developmental trajectories and outcomes (e.g., Rose, 2016).

In turn, Ford and Lerner (1992) noted, as *Criterion 4*, that the probabilistic-epigenetic process of individual↔context relations that characterizes the human psychological level, and the resulting probabilistic character of intraindividual change trajectories, means that relative plasticity always characterizes human development (e.g., Gould, 1977; Hebb, 1949; Johanson & Edey, 1981; Schneirla, 1957). As such, there is no one, ideal life trajectory for any individual (e.g., Rose, 2016). As such, development is multidimensional and multidirectional. That is, the magnitude of plasticity in a human life varies in regard to the specific variables involved in a person's trajectory across ontogeny; some facets of an individual's life path may evidence more (or less) plasticity than other facets. Ford and Lerner explained that the constraining or facilitating conditions of an individual's developmental pathways are influenced by normative and non-normative individual and historical events (e.g., Baltes et al., 2006; Elder et al., 2015) and, as a result, human development is open-ended and probabilistic rather than predetermined and rigidly deterministic (e.g., Gottlieb, 1970, 1998; Overton, 2015a).

Similarly, Ford and Lerner (1992) explained that *Criterion 5* is that, in RDS-based theories, individuals must be understood as influencing their own development and, as such, are agentic, self-organizing, and self-constructing (e.g., Schneirla, 1957; Lerner, 1982; Lerner & Busch-Rossnagel, 1981b; Overton, 2015a). Ford and Lerner explained that individuals process their world in distinct ways; as a consequence, the same contextual circumstances may have different developmental influences from one person to another or for the same individual from one point in time to another. Moreover, the agency of an individual means that he or she has purpose in regard to his or her actions in context; he or she may select to engage with specific contexts and/or specific components of a context at specific times in life. In addition, the circular functions between the individual and the context, that is, individual↔context relations, mean that the person is influencing the nature of the context that is influencing him or her (e.g., Schneirla, 1957). Ford and Lerner noted as well that, in human development, the social context is of special significance (e.g., Tobach, 1981; Tobach & Schneirla, 1968). They explained that individuals influence and elicit changes in others through individual↔individual circular functions.

Finally, Ford and Lerner (1992) noted that *Criterion 6* specifies the adaptive significance of individual↔context relations. They pointed out that a key basis of an individual's behavior is to establish and maintain coherent intraindividual organization (in order to instantiate adaptive, healthy individual, or organismic, functioning) and, as well, to establish and maintain coherent individual↔context relations; the latter serves to enable the person to function effectively as a part of (contributor to) his or her larger context. Here, then, the circular function allows the individual to contribute to a context that is contributing to him or her (Lerner et al., 2015). Accordingly, Ford and Lerner explained that establishing a goodness-of-fit between a person's specific attributes and the features of his or her context (e.g., demands, presses for survival) is an ongoing requirement for healthy development (e.g., Lerner & Lerner, 1983; Chess & Thomas, 1999). Ford and Lerner suggested that the degree to which an individual successfully matches his or her personal characteristics and functioning to the demands and opportunities of his or her contexts is an indicator of competence and healthy behavior and development.

In sum, and not surprisingly, given the consistency between the six design criteria presented by Ford and Lerner (1992) and the ideas associated with the concepts from RDS-based comparative and evolutionary analyses of development I have discussed in this and earlier chapters, the principles forwarded by Ford and Lerner are instantiated in several instances of RDS-based models of development that range across the levels of the funnel presented in Chapter 1 (Figure 1.1). That is, the design principles suggested by Ford and Lerner are reflected in both mid-range theories and variable-focus theories denoted in this figure. In the next chapter, I discuss in some detail several instances of these mid-range RDS-based theories. In the remainder of this chapter, however, I will illustrate some of the range of variable through mid-range theories that reflect RDS-based ideas and that have been and continue to be generated within developmental science. I will include in this discussion descriptions of some of my own theoretical and empirical work. However, the larger purpose of the presentation in this chapter is to illustrate how the Ford and Lerner (1992) design principles may be instantiated in a variety of RDS-based models.

EXAMPLES OF RDS-BASED MODELS

At this writing, there are several examples of variable-focused through mid-range theories in the developmental science literature. The four volumes of the seventh edition of the *Handbook of Child Psychology and Developmental Science* (Lerner, 2015e) and, as well, a *festschrift* volume in honor of Willis F. (Bill) Overton (Dick & Müller, 2017) provide rich and detailed instances of such models.

For instance, several RDS-based models have been used to understand different facets of cognitive development. For example, Marshall (2015) explains the importance of the concept of embodiment (Overton, 2015a) for research pertinent to developmental cognitive neuroscience. Reflecting the first and second design criteria discussed by Ford and Lerner (1992) (i.e., multiple levels of organization, and their coactions, are involved in human development), Marshall notes that embodiment has the potential to reframe the ways in which neuroscience data are considered in relation to other kinds of data and argues that, whereas developmental features of this reframing are currently underspecified, an RDS-based perspective provides a means for this specification. Similarly, Ricco (2017) extends these RDS-related ideas to understand the development of deductive reasoning, Brown and Lamb (2017) propose an integrative developmental and contextual model of memory development, and Pruden and Odean (2017) present an RDS-based model of motion verb and spatial-temporal term acquisition.

In turn, there are also several instances of RDS-based models within the study of personality and social development. For instance, reflecting the design criteria of multiple, coacting systems, and the role of agency in human development (Ford & Lerner, 1992), Liben and Coyle (2017) propose a relational model of gender development and Lerner and Callina (2017) apply concepts drawn from RDS metatheory to propose a model of youth thriving (a model that will be discussed in more detail later in this chapter). A similar consistency with the Ford and Lerner (1992) design criteria is found in the model proposed by Gauvain and Perez (2015), who make the point that cognitive development is not independent of an individual's relations with his or her social and, to them, critically, cultural context. Indeed, they emphasize that cognitive development occurs in cultural context, and they describe the complex and multifaceted connections between culture and cognitive development and how it unfolds over ontogeny. This discussion in framed within sociocultural theory, which draws on RDS-related ideas to depict the mutually influential relations between cognitive development and culture (see too the discussion in Chapter 10 regarding the work of Rogoff, e.g., 2011, and Mistry & Dutta, 2015).

Similarly, Goodnow and Lawrence (2015) note that there have been changes in the conceptual frames and research approaches that consider the development of children in relation to culture, or cultural contexts. They assess the splits and dichotomies that have regarded change as unidirectional and unidimensional, neglecting the intertwining of the personal with the social and the cultural. However, they explain that contemporary research has changed in regard to questions asked, in that emphasis is now placed on the nature of context, the nature of development, and the interconnections of individual development and context. They explain that, in any situation, children experience more than one, often competing, cultural contexts, and more than one way of thinking, feeling, and acting. Accordingly, they propose moving beyond separations, beyond seeing contexts as one-dimensional and individuals as solo, moving instead to analyses of the two as intertwined. Bornstein (2015) also presents a model predicated on the integration of person and context in his conception of parenting. He surveys central issues concerning children's parents and examines the two faces of parenting, parenting as a phase of adult development and parenting as an instrumental activity vis-à-vis children. Consistent with his specificity principle, Bornstein (2017) notes that parents and parenting vary tremendously. He discussed the multi-causal origins of parenting as involving mutually influential relations among characteristics of parents, characteristics of children, and contextual characteristics.

In addition, McLoyd, Purtell, and Hardaway (2015) consider how social class, race, and ethnicity shape the transition to adulthood. Focusing on four domains of functioning (conceptions of adulthood, mental health, paid employment, and educational attainment), they draw heavily on RDS-based models such as life-course theory, developmental contextualism (discussed later in the present chapter), and the concepts of risk, resilience, and social capital to understand these facets of development. Similarly, Murry, Hill, Witherspoon, Berkel, and Bartz (2015) note that children grow up in families with cultural backgrounds and beliefs, and in families with varying financial resources and social capital. They reside in communities and neighborhoods that are homogeneous or diverse, with varying levels of resources and risks. Accordingly, Murry et al. use RDS-related ideas to integrate knowledge on contextual factors for a more holistic understanding of normative development. They examine ways in which race/ethnicity, socioeconomic status, geographic residence, immigration status, and family formation status (e.g., transracial adoption) are integratively associated with both normative (e.g., social emotional well-being, academic aspiration, prosocial friendships) and non-normative (e.g., externalizing behaviors, high-risk behaviors, school dropout) child development.

As well, Masten, Narayan, Silverman, and Osofsky (2015) note that, every year, millions of young people are exposed to the dangers and trauma of disasters, war, terror, and political conflict. Accordingly, they highlight findings from research on children and youth who endure these experiences. They used a risk and resilience framework that is grounded in RDS-based theory. Variations in response to these mass-trauma experiences are discussed in relation to dose-response gradients, developmental cascades and pathways, and moderators of risk at the individual, family, and sociocultural levels. They point to the importance of efforts to integrate knowledge on neurobiological, behavioral, and cultural processes. Similarly, Luthar, Crossman, and Small (2015) also use RDS-related concepts to understand the individual↔context relations involved in resilience, which they conceptualize as involving processes predicting better-than-expected adjustment following exposure to adversity. They note that the literature points to the power of proximal relationships, particularly with primary caregivers in the family, and with teachers, mentors, and peers in the community. Forces inimical to psychological and physical survival (such as maltreatment and violence) are much more powerful than positive influences (such as affection or support). Children's own "protective attributes" such as self-efficacy are themselves continually shaped by their mutually influential engagement with specific socializing influences.

Clearly, then, there are many examples in the developmental science literature of the use of RDS-based ideas pertinent to diverse facets of ontogeny, and these models are consistent with the design criteria discussed by Ford and Lerner (1992). As such, it is of course the case that these models span what has been a traditional "split" in fields of specialization within developmental science, that is, cognitive development, on the one hand, and personality and social development, on the other. As briefly illustrated by my discussion of some of the models presented in the seventh edition of the *Handbook of Child Psychology and Developmental Science* (Lerner, 2015e) and, as well, in a *festschrift* volume in honor of (Bill) Overton (Dick & Müller, 2017), RDS-based models avoid such splits and present a holistic and integrated conception of development. It may be useful, therefore, to highlight two such integrative approaches to human development, ones that might be placed conventionally within the domain of cognitive development or personality development, respectively. However, in actuality, both examples involve scholarship that crosses domains and entail integrative approaches to human development, approaches that integrate coactions both within the individual and within individual↔context relations and, again, fit with the design criteria discussed by Ford and Lerner (1992).

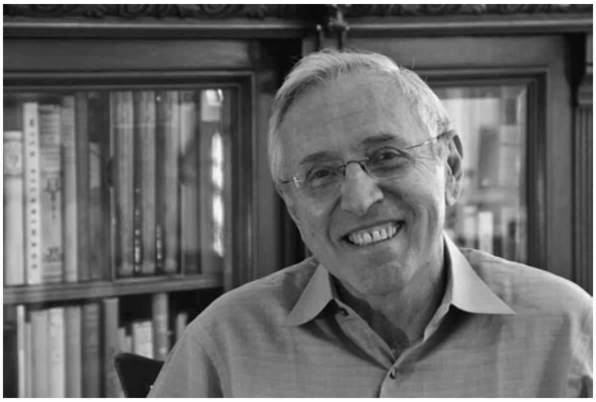
The Scholarship of Turiel and Nucci

Melanie Killen and Judi Smetana (2014, 2015) have explained that morality is a core component of humans' social life and, not surprisingly, then, theories of moral development have been presented for more than a century. Moral development theory is aimed at addressing questions about how individual psychological needs for autonomy and attachment to specific groups and society can be achieved at the same time that the integrity, dignity, and fair treatment of all people are also assured. Accordingly, the simultaneous focus in moral development theories on both the individual and others would seem to be especially suited to an RDS-based conception that integrates individual facets of development (e.g., cognitive development pertinent to moral reasoning) and social functioning (e.g., a person's contributions to a just and equitable world). Elliot Turiel and Larry Nucci (2018) have provided such an RDS-based approach to moral development.

Noting that the theoretical roots of their conception can be found in the work of James Mark Baldwin (e.g., 1897a, 1897b), Jean Piaget (e.g., 1952, 1965), Heinz Werner (e.g., 1948, 1957), and Willis Overton (e.g., 2010, 2015a), Turiel and Nucci (2017) explain that the relational character of their theory lies in the fact that the development of mind and of the individual more generally is a function of "relational bidirectional interactions with complex and multifaceted environments that continue throughout the lifespan . . . [and that] thought and emotions are not independent pieces of a puzzle. Thought and emotions are interdependent parts of a whole" (pp. 95–96).

An additional RDS-based facet of their theory —reflecting the specificity principle (Bornstein, 2017) that I discussed in previous chapters—arises when Turiel and Nucci (2018) explain that "our approach . . . has been labeled social domain theory, because . . . at young ages children begin to form moral judgments that are different in form from other social judgments" (p. 96). In addition, they emphasize that:

> In particular, systems of moral thought differ even in children as young as 4 or 5 years (if not earlier) from systems of thought about the conventions, rules, and roles of authorities in social systems, as well as from systems of thought about arenas of personal choice and jurisdiction. We have theorized about and researched the moral, social-conventional, and personal domains. As distinct domains of thought, these constitute different developmental pathways, each with its own sequence.
>
> (Turiel & Nucci, 2018, p. 96)



Elliot Turiel

Larry Nucci

And they explain that: "Young children differentiate morality from those other types of social and pragmatic judgments—hence the idea of distinct developmental pathways within domains" (Turiel & Nucci, 2018, p. 97).

Elaborating on this idea, Turiel (2015) illustrates RDS-based ideas regarding integrated, holistic functioning by noting that, in childhood, individuals construct configurations of thoughts about topics such as welfare, justice, and rights that are integrated with emotions such as affection, sympathy, and empathy. This integration of thought and emotion is directed to children making moral judgments and, as well, judgments in the personal and social-convention domains. He argues that moral, conventional, and personal judgments are distinct from each other and constitute separate, specific developmental pathways. Similarly, Nucci (2017) explains that social domain theory enhances understanding of the development of the capacity to generate moral decisions within specific contexts. By distinguishing between the development of "judgments about morality (issues of fairness, welfare, and rights) and concepts of societal convention (consensually determined norms of a given social system), and matters that fall within the personal domain of privacy and personal choice" (Nucci, 2017), social domain theory has been a useful frame for developmental research that indicates that each domain follows independent, specific pathways of development and that these different trajectories account for qualitatively differing features of social experience (Nucci 2001; Smetana, Jambon, & Ball, 2014; Turiel, 1983). As such, Nucci (2017) concludes that social contexts have different dimensions, including ones that may pertain to moral issues, issues of social convention, or issues involving personal needs and preferences. Decision-making in such complex settings may necessitate using and, perhaps, coordinating concepts from multiple domains (Smetana et al., 2014).

Given the complexity of the multiple domains involved in specific contexts, Turiel and Nucci (2017) note that it is not surprising that research on the development of moral judgments (e.g., Nucci, 2017; Smetana et al., 2014; Turiel, 2015) has indicated that:

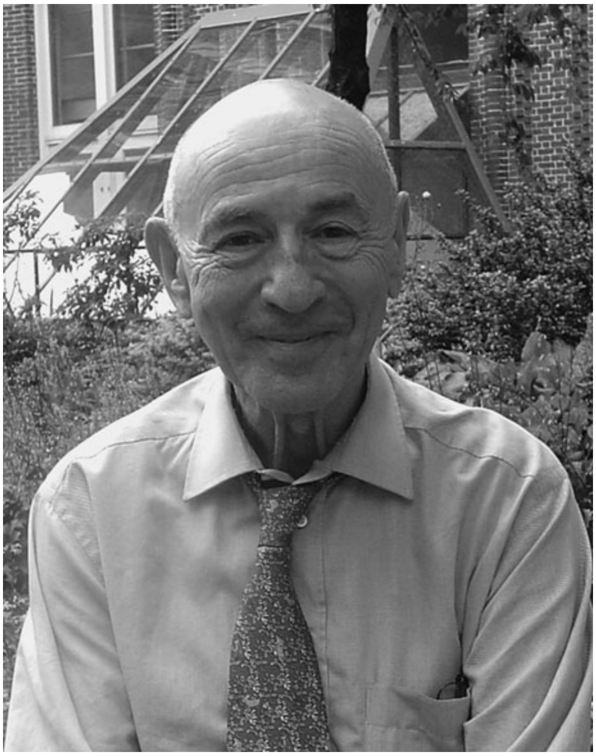
> the observed twists and turns in moral development reflect the ways that individuals attend to varying features of social interactions and their attempts to make sense of the social world. These age-related twists and turns in reasoning about conflicts embedded within the varying contextual aspects of situations should be seen as attempts by the adolescents to come to more comprehensive and adequate solutions to complex problems requiring that they account for multiple considerations.
>
> (pp. 106–107)

Moreover, the presence of the twists and turns they describe as fundamental features of the process of moral development reflects both the specificity of human development described by Bornstein (2017) and, as well, what they note is Overton's (2010) corresponding statement that the relational developmental systems is characterized by diverse pathways within and across the levels of the system.

The Scholarship of Walter Mischel

Concepts such as holism, integration of processes, agency, and individual↔context relations are all key ideas within RDS metatheory. They are also defining features of the approach to personality development taken by Walter Mischel (e.g., 1968, 1973, 2007). Mischel's work countered reductionist and positivist approaches to personality, as epitomized by Five Factor Theory (FFT)—by the postulation of the "Big Five" personality traits of conscientiousness, agreeableness, neuroticism, openness to experience, and extraversion that (as I discussed in prior chapters) are purported to constitute a taxonomy of human personality (Costa & McCrae, 1980; Goldberg, 1993; McCrae et al., 2000). Searching for nomothetic processes, such positivist approaches may lead a researcher "to disregard the possibility that an individual's psychological functioning might vary qualitatively from one context to the next, rather than generalizing broadly across domains" (Cervone, Shoda, & Downey, 2007, p. 4).

Taxonomic approaches to studying people describe observable features of human behavior to generate descriptions of differences between people;



Walter Mischel

the set of these differences is then used as a system, a taxonomy, that is held to be a scientific means to understand individuals (Cervone et al., 2007). As discussed in earlier chapters, FFT is one example of such a taxonomy. The 24 character attributes associated with the Values in Action (VIA) conception of human virtues (Peterson & Seligman, 2004) is another example.

Beginning in the middle decades of the twentieth century and continuing into the twenty-first century, the scholarship of Walter Mischel (e.g., 1968, 1973, 2007) was aimed at building a science of the individual focused on the specific features of a specific person's relations with his or her specific context and, therefore, was fundamentally different from taxonomic approaches that believed the between-person differences pertained to within-individual attributes.

In other words, Mischel rejected taxonomic approaches to personality and, instead, sought to understand specific individual↔context relations across ontogeny because he believed that the study of between-person differences did not elucidate developmental processes underlying the observable behaviors on which taxonomies were predicated (see too Nesselroade & Molenaar, 2010; Molenaar & Nesselroade, 2014, 2015; Rose, 2016). In addition, he rejected taxonomic approaches because he believed that different people might show the same overt behaviors for quite different underlying reasons (Cervone et al., 2007).

As such, Mischel (1973) developed a science of the individual predicated on four key ideas. First, study basic psychological processes, which means that the study of the personality is a holistic enterprise, one involving investigating the integration across life of biological and physiological, cognitive, affective, and social facets of the developmental system. Second, study self-control, or agency, which enables elucidation of the means through which the individual develops the capacity to intentionally regulate his or her emotions, cognitive functions (e.g., attention), and behaviors. Third, focus on idiographic facets of the person. That is, as described by Cervone et al. (2007), the study of personality and its development requires a focus on "the actual, concrete individual . . . the psychological life and social action of the potentially idiosyncratic individual" and not "an abstract, prototypical person whose qualities could be discerned by averaging features displayed by a large sample of individuals" (p. 9; see too Molenaar & Nesselroade, 2014, 2015; Rose, 2016). Finally, Mischel believed that scientists needed to study the individual in context (Cervone et al., 2007).

Mischel (2007) believes that it is important to assess the psychological meaning of a specific context to the individual, that is, how this context is mentally represented by him or her, and how such a representation moderates how he or she functions in the specific context. This focus is necessary to counter what Mischel (2007) regards as a "deeply entrenched traditional explanation of human behavior as due *either* to the internal character and traits of the individual *or* to the external situation in which the individual finds him- or herself, conceptualizing each as a mutually exclusive, independent cause" (p. 265). Accordingly, Mischel's theory and research provide a constructivist and dynamically coactionist conception of the individual, one that necessitates assessments of the meaning a specific person gives to a specific situation and how this meaning moderates his or her specific behaviors in the situation. He emphasizes that "humans could not have evolved to behave consistently across situations that vary in the challenges they pose and the solutions they require" (Mischel, 2007, p. 266).

In sum, Mischel's (2007) approach to personality development creates a cognitive-affective-personality system (CAPS) model of individual↔context relations. Mischel notes that this model is fundamentally idiographic, assessing how a specific individual makes sense of a specific setting and then acts within it to try to function effectively. However, although emphasizing that the study of personality development must start idiographically, Mischel (2007) notes, as do other idiographic-oriented developmental scientists (e.g., Molenaar & Nesselroade, 2014, 2015; Rose, 2016), that an initial idiographic analysis of the person "is not limited to $N = 1$. It lends itself easily to the nomothetic study of types of people who share common *if . . . then . . .* behavioral signatures and similar underlying processing dynamics generated by similarities in the CAPS networks" (p. 271). As will be discussed again in Chapter 13, this approach to idiographic data involves an "assess and then aggregate" strategy, one wherein researchers first assess individual trajectories and then, if empirically appropriate, aggregate to subgroups or to the nomothetic level (e.g., Nesselroade & Molenaar, 2010; Molenaar & Nesselroade, 2014, 2015).

Conclusions

At this writing, models framed by RDS metatheory provide integrative, individual↔context approaches to all facets of the individual and the ecology of human development (e.g., Bronfenbrenner & Morris, 2006). These models involve the concepts of holism and dynamic relations and illustrate that integrated, coacting relations within and across levels of organization constitute the basis of probabilistic-epigenetic change. The models indicate also the role in development of the agentic individual. This individual is engaged in mutually influential and, in the case of healthy or positive development, mutually beneficial relations with his or her context. Thus, these models are consistent with the design criteria specified by Ford and Lerner (1992).

Although the examples of RDS-based models may suffice to indicate the consistency in these conceptions with RDS metatheoretical ideas and, as well, the design criteria for RDS-based models that Ford and Lerner (1992) presented, I offer one additional example that is drawn from my own scholarship over the past 40-plus years. This example is offered in order to illustrate the way in which a primary interest in RDS metatheoretical ideas, and specifically the concept of probabilistic epigenesis, may be translated into successive lines of empirical research about individual↔context relations pertinent to adaptive, healthy development among diverse individuals.

THE SAMPLE CASE OF DEVELOPMENTAL CONTEXTUALISM

Beginning in the late 1970s (e.g., Lerner, 1979; Lerner & Spanier, 1978a, 1978b) and continuing through the mid-portion of the first decade of the twenty-first century (e.g., Lerner, 2004; see too Lerner, Johnson, & Buckingham, 2015a), I tried to develop a model of how, through the probabilistic-epigenetic process described by Gottlieb (1970), and earlier by Schneirla (1957) in regard to circular functions in ontogeny, children and adolescents could act as producers of their own development (Lerner, 1982; Lerner & Busch-Rossnagel, 1981a, 1981b). As a means to avoid confusion between the ontogenetic process I was trying to illuminate and the comparative/phyletic process discussed by Gottlieb and Schneirla, I termed the model of individual↔context relations I was developing, in the main, between children and adolescents and their parents, as *developmental contextualism* (e.g., Lerner, 1979). This label served as well to enable me to integrate my attempts to reconcile ideas from organicism and contextualism with the ideas of Overton (e.g., 1984), which I regarded then—and now—as the most thoughtful, erudite, and useful conceptualization of a means to put together ideas from these two world hypotheses in the service of creating a new approach to understanding developmental systems in a non-reductionist and non-teleological manner.

At the time that I undertook this integration, children had come to be understood as active producers of their own development (Bell, 1968; Lewis & Rosenblum, 1974; Thomas et al., 1963). These contributions of individual young people to their own development were beginning to be understood as primarily occurring through the reciprocal relations individuals had with other significant people in their context (e.g., with family members, teachers, and peers). The question that concerned me was: "Through what specific instances of, say, child↔parent relations might this agentic process function?"

There is an old adage that says that the child is father to the man. This saying means, simply, that a person's characteristics when he or she is a child relate to his or her characteristics during adulthood. However, there is another way of interpreting this saying, and this alternative interpretation framed the work on child↔parent relations I pursued.

The alternative idea is that how people behave and think as adults—and perhaps especially as parents—is very much influenced by their *experiences with their children*. Children as much rear adults as adults rear them. The very fact that parents are parents makes them different adults than they would be if they were childless. But, more importantly, the specific, and often special, characteristics of a particular child influence parents in unique ways. How parents behave toward their children depends quite a lot on how their children have influenced them to behave. Such child influences are termed *child effects*.

By influencing the parents that are influencing him or her, the child is shaping a source of his or her own development. In this sense, children are producers of their own development (Lerner, 1982; Lerner & Busch-Rossnagel, 1981b; Lerner & Walls, 1999), and the presence of such child effects constitutes the basis of bidirectional relations between parents and children. Of course, this bidirectional relation continues when the child is an adolescent and an adult. Corresponding relations exist between the person and siblings, friends, teachers, and all other significant people in his or her life. This child↔other person relationship is the basic feature of the developmental-contextual process that I hypothesized characterized human development (cf. Bronfenbrenner, 2005).

Consistent with Schneirla's depiction of the basis of circular functions in ontogeny, I reasoned that child effects emerge largely as a consequence of a child's individual distinctiveness. All children, with the possible exception of genetically identical, monozygotic (MZ) twins, have a unique genotype. Similarly, no two children, including MZ twins, experience precisely the same environment across their lives. However, all human characteristics, be they behavioral or physical, can be thought of as arising from an integration (a fusion) of genes and environment (Anastasi, 1958; Lerner, 1986; Magnusson, 1996, 1999a, 1999b; Magnusson & Stattin, 2006). Given the uniqueness of each child's genetic inheritance and environment, the distinctiveness of each child is assured (Feldman & Lewontin, 1975; Hirsch, 1970). In other words, every child is unique and, therefore, individually distinct from every other child.

To return to the example of temperament discussed in Chapter 7, all children eat and sleep but, because of variability in their respective temperaments, their style of eating or sleeping may differ (Thomas et al., 1963). If the child eats the same amount at every meal and/or gets hungry at the same time, then this child has, in regard to eating, a regular, or rhythmic, temperament. A child who gets hungry at different times of the day, or who may eat a lot or a little without any seeming predictability, has, in regard to eating, an arrhythmic temperament. Similarly, all children sleep. However, some children may sleep irregularly, that is, for seemingly unpredictable (at least to their parents) lengths of time, with periods of sleep being interspersed with wakeful periods of crying and fussing.

For example, let Child "A" have these attributes. That is, he or she is an arrhythmic eater and sleeper. Another child might sleep and eat in a more regularly patterned way, and/or when awake they may show more smiling than crying and fussing. Let this be Child "B." The importance of these individual differences arises when it is recognized that, as a consequence of their individuality, children will present different stimulation to parents. Child A and Child B stimulate their parents differently as a consequence of their respective eating and sleep–wake patterns. The experience for a parent having a pleasant, regularly sleeping child, who is also predictable in regard to eating habits, is quite different from the experience for a parent who has a moody, irregularly sleeping and eating child (Thomas et al., 1968).

However, the effect of the child's stimulation of the parent depends, in part, on the parent's own characteristics of individuality. To explain this point, I drew on the then rapidly burgeoning literature associated with the life-span developmental perspective (e.g., Baltes, 1968, 1987, 1997; Baltes et al., 1998, 1999, 2006; Nesselroade, 1970, 1977; Schaie, 1965, 1970), in psychology, and the related life-course view of the human life cycle developed by Brim (e.g., Brim, 1966; Brim & Kagan, 1980a, 1980b), Riley (1976, 1979), Featherman (1980, 1983), and Elder (1975, 1979, 1980, 1998; Elder et al., 2015) in sociology, and by other scholars in these and other disciplines (e.g., see Hetherington, Lerner, & Perlmutter, 1988; Magnusson, 1996; Sorensen, Weinert, & Sherrod, 1986). A finding in this literature that was of particular importance to the model of child↔parent relations I sought to develop was that development occurs in more than the childhood or adolescent years (Baltes, 1968, 1987; Block, 1971; Brim & Kagan, 1980a, 1980b; Elder, 1974, 1980; Featherman, 1983; Riley, 1979; Schaie, 1965). It extends across ontogeny.

The import of this point for the development of the model is that parents as well as children develop as distinct individuals across life (Lerner & Spanier, 1978a, 1978b). For instance, parents develop both as adults in general and, more specifically, in their familial (e.g., spousal) and extrafamilial (e.g., vocational or career) roles (Vondracek, Lerner, & Schulenberg, 1986). Indeed, the influence of a child on his or her parents will depend in part on the prior experience the adult has had with the parental role and on the other roles in which the parent is engaged (e.g., worker, adult child, and caregiver for an aged parent) (Hetherington & Baltes, 1988).

Thus, a person's unique history of experiences and roles, as well as his or her unique biological (e.g., genetic and epigenetic) characteristics, combine to make him or her unique—and with time, given the accumulation of the influences of distinct roles and experiences, increasingly more unique across the course of life (Lerner, 1988; Rose, 2016). This uniqueness is the basis of the specific feedback a parent gives to his or her individual child.

Parents who are stimulated differentially may be expected to differentially react to, or process (e.g., think and feel about), the stimulation provided by their child. Child A might evoke feelings of frustration and exasperation, and thoughts of concern in his or her parents (Brazelton, Koslowski, & Main, 1974; Lewis & Rosenblum, 1974). In addition, especially among first-time parents, it is possible that parents might wonder if they will have the personal and marital resources to handle such a child (Chess & Thomas, 1984). The thoughts and feelings evoked in parents by Child B might be markedly different. Certainly, the parents of Child B would be better rested than Child A's parents. When their child was awake, they would have a child with a more regularly positive mood, and this child would also present less stress on them as parents and as spouses (Chess & Thomas, 1999).

The individuality of these parental reactions underscores the idea that parents are as individually distinct as are their children. Not all parents of an irregularly eating and sleeping, moody child will react with concern and/or frustration (Korn, Chess, & Fernandez, 1978). Similarly, some parents will be stressed by even the most regular, predictable, and positive of children. Such parental individuality makes child effects more complicated to study. However, at the same time, parental individuality underscores the uniqueness of each child's context. Simply, then, it may be expected that, as a consequence of the different stimulation received from their children, and in relation to their own characteristics of individuality, parents will provide differential feedback to their children.

Such differential feedback may take the form of different behavior shown to children by parents and/or of different emotional climates created in the home (Brazelton et al., 1974). For instance, the parents of Child A might take steps to alter his or her eating and sleep–wake patterns. In regard to sleeping, they might try to cut naps short during the day so that the child will be more tired in the evening. In addition, during the time when they are appraising the success of their attempts to put the child on an imposed schedule, a general sense of tenseness might pervade the household. They might wonder: "Will we have another sleepless night?" or "Will we be too tired to be fully effective at work?"

In essence, there may be differential feedback by the parents of Child A and Child B. This feedback becomes an important part of the child's experience, and it is distinct in that it is based on the effect of the child's individuality on the parent. Thus, through circular functions, the feedback serves to further promote the child's individuality (Schneirla, 1957). These circular functions underscore the point that children (and adolescents, and adults) are producers of their own development and that people's relations to their contexts involve bidirectional exchanges (Lerner, 1982; Lerner & Busch-Rossnagel, 1981b). The parent shapes the child, but part of what determines the way in which the parent does this shaping is the child himself or herself.

Characteristics of behavioral or personality individuality allow the child to contribute to this circular function. Bornstein's (1995b, 2006, 2017) "specificity principle" underscores the individuality of child↔parent relations that are at the core of these circular functions. Writing in regard to infancy, Bornstein (1995b) noted that "The specificity principle states that specific experiences at specific times exert specific effects over specific aspects of infant growth in specific ways" (p. 21). In turn, the import of the specificity principle for the positive, adaptive, or healthy behavior and development of the child↔parent relation is moderated by the goodness-of-fit design principle discussed by Ford and Lerner (1992). That is, just as a child brings his or her characteristics of individuality to a particular social setting, there are demands placed on the child by virtue of the social and physical components of the setting. These demands may take the form of:

1. Attitudes, values, or stereotypes that are held by others in the context regarding the person's attributes (either his or her physical or behavioral characteristics).
2. The attributes (usually behavioral) of others in the context with whom the child must coordinate, or fit, his or her attributes (also, in this case, usually behavioral) for adaptive coactions to exist.
3. The physical characteristics of a setting (e.g., the presence or absence of access ramps for the motorically disabled) require the child to possess specific attributes (again, usually behavioral abilities) for the most efficient coaction within the setting to occur.

The child's individuality, in differentially meeting these demands, provides a basis for the specific feedback he or she gets from the socializing environment (Chess & Thomas, 1999). For example, considering the demand "domain" of attitudes, values, or stereotypes, teachers and parents may have relatively individual and distinct expectations about behaviors desired of their students and children, respectively. Teachers may want students who show little distractibility, but parents might desire their children to be moderately distractible, for example, when they require their children to move from playing to dinner or to bed. Children whose behavioral individuality was either generally distractible or generally not distractible would, thus, differentially meet the demands of these two contexts. Problems of adjustment to school or to home might, thus, develop as a consequence of a child's lack of match (or of goodness-of-fit) in either or both settings (e.g., Eccles, 1991; Eccles & Harold, 1996; Eccles, Lord, & Buchanan, 1996; Eccles & Midgley, 1989; Eccles, Midgley, Wigfield, Buchanan, Reuman, Flanagan, & MacIver, 1993; Fuligni, Eccles, & Barber, 1995; Midgley, Feldlaufer, & Eccles, 1989a, 1989b).

Thomas and Chess (1977, 1980, 1981; Chess & Thomas, 1984, 1999) and Lerner and Lerner (1983, 1989) forwarded ideas and conducted research about the goodness-of-fit model of child↔parent relations. Thomas and Chess and Lerner and Lerner found that, if a child's characteristics of individuality provided a goodness-of-fit (or match) with the demands of a particular setting, adaptive outcomes would accrue in that setting. Those children whose characteristics matched most of the settings within which they existed received supportive or positive feedback from the contexts and showed evidence of the most adaptive behavioral development. In turn, of course, poorly fit, or mismatched, children, those whose characteristics were incongruent with one or most settings, appeared to show alternative developmental outcomes (Lerner & Lerner, 1983, 1989).

The goodness-of-fit concept illustrates that bidirectional relationships exist between the child and the parent (Bornstein, 2015; Bornstein & Tamis-LeMonda, 1990; Lerner & Lerner, 1987). These relationships are reciprocally related to the other social networks within which the dyad exists and to the broader societal and cultural context (e.g., Bronfenbrenner, 1979, 2005; Bronfenbrenner & Morris, 2006).

For instance, both the parent and the child are embedded in a broader social network, and each person has reciprocal reactions with this network (Antonucci & Jackson, 1990). This set of relations occurs because both the child and the parent are much more than just people playing only one role in life. The child may also be a sibling, a peer, and a student; the parent may also be a spouse, a worker, and an adult child. All of these networks of relations are embedded within a specific community, society, and culture. Finally, all of these relations are continually changing across time, across history. Simply, for all portions of the system of child↔parent relations envisioned in developmental contextualism (or in the bioecological model of Bronfenbrenner, 1995; Bronfenbrenner & Morris, 2006), change is an integral, and, indeed, inescapable, feature of human life.

Thus, a child effect on the child↔parent relation (Lewis & Rosenblum, 1974) may function, in a sense, like a small pebble thrown into a quiet lake. It can prompt a large ripple. In turn, of course, the reverse of this possibility can occur. Events in settings lying far beyond the child↔parent relation can influence it. For instance, the resources in a community that support family togetherness, the laws or social programs available in a society supporting maternity and paternity leave, and the cultural values regarding families who redirect their time and energy away from professional commitments in order to care for their infants and young children, all exert an impact on the quality of the child↔parent relation (Lerner & Benson, 2003).

Time—history—cuts through all levels of the system. As with the people populating these levels of the ecology of human development, change is always occurring. Diversity within time is created as change across time (across history) introduces variation into all the levels of organization involved in the relational developmental system. This role of time is captured as well in Bronfenbrenner's (2005; Bronfenbrenner & Morris, 2006) notion of the chronosystem and, as well, in the life-course theory of Elder (1998; Elder et al., 2015) in regard to the role of time and place in linking lives across the life span (see Chapter 10).

In other words, people develop, the family changes from one having infants and young children, to one having teenagers, to an "empty nest": The children have left the home of their parents to live elsewhere and very likely to start their own families. Similarly, communities, societies, and cultures also change (Elder et al., 2015; Garbarino, 1992; Hernandez, 1993). In addition, each of these multiple "levels" is embedded in the natural and human-designed physical ecology, a physical world that, of course, also changes. Changes at one or more of these levels produce changes in the other levels as well, given their bidirectional connections.

Finally, since history "cuts through" all levels of organization (Baltes, 1987, 1997; Elder et al., 2015), the character of child↔parent relations, of family life and development, and of societal and cultural influences on the child↔parent↔family system are influenced by both "normative" and "non-normative" historical changes (Baltes, 1987; see too Chapter 10) or, in other words, by "evolutionary" (i.e., gradual) and "revolutionary" (i.e., abrupt) (Werner, 1957), historical changes. In sum, this system of multiple, interconnected, or "fused" (Tobach & Greenberg, 1984) levels constitutes the integrated organization involved in this developmental contextual view of the development of the child↔parent relation (Lerner, 1986, 1991, 1996, 2006b).

From Developmental Contextualism to Positive Youth Development

Developmental contextualism (e.g., Lerner, 1979, 2004) was an explicit attempt to translate into an ontogenetic model the comparative psychology idea of circular functions provided by Schneirla (e.g., 1957) and his and Gottlieb's (e.g., 1970, 1997, 1998) conception of the probabilistic-epigenetic process; this process occurs as the bidirectional and mutually influential individual↔context relations involved in circular functions are enacted across time and place. The relational developmental system that is both a product and a producer of the probabilistic-epigenetic process engages all levels of organization, and all variables within and across levels; as such, embeddedness within the integrated levels comprising this system provides the relative plasticity of probabilistic epigenesis and, therefore as well, child↔parent relations.

As such, although the developmental-contextual model was predicated on an interest in focusing on one specific construct—child↔parent relations—this interest, because it was embedded within an RDS-based approach to human development, inevitably involved a focus on connections (e.g., the goodness-of-fit) between these relations and the other facets of the system embodying these relations. Young people developed through individual↔context relations in multiple settings, and therefore to fully assess the empirical usefulness of an RDS-based model, such as developmental contextualism, I needed to extend my research to other key settings in the ecology of youth development. Indeed, such extension reflects the design criteria discussed by Ford and Lerner (1992) in regard to studying multiple, coacting systems, youth agency, and the potential for adaptive individual↔context relations across multiple settings. Accordingly, I turned my empirical lens on community-based youth development programs and attempted to understand the bases of positive, healthy development of youth in these settings.

Positive Youth Development

Parents, youth development practitioners, and policy-makers in all nations have a vested interest in the positive and healthy development of their youth (e.g., Petersen, Verma, Kohler, & Motti-Stefanidi, 2017; Schoon & Bynner, 2017; Smith, Petersen, & Leman, 2017; USAID, 2013). Similarly, all developmental scientists are interested in promoting (optimizing the chances of) positive development. Developmental scientists are not in the business of finding ways to harm young people! For instance, since the time that adolescence was launched as a scientific field (Hall, 1904), researchers have been interested in discovering the *processes* involved in adolescents' healthy, adaptive, or positive development. However, both across and within nations, there is variation in definitions of positive youth development and in the ideas framing efforts to promote such development; these differences in perspectives exist as well among parents, practitioners, policy-makers, and developmental scientists (e.g., Koller, Motti-Stefanidi, Petersen, & Verma, 2017; Lerner, 2017; Smith et al., 2017; Petersen et al., 2017).

For much of the twentieth century, the scientific study of adolescence was conducted in the context of a deficit-oriented perspective initiated by Hall's (1904) conception that storm and stress define the adolescent period. The deficit model, encapsulated by the belief that youth were problems to be managed (Roth & Brooks-Gunn, 2003), was often shared by the people raising youth, seeking to provide programs outside the family that might nurture them, and formulating or enacting social policies that might support youth development (e.g., their health, education, moral or spiritual growth, or citizenship).

As such, many developmental scientists and members of these other sectors viewed youth through a lens of their being at risk, beset by problems, and endangered by the inevitable, biologically-based shortcomings that were alleged to cause their storm and stress (e.g., Anthony, 1969; Freud, 1969). From this perspective, promoting positive development in youth was a matter of making youth less problematic. Being a good adolescent, therefore, was in effect defined by what youth did not do (e.g., in regard to smoking, drinking, substance use, unsafe sex, or bullying). Into the 1990s, this deficit perspective was the predominant lens for the study of adolescents, despite more than 30 years of countervailing research findings (e.g., Bandura, 1964; Block, 1971; Douvan & Adelson, 1966; Offer, 1969).

In short, whereas there has always been an interest among developmental scientists in furthering positive behavior and development among youth, until the 1990s this interest was largely instantiated through scholarship that sought to either prevent or ameliorate problems. However, in the 1990s a new lens for viewing adolescent behavior and development emerged, spurred by the convergence of the work of youth program professionals, such as Rick Little, of the International Youth Foundation, and Donald Floyd, of the (U.S.-based) National 4-H Council. Practitioner beliefs in the strengths of youth and their potential for positive development had a fortunate convergence with contemporaneous theory and research in developmental biology and

Rick Little

Donald Floyd

developmental science regarding the relative plasticity of human development across the life span (Lerner, 1984; Lerner et al., 2015; Woese, 2004)—that is, the potential for systematic change in the features and trajectories of youth development.

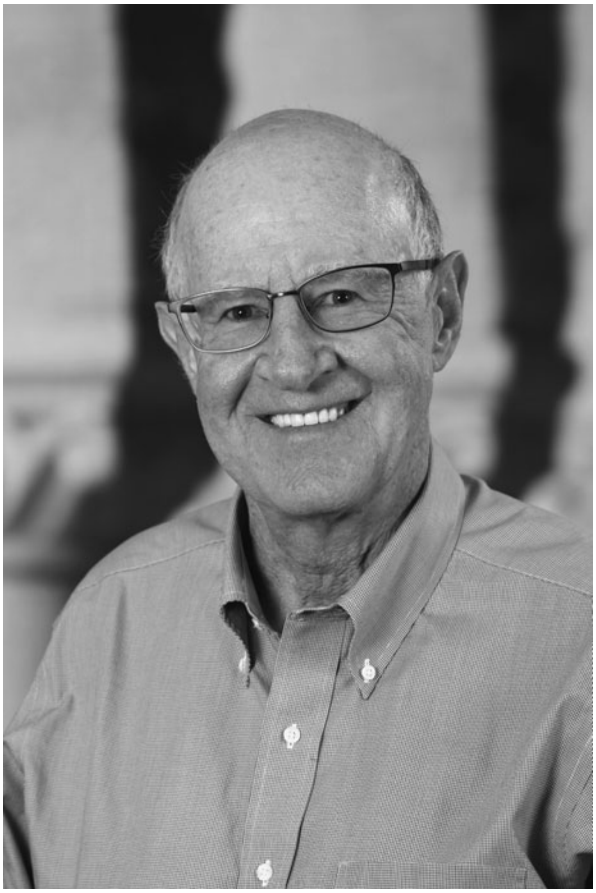
Thus, in the context of a 100-plus-year history of interest in understanding the process of optimizing positive development in adolescence (which was typically approached by trying to make youth less problematic), a new vision of adolescent development emerged, one specifically directed to capitalizing on human plasticity and testing a strength-based model (in contrast to a deficit one) of adolescence. The essence of this conception was that the *fundamental process* of human development involves the RDS-based idea that mutually influential relations between the individual and the other facets of the context within which he or she is embedded (Lerner et al., 2015) provide the basis of human development. Therefore, individuals are part of an integrated developmental system and, through the coactions within this system, all young people have strengths. Relative plasticity is an exemplar of such strengths. Relative plasticity in human development means that the attributes of behavior and development present at one point in life are malleable; as such, if young people could be placed into positive-development-promoting circumstances, then, through their coactions within the relational developmental system, they would have the capacity to change these attributes.

In other words, systematically linking the strengths of youth with resources in their contexts (termed "ecological assets"; Benson, 2008) could enhance the presence of desirable attributes, and not just decrease the presence of problematic or unhealthy ones. This conception—of the potential for positive youth development (PYD) through the *promotion* of desirable attributes of young people—sought to identify individual↔context relational processes that could enhance those attributes of young people that were valued by them and others (e.g., parents, peers, teachers, mentors, coaches, faith leaders, and other community members), as compared to processes that reduced or prevented undesirable characteristics (Damon, 2004; Larson, 2000; Roth & Brooks-Gunn, 2003).

Across the early years of the twenty-first century, and continuing through this writing, several different models of this *developmental process* believed to be involved in PYD were used to frame descriptive, explanatory, or intervention/optimization research across childhood and adolescence (e.g., Benson et al., 2011; Catalano et al., 2002, 2012; Damon, 2008; Eccles, 2004; Eccles & Gootman, 2002; Flay, 2002; Larson, 2000; Lerner et al., 2005, 2015; Masten, 2001, 2014; Spencer, 2006; Spencer et al., 2015). Despite this variation in substantive foci, all of these models reflect ideas associated with RDS metatheory (Overton, 2015a). Although I will emphasize in this discussion of PYD the model that I have developed in collaboration with Jacqueline V. Lerner over the course of the past 20 years (at this writing) (e.g., Lerner et al., 2005, 2015), it is important to illustrate the common commitment to RDS-based concepts present in the above-noted instances of PYD models. Accordingly, before turning to the Lerner and Lerner model, I will discuss three other very prominent and very important models.

William Damon and the Study of Purpose

Damon (2008) and colleagues (Bundick, Yeager, King, & Damon, 2010; Damon, Menon, & Bronk, 2003; Mariano & Damon, 2008) approach the study of the PYD process through an examination of the development of purpose in youth. Damon notes that a central indicator of PYD and youth thriving is engagement in pursuits that serve the common welfare and make meaningful contributions to communities. Damon assesses the ways in which youth go beyond their own self-centered needs and extend outward to the pursuit of goals that benefit the world.



William Damon

To Damon (2008), a purpose is a stable and generalized intention to accomplish something that is at once meaningful to the self and is of intended consequence to the world beyond the self. It is an "ultimate concern" or overall goal for one's life, helping to organize one's life decisions and actions, and is thus manifested in one's behavior. The purpose is internalized, or "owned" by the individual, and therefore is central to his or her identity. As such, Damon (2008, pp. 33–34) points out that:

> The person must have all elements of the definition: something to accomplish, a beyond-the-self rationale, plans for future action, meaningfulness to self, and incorporation into one's identity (that is, behavior that is not driven by oughts); The concern must function to organize the person's decisions and activities in support of the concern; The person must manifest the concern with visible action; and The person cannot imagine himself/herself without the concern, it is necessary to do the activities related to the concern.

In their program of research at the Stanford Center on Adolescence, Damon and his colleagues (e.g., Damon et al., 2003; Mariano & Damon, 2008) examined youth purpose through a series of studies with youth across the United States. To understand adolescents' potential sources of purpose, they surveyed a diverse group of youth from sixth, ninth, and twelfth grades and from college, and asked respondents to indicate their level of dedication to 18 categories of purpose. A "category" refers to a life area that individuals find important, and in which they may be psychologically and actively invested. The categories included: family, country, personal growth, sports, academic achievement, good health, looking good, arts, making lots of money, lifework, general leadership, romance, political or social issues, happiness, religious faith or spirituality, community service, friends, and personal values (Mariano & Damon, 2008).

Mariano and Damon (2008) indicate that contributions to community are a key indicator of positive youth development. They also present the idea that purpose is associated with increased prosocial behaviors and negatively associated with negative behaviors, and therefore is central to the study of PYD. In extending Damon's work, Mariano and Going (2011) emphasize the person–context relationship, in which individuals are constantly coacting with their environment and receiving resources and opportunities from a surrounding network. Mariano and Going (2011) state that purpose helps young people express and satisfy their individual interests, strengths, and talents. They reassert that purpose in life can serve as a guide for adolescents and a way of adapting to aspects of life that adolescents may view as threatening.

Two of the outcomes associated with having purpose in one's life are coping mechanisms and psychological cohesion. Purpose helps adolescents cope by allowing them to see the positive side of whatever challenge they may be encountering. Mariano and Going (2011) found that adolescents with a more comprehensive sense of purpose focused on future improvements and the positive states that could result from challenging situations, more so than was the case among adolescents with a less comprehensive sense of purpose. In turn, psychological cohesion is understood as a complementary set of values that adds to one's moral character, such as humility, integrity, and vitality. Purpose helps improve the psychological aspects of an adolescent's life by acting as the glue that unifies these moral characteristics.

An individual with strong psychological cohesion has character attributes, morals, and values that all flow together positively because of having an identified purpose in life. In addition to suggesting that purpose is central to youth thriving, Damon and colleagues (e.g., Mariano & Damon, 2008) suggest that a youth's purpose in life can be defined by their religion and spirituality.

The Work of Pamela Ebstyne King

Pamela Ebstyne King and her colleagues (e.g., King & Boyatzis, 2015; King, Carr, & Boitor, 2011; see also Bundick et al., 2010) have also studied the role of religion, spirituality, and PYD in people's lives from an RDS-based perspective. King and Boyatzis (2015) noted the explosion of empirical findings on religion and spirituality in childhood and adolescence that occurred across the first two decades of the twenty-first century, and explained that the constructs of religion and spirituality are complex and multidimensional, and include cognitions, feelings, behaviors, experiences, and relationships. King and Boyatzis emphasized the importance of bidirectional relationships between young people and their context in influencing religious and spiritual development. As a consequence of these individual↔context relations, they introduced the idea of reciprocating spirituality, and pointed to the use of RDS-based concepts for creating a perspective allowing for both the individual and context to be assessed in a unified fashion. As such, they emphasize the importance of examining how children and adolescents develop in religiousness and spirituality in the context of proximal and more distal relationships.

Pamela Ebstyne King

Reflecting Damon's conception of purpose, King et al. (2011) define spirituality as a developing sense of identity that motivates youth to care for themselves and, as well, to contribute to the greater good. They note that transcendence, fidelity, and generative actions are all key to the development of spirituality. Transcendence exists when people think beyond the self, and attribute or see significance in something bigger than themselves. For example, this focus may relate to God or to a higher being with a sense of divinity, to humanity in general, or to specific communities (such as the church). Fidelity is the adherence to transcendence, where people consistently connect to a world beyond themselves. When one has acquired both transcendence and fidelity, one is motivated to produce generative actions in that they promote and develop one's own life as well as the lives in one's community.

King and colleagues (2011) note that spirituality and religiosity are linked to PYD in several ways. First, cognitive development during adolescence involves the emergence of more abstract thought, and adolescents begin to understand the notion of God and better understand religious beliefs. Spirituality combines one's values and beliefs to form an individual identity and help identify a purpose in life. These developments lead to behaviors indicative of thriving. Transcendence also aids this development through providing motivation to be altruistic and more understanding of devotion, responsibility, and commitment.

In a study of urban public high school students, Furrow, King, and White (2004) found a positive relationship between religious self-understanding, personal meaning, and prosocial personality. Differences existed in the relationship of personal meaning to prosocial personality across age and gender cohorts. Furrow and colleagues found a significant, positive association between personal meaning and prosocial concerns among boys, but no significant association among girls. This finding suggests that personal meaning may be more applicable for males than for females. Overall, these findings provide support for considering that, among youth, religion is a developmental resource associated with personal meaning and with concern and compassion for others.

In sum, whereas Damon (2008) sees purpose as an indicator of PYD, he notes that a next step in his research will require a deeper understanding of the ways that young people are purposeful. Purposeful young people may indeed be contributing to something beyond themselves, but whether that contribution is for self-serving reasons and social approval or an end in itself may be an important distinction for understanding how purpose and contribution are associated with different facets of adolescent development. King's work (e.g., King & Boyatzis, 2015; King et al., 2011) constitutes an important extension of Damon's scholarship in that the study of religiosity and spirituality among youth provides insight into facets of the world of youth that may provide purpose and meaning for them.

Margaret Beale Spencer and the PVEST Model

Another important example of a PYD model, one that also focuses on identity, has been presented by Margaret Beale Spencer (e.g., Cunningham & Spencer, 1996; Spencer, 1999, 2006; Spencer, Swanson, & Harpalani, 2015; Swanson, Spencer, & Petersen, 1998). Derived from Bronfenbrenner's bioecological theory, Spencer and colleagues formulated an integrative approach to understanding the role of multiple levels of the context on ethnic identity. Termed a "phenomenological variant of ecological systems theory" (PVEST), this model is used to study the identity development process among ethnically diverse youth. For example, much of this work has been directed to understanding identity development among African American male adolescents.

Spencer's PVEST model is a dynamic and systemic framework for studying development that takes into account social structural factors, cultural influences, and individual experiences, as well as individuals' perceptions of these features (Spencer, 2006). A central feature of this model is an emphasis on the ways in which youth make sense of their contexts and the role that these understandings play in their perceptions of events, people, and opportunities in their environments.

The work of Spencer and her colleagues and students has especially focused on how youth respond to their environments when they are seen as reflecting

Margaret Beale Spencer

social inequities or injustices, using PVEST as a framework for interpreting their findings. For example, Spencer, Swanson, and Harpalani (2015) note that the social, economic, and cultural realities and complexities evident in twenty-first-century North America are central foundational themes for understanding "the self," and they extend traditional considerations of self-processes to include the implications of social inequalities experienced in the early decades of life. They do so by focusing on racial and socioeconomic inequities, along with social and cultural complexities that compound these inequalities. Although concerned with development across the life span, Spencer et al. (2015) focus on adolescence, when self-consciousness is heightened and self-processes are most salient. The breadth of normative developmental influences on self-processes makes the exploration of cross-domain manifestations, such as health, education, and juvenile justice, exceedingly important. Accordingly, Spencer et al. discuss self-processes linked to evaluative judgments about body image, color awareness, social status, nativity, religious affiliation, and gender bias. All of these attributes impact the self as a function of the individual's developmental status.

An important theoretical consequence of this model for the study of PYD is that different youth will experience the same events and settings through different lenses, which can yield different interpretations and effects. Thus, whereas an after-school homework club might promote academic competence for some youth, for others the same context might evoke disturbing reminders of earlier unavailability of resources, such as access to books and teacher help. The effectiveness of this asset, then, is likely to vary according to youth perceptions of this setting. Although attention has been paid to the importance of bidirectional relations between individual characteristics and ecological settings, Spencer argues that the role of structural inequality must be considered as well. The framework of PYD and thriving is intended to be a general theory of human development that should be applicable to all youth.

Spencer's model provides a way to include the systematic effects of shared contexts on youth perceptions of their environments in the transactional study of PYD. Spencer's research with the PVEST model has focused especially on youth of color and on poor youth. In part, her scholarship is a critique of researchers' "failure to consider their [youth of color's] unique human development experiences in socially constructed and culturally unique contexts" (Spencer, 2006, p. 271; Spencer, Swanson, & Cunningham, 1991). The contexts of underserviced neighborhoods, impoverished communities, and families under stress that often characterize urban, and frequently African American, children, and the lifelong structural effects of these contexts, are generally ignored or characterized as random error in many developmental models (Spencer, Noll, Stoltzfus, & Harpalini, 2001).

In addition, Spencer's work points directly to the need to study positive outcomes for all youth. These positive outcomes need to be defined within the cultures and contexts in which youth and their families find themselves (Spencer, 2006). The PVEST model provides a nuanced structure in which to do this research. The model explicitly includes the everyday experiences of race, involving both overt and subtle racism with which people of color must learn to cope; it also includes the demands of socialization, which all youth face (Lee, Spencer, & Harpalani, 2003).

The work of Spencer and her colleagues brings notions of injustice and inequality into developmental models. Structural inequity, racism, and poverty are not individual characteristics, nor are they context-specific. They are pervasive facts of American life that affect all segments of the population in various, complex ways. At the same time, the actual experience is perceived at the individual level. What one adolescent experiences as stress may not affect his or her neighbor or sibling in the same way (Spencer, 1995). Spencer argues that to effectively promote thriving, these factors will need to be understood better and incorporated into the models and methods of PYD. Her scholarship offers a powerful frame for such research. As well, it stands in many ways as the conscience for our field, as a means to keep issues of social justice and rigorous, theory-predicated developmental science integrated and at the forefront of our scholarly agenda.

THE LERNER AND LERNER MODEL OF PYD

The emergence in the 1990s of a strength-based conception of PYD can be attributed to the ideas of youth program innovator Rick Little, who formulated what he termed the "Four Cs" of PYD (competence, confidence, connection, and character) (Lerner et al., 2015). Developmental scientists who were attracted to this idea advanced their own formulations of the substance of PYD, and one of these formulations focused on what was subsequently labeled the Five Cs (caring or compassion was added to the above-noted four Cs), or the Five Cs plus one (contribution, seen as an outcome of the development of the other Cs) model (Lerner et al., 2015).

As I have emphasized, not all PYD models involve the Five Cs conception inspired by Rick Little. Other scholars interested in the PYD concept were attracted to this RDS-oriented idea. However, they elected to operationalize the concept differently. Among the key reasons that developmental scientists interested in understanding the bases of PYD and in learning how to promote it were attracted to RDS metatheory was that the ideas associated with this conception afforded:

1. A focus on individual↔context relations, and thus on agency and the strengths of individuals to engage their context in adaptive developmental regulations (e.g., Brandtstädter, 1998, 2006; Lerner, 1982).
2. A focus on the idiographic character of such relations and, in this regard, the use of the Bornstein (1995b, 2006, 2017) specificity principle in understanding the bases and features of each individual trajectory of thriving. This principle would frame multilevel sets of "what" questions, such as, "What features of positive development (e.g., what Cs of PYD or what features of youth purpose) emerge; that are linked to what trajectory of individual↔context relations; for youth of what sets of individual psychological, behavioral, and demographic characteristics; living in what families, schools, faith communities, neighborhoods, nations, cultures, and physical ecologies; at what points in ontogenetic development; and at what historical periods?"
3. An optimistic emphasis on the potential efficacy of attempts to promote PYD because of being able to capitalize on developmental plasticity and on the specific strengths of young people and enhance the course of their lives. Indeed, the key hypothesis of any RDS-based PYD model is that, if the specific strengths of youth and the resources in their context (assets for positive development found in their homes, schools, out-of-school-time activities, and faith communities, for instance) are aligned across adolescence, then the lives of all youth can be enhanced.

In short, then, all RDS-based models of PYD focus on the specific, positive, or healthy outcomes of individual↔context relations thought to derive from the coactions in the relational developmental system.

Why Focus on the Lerner and Lerner Model?

At this writing, there is growing use of RDS-based models of PYD in nations around the globe (e.g., see Schoon, 2017, and as well other chapters in Petersen et al., 2017; see too USAID, 2013). Nevertheless, it is fair to say most studies of PYD have been conducted on U.S. samples, a situation still common in the developmental science literature (e.g., Lerner, 2015a, 2015d; Raeff, 2016). For instance, in Bornstein's (2010) *Handbook of Cultural Developmental Science*, many chapters point to the lack of data from the majority world (see too Smith et al., 2017; Petersen et al., 2017). In addition, Koller, Motti-Stefanidi, Petersen, and Verma (2017) note that:

> Studies conducted in the majority world or focused on ethnic minorities in the minority world still focus on negative behavior more often than positive behavior (e.g., Garcia-Coll, 2013; Verma & Petersen, 2015). We want good outcomes for young people but we are undermining this goal when we focus on negative behavior of youth such as bullying and underachievement, rather than their positive counterparts (the behavior we hope to see) such as prosocial behavior and achievement.
>
> (pp. 307–308)

Whereas there is a clear need for global studies in diverse global contexts (USAID, 2013), it is useful to understand extant RDS-based models of PYD from Western countries when considering global youth development. For instance, the model on which I focus in this chapter—the Lerner and Lerner Five Cs model of PYD (e.g., Lerner et al., 2015; Lerner, Wang et al., 2017)—is the instance of RDS-based PYD models that has the most extensive empirical support (Heck & Subramanian, 2009) and, as well, is receiving the most attention in globally diverse settings (Petersen et al., 2017). This model therefore serves as a likely candidate on which global PYD research and practice can be based. Accordingly, I believe that, independent of the fact that I am focusing on a model that Jacqueline Lerner and I have developed along with our colleagues, and the fact that I therefore may know the literature about this model better than I do the literature associated with other models of PYD, the extensive empirical data base associated with this model, including its growing use (at this writing) in non-U.S. settings, suggests the usefulness of this focus.

The Five Cs Model of PYD

As is the case with all RDS-based PYD models, the Lerner and Lerner conception is a strength-based model of development that seeks to understand and enhance the lives of diverse youth through engagement with key contexts in their ecology (e.g., families, schools, peer groups, and out-of-school-time programs). Indeed, a major focus of the Lerner and Lerner PYD research has been the study of the

Jacqueline V. Lerner

out-of-school-time setting (Lerner et al., 2015). There is considerable research assessing if and how the lives of youth within the United States can be enhanced through engagement with community-based youth development programs (Vandell et al., 2015), especially if these programs align features of both youth and program strengths (as occurs when theoretical models, such as the person–stage–environment-fit model, are used to frame program design; Eccles, 2004).

The model of the PYD process constructed by Lerner, Lerner, and their colleagues has drawn explicitly on the RDS-based idea of individual↔context coaction as its foundation. This model has been elaborated in the context of the longitudinal 4-H Study of PYD conducted by Lerner, Lerner, and colleagues (e.g., Bowers et al., 2014; Lerner et al., 2005, 2009, 2013, 2014). This research seeks to identify the individual and ecological relations that may promote thriving and, as well, that may have a preventive effect in regard to risk/problem behaviors. Within the 4-H Study, thriving is understood as the growth of attributes that mark a flourishing, healthy young person. As already mentioned, these characteristics are termed the "Five Cs" of PYD—competence, confidence, character, connection, and caring.

Consistent with the central tenets of all RDS-based models of PYD, the core theory of change tested in this approach is that, if:

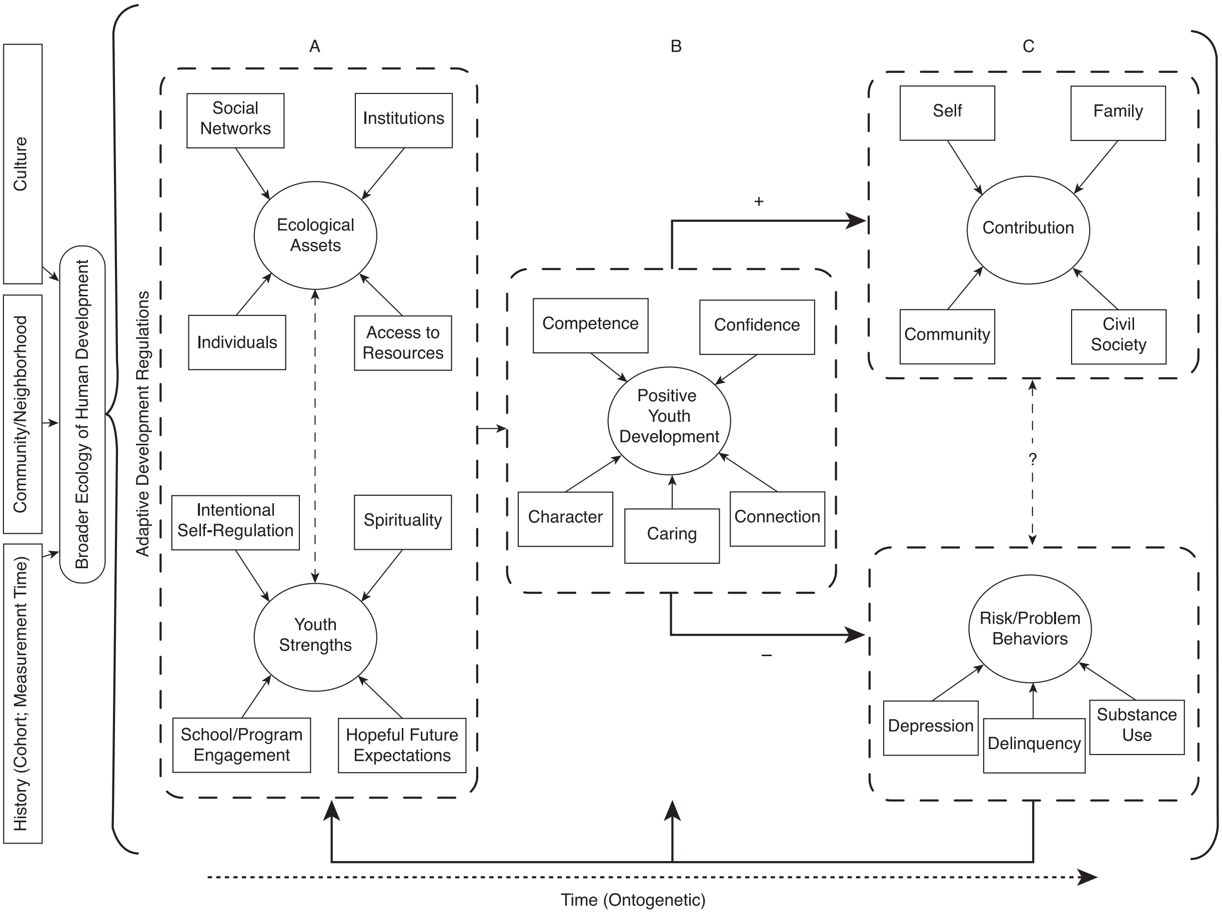


Figure 9.3 The Lerner and Lerner model of PYD as instantiated within the 4-H Study of Positive Youth Development.

1. The strengths of youth (e.g., a young person's cognitive, emotional, and behavioral engagement with the school context, having the "virtue" of hope for the future, spirituality, or possession of intentional self-regulation (ISR) skills such as Selection (S), Optimization (O), and Compensation (C)) can
2. be aligned with the resources for positive growth found in, for instance, families, schools, or youth development programs (e.g., the "Big Three" attributes of youth development programs (i.e., positive and sustained adult–youth relationships, skill-building activities, and youth leadership opportunities; Lerner, 2004; Hershberg et al., 2015, 2016); then
3. young people's healthy development will be optimized (e.g., Lerner et al., 2009, 2013; Lerner, 2004).

That is, the healthy behaviors described by the Five Cs will be promoted and, as well, the young person will demonstrate other positive attributes of behavior reflecting adaptive developmental regulations—most fundamentally, a sixth "C," youth contributions to self, family, community, and civil society. In other words, if positive development rests on mutually beneficial relations between the youth and his/her ecology, then thriving youth should be positively engaged with and act to enhance their world. Furthermore, the youth should be less prone to engage in risk/problem behaviors.

Figure 9.3 presents an illustration of the Lerner and Lerner conception of the PYD developmental process. The figure illustrates, as well, that these adaptive developmental regulations and their positive and problematic sequelae exist within the broader ecology of human development. This ecology includes the exosystem and the macrosystem discussed by Bronfenbrenner (1977, 1979, 2005; Bronfenbrenner & Morris, 2006), for instance, government and community programs and institutions, culture, and the designed and natural physical ecology. In addition, historical (temporal) variation (the chronosystem, in Bronfenbrenner's bioecological model; Bronfenbrenner & Morris, 2006) introduces change at all levels of organization within the relational developmental system.

Tests of the Lerner and Lerner PYD Model

To test the ideas presented in Figure 9.3, researchers at the Institute for Applied Research in Youth Development (IARYD) at Tufts University launched the 4-H Study of PYD, a longitudinal study beginning at Grade 5 and ending at Grade 12. Overall, across eight waves of the study, approximately 7000 youth and 3500 of their parents from 42 states were surveyed. At all eight waves, the sample varied in race, ethnicity, socioeconomic status, family structure, rural–urban location, geographic region, and program participation experiences (Lerner et al. 2015). This research identified the resources, or developmental assets (Benson, 2008), which existed in the key settings of youth (e.g., families, schools, faith-based institutions, and community-based youth programs). In addition, through obtaining information about the young person's strengths (e.g., ISR, school engagement, spirituality, and hopeful future expectations), the study assessed the individual strengths of adolescents. Patterns of participation in out-of-school-time (OST) activities were also assessed in this study. These activities included youth development programs, such as 4-H, Boy Scouts, Girl Scouts, YMCA, Boys & Girls Clubs, and Big Brothers/Big Sisters, sports, arts and crafts, interest clubs, religious clubs, performing arts organizations, and service organizations. Civic engagement/civic contribution, future aspirations and expectations, relationships with parents, friends, and other adults, and values were also measured. In addition, parents were asked about the nature and composition of their household, education, employment, and neighborhood.

At this writing, the findings of the 4-H Study have been reported in more than 100 publications (see Lerner et al., 2015, for a review). Here, I summarize some of the key findings bearing on the Lerner and Lerner model presented in Figure 9.3.

IARYD researchers studied youth development programs as settings for, or sources of, the key ecological assets linked to positive developmental outcomes. These ecological assets were divided into four categories—other individuals such as parents, peers, mentors, and teachers; community institutions, including youth development programs; collective activity between youth and adults, including program leaders; and access to the prior three types of assets. Theokas and Lerner (2006) found that, in all settings, assets represented by other individuals were the most potent predictors of PYD. Family assets such as parental involvement, autonomy granting, communication, and problem solving were most important in the lives of youth. Subsequent analyses (Urban et al., 2010) indicated that dimensions of the neighborhood coact with adolescent youth development program involvement to predict PYD, depressive symptoms, and risk behaviors—findings consistent with the theory of change model shown in Figure 9.3.

In addition, several studies have also used the 4-H Study data set to examine possible associations between self-regulatory processes and youth development program participation. For example, Urban and colleagues (2010) found that both the strengths of youth, represented by a measure of their intentional self-regulation (ISR) attributes, and the resources of their contexts, represented by out-of-school-time programs, are involved in thriving. However, youth ISR abilities moderated the effect of participation in youth development programs on PYD among adolescents living in neighborhoods with lower levels of ecological assets. Youth in these settings who had the greatest capacity to self-regulate benefited the most from involvement in youth development programs, in regard to PYD, depressive symptoms, and risk behaviors. These relations were particularly strong for girls.

Moreover, character virtues, such as hope for one's future (Callina et al., 2017; Schmid, Phelps, & Lerner, 2011a; Schmid et al., 2011b; Schmid & Lopez, 2011), along with the cognitive and behavioral skills that youth need to activate ISR skills to achieve future goals, may also play important roles in the development of civic engagement. For example, using data collected from youth participants in Grades 7, 8, and 9 of the 4-H Study, Schmid and Lopez (2011) assessed the role of hopeful future expectations in predicting growth trajectories of positive and negative developmental outcomes, including PYD, contribution, risk behaviors, and depressive symptoms. Hopeful future orientation was a stronger predictor than ISR for each of the outcomes assessed.

The 4-H Study data have also been used to examine the ecological assets of parenting and youth programs in relation to variables indexing civic engagement. For example, using data from youth in Grades 5 through 8 from the 4-H Study, Lewin-Bizan et al. (2010) found a developmental cascade through which positive parenting (indexed by warmth and monitoring) was a key contextual asset predicting subsequent ISR; in turn, ISR predicted subsequent scores for PYD which, in turn, positively predicted later youth contribution scores.

Using data from Grades 8 through 11, Zaff et al. (2010, 2011) derived a measure of active and engaged citizenship (AEC) from items within the measures used in the 4-H Study. AEC involved four first-order latent constructs: civic participation, civic duty, civic self-efficacy, and neighborhood connection. These four factors indexed the second-order latent construct of AEC. Consistent with the model presented in Figure 9.3, engagement with the ecological developmental assets represented by community-based institutions and programs (which, in the Zaff et al., 2011 study, involved youth development programs and religious institutions) was associated positively with AEC.

In sum, findings from tests of the model shown in Figure 9.3 conducted with data from the 4-H Study of PYD support the idea that links among the strengths of young people and the ecological assets in their families, schools, and communities predict their thriving and, in turn, their contributions to, and active and engaged citizenship within, their communities. However, there have also been tests of the model that have been inconsistent with expectations. For instance, the predicted inverse relation between indices of civic engagement and risk/problem behaviors was not present for all participants at all ages. Some trajectories of high, positive civic engagement were coupled with trajectories involving increasingly higher levels of risk/problem behaviors for specific youth across specific portions of adolescence (Lewin-Bizan et al., 2010; Phelps et al., 2007).

Therefore, the overall strength and valence of the relation represented in the model between civic engagement and risk/problem behaviors remains uncertain in any general sense (and is represented by a "?" in Figure 9.3). Additional theory development and research will be required to identify the individual and ecological conditions moderating the valence and strength of the relation between civic engagement and risk/problem behavior for specific youth or for groups of adolescents. There are other issues that need to be addressed in order to advance knowledge beyond the understanding of PYD provided by the 4-H Study.

Extending the Assessment of the Lerner and Lerner Model of PYD to Global Youth

Future research will need to address some methodological limitations of the 4-H Study. For instance, in the 4-H Study there were limitations of design and measurement, which have been discussed in prior summaries of this work (e.g., Bowers et al., 2014; Lerner et al., 2015). Sampling was also a major limitation of the study (Spencer & Spencer, 2014), in that 4-H Study participants were part of a convenience sample (Bowers et al., 2014). Moreover, across the eight waves of testing, about two-thirds of this sample were White and less than 10% of the group were Black; similarly, less than 10% were Latino. Other youth of color were represented in even lower frequencies. In turn, the sample was mostly suburban or rural; less than a fifth of the sample lived in urban settings. In addition, the participants came from relatively highly educated families and middle-to-above socioeconomic status. In short, researchers need to be cautious about applying what has been learned about the Lerner and Lerner model of PYD (Lerner et al., 2015) to samples that differ from the U.S. sample assessed in the 4-H Study.

Questions about the generalizability of the findings of the 4-H Study may be raised not only about minority youth in the United States (Lerner, Wang, et al., 2017; Spencer & Spencer, 2014), however. Researchers may raise questions about generalizability to youth in nations other than the United States. That is, the primary limitation of the 4-H Study, at least in regard to understanding PYD among global youth, pertains to its exclusive focus on U.S. youth. Simply, then, the 4-H Study data set is limited in its ability to illuminate what positive youth development may look like for global youth.

However, the visibility of the 4-H Study (Petersen et al., 2017) and the international interest in RDS-based models of human development more generally (e.g., Lerner, 2012, 2015a, 2015e) have been associated with several attempts to extend the assessment made in the 4-H Study to youth in nations other than the United States. Such work is consistent with ideas forwarded by USAID (2013) in regard to still unmet research needs regarding PYD research involving youth from the majority world. The USAID report (2013) calls for rigorous, longitudinal studies of holistic programs aimed at promoting PYD. The report argues that such studies should be framed within a conceptual model applicable to international settings and, as well, should be marked by the use of psychometrically strong measures. At this writing, a key focus of international work pertinent to at least the Lerner and Lerner model of PYD has been on measurement of this construct. That is, this work has focused on verifying if the Five Cs measure of PYD developed for use in the 4-H Study (e.g., Bowers et al., 2010; Geldhof et al., 2014a, 2014b; Lerner et al., 2005) may be applicable to youth from nations other than the United States.

The results of several studies (e.g., Geldhof et al., 2016; Gestsdóttir et al., 2016) suggest that the measurement and meaning of the Five Cs may not be very different between U.S. and Western European youth. However, at the time of this writing, it is fair to note that research to date remains at a preliminary stage. Key next steps should assess if there is measurement invariance across nations for more of the measures used in tests of the Lerner and Lerner model of PYD; the focus should not remain on only the PYD measure itself. At this writing, assessments have focused only on the measure of the Five Cs used in the 4-H Study and, even with this narrow focus, studies have not been longitudinal. Together, these limitations mean that the overall Lerner and Lerner PYD model (or any other PYD model for that matter) has not been assessed sufficiently among global youth, especially among samples of global youth who vary from the 4-H Study participants and, in particular, youth of color living in diverse poverty contexts.

Spencer and Spencer (2014), although critiquing the 4-H Study in regard to limitations on generalizability linked to the lack of appropriate representation of U.S. youth of color in the sample, raise some points that may be applicable to issues of generalizability pertinent to global youth and, again, particularly youth of color living in diverse poverty contexts. That is, as with youth of color in the United States, youth from nations in the majority world face structural challenges (e.g., gender prejudice, marginalization, or lack of access to institutions of power in their nation) and contextual problems (e.g., persistent and pervasive poverty, lack of adequate access to health care, deficiencies in educational opportunities) that must be considered when empirically studying them. In addition, by failing to consider the unique contextual challenges that global youth may face, especially those living in poverty, researchers may be defining PYD too narrowly for these youth. For instance, if researchers approach positive development with an exclusive focus on the Five Cs, they may be missing or, at the least, underestimating the potentially unique and creative ways in which global youth may use perhaps unconventional contextual assets to cope with their settings and thrive, particularly when faced with exceptional circumstances (Spencer & Spencer, 2014; Spencer et al., 2015). In turn, it may be that a focus on the Five Cs is appropriate for many global youth, but the way in which these Cs are operationalized in the 4-H Study may need revision; the Cs may be manifested differently in different national or cultural contexts. Clearly, future research needs to address these possibilities.

To address these possible constraints in assessments of the global generalizability of the Lerner and Lerner model of PYD, quantitative work (e.g., invariance testing) needs to be extended to the breadth of the constructs included in the model, and not only the Five Cs measure (or, perhaps, another measure of youth strengths not indexed by the Five Cs). In addition, the survey approach used in the 4-H Study needs to be triangulated with qualitative methods in order to afford understanding of what may be the distinct meaning of PYD among youth from majority-world contexts. Such qualitative work would also be useful for describing what also may be the distinct assets for engaging in adaptive developmental regulations that may exist in their settings (Spencer & Spencer, 2014).

I believe that adaptive developmental regulations are a fundamental part of thriving for youth around the globe, that is, the importance of mutually beneficial individual↔context relations involved in PYD exists for all youth, in that such relations constitute the fundamental feature of adaptive functioning (Brandtstädter, 1998). However, the *content* of these relations may vary. What global youth bring to these individual↔context exchanges, what the context provides to them, and how thriving may be actualized in global youth from the majority world have not been as yet adequately elucidated.

Developmental scientists do not know, therefore, if, or the extent to which, the Lerner and Lerner model can be applied to global youth, at least in regard to the manifest variables involved in their thriving. This same limitation on generalizability exists to varying extents in regard to the other RDS-based models of PYD I noted earlier in this chapter (e.g., Benson, 2008; Damon, 2008; Spencer, 2006; Spencer et al., 2015). Assessing the external validity of the Lerner and Lerner PYD model (again, as an instance of RDS-based models of PYD) may contribute importantly to the evidentiary base for applications of developmental science aimed at enhancing PYD among diverse individuals across diverse contexts.

Future Directions

Developmental scientists have, in the repertoire of models and methods in their intellectual "toolbox," potential means to promote youth thriving as well as active and engaged youth citizenship. Furthermore, through enhancement of the adaptive developmental regulations between individuals and their contexts, developmental scientists may afford diverse individuals the opportunities needed to maximize their aspirations and actions by engaging with social institutions that support individual agency, promote freedom and liberty, and support civil society or, in other words, that contribute to social justice (Fisher et al., 2013; Lerner, 2002, 2004; Lerner & Overton, 2008). In order to contribute significantly to creating a developmental science aimed at promoting such social justice-oriented outcomes, scholars need to conduct more culturally relevant studies of PYD and contextually specific tests of PYD models, such as the Lerner and Lerner one, to fully understand thriving in all youth. As well, scholars need to identify the means with which to alter individual↔context relations in ways that enhance the probability that all individuals, no matter their individual characteristics or contextual circumstances, have greater opportunities for PYD (e.g., see Fisher et al., 2013).

In addition, youth development practitioners have deep understanding of these contexts and are readily positioned to test and apply scientific evidence to promote the most effective and scalable approaches to youth development. As I noted earlier in this chapter, the concept of PYD arose from the insights of practitioners and, in particular, practitioners working internationally (i.e., Rick Little and the International Youth Foundation). At this writing, many practitioners working with youth from the majority world are often intuitively applying PYD and the RDS-based ideas of holism, individual development, and individual↔context relations without necessarily linking their work to knowledge of the scientific literature pertinent to these ideas.

I believe, however, that advances in knowledge about, and significant contributions to, global youth can be made if there are strong and mutually beneficial collaborations between academia and practitioners, ones where the needs and interests of both parties are fully understood and integrated towards a common goal of improved youth thriving. To create such collaborations at this point in the history of applied developmental scholarship about PYD, the growing interest in studying and enhancing PYD among global youth (e.g., Smith et al., 2017; Petersen et al., 2017; USAID, 2013) could best be instantiated if researchers relied once again on the wisdom and knowledge of practitioners.

CONCLUSIONS

RDS-based ideas, built on scholarship from the field of comparative psychology and the study of the course of human evolution, have important implications for describing, explaining, and optimizing the course of development across the life span (Baltes et al., 1977; Lerner, 2012, 2015a). The examples of RDS-based, variable-oriented models illustrate the use of these ideas within and across different ontogenetic periods. As well, the examples all reflect that, when embedded in an RDS-based approach to theory, focus on a specific variable or construct nevertheless engages the developmental scientist in the study of all of the levels of organization integrated (fused) within the relational developmental system.

The examples also reflect the six design criteria for developmental systems models discussed by Ford and Lerner (1992) and, as well, illustrate that theoretically-predicated and methodologically rigorous research can be productively engaged in by developmental scientists using an RDS-based perspective (again, additional and more detailed examples of the quality and productivity of such variable-focused work may be found in the chapters in the four volumes of the seventh edition of the *Handbook of Child Psychology and Developmental Science*; Lerner, 2015e).

As I discuss in the next chapter, the contribution of RDS-based ideas is realized even more broadly by scholarship derived from the mid-range theories associated with RDS metatheory (again, see the funnel diagram in Figure 1.1). Several important examples of such theories are discussed in the next chapter.

CHAPTER TEN

Relational Developmental Systems-Based Theories

Sample Cases

By the end of the twentieth century, relational developmental systems (RDS)-based theories emerged as a superordinate frame for several different mid-range models of human development (see Figure 1.1; see too Overton & Molenaar, 2015). Such theories are not "grand theories," or theories of everything (e.g., such as sociobiology purported to be; Wilson, 1975a). As well, mid-range theories are not solely focused on a specific topic or variable (e.g., see the models of PYD presented in Chapter 9). Mid-range theories may focus on several topics, variables, or levels of organization. Such models may encompass very broad domains of human existence (e.g., the span or course of human life or the ecology within which humans live and develop). Nevertheless, such theories do not assert that they account for all dimensions or features of human existence. As discussed in previous chapters, these instances of RDS metatheory-based mid-range theories (e.g., Bronfenbrenner & Morris, 2006; Elder et al., 2015; Lewis, 1997; Magnusson, 1995, 1996, 1999a, 1999b; Magnusson & Stattin, 2006; Sameroff, 1983, 2009; Thelen & Smith, 1998; Wapner & Demick, 1998; see too Feldman, 2000; Fischer & Bidell, 1998; Mascolo & Fischer, 2015; Raeff, 2016; Rogoff, 1998) may be regarded as members of the same theoretical "family."

All "family members" reject nature–nurture "split" concepts of reality and causation (Overton, 2015a); adopt a relational and integrated (or fused) conception of the multiple levels of organization involved in the ecology of human development (e.g., Gottlieb, 1996; Schneirla, 1957; Tobach, 1981); focus on relative plasticity over the course of the human life span (i.e., the dynamism between change and constraints on change that is derived from the integrations across the levels of the developmental system); point to the role of individuals as active agents in their own development; and emphasize the importance of including the contributions of multiple levels of organization—ranging from biology through history—in explanations of development. Together, these instances of RDS-based mid-range theories provide examples of the use of the design criteria for developmental systems theories discussed by Ford and Lerner (1992).

In this chapter, I discuss features of a sample of these theories. I selected this sample based on my judgment of the prominence and breadth of their influence on developmental science across (at this writing) the past five decades or so, as well as on the basis of the clarity of their embeddedness within RDS-based ideas. I hope to illustrate these points in this chapter and to illuminate both the distinctive features of these "family members" and their commonalities. This presentation will, I hope, underscore the use of these models in describing, explaining, and optimizing development across the life span and, as well, provide a set of concepts that will help explain why genetic reductionist accounts of human development are irreparably flawed and, at their base, counterfactual accounts of development across the life span. Given the life-span frame I am bringing to this discussion, it is useful to discuss first the life-span view of human development.

The life-span view of human development—as elaborated in particular by Paul B. Baltes, John R. Nesselroade, and K. Warner Schaie—has had a generative influence on—and was influenced by—other key members of the RDS-based theoretical family. In my view, three family members stand out as particularly involved in this association: action theories of human development (e.g., Brandtstädter, 1998, 1999, 2006; Heckhausen, 1999); the life-course perspective (Elder, 1974, 1980, 1998, 1999; Elder et al., 2015); and the bioecological view of human development (e.g., Bronfenbrenner, 1979, 2005; Bronfenbrenner & Morris, 2006). Thus, because of its central role in the past and present development of RDS-based models, I consider first the life-span view of human development.

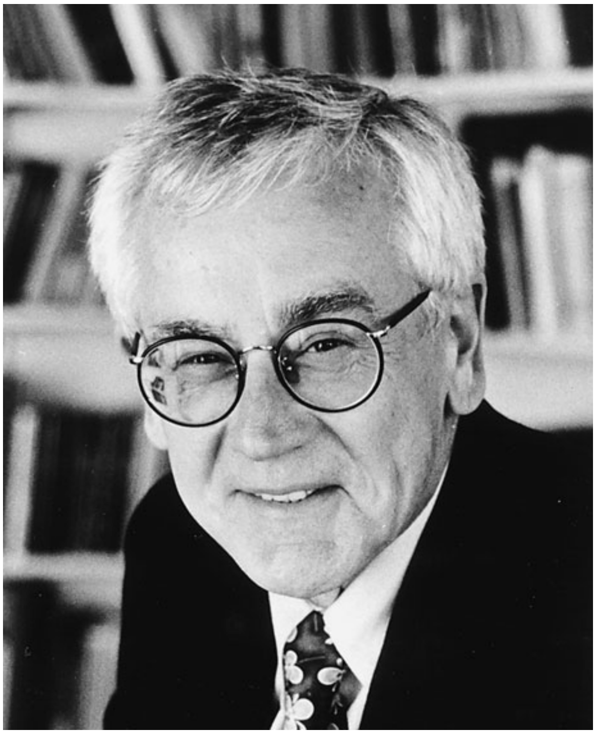
LIFE-SPAN DEVELOPMENTAL THEORY

Life-span developmental theory (Baltes, 1979a, 1979b, 1983, 1987, 1997; Baltes et al., 1998, 1999, 2006; Baltes, Reese, & Lipsitt, 1980), as stated by Baltes and colleagues (2006), "deals with the study of individual development (ontogenesis) from conception into old age . . . A core assumption of life-span developmental psychology is that development is not completed at adulthood (maturity). Rather, ontogenesis extends across the entire life course and lifelong adaptive processes are involved" (p. 569).

In the context of these assumptions, Baltes and colleagues (2006) noted that life-span developmental theory has several scientific goals, ones that span *and integrate* the basic-to-applied continuum of interest in other members of the RDS-based theory family (e.g., developmental conceptualism; see Chapter 9). That is:

> The objective of life-span psychology is: (a) to offer an organized account of the overall structure and sequence of development across the life span, (b) to identify the interconnections between earlier and later developmental events and processes, (c) to delineate the biological, psychological, social, and environmental factors and mechanisms which are the foundation of life-span development, and (d) to specify the biological and environmental opportunities and constraints which shape life-span development of individuals, including their range of plasticity (modifiability). With such information, life-span developmentalists further aspire to determine the range of possible development of individuals, to empower them to live their lives as desirably (and effectively) as possible, and to help them avoid dysfunctional and undesirable behavioral outcomes.
>
> (Baltes et al., 2006, p. 570)

Moreover, given the central interest in life-span developmental theory in how the individual↔context relation is associated with adaptive (healthy, positive) developmental trajectories, Baltes and his



Paul B. Baltes

colleagues (e.g., Baltes & Baltes, 1980, 1990) tested the idea that successful development involves the maximization of gains and the minimization of losses. Indeed, more generally, Baltes and Baltes (1990) hypothesized that *all* development involves an integration of loss and gain (see Lerner, 2011; Raeff, 2011). Consistent with the notion of integrated levels, however, Baltes and colleagues (2006) noted that a person's cultural and historical niche influences his or her development—that is, his or her synthesis across ontogeny of gains (e.g., in visual acuity or native language fluency) and losses (e.g., involved in the death of neurons not involved in acuity, or in the loss of capacity to learn non-native languages with fluency equal to that associated with one's native language).

A key illustration that Baltes and his colleagues pointed to in regard to the dynamic integration between gains and losses across the life span, and to the social and cultural embeddedness of this dynamic of individual↔context relations, pertains to the fluid mechanics and crystallized pragmatics of intelligence. Baltes and colleagues (1998) explained that:

> Very much in line with the life-span dynamic between biology and culture . . . intellectual abilities that are thought to reflect the neurobiologically based mechanics of intelligence—like working memory and fluid intelligence—typically showed normative (universal) declines in functioning beginning in middle adulthood. Conversely, intellectual abilities that primarily reflect the culture-based pragmatics of intelligence—such as professional knowledge and wisdom—may show stability or even increases into late adulthood. As to the ontogenesis of intelligence, then, gains and losses . . . co-exist.
> (p. 583)

Concepts Framing Life-Span Developmental Scholarship

To pursue the goal of life-span developmental theory, Baltes and his colleagues conducted scholarship that was framed by several theoretical ideas, which are summarized in Table 10.1. This table illustrates both (a) the commonality of theoretical ideas between life-span developmental theory and other instances of RDS-based theories (e.g., in regard to plasticity and to the embeddedness of development in a dynamic system comprised of levels of organization ranging from biology through culture and history), and (b) the ideas about human development that are specifically brought to the fore of human development theory by a life-span perspective (e.g., development as a lifelong process, the dynamic between gains and losses, the integration of ontogenetic and historical contextualism, and the functional dynamic between processes of selection, optimization, and compensation that is involved in successful/adaptive development).

Thus, to pursue the scholarship involved in life-span development theory, Baltes and his colleagues employed a set of theoretical ideas both general to RDS-based models and, as well, unique within their specific theoretical model. To illustrate how Baltes and his colleagues used the theoretical propositions summarized in Table 10.1 to study development across the life span, consider the interest in understanding the structure of gain and loss integrations across ontogeny. To appreciate the character of the developmental process involved across the life span in these integrations, Baltes and colleagues (1998) saw it necessary to investigate four dimensions of changing person–context relations: (1) an age-related reduction in the quantity and quality of biology-based resources as individuals age; (2) an age-correlated increase in the quantity and quality of culture needed to generate increasingly higher levels of growth; (3) an age-associated, biology-based loss in the efficient use of cultural resources; and (4) a relative absence of "old-age-friendly" cultural resources. This example illuminates the use by Baltes and colleagues (1998) of ideas both common to members of the developmental systems theoretical family (e.g., in regard to life-span changes in plasticity) and specific to life-span theory (e.g., the thorough integration of gain/loss influences across the breadth of the life span).

Given the unique and important role played in life-span developmental theory of propositions specific to this instance of RDS-based theories, it is useful to discuss these features of the conceptual repertoire of this perspective in more detail. In particular,

Table 10.1 Family of Theoretical Propositions Characteristic of Life-Span Developmental Theory

Life-span development
Ontogenetic development is a lifelong process. No age period holds supremacy in regulating the nature of development.

Life-span changes in the dynamic between biology and culture
With age and certainly after adulthood, there is a growing gap between biological potential and individual-cultural goals. This gap is fundamental to ontogenesis as the biological and cultural architecture of life is incomplete and inevitably results in loss of adaptive functioning and eventually death.

Life-span changes in allocation of resources to distinct functions of development: growth versus maintenance versus regulation of loss
Ontogenetic development on a systemic level involves the coordinated and competitive allocation of resources into distinct functions: growth maintenance including recovery (resilience), and regulation of loss. Life-span developmental changes in the profile of functional allocation involve a shift from the allocation of resources to growth (more typical of childhood) toward an increasingly larger and larger share allocated to maintenance and management of loss.

Development as selection (specialization) and selective optimization in adaptive capacity
Development is inherently a process of selection and selective adaptation. Selection is due to biological, psychological, cultural, and environmental factors. Developmental advances are due to processes of optimization. Because development is selective and because of age-associated changes in potential, compensation is also part of the developmental agenda.

Development as gain–loss dynamic
In ontogenetic development, there is no gain without loss, and no loss without gain. Selection and selective adaptation are space-, context-, and time-bound. Thus, selection and selective adaptation imply not only advances in adaptive capacity but also losses in adaptivity for alternative pathways and adaptive challenges. A multidimensional, multidirectional, and multifunctional conception of development results from such a perspective.

Plasticity
Much intraindividual plasticity (within-person variability) is found in psychological development. The key developmental agenda is the search for the range of plasticity and its age-associated changes and constraints.

Ontogenetic and historical contextualism as paradigm
In principle, the biological and cultural architecture of human development is incomplete and subject to continuous change. Thus, ontogenetic development varies markedly by historical–cultural conditions. The mechanisms involved can be characterized in terms of the principles associated with contextualism. As an illustration: Development can be understood as the outcome of the interactions (dialectics) between three systems of biological and environmental influences: normative age-graded, normative history-graded, and non-normative (idiosyncratic). Each of these sources evinces individual differences and, in addition, is subject to continuous change.

Toward a general and functionalist theory of development: the effective coordination of selection, optimization and compensation
On a general and functionalist level of analysis, successful development, defined as the (subjective and objective) maximization of gains and minimization of losses, can be conceived of as resulting from collaborative interplay among three components: selection, optimization, and compensation. The ontogenetic pressure for this dynamic increases with age, as the relative incompleteness of the biology- and culture-based architecture of human development becomes more and more pronounced.

Note. From "Life-Span Theory in Human Development," by P. B. Baltes, U. Lindenberger, and U.M. Staudinger (1998). In W. Damon (Series Ed.) and R. M. Lerner (Volume Ed.),*Handbook of Child Psychology: Vol. 1. Theoretical Models of Human Development* (5th ed.), p. 1043. New York: Wiley. Copyright © 1998 by Wiley. Reprinted with permission.

it is useful to review the ideas of Baltes and his colleagues regarding ontogenetic and historical contextualism and the concepts of selection, optimization, and compensation. The former instance of the propositions of life-span developmental theory serves as an important conceptual bridge to life-course models of human development, whereas the latter instance is associated closely with action-theoretical accounts of human development.

Ontogenetic and Historical Contextualism as Paradigm

To illustrate the specific theoretical contributions of the life-span developmental perspective, it is useful then to first discuss how Baltes and his colleagues integrated individual ontogeny and the historical context of human development. Baltes and colleagues (1998, 2006) emphasized that individuals obviously live within contexts, and that these settings create both opportunities for and limitations to individual development. The macrostructural features of these contexts include attributes such as social class, ethnicity, roles, age-based passages, and historical periods. Specification of the impact of such contextual features on individual development is a goal of the sociological analysis of the life course (e.g., Elder, 1998; Elder et al., 2015).

Baltes and his colleagues offered a tripartite model for integrating ontogenetic development with features of historical change, and, thus, for synthesizing sociological approaches (e.g., Elder, 1998; Elder et al., 2015) and individual-psychological approaches (Hetherington & Baltes, 1988) to understand the bases of development. The three components of this model involve: (a) normative, age-graded influences; (b) normative, history-graded influences; and (c) non-normative, life-event influences (Baltes et al., 1980).

Normative, age-graded influences consist of biological and environmental determinants that are correlated with chronological age. They are normative to the extent that their timing, duration, and clustering are similar for many individuals. Examples include maturational events (changes in height, endocrine system function, and central nervous system function) and socialization events (marriage, childbirth, and retirement).

Normative, history-graded influences consist of biological and environmental determinants that are correlated with historical time. They are normative to the extent that they are experienced by most members of a *birth cohort* (i.e., a group of people who share a common year of birth or, somewhat more broadly, a group born during a specific historical period such as the "baby boom" generation of the immediate post-World War II period). In this sense, normative, history-graded events tend to define the developmental context of a given birth cohort. Examples include historic events (wars, epidemics, and periods of economic depression or prosperity) and sociocultural evolution (changes in sex-role expectations, the educational system, and childrearing practices). Both age-graded and history-graded influences *covary* (change together) with time.

Non-normative, life-event influences—the third system—are not directly indexed by time since they do not occur for all people, or even for most people. Rather, they are idiosyncratic in development (Baltes et al., 2006). Thus, when non-normative influences do occur, they are likely to differ significantly in terms of their clustering, timing, and duration. Examples of non-normative events include experiences such as illness, divorce, promotion, or death of a spouse.

In short, variables from several sources, or dimensions, influence development. As such, life-span developmental theory stresses that human development is *multidimensional* in character. In other words, variables from many dimensions (ones ranging from biology-related, age-graded events through the normative and non-normative events constituting history) are involved in developmental change. As I have emphasized, in life-span developmental theory the relationships among the sources of contextual influence—normative, age-graded; normative, history-graded; and non-normative, life-events—are seen as *dynamic* (i.e., *reciprocal*). They may continually change, and each influence has an effect on the others and, in turn, is affected by them.

Baltes and colleagues (1980) suggested that these three sources of influence exhibit different profiles over the life cycle. Normative, age-graded influences are postulated to be particularly significant in childhood and again in old age, whereas normative, history-graded influences are thought to be more important in adolescence and the years immediately following it; this difference is thought to reflect the importance of the sociocultural context as the individual begins adult life. Finally, non-normative, life-event influences are postulated to be particularly significant during middle-adulthood and old age, promoting increasing divergence as individuals experience unique life events. Such a perspective is consonant with a concept of multidirectional development across the life span.

Baltes and colleagues (2006) concluded that, in regard to the links between human ontogeny and historical change, each individual's development involves the close interweaving of age-graded, history-graded, and non-normative life events. They go on to note that:

> None of these patterns of biologically and environmentally based influences is likely to operate independently from the other. They are part of biocultural co-construction with reciprocal and modifying influences. Such a focus on the dynamics of biocultural co-construction also makes explicit the lack of full predictability of human development as well as the boundedness individuals experience as they engage in the effort to compose and manage their lives . . . And finally, such a focus on contextualism places individual development in the context of the development of others. It is not surprising, therefore, that life-span researchers have easily embraced concepts such as collaborative development, collaborative cognition, or interactive minds.
>
> (Baltes et al., 2006, p. 587)

It is important to note the interrelation between the ideas of Baltes and his colleagues regarding the connection of (a) ontogenetic and historical-contextual changes, with (b) the role of individuals as active agents in their own development. This focus on the interrelation of the action of individuals on their context and of the context on individuals leads to a consideration of another key, and specific, feature of life-span developmental theory: the use of concepts of selection, optimization, and compensation within a systematic and overall theory of development across the life span.

The Baltes, Baltes, and Freund Selective Optimization with Compensation (SOC) Model

Margret M. Baltes, Paul B. Baltes, Alexandra Freund, and their colleagues (Baltes & Baltes, 1980, 1990; M. Baltes, 1987; M. Baltes & Carstensen, 1996, 1998; Baltes, 1987, 1997; Baltes, Dittmann-Kohli, & Dixon, 1984; Carstensen, Hanson, & Freund, 1995; Freund & Baltes, 1998, 2000, 2002; Gestsdóttir & Lerner, 2008; Lerner, Freund, De Stefanis, & Habermas, 2001; Marsiske, Lang, Baltes, & Baltes, 1995; McClelland et al., 2015) developed a model of development—selective optimization with compensation (SOC)—that is aimed at providing a systemic view of human development across the life span. The SOC model integrates the theoretical propositions summarized in Table 10.1 and, in turn, provides a general theoretical framework for the understanding of *processes of developmental regulation*—that is, the processes through which individual↔context

Margret M. Baltes

Alexandra Freund

relations occur across the life span or, in other words, the processes through which individuals affect their context at the same time that their context is influencing them (Brandtstädter, 1998, 1999; Lerner, 1982; Lerner & Walls, 1999; Schneirla, 1957). The SOC model seeks to depict these person↔context processes across the different levels of analysis (ranging from the micro to macro levels) involved in the relational developmental system, across different domains of functioning (such as cognitive functioning or social relations), and across the entire life span.

In the SOC model, the three processes posed to be central to developmental regulation—selection, optimization, and compensation—are conceptualized to involve goal-selection, goal-pursuit, and goal-maintenance. Selection, optimization, and compensation need to be considered conjointly to adequately describe and understand development. For the sake of clarity, however, it is helpful to introduce each of the processes separately.

Based on the assumption that constraints and limitations of (internal and external) resources (e.g., stamina, money, and social support) are present throughout the entire life span (e.g., Baltes, 1997), the SOC model posits that the range of alternative developmental options (goals, ecologies, and domains of functioning) needs to be delineated (this action is termed "elective selection"). Selection gives direction to development by orienting and focusing resources (i.e., means to reach goals) on specific domains of functioning and preventing diffusion of resources.

In order to actually achieve higher levels of functioning in the selected domains, optimization needs to take place. Optimization denotes the process of acquiring, refining, coordinating, and applying goal-relevant means or resources in the selected domains (or goals). Typical instances of optimization are (a) the acquisition and training of specific goal-related skills (e.g., engaging in weight-training to increase athletic ability, or working on test-taking skills in order to improve performance on an exam important for college admission); and (b) persistence in goal-pursuit.

Optimization describes a process of developmental regulation addressing the growth aspect of development, as optimization is geared toward achieving higher levels of functioning. Throughout the life span, however, development can be characterized as multidirectional, that is, as encompassing growth and decline (Baltes, 1997; Baltes et al., 2006; Brandtstädter & Wentura, 1994; Labouvie-Vief, 1981). The SOC model addresses the aspect of decline and management of loss by stressing the importance of *compensation.* When loss or decline in goal-relevant means threatens a person's level of functioning, it is necessary to invest resources or apply means geared toward the maintenance of functioning (Carstensen et al., 1995; Staudinger, Marsiske, & Baltes, 1995; Marsiske et al., 1995). Prototypical instances of compensation are the substitution of means or the use of external aids (e.g., help of a tutor to improve academic test performance in a particular subject area).

It is, of course, possible that compensatory efforts fail (e.g., weight-training fails to improve one's strength and athletic ability sufficiently to gain a place on a high school football team) or that their costs outweigh their gains (e.g., one learns that to have a chance to make the team, the amount of training required would mean that no other activity—including studying or social interaction—would be feasible). In such circumstances, an individual's more adaptive response to loss or decline in goal-related means might be to restructure his or her goal hierarchy (e.g., place academics ahead of football), to lower his or her standards (e.g., settle for being a junior varsity football player), or to look for new goals, an action termed "loss-based selection" (e.g., here one could make weight-lifting, or body-building, a goal instead of a means). This component of selection is functionally different from elective selection in that it occurs as a response to loss, most likely leading to different motivational and affective consequences (e.g., Shah, Higgins, & Friedman, 1998).

To illustrate how the SOC model may be useful in depicting the actual behaviors of people across their lives, Table 10.2 provides some examples of actions associated with selection, optimization, and compensation that are derived from the biographies of a famous athlete, scientist, and performing artist. In turn, Table 10.3 presents the specific actions individuals may take in regard to either elective or

Table 10.2 Selective optimization with compensation: biographical examples

Source	Selection	Optimization	Compensation
Athlete Michael Jordan (Greene, 1993)	Focused only on basketball in youth excluding swimming and skating	Daily line drills and upper body training	Reliance on special footwear to deal with chronic foot injury
Scientist Marie Curie (Curie, 1937)	Excluded political and cultural activities from her life	Spent a fixed number of hours daily in isolation in her laboratory	Turned to the advice of specific colleagues when encountering scientific problems that were beyond her expertise
Concert pianist Rubinstein (Baltes & Baltes, 1990)	Played smaller repertoire of pieces in late life	Practiced these pieces more with age	Slowed performance before fast movements (ritardando) to heighten contrast

Note. From "Life-Span Theory in Human Development," by P. B. Baltes, U. Lindenberger, and U. M. Staudinger (1998). In W. Damon (Series Ed.) and R. M. Lerner (Vol. Ed.), *Handbook of Child Psychology: Vol. 1. Theoretical Models of Human Development* (5th ed.), p. 1058. New York: Wiley. Copyright © 1998 by Wiley. Reprinted with permission.

Table 10.3 Selection, optimization, and compensation (SOC) embedded in an action-theoretical framework (after P. Baltes, M. Baltes, Freud, & Lang, 1995)

Selection (goals–preferences)	Optimization (goal-relevant means)	Compensation (means–resources for counteracting loss–decline in goal-relevant means)
Elective selection	—attentional focus	—substitution of means
—specification of goals	—effort–energy	—neglect of optimizing other means
—evolution of goal system (hierarchy)	—time allocation	—increased effort–energy
	—practice of skills	—increased time allocation
—contextualization of goals	—acquiring new skills–resources	—activation of unused skills–resources
—goal-commitment	—modeling successful others	—acquiring new skills–resources
	—motivation for self-development	—modeling successful others who compensate
Loss-based selection goal(s)		—focusing on most important
—search for alternate goals		—use of technical aids
—reconstruction of goal hierarchy		—use of assistance/help/therapy
—adaptation of standards		

Note. This specification reflects the specific explication of Baltes' general theoretical orientation, that is, from the point of view of action theory.

Source: From "Life-Span Theory in Human Development," by P. B. Baltes, U. Lindenberger, and U. M. Staudinger (1998). In W. Damon (Series Ed.) and R. M. Lerner (Vol. Ed.), *Handbook of Child Psychology: Vol. 1. Theoretical Models of Human Development* (5th ed.), p. 1056. New York: Wiley. Copyright © 1998 by Wiley. Reprinted with permission.

loss-based selection, optimization, or compensation. It is relevant to note that the terms used by Baltes and colleagues (2006) in this table reflect actions that may be conceptualized as consistent with what I shall discuss as "action theory."

In sum, selection refers to the development of preferences or goals, the construction of a goal hierarchy, and the commitment to a set of goals or domains of functioning. Optimization denotes the investment of goal-related means in order to achieve higher levels of functioning. Compensation refers to the process involved in maintaining a given level of functioning in the face of loss or decline in goal-related means. As already noted, although it is possible to differentiate these components of SOC, successful development encompasses their coordinated integration (Freund & Baltes, 2000; Marsiske et al., 1995). For instance, optimization efforts most likely only lead to higher levels of functioning when they are focused on a delineated number of domains of functioning instead of diffused among many domains. Similarly, selection per se does not ensure high achievement if no goal-relevant means are applied (e.g., wanting to have high peer status but using no means—neither athletic nor academic success—to attain it).

Finally, the adaptiveness of compensation needs to be seen in the context of the entire goal-system (e.g., "How many other goals are there that need resources for optimization?" and "How important, relative to other goals, is a threatened goal?") and the availability of resources. It does not appear to be adaptive to put a lot of one's resources into a relatively unimportant domain of functioning at the cost of having to neglect more important goals (Freund, Li, & Baltes, 1999).

Figure 10.1 provides an illustration of the life-span character of the SOC model. The figure depicts the reciprocal connections among the components of this instance of a relational developmental system (and note, for instance, the structural similarity between the Baltes et al. figure and Figure 9.3, which depicted the Lerner and Lerner PYD model). The figure also presents the ideas of Baltes and his colleagues about the developmental bases of selection, optimization, and compensation processes and, in

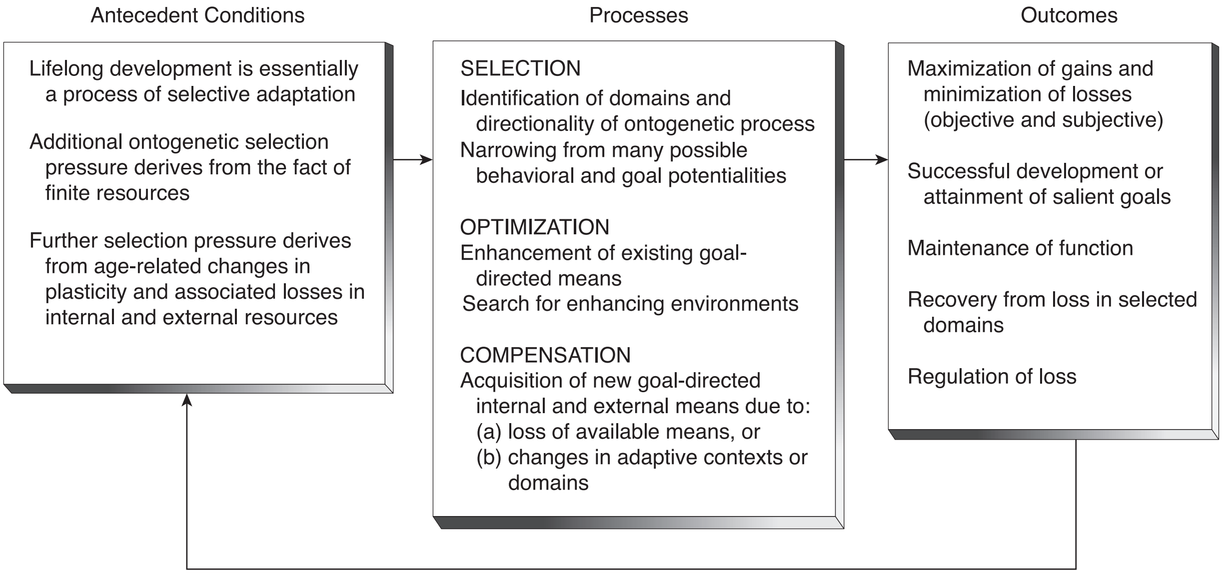


Figure 10.1 The life-span model of selective optimization with compensation. The essentials of the model are proposed to be universal, but specific phenotypic manifestations will vary by domain, individual, sociocultural context, and theoretical perspective.

Source: Baltes et al., 1998, p. 1055.

turn, their developmental outcomes. The role, then, of the SOC model within the frame of the goals that are pursued by life-span developmental theory is clear. The actions depicted in the SOC model enable individuals to engage their contexts in ways that promote their positive development across the life span.

Conclusions Regarding Life-Span Developmental Theory

Life-span developmental theory constitutes a conceptually rich and empirically productive instance of an RDS-based theory. The breadth and depth of the sets of ideas of Baltes and his colleagues offer a singularly creative means to understand the dynamic links between individuals and contexts. These relations underscore the changing character of plasticity across the life span and enable individuals to play an active role throughout their lives in promoting their own, positive development (again, compare the Baltes et al. model and the Lerner and Lerner model, discussed in Chapter 9, in regard to the process through which individuals promote their own positive development).

The conceptual integrations involved in life-span developmental theory span levels of organization ranging from biology through culture and history and, as such, provide a means to achieve another sort of integration, one related to the five levels of analytic work pursued by life-span developmental scientists. That is, life-span developmental theory provides a means to synthesize into discussions of the course of human life other instances of RDS-based theories, those spanning a range of interests from more micro, individual-level (e.g., psychological) interests to more macro, social institutional and historical interests.

For instance, the theoretical propositions of life-span developmental theory provide an integration of models associated with historical contextualism and the individual actions taken by people seeking to pursue their immediate and long-term goals within the context of the actual ecologies of their lives. In other words, life-span developmental theory provides a means to see the integrative relevance of individual action, of the institutional/sociological setting of the life course, and of the broad ecology of human development.

Accordingly, I now discuss theories associated with each of these other domains of RDS metatheory. I proceed from the micro (action-theory perspective) to the macro, that is, the life-course and the bioecological perspectives. However, as perhaps implied by the label attached to the latter approach, and consistent with the fused character of the levels integrated within the relational developmental system, my discussion of the bioecological model will return the discussion to the linkage between micro and macro levels of organization in human development.

ACTION THEORIES OF HUMAN DEVELOPMENT

Scholarship pertinent to the nature of human plasticity within RDS-based theories suggests that developmental *regulation*—processes that govern, or that moderate, the content and the pace, duration, and magnitude of the relations involving the actions of individuals on their contexts and the actions of contexts on individuals (i.e., processes of dynamic person↔context relations)—should be a key focus of inquiry in the study of human development. Action theory (Brandtstädter, 1998, 1999, 2006; Brandtstädter & Lerner, 1999) is an exemplar of an approach that is focused on these relational processes. For example, I have noted that one key instance of this theoretical approach is the Baltes and Baltes (1990) selection, optimization, and compensation (SOC) model. Table 10.3 displayed the components of the SOC model by emphasizing the actions involved in regulating people's goal-related behaviors.

The focus on such self-regulative actions, on the ways that the "individual is both the active producer and the product of his or her ontogeny . . . [and thus on] self-regulative loops that link developmental changes to the ways in which individuals, by action and mentation, construe their personal development" (Brandtstädter, 2006, p. 516), is the essence of action perspectives about human development. Thus, the central feature of action theories is isomorphic with a key idea in RDS-based theories (see

Ford & Lerner, 1992), that of individuals acting as producers of their own development (Lerner, 1982; Lerner & Busch-Rossnagel, 1981a, 1981b; Lerner & Walls, 1999). As is also illustrated by life-span developmental theory, this emphasis on the role of the active individual as an agent in his or her own development exists for other instances of developmental systems theory. In fact, this central use of the idea of individual action as a source of the person's own development arises because of the importance, underscored in action theory, of the link between developmental regulation and human plasticity.

Regulation and Plasticity in Human Development

Across their ontogeny, humans actualize a rich potential for cognitive and behavioral plasticity (Lerner, 1984). As discussed in Chapter 9, the evolutionary gains in complexity (anagenesis) that underlie human plasticity have come "at a price," however, that is, neotenous development (Gould, 1977); in other words, there is ontogenetically protracted development of humans' eventually high-level cognitive and behavioral capacities. Other organisms, whose nervous systems have lower ratios of association-to-sensory fibers (A/S ratios; Hebb, 1949), are relatively more stereotyped in their eventual, final level of ontogenetic functioning (Schneirla, 1957). That is, as discussed in Chapter 7, stimulus input is more highly correlated with behavioral output in organisms with lower A/S ratios than is the case with organisms with higher A/S ratios. These organisms are adapted to ecological niches where they can survive and reproduce despite the fact that their behavior is strictly regulated by their context. Their relatively low level of ontogenetic plasticity (and their low A/S ratio) solves the problem of the regulation of organism↔context relations and, thus, of adaptation (Hebb, 1949; Schneirla, 1957). For humans, however, the situation is quite different. As discussed by Heckhausen (1999):

> The relative dearth of biologically based predetermination of behavior gives rise to a high regulatory requirement on the part of the human individual and the social system. The social and cultural system and the individual have to regulate behavior so that resources are invested in an organized and focused way, and that failure experiences lead to an improvement rather than to a deterioration of behavioral means.
>
> (p. 8)

For humans, then, the complexity of their nervous systems and the multiple levels of their contexts mean that there is no one necessarily adaptive relation between context and behavior; what behaviors are requisite for adaptation are uncertain. As a consequence, whereas plasticity affords vast variation in behavior, the evolutionary status of humans means that the selection of adaptive options, from within the array of behaviors available to them, constitutes the key challenge in human development. Thus, according to Heckhausen (1999):

> Selectivity and proneness to failure as basic challenges both result from the extensive variability and flexibility of human behavior. Other nonprimate species are far more programmed in terms of their repertoire of activities and behavioral responses to the environment, with more instinct-driven behavior and substantially more constrained behavioral options. Humans, in contrast, have evolved with the ability to adapt flexibly to a great range of environmental conditions, and in particular with the ability to generate new systems of behavior.
>
> (p. 7)

Similarly, Brandtstädter (1999) indicated that:

> A basic evolutionary feature that makes possible—and at the same time enforces—cultural and personal control of ontogeny is the great plasticity and openness of development . . . These features of human ontogeny imply adaptive potentials as well as vulnerabilities, and they have concomitantly evolved with mechanisms to cope with the latter. The capacities to create, maintain, and enact culture, and to plot the "trajectory of . . . life on the societal map" (Berger, Berger, & Kellner, 1967, p. 67), are rooted in this coevolutionary process. Generally, developmental plasticity is already implicated in the notion of

> culture, as far as this notion connotes the cultivation of some process that is open to modification and optimization.
>
> (p. 46)

In essence, then, the regulation by individuals of their relations with their complex and changing physical, social, cultural, and historical context is the key problem for successful development across life (Baltes et al., 1998, 1999, 2006). Arguably, the understanding of the system involved in linking individuals and contexts becomes the essential intellectual challenge for developmental science. Indeed, as noted in my earlier discussion in this chapter of the approach that Baltes and his colleagues pursued in order to understand the cultural embeddedness of gain–loss processes, as the biological underpinnings of human behavior recede in ontogenetic significance as people traverse their post-reproductive years, the need for humans to intentionally draw on either individual-psychological or collective (e.g., cultural) resources (means) to promote their successful development becomes both increasingly salient and, as well, the necessary target of life-span developmental analysis (Baltes & Baltes, 1990; M. Baltes & Carstensen, 1998).

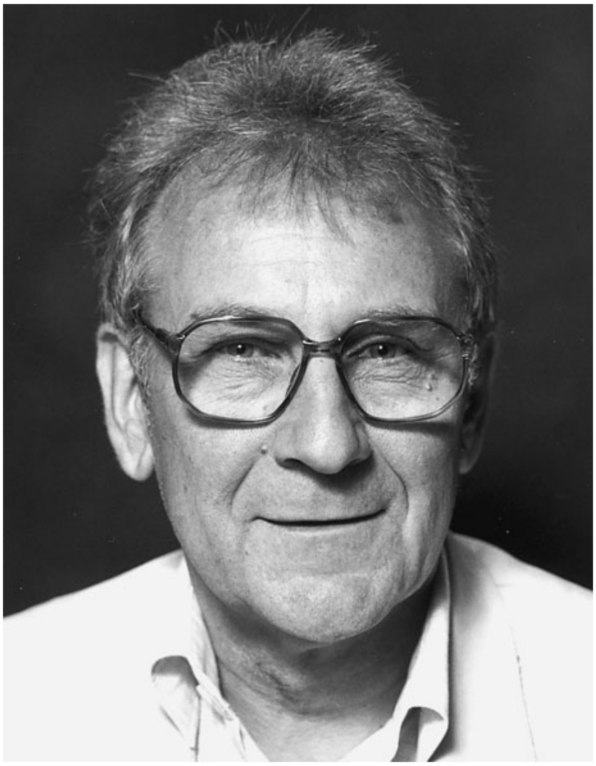
Accordingly, to understand development as conceived of within a dynamic, RDS-based perspective and, centrally, to appreciate the role of a person's own contributions to this development, focus should be placed on the role of an individual's actions in regulating the course of engagement with the context and in fostering constancy and change (in actualizing plasticity) across life. In the theoretical and empirical scholarship associated with this action-theory perspective, the work of Jochen Brandtstädter has been foundational in framing and advancing the key conceptual issues in this instance of RDS-based theories.

The Contributions of Jochen Brandtstädter

Brandtstädter (2006) conceptualizes actions as a means through which individuals affect their contexts and, through the feedback resulting from such actions, organize their ideas about their contexts and themselves. As a consequence of this understanding, individuals then develop a set of "guides" (i.e., motivations (e.g., intentions and goals), or regulators) for or of future actions. The outcome of this reciprocal, "action–feedback–self-organization–further action" process is, to Brandtstädter (1998, 1999), human development. Thus, action constitutes the "engine" of development and, as such, of person↔context relations. As Brandtstädter (2006) explained:

> Through action, and through experiencing the consequences of our actions, we construe representations of ourselves and of our material, social, and symbolic environments, and these representations guide and motivate activities, which shape and influence our behavior and personal development . . . Action thus forms development, and development forms action . . . The central tenet of an action-theoretical perspective thus holds that human ontogeny, including adulthood and later life, cannot be understood adequately without paying heed to the self-reflective and self-regulative [bases of] . . . personal development. This should not imply that individuals are the sole or omnipotent producers of their biographies. Just like any other type of activity, activities related to personal development are subject to cultural, sociohistorical, and physical constraints that lie partly or even completely outside one's span of control but decisively structure the range of behavioral and developmental options. Action-theoretical perspectives on development must therefore consider not only the activities through which individuals try to control their development over the life course, but also the nonpersonal or subpersonal forces that canalize such activities.
>
> (pp. 516–517)

Accordingly, Brandtstädter emphasizes the role of individuals as producers of their own development and, as such, conceives of action as both a dynamic means through which individuals regulate their linkages with their contexts and a basis for the development of the self (see too Baltes, 1998, and Table 10.1). Indeed, it is the self—the person who reflects on his or her own intentions, goals, and interests and who understands, therefore, who he or she



Jochen Brandtstädter

is at the moment and who he or she would like to be at some future time—that acts to regulate relations with the context.

Thus, akin to other members of the RDS-based theoretical family, action theory as conceptualized by Brandtstädter (1988, 1999, 2006) emphasizes the fused, dynamic relations between individuals and their contexts as constituting the core process of human development. However, as is the case with other members of this theoretical family, Brandtstädter's action theory also has attributes specific to it. One key distinctive feature is the central role given to the intentionality of the individual in moderating the exchanges occurring between person and context, and the changes in development deriving from these intention-based exchanges. That is, as Brandtstädter (2006) explained, other instances of developmental systems theory have placed primary emphasis on "development as the result of person–environment transactions, rather than as a target area of intentional action; in other words, the relation between action and development has been conceptualized primarily as a functional rather than an intentional one" (p. 826). Although Brandtstädter (2006) noted that the functional emphasis is appropriate for the early portions of the life span (e.g., the initial infancy period), by the end of this initial phase of life, and certainly thereafter across the life span, intentionality must play a central role in moderating the individual's interactions with his or her physical and social world.

Given, then, this central role of the individual's intentions within the person↔context fusions involved in the developmental system, Brandtstädter (2006) defined actions as

> behaviors that (a) can be predicted and explained with reference to intentional states (goals, values, beliefs, volitions); (b) are at least partly under personal control, and have been selected from alternative behavioral options; (c) are constituted and constrained by social rules and conventions or by the subject's representation of these contextual constraints; and (d) aim to transform situations in accordance with personal representations of desired future states.
>
> (pp. 523–524)

For instance, Brandtstädter (2006) explained that this individual↔context self-regulatory process involves the integration in life of two opposing processes (see Overton, 2015a, in regard to the analytic moment of the integration of opposites): *tenacious goal pursuit* and *flexible goal adjustment*. Akin to what Piaget (1950, 1970) described as the process of assimilation, tenacious goal pursuit involves acting on the context in order to maintain progress in reaching a goal. In turn, flexible goal adjustment, akin to what Piaget (1950, 1970) described as the accommodation process, involves changing goal-directed actions in the face of contextual constraints.

Contextual and Developmental Constraints on Action

Accordingly, to Brandtstädter (e.g., 1999, 2006), actions link the person dynamically to his or her social context. The plasticity of the individual enables him or her to regulate what he or she does, to and in the context, and to circumscribe to some extent the influence of the context on him or her. Of course, as Brandtstädter noted within his definition of action, the person's control over the context is not limitless. There are both individual and contextual constraints on action. First, on the individual level, human plasticity is, of course, not infinite (e.g., Lerner, 1984, 2012, 2015a) and, in any case, the level of plasticity of which a human is eventually capable must be developed (actualized) across ontogeny (Hebb, 1949; Schneirla, 1957). In turn, some features of the context are simply not under the control of individual actors (e.g., as much as a person might rage against the storm, no person controls the course of a tornado or a hurricane). In addition, the social and cultural context imposes rules on actions.

In fact, Brandtstädter (1998, 1999, 2006) sees two types of such rules (i.e., constitutive and regulative rules). In regard to regulative rules, Brandtstädter (2006) noted that:

> Personal action is regulated by a variety of cultural prescriptions and restrictions, and these can be more or less formal and explicit (laws, norms, customs, social expectations, etc.). Such rules delimit situationally defined zones and margins of action. The limits imposed by regulative rules, however, are not rigid; cultural laws, in contrast to natural laws, can be violated.
>
> (p. 524)

In turn, in regard to constitutive rules, Brandtstädter (2006) indicated that:

> When considering acts or action episodes such as marrying, formulating an excuse, promising something, or taking a penalty kick, it is evident that such actions are not simply regulated, but, in a stronger sense, are constituted by rules . . . Through constitutive rules, certain types of action are linked inseparably to cultural institutions.
>
> (p. 524)

For instance, "without the system of constitutive rules called football, the behaviors of scoring, blocking, passing, and so on would not exist" (D'Andrade, 1984, p. 94).

The developmental capacities of the individual also constrain, or moderate, his or her interactions with the context and, especially in regard to Brandtstädter's emphasis on the centrality of intentions in developmental regulation, the person's changing cognitive capacities are particularly important in respect to possessing the ability to form intentions. For instance, Brandtstädter (2006) noted that:

> Development-related action presupposes particular representational capacities. The individual must have formed goals and standards for personal development, and must be able to evaluate the current situation with regard to these self-guides; furthermore, he or she must have acquired some knowledge about probable and possible courses of future development and means and strategies for attaining personally and socially desired outcomes. Moreover, specific regulatory competencies are required for enacting self-regulatory intentions and maintaining them over longer intervals. Personal concepts of actual, desired, and possible selves (i.e., representations of how and what an individual is, should be, could be, and would like to be) provide the motivational basis for such processes . . . These representations also change, and are socially expected to change in particular ways, over the life cycle.
>
> (p. 545)

As such, across life, the individual must balance tenacious goal pursuit and flexible goal adjustment to maintain adaptive individual↔context relations. This integration will, therefore, result in an individual's thriving (his or her positive development; Lerner et al., 2015) when these two facets of action are enacted appropriately in time and place.

Conclusions Regarding the Ideas of Brandtstädter

Brandtstädter's (e.g., 1999, 2006) action theory places central emphasis on an individual's intentions in his or her regulatory actions. These actions both reflect and propel development. As such, actions constitute the means through which the active individual, fused with his or her active context, actualizes his or her potential for plasticity in ways that develop, support, and elaborate the self. At the same time, Brandtstädter (1998, 1999, 2006) explained that the intentions of the self are limited in the developmental goals that can be actualized due to both individual and contextual constraints on plasticity.

Accordingly, Brandtstädter (1998) envisioned three dimensions of scholarship that should be pursued in order to understand the dynamic relations between plasticity and constraints, a relation brought to the fore of conceptual attention by an action-theoretical perspective. That is, Brandtstädter (2006) recommended that:

> In analyzing the ontogeny of intentional self-development, three basic lines of development should be considered: (1) the development of intentional action in general and of cognitive and representational processes related to intentionality; (2) the formation of beliefs and competencies related to personal control over development; and (3) the development of the self (or self-concept) as a more or less coherent structure of self-referential values, beliefs, and standards that guides and directs self-regulatory processes.
> (p. 545)

Other action theorists have pursued theoretical and empirical agendas that correspond to the scholarly vision of Brandtstädter. In particular, Jutta Heckhausen (1999) has taken on the challenge of developing a program of work that addresses directly the issue of plasticity and constraints that is of concern in action theory.

Jutta Heckhausen's Life-Span Theory of Control

Heckhausen (1999) extended action models of human development in a theoretically creative and empirically productive way. Heckhausen and her colleagues developed a life-span theory of control (e.g., Heckhausen, 1999, 2003, 2011; Heckhausen, Dixon, & Baltes, 1989; Heckhausen & Krueger, 1993; Heckhausen & Schulz, 1995; Heckhausen & Wrosch, 2016; Schulz & Heckhausen, 1996). The theory and research associated with it describe how humans—and particularly adults, given her empirical interests—regulate their behavior in the face of (a) their enormous ontogenetic potential for plasticity and (b) the biological, sociocultural, and age-normative constraints on their flexibility, that is, on their creativity in finding the means to control their behavior in ways that are desired by them and that are optimal for healthy functioning.

Jutta Heckhausen

Heckhausen (1999) noted that, across human life, these biological and ecological constraints on humans provide a developmental scaffold, both channeling behavior and making the vast range of potential behaviors that could be generated more manageable for the individual and more likely to be associated with positive outcomes. Heckhausen

(1999) indicated how this dialectic between plasticity and constraints requires, on the one hand, selection of goals and investments of resources to reach them and, on the other, the compensation for failure when resource investments do not eventuate in successful (i.e., desired) outcomes.

Here, in regard to selection and compensation for failure and losses, Heckhausen's model converges with other models of successful development, such as the selection, optimization, and compensation (SOC) model (Baltes & Baltes, 1990; Freund & Baltes, 1998, 2002). However, Heckhausen's theory is distinctive in regard to the conceptualization of the way in which the dialectic between plasticity and constraints occurs.

Consistent with the two-process conception of Brandtstädter (2006), involving tenacious goal pursuit and flexible goal adjustment, Heckhausen (1999) explained that the enactment of this dialectic also involves two processes. In her theory, the two processes are *primary control*, behaviors aimed at influencing the ecology in order to alter the context to *fit* the needs and goals of the individual, and *secondary control*, internal (e.g., cognitive) processes that are used to minimize losses of and failures in control and/or to maintain or even expand existing primary control capacities. Heckhausen (1999) argued that, across life, primary control striving takes primacy in human behavior, although the potential for primary control shows an inverted U-shape across ontogeny—involving a marked increase in childhood and decline in old age. As a consequence of this age-related developmental loss, individuals need to compensate by using secondary control in later life.

Heckhausen (1999) demonstrated the applicability of her theory of control through a series of conceptual presentations about: the different types of primary and secondary control strategies that may be used across life (i.e., selective primary control, selective secondary control, compensatory primary control, and compensatory secondary control); developmental goals as basic units of action; the way in which control occurs in different developmental ecologies (e.g., those differentiated by age and those that vary as a consequence of historically unprecedented sociocultural change—for instance, involving transformations in East Germany in the 1990s, after the fall of the Berlin Wall); and social comparisons as prototypic strategies for developmental regulation (see too Heckhausen, 2003, 2011; Heckhausen & Wrosch, 2016).

A particularly intriguing conceptual proposal Heckhausen (1999) made relates to the concept of developmental deadlines. Developmental deadlines mark the age-graded transition from favorable to unfavorable opportunities for attaining important developmental goals. Once the deadline is passed, the goal has to be given up. In what she termed an *action-phase* model of developmental regulation, Heckhausen (1999) proposed adaptively sequenced control strategies which serve pre-deadline urgency and post-deadline goal-disengagement. This model of deadline-related action cycles may be applied to a broad range of important developmental pursuits across the life span (e.g., regarding the purported "biological clock" that some individuals associate with childbearing).

In addition to these conceptual contributions, Heckhausen (1999) has conducted several studies that bring empirical evidence to bear on the salience for regulation across life of primary and secondary control, and underscores the role of both facets of control for understanding life-span adaptability, and, particularly, adaptation during portions of ontogeny (e.g., old age) wherein behavioral means may become increasingly compromised due to age-associated losses.

In sum, Heckhausen's (1999, 2003, 2011) theory of life-span control provides at least three important contributions. First, and reflecting key ideas as well in life-span developmental theory (Baltes et al., 1998, 1999), Heckhausen's (1999) ideas extend action-theoretical conceptions by offering an innovative and empirically supported set of ideas about the gains and losses that characterize humans' attempts to regulate their behavior across life.

Second, Heckhausen's (1999, 2003, 2011) theory complements Brandtstädter's integrative, two-process action model, and provides another useful two-process model of actions that constitutes a rich set of ideas for further research about control across the life span and in regard to different ecological settings. For example, Heckhausen offered several ideas that pertain to portions of life other than adulthood. To illustrate, she explained why future scholarship "should systematically investigate individuals' regulatory behavior in life-course settings that are rich in . . . long-term/short-term or interdomain conflicts" (Heckhausen, 1999, p. 195), and, thus, suggested the importance of her model for elucidation of the purported conflicts in (or at least the connections among) the parent–family and friend–peer group social relationships that occur during adolescence.

In turn, she also suggested the importance of studying "developmental regulation in individuals who lead exceptional lives" (Heckhausen, 1999, p. 195). In fact, an illustration of the use of action theory for such an analysis was provided by Baltes and colleagues (1998) in regard to the SOC model, and has been previously presented in Table 10.2. Given the person–context relational focus of action theory, such scholarship might focus on gifted or disabled children, adolescents, or adults, on the one hand, or on individuals embedded in non-normative contexts such as abusive homes, inner-city gangs, or war-torn villages, on the other.

A third contribution of Heckhausen's (1999, 2003, 2011) theory, related to the second contribution, is that her ideas provide a compelling theoretical frame and a research base for the use of ideas about control in applied scholarship aimed at understanding the bases of successful and failed attempts to regulate behavior across life. Her presentation may motivate developmental scientists to identify the ways in which processes of primary and secondary control may be promoted across life to enable individuals to optimize their plasticity in manners that eventuate in successful development.

Conclusions Regarding the Ideas of Heckhausen

Action theory provides a means to understand the dynamic relations between individuals and their contexts that exist across the life span. From the point in ontogeny when cognitive development is sufficiently advanced to form intentions and/or to devise strategies for primary or secondary control, and then for the rest of the life span, individuals may influence their social world that is influencing them.

However, as both Brandtstädter and Heckhausen emphasize, the actualization across the life span of an individual's capacity for plasticity sufficient to enable him or her to act in ways realizing his or her intentions or exerting his or her primary control is not limitless. Human action is plastic but it is also constrained. Human development is relatively plastic; it is not absolutely plastic. The features of, and the historical changes in, the physical and social contexts of human development represent a source of behavior across life that may constrain or circumscribe human development. In addition, of course, the social system within which the person lives may promote particular directions—particular courses or trajectories—of development across life.

Indeed, this linkage between individual action and the social context is the essence of the process of developmental regulation of concern in action theory. In addition, such regulation is a core interest within RDS-based theories in general and, as well, within the instances of such theories discussed in this chapter. For instance, the paradigmatic linkage between ontogeny and historical-contextual levels is a key proposition in the life-span developmental theory of Baltes and his colleagues (e.g., see Table 10.1).

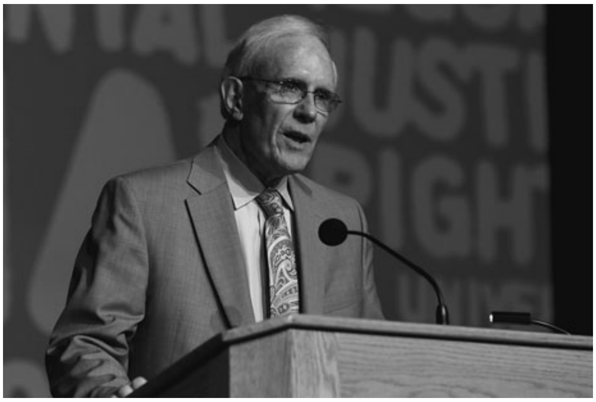
Accordingly, to understand the integrations among the levels of the relational developmental system that comprise the action context for human development, developmental scientists must include a discussion of the social system within which people develop and use a historical/contextual focus to specify the role of the social world within the developmental system. This social system approach to human development has been termed *life-course theory* and the scholarship of Glen H. Elder, Jr. has been central in understanding the importance of the life course in influencing the character of human development—the transitions in social situations or institutions involved in people's lives and the shaping of the trajectory of human life by its embeddedness in the institutions of society.

GLEN H. ELDER, JR. AND LIFE-COURSE THEORY

Glen H. Elder, Jr. (e.g., 1974, 1975, 1980, 1998, 1999; Elder & Shanahan, 2006; Elder et al., 2015) has been the major contributor to theory and research framed by what is termed the life-course theory of human development. As envisioned by Elder (1998), this theory is predicated on the following proposition:

> Human lives are socially embedded in specific historical times and places that shape their content, pattern, and direction. As experiments of nature or design, types of historical change are experienced differentially by people of different ages and roles . . . The change itself affects the developmental trajectory of individuals by altering their life course.
>
> (p. 969)

Thus, lives are embedded in time and place (Elder et al., 2015). There will be interindividual differences in intraindividual change as a consequence of people growing up at specific times in history and in specific locations at these historical points in time (e.g., Elder, 1974, 1980, 1999).



Glen H. Elder, Jr.

The life-course model of human development emerged over the last four-plus decades, based on theoretical and empirical contributions elaborating and testing this conception. As shown in Figure 10.2, this model is linked to:

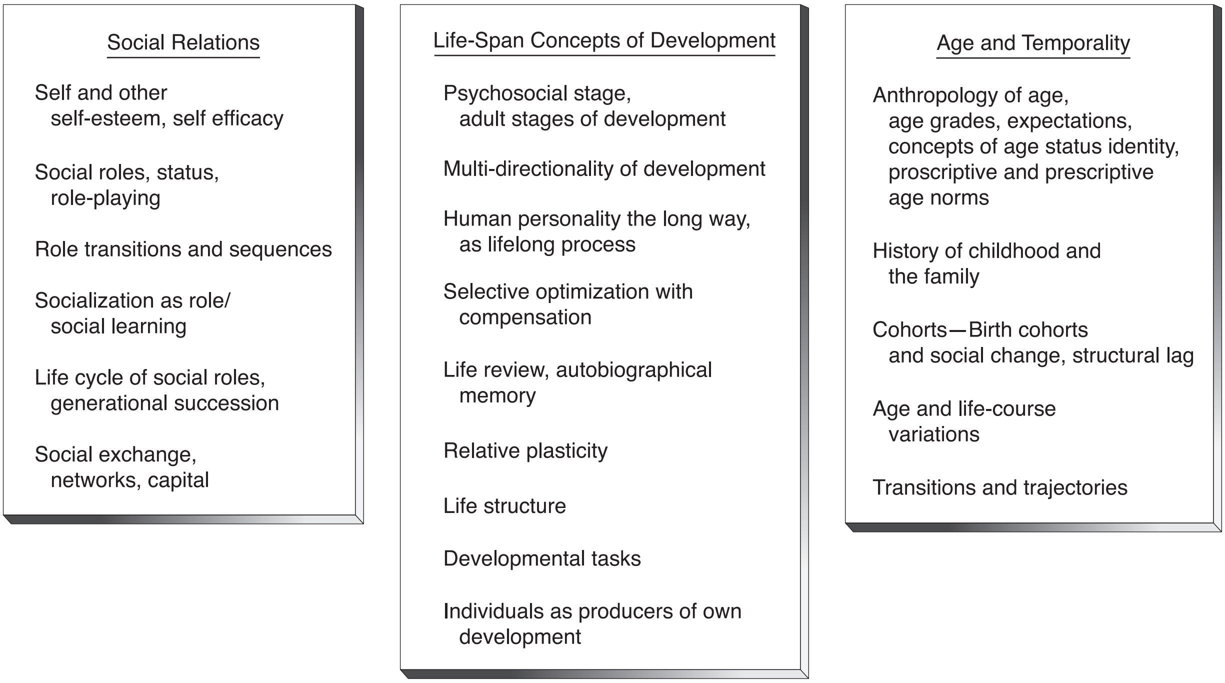


Figure 10.2 The emergence of life-course theory (1960s to present): research traditions and their concepts.

Source: Elder, 1998, p. 952.

1. Social relations, for example, involving scholarship about the study of self (i.e., as in action theory), social roles, role transitions (i.e., from student to worker, or from married with no children to parenthood), and the linkages among generations (i.e., involving children, parents, and the parents of the parents—or the grandparents). Simply, in Elder's (e.g., 1974, 1980, 1998; Elder & Shanahan, 2006; Elder et al., 2015) conception of the life course, linked lives coacting at specific times and places are the fundamental foci of life-course scholarship.
2. Life-span developmental theory, for instance, regarding the interest of Baltes and his colleagues (e.g., 2006) in understanding the integration of ontogenetic and historical contextualism.
3. Age and temporality, involving birth cohort, age, and the role of normative and non-normative historical variation (e.g., see Table 10.1).

Thus, as was the case in regard to action theory and life-span developmental theory, life-course theory shares common interests and intellectual bases with other members of the RDS-based theoretical family.

Elder (1998; Elder & Shanahan, 2006; Elder et al., 2015), in recounting these roots of life-course theory, explained that this perspective emerged, often in collaboration with life-span developmental theory, to meet three interrelated sets of conceptual and empirical challenges to devising an integrated and dynamic view of the entire course of human life. A first challenge was to extend the theoretical frame used to study people from a child-focused view that only emphasizes development or growth to one that is useful across the life span, and, thus, one encompassing development and aging, growth, and decline, or gain and loss. A second challenge was to employ such a frame to develop a set of concepts for depicting the changes and the organization of changes in humans' lives across their ontogenies and, as well, across different historical events and eras. The third challenge was to use these concepts about ontogeny and history to integrate human lives with the changing social contexts within which each individual and all birth cohorts live across their life spans.

Thus, in 2006, Elder and Shanahan explained that, in the 30 years prior to their writing, life-course theory grew in influence. This growth occurred because life-course theory effectively challenged traditional theory and research in developmental science. This challenge occurred by explaining the need to integratively consider the development of relations between person and context beyond the first two decades of life; by providing a new, dynamic, and developmental conceptualization linking social patterning and individuals' pathways across life; and by providing evidence that changes in society may moderate the developmental trajectories of individuals. Accordingly, they note that:

> Social theories of relationships and age converged in the 1960s with emerging concepts of life-span development to produce a theoretical orientation to the life course. More than any other theoretical initiative, life-span developmental psychology has responded to the first challenge by advancing a conceptual orientation on human development and personality across the life span. One result is a concept of ontogenetic development in which social structures and cultures merely establish behavioral settings. By contrast, life-course theory views human development as a coactive process in which sociocultural, biological, and psychological forces interact over time. Social structures and cultures are constituent elements in the developmental process. People play an important role in shaping their life course and development, although choices and initiatives are always constrained by social forces and biological limitations.
>
> (Elder & Shanahan, 2006, p. 706)

Accordingly, Elder (1998) believes that life-course theory adds value to life-span developmental theory through providing a productive means to address the second and third challenges to devising a dynamic model of the breadth of human development. He sees life-course theory as enabling scholars to move beyond an additive or simple interactional view of the social system within which development unfolds. Rather, life-course theory synthesizes the social systems into the actual constitution of the structures and functions comprising human development. The means through which this integration is seen to occur in life-course theory is one that is also emphasized in life-span developmental theory

(Baltes et al., 1998, 1999, 2006) and in action theory (e.g., Brandtstädter, 1998, 1999, 2006), that is, through the selective and intentional regulative actions of individuals, functioning as producers of their own development (Lerner, 1982; Schneirla, 1957).

Through this vision of the contribution of life-course theory, Elder (1998) believes that this perspective addresses the other two challenges involved in devising a comprehensive understanding of human development. Elder and Shanahan (2006) explained that:

> In concept . . . the individual life course provides a response to the second challenge, a way of thinking about life patterns or organization. Lives over time do not merely follow a sequence of situations or person–situation interactions. Instead, the life course is conceived as an age-graded sequence of socially defined roles and events that are enacted and even recast over time. It consists of multiple, interlocking trajectories, such as work and family, with their transitions or changes in states. People generally work out their life course in relation to established, institutionalized pathways and their regulatory constraints such as the curricula or tracks of a school, the age-graded expectations of a family, and the work careers of a firm or culture.
>
> The individual life course, developmental trajectories and transitions (as psychobiological continuities and change), and established pathways are important elements in the life-course study of human development. Any change in the life course of individuals has consequences for their developmental trajectory, and historical change may alter both by recasting established pathways. Thus, adultlike expectations for productive work in World War II communities were lowered toward childhood to enable young people to fill needed roles. By placing people in historical locations, life-course theory has oriented research to the third challenge, to understand the process by which societal changes make a difference in the primary world and development of children.
>
> (pp. 706–707)

Accordingly, because of its evolution in intellectual proximity to the also evolving theory of life-span development, the two perspectives have come to rely on very similar ideas about the dynamics of individuals and contexts in the development of the structures and functions comprising the course of human life. Moreover, through this collaboration Elder also draws on action-theoretical concepts (which, of course, life-span developmental theory does as well), and emphasizes the role of the active individual in the construction of life-course changes. Indeed, as a consequence of these linkages, Elder and Shanahan (2006) adopt a theoretical view of developmental process that is completely consistent with life-span developmental theory, with action theory, and with the other instances of RDS-based models that I have discussed in this chapter and previous ones. Elder and Shanahan (2006) stated that:

> Human development in life-course theory represents a process of organism–environment transactions over time in which the organism plays an active role in shaping its own development. The developing person is viewed as a dynamic whole, not as separate strands, facets, or domains such as emotion, cognition, and motivation.
>
> (p. 679)

Thus, as did Baltes and colleagues (1998), Elder saw human development as an interpersonally relational—a dynamically collaborative (Fischer & Bidell, 1998, 2006; Mascolo & Fischer, 2015; Rogoff, 1998), social—process. Hence, as is the case with all instances of RDS-based theories I have discussed, there are important commonalities among members of this theoretical family. In addition, each member of the family has specific theoretical features associated with it. In fact, the distinctive features of life-course theory are associated with the link that Elder (1998; Elder & Shanahan, 2006; Elder et al., 2015) draws between individual development and the social relationships within which the person's ontogeny is dynamically collaborative.

Constructing the Life Course

I have noted that Elder (1998; Elder & Shanahan, 2006; Elder et al., 2015) specified that the substantive roots of life-course theory lie in the integration of scholarship pertinent to life-span developmental theory, social relations, and age and temporality. As such, in meeting the challenge of developing a model that enables individual lives to be interrelated with their changing social settings, theorists elaborating a life-course perspective would be expected to draw on ideas from these three domains of scholarly influence. Elder (1998) explained that, in fact, such conceptual integration exists. He noted that there are four central principles in life-course theory. These are: "(1) the interplay of human lives and development with changing times and places; (2) timing of lives; (3) interdependence of human lives, including the relation between social and developmental trajectories; and (4) human agency in choice-making and actions" (Elder, 1998, p. 961).

Simply, then, linked lives in time and place constitute fundamental foci in life-course theory. In explaining these ideas, Elder (1998) indicated that:

> The first principle of historical time and place asserts that (1) the life course of individuals is embedded in and shaped by the historical times and places they experience over their life time. This principle also reflects the premise that developmental trajectories are changed by changing the life course. The extent to which this occurs depends in part on the nature of the change. The second principle of timing expresses the fundamental bond between age and time; that (2) the developmental impact of a life transition or event is contingent on when it occurs in a person's life. Social age, for example, refers to the age at which people enter and leave particular roles. Timing may also be expressed in terms of biological events and transitions, such as puberty, whether relatively early or late.
>
> The third principle . . . states that (3) lives are lived interdependently and that social and historical influences are expressed through this network of shared relationships. Social roles expose individuals to the stresses and strains of others, as well as to the possibility of social support. The fourth principle on human agency reflects an enduring premise of biographical studies on the constructionist role of individuals in shaping their life course . . . It states that (4) individuals construct their own life course through the choices and actions they take within the constraints and opportunities of history and social circumstances. The principle expresses the dynamic relation between people and social roles in life course theory. Social roles and situations are selected and shaped by people, but they also constrain behavior, as do internal forces.
>
> (pp. 961–962)

Accordingly, in Elder's (1998, Elder & Shanahan, 2006; Elder et al., 2015) view, the life course is constructed through the sorts of dynamic coactions—the person↔context regulations—that I have discussed as being of central interest within action-theoretical accounts of human development (e.g., Brandtstädter, 1998, 1998, 2006; Heckhausen, 1999, 2003, 2011). What Elder's (1998; Elder & Shanahan, 2006; Elder et al., 2015) view of the life course adds to this focus is the idea that the life course is constructed through the *simultaneous* contribution of (a) these coactions, (b) made by individuals dynamically coacting with other individuals, while (c) embedded in a context changing along three temporal dimensions: "life" or "ontogenetic" time (one's age from birth to death), "family" time (one's location within the flow of prior and succeeding generations), and "historical" time (the social and cultural system that exists in the world when one is born and the changing circumstances regarding this system that occur during one's life). That is, Elder (1998) pointed out that:

> The life course is age-graded through institutions and social structures, and it is embedded in relationships that constrain and support behavior. In addition, people are located in historical settings through birth cohorts and they are also linked across the generations by kinship and friendship . . . Both the individual life course and a person's developmental trajectory are interconnected with the lives and development of others.
>
> (pp. 951–952)

The postulation of a dynamic integration between an individual's regulatory actions and a social system constituted by the people, social institutions, and historical events that vary across these three temporal dimensions provides, for Elder (1998; Elder & Shanahan, 2006; Elder et al., 2015), a means to represent the life course of an individual. As such, Elder's vision results in a theoretical system of singular creativity and enormous value to RDS-based theories of human development. His theory merges within a given person the micro (ontogenetic biological, behavioral, and psychological) and macro (social system) levels of organization that are held to be fused within RDS-based models (e.g., see the developmental systems theory design criteria discussed by Ford & Lerner, 1992, as summarized in Chapter 9 and in Overton, 2015a). In explanation of this model, Elder (1998) indicated that:

> A skeletal life course for a person can be mapped in the three-dimensional space of life, family, and historical time [Figure 10.3]. Historical time of birth, coupled with passage through the age structure, define particular life trajectories on the grid of history and age. Persons born in 1920, 1940, and 1960 follow the age gradient, though divergent paths may arise from the variable relation between age and events/roles. Historical events, such as war and economic recession, may alter the correlation between life events and age, or change their temporal arrangements—for example, full-time employment may come after first marriage in the lives of World War II servicemen. Another source of variation is the unstable path of family time.
>
> (p. 949)

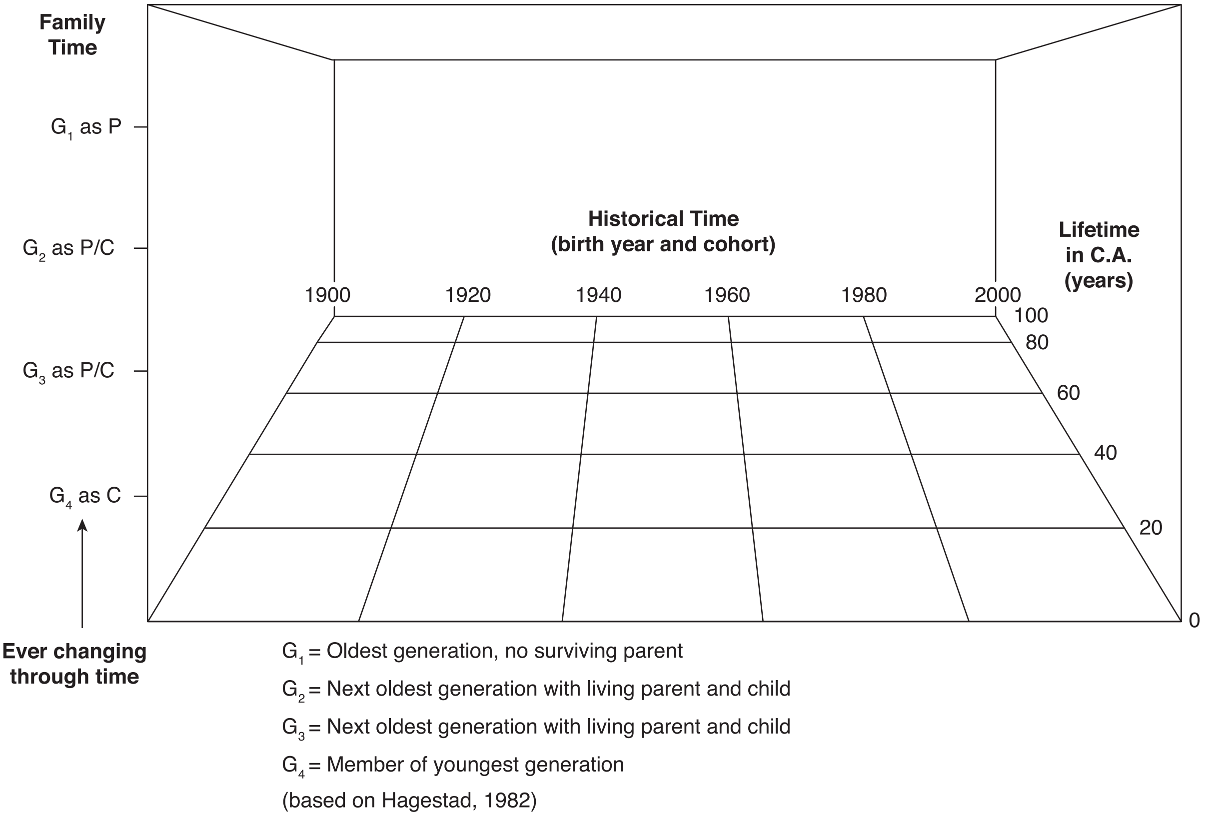


Figure 10.3 Life-course trajectories in three-dimensional space: life, family, and historical time.

Source: Elder, 1998, p. 949.

One's birth year is the point of entry into the social system depicted in Figure 10.3. As a person enters this system, the life course is constructed by his or her location along the three temporal dimensions presented in the figure. However, this location may rapidly change. Elder (1998) points out that the number and pattern of the generations can vary considerably even within one year. In addition, Elder and Shanahan (2006) explained that:

> Birth year indicates historical time, and chronological age acquires the meanings of social timing and life stage. Birth cohorts provide a link between historical change and the life course . . . Birth year or date of entry into a system (such as school graduation or marriage) locates the individual according to historical time and related social changes: With age peers in the cohort, this person is exposed to a particular segment of historical experience as he or she moves across the sequence of age-graded roles.
>
> (p. 675)

In short, then, the model presented in Figure 10.3 constitutes a means to integrate an individual's life into the social system from the moment of his or her birth. Birth provides for his or her immediate membership into (a) a familial flow of generations, and (b) a society that exists at a given point in history with its extant but evolving set of institutions, roles, and socially defined life pathways. Accordingly, Elder and Shanahan (2006) explained that:

> The individual life course and its relation to developmental trajectories represent a common meeting ground for life-course theory and developmental science, with its perspective on individual functioning that emphasizes the dynamic interplay among processes that operate across time frames, levels of analysis, and contexts . . . Building on advances since the 1960s, life-course theory has uniquely forged a conceptual bridge between developmental processes, the life course, and ongoing changes in society based on the premise that age places people in the social structure and in particular birth cohorts.
>
> (p. 679)

Conclusions about the Ideas of Elder

Life-course theory adds a significant and unique dimension to the set of concepts associated with RDS-based theories. Building on the ideas associated with other members of this theoretical family—and, most prominently, life-span developmental theory and, to a somewhat lesser but nevertheless significant extent, action theory—Elder's (1998; Elder & Shanahan, 2006; Elder et al., 2015) view of the life course provides a dynamic means to integratively bring the social system into the ontogeny of individuals.

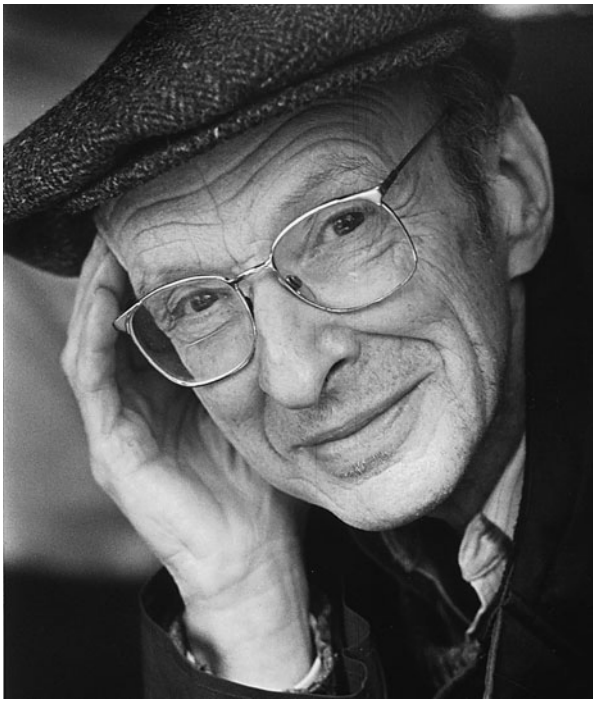
There is always the danger that, when scholars whose training or interests are in a discipline (such as sociology, anthropology, or history) more macro than those disciplines having focal units of analysis involving individuals (e.g., psychology), or even units more molecular than individuals (e.g., genes, as may be the case in some branches of biology), the course of an individual life may be interpreted in "sociogenic" terms, that is, by exclusive reference to the institutions of society, the rules of culture, or the events of history. Just as a developmental scientist would wish to avoid the alternative conceptual "danger," of a psychogenic or a biogenic interpretation of the life span of a person, such a sociogenic view of human development would not be theoretically desirable (in regard, at least, to the perspective of human development advanced by RDS metatheory) or empirically supportable. At best, an incomplete view of the course of life would be provided by a sociogenic appeal to macro institutional influences, in the same way that an incomplete picture of human life would be derived from a psychogenic appeal to, for instance, cognitive functioning in and of itself, or from a biogenic reliance on genes. Just as Overton (2015a) has cautioned scholars of human development to "avoid all splits," developmental scientists should offer a similar warning: Avoid all interpretations of human development that are based on the hegemony of one discipline over all others.

The enormous significance of Elder's formulation of life-course theory, then, is that he is able to weave the importance of macro, social system influences into the development of individuals in a manner that is neither disciplinarily "isolationist" (or hegemonic) nor simply additive. Elder's scholarship is an exemplar of the relationism, the multilevel fusions, that define an RDS-based perspective. He brings the social system to human development, *not* as a context for development but—in the essence of what is sought for in RDS-based theories—as part of the very constitutive fabric of human ontogeny.

Elder provides a standard against which other theorists interested in a non-reductionist, synthetic view of development may measure the quality of their contributions. There is at least one scholar whom I am certain Elder and I would agree met this standard: Urie Bronfenbrenner. For more than a half-century, Bronfenbrenner provided a vision for—and a rich theoretical and empirical literature supportive of—the seamless integration of all levels of organization within the ecology of human development.

URIE BRONFENBRENNER'S BIOECOLOGICAL MODEL OF HUMAN DEVELOPMENT

In his 1979 book, *The Ecology of Human Development*, Urie Bronfenbrenner explained the importance for human development of the interrelation of several ecological levels, conceived of as nested systems. As noted in earlier chapters, Bronfenbrenner described four such levels: The *microsystem* is the setting within which the individual is behaving at a given moment in his or her life. The *mesosystem* is the set of *microsystems* constituting the individual's developmental niche within a given period of development. The *exosystem* is composed of contexts that, although not directly involving the developing person (e.g., the workplace of a child's parent), nevertheless has an influence on the person's behavior and development (e.g., as may occur when the parent has had a stressful day at work and, as a result, has a reduced capacity to provide quality caregiving to the child). Finally, the *macrosystem* is the superordinate level of the ecology of human development; it is the level involving culture, macro institutions (such as the federal government), and public policy. The *macrosystem* influences the nature of relations within and among all other levels of the ecology of human development.



Urie Bronfenbrenner

Bronfenbrenner's (1979) formulation had a broad impact on the field of human development, promoting considerable interest through the 1980s in the role of the ecological system in texturing the life course of individuals. Yet, by the end of that decade and into the 1990s, Bronfenbrenner indicated that he was not pleased by the nature of his contribution to either theory, research, or policy applications pertinent to enhancing the ecology of a child's life to promote his or her positive development. For instance, in 1989 Bronfenbrenner observed that:

> Existing developmental studies subscribing to an ecological model have provided far more knowledge about the nature of developmentally relevant environments, near and far, than about the characteristics of developing individuals, then and now . . . The criticism I just made also applies to my own writings . . . Nowhere in the 1979 monograph, nor elsewhere until today, does one find a parallel set of structures for conceptualizing the characteristics of the developing person.
>
> (p. 188)

In my experience, few scholars have the intellectual humility to announce to their colleagues—*in print, especially*—that their prior work was deficient. Bronfenbrenner was, during this portion of his career and through the remainder of his life, one of the most prominent and deservedly acclaimed developmental scientists in the world. His statement involved more than humility, in my view. It involved courage. The statement also made a very significant substantive point. Bronfenbrenner believed, as do other theorists drawn to RDS-based ideas about human development, that *all* levels of organization involved in human life are linked integratively in the constitution of the course of individual ontogeny. His 1979 book made an enormous contribution to such a conception of human development, through giving scholars conceptual tools to understand and study the differentiated but integrated levels of the context of human development. However, Bronfenbrenner recognized that this theory would be incomplete until he included in it the levels of individual structure and function (biology, psychology, and behavior) fused dynamically with the ecological systems he described.

Accordingly, Bronfenbrenner and his colleagues (e.g., Bronfenbrenner, 2005; Bronfenbrenner & Ceci, 1993, 1994; Bronfenbrenner & Morris, 2006) worked to integrate the other levels of the developmental system, starting from biology, psychology, and behavior, into the model of human development he was formulating. The span of the levels he sought to synthesize in his model—biology through the broadest level of the ecology of human development—accounts for the label, *bioecological*, which he attached to the model. In short, Bronfenbrenner (2005; Bronfenbrenner & Morris, 2006) sought to bring the features of the developing person into the ecological system he elaborated.

Thus, as Bronfenbrenner describes it, the defining properties of the model that emerged from this scholarship involved four interrelated components:

1. The developmental *process*, involving the fused and dynamic relation of the individual and the context.
2. The *person*, with his or her individual repertoire of biological, cognitive, emotional, and behavioral characteristics.
3. The *context* of human development, conceptualized as the nested levels, or systems, of the ecology of human development he depicted (Bronfenbrenner, 1977, 1979, 2005; Bronfenbrenner & Morris, 2006).
4. *Time*, conceptualized as involving the multiple dimensions of temporality that Elder (1998; Elder & Shanahan, 2006; Elder et al., 2015) explained are part of life-course theory.

Together, these four components of Bronfenbrenner's formulation of bioecological theory constitute a process–person–context–time (or PPCT) model for conceptualizing the integrated developmental system and for designing research to study the course of human development. That is, Bronfenbrenner believed that just as each of the four components of the PPCT model should be included in any adequate conceptual specification of the dynamic, human development system, so too must research appraise all four components of the model to provide data that are adequate for understanding the course of human development.

Indeed, neither research nor theory could exclude the developmental process, the person and the context integrated by this process, or the changes across ontogeny that occur as a consequence of this process, and still hope to have a full depiction of the dynamics of development within the relational developmental system. Accordingly, in describing the PPCT model, Bronfenbrenner and Morris (2006) noted that Bronfenbrenner must explain the

> four principal components and the dynamic, interactive relationships among them. The first of these, which constitutes the core of the model, is Process. More specifically, this construct encompasses particular forms of interaction between organism and environment, called proximal processes, that operate over time and are posited as the primary mechanisms producing human development. However, the power of such processes to influence development is presumed, and shown, to vary substantially as a function of the characteristics of the developing Person, of the immediate and more remote environmental Contexts, and the Time periods, in which the proximal processes take place.
>
> (p. 796)

In regard to the three remaining defining properties of the model—person, context, and time—Bronfenbrenner and Morris (2006) noted that they give priority in their scholarship to defining the biopsychosocial characteristics of the "person," since, as noted by Bronfenbrenner in 1989, his earlier formulations of the model (e.g., Bronfenbrenner, 1979) left a gap in regard to this key feature of the theory. As a consequence, Bronfenbrenner and Morris (2006) noted in regard to these person characteristics, that:

> Three types of Person characteristics are distinguished as most influential in shaping the course of future development through their capacity to affect the direction and power of proximal processes through the life course. First, dispositions can set proximal processes in motion in a particular developmental domain and continue to sustain their operation. Next, bioecological resources of ability, experience, knowledge, and skill are required for the effective functioning of proximal processes at a given stage of development. Finally, demand characteristics invite or discourage reactions from the social environment that can foster or disrupt the operation of proximal processes. The differentiation of these three forms leads to their combination in patterns of Person structure that can further account for differences in the direction and power of resultant proximal processes and their developmental effects.
>
> (pp. 795–796)

Consistent with the integrative character of development systems theory, Bronfenbrenner and his colleagues point out that, when the characteristics of the person component of the bioecological model are expanded in this way, the result is a richer understanding of the context—the ecological system—with which the developing person is fused. Thus, as explained by Bronfenbrenner and Morris (2006):

> These new formulations of qualities of the person that shape his or her future development have had the unanticipated effect of further differentiating, expanding, and integrating the original 1979 conceptualization of the environment in terms of nested systems ranging from micro to macro . . . For example, the three types of Person characteristics previously outlined are also incorporated into the definition of the microsystem as characteristics of parents, relatives, close friends, teachers, mentors, coworkers, spouses, or others who participate in the life of the developing person on a fairly regular basis over extended periods of time.
>
> (p. 796)

Indeed, Bronfenbrenner redefines the character of the microsystem to link it centrally to what he regards as the "center of gravity" (Bronfenbrenner & Morris, 2006, p. 814)—the biopsychosocial person—within his theory as it has now been elaborated. That is, although, as in 1979, he sees the ecology of human development as "the ecological environment . . . conceived as a set of nested structures, each inside the other like a set of Russian dolls" (p. 3), he magnifies his conception of the innermost, microsystem structure within this ecology by incorporating the activities, relationships, and roles of the developing person into this system. That is, Bronfenbrenner (1994) noted that:

> A microsystem is a pattern of activities, social roles, and interpersonal relations experienced by the developing person in a given face-to-face setting with particular physical, social, and symbolic features that invite, permit, or inhibit, engagement in sustained, progressively more complex interaction with, and activity in, the immediate environment.
>
> (p. 1645)

What may be particularly significant to Bronfenbrenner in this expanded definition of the microsystem is that he includes not only the person's relations with other people in this level of the ecology but also the relations the person has with the world of symbols and language (with the semiotic system)—a component of ecological relationships that action theorists also believe is especially important in understanding the formulation of intentions, goals, and actions (cf. Brandtstädter, 1998, 1999, 2006). Bronfenbrenner and Morris (2006) noted that:

> The bioecological model also introduces an even more consequential domain into the structure of the microsystem that emphasizes the distinctive contribution to development of proximal processes involving interaction not with people but with objects and symbols. Even more broadly, concepts and criteria are introduced that differentiate between those features of the environment that foster versus interfere with the development of proximal processes. Particularly significant in the latter sphere is the growing hecticness, instability, and chaos in the principal settings in which human competence and character are shaped—in the family, child-care arrangements, schools, peer groups, and neighborhoods.
>
> (p. 796)

Finally, Bronfenbrenner noted that the emphasis on a redefined and expanded concept of the microsystem leads to the last defining property of the reformulation of his theory of human development. Bronfenbrenner and Morris (2006) indicated that

> the fourth and final defining property of the bioecological model and the one that moves it farthest beyond its predecessor—[is] the dimension of Time. The 1979 volume scarcely mentions the term, whereas in the current formulation, it has a prominent place at three successive levels: (1) micro-, (2) meso-, and (3) macro-. Microtime refers to continuity versus discontinuity in ongoing episodes of proximal process. Mesotime is the periodicity of these episodes across broader time intervals, such as days and weeks. Finally, Macrotime focuses on the changing expectations and events in the larger society, both within and across generations as they affect and are affected by, processes and outcomes of human development over the life course.
>
> (p. 796)

As I have noted, Bronfenbrenner and Morris (2006) indicated that the inclusion of a temporal dimension in the model draws on the work of Elder (e.g., 1974, 1980; Elder & Shanahan, 2006; Elder et al., 2015) in regard to the multiple dimensions of time that are involved in linking the ecology of human development (or the social system, in the terms of Elder, 1998; Elder & Shanahan, 2006) to individual development. Thus, as is the case in regard to the other instances of RDS-based theories that I discussed in this chapter, Bronfenbrenner's theory integrates ideas unique to his model with those associated with other members of the RDS-based theoretical family.

Conclusions about Bronfenbrenner's Bioecological Model

Bronfenbrenner's bioecological model is, in at least two senses, a living system (Ford & Lerner, 1992). First, the theory itself depicts the dynamic, developmental relations between an active individual and his or her complex, integrated, and changing ecology. In addition, the theory was itself developing across Bronfenbrenner's career (e.g., see Bronfenbrenner, 2005). Over the span of his career, he sought to make the features of the theory more precise and, as such, a more operational guide for PPCT-relevant research about the dynamic character of the human development process.

The bioecological model developed to include two propositions. Both of these sets of ideas promote a dynamic, person↔context relational view of the process of human development. As explained by Bronfenbrenner and Morris (2006), Proposition 1 of the bioecological model states that:

> Especially in its early phases, but also throughout the life course, human development takes place through processes of progressively more complex reciprocal interaction between an active, evolving biopsychological human organism and the persons, objects, and symbols in its immediate external environment. To be effective, the interaction must occur on a fairly regular basis over extended periods of time. Such enduring forms of interaction in the immediate environment are referred to as proximal processes. Examples of enduring patterns of proximal process are found in feeding or comforting a baby, playing with a young child, child–child activities, group or solitary play, reading, learning new skills, athletic

> activities, problem solving, caring for others in distress, making plans, performing complex tasks, and acquiring new knowledge and know-how. (p. 797)

Thus, in the first proposition, Bronfenbrenner emphasizes a theme found in the other instances of RDS-based models discussed in this chapter—the role of the active individual as an agent in his or her own development.

In fact, the idea of the contribution of the individual to the developmental process is also present in the second proposition of bioecological theory. That is, the second proposition of the model (Bronfenbrenner & Morris, 2006) specifies that:

> The form, power, content, and direction of the proximal processes effecting development vary systematically as a joint function of the characteristics of the developing person, the environment—both immediate and more remote— in which the processes are taking place, the nature of the developmental outcomes under consideration, and the social continuities and changes occurring over time through the life course and the historical period during which the person has lived. (p. 798)

As is evident from the two propositions, Bronfenbrenner regards proximal processes as the primary sources of development, an assertion that is compatible with the several versions of action theory discussed in this chapter (e.g., Baltes & Baltes, 1990; Brandtstädter, 1998, 1999, 2006; Heckhausen, 1999, 2003, 2011). That is, in all of the proximal processes described by Bronfenbrenner in the first proposition of the bioecological model, goal-selections, intentions, developing means to engage goals, the primacy of primary control, and the importance of compensatory behaviors and/or of secondary control may be involved. In turn, the propositions also point to the fusions across the developmental system described by Bronfenbrenner as providing the dynamism that enables the proximal processes to drive the relational developmental system.

Finally, as I have noted, the role of the individual, as an active agent in his or her own development, is central in the bioecological model. Bronfenbrenner and Morris (2006) underscored the central role of the individual in the PPCT model by explaining that:

> Characteristics of the person actually appear twice in the bioecological model—first as one of the four elements influencing the *form, power, content, and direction of the proximal process*, and then again as *developmental outcomes*—qualities of the developing person that emerge at a later point in time as the result of the joint, interactive, mutually reinforcing effects of the four principal antecedent components of the model. In sum, in the bioecological model, the characteristics of the person function both as an indirect producer and as a product of development (see Lerner, 1982, 2002; Lerner & Busch-Rossnagel, 1981b). (p. 798)

In sum, then, as has been the case in all of the instances of RDS-based theories discussed in this chapter and, as emphasized, at this writing, more than a half-century ago by Schneirla (1957), the active, developing individual is seen by Bronfenbrenner as a central force of his or her own development. This contribution to the process of development is made by a synthesis, an integration, between the active person and his or her active context. This individual↔context relation is integral in the other sample cases of RDS-based theories discussed in the rest of this chapter.

THELEN AND SMITH'S DYNAMIC SYSTEMS THEORY

Thelen and Smith (1998, 2006) noted that their version of developmental systems theory—which they termed dynamic systems theory—derives from both systems thinking in biology and psychology and the study of complex and nonlinear systems in physics and mathematics. They explained that, in its simplest sense, the idea of dynamic systems refers to changes among elements that are interrelated systemically. Although this idea can be extended more technically or formally, through specific mathematical equations, Thelen and Smith (2006) indicated that there are two key features of any physical or biological system:

Esther Thelen

Linda Smith

> 1. Development can only be understood as the multiple, mutual, and continuous interaction of all levels of the developing system, from the molecular to the cultural.
> 2. Development can only be understood as nested processes that unfold over many time scales, from milliseconds to years.
>
> (p. 258)

To Thelen and Smith (1998, 2006), dynamic systems theory can be applied to different species, age levels, or domains of development (e.g., from "molecular" patterns of motor functioning involved in walking or reaching to "molar" changes in cognition that may be gained through the integration of humans' actions on their context and the context's actions on them).

The Development of Novel Forms across Life

Thelen and Smith (1994, 1998, 2006) believe that dynamic systems theory affords understanding of what they regard as the defining feature of development: the creation of new forms. That is, consistent with my discussion in prior chapters of the definition of development, Thelen and Smith contend that the essence of those changes termed *developmental*—the property of change that enables one period of life to be designated as involving a distinct point in development—is qualitative discontinuity, emergence, epigenesis, or simply, novelty (e.g., Gottlieb, 1970, 1998; Raeff, 2016; Werner, 1948, 1957). Once such novelty has been described, however, a central explanatory issue becomes evident: "Where does this novelty come from? How can developing systems create something out of nothing?" (Thelen & Smith, 2006, p. 259).

As I discussed in Chapters 6 and 7, answers to these questions have been associated with nature, nurture, and interactionist perspectives. Not surprisingly, Thelen and Smith rejected both nature and nurture explanations and, implicitly, those conceptions of nature and nurture that maintained the split between them by asserting some additive or simple multiplicative relation between the two. Thelen and Smith (2006) noted that:

> The tradition we follow, that of *systems theories of biological organization*, explains the formation of new forms by processes of *self-organization*. By self-organization we mean *that pattern and order emerge from the interactions of the components of a complex system without explicit instructions*, either in the organism itself or from the environment. Self-organization—processes that by their own activities change themselves—is a fundamental property of living things. Form is constructed during developmental process.
>
> (p. 259)

For instance, we have discussed how both Gottlieb (e.g., 1997), in his view of the coactions that are involved in epigenesis, and Schneirla (1957), in his notion of circular functions and self-stimulation in ontogeny, provide examples of these self-organizational, autopoietic processes (see too Overton, 2015a; Witherington & Lickliter, 2016).

In turn, Thelen and Smith (1998, 2006) drew on evidence from embryology and morphology that indicates how highly complicated structural patterns arise within dynamic systems *not* from information specifically coded in genes but, instead, from simple initial conditions. For instance, they explained that neither the spots of leopards nor the striped tails of raccoons are derived from genes for these bodily features. Rather, these features are constructed during development when specific chemical and metabolic attributes of these animals—each one mutually facilitating and constraining the others—spontaneously organize themselves into patterns (Thelen & Smith, 2006; see too Lickliter, 2016; Lickliter & Honeycutt, 2015; Witherington & Lickliter, 2016).

Similarly, behavioral characteristics and patterns can emerge in development without the requirement of specific genetic coding for them, as is held in theories that rely on split concepts of nature and nurture, such as models within the field of behavior genetics (e.g., Plomin, 2000; Plomin et al., 2016), as forwarded in concepts such as instinct (e.g., Lorenz, 1937a, 1937b, 1937c, 1965), or as found in evolutionary developmental psychology (e.g., Bjorkland, 2015, 2016; Bjorkland & Ellis, 2005). The processes that produce developmental change are those associated with the probabilistic-epigenetic view of organism↔context relations associated with the work of Schneirla, Gottlieb, Tobach, Lehrman, and others. For instance, Thelen and Smith (2006, p. 264–265) pointed to the work of Zing-Yang Kuo (1967, 1970, 1976) as exemplifying this dynamic coaction view of the development of novel forms of behavior across the life span:

> Ontogenesis of behavior is a process of modification, transformation, or reorganization of the existing patterns of behavior gradients in response to the impact of new environmental stimulation; and in consequence a new spatial and/or serial pattern of behavior gradients is formed, permanently or temporarily ("learning") which oftentimes adds to the inventory of the existing patterns of behavior gradients previously accumulated during the animal's developmental history . . . Thus, in every stage of ontogenesis, every response is determined not only by the stimuli or stimulating objects, but also by the total environmental context, the status of anatomical structures and their functional capacities, the physiological (biochemical and biophysical) condition, and the developmental history up to that stage.
>
> (Kuo, 1970, p. 189)

In short, then, Thelen and Smith (1998, 2006) drew on the evidence provided by the scholarship of embryologists, and by comparative psychologists taking a probabilistic-epigenetic perspective, to assert that the basis for novelty in development arises from the integrated relation of intraorganism and extraorganism levels of organization—and not from either genetic or environmental "instructions" for such change.

The Dynamics of the Developmental System

The probabilistic-epigenetic character of the developmental process meant, to Thelen and Smith (1998, 2006), that the duality, or split (Overton, 2015a), between individual and context, or between structure and function, should be eliminated from scientific discourse (see too Lerner, 2016, in which a comparable recommendation is made). In the view of Thelen and Smith (2006), contextual levels of organization (e.g., culture) do not just support the course of development, they "are the very stuff of development itself" (p. 266; see too Raeff, 2016). Thelen and Smith (2006) explained that the essential difference between the version of the RDS-based theoretical perspective they favor

> and more individual-centered approaches is that the levels are conceptualized as *more* than just interacting; instead they are seen as integrally fused together. Behavior and its development are melded as ever-changing sets of *relationships* and the history of those relationships over time. (p. 267)

Thelen and Smith (1998, 2006) believed that, because of this fusion, developmental scientists must reject linear systems of causality wherein there is a direct, unidirectional line from an antecedent, "causal" event or structure (e.g., the possession of a gene) to a consequent behavior (e.g., a specific motor behavior, personality attribute, or cognitive capacity, that is, where "X" → "Y"). In the place of such linear notions of causality, RDS-based theories suggest a configural view of causality (Ford & Lerner, 1992; Overton, 2015a), wherein bidirectional relations within and across fused levels of organization change interdependently across time. One depiction of such a coactional, configural view of causality within the developmental system has been offered by Gottlieb (e.g., 1998) and is shown in Figure 10.4, which in part inspired Figure 2.1 (see Chapter 2).

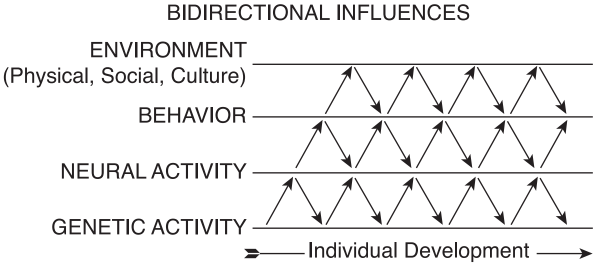


Figure 10.4 A developmental-psychobiological systems framework.

Source: From Gilbert Gottlieb (1991). *Individual development and evolution: The genesis of novel behavior.* Copyright © 1991 by Oxford University Press, Inc. Reprinted by permission.

The causal system presented in Figures 10.4 and 2.1 coincides with the view of causality conceived of by Thelen and Smith (1998, 2006), wherein the key features of developing individuals—self-organization, nonlinearity, openness, stability, complexity, wholeness, the emergence of novelty, and change—are produced by the fused, multilevel influences comprising the developmental system. The outcomes of development—"form"—are products of this process of bidirectional relations (Thelen & Smith, 1998, p. 586).

Thelen and Smith (1998, 2006) indicated that the key feature of dynamic systems is that the many heterogeneous parts of the system (e.g., the different cells, tissues, and organs within the individual and the various individuals, institutions, and physical features of the context of any person) are free to combine in a virtually infinite number of ways. Theoretically at least, there is no limit to the actual number of combinations that might occur. However, in actuality, the patterns of relations that are seen are limited in number. As I discussed in regard to the notion of "relative plasticity," the relations among the multiple parts of the system are sources of constraints as well as of variability. Thus, because of this relative plasticity, an order (a pattern) emerges from the complexity of the system as, through the relations within the system, the system organizes itself.

Thelen and Smith (1998, 2006) explained that order emerges from disparate parts because human development is an *open system*, that is, a system wherein energy is taken into the system and is used to increase order within it (see too Prigogine, 1978, 1980). Such a system stands in contrast to a *closed system*, wherein there is no infusion of energy into the system. In that an open, human development system increases its organization over time, it exists in "violation" of the second law of thermodynamics (Brent, 1978; Prigogine, 1978). As previously discussed, according to this law, a system changes in the direction of greater disorganization, termed *entropy.* However, some systems—open ones—can show *negentropy*, that is, changes in the direction of greater organization (cf. Raeff, 2016; Werner, 1957).

The Nobel laureate chemist, Ilya Prigogine (1978), has shown that negentropic change can occur because an open system draws energy from its context to increase its internal order. Prigogine demonstrated that such use of energy within an open system does result in an overall dissipation in order outside of it, that is, in the universe as a whole; thus, in the broader system there is an increase in entropy, and the second law of thermodynamics is not violated.

Thelen and Smith (1998, 2006) noted that when the parts involved in an open system interrelate in a nonlinear manner (e.g., as shown in Figures 10.4 and 2.1), integration (i.e., a pattern, an organization, and structural relations) emerges. Such integration enables the system to be described via reference to fewer dimensions, or parameters, than was the case at the beginning of the development of the system. For instance, as already discussed, Werner's (1948, 1957) concept of orthogenesis is an example of a general principle of systematic change in developing organisms wherein the globality of the individual's organization is changed through the emergence of hierarchic integration. In turn, Thelen and Smith (1998, 2006) noted that the integrative variables that emerge within an open system to reduce its dispersion and increase its organization, or pattern, may be termed either *collective variables* or *order parameters.*

The emergence of such collective variables not only reduces the theoretically infinite number of combinations within a dynamic (open) system to some much smaller actual subset but, in so doing, the integration reflected by the collective variables provides continuity and stability within the system. As Thelen and Smith (2006) explained:

> The system "settles into" or "prefers" only a few modes of behavior. In dynamic terminology, this behavioral mode is an *attractor* state, because the system—under certain conditions—has an affinity for that state. Again in dynamic terms, the system prefers a certain location in its *state*, or *phase space*, and when displaced from that place, it tends to return there . . . All the initial conditions leading to a particular fixed point attractor are called *basins of attraction.*
>
> (pp. 272–273)

Thelen and Smith (1998, 2006) described one type of attractor, the *chaotic* attractor, that seems to be involved in many biological systems (e.g., involving changes in heart rate, the sense of smell, and motor movements during the fetal period; however, see Witherington & Heying, 2013, and below, in this chapter, for a critique of this point). Within dynamic systems, chaos describes a situation wherein the relations among the parts of a system seem random (i.e., lacking any pattern or order). However, when the time period used for viewing a state space is extended over a significantly long time period, nonrandomness—order—is evident. In fact, chaotic change is represented by highly elaborate geometric patterns (Gleick, 1987).

Stability and Change in Dynamic Systems

Thelen and Smith (1998, 2006) noted that in the study of human development the most important characteristic of an attractor is its relative stability, that is, the likelihood that the system will exist in a given state (or show a specific behavioral pattern) as compared to other ones. The presence of relative stability means that there is a higher statistical probability of one specific behavioral pattern than another and that, as well, if the system is dislodged from its preferred state it will return back to it. Moreover, the system will "work" to maintain the preferred state. Thus, in regard to the idea that continuity of behavior can be underlain by dynamic relations between the individual and the context (Cairns & Hood, 1983), the relative stability of a developmental system does not gainsay the fact that dynamic exchanges are occurring within it.

The relative stability of a system is related to the relative plasticity of the course of development. In my earlier discussion of plasticity and its linkage to continuity–discontinuity in development, I noted that although organisms—through their dynamic and integrated coactions with their context—maintain the capacity for systematic change across the life span (Baltes et al., 1999, 2006; Lerner, 1984), these same organism↔context relations constrain the variability in functional change that can be seen; as a consequence, plasticity—although ubiquitous—is relative, not absolute. Similarly, Thelen and Smith (1998, p. 626) observed that "adaptive systems live in quasi-stability; reliable enough to make predictions about what is appropriate in a context, but flexible enough to recruit different solutions if the situation changes."

The ontogenetic changes that exist in plasticity mean that, at advanced developmental levels, when the reserve capacity for plasticity has narrowed (Baltes, 1997; Baltes et al., 1999, 2006), change is still possible but a larger than previously necessary level of intervention would be required to produce it (Lerner, 1984; MacDonald, 1985). Similarly, Thelen and Smith (2006) noted that, "Very stable attractors take very large pushes to move them from their preferred positions, but they are dynamic and changeable nonetheless" (p. 274). In other words, the system is not fixed, with hardwired, immutable connections; rather it is *softly assembled.*

Such soft assembly is the essence of plasticity in human development and, to Thelen and Smith (1998, 2006), the defining feature of a dynamic view of development. The presence of soft assembly means that the concept that human development involves the functioning of permanent, immutable structures is not valid. Rather, developmental scientists must view the development of the person as involving a dynamic linkage between (a) the stability of the system, conceived of as the resistance to change among the collective states, and (b) the fluctuations around the stable states, changes that provide the functional source of novelty within the system.

Transitions in Systems

Fluctuations within the system, as well as changes from the context that impinge on the system, can alter the patterns of the system. In either case, the system will change in a manner that increases order, that enhances coherence. The parts of the system will coact, or "cooperate," in the terms of Thelen and Smith (2006, p. 271), in the occurrence of a "phase shift" or, in other terms, a "nonlinear phase shift." To illustrate, Thelen and Smith (2006) indicated that:

> For example, we can walk up hills of various inclines, but when the steepness of the hill reaches some critical value, we must shift our locomotion to some type of quadrupedal gait—climbing on all fours . . . In dynamic terminology, the slope change acted as a *control parameter* on our gait style. The control parameter does not really "control" the system in traditional terms. Rather, it is a parameter to which the collective behavior of the system is sensitive and that thus moves the system through collective states.
>
> (p. 275)

Thelen and Smith (1998) noted that the "disappearance" of the newborn stepping response (i.e., stepping movements made by the newborn when he or she is held upright), which occurs after a few months of life, occurs in relation to the gain in weight, and especially in body fat, during this period. As the infant's legs get heavier across these months, there is no corresponding increase in muscle mass. As a consequence, infants have difficulty lifting their legs—not because of a neuronal change within the brain that "suppressed" the reflex, but because they do not have the muscles to lift when in the biomechanically difficult upright position (Thelen & Smith, 1998, 2006). As I noted earlier in this chapter, Witherington and Heying (2013) critiqued this example in regard to its usefulness in depicting the fully coactional relational developmental system, represented for instance in Figures 10.4 and 2.1. They note that this example reflects a sort of "bottom-up" approach (in that the individual's actions and attributes are driving the changes in the system) and that integration with the opposite, "top-down" system properties needs to be added to fully reflect the individual↔context relations involved in the relational developmental system. However, although the example of body fat disposition may need some modification, the overall point of Thelen and Smith, that the coherence of the changing dynamic system involves patterns emerging through self-organization among components, remains important.

Time Scales within Dynamic Systems

The time frame for the phase shift involved in the infant stepping response involves several months within the early life of humans. One important temporal parameter of dynamic systems that is illustrated by this example is that the state of the system in regard to stepping when upright at a later time in ontogeny is related to the system state at the prior time. This temporal linkage is an example of the point that the condition of the system at any one point in time provides the basis for the condition of the system at the next immediate point in time.

Thus, in regard to the notion of successive change as being a core component of the definition of development, Thelen and Smith (1998, 2006) noted that there is always a successive character to change within a dynamic system; that is, the state of the system at Time 1 shapes the state of the system at Time 2, and the state of the system at Time 2 determines the state at Time 3, etc. Thelen and Smith (1998, 2006) noted, then, that dynamic systems are *reiterative*, that is, each state within the system is shaped by the prior state of the system.

Moreover, the time scale dividing the successive influences may vary considerably. Times 1, 2, and 3 may be divided (e.g., along the "X" axis of a graph) by seconds, days, weeks, months, years, etc. (Lerner, Schwartz, & Phelps, 2009). Nevertheless, the same sort of successive interdependency of states, and therefore the same linkages across time, will be evident, whether the state-to-state observational interval is months (as in the example of the infant stepping response) or years (as may be seen in regard to changes in IQ scores; Bloom, 1964). Thelen and Smith (1998, 2006) explained that there is, then, a self-similarity of the system across many different levels of temporal observation. They noted that, in regard to dynamic systems, time scales are said to be *fractal.*

However, because different components of the system have their own developmental course and, as a consequence, because the relations among components continuously change, the time scale used within developmental studies to observe the system, and make judgments about its stability or fluctuation, is critical. For example, in attempting to understand the connections between the state of the system in early infancy in regard to the presence and disappearance of the stepping response, appraisal of fat-to-muscle ratios across a monthly time parameter may be useful; however, if the interest is the emergence within the system of the ability to run efficiently, then neither such ratios nor a month-by-month perspective would be useful (Thelen & Smith, 1998, 2006). Instead, different system components (involving, for instance, the development of muscle coordination and lung vital capacity) and different time divisions (e.g., years) may be required to see the reiterative character of the system and the bidirectional influences across levels within it.

Conclusions about the Ideas of Thelen and Smith

Thelen and Smith (1998, 2006) offered a nuanced conception of the dynamic character of the human developmental system. Their theory underscored the important role of dynamic relations, fusions, in human development, and the centrality of plasticity—of softly assembled systems—in providing within-person variability across life and between-person differences in such life-span changes. Their theory, and the data they marshal in support of it (Thelen & Smith, 1994, 1998, 2006), thus highlights the active role of the individual as a central agent in his/her own development and fosters an integrative, holistic understanding of the individual and his/her context. They saw important and singular promise for their dynamic systems theory:

> Only a dynamic account captures the richness and complexity of real-life human behavior. The issue is not just how people learn to think in formal, logical, and abstract terms, but how they can do that *and* all the other things people do in this society: use tools, operate sophisticated machinery, find their way around, play sports and games, create art and music, and engage in complex

> social interactions. These activities require active perception, precisely timed movements, shifting attention, insightful planning, useful remembering, and the ability to smoothly and rapidly *shift* from one activity to another as the occasion demands. They happen in time and they recruit all the elements in the system. The challenge for developmentalists is to understand the developmental origins of this complexity and flexibility. Only dynamics, we believe, is up to the task.
> (Thelen & Smith, 1998, p. 626)

I agree with the appraisal of Thelen and Smith about the challenge that may be met by, and the potential benefits of meeting it through, the dynamic, RDS-based theory they forward. Other developmental scientists agree as well and, in addition, have advanced theories consonant with Thelen and Smith's theory (1998, 2006). One instance of such a theory has been formulated by David Magnusson who, over the course of several decades, contributed mightily to scholarship about developmental systems.


David Magnusson

MAGNUSSON'S HOLISTIC PERSON↔CONTEXT INTERACTION THEORY

David Magnusson, the renowned Swedish developmental scientist, has provided singularly creative and historically influential scholarly and professional leadership facilitating and integrating the research and applied activities of developmental scientists from across the world. Magnusson's theoretical formulations and research programs have emphasized the fundamental role of context in human behavior and development (e.g., Magnusson, 1995, 1999a, 1999b; Magnusson & Stattin, 1998, 2006). His intellectual vision includes a compelling conceptual rationale and substantive basis for internationally contextualized, comparative scholarship (e.g., Magnusson, 1995, 1999a, 1999b) and is built on four conceptual pillars: interactionism, holism, interdisciplinarity, and the longitudinal study of the person.

These themes emerge in Magnusson's theory, which stresses the synthesis, or fusion, of the person↔environment system. Magnusson sought to understand the structures and processes involved in the operation of this system and the way in which the individual behaves and develops within it. Given this integrative emphasis on person and context, Magnusson (1995) termed his theory a *holistic approach.* He stated that:

> The individual is an active, purposeful part of an integrated, complex, and dynamic person–environment (PE) system . . . Consequently, it is not possible to understand how social systems function without knowledge of individual functioning, just as individual functioning and development cannot be understood without knowledge of the environment.
> (Magnusson & Stattin, 2006, p. 401)

As did Thelen and Smith (1998, 2006), Magnusson saw the fusions among cognitive, biological, and behavioral subsystems, and their complex interplay with the levels of the environment within which they are embedded, as involving nonlinear and probabilistic relations. The probabilistic character of the changes occurring across the life span, and the fact that these changes take place in reciprocal interdependence with a multilevel, changing world, meant, to Magnusson (1995, 1999a, 1999b), as it did to Thelen and Smith (1998, 2006), that the course of human development is characterized by the emergence of "novel foci of individual functioning" (Magnusson & Stattin, 1998, p. 687).

Causality in Holistic Relations

As is seen also in respect to the theories of Schneirla (1957), Kuo (1976), Gottlieb (1997), and Thelen and Smith (1998, 2006), Magnusson regarded the cause of development as an outcome of the coactions of the components of the dynamic, person↔context system. This self-organizational source of developmental change stands in contrast to either the unidirectional, single source (nature or nurture), or additive or multiplicative conceptions regarding the causes of development.

Magnusson termed his theory the *modern interactionist perspective*, or the *holistic interactionist viewpoint*. Understandably, he used the terminology of his era. Magnusson's intent was to move beyond past ideas of nature–nurture relations that, although admitting that both domains were involved in development, nevertheless retained split conceptualizations. Split approaches to nature and nurture were reflected in the use of formulations of the idea of "weak interaction," that is, that variables from the two domains could be added together to "produce" development. Split approaches to nature and nurture were reflected as well in the idea of "moderate interaction," that is, that variables from the two domains are separate but could multiply in their influence on development, as in the "multiplication" that occurs in the analysis of variance statistic. However, in moderate interactions nature and nurture return to their separate, split, existence after they intersect.

To correct this split conception of nature and nurture, developmental scientists in the 1970s (e.g., Lerner, 1978), 1980s (e.g., Lerner & Kauffman, 1985), and 1990s (e.g., see the chapters in Lerner, 1998) used the term *dynamic interaction* to connote a strong, fused relation between nature and nurture. However, by the end of the first decade of the twenty-first century, several developmental scientists (e.g., see chapters in Damon & Lerner, 2006 and, in particular, chapters in Overton & Molenaar, 2015) sought to avoid confusion that might occur with the continued use of the interaction term in regard to both (a) integrative, coactions within the relational developmental system; and (b) the continuing use of this same term in statistical methods that treated interacting variables as split in order to meet the assumptions of the data analytic procedure.

Accordingly, I, and others (e.g., Overton, 2015a), have adopted the convention of using the term interaction only in its statistical sense, that is, when referring to the relation among variables in statistical procedures such as analysis of variance or multiple regression. In turn, terms such as fused, integrated, coacting, or simply relational are used to connote the non-split links among all variables in the relational developmental system and, of course therefore, in all current discussions of RDS-based theories.

Given the metatheoretical and theoretical orientation of Magnusson (e.g., 1995, 1999a, 1999b; Magnusson & Stattin, 1998, 2006), the term interaction would likely not appear in a version of his theory were it to be written at the time that I am writing this book (in 2017). However, for historical and scholarly accuracy, I will use the term "interaction" as Magnusson used it when quoting his work. Nevertheless, I urge readers to recognize that his use of this term reflects the dynamic, and non-split, contemporary conception of nature↔nurture or, more pertinent to the focus of his theory, individual↔context relations. Thus, when reading the term "interaction" within the context of his theory, it might be useful to conceptually recast the term with one of the above-noted ones used within contemporary RDS-based ideas, for instance, coaction, fusion, etc.

Returning, then, to Magnusson's theory, it is important to note that he regarded the basis of development as involving bidirectional relations among biological, psychological, and behavioral characteristics; and outer, person↔context relations, involving continual exchanges between the person and his or her environment. Importantly, and in fact anticipating and as well promoting the changes in the vocabulary of developmental scientists that I have just discussed, Magnusson explained that holistic interaction builds and extends the ideas of interactionism found in what he termed "classical interactionism" (Magnusson & Stattin, 2006, p. 406).

Holistic interactionism expands on this classic conception of interaction by, first, placing greater emphasis on the dynamic, integrated character of the individual within the overall person↔environment system and, second, stressing both biological and behavioral action components of the system. Thus, and drawing on many of the same literatures relied on by Gottlieb (e.g., in regard to neuropsychology and developmental biology, e.g., Damasio & Damasio, 1996; Rose, 1995) and by Thelen and Smith (e.g., in regard to chaos and general systems theory, e.g., Gleick, 1987; von Bertalanffy, 1968), and buttressed by what Magnusson (1995, 1999a, 1999b) saw as the growing importance of holistically oriented longitudinal studies of human development (e.g., Cairns & Cairns, 1994), Magnusson and Stattin (2006) specified the five basic propositions of holistic interaction:

> 1. The individual is an active, intentional part of a complex, dynamic PE system.
> 2. The individual functions and develops as a total, integrated organism.
> 3. Individual functioning in existing psychobiological structures, as well as development change, can best be described [as] an integrated, complex, and dynamic process.
> 4. Such processes are characterized by continuously ongoing interactions (including interdependence) among mental, behavioral, and biological components of the individual and social, cultural, and physical components of the environment.
> 5. The environment functions and changes as a continuously ongoing process of interactions and interdependence among social, cultural, and physical factors.
>
> (p. 407)

Features of the Person↔Environment System

The holistic interactionist theory has profound implications for the conduct of developmental science. Indeed, the far-reaching character of these implications extends to even the role of the concept of "variable" in developmental research. Magnusson and Stattin (1998, 2006) noted that, in most approaches to developmental science, the concept of "variable" is embedded within a theoretically reductionistic model of humans. Within this perspective, the "variable" becomes the unit of analysis in developmental research. However, within the context of what they term "*the holistic principle*," Magnusson and Stattin (1998, 2006) forwarded a person-centered view of development and, as such, conceptualized the individual—the whole person—as the core unit of developmental analysis. That is, the holistic principle

> emphasizes an approach to the individual and the PE system as organized wholes, functioning as totalities and characterized by the patterning of relevant aspects of structures and processes in the individual and in the environment. At all levels, the totality derives its characteristic features and properties from the functional, dynamic interaction of the elements involved, not from each isolated part's effect on the totality. Each component of the individual structures and processes that are operating, as well as each component of the environment, takes on meaning from its role in the total, integrated functioning of the individual.
>
> (Magnusson & Stattin, 2006, p. 404)

Accordingly, if the totality, the whole person, or, better, the person↔environment relation, characterizes the essence of developmental change, then developmental analysis that assesses single aspects of the system (single variables, for instance) is necessarily incomplete. Only a distorted view of development can be derived from appraising variables divorced from the context of other, simultaneously coacting variables (Magnusson & Stattin, 1998, 2006). It is this integration of variables from across the person↔environment system that constitutes the core process of human development and, as such, the necessary focus of developmental science.

Indeed, within holistic interactionist theory, the developmental *process* involves a continual flow of integrated, reciprocally related events. Time becomes a fundamental feature of individual development given that, within the probabilistic-epigenetic view taken by Magnusson (1995, 1999a, 1999b) of the interrelation of the constituent events comprising the process of development, the same event occurring at different times in ontogeny will have varying influences on behavior and development. As a consequence, “A change in one aspect affects related parts of the subsystem and, sometimes, the whole organism . . . At a more general level, the restructuring of structures and processes at the individual level is embedded in and is part of the restructuring of the total person–environment system” (Magnusson & Stattin, 2006, p. 433).

Thus, to Magnusson (1995, 1999a, 1999b; Magnusson & Stattin, 1998, 2006), individual development is marked by a continual restructuring of existing patterns and—through the facilitation and constraint of the biological through sociocultural levels of the total person↔environment system—by the emergence as well of new structures and processes (i.e., of developmental novelty). In other words, as also specified within the Thelen and Smith (1998, 2006) dynamic systems theory, *novelty* in structures and processes, in forms and patterns, arises through principles of system self-organization. Indeed, *self-organization* is a guiding principle within the RDS-based theory proposed by Magnusson. Thus, development, novelty, arises in the living world because the parts of the organism produce each other and, as such, through their association create the whole (Magnusson & Stattin, 1998, 2006). A pre-existing entity, for example, a gene, placed into an individual at the moment of his or her conception, is not needed to prepare the individual to develop in specific ways (e.g., as is claimed by Bjorkland, 2016). The autopoietic relational developmental system within which the individual is embodied is the source of development (e.g., see Overton, 2015a; Thelen & Smith, 2006; Witherington & Lickliter, 2016).

In short, then, Magnusson’s view (1995, 1999a, 1999b; Magnusson & Endler, 1977) of the character of the relation among the components of this system is consistent with the theories of Gottlieb (1997, 2004), Thelen and Smith (1998, 2006), and others (e.g., Lerner, 1991, 1996, 2006, 2012, 2015a, 2015e, 2016; Schneirla, 1957; Tobach & Greenberg, 1984). Indeed, Magnusson and Stattin (2006, p. 434) noted that, “Functional interaction is a characteristic of the developmental processes of an individual in the life-span perspective; from the interaction that takes place between single cells in the early development of the fetus . . . to the individual’s interplay with his or her environment across the life span.”

Magnusson (1995, 1999a, 1999b, Magnusson & Stattin, 1998, 2006) noted that there are two key concepts that are involved in understanding the character of dynamic coactions within a relational developmental system: *reciprocity* and *nonlinearity.* Magnusson and Stattin (1998, 2006) pointed to data on the mutual influences of parents and children (e.g., Lerner et al., 1995) as the best illustration of reciprocity in the person↔environment system. Similar to my discussion of Schneirla’s (1957) idea of circular functions (Chapter 7), Magnusson and Stattin noted that reciprocity occurs in parent↔child relations. The behaviors of each person in the relationship act as an influence on the behavior of the other person. At the same time, change occurs as a consequence of the influence of the other person’s behavior.

As do Thelen and Smith (1998, 2006), Magnusson (1995, 1999a, 1999b; Magnusson & Stattin, 1998, 2006) noted that nonlinearity is the prototypic characteristic of the relationship among constituents of the person↔environment system. Non-systems perspectives typically approach scholarship with the perspective that the relationship among variables is linear and, as well, that linear relations among variables that are identified by appraising differences between people may be generalized to the relations that exist among variables within a person (Magnusson & Stattin, 1998, 2006). As discussed in prior chapters, such flawed generalization is based on the ill-advised use of the ergodic theorems to understand individual development (e.g., Molenaar & Nesselroade, 2014, 2015; Rose, 2016). Human development is non-ergodic. Moreover, increases (or decreases) in one variable are not always accompanied by proportional increases (or decreases) in another variable, either across people or within individuals. That is, rather than finding such linear changes to be ubiquitous, changes in one variable may be accompanied by disproportionate changes in another variable. Such relationships are curvilinear in character and, for instance, may take the form of "U"- or inverted "U"-shaped functions. For example, low levels of stress may not provide enough impetus to elicit high levels of performance on a given task or skill; high levels of stress may overwhelm the person and produce performance "paralysis" rather than high-level performance; but moderate levels of stress may be associated with the greatest likelihood of high-level performance (Magnusson & Stattin, 1998, 2006; Strauss, 1982).

Together, the notions of reciprocity and nonlinearity associated with dynamic coactions underscore the *bidirectional causality* involved in the developmental system envisioned by Magnusson (1995, 1999a, 1999b), and support the point that his model challenges the key concepts of non-systems approaches to human development, even insofar as fundamental notions, such as the definition of the concept of "variable," are concerned. For instance, within the system of bidirectional, or configural, causality, such as the one seen in Figure 10.4 and 2.1:

> The concepts of independent and dependent variables and of predictors and criteria lose the absolute meaning they have in traditional research assuming unidirectional causality. What may function as a criterion or dependent variable in statistical analyses at a certain stage of a process, may at the next stage serve as a predictor or independent variable.
>
> (Magnusson & Stattin, 2006, p. 436)

Moreover, Magnusson's theory, presaging the scholarship of Molenaar and Nesselroade (e.g., 2014, 2015) and Rose (e.g., 2016; Rose et al., 2013), changes the emphasis in developmental science from one of a search for information that will allow generalizations to be made about how variables function across individuals to one of attempting to understand how variables function within the person. That is, because of the nonlinear relation among variables within the individual, and because the individual's "internal" distinctiveness is both a product and a producer of his or her distinct pattern of exchanges with the other levels of organization within the total person–environment system, *individual distinctiveness*, idiographic change, is a fundamental feature of human development. Indeed, to understand the development of the individual, one must identify the particular factors that are pertinent to his or her life and the specific ways these factors are organized and operate within him or her (Magnusson & Stattin, 1998, 2006). In short, "developmental changes do not take place in single aspects isolated from the totality. The total individual changes in a lawful way over time; individuals, not variables, develop" (Magnusson & Stattin, 1998, p. 727).

The complexity of this person-centered analysis is underscored when, as Magnusson (1995, 1999a, 1999b; Magnusson & Stattin, 1998, 2006) explained, one understands that the contextual component of the person↔environment system is as multifaceted and individualistic as are the levels of organization having their primary loci within the individual (e.g., biology, cognition, personality, behavior). That is:

> The total, integrated, and organized PE system, of which the individual forms a part, consists of a hierarchical system of elements, from the cellular level of the individual to the macrolevel of environments . . . In actual operation, the role and functioning of each element depends on its context of other, simultaneously working components, horizontally and vertically.
>
> (Magnusson & Stattin, 2006, p. 421)

Magnusson and Stattin (1998, 2006) depicted the complexity of these contextual components of the person↔environment system by noting that the environment may be differentiated on the basis of its physical and social dimensions, and that a person may be influenced by either the actual and/or the perceived features of these two dimensions (see my discussion in Chapter 6 about the role of perceptions in the continuum of indirectness proposed by

Anastasi, 1958). Either dimension may serve as a source of stimulation for behavior and/or a resource for information. In addition, environments may differ in the extent to which they provide an optimal context for healthy development, and in regard to the extent to which they serve over time as a basis for developmental change (i.e., as a *formative* environment; Magnusson & Stattin, 1998, 2006), or as a source for a specific behavior at a particular point in time (i.e., as a *triggering* environment; Magnusson & Stattin, 1998, 2006).

In addition, environments may be differentiated on the basis of their proximal or distal relationship to the person. For instance, the family or the peer group may constitute proximal contexts for the person, whereas social policies pertinent to family resources (e.g., policies regarding welfare benefits for poor families) may be part of the distal, macrosystem of human development (Bronfenbrenner & Morris, 1998, 2006).

Conclusions about the Ideas of Magnusson

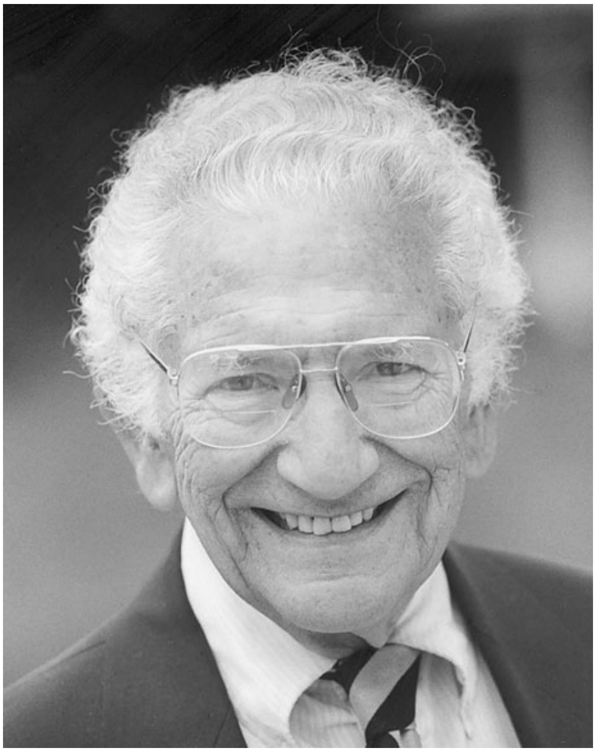
When the complexity of the environment is coupled with the multiple dimensions of the person (e.g., his or her biology; mental system; subconscious processes; values, norms, motives, and goals; self-structures and self-perceptions; and behavioral characteristics; Magnusson and Stattin, 1998, 2006), the need for a holistic, integrated theory of the developmental system is apparent. This system must be engaged to understand the course of human development and, as well, to enhance or optimize it. Consistent with our earlier discussions of the implications of plasticity for intervention to enhance the course of human life, Magnusson saw the need to involve all levels of the person and the system, not only to design a comprehensive scientific research agenda but, as well, to devise strategies to apply developmental science in ways that will integratively promote positive human change:

> The holistic-interactionistic model for individual development implies that the total person-environment system must be considered, not single problems of individual functioning and single risk factors in the social context, in the organization and implementation of societal programs for intervention and treatment. Long-term programs and strategies must be worked out based on knowledge from all relevant fields of developmental science, and planned and implemented in close collaboration among professionals representing multiple agencies, programs, and initiatives, which must be integrated so that the breadth of the individual person-environment system is adequately engaged.
>
> (Magnusson & Stattin, 2006, p. 450)

Thus, Magnusson's ideas about holistic coactions underscore the integral connection between science and application involved in an RDS-based perspective about human development. His views of the scientific and societal utility of such theories are consistent with, and buttressed by, the ideas of other developmental systems theorists. Another significant instance of such a theory—one also spanning the research–application continuum and stressing the ideas of holism advanced by Magnusson—was developed by Heinz Werner and his colleagues at Clark University, for instance, Bernard Kaplan and Seymour Wapner (see Raeff, 2016, for a review). In Chapter 8, I discussed some of the contributors to this collaborative work. Here I will continue this discussion by focusing in particular on the contributions of Wapner (e.g., Wapner, 1969, 1977, 1981, 1987, 1995; Wapner & Demick, 1991, 1992, 1998; Werner & Wapner, 1949, 1952).

WAPNER'S HOLISTIC, DEVELOPMENTAL, SYSTEMS-ORIENTED PERSPECTIVE

Seymour Wapner (e.g., 1969, 1977, 1981, 1987, 1995; Wapner & Demick, 1991, 1992, 1998), in collaboration with colleagues at Clark University and, most notably there, with Heinz Werner (e.g., Werner & Wapner, 1949, 1952; see too Chapter 8), developed an RDS-based theory that sought to understand the "human in relation"; that is, the person as he or she interacts with all the levels of his or her context across the course of the entire life span. To devise such a broadly applicable theory, Wapner explicitly



Seymour Wapner

attempted to synthesize features of both organismic and contextual world views.

As discussed earlier, and as noted by Overton (1984) as well, Wapner (1986, 1987; Wapner & Demick, 1998) also believed it useful to "violate" Pepper's (1942) proscription against "mixing metaphors" and trying to integrate ideas associated with different world views. Accordingly, Wapner formulated a theory that integrates the ideas of synthesis and holism fundamental to organicism and the dynamic coactions of contextualism, wherein "Relations among the aspects of the whole are not conceived of as involving mutual influences of antecedent–consequent causation. Instead the different aspects of wholes co-exist as intrinsic and inseparable qualities of the whole" (Altman & Rogoff, 1987, p. 25).

Moreover, as in the other instances of RDS-based theories that are linked to such organismic–contextual integration—most notably, the probabilistic-epigenetic ideas of Schneirla (1957) and Gottlieb (1997), as well as Lerner's instantiation of relational developmental systems theories, developmental contextualism (see Chapter 9, and Lerner, 1978, 1991, 1996, 2006b)—Wapner's synthesis of the organismic and contextual philosophies is linked to a commitment to the idea of levels of integration in human development. That is, Wapner (1986, 1987; Wapner & Demick, 1998) rejects reductionism and, quite specifically, biological reductionism. Rather than viewing developmental events as epiphenomena or as by-products of other phenomena (e.g., genes), as in biological reductionism (e.g., Bjorkland, 2015; Bjorkland & Ellis, 2005; Plomin, Defries, Knopik, & Neiderhiser, 2016; Rimfeld, Ayorech, Dale, Kovas, & Plomin, 2016), Wapner contends that phenomena at any one level of organization affect and are affected by phenomena at all other levels.

Given, then, this integrative orientation to developmental science, Wapner synthesized three key ideas within the instance of RDS-based theory he forwarded: holism, development, and systems-orientation. The theory, according to Wapner and Demick (1998),

> is *holistic*, insofar as it assumes that all part processes—biological/physical, psychological (cognitive, affective, valuative), sociocultural—are interrelated; *developmental*, insofar as it assumes, in keeping with the orthogenetic principle (Werner, 1957), that development proceeds from a relative lack of differentiation toward the goal of differentiation and hierarchic integration of organismic functioning; and *systems-oriented* insofar as the unit of analysis is the *person-in-environment*, where the physical/biological (e.g., health), psychological (e.g., self-esteem), and sociocultural (e.g., role) levels of organization of the *person* are operative and interrelated with the physical (e.g., natural and built environment), interpersonal (e.g., friend, relative), and sociocultural (e.g., regulations and rules of society) levels of organization of the *environment*.
>
> (p. 761)

Accordingly, to Wapner (1987, 1995; Wapner & Demick, 1991, 1998), the unit of analysis within this theory is the organism-in-environment system. He specified that the organism and the environment

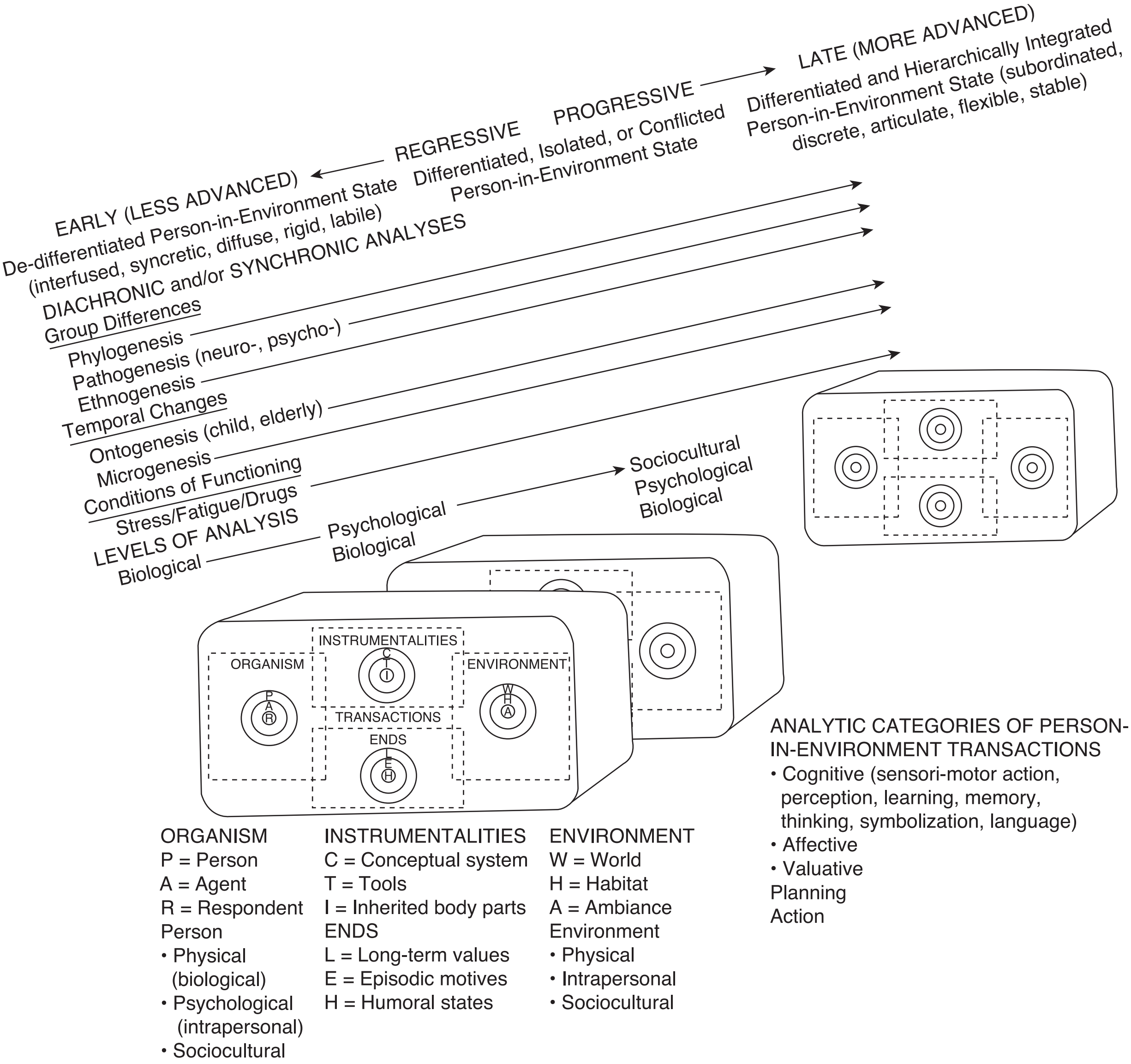


Figure 10.5 A holistic, developmental, systems-oriented approach to person-in-environment functioning.

Source: S. Wapner and J. Demick (1998, p. 767). Developmental analysis: A holistic, developmental, systems-oriented perspective. In R. M. Lerner (Vol. Ed.), *Handbook of child psychology: vol. 1. Theoretical models of human development* (pp. 761–805). W. Damon (Series Ed.). New York: Wiley. Copyright © 1998 by John Wiley & Sons, Inc. Reprinted with permission.

are the structural components of this system and that they are related through *transactions*, that is, by exchanges involving experience (cognitive, affective, and valuative) and action, and possessing dynamic components characterized by ends (or goals) and means (or instrumentalities; Wapner, 1987, 1995; Wapner & Demick, 1991, 1998). Figure 10.5 is an illustration of this complex system, what Wapner and Demick (1998) saw as a holistic, developmental, and RDS-oriented approach to person-in-context functioning.

Person-in-Environment Functioning within the Holistic, Developmental System

Consistent with the ideas of Gottlieb (e.g., 1997), Magnusson (e.g., 2000; Magnusson & Stattin, 2006), Schneirla (e.g., 1957), and Thelen and Smith (e.g., 2006), Wapner and Demick (1991, 1998) noted that the organism-in-environment system represented in Figure 10.5 reflects the integration of levels of organization. The least complex level is termed the *respondent-in-ambience* system, and involves reflex-like reactions of the organism to ambient stimulation. Wapner and Demick (1998, p. 767) noted that whereas "biological drives, sensory systems, and locomotor systems are clearly evident in these respondents, means and ends are more difficult to identify."

The next higher level in the system is termed the *agent-in-habitat* system, and involves the organism transacting with a context comprised of social and nonsocial objects. There is a goal for such transactions: satisfaction of episodic motives, such as the attainment of food or attaining a relationship with a mate. These goals are accomplished through the use of instrumentalities such as tools (Wapner, 1986, 1987, 1995; Wapner & Demick, 1991, 1998).

The most complex level in the system is the *person-in-world* system. Here, the person is shaped by his or her culture, and transacts with a context composed of sociocultural objects such as educational, religious, and political institutions and family and kinship rules (cf. Raeff, 2016). Both short- and long-term goals are involved in these transactions and, to Wapner (1986, 1987; Wapner & Demick, 1998), values are always a part of these ends. In addition, instrumentalities used in these transactions involve the formulation of plans and the use of conceptual systems, such as language or mathematics (Wapner, 1987, 1995; Wapner & Demick, 1991, 1998).

Figure 10.5 illustrates Wapner's view and, as well, the idea that there exist several analytic categories of experience, ones involving cognition, affect, and values, and of planning and action. In addition, the figure depicts progressive and regressive change, both presented in regard to the orthogenetic principle (Werner, 1957). Moreover, the figure displays the idea that the analyses of the system may proceed diachronically (across time) or synchronically (within time) and may involve the assessment of differences between groups, changes within individuals across time, or different conditions or states of the organism; these analyses can occur on one or more levels of organization (biological, psychological, and/or sociocultural).

The person-in-environment system that Wapner envisioned operates in accordance with the principles of dynamic equilibrium, for instance, as Thelen and Smith (1998, 2006) described. Indeed, consistent with the views of these other developmental scientists, Wapner and Demick (1998) noted that, within the dynamic equilibria of the person-in-environment system:

> Ongoing person-in-environment relations may be disturbed or perturbed by a change in the organism, in the environment, or in both. This may make for dramatic qualitative changes in the relations among system components (organism, environment, transactions, means, ends). Moreover, following perturbation of the organism-in-environment system, the reestablishment of a new dynamic equilibrium or ongoing state directed toward accomplishing goals is assumed to take place.
>
> (p. 771)

The goals of the actor, of the person within the *person-in-environment* system, introduce a *teleological-directedness* (Wapner & Demick, 1998, p. 773) to the holistic system. However, this teleology is not one of a suprasystem agent, or system designer, giving form, organization, or direction to the system from a vantage point outside of it. Rather, teleology here is meant to reflect the fact that humans, especially at the level of the *person-in-world* system, have goals and intentions and use means to enact the ends they have selected. As discussed earlier in this chapter, Brandtstädter (e.g., 1998, 1999, 2006) presented similar ideas. Humans' active transactions with their world, predicated on their goals and intentions (on their *teleological prospections*), are part of the self-organizational influences within the relational developmental system.

Conclusions about the Ideas of Wapner

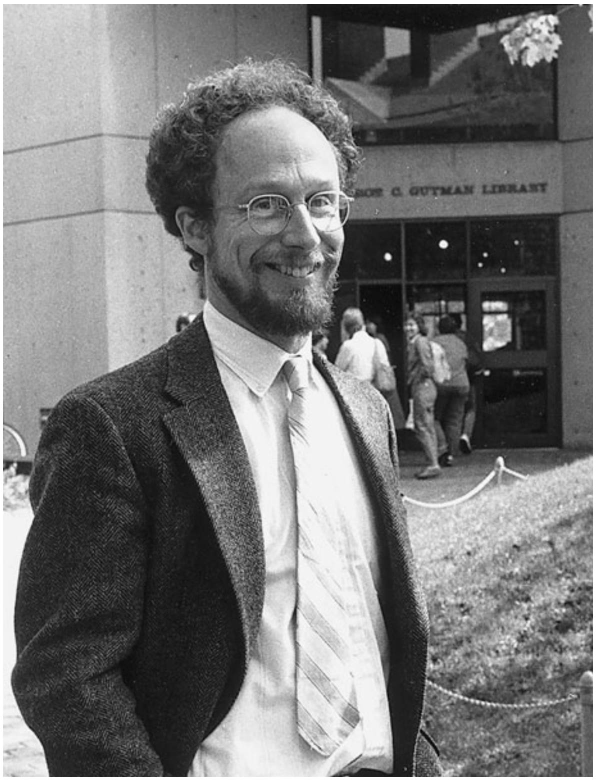
By bringing the goals and intentions of the active individual into the levels of organization integrated within the holistic developmental system theory he has developed, Wapner is asserting the importance for understanding human development not only of cognition, affect, and action but also of values, aspirations, and hopes. By incorporating these features of human functioning into his theory, Wapner is being consistent with his interest in forwarding a comprehensive and integrative theory of human development. His interest in valuation is central in his theory because he believes that all dimensions of psychological functioning—and critically ones involving valuing, planning, and acting with instrumentalities to attain one's planned ends—need to be synthesized in order to understand the rich panoply of person-in-environment transactions that are involved in healthy development—what he terms *adaptation* (Wapner, 1986, 1987; Wapner & Demick, 1998)—across life.

To promote such adaptation—conceived of as *optimal* relations between the person and his or her environment (Wapner & Demick, 1998)—Wapner conceives of actions that foster the person's altering self to fit the context, altering the context to fit the person, or mutual accommodations between person and context. In this view of the components that may be included in attempts to promote optimal development in people, Wapner's theory underscores again the use of RDS-based ideas for application.

KURT W. FISCHER'S DYNAMIC COACTIVE SYSTEMS THEORY

For several decades, Kurt W. Fischer and his colleagues (e.g., 1980, 1987; Fischer & Bidell, 1991, 1998; Fischer & Pipp, 1984; Fischer & Rose, 1994; Mascolo, 2013; Mascolo & Fischer, 2015) have formulated a theory of human development that focuses on the dynamic relations between the person and his or her context within the relational developmental system. This theory is significant for several reasons.

First, Fischer's model uses a probabilistic-epigenetic approach to integrate all levels of organization and all substantive foci with every level. This model provides a compelling counterpoint to neo-nativist formulations of development (e.g., those of Keil, 1998, 2006, and of Spelke & Newport, 1998). Second, Fisher's theory provides a dynamic conceptualization of psychological structures of the individual in regard to both thoughts and actions. Third, the theory underscores the importance of a focus on the individual and on non-ergodic, idiographic processes in human development (e.g., Molenaar & Nesselroade, 2014, 2015; Nesselroade & Molenaar, 2010; Rose, 2016; Rose et al., 2013). Fourth, the theory leads to a set of methodological ideas that are important if developmental science is to effectively understand the course of the dynamic development of an individual's psychological structures.


Kurt W. Fischer

Reflecting the probabilistic-epigenetic process (e.g., Gottlieb, 1970, 1998) discussed in earlier chapters, Mascolo (2013) explained that the theory that

Fischer, he, and others have collaboratively developed is "a coactive systems model of psychological functioning and its development. The core assumption of coactive systems theory is that integrative structures of thinking, feeling and acting are the emergent products of coactions among systems that operate both within and between people" (p. 186). Mascolo (2013) notes as well that:

> At their most basic level, relational and systems approaches analyze the developmental origins of order and variability not in terms of sets of separable causal forces but instead in analyses of relations between causal systems. From this view, genes and environment, biology and culture, cognition and emotion, self and other, and so forth are *inseparable* as causal processes in the development of action and experience.
>
> (pp. 185–186)

Mascolo (2013) also explains that individuals' psychological structures are foundational features of this instance of an RDS-based theory. Consistent with the action-theory approaches I discussed earlier in this chapter (e.g., Brandtstädter, 1998, 1999, 2006), Mascolo (2013) notes that:

> The first element of the system consists of the psychological structures of individual persons . . . Psychological acts consist of goal-directed integrations of meaning, affect, and experience. As such, actions have several properties. First, actions are intentional processes in the sense that they are performed on something, directed toward something, or are about something, real or imagined . . . Second, psychological activity is mediated by meaning and experience. It is meaning that transforms any given process into a psychological one. People do not simply react to the physical properties of external or internal stimulation; persons act on the basis of the meaning that events have for them . . . Finally, acting is a form of doing. Actions are goal-directed operations on the world. To say that persons act implies that they have some degree of agency or control over their representational, experiential and motoric processes.
>
> (pp. 190–192)

Based on this approach to RDS-based theory construction, Fischer and his colleagues devised a theory that views developmental structures as active entities organizing person↔context relations. This RDS-based model sought to avoid the problems of reductionism associated with neo-nativist ideas about human development. As I will discuss again in Chapter 12, developmental scientists associated with neo-nativism (e.g., Keil, 1998, 2006; Spelke & Newport, 1998) have sought to avoid the pitfalls of early formulations of genetic reductionist approaches that attempted to account for facets of human development (i.e., they have sought to avoid following approaches that adopted what Anastasi, 1958, explained were the "Which one?" or the "How much of each" questions; see Chapter 6). Nevertheless, their models, which contrast nativism with empiricism as alternative approaches to explaining cognitive development, appeal ultimately to genes as the basis of human attributes (e.g., cognitive structures). A key impetus for the development of Fischer's theory has been to devise a means to treat the variability that exists in human cognitive development in a manner that avoids the problems of genetic reductionist approaches.

Dynamic Systems Theory as an Alternative to Neo-Nativism

Using cognitive development as a target case, Fischer and Bidell (2006) noted that the core issue confronting developmental theory is:

> how to account for the tremendous variability in developmental phenomena, which during the past 30 years has increasingly moved from the background to the foreground of developmental research and theory . . . The static stage structure, which dominated theories of cognitive development from its inception through the early 1980s, proved incapable of accounting for the massive evidence of (a) both wide-ranging variation and sometime consistency within and across individuals in the age of acquisition of logical concepts across domains and contexts, (b) systematic sequences in acquisition of many of these concepts and their components, and (c) variation

> from high to low synchrony in development of concepts under various conditions.
>
> (p. 336)

Neo-nativism represents one means to deal theoretically with the variability in cognitive development. However, Fischer and Bidell (2006) criticized neo-nativist accounts because of selective attention to extant developmental data. They note that the:

> neonativist argument is that cognitive structure must be innate because acquisition of certain concepts can be demonstrated at very young ages. However, this argument from precocity takes into account only half the evidence for variability—the downward half . . . It treats the earliest age as the "real" age for a concept's emergence, ignoring evidence of wide variations in age of acquisition both upward and downward.
>
> (p. 344)

Moreover, compounding this problem of selective attention among neo-nativist scholars is the tendency in neo-nativist approaches to adopt a Cartesian split approach to human development that relies on reductionism to the essential or real component that explains development (purportedly genes) (Lerner, 2016; Overton, 2015a; Witherington & Lickliter, 2016). Thus, Fischer and Bidell (2006) explain that:

> The exclusive use of reductionism as an analytical method fosters the related problems of reification and dualism, both arising from the neglect of relations in theoretical constructs. Without an account of the relations among systems that can explain movement and change, abstractions such as mind, thought, and structure appear static and isolated from other constructs such as body, action, or function. These static abstractions reify the phenomena they refer to, treating dynamic processes as frozen objects. The self-organizing, goal-directed activity of the human agent is ruled out of the accounts of development.
>
> (p. 338)

Fischer and Bidell (1998) not only provided a critique of neo-nativism but also offered an alternative to it, one based on a dynamic, RDS-based approach to understanding cognitive abilities and, as well, the breadth of the individual↔context system, of which the development of cognitive structures is one part (Mascolo, 2013; Mascolo & Fischer, 2015). Fischer and Bidell (2006) explained that:

> To build successful models of dynamic psychological structure, it is essential to understand how dynamic structure differs from static form. An essential first step is to focus simultaneously on variability and stability. Indeed, the neglect of variability helps ensure that models remain static, missing the sources of order in the variation and treating structures as static forms. Any adequate account of psychological structure must explain not only the stability that allows systems to function and maintain themselves over time and space but also the wide variability that arises from the dynamics of self-organizing systems. Models of psychological structure must specify mechanisms by which activities are organized dynamically in relation to multiple influences that are biological, psychological, and social.
>
> (p. 318)

Accordingly, to provide an alternative to the reductionism and reification associated with the positing of native competence and of the conceptual split between nature and nurture within which such a contextually disembodied entity is presumed to exist, Fischer and Bidell presented a theory of psychological structure that is predicated on the features of a dynamic, living system (cf. Ford & Lerner, 1992; Sameroff, 1983, 2009; Thelen & Smith, 1998). In explaining their view of this system, Fischer and Bidell (2006) noted that:

> All living systems—whether biological, psychological, or social—must be organized to function. A living organism that becomes sufficiently disorganized dies. A disorganized society collapses. A disorganized mind leaves a person helpless in the face of everyday problems. This organizational aspect of living systems is what we call *structure*, a dynamic patterning and relating of components that sustain the organized activities that define life and living things . . . dynamic

> structure exists only where relationship exists, and relations among the parts of a system provide its specific organization . . . A system that becomes static—unable to change and adapt to varying conditions—will quickly perish . . . An organism or society that becomes inflexible and incapable of adaptive response to variations in its environment will die as surely as one that becomes disorganized. Thus, structure must be distinguished not only from disorganization but also from static form, which really is the antithesis of structure. Structure is fundamentally dynamic because it is a property of living, changing, adapting systems . . . Dynamic variation is a fundamental property of human action and thought.
>
> (p. 318)

The sort of dynamic, developmental systems view of human development proposed by Fischer and Bidell (1998) results in a conception of cognitive structure that stands in marked contrast to the view of cognitive structure found in neo-nativism. In neo-nativism, structure exists independent of and prior to the activity of the organism, and this structure is argued to arise through innate, experience-independent means. In turn, in Fischer and Bidell's conception, structure and activity are inextricable, fused components of the integrated, organism–context relations that constitute the developmental system. Thus, in contrasting the neo-nativist versus the dynamic, RDS-based view of cognitive structure, Fischer and Bidell (2006) noted that:

> Traditional static conceptions of development in psychological structure are closely related to the widespread cultural metaphor of a ladder. Development is conceived as a simple linear process of moving from one formal structure to the next, like climbing the fixed steps of a ladder. It matters little whether the steps of the ladder are conceived as cross-domain stages, levels of a domain-specific competence, or points on a psychometrically based scale. In each case, the beginning point, sequence of steps, and endpoint of the developmental process are all linear and relatively fixed, forming a single ladder. With such a deterministic, reductionist metaphor, it is difficult to represent the role of constructive activity or contextual support because there appears to be no choice of where to go from each step. The richness of children's development, including the variability in their skills across contexts, is simply lost with the ladder metaphor. Development means just moving to the next step.
>
> (p. 319)

In place of the ladder metaphor, Fisher and Bidell (2006) offer what they regard as a more dynamic conception: the metaphor of a constructive web. They argue that this metaphor is useful for dynamic models in that it enables developmental scientists to conceptualize active skill construction in diverse manners and in relation to diverse contexts. In contrast to the steps of a ladder, Fischer and Bidell point out that the strands in a web do not exist in a fixed order. Instead, the strands are an outcome of the coactions of construction of the organism building the web and the context within which the web is being built. For example, they note that the web constructed by a spider varies in relation to the supports or resources in its context, that is, things such as leaves or branches. However, human webs are typically constructions of many individuals, although research often assesses individuals independent of their social relationships (Fischer & Bidell, 2006).

Thus, Fischer and Bidell integrated the notion of developmental strands found in the organismic world view (Pepper, 1942) with the richness of variations present in the contextual world view (Pepper, 1942). As a result, they proposed a process of dynamic, person↔context relations that reflects the features of fusion and reciprocal relationships involved in the probabilistic-epigenetic view of developmental process (Gottlieb, 1970, 1983, 1997; Gottlieb et al., 1998). For instance, Mascolo and Fischer (2015) note that:

> People are different. The differences are pervasive. Humans act differently in different contexts. There is no average person, and only under some very special conditions (see Nesselroade & Molenaar, 2010; Molenaar & Nesselroade, 2015) can means or averages adequately represent any individual's performance. These ideas constitute the fundamental starting point that is omitted in

> most frameworks for explaining human action. Analysis should *begin* with analysis of the role of context, and with an appreciation that variability is fundamental to human action—that we human beings naturally vary our actions based on context and support for skilled performance. People do not follow narrow models of ability that force them into little boxes such as intelligence or learning style.
>
> (p. 114)

Mascolo and Fischer (2015) argue, then, that human action is fundamentally integrative. People differ in regard to their actions in specific contexts (e.g., Cervone et al., 2007; Mischel, 2007). Nevertheless, Mascolo and Fischer contend that actions in any context necessarily involve an integration of several facets of the person, for instance, his or her cognition, emotions, motivation, and behavioral acts (e.g., Mischel, 2007; see too Chapter 9). They emphasize that human action is holistic and not a linear summation of separated parts of the individual. Patterns of integration may, and typically do, vary for a person across the contexts within which he or she acts, for example, based on the meaning making and affective processes of the agentic individual (Mischel, 2007; and see Chapter 9). The specific actions of the specific individual in a specific context engage specific facets of the person's cognitive, affective, and behavioral attributes and, as such, coalesce in a manner depicted by Gottlieb's (1970, 1991, 1992, 1998, 2004) conception of probabilistic epigenesis.

Michael Mascolo

As such, Mascolo and Fischer (2015) argue that the explanation of human action and its development lies in understanding the individual's embodiment within a dynamic, relational system that is textured by the specific physical and sociocultural components present at specific times and places (Bornstein, 2017). With human action embedded in such a system, Mascolo and Fischer note that human activity is therefore both structured and plastic, and therefore changing (variable) according to the principles of holism, integration, individual↔context relations, and autopoiesis that mark dynamic, relational systems. Mascolo and Fisher (2015) explain, then, that the variation of an individual's actions across time and place provides important information about the character of the relational developmental system. In fact, they indicate that the principles of person-in-context and variability-as-information constitute the backbone of dynamic systems theory.

The dynamism of this integrated, multilevel system synthesizes continuity and discontinuity, constancy and change, and structure and variability. That is, as Fischer and Bidell described it, the web-like system they envisioned gives rise to dynamically developing, integrative structures. Fischer and Bidell (2006) explained that:

> From a dynamic systems viewpoint, psychological structure is the actual organization of systems of activity. It is not a separately existing entity, such as a logical stage dictating behavior, or a preformed linguistic or cognitive capacity awaiting actualization, but instead is a property of human activity systems. Because real systems

> of activity are dynamic—constantly moving, adapting, and reorganizing—they must be dynamically structured. Variability is a natural consequence of system dynamics, and because systems are organized, the variability is not random but patterned . . . Just as geologists have modeled the structures of coastal evolution and biologists have modeled the structures of evolution of living species, developmental scientists can build models of the dynamic structures of development and learning in human action and thought.
>
> (p. 320)

According to Fischer and Bidell (1998), as well as other theorists who use RDS-based ideas to frame their developmental scholarship (e.g., Ford & Lerner, 1992; Gottlieb, 1997; Magnusson, 1995, 1996, 1999a, 1999b; Magnusson & Stattin, 2006; Sameroff, 1983, 2009; Thelen & Smith, 2006; Wapner & Demick, 1998; see too the chapters in Overton & Molenaar, 2015), all facets of human behavior develop within this system. However, specifically in regard to the development of human cognition, Fischer and Bidell (2006) stated that:

> The human mind is a specialized living system that participates in and with other bodily, environmental, and social systems. The specialized function of the human mind is to guide and interpret human activity in relation to the world of people and objects. The activity takes places *in medias res*, in the middle of things, not in the person alone or in the brain. The objects and people in the physical and social world of the actor are actually part of the activity . . . Moreover, living systems are *agentive*—self-regulating and self-organizing, adapting and changing as a consequence of goal-oriented activity . . . In seeking its goals, a living system is involved in multiple relations with other living and nonliving systems, and they are part of one another's dynamics.
>
> (p. 318)

In sum, instead of positing the innate existence of structures that provide an external-environment-independent competence, the dynamic, developmental system depicted by Fischer and Bidell gives rise to structures through a process of organism↔environmental fusions. The structures created through this fusion integrate the capacities for constancy and change that are requisite for a living entity to survive. Fischer and Bidell labeled these structures dynamic skills.

This concept is perhaps the one that serves as the best point of contrast between the neo-nativist idea of cognitive structure as an experimentally disembodied, intrinsic competence and the dynamic, developmental systems view of structure as an active, integrated link between the person and his or her multilevel context.

The Concept of Dynamic Skill

The concept of dynamic skill presented by Fischer (e.g., Fischer & Bidell, 1998) is embedded in the dynamic systems theory he and his colleagues have developed. As explained by Mascolo and Fischer (2015):

> The task of developmental science is to capture organized patterns in this variability and to propose models to account for both the variability and the stability (Nesselroade & Molenaar, 2010) of behavior and development . . . we demonstrate how the concepts and methods of *dynamic systems theory* provide a framework and tools for analyzing this variability and detecting the order within it. This framework is consistent with what others (e.g., Lerner, 2006b; Lerner & Benson, 2013c; Overton, 2006, 2010, 2013, 2015a: Overton & Lerner, 2012) refer to as the relational-developmental-systems model. One set of key findings associated with dynamic systems theory is that qualitatively new cognitive abilities emerge naturally in learning and development, transitioning from one form of action or representation to another. Humans are self-creating, self-organizing, and self-regulating systems grounded in meaning through the action of our bodies and our cultures.
>
> Tools from dynamic systems analysis provide ways of embracing the variability in order to find the order within it . . . we present a framework for conceptualizing psychological structure as dynamic self-organizing systems constructed by

> human agents . . . This position contrasts with traditional static views of psychological structure based on reductionist scientific theory inherited from the Cartesian-Mechanistic tradition in philosophy (see Overton, 2013, 2014, 2015a for a discussion) and producing a systematic but inadequate understanding of psychological structure including failures to explain the scope of developmental variability. The dynamic framework and research tools crafted for analyzing development and learning provide a methodology for the study of psychological structures including both their variability and the order in the variation (Molenaar, Lerner, & Newell, 2014). These concepts and tools explain both long-term macrodevelopment and short-term microdevelopmental variability in dynamic structures. These models and methods illuminate analysis of relations among cognitive, social, emotional, and neurological development. All these parts work together to explain how we humans act in all our rich complexity.
>
> (pp. 114–115)

Accordingly, this view of dynamic, relational developmental systems requires that any concept of cognitive structure that is employed can, at one time, be used to understand several different manifestations of the variability that exists in cognitive development: the range of cognitive abilities that exists in human development, and its emergence within and across time; the convergence in ontogeny of particular cognitive developments; and the relatively general, species-specific sequence across ontogeny of changes in cognitive ability. However, to be consistent with the probabilistic-epigenetic view of developmental process involved in RDS-based theories, Fischer and Bidell (2006) proposed a concept of structure that accounts for these dimensions of variability in relation to a fused, multilevel developmental system involving reciprocal relations between the developing person and his or her active context. As explained by Mascolo and Fischer (2015):

> Given that development proceeds according to the principle of probabilistic epigenesis, the next issue [in developmental analysis] concerns the embeddedness of this process in the coactive person↔environment system as development proceeds across the life span . . . the person↔environment system is composed of five categories of coacting processes. These include: (a) individual *acts*, (b) the physical and psychological *objects* toward which acts are directed; (c) *other people*; (d) some form of *mediational means*—that which we act *with*—and (e) physical and socio-cultural *contexts*. The foundational assertion of the *dynamic coactive systems* approach is that what we *do* and how we *develop* are *emergent* products of *coactions* that occur between and among component parts of the *person↔environment system*. The parts of the person↔environment system are inseparable (i.e., the system is holistic) as causal processes in the production of action and experience.
>
> (p. 116)

Mascolo and Fischer (2015) explain that skills are specific types of developmental structures. They are control structures in that they involve an individual's ability to have control over specific facets of his or her thinking, feeling, and behaving in a specific sociocultural setting. In other words, skills are context-specific. Consistent with Bornstein's (2017) specificity principle and Mischel's person-in-context conception of the individual, skills involve specific psychological domains (e.g., mathematics, social cue interpretation, planning or prioritizing specific home management tasks) that reflect dynamic features of specific person-in-context relations. In addition, Fischer and Bidell (2006) add that:

> Skills do not spring up fully grown from preformed rules or logical structures. They are built up gradually through the practice of real activities in real contexts, and they are gradually extended to new contexts through this same constructive process . . . The concept of skill also helps to conceptualize the relations among various psychological, organismic, and sociocultural processes and to cut through artificial dichotomies between mind and action, memory and planning, or person and context. A skill—such as telling children stories about emotional interactions with other children—draws on and unites

> systems for emotion, memory, planning, communication, cultural scripts, speech, gesture, and so forth. Each of these systems must work in concert with the others for an individual to tell an organized story to specific children in a particular context, in a way that it will be understood and appreciated. The concept of dynamic skill facilitates the study of relations among collaborating systems and the patterns of variation they produce and inhibits treating psychological processes as isolated modules that obscure relations among cooperating systems.
>
> (p. 321)

Thus, to Fischer and Bidell (2006) the "unit" of cognitive structure is not an internal and intrinsic entity, arising innate and thus independently of exogenous experience. Rather, the unit within their system—the dynamic skill—is a structure that influences and is influenced by the set of interlevel relations ongoing within the relational developmental system. Accordingly, a dynamic skill is a means through which the inner and outer worlds of the developing person become dynamically (i.e., reciprocally and developmentally) linked. A dynamic skill, then, both enables the person to act on his or her world while, simultaneously, it reflects the impact of all levels of the world (family, community, culture, and history) on the person. Indeed, Fischer and Bidell (2006) discussed the cultural embeddedness of a dynamic skill by noting that:

> Skills are context-specific and culturally defined. Real mental and physical activities are organized to perform specific functions in particular settings. The precise way a given skill is organized—its structure—is essential to its proper functioning, as well as specific to that skill at any moment. Good basketball players do not automatically make good baseball players; good storytellers in one culture do not automatically have their stories understood and appreciated in other cultures . . . The context specificity of skills is related to the characteristics of integration and interparticipation because people build skills to participate with other people directly in specific contexts for particular sociocultural systems. In turn, people internalize . . . or appropriate . . . the skills through the process of building them by participating in these contexts; and as a result, the skills take on cultural patterning.
>
> (p. 322)

Moreover, given that skills represent this bidirectional linkage between the inner and outer levels of organization comprising the developmental system and, as well, constitute the means through which the active individual influences (as well as is influenced by) his or her social context, it is clear that for Fischer and Bidell (2006): "Skills are self-organizing. Part of the natural functioning of skills is that they organize and reorganize themselves. These self-organizing properties go beyond maintenance to include growth of new, more complex skills" (p. 322).

Thus, the concept of dynamic skills represents a view of an active individual engaging his or her active world. The person envisioned in the theoretical position presented by Fischer and his colleagues is not just the "host" of the innate structure somehow placed in his or her head by an innate process of indeterminate epigenetic character; rather, the person is an active constructor of the cognitive abilities that give him or her the competence to stay the same or change, as the requirements of his or her world impinge on him or her, and do so through dynamic structures that bridge the inner and outer worlds of the living, developmental system. Accordingly, Fischer and Bidell (2006) noted that:

> skills are organized in multilevel hierarchies . . . People construct skills through a process of coordination . . . Existing component skills, controlling activities in specific contexts, [may be] intercoordinated to create new skills that [control] a more differentiated and integrated range of activities. In the newly integrated skills, the component skills still [function] as subsystems in the new skill as a whole.
>
> (p. 324)

In sum, dynamic skills link the active person in a dynamic relation with his or her changing world. The probabilistic-epigenetic system—the developmental web, in the terms of Fischer and Bidell (2006)—that is a product and a producer of these skills embeds the person in a process that enables him or her to be an active agent in his or her own development. This agency occurs through dynamic collaborations with the components of his or her context, including other people and the products of these other people, for example, their dynamic skills, and the social institutions, culture, and the designed ecology they construct (e.g., Brandtstädter, 1998, 1999; Bronfenbrenner, 2005; Bronfenbrenner & Morris, 2006; Lerner & Busch-Rossnagel, 1981a, 1981b; Lerner & Walls, 1999; Mistry & Dutta, 2015; Rogoff, 1998, 2003, 2011).

Conclusions about the Ideas of Fischer and His Colleagues

Fischer and his colleagues have provided an alternative to the neo-nativist view of cognitive development. Their view places the genesis of cognitive abilities not within the head of the developing person, and certainly not centered in intrinsic structures that are purported to exist independently of organism↔context activity and of endogenous experience. Fischer and his colleagues' conception of cognitive structures is one associated with a probabilistic-epigenetic view of person↔context relations and, as such, relies on the "dynamic collaborations" between the person and his or her world to understand the character of cognitive development.

Thus, the dynamic coactive systems theory that Fischer and his colleagues have developed provides a compelling alternative to genetic reductionist approaches to the analysis of development. As explained by Mascolo (2013):

> Analysis is indispensable in any scientific endeavor, including psychology. It is not possible to study psychological activity without breaking it into its component parts. Problems arise, however, not from breaking down wholes into parts, but from the privileging of parts at the expense of the whole . . . Analysis is essential *but must occur with sensitivity to the ways in which component processes make up the whole*. We might call this stance *analytic holism*. Analytic holism privileges neither the parts nor the whole, but the *relations among parts and wholes* . . . Although one cannot study coactions among . . . multilayered processes in any given project, program or lifetime, psychological theory and research would assume a dramatically different form if operated from the *mindset* of analytic holism.
>
> The developmental sciences are leading the way in ushering such a mindset into psychology and related fields. The study of development focuses on origins, transformations and trajectories. As the early Greeks might say, from a consistently *developmental* approach, "nothing is, everything becomes." From this view, we cannot take the forms of behavior that appear before as fixed, final or fossilized; they are products of development. Our search for the developmental origins of order requires that we explain how more powerful structures can emerge over time from less powerful ones. The moment we reject the primitivism that higher order structures have their origins in some sort of first cause (e.g., God, genes, and nature), we are ready to build a path toward dynamic, emergent, embodied, and relational modes of knowing.
>
> (pp. 220–221)

In sum, Fischer and his colleagues see development as a fully relational process. This relationism involves all levels of organization in the developmental system, and includes, therefore, the general, but abstract, linkage between all endogenous and exogenous stimulation and, as well, more specific and concrete linkages between a person and others in his or her social world. A growing theoretical and empirical literature underscores the importance of such person-to-person "dynamic collaborations" in cognitive development. The scholarship of Barbara Rogoff exemplifies this work.

ROGOFF'S SOCIOHISTORIC-CULTURAL THEORY

Many conceptually important and empirically useful sociocultural theories have followed from the pioneering contributions of Vygotsky (e.g., 1927, 1933). Examples of this scholarship include the contributions of Cole (e.g., 2006), Gauvain and Perez (2015), Greenfield, Suzuki, and Rothstein-Fisch (2006), Goodnow and Lawrence (2015), Mistry and Dutta

Barbara Rogoff

(2015), and Shweder, Goodnow, Hatano, Levine, Markus, and Miller (2006). I focus here on the contributions of Barbara Rogoff (e.g., 1998, 2003, 2011), in that her formulation of a sociohistoric-cultural model for studying the contributions of culture to human development involves ideas strongly tied to RDS metatheory. Her approach focuses on the mutually constitutive or integrative nature of individual development and culture (e.g., Rogoff, 1998). As such, Rogoff's (1998, 2003, 2011) model reflects the individual↔context relations focus—in this case, an individual↔culture focus—of other RDS-based models.

As Mistry and Dutta (2015) explain, and as reflected as well in the dynamic coactive systems theory of Fischer and his colleagues, Rogoff's approach is predicated on the inseparability of individual and cultural levels of functioning. In contrast to the approaches that regard individual development and culture as split—what Mistry and Dutta (2015) term the separate and independent view—culturally-oriented theorists such as Rogoff (e.g., 1998, 2003) and theorists who derive their ideas from RDS metatheory (e.g., see the models discussed earlier in this chapter and, as well, in the chapters in Overton & Molenaar, 2015) agree about the inseparability of individual and culture/context.

Moreover, and consistent with the action theories discussed earlier in this chapter (e.g., Baltes & Baltes, 1990; Brandtstädter, 1999, 2006; Heckhausen, 1999, 2011), Rogoff's sociohistoric-culture theory involves a focus on *action* as the unit of analysis. For instance, and reflective of the SOC model proposed by Baltes (1997; Baltes & Baltes, 1990; Baltes et al., 2006; Freund & Baltes, 2002), the unit of analysis consists of agentic individuals enacting goal-directed behavior (i.e., making selections) and pursuing goals by employing culturally-valued tools (i.e., optimization) (e.g., Cole, 2006; Mistry & Dutta, 2015; Rogoff, 2003).

Because of the individual↔culture relation, actions involving goal-directedness and the use of cultural tools involve both the employment of physical tools and objects and, as well, conceptual tools, such as language, writing, and number systems (Mistry & Dutta, 2015). Therefore, the cognitive processes involved in the use of conceptual tools—in the sign systems of a culture—demonstrate the embodiment (Overton, 2015a) of mental life in culture, of culturally facilitated cognitive processes (Mistry & Dutta, 2015). Thus, Rogoff (2003) discusses the existence of, and provides examples of, cognition beyond the skull, and notes that "cognition is distributed across individuals, other people, and cultural tools and institutions" (p. 271). Thus, developmental processes involving cognition or, as well, all other features of ontogenetic functioning (e.g., see Raeff, 2016) involve an individual's engagement in culturally-based activities that include use of culturally-mediated conceptual tools and sign systems (Mistry & Dutta, 2015; Rogoff, 2003).

As with the dynamic coactive systems model of Fischer and his colleagues, Rogoff's (e.g., 1998, 2003, 2011) sociohistoric-cultural theory stands in contrast to reductionist models of development as, for instance, involved in neo-nativist conceptions (e.g., Keil, 1998, 2006; Spelke & Newport, 1998). Neo-nativists place learning and/or cognitive development within the "head" of the individual. Denying the relevance for the development of intrinsic abilities of exogenous experience (of the empiricism position, in their terms; Spelke & Newport, 1998), neo-nativists believe that the variables or processes involved in the genesis of cognitive competence are intraindividual ones. In contrast, scholars following an RDS-based view stress the relations among all the intraindividual and interindividual levels of organization involved in the developmental system and, as such, see these levels dynamically collaborating (e.g., Fischer & Bidell, 1998; Mascolo, 2013; Mascolo & Fischer, 2015) in the development of cognition.

Critically, to Rogoff (1998, 2003, 2011) these levels contributing to the development of an individual's cognition include those involving other people (e.g., families, peer groups, communities, and cultures). Thus, in Rogoff's model, cognition is not an individual activity. Cognition involves interindividual relationships, associations embedded within a sociohistoric-cultural context (e.g., Baltes & Staudinger, 1996; Cole, 1985, 2006; Cooper, 1980; Damon, 1984; Mistry & Dutta, 2015; Rogoff, 2003; Leont'ev, 1981; Rogoff & Chavajay, 1995; Staudinger & Baltes, 1996; Valsiner, 1998; Vygotsky, 1978; Wertsch, 1981, 1985). Describing cognition as an interpersonal and sociohistoric-cultural collaborative process, Rogoff (1998) noted that:

> The paradigm shift required to move from thinking of cognition as a property of individuals to thinking of cognition as an aspect of human sociocultural activity (without attempting to locate the process only in individuals) is at the edge of the "zone of proximal development" of the field at this point.
>
> (p. 680)

Rogoff (1998) noted, however, that this emerging, cutting-edge concern with the interpersonal and contextual bases of an individual's cognitive competence did not begin at the end of the twentieth century, with the emergence of RDS-based theories as cutting-edge models of human development in general (e.g., Lerner, 2006a, 2006b, 2012, 2015a, 2015e) and in cognitive development in particular (e.g., Feldman, 2000; Fischer & Bidell, 1998; Mascolo, 2013; Mascolo & Fischer, 2015; Thelen & Smith, 2006). Rather, the idea of cognition as a collaborative process arose at the beginning of the twentieth century, through the contributions of Vygotsky (e.g., 1927/1982, 1933/1966, 1978) and Piaget (e.g., 1923, 1950, 1952, 1954). That is, Rogoff (1998, p. 680) noted that the former theorist used cultural/theoretical ideas to argue "that individual development was an aspect of cultural/historical activity," whereas the latter one contended that cognitive development occurred "through co-operation as individuals attempt to resolve conflicts between their perspectives." Moreover, Rogoff (1998) explained that:

> One of the key commonalities between the cultural/historical and Piagetian approaches to cognition as a collaborative process is an emphasis on achievement of shared thinking. In the process of everyday communication, people share their focus of attention, building on a common ground that is not entirely shared (for each person works with a somewhat unique perspective). To engage in shared endeavors, there must be some common ground, even to be able to carry out disputes . . . Mutual understanding between people in communication has been termed intersubjectivity, a process that occurs between people; it cannot be attributed to one person or the other in communication . . . Some modification in the perspectives of each participant are necessary to understand the other person's perspective. The modifications can be seen as the basis for development—as the participants adjust to understand and communicate, their new perspectives involve greater understanding and are the basis for further growth.
>
> (pp. 681–682)

Within this interpersonal and culturally embedded frame, Rogoff described several means through which interpersonal relations influence the cognitive development of an individual. For instance, Rogoff (1998) noted that:

> Cognitive development occurs as new generations collaborate with older generations in varying forms of interpersonal engagement and institutional practices. For example, in some communities, conversation between adults and young children is common, but children seldom have the opportunity to observe and participate in adult activities; in other communities, engagement between adults and young children occurs in the context of children's involvement in the mature activities of the community, but not in peer-like conversation . . . The topic of cognition as a collaborative process necessarily includes all such forms of collaboration.
>
> (p. 680)

Moreover, Rogoff (1998, p. 681) described the importance of the aid that experts in a given knowledge domain give to "novices" in that domain, and explained that the use by experts of various techniques to support the learning of novices (e.g., tutoring, scaffolding of concepts, or adjustment of learning supports to match the needs novices have for assistance) promotes learning through enactment of "the mutual roles of children and adults in structuring adult–child interaction" in the service of cognitive development. In turn, Rogoff (1998) explained the significant role that child-to-child (peer) interaction plays as well in the development of an individual's cognitive capacity. Rogoff (1998) indicated that:

> How peers assist each other in learning addresses concepts . . . [such as] . . . collaboration in peer play and child caregiving, the role of similarity of status in collaborative argumentation, and peers' facilitation of each other's learning in classrooms. It also includes consideration of how children and the adults and institutions that work with them learn to collaborate.
>
> (p. 681)

In fact, given the embeddedness of child–adult and child–child learning in a socioculturally shaped institutional context, Rogoff (1998) pointed out that: "Collaboration involves groups larger than dyads and includes specialized asymmetrical as well as symmetrical roles between participants, discord as well as harmony, and collaboration among people of different eras and locations" (p. 681). As a consequence of this diversity of social players, of the roles they enact, and the behaviors they manifest, collaboration "is a process that can take many forms, whether intended or accidental, mutual or one-sided, face-to-face, shoulder-to-shoulder, or distant, congenial or contested; the key feature is that in collaboration, people are involved in others' thinking processes through shared endeavors" (Rogoff, 1998, p. 728).

As did Fischer and his colleagues (e.g., Mascolo, 2013; Mascolo & Fischer, 2015), Rogoff (2003) recognized that the variability in cognitive development was a critical topic for analysis by developmental scientists framing their work within RDS-based models. To conceptualize such variability within an RDS-based approach, Rogoff (2003) introduced the idea of abstracting regularities in variation (that is, of consistent patterns of variation) across cultural communities. Simply, Rogoff recognized the importance of understanding individual differences in human development (non-ergodicity, idiography; Molenaar & Nesselroade, 2015; Rose, 2016) and, at the same time, potential commonalities across individuals.

Mistry and Dutta (2015) explain that this dual focus involves Rogoff's qualitative methods of investigation and is analogous to quantitative methods (e.g., of Molenaar & Nesselroade, 2014, 2015; Nesselroade & Molenaar, 2010) that focus on latent constructs derived from first-order, idiographic factors and their loadings on the manifest variables. That is, there is an alignment between the idea of regularities or patterns in variation found in Rogoff's (2003) work and the commonality existing at a latent-variable level that was described by Molenaar and Nesselroade (2014, 2015). Mistry and Dutta (2015) explain that, in differentiating between manifest and latent levels of variation, Molenaar and Nesselroade (2015; Nesselroade & Molenaar, 2010) use the concept of the *idiographic filter* to help developmental scientists explain how commonalities can emerge in a second-level analysis of individually distinct, that is, idiographic, patterns (i.e., interindividual variation in individuals' intraindividual change).

In sum, Molenaar and Nesselroade (2015) used quantitative procedures to generate from idiographic variability nomothetic latent variables

Jayanthi Mistry

Ranjana Dutta

reflecting commonality in group patterns. Quite creatively, Rogoff (2003) addressed such variability by using qualitative methods to identify cross-individual regularities from idiographic data, thus enabling commonality to be identified across the groups she studied. As such, Rogoff (2003) used qualitative and interpretive approaches to conduct in-depth analysis of a few cases of individuals but was able to make more general, collective statements through the use of her qualitative version of the Molenaar and Nesselroade (e.g., 2015; Nesselroade & Molenaar, 2010) idiographic filter (Mistry & Dutta, 2015).

Conclusions about the Ideas of Rogoff

Rogoff (1998) advanced a concept of cognitive development that richly capitalizes on the idea of dynamic collaborations, advanced by Fischer and Bidell (e.g., 1998, 2006; Mascolo & Fischer, 2015), to depict the thorough, interlevel integrations that constitute the process of human development depicted within RDS-based theories. By explaining how the dynamic collaborations within the developmental system are significantly instantiated by interpersonal, intergenerational, and person↔institutional relationships, all embedded with the sociohistoric-cultural moment, Rogoff (1998)

> goes beyond regarding the individual as a separate entity that is the base unit of analysis to examine sociocultural activity as the unit of analysis, with examination of the contributions of individual, interpersonal, and community processes. Thus, analysis goes beyond the individual and the dyad to examine the structured relations among people in groups and in communities, across time.
>
> With sociocultural activities as the units, analysis emphasizes the purposes and dynamically changing nature of events. Analysis examines the changing and meaningful constellations of aspects of events, not variables that attempt to be independent of the purpose of the activity. Central to analysis of cognition as a collaborative process is a focus on the shared meaning in endeavors in which people engage in common. Cognition is not conceptualized as separate from social, motivational, emotional, and identity processes—people's thinking and development is conceived as involved in social relations, with purpose and feeling central to their involvement in activities, and transformation of their roles as a function of participation.
>
> (p. 729)

In short, the ideas of Rogoff (e.g., 1998, 2003, 2011) help transform the concept of cognitive development from a phenomenon seen, within neo-nativist theory, to have exclusive, endogenous (genetic) bases to one linked dynamically to other people and to the social institutions and cultures created by their actions. Indeed, individuals' own actions, in concert with the other levels of the dynamic developmental system that they influence and are influenced by, are brought to the fore of concern in the developmental systems theoretical perspective exemplified by Rogoff's (1998) perspective.

Her ideas, like those of other cognitive theorists exploring the use of RDS-based ideas (e.g., Baltes et al., 1998, 2006; Feldman, 2000; Fischer & Bidell, 1998, 2006; Magnusson, 1996; Magnusson & Stattin, 2006; Overton, 2015a; Sameroff, 1983, 2009; Thelen & Smith, 2006; Wapner & Demick, 1998), underscore the importance of approaching the description and explanation of cognitive phenomena from a perspective that integrates (that does not split) the levels of organization comprising the ecology of human development. Indeed, the idea of levels of integration (instead of split levels) emphasized (at this writing) more than a half-century ago by Schneirla (1956, 1957; Maier & Schneirla, 1935), is an idea of substantial use to scholars framing their theoretical and empirical work within RDS metatheory.

If the past is at all prelude, then it seems safe to predict that the study of cognitive development will, as it did across the twentieth century, be likely to continue to engage the interest and energy of scholars of human development across the twenty-first century. However, I believe that it is likely that the admonition of Overton (2015a) to "avoid all splits" will become very much a rallying cry of scientists interested in both richly describing and adequately explaining the features of cognitive development.

LEVELS OF INTEGRATION AND THE EXPLANATION OF COGNITIVE DEVELOPMENT

As illustrated by the discussion in this chapter, approaches to development that are predicated on split notions of nature and nurture—either those associated with hereditarian conceptions of development, neo-nativist ones, or nurture ones—fail on several logical, theoretical, and empirical grounds. In turn, theories that integrate levels, and do so within a frame provided by dynamic, RDS-based ideas, appear at this writing to succeed on these very same grounds. It may be, then, that such theories will be the ones that prove most engaging to developmental scientists studying cognitive processes throughout the rest of the twenty-first century.

A similar prediction was made by Deanna Kuhn, in the afterword she provided to the second volume of the fifth edition of the *Handbook of Child Psychology* (Damon, 1998). Specifically, Kuhn (1998) indicated that:

> One prediction, however, is that we will demand of future theories that they account for more than a very narrow range of phenomena, particularly those tied to a specific task, even if they account for those phenomena quite well. We also can predict that future theories will be more inclusive than past ones, not only in incorporating multiple mechanisms as suggested earlier, but also in integrating multiple levels of explanation. At the moment, progress is being made in constructing accounts of cognitive development at the neurological level; at the level of action systems and perceptual systems; as well as at the various cognitive levels of representational systems, knowledge, strategies, information processing mechanisms, and metacognitive awareness and control. Social systems represent yet another

Deanna Kuhn

> level of explanation. It is a misconception to treat these explanatory accounts as alternatives . . . recognizing the coexistence of all of these different levels of explanation should remind us that it is in fact one individual who incorporates these systems—from the physical and molecular to the teleological and reflective—and that ultimately these levels of explanation must be integrated.
> (p. 981)

As illustrated by the sample cases of RDS-based theories discussed in this chapter (and see as well other examples, published in volumes devoted to theories of development, e.g., Hood et al., 2010; Lerner & Benson, 2013a, 2013b; Molenaar et al., 2014; Overton & Molenaar, 2015), Kuhn's (1998) predictions are being confirmed in regard to the elaboration or updating of diverse theories of human development.

CONCLUSIONS: USING RDS-BASED IDEAS AS A FRAME FOR DISCUSSING GENETIC REDUCTIONIST MODELS

In the view of the theories discussed in this chapter, as well as the other instances of RDS-based theories reviewed in prior chapters, individuals are part of a fused, multilevel, dynamic system. A key to understanding development from the perspective of these theories is in the notion of integrative levels, for example, as formulated by Schneirla (1957), and earlier by Novikoff (1945a, 1945b). These ideas and, as well, the other defining features of RDS-based theories are summarized in Table 10.4. Simply, then, from this table as well as from my discussion across this chapter and prior ones, a key idea involved in all RDS-based models of the dynamics of the developmental system is to "avoid all splits" (Overton, 2015a).

Table 10.4 Defining features of the relational developmental systems metatheory

Process-Relational Paradigm

Predicated on a philosophical perspective that transcends Cartesian dualism and atomism, theories derived from the relational developmental systems metatheory are framed by a process-relational paradigm for human development. This focus includes an emphasis on process and a rejection of all splits between components of the ecology of human development (e.g., between nature- and nurture-based variables, between continuity and discontinuity, and between stability and instability). Holistic syntheses replace dichotomies, as well as reductionist partitions of the developing relational system, through the integration of three relational moments of analysis: the identity of opposites, the opposites of identity, and the syntheses of wholes. Deriving from the process-relational paradigm, relational developmental systems-based models posit the organism as an inherently active, self-creating, self-organizing and self-regulating nonlinear complex adaptive system, which develops through embodied activities and actions, as they coact with a lived world of physical and sociocultural objects.

The Integration of Levels of Organization

Relational thinking, with the rejection of Cartesian splits, is associated with the idea that all levels of organization within the ecology of human development are integrated or fused. These levels range from the biological and physiological through the cultural and historical.

Developmental Regulation across Ontogeny Involves Mutually Influential Individual↔Context Relations

As a consequence of the integration of levels, the regulation of development occurs through mutually influential connections among all levels of the developing relational system, ranging from genes and cell physiology through individual mental and behavioral functioning to society, culture, the designed and natural ecology, and, ultimately, history. These mutually influential relations may be represented generically as Level 1↔Level 2 (e.g., Family↔Community), and in the case of ontogeny may be represented as individual↔context.

Integrated Actions, Individual↔Context Relations, Are the Basic Unit of Analysis within Human Development

The character of development regulation means that the integration of actions—of the individual on the context and of the multiple levels of the context on the individual (individual↔context)—constitute the fundamental unit of analysis in the study of the basic process of human development.

Table 10.4 *continued*

Temporality and Plasticity of Human Development
As a consequence of the fusion of the historical level of analysis—and therefore temporality—in the levels of organization comprising the ecology of human development, the developing relational system is characterized by the potential for systematic change, by plasticity. Observed trajectories of intraindividual change may vary across time and place as a consequence of such plasticity.

Relative Plasticity
Developmental regulation may both facilitate and constrain opportunities for change. Thus, change in individual ↔ context relations is not limitless, and the magnitude of plasticity (the probability of change in a developmental trajectory occurring in relation to variation in contextual conditions) may vary across the life span and history. Nevertheless, the potential for plasticity at both individual and contextual levels constitutes a fundamental strength of all human development.

Intraindividual Change, Interindividual Differences in Intraindividual Change, and the Fundamental Substantive Significance of Diversity
The combinations of variables across the integrated levels of organization within the developmental system that provide the basis of the developmental process will vary at least in part across individuals and groups. This diversity is systematic and lawfully produced by idiographic, group differential, and generic (nomothetic) phenomena. The range of interindividual differences in intraindividual change observed at any point in time is evidence of the plasticity of the developmental system, and gives the study of diversity fundamental substantive significance for the description, explanation, and optimization of human development.

Interdisciplinarity and the Need for Change-Sensitive Methodologies
The integrated levels of organization comprising the developmental system require collaborative analyses by scholars from multiple disciplines. Interdisciplinary knowledge is a central goal. The temporal embeddedness and resulting plasticity of the developing system requires that research designs, methods of observation and measurement, and procedures for data analysis be change- and process-sensitive and able to integrate trajectories of change at multiple levels of analysis.

Optimism, the Application of Developmental Science, and the Promotion of Positive Human Development
The potential for and instantiations of plasticity legitimate an optimistic and proactive search for characteristics of individuals and of their ecologies that, together, can be arrayed to promote positive human development across life. Through the application of developmental science in planned attempts (interventions) to enhance (e.g., through social policies or community-based programs) the character of humans' developmental trajectories, the promotion of positive human development may be achieved by aligning the strengths (operationalized as the potentials for positive change) of individuals and contexts.

Source: Based on Lerner (2006b) and Overton (2013).

However, as I also noted in previous chapters, this integrative view of the nature and nurture of human development has not been, and at this writing is still not, the only theoretical formulation used to understand the contributions that different levels of organization make to human development. Rather, split conceptions continue to exist in the study of human development (Lerner, 2014, 2017). Although I noted in previous chapters that there have been and are some exclusively nurture versions of split conceptions (e.g., Bijou, 1976; Bijou & Baer, 1961; Gewirtz & Stingle, 1968), the split conceptions most frequently employed at this writing emphasize the primary role of nature variables in human development.

Just as a range of developmental systems theories exist, so, too, is there a range of such split, nativist theories that have been used and/or that are currently used (at this writing) in the study of human life. In all cases, these theories possess significant, indeed fatal, conceptual flaws. In addition, in all cases, ideas associated with RDS-based theories provide information useful in critiquing these nature-based, reductionist models. My goal is to illustrate this use in the next two chapters.

CHAPTER ELEVEN

Genetic Reductionism in Developmental Science

Sample Cases from the Twentieth Century

Many experimental biologists outside of the biomedical–industrial complex are just now coming (back) to grips with the facts of epigenesis; with the profound mystery that developmental biology is, with the poverty of gene programs as an explanatory device; and with a crisis defined by the realization that an increasingly deficient theory of developmental genetics is the only theory currently available. The question remains: if biologists are starting to learn this lesson, will the psychologists be far behind?
Richard C. Strohman (1993b, p. 101)

Genes are part of the developmental system in the same sense as other components (cell, tissue, organism), so genes must be susceptible to influence from other levels during the process of individual development.
Gilbert Gottlieb (1992, p. 167)

Imagine for a moment that dropping out of high school was a crime. Imagine as well that you are a judge of a case involving a 15-year-old girl who had been convicted of dropping out of high school, as had her mother and her grandmother. Declaring that "Three generations of high-school dropouts are enough!" you sentence the girl not to prison but to being sterilized. For at least this family, you intend to put an end to the transmission across generations of dropping out of high school.

Ridiculous, you say. A nonsensical, pointless, and even stupid anecdote. Perhaps. But forget about imagination. Consider this actual set of events:

In 1927 Supreme Court Justice Oliver Wendell Holmes wrote in support of a decision upholding a Virginia law that authorized sterilization of "mental defectives" without their consent. He agreed that a young woman, Carrie Buck, should be sterilized because she was unfit to reproduce. Raped, and now pregnant, her mental defect was evidenced by the fact that she was going to have a baby out of wedlock. Justice Holmes wrote:

> We have seen more than once that the public welfare may call upon the best citizens for their lives. It would be strange if it could not call upon those who already sap the strength of the State for these lesser sacrifices, often not felt to be such by those concerned, in order to prevent our being swamped with incompetence. It is better for all the world if, instead of waiting to execute degenerate offspring for crime or to let them

> starve for their imbecility, society can prevent those who are manifestly unfit from continuing their kind . . . *Three generations of imbeciles are enough*.
>
> (Buck v. Bell, 274 U.S. 200, italics added)

And so Carrie Buck was sterilized. Although her pregnancy was not aborted and she eventually gave birth, she was kept from passing along her genes any further, so that her mental defectiveness could not be a further infliction on society. But Carrie Buck's experience was not unique. Doerr (2009) explained that "State laws permitting sterilization of individuals deemed unfit to reproduce—most commonly institutionalized persons with mental illness, or even conditions such as epilepsy—were common in the first half of the twentieth century" (p. 1). The Virginia law that resulted in Carrie Buck's forced sterilization was not repealed until 1974. However, before this law and the comparable laws in more than 30 other states were repealed, more than 65,000 people were forcibly sterilized in the United States—to protect society from them spreading their allegedly defective genes.

Moreover, doctors under contract with the California Department of Corrections and Rehabilitation forcibly sterilized about 150 female inmates from 2006 to 2010; the women targeted for sterilization were those deemed likely to return to prison in the future (Johnson, 2013). It was not until September 2014 that California Governor Jerry Brown signed a bill prohibiting forced sterilizations in prisons.

THE CONCEPTUAL AND EMPIRICAL FAILURES OF GENETIC REDUCTIONISM

I have often reflected on a calamitous irony besetting social and behavioral science: Some people holding advanced degrees in their fields (and therefore who presumably should know better) do not recognize the logical flaws associated with their use of the 70-plus-year-old so-called evolutionary modern synthesis, which entailed the integration of Mendelian genetics with neo-Darwinian variation and natural selection. This position posits that there is a unit of natural selection—a gene—that remains fixed and immutable in its functional significance across eons of exchanges between individuals and their contexts, that is, across incalculable instances of the very process of evolutionary change, of natural selection and adaptation, discussed by Darwin. Examples of such fallacious reasoning occur in what is termed parental investment theory within evolutionary psychology (e.g., Belsky, 2012; Ellis, Schlomer, Tilley, & Butler, 2012), and in the use of sociobiological models of intellectual differences among racial groups (Rushton, 1992, 2000; Wade, 2014).

This logical problem is coupled with abundant and burgeoning evidence that genes are outcomes of evolutionary processes and not bases of them (Pigliucci & Müller, 2010; West-Eberhard, 2003). There is a similarly large and convincing literature that genetic function is a relatively plastic outcome of mutually influential relations among genes and the multiple levels of the context within which they are embedded: cellular and extracellular physiological processes, psychological functioning, and the physical, social, and cultural features of the changing ecology that, together, create epigenetic change (e.g., Bateson, 2015, 2016; Cole, 2014; Jablonka & Lamb, 2005; Keller, 2010; Lester et al., 2016; Meaney, 2010, 2014; Misteli, 2013; Moore, 2015a, 2016; Slavich & Cole, 2013).

These data make fanciful, at best, the Kipling-like "Just-So Stories" about how genes function (Gould & Lewontin, 1979). The imagined stories of how the genes that afforded survival on the African savannah now explain sexual and reproductive behaviors among contemporary girls of color (e.g., Belsky, 2012; Belsky, Steinberg, & Draper, 1991) stretch credulity beyond reasonable bounds. Current biological science data indicate that epigenetic changes in mutually influential gene–context relations may persist across generations (Lester et al., 2016; Meaney, 2010, 2014; Misteli, 2013; Moore, 2015a, 2016). Therefore, the illogical claims of social and behavioral scientists who tell these stories, and thereby adopt Cartesian split and reductionist claims about the function of genes, are also counterfactual. Given the bad science emblematic of these genetic reductionist claims, a neophyte social or behavioral scientist—or a molecular geneticist happening on such egregiously flawed ideas—might expect that genetic reductionism would not be taken seriously by competent social and behavioral scientists. They might expect that such scientists would vociferously and visibly dismiss this thinking, in any form that it might occur.

However, if these observers continued to pay attention to the literatures of these fields, they would learn that the presence of these ideas persists. As in the children's game, Whack-A-Mole, observers would see that, as soon as the failures of one instantiation of genetic reductionism are compellingly refuted, other instances of this problem-riddled conception pop up. I discuss some features of this sorry history in this chapter and in the next one.

In the 1940s through the 1970s Konrad Lorenz (e.g., 1939, 1940a, 1940b, 1966, 1974a, 1974b, 1974c) presented these fundamentally flawed ideas about the "hard-wired" links between genes and behaviors to explain imprinting in precocial birds *and, as well, the ethical inferiority of Jews*! His ideas were then thoroughly countered and dismissed, for instance, by Lehrman (1953, 1970) and by Schneirla (1957, 1966). Nevertheless, the ideas resurfaced again in the heritability work of Jensen (1969, 1980) regarding racial differences in intelligence. Herrnstein and Murray (1994) and Rushton (e.g., 2000) reiterated genetic reductionist arguments for the bases of racial differences in intelligence test scores.

In addition, genetic reductionist conceptions resurfaced in human sociobiological ideas about gender differences in sexuality and parenting (e.g., Dawkins, 1976; Freedman, 1979) and, as noted in prior chapters (e.g., see Chapter 10), they arose as well in the postulation of the taxonomic approach to human individuality that is termed Five Factor Theory (e.g., Costa & McCrae, 1980; McCrae et al., 2000), that is, that there are five "big traits" (conscientiousness, agreeableness, neuroticism, openness to experience, and extraversion) that are fixed, stable, and biologically-set facets of personality. These purportedly fundamental facets of individual functioning are held to reflect "nature over nurture" and to involve attributes that "are more or less immune to environmental influences . . . significant variations in life experiences have little or no effect on measured personality traits" (McCrae et al., 2000, pp. 175–176).

Moreover, these ideas surfaced once again in evolutionary developmental psychology. Here genetic reductionism is used to generate explanations of how problematic father–daughter relations result in also problematic reproductive behavior of the daughters during adolescence (e.g., Belsky et al., 1991; Ellis et al., 2012).

These flawed and empirically counterfactual ideas just continue to appear, much like a virulent virus that mutates to preserve itself. The dimensions of bad science reflected in these genetic reductionist ideas are legion and go well beyond logical problems and the misrepresentation of the fundamental features of genetic functioning within the ecology of human development. There are issues of conflating description with explanation and of equating purported analogy with biological homology (e.g., Atz, 1970). In addition, genetic reductionists use statistics that summarize group trends and neglect problems in data analysis and interpretation due to inappropriately inferring homogeneity and stationarity of data sets. That is, in their computations of heritability coefficients, genetic reductionists implicitly accept or infer ergodicity (Molenaar, 2014; Molenaar & Nesselroade, 2012, 2014, 2015; Nesselroade & Molenaar, 2010; Rose, 2016), when human development is fundamentally non-ergodic (Molenaar & Nesselroade, 2015). Moreover, genetic reductionists typically ignore low levels of variance and weak effect sizes in promulgating sweeping generalizations about genetic influences on behavior. Furthermore, they often advocate, without adequate evidence, ways in which their biological reductionist interpretations can be applied to programs and policies (e.g., Belsky, 2014; Rushton, 2000; Wade, 2014).

These erroneous extensions and interpretations of the idea of genetic reductionism create social mischief, at best, and racial, ethnic, class, gender, and so on, divisiveness and social turmoil, at worst. The recommended applications of bad science reinforce the fears of institutionalized racism in America and further the marginalization of minority group members in society (Lerner, Harris, Agans, Arbeit, & DeSouza, 2014). However, these are issues that are never publicly considered by those who promulgate these flawed extensions of counterfactual genetic reductionism.

The past and present use of genetic reductionist ideas in developmental science influence both the overall quality of the scholarship in this field and the quality of life of millions of people (and, indeed, as in the case of Carrie Buck, and millions of others faced with the social action agenda of people who believe in ideas derived from genetic reductionism, life itself). Accordingly, in this chapter and Chapter 12 I review past and present genetic reductionist formulations in developmental science. The historian George Santayana famously said that those who do not remember the past are condemned to repeat it. My hope is that by explaining the past and present instances of genetic reductionist formulations in developmental science, developmental scientists will not repeat these mistaken ideas in the future of the field or in the society of today's or tomorrow's children and grandchildren. In this chapter, I focus on twentieth-century examples of these flawed ideas, and I begin this discussion by recounting the ideas of Nobel Laureate Konrad Lorenz.

SOME TWENTIETH-CENTURY EXAMPLES OF GENETIC REDUCTIONISM: 1. KONRAD LORENZ

> *The ultimate wisdom is always the understanding of instinct.*
> Hitler (*Mein Kampf*, 1925, pp. 244–245)

> *I am by inheritance obsessed with eugenics.*
> Konrad Lorenz (1974b, quoted in Cox, 1974, p. 20)

Konrad Zacharias Lorenz (1903–1989) was the foremost proponent of a branch of biology termed ethology, which involves the study of the evolutionary and, it was argued, the hereditary bases of animal behavior. Lorenz's ethological work was built around his conceptualization of the notion of "instinct," a conceptualization that was a key intellectual basis of the hereditarian theory of behavior and development termed sociobiology (Wilson, 1975a). Historian Robert Richards (1987, p. 528) explained that Lorenz "gave conceptual and empirical shape to the modern science of ethology, the science which has been further elaborated into (and . . . Wilson believes absorbed by) sociobiology."

Konrad Lorenz

As suggested by his influence on the field of sociobiology, Lorenz's contributions to science were numerous and highly valued by many scholars. In fact, with two other eminent ethologists—Nikolaas Tinbergen and Karl von Frisch—Konrad Lorenz was awarded the Nobel Prize for Medicine or Physiology in 1973. The award was given for his ethological theory and research regarding instinctual behavior in animals, particularly precocial birds (i.e., birds that walk or swim efficiently immediately after hatching).

As discussed in Chapter 7, Lorenz (1965) used the term imprinting to describe such birds' social attachment to, and following along after, the first moving object they saw after hatching. Usually this "object" was a member of their own species, typically their mother, and the newly hatched birds would follow after this other bird. Upon reaching sexual maturity, the birds would try to mate with another such social object, in this case another bird of their own species.

Lorenz (1965) attempted to demonstrate the fixed nature of imprinting by showing that following and, later, mating behaviors could be directed to virtually any living organism, as long as it was the first moving object a newly hatched bird saw in the first critical hours after its birth. Birds could also be made to imprint on boots or even on Lorenz himself. Numerous introductory psychology texts accompany their discussions of imprinting with a photograph of a somewhat stooped and kindly appearing Dr. Lorenz, an elderly man with white hair and beard, being followed by a troop of young goslings.

Lorenz's Conception of "Instinct"

As discussed in Chapter 7, to Lorenz (1965), the imprinting phenomenon was an instance of instinctual behavior in animals. Beginning in his earliest publications (e.g., Lorenz, 1932/1970, 1935, 1937a, 1937b), Lorenz was concerned with the concept of instinct. Following the "discovery" by Oskar Heinroth of such phenomena, Lorenz (1932/1970) noted "that there are motor patterns of constant form which are performed in exactly the same manner by every healthy individual of a species." He presented five criteria for determining whether an observed pattern of behavior reflected "inherited drives of fixed behaviors."

Lorenz's (1937a, 1943b/1954) criteria for an instinctual behavior pattern (see Richards, 1987) were:

1. Appearance of the behavior pattern in virtually all individuals of a species.
2. Appearance of the behavior pattern in species members who were reared in experimentally controlled isolation (i.e., who were [purportedly] deprived of experience).
3. Complexity of the behavior pattern (i.e., the learning capacity of the individual should not be sufficient for the acquisition of the behavior pattern yet the behavior pattern is present).
4. Appearance, or "release," of the behavior pattern either at inappropriate times or in incomplete ways (e.g., a bird may try to build a nest outside of the mating season).
5. Fixity and rigidity (i.e., stereotypy and non-plasticity) of the behavior pattern—in other words, the behavior pattern takes the same form whenever it appears.

According to Lorenz (1937a, 1937b), an instinctual behavior pattern could occur in one of two ways. First, an instinct could be observed when the individual experienced a specific "releasing" stimulus—that is, when the organism encountered a certain stimulus that "triggered" a given instinct—or, second, the instinct could be released in a seemingly spontaneous manner (Lorenz, 1937a). To explain these bases for the occurrence of an instinctual behavior pattern, Lorenz (1937a, 1937b, 1965; see too Richards, 1987) posited the existence of an "innate releasing mechanism" (IRM), a hypothetical mechanism believed to involve a set of receptor cells that released the instinctual behavior pattern when activated by a specific environmental stimulus.

More specifically, Lorenz saw as instinctual specific inherited properties of nervous system structures (Lehrman, 1970). Some groups of neurons, he claimed, have specific instinctive properties built into them (Lehrman, 1970). The structures obtain these properties directly from the organism's genetic inheritance, from the "interaction of the species with its environment during evolution . . . [that is,] by mutation and selection, a method analogous to learning by trial and success."

According to Lorenz (1937a, 1965), experience over the course of an organism's life (its ontogeny) has no role in the shaping (the development of) this neural structure. Instead, as Lehrman pointed out, the key innate feature of such a neural structure is "its ability to select, from the range of available possible stimuli, the one which specifically elicits its activity, and thus the response seen by the observer" (Lehrman, 1970, p. 24).

A classic example of a "fixed action," or instinctual, pattern deriving from an IRM involves the male three-spined stickleback fish (Lorenz, 1965; Richards, 1987). When this fish encounters another male three-spined stickleback with a red belly, the fish displays a set of behaviors indicative of threat; however, when the fish encounters a female with a swollen (but non-red) belly, the male displays the behavior pattern indicative of mating. Similarly, graylag geese display a fixed action pattern involving escape responses when they encounter a white-tailed eagle, the only flying predator that is a danger to these geese (Lehrman, 1970). However, the instinctual escape response can also be released if the goose is exposed to any object gliding slowly and silhouetted against the sky (Lorenz, 1965; Richards, 1987).

Thus, more than one specific stimulus can engage the IRM and release an instinct. Indeed, as noted, fixed action patterns can occur "spontaneously"—that is, if the appropriate releasing stimulus has not been encountered for some period of time, then (apparently because of an accumulation of energy associated with the instinct and/or the IRM) the fixed action pattern "might go off in vacuo, as if dammed energy burst through containing valves" (Richards, 1987, p. 531). Because of this "spontaneous release" feature in IRMs, Lorenz (1965) came to view instincts not as sets of reflexes but as drives, as constructs having motivational properties. That is, Lorenz believed animals sought out stimuli that would release their instincts, which would dissipate the energy associated with the instincts that had presumably been built up (Lorenz, 1937b). In other words, Lorenz was saying that, unlike a reflex, wherein one behaves automatically, instincts have motivational properties: instincts drive one to engage in particular behaviors.

Criticisms of Lorenz's Conception of Instinct

Throughout his writings, Lorenz did not divorce himself from a commitment to the IRM concept, or from the belief that specific stimulus conditions can release, even in humans, a quite complex fixed action pattern (e.g., involving aggression and what Lorenz termed *militant enthusiasm* (Lorenz, 1965, 1966). However, his concept of instinct evolved in ways other than changing from a reflex-like construct to a motivational construct. Lorenz changed his conception of instinct, at least in part, in response to criticisms from several comparative psychologists.

For instance, some of the flaws in Lorenz's ideas were discussed in Chapter 7, in relation to the work that comparative psychologist T. C. Schneirla (1956, 1957) and his colleagues (e.g., Lehrman, 1953) did to counter hereditarian concepts such as Lorenz's view of instinct. Other scholars added to Schneirla's (1956, 1957) criticisms of Lorenz's concept of instinct, for instance, by pointing to the artificial and simplistic distinction Lorenz (1965) drew between instinct and learning (e.g., Hebb, 1949; Lehrman, 1970). That is, Lorenz (1965) often seemed to equate or subsume all non-evolutionary experience with the term "learning." However, there are actually many more ways in which experience can influence behavior. For example, diseases, natural events (e.g., storms and earthquakes), wars, famine, dietary regimens, technological changes, and social and political policies, laws, and cultural movements can all influence behavior—although those phenomena are not readily subsumed under the concept of "learning" (Baltes et al., 2006; Bronfenbrenner & Morris, 2006; Elder et al., 2015).

Scholars also objected to problems with experimental isolation studies (Lehrman, 1970). For instance, in such studies the researcher can deprive the animal of only some experiences, since it is not possible to deprive a living organism of all experience; even a dark box is an environment, although potentially a noxious or toxic one. Thus, an isolation experiment can tell researchers only that a particular experience is not necessary for a specific behavior; researchers can never determine from such a procedure that experience per se is not involved (Hebb, 1949).

Lorenz's (1937a, 1965) concept of instinct was also criticized for ignoring the problem of development across life, at least as far as the presence of the IRM was concerned. The issue of how genes, which are chemicals, interact with cells, tissues, organs, and the environment to "build" across life the neural structures involved in the IRMs is never adequately discussed by Lorenz (1937a, 1965, 1966; cf. Schneirla, 1956, 1957, 1966). Furthermore, Lorenz's concept of IRM was seen as problematic because it had an element of nonfalsifiability to it: An instinct was released either under specific environmental stimulus conditions or in the absence of them (Lehrman, 1970).

Lorenz's Responses to His Critics

In response to such criticisms, Lorenz contended in his 1965 book, *Evolution and Modification of Behavior*:

> What is preformed in the genome and inherited by the individual is not any "character," such as we can see and describe in a living organism, but a limited range of possible forms in which an identical genetic blueprint can find its expression in phenogeny . . . The term "innate" should never, on principle, be applied to organs or behavior patterns, even if their modifiability should be negligible.
>
> (p. 1)

Thus, Lorenz was arguing that it is not the behavior pattern, no matter how fixed it may seem, that is instinctual; rather, it is the "range" of the behaviors involved in the instinct that is innate. In other words, what is preformed in, or built into, the genome (the set of genes received or inherited at conception) is information about the forms of a given behavior that are possible for the species. Moreover, this conception of "instinct as information" requires the involvement of the environment in order for the presence of a behavior within the range of the instinct to be in evidence. This need for "nurture" in order to identify features of "nature" arises because, Lorenz (1965) contended, the environment is involved in the: "'Decoding' of genome-bound information . . . [and as a consequence] contrasting of the 'innate' and the 'learned' as mutually exclusive concepts is undoubtedly a fallacy" (p. 79).

Lorenz (1965) believed that by conceptualizing the information genes contain as behavior ranges (instead of fixed action sequences), he had addressed the criticisms of his purportedly simplistic division between "instincts" and "learning." Of course, it is not possible to observe this innate information (which purportedly pre-dates the conception of the organism; as in Bjorklund, 2015, 2016; Bjorklund & Ellis, 2005). One can see only behavior, which, Lorenz (1965) contended, is not instinctual per se but rather the product of "morphological ontogeny producing structure . . . and . . . of trial-and-error behavior exploiting structure as a teaching apparatus" (p. 79).

In my view, however, as a consequence of his new 1965 conception of instinct, Lorenz replaced one set of problems with another. He gave up the five observational criteria for identification of instinctual behavior patterns and defined instinct as a non-empirical construct, which cannot be known directly. Information about the range of forms can only be inferred from behaviors that, by his own insistence, are not independent of experience, or "learning."

The changes Lorenz introduced into his conception of instinct in 1965 led him away from being an empirical scientist. His statements about the instinct concept became increasingly less linked to clear observational criteria and, as such, took on more of the character of an intuitive construct (Richards, 1987, p. 530). Nevertheless, an interest in the instinct concept spanned Lorenz's entire career, and he continued to be an advocate for the importance of the construct for understanding both animal and human behavior. Indeed, this advocacy was based on Lorenz's belief that human civilization was at risk because of phenomena associated with specific types of changes in instinctual patterns (Lorenz, 1974a).

Lorenz's Application to Humans of His Concept of Instinct

As a committed Darwinist, Lorenz (1965, 1966, 1974a, 1974c) believed that instincts were shaped by evolution, by natural selection. Instincts—whether defined behaviorally or through reference to genome information—afford, then, survival; their function is to allow the organism to "fit" the demands of the natural environment within which the particular set of behaviors, or range of forms, comprising the instinct has been selected. In this vein, Lorenz (1965) contended:

> Some information underlying an individual's behavior has indeed been "preformed" by the species . . . It becomes all too easy to overlook

> the survival function of behavior altogether and, therewith, the selection pressure which caused its mechanisms to evolve. To anyone tolerably versed in biological thought, it is a matter of course that . . . any function of . . . survival value . . . must necessarily be performed by a very special mechanism built into the organic system in the course of its evolution.
>
> (p. 13)

Therefore, according to Lorenz (1965), instincts allow the organism to survive within the natural surroundings within which the instinct has been selected. The essence of instincts is to allow a fit with the demands of the environment within which the instinct evolved.

But what happens if and when individuals are taken from the environments within which their instincts have been naturally selected? What are the implications for survival when an organism finds itself in a setting other than the one within which its instincts evolved to fit? It is with these questions, involving the implications of taking individuals out of their natural selection environment and placing them in, in particular, a tamer and more domesticated and civilized setting, that much of Lorenz's scholarship, beginning in the 1930s and engaging him for much of the remainder of his life, was concerned.

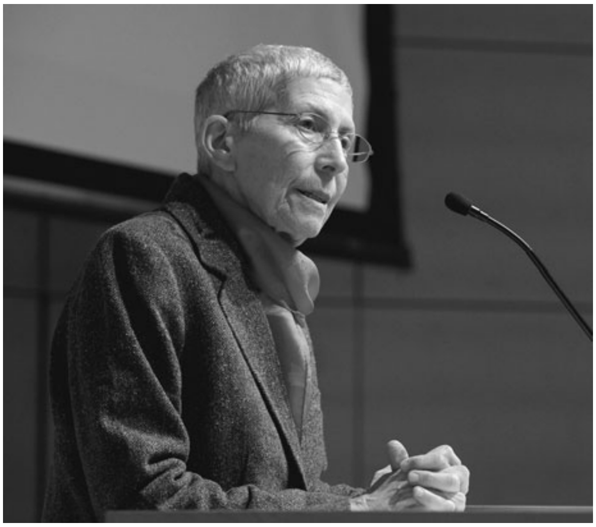
This central theme in the work of Lorenz has been the focus of reviews by philosopher Theodora Kalikow (1978a, 1978b, 1983). Kalikow (1978a, 1983) adopted this focus because of her view that Lorenz interwove political ideology with his scientific focus, which was the discussion of the nature of instincts among domesticated animals or among humans "encountering" civilization. Kalikow (1983) stated:

> Ideology played a triple role in Lorenz's speeches and writings during the years from 1938 to 1943. (1) He saw changes in the instinctive behavior patterns of domesticated animals as symptoms of decline. (2) He assumed a homology between domesticated animals and civilized human beings, that is, he assumed there must be similar causes for effects assumed to be similar, and he further believed that civilization was in a process of "decline and fall." Finally, (3) he connected the preceding concerns to racial policies and other features of the Nazi program.
>
> (p. 39)

Kalikow (1983) also drew a distinction between Lorenz's early work (in regard to the three points noted in the prior quote) and his later work: "An examination of Lorenz's writings from before and after World War II shows that (1) and (2) have remained as features of his work, while (3) has disappeared, at least in its overt manifestations" (p. 39).

In other words, Lorenz (1940b, 1974a; see too Richards, 1987) offered the hypothesis that human biological degeneration has been brought about through domestication. According to this hypothesis, the instinctual behaviors of "civilized," urban human beings, behaviors that evolved to fit more rural settings, have become increasingly more diseased and degenerate. Akin to the domestication-induced degeneracy that, he believed, afflicts animals reared away from their natural, or wild, setting, Lorenz (1940b, 1974a) contended that modern society's protection of humans from natural selection has resulted in the degeneration of human beings both intellectually and morally (Richards, 1987).

As noted in the epigraph beginning this section of this chapter, Lorenz admitted that, through his


Theodora Kalikow

inheritance, he was obsessed with eugenics (Lorenz, 1974b, in Cox, 1974). Thus, if Kalikow's (1983) views are correct, the focus Lorenz (1937a, 1937b, 1965, 1966, 1974a) adopted for his work on instincts is redolent of the Social Darwinist/racial hygienist thinking that was part of the intellectual, social, and political milieu of Germany before the Third Reich (Kalikow, 1978a, 1983; Lifton, 1986; Müller-Hill, 1988; Proctor, 1988).

But are Kalikow's (1983) views of the conflation of science and politics in Lorenz's work correct? If so, was there a connection between Lorenz's scientific and political writings? Did his scientific beliefs shape his political ones? Did political ideology affect the quality of his scientific work?

In Chapter 4, I discussed the ubiquitous connections between scholars' philosophies of science and their theoretical and empirical work. In addition, in Chapter 8 I noted the linkage between assumptions about constancy and change in human development and the empirical identification by a scientist of continuity or discontinuity in development. Accordingly, it would not be unusual for any scholar—Lorenz, or me for that matter—to have one aspect of his or her intellectual life commingle with other aspects (cf. Lewontin, 1992, 2000). Indeed, humans, as living, open, and relational systems, may be marked by such integration (e.g., Fischer & Bidell, 2006; Mascolo & Fischer, 2015; Rogoff, 1998, 2003, 2011).

Moreover, as a part of human life, science is not independent of politics; it is inevitably integrated with other facets of human behavior and development (e.g., see Kamin, 1974). Scientists often lobby political leaders to garner support for and funding of their areas of research. Most scientific organizations have staff whose assignment is to work with governmental bodies to promote or protect the funding of their domain of scholarship. In turn, political pressures on governmental bodies may result in politicians increasing or decreasing support for politically sensitive or controversial areas of research (e.g., early-life intervention, fetal tissue research, human cloning, daycare effects, character education, family leave policies, or sex education). In the case of political issues resulting in support for a given line of scholarship, it is certainly clear that scientific theory and research may serve political purposes. The "fate" of such lines of scholarship (e.g., whether they continue to attract governmental funding) may hinge, then, on the political context.

As such, the questions I am raising here about Lorenz's work are not ones that criticize him per se for manifesting a situation wherein one facet of his beliefs influences another. In addition, I am not faulting him per se because there may have been a connection in his work between science and politics. Rather, the questions I am raising are directed at understanding whether such connections existed and, if so, how they may have affected his theoretical ideas about human development and about the social uses or applications he and others saw—and some may still see—for such ideas. If, as I discuss at the end of this chapter and again in Chapter 12, there is a strong connection between Lorenz's ideas and sociobiological thinking, then it may be legitimate to question whether any of the science–politics connections relevant to Lorenz's work are also pertinent to sociobiology.

To address these questions, I first review the "evidence" Kalikow (1983) and others (e.g., Chorover, 1979; Eisenberg, 1972) marshaled in support of the contention that Lorenz's 1938–1943 writings—his work that was published during the Nazi political era (which occurred between 1933 and 1945)—combined science and politics. I then consider an analysis of Kalikow's (1983) views presented by Richards (1987), the historian who has been most critical of Kalikow's (1983) interpretation of Lorenz's Nazi-era writings. Finally, I note the themes in Lorenz's writings that appear to have been carried beyond World War II. As such, I discuss briefly the connection between the biological-determinist views of Lorenz and those found among sociobiologists.

The Nazi-Era Work of Lorenz

The National Socialist German Workers' Party (abbreviated as the NSDAP or simply as the Nazi Party) controlled Germany and, eventually, much of western and eastern Europe from January 30, 1933, when Adolf Hitler became Chancellor of Germany, to May 7, 1945, when Germany surrendered unconditionally to the Allied forces to end World War II in Europe. Throughout much of this period, the career of Konrad Lorenz flourished.

Many of Lorenz's writings pertinent to his commitment to Nazi racist ideology and policies have been identified and translated by Kalikow (1978a, 1978b, 1983), although other scholars have drawn attention to Lorenz's "Brown past" (i.e., his participation in Nazi party ("Brown Shirt") activities) (e.g., Chorover, 1979; Eisenberg, 1972; Lerner, 1992a; Lewontin, Rose, & Kamin, 1984; Lifton, 1986; Nisbett, 1977; Proctor, 1988). However, it was Kalikow (1983) who, in searching the records of the Berlin Document Center, found that Lorenz had applied for membership in the Nazi party on May 1, 1938, and was accepted (and given membership number 6170554) on June 28, 1938. Lorenz, then, was a scientist with doctoral degrees in medicine and zoology and, as well, was literally a card-carrying member of the Nazi party. It seems reasonable to inquire about the extent to which biological science and National Socialist ideology and policies were combined in Lorenz's work.

As I have noted, throughout his career, Lorenz (1937a, 1937b, 1965, 1966, 1974a, 1975) was concerned with the degeneration of instincts brought about by the domestication of animals with purportedly inferior genes. This theme has had time-honored status in German biological-determinist writings, since at least the period of Ernst Haeckel (1876, 1891, 1905) and the "Monist League" that he started in order to bring scholars from different fields together to use one set of ideas—Darwinian thinking about evolution—in order to understand all areas of biological, social, and cultural life (Gasman, 1971; Proctor, 1988; Stein, 1987).

In 1938 Lorenz first presented his views on how domestication was associated with human degeneration (Kalikow, 1983). He made this presentation at a meeting of the German Psychological Association in a paper titled "Deficiency Phenomena in the Instinctive Behavior of Domestic Animals and Their Social Psychological Meaning" (his presentation was published in 1939). First, Lorenz discussed the connection between instincts in animals and instincts in humans. Second, and sounding the theme first raised by Haeckel (1876, 1891, 1905), Lorenz talked about how the domestication-induced degeneracy of instinctual behavior threatens the survival of the German people—the *Volk*. Third, he discussed how differences between, in his terms, the genetically fit and the genetically unfit (and degenerate) are manifested—one way being that fit people appraise beauty and aesthetic appeal as associated with the fit and not with the unfit. Fourth and again similar to Haeckel, Lorenz (1939) argued that judgments of good and bad, or of moral or immoral, are associated with the hereditarily fit and the hereditarily unfit, respectively (Kalikow, 1983, pp. 58–61).

For instance, with respect to the connection between instincts in animals and humans, Lorenz (1939; translated by Kalikow, 1983) contended:

> What ought to be compared, in these inferences from animals to human beings, are the hereditary changes in the system of innate species-specific behavior patterns, changes that arise in animals in the course of domestication and in human beings in the course of the civilization process. These two processes, seen from the standpoint of the biologist, have much in common.
>
> (pp. 58–61)

In regard to the threat to the survival of the *Volk* caused by this biological degeneracy, Lorenz (1939; translated by Kalikow, 1983) stated:

> The similarity of the biological foundations makes it quite believable that these parallels, which extend to the smallest details of human and animal behavior, are not just superficial analogies, but are founded on underlying causes. Thus, through a closer investigation of the behavior of domestic animals, we may hope to further our understanding of the biological causes of many menacing decay phenomena in the behavior of civilized human beings.
>
> (pp. 58–61)

Finally, in regard to the connection between innate goodness and badness and the presence of hereditarily fixed social behaviors that are either fit or not fit, respectively, Lorenz (1939; translated by Kalikow, 1983) argued:

> Even the observer striving for complete objectivity cannot stop himself from evaluating the decay of social behavior patterns negatively, even in animals. This is even more the case with

> respect to our conspecifics. For humans we mean by "good" and "bad" really nothing other than "complete with respect to innate social behavior patterns" and the opposite of this. If a person in fact detachedly exhibits a thoroughly social behavior, but does this not according to feeling, or instinctively, but calculatingly, and we see through this, we never feel this person to be "good." Our instinctive evaluation thus really relates to the presence of absolutely specific hereditary properties in our conspecifics.
>
> (pp. 58–61)

Lorenz (1939) closed by cautioning that the carriers of "bad" hereditary properties can degenerate the health of the *Volk* "like the cells of a malignant tumor." This phrasing is a metaphor that was employed by a group of Nazi-era scientists working expressly on improving the biological fitness of the *Volk*—a group who were termed racial hygienists (Proctor, 1988). The aforementioned metaphor was directed expressly at Jews, who were seen by the Nazis as the nonhuman "anti-race." Given this threat, Lorenz (1939) made what must have been seen as the necessary call for social action, again quite similar to the appeals of other National Socialist physicians (Lifton, 1986; Müller-Hill, 1988; Proctor, 1988). Lorenz contended that those who possess fit instinctual patterns must capitalize on their aesthetic/valuational reactions to those who possess unfit, degenerating genes (Kalikow, 1983, p. 61). Using these reactions to recognize the unfit, the fit must *eliminate* the unfit in order to ensure the "racial health and power" of the *Volk*. Specifically, in closing his 1939 publication, Lorenz (1939, pp. 146–147; translated by Kalikow, 1978a) argued:

> This high valuation of our species-specific and innate social behavior patterns is of the greatest biological importance. In it as in nothing else lies directly the backbone of all racial health and power. Nothing is so important for the health of a whole *Volk* as the elimination of "invirent types": those which, in the most dangerous, virulent increase, like the cells of a malignant tumor, threaten to penetrate the body of a *Volk*. This justified high valuation, one of our most important hereditary treasures, must however not hinder us from recognizing and admitting its direct relation with Nature. It must above all not hinder us from descending to investigate our fellow creatures, which are easier and simpler to understand, in order to discover facts which strengthen the basis for the care of our holiest racial, *Volkish* and human hereditary values.
>
> (pp. 174–175)

In short, as did Hitler (1925) and Haeckel (1876, 1891, 1905) before him, Lorenz (1939) saw the mission of race purification—protecting the *Volk* from the "malignant tumor" threatening it by the presence of genetic inferiors—to have cosmic, mystical, and, indeed, holy characteristics.

In 1940, Lorenz published a paper titled "Systematics and Evolutionary Theory in Teaching" in the journal *Der Biologe* (*The Biologist*) (Lorenz, 1940b), expanding on the themes of his 1939 publication and emphasizing, as had racial hygienists in Germany (e.g., Ploetz, 1895), that the natural selection process had been eroded by modern civilization and that this erosion was the basis of the degeneration process threatening the survival of the *Volk*. Lorenz (1940a; translated by Kalikow, 1983) cautioned:

> Whether we share the fate of the dinosaurs or whether we raise ourselves to a higher level of development, scarcely imaginable by the current organization of our brains, is exclusively a question of biological survival power and the life-will of our *Volk*. Today especially the great difference depends very much on the question whether or not we can learn to combat the decay phenomena in *Volk* and in humanity which arise from the lack of natural selection. In this very contest for survival or extinction, we Germans are far ahead of all other culture-*Volks*.
>
> (p. 63)

Given the nature of the journal in which the paper was published (noted below), it is not surprising that Lorenz (1940b) sounded this caution about the need to combat the "decay phenomena" endangering the *Volk* and that he complimented his fellow Germans (although he himself was Austrian) for having the wherewithal to be winning this fight (through the racial policies of the National Socialist state); an example of such a "winning strategy" involved Hitler's "final solution" to the Jewish problem, a policy that involved mass extermination in the service of genocide.

As Kalikow (1983) explained, *Der Biologe* was an organ of the Biology Section of the National Socialist Teachers' League, and its editorial board members came from such politically correct organizations as the National Socialist University Teachers' League, the SS, and the Race-Political Department of the Nazi party. Given the striking similarity between the views expressed by Lorenz and those promulgated by Nazi physicians and politicians, and given the nature of the publication in which Lorenz presented his views, it is difficult to determine whether this 1940 article is a scientific statement or a Nazi political statement. If the article was meant as only a scientific statement, then at the very least one can wonder whether another publication outlet would have been more appropriate.

The article in *Der Biologe* (Lorenz, 1940b) is not the only one of Lorenz's Nazi-era publications to have a combined scientific and political message. In another paper published in 1940, "Domestication-Caused Disturbances in Species-Specific Behavior," appearing in the *Zeitschrift für angewandte Psychologie und Charakterkunde* (*Journal of Applied Psychology and Personality*), several reviewers of Lorenz's work (e.g., Chorover, 1979; Evans, 1974; Kalikow, 1983; Nisbett, 1977) and Lorenz himself (Lorenz, 1974b, in Cox, 1974) admitted that the most explicit Nazi-oriented statements are made in regard to his interpretation of domestication-induced degeneracy. Indeed, Lorenz (1940a) included at the end of that paper a section entitled "Practical Applications" (Kalikow, 1983). Throughout the paper, however, Lorenz repeated his themes of the danger of racial degeneration, the erosion of natural selection factors, and the need to applaud the National Socialist state's endeavors to institute their own selection measures and thereby exterminate the cancerous cell—the aesthetically ugly and the ethically evil—from the midst of the *Volk*.

Clearly, then, Lorenz (1940a) is calling for a eugenicist application of his ideas. After World War II, genetic reductionists in the United States rejected the Nazi-era approach to genetic selection. However, it is an important historical sidebar to my recounting the work of Lorenz to note that the idea of eugenics continued to have some appeal to American behavior geneticists (Joseph, 2015). For instance, Jay Joseph (2015) pointed out that two of the founding behavioral geneticists in the United States, Gerald McClearn and John DeFries, wrote in their 1973 textbook, *Introduction to Behavioral Genetics*, that:

> it is clear that the basic problems that aroused the early eugenicists have not gone away; nor have they been ignored . . . The opprobrium attached to eugenic action programs, particularly the deformed version of the Nazis, has prompted a desire for a new label to describe the academic pursuit of these issues . . . It appears that the term "social biology," or some variant thereof, is emerging as an appropriate label to describe this interdisciplinary effort. As indicated below, behavioral genetics constitutes a central core of this new social biology.
>
> (McClearn & DeFries, 1973, p. 305)

Joseph (2015) also points out that subsequent editions of this textbook, which included Robert Plomin as the lead author (e.g., Plomin, DeFries, McClearn, & McGuffin, 2008), did not include positive statements about eugenics. However, other genetic reductionists have continued to forward eugenicist ideas into the second decade of the twenty-first century (e.g., Belsky, 2014).

Returning here, however, to the ideas of Lorenz, he believed that his 1940 paper addressed the question of whether the life-conditions of civilization and of domestic animal behavior contain factors that encourage mutations. Lorenz (1940a; translated by Kalikow, 1978a) contended that:

> This problem receives its particular importance first through the knowledge that among the most dangerous and race-hygienically most damaging decay phenomena in the social behavior of civilized people are those which have their precise equivalents in the "domestication characteristics" of many domestic animals, and which, in all probability, depend on the same causes. On the answering of the question about these

> causes, however, depend the counter-measures to be taken. If there should be mutagenic factors, their recognition and elimination would be *the most important task of those who protect the race*, because the continuing possibility of the novel appearance of people with deficiencies in species-specific social behavior patterns constitutes a danger to *Volk* and race which is more serious than that of a mixture with foreign races. The latter is at least knowable as such and, after a one-time elimination of breeding, is no longer to be feared. If it should turn out, on the other hand, that under the conditions of domestication no increase in mutations takes place, but the mere removal of natural selection causes the increase in the number of existing mutants and the imbalance of the race, then race-care must consider an even more stringent elimination of the ethically less valuable than is done today, because it would, in this case, literally have to replace all selection factors that operate in the natural environment.
>
> (p. 176)

Later in the paper, Lorenz (1940a; translated by Eisenberg, 1972) expanded on this argument, especially in regard to how to deal with the threat posed by the "ethically less valuable":

> The only resistance which mankind of healthy stock can offer against being penetrated by symptoms of degeneracy is based on the existence of certain innate schemata . . . Our species-specific sensitivity to the beauty and ugliness of members of our species is intimately connected with the symptoms of degeneration, caused by domestication, which threaten our race . . . Usually, a man of high value is disgusted with special intensity by slight symptoms of degeneracy in men of the other race . . . In certain instances, however, we find not only a lack of this selectivity . . . but even a reversal to being attracted by symptoms of degeneracy . . . Decadent art provides many examples of such a change of signs . . . The immensely high reproduction rate in the moral imbecile has long been established . . . This phenomenon leads everywhere . . . to the fact that socially inferior human material is enabled . . . to penetrate and finally to annihilate the healthy nation. The selection for toughness, heroism, social utility . . . must be accomplished by some human institution if mankind, in default of selective factors, is not to be ruined by domestication-induced degeneracy. The racial idea as the basis of our state has already accomplished much in this respect. The most effective race-preserving measure is . . . the greatest support of the natural defenses . . . We must—and should—rely on the healthy feelings of our Best and charge them with the selection which will determine the prosperity or the decay of our people.
>
> (p. 124)

In addition, Lorenz (1940a; translated by Chorover, 1979) expanded: "[that is, charge them with] the extermination of elements of the population loaded with dregs. Otherwise, these deleterious mutations will permeate the body of the people like the cells of a cancer" (p. 105).

Continuing the analogy between the presence of cancer cells within a body and the presence of a group of people within a society, Lorenz (1940a; translated by Fraser in Müller-Hill, 1988) maintained:

> There is a certain similarity between the measures which need to be taken when we draw a broad biological analogy between bodies and malignant tumors, on the one hand, and a nation and individuals within it who have become asocial because of their defective constitution, on the other hand . . . Any attempt at reconstruction using elements which have lost their proper nature and characteristics is doomed to failure. Fortunately, the elimination of such elements is easier for the public health physician and less dangerous for the supra-individual organism, than such an operation by a surgeon would be for the individual organism.
>
> (p. 14)

Because Lorenz (1940a) called so clearly for reliance on the selection policies of "the Best" of Nazi Germany and for the extermination of elements of the population permeated with "dregs," and a more severe elimination of the morally inferior, it is difficult to reconcile his claim of 34 years later, "that they meant murder when they said 'selection' was beyond the belief of anyone. I never believed the Nazi ideology" (Lorenz, 1974b, in Cox, 1974).

It is puzzling, to say the least, that someone who called for "elimination" and "extermination" is surprised that those whose selection practices he congratulated exterminated those selected for eliminations. For someone to claim that he never believed in Nazi ideology, when his publications make claims about biological determinism and call for social policies that dovetail precisely with the explicit details of such ideology, would seem to be a remarkable coincidence. The contradictions that might appear to exist between Lorenz's Nazi-era statements, and his postwar, later-life recollections of his wartime thoughts and meanings, are only compounded when we learn about his other publications during the Nazi period.

In a 1943 article titled "The Innate Forms of Possible Experience," published in the *Zeitschrift für Tierpsychologie* (*Journal of Animal Psychology*), Lorenz (1943a; translated by Kalikow, 1983) reiterated his concept of the links between domestication-produced racial degeneration and aesthetic value judgments about what is ugly, and, therefore, about what is threatening and dangerous (and, hence, "bad," in the moral sense) for society:

> If one systematically goes through the—on close observation—astonishing short list of the characteristics which clearly produce the ugly in human beings and animals, one comes to the result that they are all relational characteristics which in human beings indicate domestication- or civilization-caused decay phenomena. If the ugly is to be represented in art, the artist accordingly resorts, not to any old arbitrary distortions of the human ideal Gestalt, but with great regularity to the few typical characteristics of domestication. Classic Greek sculpture represented Silenus as the opposite of the god-and-hero-type, always pinch-headed, with pot-belly and too-short limbs . . . and in just the same way the traditionally ugly Socrates is always pictured as a chondrodystrophic.
>
> (p. 68)

The image Lorenz (1943a) presented of the genetically degenerate and exemplary ugly person is remarkably akin to the depiction of the Jew presented in drawings found in Nazi-publisher Julius Streicher's rabidly anti-Semitic "newspaper," *Der Stürmer*, as well as in various elementary school primers and children's books that Streicher published at the time Lorenz was writing these papers. Figure 11.1 presents two illustrations from Streicher's publications depicting Jews (in contrast to Aryans—the race of the German *Volk*) as conforming closely to the characteristics Lorenz contended exemplified the domestication-induced degenerate ugly (e.g., "pot-belly and too-short limbs"; Kalikow, 1983, p. 68).

Still other Nazi-era papers by Lorenz, consistent with Nazi ideology and social policy, appeared repeatedly between 1938 and 1943. For instance, another 1943 paper, "Psychology and Phylogeny" (Lorenz, 1943b; translated by Kalikow, 1978a), appearing in a volume edited by G. Heberer (*Die Evolution der Organismen* [*The Evolution of Organisms*]), draws connections between domestication-induced degeneration phenomena in animals and humans and concludes that this "scientific" evidence has clear and necessary racial-political implications:

> A domestic goose will mate nonchalantly with any gander, while the mating of the wild form is dependent on a vast quantity of complicated [and innate] "betrothal customs." In human beings, on the other hand, the expansion of [innate] schemata leads to the race-politically highly undesirable increase in the rate of reproduction of the inferior classes . . . No inevitable "logic of time" brings the "senescence" of culture-nations with it, as Spengler believed—rather it is factors in the environment, which are concrete, accessible to experiment, and thus certainly possible to combat. The race-political necessity of their immediate, precise investigation is obvious.
>
> (pp. 177–178)

Thus, to Lorenz, the need to interrelate hereditarian ideas about race and political necessities was obvious.

Given such a perspective, the need to take political action was clear to Nazi Germany (even if it meant the extermination of a people). Perhaps

Figure 11.1 Taken from a racist primer published by Julius Streicher in 1936, *Trust No Fox in the Green Meadow and No Jew on His Oath*, these illustrations depict Jewish adults and children being expelled from a school and from a town as Aryan children look on and/or jeer. As is typical in Streicher's publications, Jews are drawn as potbellied and as having limbs that are too short, features that Lorenz claimed exemplified the domestication-induced degenerate ugly. Courtesy of United States Holocaust Memorial Museum.

this is why Rudolf Hess, the Deputy Führer of Nazi Germany, contended at a mass meeting that "National Socialism is nothing but applied biology" (cited in Lifton, 1986, p. 31).

Conclusions

Given, then, this sample of Lorenz's Nazi-era writings, it is possible to conclude that at least from the time Lorenz joined the Nazi party in 1938 through 1943, when he entered military service for the Third Reich, Lorenz's papers (e.g., 1939, 1940a, 1940b, 1943a, 1943b) contained consistent themes that increasingly more clearly and stridently appeared to combine his science with his racist-political views, views that were entirely consonant with other statements by National Socialist scientists that merged politics and "scientific" Nazi racial hygiene ideology (Lifton, 1986; Müller-Hill, 1988; Proctor, 1988).

Common among Lorenz's Nazi-era papers was the theme of domestication-induced degeneracy; of aesthetically repulsive and immoral genetic misfits multiplying at dangerous rates in society because of the erosion of natural selection; of the need, therefore, to rely on "the Best" of the *Volk* to institute selection measures to fight the threat to the race posed by these hereditarily unfit "cancers"; and of the need for these state-designed selection measures to involve elimination—extermination—of these degenerate "dregs."

The consistent repetition of these themes in several papers spanning a half-decade cannot be interpreted as simply a temporary or minor aberration of a scientist toying with the implications of his work for political ideology and social policy. It seems, rather, to be the work of a person energetically explaining the important congruence between his science and his politics, a person who wants to demonstrate to his audience how his theory and research coalesce to give credibility to National Socialist biological-determinist ideology and legitimacy to Nazi racial policies.

This conclusion is clearly predicated on the scholarship of Kalikow (1978a, 1978b, 1983), who provided the seminal work documenting the linkage between Lorenz's scientific views and National Socialist ideology. However, Kalikow's (1983) view of this linkage has been questioned by Richards (1987), in his authoritative and acclaimed book, *Darwin and the Emergence of Evolutionary Theories of Mind and Behavior* (1987). It is, therefore, important to review and evaluate Richards's (1987) discussion of Kalikow's (1978a, 1978b, 1983) interpretation of

Lorenz's Nazi-era work. This discussion also helps to clarify the nature of Lorenz's work during World War II and to aid developmental scientists' understanding of the ideas Lorenz presented after the end of the war.

The Science and Politics of Lorenz's Work: Evaluating the Evidence

According to Richards (1987), Kalikow (1978a, 1978b, 1983) maintained that Lorenz's (e.g., 1939, 1940a, 1940b, 1943a, 1943b) ideas about domestication-induced human degeneracy are tied both to the thinking of Haeckel (1876, 1891, 1905) and the politics of the Nazis. Influenced by the work of Gasman (1971), who also argued that the Nazi "biological mission" was promoted by the Social Darwinism of Haeckel, Kalikow (1983) found in Haeckel (and other Monist League members), in National Socialist ideology, and in the writings of Lorenz four ideas:

1. A biological view of the world, a world in which the laws of nature and the laws of society are the same.
2. The belief that human evolution has been moving with constancy until the present era, in which high reproduction rates and "humanistic" attitudes toward the less fit put the human race at risk for survival.
3. The belief that there is a one-to-one relationship between outer human appearance and internal moral value (i.e., "what is beautiful is good"), and that the Aryan race, which exemplifies the pinnacle of this correspondence, has its ancestry among the ancient Greeks.
4. The idea that evolution is the creative force in the world, a notion that replaces the belief that God is the creator and shaper.

Richards (1987) presented several reasons why the links Kalikow (1978a, 1978b, 1983) saw among the Haeckelian/Monist League views, National Socialist ideology, and Lorenz's ideas may be more apparent than real. One reason is "nondistinctiveness"—that the first two sets of ideas are present in the general literature on evolution whereas the last two sets were common at the turn of the nineteenth century. Thus, Richards (1987) dismissed Kalikow's (1983) arguments regarding convergence among the three positions in regard to the four sets of ideas by contending, "If such vague similarities suffice here, we should all be hustled to the gallows" (Richards, 1987, p. 533).

It is possible to question, however, whether the similarities Kalikow (1983) found are as vague as Richards (1987) portrayed. Furthermore, it is appropriate to ask whether there is support for the convergence among the three positions—involving Haeckel and the Monist League, Nazi ideology, and Lorenz's work—in addition to the four sets of ideas noted by Kalikow. An examination of the other reasons that Richards rejected Kalikow's ideas allows these issues to be addressed.

Richards's (1987) second reason for disagreeing with Kalikow's (1983) linkage of the three positions is that the intellectual influence of Haeckelian/Monist League views on National Socialist ideology was not completely clear. That lack of clarity may be the case. However, there is little reason to expect that the hodgepodge of concepts, and the opportunistic twisting of the motley set of ideas that constitutes the corpus of Nazi ideology, should show a neat and logical pattern of influence. For instance, Ralph Manheim (in Hitler, 1925 [1927/1943]), translator of the most frequently cited English version of Hitler's (1925) magnum opus, *Mein Kampf* (*My Struggle*), indicated in his notes to that edition that Hitler never attempted to systematize his knowledge and, instead, relied largely on disjointed facts. Manheim (in Hitler, 1925 [1927/1943]) indicated that:

> Even where he is discussing theoretical matters like "the state," "race," etc., he seldom pursued any logic inherent in the subject matter. He makes the most extraordinary allegations without so much as an attempt to prove them. Often there is no visible connection between one paragraph and the next.
>
> (pp. xi–xii)

In short, if Nazi ideology was not a logical and coherent system, it is not appropriate to make the presence of a coherent, clear, or linearly direct pattern of influence a criterion for linkage between Haeckelian/Monist League views and Nazi ideology.

Accordingly, in order to understand the "intellectual influences" on Nazi ideology, Hitler's (1925) presentation in *Mein Kampf* and the succeeding tracts by Nazi ideologues may best be scrutinized for the sources that (not necessarily logically or correctly) are reflected in them. For instance, whereas both Richards (1987) and Kalikow (1983) noted that evolutionary theory was not fully accepted in the Third Reich, it is known that Hitler was influenced by German Social Darwinist/racial hygiene thinking. For example, the eminent molecular geneticist Benno Müller-Hill (1988) noted that, while Hitler was imprisoned in Landsberg Prison in 1923, he read the textbook by Bauer, Fischer, and Lenz (1927), *Grundriss der menschlichen Erblicbkeitslehre und Rassenhygiene* (*The Principles of Human Heredity and Racial Hygiene*). Subsequently, Hitler (1925) incorporated racial ideas into *Mein Kampf*, which he was preparing during his imprisonment (Müller-Hill, 1988).

Thus, whereas all features of evolutionary thinking are not necessarily present in Hitler's writings, scholars can find ideas such as: (1) selection by a "wise" but ruthless nature (i.e., by a nature making hard but appropriate choices); (2) a hardened race of high accomplishment will eventually emerge under such conditions; and (3) societal interference with this process will permit the weak and the sick ("lives unworthy of life"; Binding & Hoche, 1920) to survive and the quality of the race to be thereby diminished (Proctor, 1988). This later idea was present also in German Social Darwinist/racial hygiene writings. Thus, Hitler (1925 [1927/1943]) wrote:

> Nature herself in times of great poverty or bad climatic conditions, as well as poor harvest, intervenes to restrict the increase of population of certain countries or races; this, to be sure, by a method as wise as it is ruthless. She diminishes, not the power of procreation as such, but the conservation of the procreated, by exposing them to hard trials and deprivations with the result that all those who are less strong and less healthy are forced back into the womb of the eternal unknown. Those whom she permits to survive the inclemency of existence are a thousandfold tested, hardened, and well adapted to procreate in turn, in order that that process of thoroughgoing selection may begin again from the beginning. By thus brutally proceeding against the individual and immediately calling him back to herself as soon as he shows himself unequal to the storm of life, she keeps the race and species strong, in fact, raises them to the highest accomplishments.
>
> At the same time the diminution of number strengthens the individual and thus in the last analysis fortifies the species.
>
> It is different, however, when man undertakes the limitation of his number. He is not carved of the same wood, he is "humane." He knows better than the cruel queen of wisdom. He limits not the conservation of the individual, but procreation itself. This seems to him, who always sees himself and never the race, more human and more justified than the opposite way. Unfortunately, however, the consequences are the reverse:
>
> While Nature, by making procreation free, yet submitting survival to a hard trial, chooses from an excess number of individuals the best as worthy of living, thus preserving them alone and in them conserving their species, man limits procreation, but is hysterically concerned that once a being is born it should be preserved at any price. This correction of the divine will seem to him as wise as it is humane, and he takes delight in having once again gotten the best of Nature and even having proved her inadequacy. The number, to be sure, has really been limited, but at the same time the value of the individual has diminished; this however, is something the dear little ape of the Almighty does not want to see or hear about.
>
> For as soon as procreation as such is limited and the number of births diminished, the natural struggle for existence which leaves only the strongest and healthiest alive is obviously replaced by the obvious desire to "save" even the weakest and most sickly at any price, and this plants the seed of a future generation which must inevitably grow more and more deplorable the longer this mockery of Nature and her will continues.
>
> (pp. 131–132)

The link between the German Social Darwinists/racial hygienists and Hitler's (1925) ideology is underscored by the consistency between the recommendations Binding and Hoche made regarding the treatment of the weak, lame, and ill in their 1920 book *Die Freigabe der Vernichtung lebensunwerten Lebens* (*The Sanctioning of the Destruction of Lives Unworthy to Be Lived*) and Hitler's (1925) views in *Mein Kampf*. Hitler (1925 [1927/1943]) said:

> It is a half-measure to let incurably sick people steadily contaminate the remaining healthy ones. This is in keeping with the humanitarianism which, to avoid hurting one individual, lets a hundred others perish. The demand that defective people be prevented from propagating equally defective offspring is a demand of the clearest reason and if systematically executed represents the most humane act of mankind. It will spare millions of unfortunates undeserved sufferings, and consequently will lead to a rising improvement of health as a whole . . . The right of personal freedom recedes before the duty to preserve the race.
>
> (p. 255)

Hitler turned these ideas into policy (e.g., involving the "forced euthanasia" of German children) when he assumed power (Lifton, 1986). But to Hitler (1925)—and to at least some proponents of the Haeckelian/Monist League views, as well as to Lorenz (1940b, 1966) during both the Nazi era and more than two decades after it—forced euthanasia programs were not the best way to "prune the weak" and to move the race in the direction a wise and ruthless Nature would select. Rather, it was through warlike behavior—aggression, struggle, and killing of other humans—that such selection was thought to best occur. Thus, according to Hitler (1925 [1927/1943]):

> There will be but two possibilities[,] either the world will be governed according to the ideas of our modern democracy, and then the weight of any decision will result in favor of the numerically stronger races, or the world will be dominated in accordance with the laws of the natural order of force, and then it is the peoples of brutal will who will conquer, and consequently once again not the nation of self-restriction.
>
> No one can doubt that this world will some day be exposed to the severest struggles for the existence of mankind. In the end, only the urge for self-preservation can conquer. Beneath its so-called humanity, the expression of a mixture of stupidity, cowardice, and know-it-all conceit, will melt like snow in the March sun. Mankind has grown great in eternal struggle, and only in eternal peace does it perish.
>
> (p. 135)

Similarly, Heinrich Ziegler (1893), a founding member of the Monist League, argued:

> According to Darwin's doctrine war has been of the greatest importance for the general progress of the human race, since the physically weaker, the less intelligent, and the morally degenerate must make way for the stronger and better developed people . . . If one accepts the insights of modern science, he must see war between different races or people as a form of the struggle for existence in the human race.
>
> (pp. 168–169)

Lorenz (1966), in turn, noted:

> It is quite typical of man that his most noble and admirable qualities are brought to the fore in situations involving the killing of other men, just as noble as they are . . . Aggression, far from being the diabolical, destructive principle that classical psychoanalysis makes it out to be, is really an essential part of the life-preserving organization of instincts.
>
> (pp. 251, 248)

Hitler's (1925) idea of racial greatness through eternal struggle and Ziegler's (1893) notion of racial war as a feature of the human race's struggle for existence converge with Lorenz's 1966 view that aggression is life-preserving and that, by acting on their aggressive instincts, humans have often attained nobility and other admirable characteristics.

Such linkages among the ideas of Hitler, the Monist League literature, and Lorenz are not consistent with Richards's (1987, p. 533) view that only "vague similarities" exist across the three positions.

Moreover, although Richards (1987) pointed out that the Monist League had a pacifist, socially liberal orientation, such a general stance does not gainsay either Ziegler's (1893) conception of the race-preserving function of war, or the possibility that individual scientists may be personally committed to a pacifist political ideology and yet committed as scientists to a belief about the inevitable, or even instinctual, basis of human aggression. Indeed, this view is just the stance Lorenz (1966) took in his book *On Aggression*, in which he argued that, to avoid the release of instinctual militant enthusiasm, society must find means to discharge aggression in innocuous ways.

In short, the linkages Kalikow (1983) drew appear to be real, perhaps even beyond the extent she posited. For instance, although Kalikow (1983) pointed out that the Nazis did not share with either Haeckel (1876, 1891, 1905) or Lorenz (1965, 1966) the commitment to evolution per se as the creative force in the world, I have noted that in Hitler's (1925) *Mein Kampf* there is an emphasis on the selective and shaping force of nature and on several ideas associated with German Social Darwinist/racial hygiene thinking, and quite notably the notion of domestication-induced degeneracy. Indeed, Richards (1987) indicated that some National Socialist ideologues did, in fact, eulogize Haeckel, crediting him with providing scientific support for ideas central to the Nazis' biologized view of the world.

Nevertheless, although Richards (1987) himself provided some evidence for the links between Nazi ideology and Haeckelian/Monist League views, he offered two additional reasons why Kalikow (1983) is mistaken in making this dyad a triad by adding the views of Lorenz. Richards (1987) pointed out that Lorenz never cited Haeckel's work as supportive of his own, and that Lorenz held that the key facet of Haeckel's theory of heredity—the idea of the inheritance of acquired characteristics—was scientifically unsound. Neither of these objections to Kalikow's (1983) argument seems strong, however.

Kalikow's (1983) point appears to be more that Lorenz's ideas were consistent with the views found within the general orientation of Haeckel and the Monist League, and not that Lorenz adopted either all of these ideas, or any of the ideas of a particular member of the league—including (as it seems) Haeckel. Indeed, the above presentation of converging quotes from Hitler (1925), Ziegler (1893), and Lorenz (1966) suggests that there is some correspondence between the views of Lorenz and at least one prominent member of the Monist League. In addition, both Richards (1987) and Kalikow (1983) pointed out that Lorenz's (1937a, 1937b) original formulation of "instinct" followed the conceptual lead of Ziegler (1893). Furthermore, Kalikow (1983) and, earlier, Nisbett (1977) noted that Lorenz's early interest in evolutionary biology was prompted by his reading a book by Wilhelm Boelsche (1906), cofounder of the Monist League, titled *Die Schöpfungstage* (*The Days of Creation*).

Perhaps because of the nature of these last two points made in argument against Kalikow's (1983) position, Richards (1987) concluded his analysis of her position by interpreting the historical record of Lorenz's Nazi party affiliation and of his publication record (from 1938 to 1943) during the era of the Third Reich as "a gossamer thread by which to tie Lorenzian biology to the Nazis" (Richards, 1987, p. 535). However, Richards (1987) did not deny that Lorenz wrote papers consistent with National Socialist ideology, but only claimed that such papers represent Lorenz's "few occasions of public Nazi association" (Richards, 1987, p. 535). One must, therefore, decide how often a person must make a public commitment, in speeches and/or in writing, to a given ideology before that person can be justifiably linked with it. Perhaps what is operating in Richards's stance on this issue is a commitment to the Frankonian proverb "Amol schad' kan Malda nix," "Once does not do a maiden any damage."[1]

In any case, Richards (1987) did conclude that "Lorenz [in 1940] undoubtedly descended to accommodate some of his biological views to the ideology of his time and place" and that "at this point in Lorenz's career, certain well entrenched evolutionary ideas happened to intersect with despicable Nazi dogma" (pp. 535–536). Thus, Richards appeared to come full circle to admit Kalikow's (1983) point about the convergence of Lorenzian and Nazi "biology," and he left scholars to wonder only about the pervasiveness of the association and Lorenz's enthusiasm for it.

In regard to pervasiveness, scholars have the historical record provided by Kalikow (1978a, 1978b, 1983), as well as by some other authors (e.g., Müller-Hill, 1988). Regarding enthusiasm, it may be (as Richards, 1987, speculated) that, had the Weimar Republic survived, the main features of Lorenz's work would have remained the same. Indeed, I have noted that the racial ideas found in Nazi ideology had a long history antedating the Third Reich, and that history might have involved Lorenz in some other manner had the events of 1933–1945 not taken place. Nevertheless, it is difficult to gainsay Lorenz's enthusiasm for Nazi ideology when, in his 1940 paper in *Der Biologe*, Lorenz (1940b) called it "one of the greatest joys of [his] life" to have converted a student to "our concept of the world (Weltanschauung)"—that is, to National Socialism (translated by Fraser in Müller-Hill, 1988).

In my view, then, there seems to be both appearance and reality to Kalikow's (1978a, 1978b, 1983) views of the linkage between Lorenz's writings during the Nazi era and the biologized world view of National Socialist politics. Moreover, Kalikow's interpretation of the connection between themes in the writings of Lorenz and in Nazi ideology is underscored when the continuity between the key theoretical ideas found in Lorenz's Nazi-era writings and his post-Nazi-era work is recognized. It is, therefore, useful to focus on this continuity, and also to explain its important role in legitimizing more contemporary biological-determinist claims such as those found in sociobiology (e.g., Freedman, 1979; Rushton, 2000).

Lorenz's Work after World War II

Throughout his scholarly career, Konrad Lorenz maintained a central interest in the role of evolution and of heredity in animal and human behavior. He focused on the importance of instincts in understanding behaviors such as social attachment, aggression, and moral or ethical functioning. Morality may involve not only behaving in particular ways, but also knowing right from wrong and good from bad. Given this connection between morality and knowledge, Lorenz's career-long interest in the evolutionary basis of humans' knowledge or of their cognitive system is understandable (Lorenz, 1941, 1974b, 1974c).

All these themes in Lorenz's scholarly work are indisputably appropriate arenas for academic intellectual endeavor. His continuity of interest in these topics could be evidence of an admirable scholarly commitment to long-term programmatic research, research that would be regarded as of scholarly interest per se if developmental scientists were to judge it in isolation from the theoretical ideas from which it sprang. But, if his work involved a merger of scientific and racist political ideas, his continuity of interest might reveal as much about enduring political agendas as scientific ones. If little had changed in Lorenz's core scientific political message, other than the deletion after World War II of Nazi terminology to present the message, then scholars would be justifiably skeptical (at the least) about Lorenz's (1974b, in Cox, 1974) claim that his involvement with the Nazis and their ideology was merely a foolish, "naive error": "Like a fool I thought I could improve them [the Nazis and their ideology], lead them to something better. It was a naive error" (p. 20). The issue that reviewers of the aims of Lorenz's work need to address, then, is whether: (a) the message in the writing of the Nazi-era Lorenz was a combined scientific–racist one, whereas the message after the Nazi era was solely scientific; or (b) the messages remained essentially the same, with only the Nazi-era terminology omitted.

If a case can be made for the second alternative, the implications for present-day science and social policy would be considerable, in light of the scientific eminence Lorenz enjoyed and the credibility and respect thereby accorded his work. That is, politicians and policy-makers could get the impression that there is broad-based scientific acceptance of biological-determinist thinking that, at its core, has pejorative, racist-political aims. The resulting danger would be that these policy-makers might promote social policies that are consistent with those particular assertions of biological determinism. For example, interpretations that the bases of criminality and immorality, and/or the inability to profit from progressive intervention programs (e.g., as argued in Belsky, 2014) are genetically based, might come to the fore. It might, therefore, seem that devoting resources to prevention programs or to programs that sought to promote positive development among people who possessed such genes would not be economically prudent or scientifically realistic.

The most straightforward way to decide about the continuity or discontinuity in Lorenz's core message is to refer to his own post-Nazi-era statements. Has the tune really changed, or has the song only been given a new name? Continuity alone in the topics Lorenz studied is not enough to make a decision; an assessment must be made of whether his stance on the key theoretical theme in his work, if one exists, has been altered appreciably.

There *is* a core theme uniting Lorenz's Nazi-era and post-Nazi-era work: the threat posed by domestication-induced degeneration of human instincts for the survival and further evolution of human moral or ethical functioning, and thus for the future survival or progress of civilization. I believe that Lorenz's post-World War II interpretation of this theme continued to be identical to that in his Nazi-era papers. Lorenz may have claimed that he never believed Nazi ideology, and that he saw his use of it as a "naive error." However, he never apologized or claimed regret for the interpretations he made—which were consistent not only with Nazi ideology but also, as I noted, with the ideas of pre-Nazi- and Nazi-era German Social Darwinists, eugenicists, and racial hygienists, such as Haeckel, Ploetz, Schallmayer, Binding, Hoche, and Lenz (see Proctor, 1988). Indeed, the only specific facet of his message for which I can find an apology is his choice of terminology; he did not apologize for the underlying ideas the particular terms conveyed. Lorenz (1974a; in Cox, 1974) said: "In retrospect, I deeply regret having employed the terminology of the time . . . which was subsequently used as a tool for the setting of horrible objectives" (p. 20). However, perhaps revealing of the actual continuity in the core, underlying theme of his work, Lorenz admitted in 1974 that, as indicated by the epigraph at the beginning of this section, he was intrigued by eugenics to the point of obsession.

Given such obsession, a reasonable expectation might be that the only changes in Lorenz's views about the threats posed to civilization by domestication-induced genetic degeneracy are in the way the views are phrased. Although some change in terminology can be identified, a continuing ideological and terminological emphasis can be found as well in regard to: (1) The need to change the distortions of natural selection that modern civilization's domestication practices have wrought; (2) the genetic basis of morality, of the human sense of good and bad; (3) the decay brought about in this instinctual capacity by domestication phenomena; (4) the fact that some people have genes for good morality and/or ethics and that others have ethically bad genes; and (5) the inevitable need for elimination (if not extermination) procedures to protect society against further degeneration.

For instance, Lorenz (1954; translated by Kalikow, 1983) equated domestication phenomena with deleterious mutations and pathologies and indicated that civilization's interference with (or removal of) natural selection processes was responsible for the appearance of such phenomena:

> One might possibly be inclined to think that environmental conditions . . . have favored homologous mutations. However, this would definitely seem to be a false assumption; instead the blame for the appearance of these characters seems to be exclusively due to the removal of natural selection . . . Domestication-induced alterations of instinctive behavior are, by nature, processes bordering closely on pathological events.
>
> (p. 70)

Moreover, as in the Nazi era, the postwar Lorenz (1954) indicated that this domestication-induced genetic degeneracy occurs in humans as a consequence of modern civilization's interference with naturally selected instinctual behavior patterns. Lorenz (1950; translated by Kalikow, 1978b) contended:

> With every organism that is plucked out of its natural environment and placed in novel surroundings, behavior patterns occur which are neutral or even detrimental for the survival of the species . . . Modern man represents such an animal, torn from his natural environmental niche . . . The flowering of human culture has so extensively changed the entire ecology and sociology of our species that a whole range of previously adaptive endogenous behavior patterns have become not only non-functional but extremely disruptive.
>
> (p. 184)

To Lorenz (1950, 1954), then, instinctive behavior patterns arose as naturally selected adaptations to humans' premodern context. These instincts are fixed patterns of action; they are not flexible (or plastic) in and of themselves, and they are not available for modification either in or through the action of an altered environment. Thus, when humans find themselves in the radically new setting of modern civilization, they are in twofold peril: (1) Their previously adaptive instincts may no longer be useful in the new setting, and (2) the removal of natural selection from the new setting will allow degenerative instincts (what Lorenz terms deleterious mutations and pathologies) to survive and be reproduced.

The Example of Human Aggression

Lorenz's views regarding aggression provide an instructive example of how an instinctual pattern that purportedly evolved to facilitate human survival may undermine it in the context of modern civilization. In his 1966 book, *On Aggression*, Lorenz described humans' aggression as involving instinctual "militant enthusiasm," an inherited vestige of their past and an instinctual response that allowed the individual to respond with confederates in his or her group to threats from organisms outside the community. Indeed, Lorenz (1966) saw such an instinctual pattern as one that, with no thought involved, allowed communities of even fully evolved humans to survive:

> To the humble seeker of biological truth there cannot be the slightest doubt that human militant enthusiasm evolved out of a communal defense response of our prehuman ancestors. The unthinking single-mindedness of the response must have been of high survival value even in a tribe of fully evolved human beings. It was necessary for the individual male to forget all his other allegiances in order to be able to dedicate himself, body and soul, to the cause of the communal battle.
>
> (p. 270)

Lorenz (1966) contended that, with changes in cultural development, the "object" that is defended by the militant enthusiasm instinct may change as well. For example, in early human evolution, the immediate group may have been the object toward which a threat would have elicited militant enthusiasm, whereas among contemporary humans the nation or an abstract idea (e.g., "democracy") may elicit the instinct (Lorenz, 1966). Whatever the object, Lorenz believed two points were certain. First, the object that is salient in a culture becomes so "by a process of true Pavlovian conditioning plus a certain amount of irreversible imprinting," and, second, culture owes a great debt to militant enthusiasm, because "without the concentrated dedication of militant enthusiasm neither art, nor science, nor indeed any of the great endeavors of humanity would ever have come into being" (Lorenz, 1966, pp. 270–271).

Although all these positive outcomes of civilization derive from the instinctual aggression of human beings, civilized humans are not, in Lorenz's (1966) view, entirely in control of whether these outcomes will materialize. Because militant enthusiasm is an instinct that attaches to an object through irreversible imprinting and reflex-like learning or conditioning in early life, negative outcomes of the instinct's release and attachment to a cultural object (outcomes such as war) may occur. Thus, in speaking of whether militant enthusiasm will, in fact, lead to positive social outcomes, Lorenz (1966) contended:

> Whether enthusiasm is made to serve these endeavors, or whether man's most powerfully motivating instinct makes him go to war in some abjectly silly cause, depends almost entirely on the conditioning and/or imprinting he has undergone during certain susceptible periods of his life. There is reasonable hope that our moral responsibility may gain control over the primeval drive, but our only hope of its ever doing so rests on the humble recognition of the fact that militant enthusiasm is an instinctive response with a phylogenetically determined releasing mechanism and that the only point at which intelligent and responsible supervision can get control is in the conditioning of the response to an object which proves to be a genuine value under the scrutiny of the categorical question.
>
> (p. 271)

Lorenz (1966), then, offered hope that, if civilization recognizes the instinctive nature of aggression, future generations can be attached to cultural objects subserving the most prized and positive achievements, and also the moral responsibility, of human beings. Indeed, in *On Aggression*, he described what he believes are "simple and effective" ways of "discharging aggression" in an "innocuous manner" through attempting to "redirect it at a substitute object"; he suggested that sports may be particularly useful in such attempts to channel militant enthusiasm in non-destructive ways (Lorenz, 1966).

What if instinctual aggression is not redirected by civilization? What if militant enthusiasm is released and attached to an object that is associated with war? Given the instinctual, reflexive, and irreversible character that Lorenz attributed to human aggression, there is little a person or group can do if early experience leads militant enthusiasm to be associated with negative—dangerous and destructive—outcomes.

It is possible to view this "double-edged sword" character of human aggression in a historical context. On the one hand, the instinctual and reflexive character of militant enthusiasm can be controlled in the future, if society presents appropriate imprinting, conditioning, and redirection; thus, there is hope that aggressive instincts can subserve moral and positive aims. On the other hand, Lorenz's (1966) formulation excused the past: If society did not recognize the evolutionarily determined, instinctual nature of aggression, and if, therefore, a cohort of people were exposed in their early youth to an inappropriate object, they are not morally culpable for having had their instinct released by this object. Knowing now that human aggression is instinctual may make the leaders of society morally responsible for building programs for the future, ones that will involve the non-destructive release of instinctual aggression (e.g., through sports programs). However, current groups of adults cannot be blamed if leaders of the society they experienced as children did not act in this responsible manner. In short, it is possible to interpret Lorenz's formulation of instinctual militant enthusiasm as excusing the past—perhaps, more specifically, his past—while providing hope for the future.

This interpretation is bolstered when one reviews Lorenz's (1966) ideas about "the stimulus situation which releases" militant enthusiasm. Lorenz contended that there were four stimulus conditions that led to the appearance of militant enthusiasm; and when militant enthusiasm appeared in this way, Lorenz believed it occurred with a degree of certainty equivalent to an inborn reflex such as an eye blink (van der Dennen, 1987). Lorenz (1966) argued:

> Militant enthusiasm can be elicited with the predictability of a reflex when the following environmental situations arise. First of all, a social unit with which the subject identifies himself must appear to be threatened by some danger from outside . . . A second key stimulus which contributes enormously to the releasing of intense militant enthusiasm is the presence of a hated enemy from whom the threat to the above "values" emanates. This enemy, too, can be of a concrete or of an abstract nature. It can be "the" Jews, Huns, Boches, tyrants, etc., or abstract concepts like world capitalism, Bolshevism, fascism, and any other kind of ism; it can be heresy, dogmatism, scientific fallacy, or what not . . . A third factor contributing to the environmental situation eliciting the response is an inspiring leader figure . . . A fourth, and perhaps the most important, prerequisite for the full eliciting of militant enthusiasm is the presence of many other individuals, all agitated by the same emotion.
>
> (pp. 272–273)

That Lorenz's (1966) specification of the four eliciting conditions of instinctual militant enthusiasm parallels the social conditions he and other members of his generation experienced during the Nazi era is striking. The four stimulus conditions correspond to, respectively, (1) the German *Volk*, threatened by the danger of biological annihilation by the (2) hateful (diseased, criminal, and biologically degenerate) Jew. The *Volk* will be protected by (3) the inspiring leader, the Führer, Hitler, who will (4) inflame the emotions of all members of the superior, Aryan race and thus elicit actions—militantly enthusiastic actions—aimed at totally destroying the arch, biological enemy of the *Volk*, the Jew.

It is a remarkable coincidence that Lorenz (1966) would be able to report, more than 20 years after the end of World War II, that there was scientific "evidence" for the existence of a reflex in humans that, in effect, freed the German people from any guilt in following Hitler. Indeed, it is bordering on the incredible that Lorenz "discovered" an instinctual reflex whose path of elicitation paralleled exactly the social events involved in Hitler's "war against the Jews" (Dawidowicz, 1975). Humans certainly cannot be guilty if they possess a knee-jerk reflex, since they were "designed" by evolution to possess such an automatic behavior in response to stimulation.

In the same sense, it would be consistent with Lorenz's (1966) argument to assert that the people of Nazi Germany could not help but follow Hitler once their "militant enthusiasm" reflex was imprinted and conditioned in the manner that occurred during the Third Reich. In other words, who could fairly blame the German people for the militant enthusiasm with which they murdered the Jews—and other targets of Nazi murder such as Gypsies, communists, homosexuals, the disabled, Catholic clergy, and political opponents (Proctor, 1988)—if they were acting in the unthinking, irreversible, and reflexive manner that, Lorenz said, was the case with instinctual military enthusiasm?

Is There a Militant Enthusiasm Instinct?

But what if the notion of instinct, as a hereditarily predetermined, genetically fixed, and immutable set of behaviors, is a scientific fiction? What if the very behaviors that Lorenz described as genetically predetermined to emerge are neither inevitable nor immutable? What if there is no instinctual reflex such as militant enthusiasm—and thus no evolutionarily preprogrammed apologia for Nazi genocide? What if, even in the fish or the bird, much less the human being, nature↔nurture relations can alter both the nerve cells and the behaviors purportedly associated with instincts? In short, what if the supposedly predetermined and fixed genes–nerve cells–behavior connection is neither predetermined nor fixed but instead a readily modifiable, "plastic" linkage? If such plasticity existed, then social policies and programs designed to redirect innate militarism would be time and money misspent. In addition, if such plasticity existed it would also be the case that policies and programs based on the existence of innate militant enthusiasm would derive from the assumption of an evil (or at least undesirable) basic nature for human beings; if this assumption is wrong, then the programs that follow from it have no justification.

What can be wrong with promoting social policies and programs to diminish aggression and militarism? Even if those behaviors are not really instinctual, humans do engage in them all too often. Would the time and money spent on such programs then be wasted or unjustified? There are at least three reasons why these resources would be squandered. First, by building social programs to counter the occurrence of a scientific fiction, one is legitimating the use of what is, in effect, a lie in order to shape social policy.

Second, the efforts directed at deriving policies based on a scientific "lie" divert limited resources from scientifically supportable policies, which may actually be less pessimistic than the predetermined, instinctual views of Lorenz. The social policy implications of this instance of Lorenz's views about "instinct" were perhaps best presented in a review of *On Aggression* written by Schneirla (1966):

> It is as heavy a responsibility to inform man about aggressive tendencies assumed to be present on an inborn basis as it is to inform him about "original sin," which Lorenz admits in effect. A corollary risk is advising societies to base their programs of social training on attempts to inhibit hypothetical innate aggressions, instead of continuing positive measures for constructive behavior.
>
> (p. 16)

Thus, if nature and nurture are both systemically integrated in shaping human behavior, programs to develop positive and/or valued social behaviors may be designed proactively. There would be no need to have to expect only the worst and to have as the only option the design of "containment or rechanneling" programs to constrain the undesirable but inevitable behaviors.

Third, when one legitimates a scientific lie for use in shaping social policy, one is creating a potential for the lie to be used again in other policy areas.

If humans are instinctually militaristic, might they also be controlled by other instincts? Can people not find instincts or, in other words, innate or inborn behavioral differences to account for differences between African Americans and European Americans (e.g., in intelligence), between men and women (e.g., in their sexuality and family orientations), and between the socially privileged and the socially powerless (e.g., in their resources and life options)? People certainly can, and, as colleagues of Schneirla pointed out, the biological-determinist thinking exemplified by Lorenz has been used to legitimate not only militarism but also racism, sexism, and Social Darwinism (Tobach, Gianutsos, Topoff, & Gross, 1974).

The view of human nature exemplified by Lorenz leads to a pessimistic and, indeed, bleak view of the social world. The genetic reductionist story asserts that humans have evolved to possess genes that inevitably give them specific behaviors. Some of these behaviors (e.g., aggression) are shared by all people because evolution has provided all humans with an almost identical array of genes (i.e., an almost equivalent genotype). There are, of course, also differences in genotypes, and to Lorenz (1965, 1966) it is those differences that are the basis of the most socially important (in my view, pernicious) implication of his concept of instinct.

Some differences are obvious—for instance, between men and women (females have two "X" chromosomes, and men have one "X" and one "Y" chromosome). Other differences may be more subtle and complex, reflecting the differing evolutionary histories of particular groups. In all cases, however, Lorenz, as a committed Darwinist, would hold that genetic differences are outcomes of differences in the history of selection experienced by the groups in question.

The social policy implications of these genetic differences arise when, in Lorenz's (1965, 1966) view, the different selection histories involve civilization's attempts to domesticate and permit the continued survival of individuals who, under the conditions of natural selection, would not otherwise have survived. This point brings back the issue of domestication-induced degeneracy, a theme of central concern to Lorenz during the Nazi era. It appears also in Lorenz's post-World War II writings.

Claims of Selection and Ethical Degeneration in Modern Civilization

In *Civilized Man's Eight Deadly Sins* (1974a), Lorenz spelled out the perils to modern human beings quite specifically and warned:

> If the progressive infantilism and the increasing juvenile delinquency are, as I fear, signs of genetic decay, humanity as such is in grave danger. In all probability, our instinctive high valuation of goodness and decency is the only factor today exerting a fairly effective selection pressure against defects of social behavior.
>
> (p. 58)

Thus, to Lorenz in 1974, domestication had eroded humans' sense of the normal: their ability to tell the difference between pathological and nonpathological, or between good and healthy versus bad and unhealthy. Moral or ethical deterioration, then, was an outcome of domestication phenomena.

Is such ethical degeneration essentially the result of a generalized decline in the genes of all humans, or are there individual differences? Is the threat more a matter of some individuals carrying inferior genes—genes that produce moral and ethical degeneracy? Simply, did the post-Nazi-era Lorenz (1974a) believe, as did the Nazi-era Lorenz (e.g., 1939, 1940a, 1940b, 1943a, 1943b), that some people have ethically inferior genes and some have ethically superior ones? Did the postwar Lorenz continue to believe that the "moral imbeciles" that threatened the health and survival of the *Volk* in the 1940s (Lorenz, 1940a) also existed in post-World War II society? If so, did they put in peril the survival of all nondegenerate humanity, if not the German *Volk*?

Ultimately, the key question is: "What did Lorenz believe must be done to protect humanity against such a threat?" Did he make recommendations redolent of Nazi-era selections and eliminations, albeit perhaps not using the "unfortunate terminology" of that era? Did he once again call on the best among humans—those with their instinctual ethics intact—to help create selection procedures aimed at restoring the natural order? In my view, the answer to these last questions is yes. Lorenz (1975) wrote:

> Selection is and always has been the main creative and developing agent, from the molecular stage at the very beginnings of life up to the process of gaining knowledge by falsification of hypotheses . . . By the very achievements of his mind, man has eliminated all those selecting factors which have made that mind. It is only to be expected that humaneness will presently begin to decay, culturally and genetically, and it is not surprising at all that the symptoms of decay become progressively more apparent on all sides . . . The genetic "domestication" of civilized man is, I am convinced, progressing quite rapidly. Some cardinal symptoms which are present in most of our domestic animals are an increase in size and the hypertrophy of eating as well as of sexual activity. That all three of these symptoms have noticeably increased in man during the short span of my own life, is, to say the least, alarming . . . Equally widespread is the quantitative increase of eating and sexual drive, accompanied in both cases by a loss of selectivity in releasing mechanisms. One has only to go to a beach where many urbanized people are bathing to note the rapidly increasing incidence of fat boys and young men or to look at a great modern illustrated paper in order to be confronted with both symptoms in a thoroughly alarming manner . . . Of course, I do not know for sure that these symptoms are genetic, they may well be cultural, at least in part, but that does not matter much. Cultural development is analogous to genetical evolution in so many areas that the causal distinctions become immaterial as regards the phenomenon here under discussion, except that cultural processes are not less, but more dangerous because of their incomparably greater speed . . . I am convinced that it is one of technocracy's most insidious stratagems to avoid all coercive methods and rely on kind-seeming reinforcements alone . . . I do not think that a healthy philosophy of values can develop without a sense not only of what is good but also of what is evil. It is my chief reproach against the ideology of the pseudodemocratic doctrine that it tends to eradicate, throughout our whole culture, the sense of values on which alone the future of humanity depends . . . I do not believe that the death penalty or incarceration are able to prevent our genetic stock from decay; in fact there is nothing left in civilized society which could prevent retrograde evolution except our nonrational sense of *values*, which I still believe and hope can take a decisive hand in human evolution, both genetic and cultural . . . There is such a thing as good and evil, there are decent guys and there are scoundrels and the difference between them is indubitably partly genetic. No living system can exist without elimination, however humanely it can be brought about and however much one tries not to make it appear as a punitive measure . . . We *know* that evolution stops on its way upward and steps backward when creative selection ceases to operate. Man has eliminated all selective factors except his own nonrational sense of values. We must learn to rely on that.
>
> (pp. 126–128)

This passage underscores quite clearly Lorenz's continuing belief in the genetic basis of society in general, and in the hereditary determination of either the ethical value or the ethical worthlessness of people in particular. As did Haeckel (1876, 1891, 1905), Lorenz (1940a, 1975) saw genetic and cultural evolution as essentially interchangeable, if not identical, processes that have a mystical, or at least nonrational, component which imbues only specific people with a proper sense of values. In addition, I believe that Lorenz saw in 1975, as he did in 1940, a need to rely on the moral responses of the people who have their innate ethical values intact to lead society. That is, he believed that society must rely on people possessing innate ethical values to bring civilization back to the path of healthy evolution from which it has been diverted—given the loss of natural selection processes and the institution of "kind-seeming reinforcements," a phrase reminiscent of Ploetz's (1895; see too Lenz, 1930) characterization of the "misguided" humanitarian social programs that allowed the genetically unfit to survive and reproduce.

Moreover, as did Haeckel (1876, 1891, 1905) and the Nazi-era racial hygienists (Bauer et al., 1927; Lenz, 1930), Lorenz (1975) called for the institution of "creative selection" procedures, which it may be inferred should be conducted by those with the best values. What these procedures should involve is not specified. However, I noted that Lorenz criticized technocracy's avoidance of coercive methods and insisted on the need for elimination procedures beyond the death penalty and incarceration to ensure the continued existence of human beings. These views, frankly, are too consistent with his writings and those of other Nazi-era racial hygienists to lead one to conclude anything other than the continuity of a core theme.

Finally, although Lorenz (1975) was apparently flirting momentarily with environmental, or cultural, causation, he ultimately continued to take a hereditarian stance consistent with the biological determinism of the Nazi racial hygienists: Even if a scientist were to label society's problems as cultural and not biological in origin, with close analysis it would be clear that any causal distinction becomes unimportant. Because of the ontological and material priority of biological processes over cultural processes, the latter ones can be reduced to the former; as such, therefore, at the core, the differences between good and bad people are reduced to genetic inheritance.

If there could be any remaining doubt that Lorenz (1975) continued to insist on the biological basis of morality and ethics and on the possibility of sorting people into good and bad groups, or ethically superior and inferior groups, on the basis of their environmentally immutable genetic inheritance, then some of his own statements should remove this doubt. Shortly after Lorenz was awarded the Nobel Prize, an article by a freelance writer living in Munich, Vic Cox, appeared in the March 1974 issue of *Human Behavior*. Titled "A Prize for the Goose Father," it summarized Lorenz's career, contained excerpts from an interview with him, and discussed Lorenz's Nazi past and current thinking and work. As part of that article, Cox quoted a passage from Lorenz's 1940 publication on "Domestication-Caused Disturbances in Species-Specific Behavior," an article (Lorenz, 1940a) that I discussed and cited earlier in this chapter.

In two places on page 19 of Cox's (1974) article the passage was quoted. In both places the quote was incorrect, containing a small typographical error in one word which, nevertheless, changed the meaning of the passage. The passage was misquoted to read that Lorenz called for "a more severe elimination of the ethnically inferior than has been done so far" (Cox, 1974, p. 19), although as noted in my own quote from this same article (see above), Lorenz actually said: "an even more stringent elimination of the ethically less valuable than is done today."

An extra "n" had been added to the word "ethical," to make the word "ethnical." Thus, it is possible to infer that, in 1940, Lorenz called for elimination, indeed, the "extermination," of people who were ethically inferior by virtue of their genes, but that he did not call for the elimination of any particular ethnic groups that, because of their "race," may have been carriers of inferior genes. My inference about Lorenz's meaning is supported by Lorenz's own words. Lorenz wrote a letter to *Human Behavior* to correct the typographical error and to clarify the views the misquoted passage represented. The letter, appearing in the September 1974 issue, reads in its entirety as follows (Lorenz, 1974c, p. 6):

> I thank you very much for the readiness to correct what was obviously more an error of the printer than of the editor. However, I beg you to realize that changing ethical into ethnical ("A Prize for the Goose Father," March 1974) makes me appear a rabid racist, which I never was. I never believed in any ethnical superiority or inferiority of any group of human beings, though I strongly hold that ethical inferiority of individuals due to heredity or to bad upbringing (lack of motherly love during the first year of life) is indeed a reality, which has to be taken seriously.
>
> I should highly appreciate it if you could include that in the intended correction.
>
> Prof. Dr. Konrad Lorenz, Altenberg, Austria
> (p. 6)

Although Lorenz (1974c) thus insisted he was not a racist, claiming that he had never believed there was a group of humans who, by virtue of their "ethnical" heredity, were inferior, he did believe in 1974, and in the Nazi period, that there was a group of humans who, by virtue of their ethical heredity, were inferior. It was this group—the moral imbeciles and dregs discussed in 1940—that should be eliminated, Lorenz believed. Such a fine conceptual distinction about who is and who is not to be the target of such "special treatment" (to use the Nazi euphemism for extermination) provided little comfort to the men, women, and children who were sent to the gas chambers and crematoria. In addition, Lorenz's conceptual distinction between ethnical and ethical has little historical validity, given the fact that leaders in Nazi Germany equated ethical degeneracy with membership in a particular ethnic group (i.e., Jews).

Lorenz and Sociobiology

The core message of the Lorenz of the post-World War II era is, therefore, not conceptually different from that of the Lorenz of the Nazi era. There is continuity in his views of the basic causes of both individual behavior and the social order: They are evolutionarily based, genetic causes. There is also continuity in his beliefs about the basis of social problems and the threats to civilization: the erosion of natural selection; the reproduction, therefore, of hereditary moral inferiors who otherwise would have not survived; and the degeneracy in healthy instinctual patterns produced by the domestication of these inferiors. Ultimately, there is also continuity in the remedies Lorenz sees as requisite for saving civilization: Humans must rely on the nonrational ethical responses of the genetically-ethically superior among them, and charge them with creating selection procedures to replace the eroded natural selection ones. Such actions will therefore eliminate the threat that domestication-induced ethical degeneracy poses to healthy human genetic stock.

Given what I regard as the quite evident Nazi-era/post-Nazi-era continuity in the core message of Konrad Lorenz, it is extremely puzzling that he received the world's most prestigious scientific award. Nevertheless, when other biological-determinist positions were then promulgated, the broad scientific and societal legitimization of biological-determinist ideology that the Nobel Prize provided could not help but impart to that ideology, for scientists and for other citizens alike, an aura of believability and the impression that this perspective was on the cutting-edge of "normal science." This creation of a biological-determinist *Zeitgeist* (spirit of the times) may have contributed in part to the broad scientific and social attention E. O. Wilson received in 1975, when he announced the presence of the new, synthetic discipline of sociobiology (see Chapter 12).

The affinity between the work of Lorenz and that of contemporary sociobiologists is highlighted by the frequent citations of Lorenz's work in the sociobiological literature, citations usually made approvingly and in the service of marshaling support for one or another sociobiological idea (e.g., Barlow, 1980; Dawkins, 1976; Freedman, 1979; Konner, 1982). In addition, this reliance on Lorenz's work has led some contemporary sociobiologists to go out of their way to defend Lorenz against often unnamed critics and vaguely described criticism.

For instance, sociobiologist Melvin Konner, in his 1982 book, *The Tangled Wing*, praises one of Lorenz's Nazi-era papers ("Der Kumpan in der Umwelt des Vogels" [The Companion in the Bird's World, 1935]), reminds the reader that Lorenz was a Nobel Prize recipient, and then, in what is clearly a non sequitur, asserts that people are incorrect if they judge Lorenz only on his late (but uncited) popular writings or on the (undescribed) comments of his (unnamed) critics:

> It is a magnificent paper, not only informative and convincing, but sweeping, incisive, beautiful. Reading it gives an impression very similar to that gained by reading Freud's early anatomical writings: that one has been very wrong to judge Lorenz only by his late popular writings, or worse, secondhand, by the opinions of his critics.
>
> (Konner, 1982, p. 124)

Among the several problems with Konner's (1982) vague defense of Lorenz is the marked continuity between the topics, themes, and opinions found in the writings of Lorenz published in the Nazi era and in the post-Nazi era: There was little change in Lorenz's views about domestication-induced degeneracy, about selection, and about the genetic basis of moral worth and moral degeneracy. Simply, Konner (1982) proposed a division of Lorenz's work that cannot be supported by a close analysis of Lorenz's views across this span of time. What Konner's (1982) remarks do suggest, however, is that perhaps sociobiologists may look approvingly on the core ideas of Lorenz because his ideas are similar to the ideas they themselves promote. In the next chapter, I consider these latter, sociobiological ideas in more detail and draw further parallels with the work of Lorenz. Here, however, I discuss other genetic reductionists whose work also influenced contemporary instantiations of such reductionism.

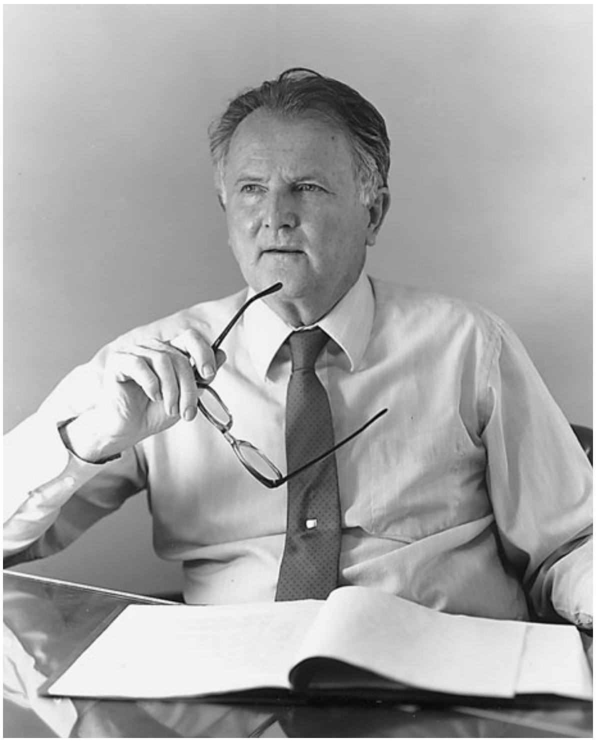
SOME TWENTIETH-CENTURY EXAMPLES OF GENETIC REDUCTIONISM: 2. THE WORK OF SIR CYRIL BURT AND ARTHUR JENSEN

In the late 1960s, the different average group scores of African American and European American children on intelligence tests (i.e., scores that reflected the computation of an "intelligence quotient" or IQ) became a point of major public concern. The mean (i.e., the arithmetic average) difference between these two groups is often reported to be as high as 15 IQ points (e.g., Jensen, 1980; Rushton, 1999, 2000) in favor of the European American children. That is, on standardized intelligence tests, European American children as a group typically score higher than do African American children as a group. However, this difference does not mean that African Americans always do worse on IQ tests than do European Americans. In fact, as Jensen (1973) pointed out:

> Although the average IQ of the Negro population of the United States, for example, is about one standard deviation (i.e., 15 IQ points) below that of the white population, because of the disproportionate sizes of the Negro and white populations, there are more whites with IQs below the Negro average than there are Negroes. (p. 16)

Until the late 1960s, psychologists in the United States interpreted these group differences in IQ scores as being environmentally based. That is, stress was placed on the cultural disadvantages of African Americans. The leading hypothesis was that a complex of environmental factors associated with poverty prevented a child from achieving optimum development (Scarr-Salapatek, 1971a, 1971b). Such environmental disadvantage, it was argued, accounts for the inferior performance of African American children on standardized IQ tests. In essence it was hypothesized that it is not African American children but their environments that are deficient.

Assuredly, no one could argue against the point that African Americans as a group have experienced a history of inferior and possibly even pernicious environmental circumstances. In fact, in my discussion of social stereotypes in Chapter 6, I noted that environmentally-based social attitudes may have a destructive effect on African Americans' intellectual development. Accordingly, developmental scientists working with the "environmental-differences" hypothesis have attempted to determine the nature of the environmental variables that led African American children to inferior performance on IQ tests. These scientists also contributed to social projects designed to ameliorate African Americans' environmental disadvantages (e.g., Project Head Start).



Arthur Jensen

The Genetic-Differences Hypothesis

What brought the IQ score differences between African Americans and European Americans to the general public's attention was that an alternative hypothesis, suggested by Arthur R. Jensen (1969), was offered for investigation. Writing in the *Harvard Educational Review*, Jensen (1969) proposed a genetic-differences hypothesis as an alternative to the environmental-differences explanation of the IQ differences between African Americans and European Americans. Jensen suggested that, if behavior and characteristics of behavioral functioning (such as intellectual behavior, as indexed by IQ) can be measured and found to have a genetic component, then such behavior can be regarded as no different from other human characteristics, at least insofar as a genetic viewpoint is concerned. Moreover, he asserted that "there seems to be little question that racial differences in genetically conditioned behavioral characteristics, such as mental abilities, should exist, just as physical differences" (Jensen, 1969, p. 80).

This hypothesis, in one form or another, has remarkable persistence in Western culture. For instance, Herrnstein and Murray (1994) and Rushton (2000) reiterated it over a quarter-century after Jensen (1969) presented his formulation of the idea—despite the broad and thorough documentation of the egregious conceptual and methodological flaws associated with it. Indeed, Gould (1981, 1996) noted that the idea of genetically-based intellectual differences between "racial" groups has not only had a long record of presentations after Jensen (1969) but also a history of more than 100 years before him.

Thus, in 1969, Jensen was only the latest in a long series of individuals contributing to a hereditarian view of the psychological and social differences between racial groups. Accordingly, after reviewing several lines of evidence bearing on the general idea of race differences in intelligence and their possible sources, Jensen (1969) advanced what was to him:

> a not unreasonable hypothesis that genetic factors are strongly implicated in the average Negro–white intelligence difference. The preponderance of the evidence is, in my opinion, less consistent with a strictly environmental hypothesis than with a genetic hypothesis, which, of course, does not exclude the influence of environment or its interaction with genetic factors.
>
> (p. 82)

Jensen proposed what J. Philippe Rushton (1999, 2000) later asserted. Rushton was a strong advocate of nature-based race differences. He believed that the differences in mean IQ between African Americans and European Americans were *not* due to differences in environmental opportunity but, instead, to differences in the gene distributions for these groups (Scarr-Salapatek, 1971a). In Jensen's attempt to support ideas akin to those that Rushton espoused, Jensen presented empirical data bearing on the racial difference in IQ scores and interrelated these findings with data bearing centrally on the concept of heritability.

According to behavioral geneticists (e.g., Bouchard & McGue, 1981; Jensen, 1969), heritability is the proportion of a group's individual differences in a "trait" (e.g., a psychological characteristic such as intelligence) that is due to the individual genetic differences in that group. If a group of people are given an intelligence test, not everyone in the group will get the same score; there will be differences between people. Heritability is a concept that indicates the percentage (or proportion) of these differences in intelligence test scores that can be attributed to (accounted for by) genetic differences between these people.

Jensen argued that IQ is a highly heritable trait—that is, that individual differences (variation among people) in IQ scores within a group are mostly due (e.g., 80%) to the genetic variation in that group. In other words, he pointed out that about 80% of the differences between the people within specific groups are attributable to genetic differences among these people. Therefore, because of these relations, it might seem tenable to argue that, because heritability appears to be a genetic concept, the IQ differences between African Americans and European Americans are, in turn, genetically based. Of course, Jensen recognized that most of the studies done to assess the heritability of intelligence had been done on European American individuals; such estimates—of how much of the differences in IQ scores between members of specific populations can be attributed to genetic differences between these people—cannot be appropriately applied to other populations. Thus, Jensen (1973) pointed out:

> Although one cannot formally generalize from within-group heritability to between-groups heritability, the evidence from studies of within-group heritability does, in fact, impose severe constraints on some of the most popular environmental theories of the existing racial and social class differences in educational performance.
>
> (p. 1)

In addition, although Jensen recognized that it was not perfectly legitimate to attempt to apply heritability findings derived within groups of European Americans to an analysis involving a comparison between European American and African American groups, he still believed that the findings with European Americans were impressive enough to cast doubt on the environmental-differences hypothesis.

As such, Jensen (1969) offered for consideration a hereditarian, genetic-differences hypothesis. He proposed this hypothesis in an attempt to explain why major educational intervention programs, such as Head Start, were apparently failing in the 1960s in the attempt to raise the IQs of both African American and European American lower-class children.

Moreover, Jensen's (1969, 1972) ideas were linked to appeals to consider the import of his hypothesis for the control of the breeding (the reproduction) of what he termed "Negro Americans." The link between hereditarian claims about the genetic bases of racial differences in mental and behavioral functioning and appeals for selective breeding of humans (i.e., for eugenics) has been discussed earlier in this chapter. The work of Konrad Lorenz exemplified the call for such linkages (e.g., Lorenz, 1940a, 1940b, 1943a, 1943b, 1965, 1966). Jensen (1969, 1972) held eugenic ideas consistent with those of Lorenz. For instance, Jensen (1969) suggested:

> Is there a danger that current welfare policies, unaided by eugenic foresight could lead to the genetic enslavement of a substantial segment of our population? The possible consequences of our failure seriously to study these questions may well be viewed by future generations as our society's greatest injustice to Negro Americans.
>
> (p. 95)

This issue was of such concern to Jensen that he republished this statement three years later (Jensen, 1972).

Given, then, the quite significant scientific and social policy implications that Jensen associated with his hypothesis—implications also noted by reviewers (Goldberger & Manski, 1995; Hirsch, 1997) of later work that drew on Jensen (1969) as a basis for much the same arguments about the genetic bases of race differences in IQ scores (Herrnstein & Murray, 1994)—it is important to assess the logical and methodological features of Jensen's hypothesis. I think it is useful to consider the analysis by Jerry Hirsch (1970) of the reasoning used in formulating this genetic-differences hypothesis, and, thus, inferring the implication that African Americans are genetically inferior to European Americans in intellectual capacity:

1. First, a trait (such as intelligence) is defined.
2. A means of measuring this trait is devised; a psychological test, designed to measure the trait (intelligence), is constructed. Needless to say, if another definition of the trait were offered, and if other tests of the trait were constructed and used, the empirical expression of the trait could be expected to be different. The possibility that the use of different intelligence tests could lead to different findings in terms of African-American–European-American differences in intelligence is important. Intelligence tests do not correlate with each other perfectly; that is, the scores for the same individual on two different intelligence tests are often not exactly equivalent. Therefore, if other tests are given to African-American and European-American populations—tests not standardized exclusively on European-American, middle-class populations, for example, but rather tests that take into account the specificities of the African-American cultural milieu—then the status of racial differences in IQ might be different. Holding the

> tenability of this point in abeyance, however, let us assume for argument's sake that the same test is used to measure the trait expression in people.
>
> 3. Through a series of studies of test scores for this trait done on populations comprised of people of various degrees of kinship (relationship), the heritability of this trait is estimated.
> 4. Different racial populations are then tested, and their performances on this test of the trait are compared.
> 5. If the racial populations differ on the test, then because the heritability of the trait measured by the test is now known (and in the case of intelligence has been found to be high), the racial population with the lower mean score is considered to be genetically inferior.
>
> (p. 69)

Harrington (1975, 1988) demonstrated crucial flaws in the reasoning associated with Jensen's (1969) hypothesis and, as well, with the scientific procedures/methods pursued to support this reasoning. Harrington (1975, 1988) provided a compelling experimental demonstration showing:

> that the very assumptions and procedures employed in psychometric test construction ensure minority-group inferiority and principal-group superiority in testing outcome. With several races ("homogeneous lines") of rats combined in various proportions to comprise different populations (i.e., the same race was a plurality in one, a minority in another, absent in a third, and of intermediate proportions in still other populations), standard psychometric procedures were used to select items to construct a separate intelligence test for each population. When all tests were administered to each race, the test performance of a race was found to be positively correlated with the proportional representation of that race in the base population on which a test had been standardized; that is, races received higher scores on tests derived from populations in which they comprised a plurality and lower scores on others from populations in which they were a minority or absent.
>
> (Harrington, 1975, p. 709)

Harrington (1975) interpreted this study as indicating that:

> Generalisation from these data to man is direct and not analogical: the experiment was an empirical test of common psychometric assumptions and procedures. Generalisation is therefore to those assumptions and procedures. The implications are far ranging. Majorities will score higher than minorities as a general artifact of test-construction procedures. Theoretical approaches which ignore the existence of genetic–environment interactions ignore a significant source of individual and of group variation.
>
> (p. 709)

On the basis of his formulation of the genetic-differences hypothesis, Jensen (1969) suggested that early intervention programs had failed to boost IQ and the reason for this outcome was due to the high heritability of IQ (see also Scarr-Salapatek, 1971a, 1971b). Moreover, because to at least some reviewers of Jensen's ideas (see Jensen, 1973) it was erroneously assumed that high heritability of a characteristic indicates that the characteristic is minimally available to environmental influence, it followed that (a) because IQ is a highly heritable characteristic, the environment can have little influence in affecting the expression of that trait; and (b) therefore programs such as Head Start, that attempt to present alternative environmental influences to some children, have little effect because the target of influence is IQ.

The hereditarian, genetic-differences hypothesis is both complex and important. Its evaluation is a major burden of this chapter. In order to evaluate it, I have to discuss general limitations of the concept of heritability (e.g., Moore & Shenk, 2016) and the specific problems associated with appraising the heritability of intelligence (e.g., Richardson, 2017).

Estimating the Heritability of Intelligence

The term "heritability," as it is used today in human behavioral genetics, is one of the most misleading in the history of science. Contrary to

> popular belief, the measurable heritability of a trait does not tell us how "genetically inheritable" that trait is. Further, it does not inform us about what causes a trait, the relative influence of genes in the development of a trait, or the relative influence of the environment in the development of a trait. Because we already know that genetic factors have significant influence on the development of all human traits, measures of heritability are of little value, except in very rare cases. We, therefore, suggest that continued use of the term does enormous damage to the public understanding of how human beings develop their individual traits and identities.
>
> David S. Moore and David Shenk (2016, p. 1)

How does one find data pertinent to the inheritance of a human characteristic such as intelligence? Intelligence, if it is inherited, is not believed by any scholar to derive from the inheritance of a single pair of genes (Plomin, 1986, 2000; Rushton, 1999). That is, neither nature- nor nurture-oriented scholars contend that a child inherits his or her intelligence by the pairing of just two alleles at conception (an allele is a particular form of a gene; the presence of different alleles creates genetic variation), one from the mother and one from the father. Rather, if intelligence is inherited it is a polygenic (many-gene) type of inheritance. In addition, intelligence is quite a plastic human characteristic; plasticity here refers to the fact that intelligence can take many values (it is a continuous variable).

The study of intelligence as a polygenic and plastic human characteristic is complicated by the fact that for obvious ethical (and less obvious but important technological) reasons, a researcher cannot do true experimental studies of the relative effects of genes and environment on human intelligence. Such a true experiment might involve holding genes constant and seeing if environmental variation led to changes in IQ scores. However, a co-twin control study using MZ pairs could provide such evidence, and would probably be ethical (Joseph, 2015). If it did, then hereditarians would see this finding as a problem for their perspective (e.g., Bouchard, 1995; Rushton, 1999), in that it would document the impact of the context on the development of intelligence. If it did not—if despite environmental variability a particular set of genes was invariantly related to a particular IQ score—then support for a nature position would be claimed by hereditarians. In turn, one could vary genes, or actually genetic similarity among people, and see if, in identical environments, any differences are obtained in IQ scores. If such variation was associated with IQ score differences, then hereditarians would contend that support for a nature view was provided. Alternatively, if such genetic variation was not associated with IQ score differences—if, despite differences in genes, people in comparable environments had corresponding IQ scores—then support for the hereditarian position would be diminished.

But, as I have noted, such manipulations cannot be intentionally implemented for ethical and technological reasons. For instance, how could one hold all potentially significant features of the human environment constant? However, variations may naturally occur that present conditions that it might be possible to assume were comparable to a true experiment involving holding environment constant. For example, some children are born as monozygotic (MZ) twins; that is, as noted earlier, after the ovum is fertilized by the sperm, the one zygote that is formed splits into two genetically identical zygotes. Typically, such MZ twins not only have identical genotypes but also quite similar environments; that is, they are reared in the same intrauterine environment and, then, usually in the same home by the same parents. Thus, for most MZs there is a gene–environment correlation.

Imagine, however, that one could locate a group of MZs who were immediately separated after birth (e.g., because of maternal death or because of financial stresses on the family) and placed in two radically different environments. If, despite the twins' rearing in separate and distinct environments, their IQs were quite similar, this correspondence would be seen as evidence in support of the hereditarian position (Rushton, 1999). Similarly, if one assessed the naturally occurring resemblance among all types of genetically related people—for instance, MZs, dizygotic twins (i.e., twins born of the same pregnancy but developed from two different fertilized eggs), cousins, and unrelated people—and found that, as genetic resemblance decreased (as it does in the above ordering), IQ resemblance also decreased, then such a result might seem to lend additional support to the hereditarian position.

Although data from true experiments of the role of genes and environment in human intelligence do not exist, purported approximations of such studies are reported (however, see Joseph, 2015; Richardson, 2017, for reviews of such twin research). In the main, these data were the ones most relied on by Jensen (1969). Information from such studies provided the key information from which Jensen derived the heritability estimate of 0.8 for human intelligence. As such, I will evaluate the data used by Jensen and others (e.g., Eysenck, in Eysenck & Kamin, 1981a) in these heritability estimates. Here, it is important for both historical and substantive reasons to focus on the role that the writings of Sir Cyril Burt played in regard to the data base relied on by Jensen (1969) to discuss the heritability of intelligence.

Although Jensen relied on many data sets pertinent to family resemblance in genes and in IQ scores, he relied most centrally on the information reported by Sir Cyril Burt (1883–1971) who, during his lifetime, was one of the world's most famous and celebrated psychologists. Burt was the official psychologist of the London County Council for 20 years, where his responsibilities included administration and interpretation of mental tests in the London schools (Gould, 1981, 1996). After this service, from 1932 to 1950, he held the most influential professorial chair in psychology in Great Britain, the one in University College, London. For his accomplishments in science and service he was knighted and then, in 1971, given the prestigious Edward Lee Thorndike Award of the American Psychological Association.

During his long career, Burt published numerous papers reporting his research on family resemblance and IQ (e.g., Burt, 1955, 1966). In particular, Burt's work involved reports of assessments of a relatively large sample (i.e., large considering their seeming rarity) of MZs reared apart (e.g., Burt, 1955, 1958, 1966; Conway, 1958). The findings that Burt reported from this research were such as to lend strong support to the hereditarian position.

Thus, not only because of his positive scientific reputation but also because of the scope of his research and the nature of his findings, Burt's reports were heavily relied on by Jensen (1969). For instance, Jensen relied on Burt's findings because Burt claimed that his separated MZs were reared in a wide range of socioeconomic environments (Joseph, 2015). As such, in order to begin my evaluation of the scientific database used in support of the heritability of IQ, and of the genetic-differences hypothesis, it is important to discuss the scholarly record of Cyril Burt.

The Work of Sir Cyril Burt

Why were the Burt data of such importance to Jensen's (1969) argument and to any assertion that most of the variance in intelligence is associated with one's genotype? The evolutionary biologist Stephen J. Gould (1981), in his typically engaging style, answered these questions by noting:

> If I had any desire to lead a life of indolent ease, I would wish to be an identical twin, separated at birth from my brother and raised in a different social class. We could hire ourselves out to a host of social scientists and practically name our fee. For we would be exceedingly rare representatives of the only really adequate natural experiment for separating genetic from environmental effects in humans—genetically identical individuals raised in disparate environments.
>
> Studies of identical twins raised apart should therefore hold pride of place in literature on the inheritance of IQ. And so it would be but for one problem—the extreme rarity of the animal itself. Few investigations have been able to rustle up more than twenty pairs of twins. Yet, amidst this paltriness, one study seemed to stand out: that of Sir Cyril Burt (1883–1971).
>
> During his long retirement, Sir Cyril published several papers that buttressed the hereditarian claim by citing very high correlation between IQ scores of identical twins raised apart. Burt's study stood out among all others because he had found fifty-three pairs, more than twice the total of any previous attempt. It is scarcely surprising that Arthur Jensen used Sir Cyril's figures as the most important datum in his notorious article (1969) on supposedly inherited and ineradicable differences in intelligence between whites and blacks in America.

> The story of Burt's undoing is now more than a twice-told tale. Princeton psychologist Leon Kamin first noted that, while Burt had increased his sample of twins from fewer than twenty to more than fifty in a series of publications, the average correlation between pairs for IQ remained unchanged to the third decimal place—a statistical situation so unlikely that it matches our vernacular definition of impossible.
>
> (pp. 234–235)

It is useful to review the information that Kamin (1974) found as he reviewed Burt's (1955, 1958, 1966; Conway, 1958) publications. This information reveals so many major problems with Burt's work that a close and fair scrutiny of it will indicate that "the apparent evidence for IQ heritability will evaporate to nothing" (Kamin, 1974, p. 35).

Cyril Burt

Kamin (1974) noted that the Burt study would seem to be impressive and important in several ways. Not only did it purport to study more twin pairs than any of the other three similar studies for which quantitative evidence was available when Kamin did his review (i.e., the studies by Juel-Nielsen, 1965; Newman, Freeman, & Holzinger, 1937; and Shields, 1962), but also the Burt study reported the largest correlation between the separated MZ twins. These two features of the Burt study (its larger sample and the higher correlation found) are displayed in Table 11.1, which also presents the corresponding information for the other three studies reviewed by Kamin (1974). Kamin noted, too, that the most important feature—unique to Burt's (1955, 1958, 1966) study—is that it claimed to provide quantitative evidence that the environments in which the separated twins were reared *were not* correlated.

If the environments of the separated twins *were* correlated, one might be able to attribute any similarity in their IQ scores to the experience of being reared in comparable settings; however, if there was no similarity between their environments, then it is argued that only their genetic similarity could account for any correspondence in their IQs. As I will discuss later, a key point about the computation of heritability is that, for the hereditarian position to be supported, it is important that genotype not interact with environment (in the statistical sense of interaction; see Chapter 10). If such interactions occurred, they would diminish the impact of genes alone in the determination of IQ scores. I will discuss later whether it is possible to determine—in studies of human intelligence—whether genotype and environment are uncorrelated, or whether they do not interact (see Richardson, 2017). If one cannot empirically demonstrate these two points or if one cannot legitimately assume the two points to be the case, then it would not be possible to assert unequivocally that it was primarily genetic similarity that provided the basis for correspondences in IQ scores. Here, however, I should reemphasize the point that Burt's study was unique in offering quantitative evidence that the environments of the separated MZ twins were uncorrelated. In short, there seemed to

Table 11.1 IQ correlations in four studies of separated MZ twins

Study	Test	Correlation	Number of pairs
Burt (1955, 1958, 1966)	"Individual Test"	0.86	53
Shields (1962)	Dominos + 2 × Mill Hill	0.77	37
Newman, Freeman, and Holzinger (1937)	Stanford-Binet	0.67	19
Juel-Nielsen (1965)	Wechsler	0.62	12

Source: Adapted from Kamin (1974, p. 35).

Table 11.2 Correlations reported by Burt

	Siblings reared apart		DZs reared together	
	1955 (*N* = 131)	1966 (*N* = 151)	1955 (*N* = 172)	1966 (*N* = 127)
Intelligence				
Group test	0.441	0.412	0.542	0.552
Individual test	0.463	0.423	0.526	0.527
Final assessment	0.517	0.438	0.551	0.453
School attainment				
Reading, spelling	0.490	0.490	0.915	0.919
Arithmetic	0.563	0.563	0.748	0.748
General	0.526	0.526	0.831	0.831
Physical				
Height	0.536	0.536	0.472	0.472
Weight	0.427	0.427	0.586	0.586

Note. N refers to number of pairs, as reported by Burt.

Source: Kamin (1974, p. 37).

be several quite distinct and important attributes of Burt's (1955, 1958, 1966) study, and because of these "assets," it is clear why Burt's study played such a central role in Jensen's (1969) argument.

But what of the scientific adequacy of Burt's "data"? Kamin (1974) noted that Burt's study of separated MZ twins was part of a larger research effort aimed at accumulating IQ data for several categories of biological relationships (e.g., parent–child and uncle–nephew). Over the years, as the sample size for each category of biological relationship grew, the correlations were reported in a series of publications. But here is where the purported scientific importance of Burt's data begins to unravel. Kamin (1974) indicated that the series of publications by Burt:

> contain virtually no information about the methods employed in testing IQ, but the correlations were usually reported to three decimal places. They were astonishingly stable, seeming scarcely to fluctuate as the sample size was changed. Two forms of such stability are illustrated in [Table 11.2], which reproduces data contained in Burt's 1955 and 1966 reports.
>
> The intelligence correlations were reported in three forms: for "group test," for "individual test," and for "final assessment." There is, as we shall see, much ambiguity concerning what these terms mean.
>
> This remarkable stability also characterized the unknown "group test" of intelligence which Burt administered to his separated MZ twins.

Table 11.3 Correlations for MZ twins, "group test" of intelligence

Source	Twins reared apart	Twins reared together
Burt, 1955	0.771 (*N* = 21)	0.944 (*N* = 83)
Burt, 1958	0.771 (*N* = "over 30")	0.944 (*N* = ?)
Conway, 1958	0.778 (*N* = 42)	0.936 (*N* = ?)
Burt, 1966	0.771 (*N* = 53)	0.944 (*N* = 95)

Source: Kamin (1974, p. 38).

> [Table 11.3] reproduces the correlations reported by Burt on that test for MZ twins reared apart, and for MZ twins reared together. The table includes data from a 1958 paper by Burt and a 1958 paper by Conway, as well as data from Burt's 1955 and 1966 papers. The sample sizes increased over time by 32 pairs for MZs reared apart, and 12 pairs for MZs reared together. There is a minor perturbation which simultaneously afflicted both correlations in late 1958, but a benign Providence appears to have smiled upon Professor Burt's labors. When he concluded his work in 1966, his three decimal place correlations were back to where they had been in the beginning. The 1943 paper had contained his first reference to separated MZs. Then, for 15 pairs, the correlation had been reported as 0.77.
>
> Burt, often without specific acknowledgment, employed "adjusted assessments" of IQ rather than raw test scores. The reader must be sharp-eyed to detect this on occasion. The 1956 Burt and Howard paper reported correlations for "assessments of intelligence" for 963 parent–child pairs, 321 grandparent–grandchild pairs, 375 uncle–nephew pairs, etc. The term "assessments" was not defined, and the description of procedure is characteristic of Burt. "The sources for the latter [assessments], the procedures employed, and the results obtained have already been described in previous publications (Burt, 1955, and refs.)." The Burt and Howard paper, which has been very widely read and cited, goes on to fit a mathematical-genetic model of inheritance to the reported correlations. The fit is excellent.
>
> The reader who troubles to refer to the 1955 paper will discover that many of the 1956 results were not reported there, and he will also discover that the entire description of procedure is contained in a footnote. The footnote includes the following sentence: "For the assessments of the parents we relied chiefly on personal interviews; but in doubtful or borderline cases an open or a camouflaged test was employed." That sentence bears pondering. The scores of children, on the other hand, were "based primarily on verbal and non-verbal tests of intelligence . . . transformed into standard scores . . . for each age converted to terms of an IQ scale." Whatever ambiguity exists in the case of children, clearly the intelligence of adults was simply guessed at in the course of a personal interview. The spectacle of Professor Burt administering a camouflaged test of intelligence to a London grandparent has considerable comic merit, but it does not inspire scientific confidence. The only reported IQ correlation between uncle and nephew in the entire scientific literature appears to be Burt's, obtained in this survey in this way.
>
> The same survey was cited by Burt and Howard in 1957, who in reply to a critic stressed that "in each of our surveys, assessments were individually obtained for a representative sample of parents, checked, for purposes of standardization, by tests of the usual type." The 1955 footnote is cited. There is some ambiguity in the meaning of "for purposes of standardization"; but it is entirely clear that over two years Professor Burt's memory had magically transmuted "doubtful or

> borderline cases" to "a representative sample of parents." The "open or a camouflaged test" of 1955 had become by 1957 "tests of the usual type." Professor Burt, we may conclude, was not always precise in his use of language. The procedural ambiguities are no less marked in the case of Burt's 53 pairs of separated MZ twins. These cases had been gradually accumulated over a period of some 45 years. The most explicit and extended discussion of the twin data was given in Burt's 1966 paper. That paper indicates that all the twins had been separated before the age of 6 months, but it contains no information about the extent or duration of separation. There is no information about the sexes of the twin pairs, nor is their age at testing indicated. They were all, however, "children," and except in three cases "the tests were applied in school." Three very early cases had been dropped from the sample because of a relatively late age of separation. There were, "in the initial survey," some children outside London "originally tested by the local teacher or school doctor, but these have all been since retested by Miss Conway." We are not told whether Miss Conway's test results corresponded to the teachers', nor whether discrepancies were averaged, or handled in some other way.
>
> (pp. 37–40)

Because of these egregious scientific deficiencies in Burt's (1955, 1958, 1966) study, Kamin (1974, p. 41) summarized his assessment of its value by simply indicating that "we do not know what was correlated with what in order to produce the coefficient of .77" between MZ twins reared apart (as shown in Table 11.3). In sum, I agree with Kamin's (1974) overall appraisal that:

> The conclusion seems not to require further documentation, which exists in abundance. The absence of procedural description in Burt's reports vitiates their scientific utility. The frequent arithmetical inconsistencies and mutually contradictory descriptions cast doubt upon the entire body of his later work. The marvelous consistency of his data supporting the hereditarian position often taxes credibility; and on analysis, the data are found to contain implausible effects consistent with an effort to prove the hereditarian case. The conclusion cannot be avoided: The numbers left behind by Professor Burt are simply not worthy of our current scientific attention.
>
> (p. 47)

Reactions to Kamin's (1974) Analysis and Subsequent Developments

Many of Burt's supporters and/or advocates of the hereditarian position reacted unfavorably to Kamin's (1974) work (for a detailed history of this reaction, see Hirsch, 1981). For example, some of those sympathetic to Burt's position denigrated Kamin's competence and claimed that his motives in writing his critique were purely political. For example, Hans J. Eysenck, whose own work in this area I consider shortly, wrote to Burt's sister and said that (quoted in Gould, 1981):

> I think the whole affair is just a determined effort on the part of some very left-wing environmentalists determined to play a political game with scientific facts. I am sure the future will uphold the honor and integrity of Sir Cyril without any question.
>
> (p. 234)

In turn, some of Burt's defenders acknowledged that Burt's work and publications may have included some errors, but attributed this to the poor memory and unfortunate carelessness of an aging and infirm scholar (e.g., Jensen, 1974; see also Gould, 1981, 1996, and Hirsch, 1981, for a discussion of this reaction).

But on October 24, 1976, Oliver Gillie, medical correspondent of the London *Sunday Times*, wrote a front-page story with the headline "Pioneer of IQ Faked His Research Findings," which presented information that, as Gould (1981) put it,

> elevated the charge from inexcusable carelessness to conscious fakery. Gillie discovered, among many other things, that Burt's two "collaborators," a Margaret Howard and a J. Conway, the women who supposedly collected and processed

> his data, either never existed at all, or at least could not have been in contact with Burt while he wrote the papers bearing their names. These charges led to further reassessments of Burt's "evidence" for his rigid hereditarian position. Indeed, other crucial studies were equally fraudulent, particularly his IQ correlations between close relatives (suspiciously too good to be true and apparently constructed from ideal statistical distributions, rather than measured in nature (Dorfman, 1978), and his data for declining levels of intelligence in Britain.
>
> (p. 235)

The Burt issue may be resolved by the findings made and conclusions drawn in a biography of him written by L. S. Hearnshaw (1979) and authorized by Burt's sister before the "scandal" about Burt began to appear. As summarized by Goldberger (1980), prior to beginning work on the biography Hearnshaw's assessment of Burt was

> almost wholly favorable. But as he worked through the large collection of Burt's writings, correspondence, memoranda, diaries, lecture notes, and other material made available to him, Hearnshaw "became convinced that the charges against Burt were, in their essentials, valid."
>
> Herewith some specifics. First, on the separated identical twins, "no data were collected between 1955 and 1966," the period during which Burt claimed to have increased his sample size from 21 pairs to 53 pairs. (On my reading, there is no evidence that the first 21 pairs were any more real.) Second, on the mystery women: "of the more than forty 'persons' who contributed reviews, notes and letters to the journal during the period of Burt's editorship, well over half are unidentifiable, and judging from the style and content of their contributions were pseudonyms for Burt. Howard and Conway were members of a large family of characters invented."
>
> Third, and most intriguing, on the list of individual IQ scores and social class for the 53 pairs of separated identical twins: Christopher Jencks, having seen the summary-statistics from this sample in Burt's publications, wrote in December 1968 to ask for the individual figures in order to undertake some regression analysis for his book in progress, *Inequality*. (Jensen, who had many visits and discussions with Burt, never attained that advanced level of curiosity.) Seven weeks later, Burt sent the list with a covering letter which began, "I apologize for not replying more promptly; but I was away for the Christmas vacation, and college (where the data are stored) was closed until the opening of term."
>
> But Hearnshaw reports, "As a matter of fact Burt had not been away for Christmas; his data were not stored at college; and the college had only been closed for a week . . . According to his diary Burt spent the whole week from 2 January 1969 onwards 'calculating data on twins for Jencks.' . . . Had the IQ scores and social class gradings been available they could have been copied out in half an hour at the most. So quite clearly the table of IQ scores and social class gradings was an elaborately constructed piece of work." (I confess to some sympathy with Burt at this point: at the age of 86, he was faced with the task of making up 53 pairs of numbers consistent with the mean, standard deviation, and correlation coefficient which he had previously published.)
>
> On the cross-tabulation of fathers' and sons' IQs by social class, Hearnshaw reviews the evidence and finds that "there is no doubt that Burt's reporting of his sources and methods was grossly inadequate, and little doubt that the data he possessed had been subjected to a good deal of 'adjustment.'" (Here Hearnshaw left one avenue of investigation unexplored. In *Science* [September 29, 1978], D. D. Dorfman gave a persuasive demonstration that the source of Burt's table was the formula for the bivariate normal distribution function rather than a representative sample of British men.)
>
> Hearnshaw's study of Burt's private papers thus documents what would have been apparent to any thoughtful reader of Burt's public papers: while his figures may have been numbers, they were surely not data. It is remarkable that they passed as data—indeed as crucial data—for so many years.
>
> (p. 62)

Indeed, Goldberger's (1980) last point was also raised by Gould (1981) in his own comments about the Hearnshaw (1979) biography. Gould noted that the discovery of such seemingly obviously flawed data "does not touch the deeper issue of why such patently manufactured data went unchallenged for so long, and what this will to believe implies about the basis of our hereditarian presuppositions" (Gould, 1981, p. 236).

One point that is obviously implied is that the power of this presupposition may lead those holding it to misrepresent and distort information. As a case in point, Hirsch (1981) noted that the "balance sheet on Burt" published by the British Psychological Society (Gillie, 1980, p. 12):

> documents the following "falsehoods" propounded by . . . Jensen in the attempt to "cover-up" the Burt scandal:
>
> 1. Jensen said that Professor Hearnshaw had "located some of the identical twins reared apart, tested by Burt and Margaret Howard." This is quite untrue.
> 2. Jensen suggested that I was related in some way to Professor William Stephenson, a student of Burt who had quarreled with him. This is untrue.
> 3. Jensen said that Margaret Howard was a faculty member of the Mathematics Department of the University of London yet there is absolutely no evidence for this.
> 4. Finally, Jensen attributed political motives to all Burt's detractors, whether or not he had met them, saying that "anyone who has had any contact with them, or even lunch with them knows . . . in the first 15 minutes." However, Jensen had never met the Clarkes nor me. And if he could tell me my own political position after 15 minutes I would be grateful because I am not clear what it is myself.
>
> (p. 31)

Earlier, in his discussion of "Burt's Missing Ladies" in *Science*, Gillie (1979, p. 1036) documented another Jensen fabrication. "Conway's case is . . . curious. No one . . . knows anything . . . about her . . . Jensen has given her the name Jane . . . but I can find no documentary evidence for this forename" (Hirsch, 1981, p. 31).

In sum, and as some reviews of Jensen's (1969) work (e.g., Hirsch, 1975) have noted, the directions in which people's hereditarian presuppositions may lead them reflect the bankruptcy of science when it is conducted without adequate scholarship. A situation may result wherein scholarly scrupulosity is lost in the presence of gross misrepresentations of facts (Hirsch, 1975, 1976a, 1976b; see too Goldberger & Manski, 1995). As such, in my view, the events following Kamin's (1974) presentations about Burt's study did nothing to diminish the appropriateness of Kamin's conclusions. In fact, Gillie's reports (1976, 1979, 1980) and Hearnshaw's biography (1979) buttressed Kamin's (1974) view that Burt's "evidence" was not evidence at all.

Indeed, although I will not treat the other three studies that Kamin (1974) reviewed (Juel-Nielsen, 1965; Newman, Freeman, & Holzinger, 1937; Shields, 1962) in any detail here, the data they provide—although not faked—do not provide any better evidence for high IQ heritability. Thus, Kamin (1974) closed his review of these studies with a position with which I agree:

> The four separated MZ twin studies reviewed in this chapter led Professor Jensen (1970, p. 146) to conclude: "The overall intraclass correlation . . . .824 . . . may be interpreted as an upper-bound estimate of the heritability of I.Q. in the English, Danish, and North American Caucasian populations sampled in these studies." The conclusion of our own review is vastly different. We have seen that Burt's data, reporting by far the strongest hereditarian effects, are riddled with arithmetical inconsistencies and verbal contradictions. The few descriptions of how the data were collected are mutually inconsistent, as are the descriptions of "tests" employed. The "assessments" of IQ are tainted with subjectivity. The utter failure to provide information about procedural detail can only be described as cavalier. There can be no science that accepts such data as its base.
>
> To the degree that the case for a genetic influence on IQ scores rests on the celebrated studies of separated twins, we can justifiably conclude

> that there is no reason to reject the hypothesis that IQ is simply not heritable.
>
> (pp. 66–67)

OTHER DATA IN PURPORTED SUPPORT OF THE HEREDITARIAN POSITION

Data from MZ twins reared apart may be the best evidence, but are not the only evidence, that may be used to support the hereditarian position. These less-direct potential bases of support have been reviewed by Goldberger (1980; see too Goldberger & Manski, 1995), Gould (1981, 1996), Ho (2013), Joseph (e.g., 2010, 2013, 2015), Kamin (1974, 1980), Layzer (1974, 2000), and Richardson (2017). These reviewers do not lead me to alter my position regarding the apparently minimal level of support for the hereditarian position. Nevertheless, it is useful to bolster my conclusion by briefly discussing some findings from other lines of work purporting to provide evidence for the hereditarian position. These other lines of work involve data that come from studies of families, twins reared together, twins reared apart (e.g., most notably, the Minnesota Study of Twins Reared Apart, or MISTRA; Bouchard, Lykken, McGue, Segal, & Tellegen, 1990), IQ adoption studies, and molecular genetic research.

Correlations between IQ and Amplitude and Latency of Evoked Potentials

Hans J. Eysenck (1979, 1980), whose views about the Burt "case" I noted earlier, was a strong advocate of the hereditarian position. In his 1979 book, *The Structure and Measurement of Intelligence*, Eysenck made the claim that culture-bound intelligence tests are appropriate for selection/admission of students into schools because "we're often justified in assuming considerable uniformity in cultural background among candidates" (1979, p. 23). However, Eysenck (1979) provided no support for this belief in cultural uniformity.

What he did do was describe evidence that a feature of brain functioning that, he believed, is not influenced by cultural variables is highly related to scores on culture-bound tests. The feature of brain functioning on which he focuses is "evoked cortical potentials," an index of the brain's activity in response to stimulation. Eysenck claimed that evoked cortical potentials with higher amplitude and shorter latencies indicate greater or more efficient processing of stimulus input. For instance, he would expect a high negative correlation between latency of response and IQ score, a finding that would indicate that people with higher IQs processed information more rapidly. However, Dorfman (1980) noted:

> The figures that he displays in support of a strong correlation are taken from Ertl's early work which even Eysenck admits "suffered from technical and methodological deficiencies" (p. 50). He then asserts that Shucard and Horn have also obtained "quite sizable correlations between AEP's [averaged evoked potentials] and IQ" (p. 50). In fact, those investigators reported a correlation of only 0.24 for fluid intelligence and an absence of correlation for crystallized intelligence in the article cited by Eysenck. He also presents data from "our own laboratories" collected in about 1973. No details are given and no reference is made to any relevant publications in scholarly journals.
>
> (p. 643)

In 1980, Eysenck again misrepresented the character of the literature pertaining to the correlation between IQ and evoked cortical potentials. Gould (1980) wrote a review of a book of Jensen's (*Bias in Mental Testing*, 1980), wherein Gould criticized the evidence in support of a strong biologically based general factor of intelligence. Commenting on Gould's (1980) review, Eysenck (1980) asserted:

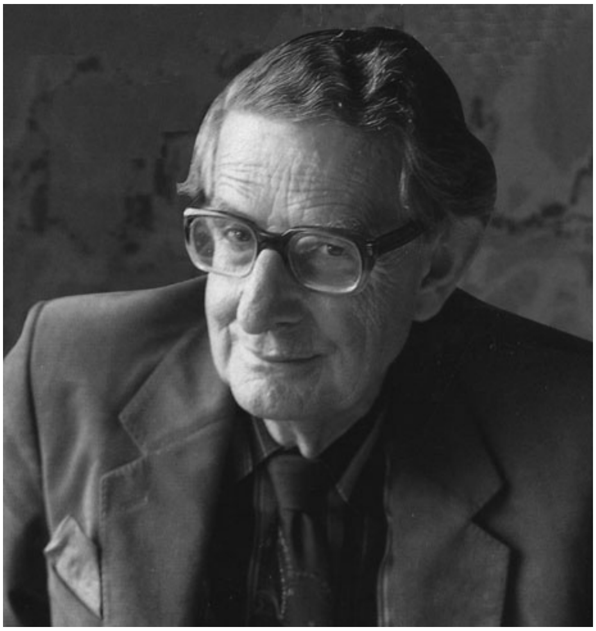
> One line of evidence which has become very important in recent years, and which supports the hypothesis of the strong general factor of intelligence, is work with non-cognitive tests, such as measures of reaction time, speed and sensory discrimination, and in particular EEG evoked potentials. None of these are tainted by cultural factors, and all are reactions to simple sensory

> stimuli, yet they all correlate highly with IQ as measured by traditional tests. In our own recent work, EEG evoked potentials show correlations with Wechsler IQ as high as does the Wechsler IQ with, say, the Binet IQ. In other words, non-cognitive tests of this kind, which aim to disclose the biological basis of intelligence, correlate as highly with IQ tests as these correlate with each other.
>
> (p. 52)

Eysenck (1980) did not cite any specific scientific literature that supported these claims.

Fortunately, however, although Eysenck (1980, p. 52) characterized Gould as "little more than an amateur in this field" and noted that being able to adequately review books on psychology requires "a degree of knowledge and expertise which outsiders simply do not possess," it was Gould who cited the relevant studies and, as such, let the data speak for themselves. Gould (1980) indicated that Eysenck (1980) contended

> that IQ would be affirmed as a measure of "intelligence" if it correlates strongly with basic, neurological reactions of the brain that cannot (so he claims) be attributed to cultural or environmental differences. These include: (1) reaction time (in which an experimenter measures how long it takes a subject to react to a stimulus—time from seeing a flashing light to pushing a button, for example); and (2) EEG evoked potentials (in which an experimenter attaches electrodes to a subject's head and records the timing and intensity of electrical responses within his brain to various stimuli). Eysenck then makes a specific claim about such studies that they correlate as highly with IQ tests as various IQ tests correlate with each other. Ignoramus that I am, I dare not venture into this area of psychological professionalism. So let me, instead, simply cite the contrary opinions of an expert namely, Arthur Jensen. In *Bias in Mental Testing*, the book that I reviewed, Jensen summarizes [p. 314] many studies on the correlation of Wechsler IQ with Binet IQ, the two tests that Eysenck chooses as his standard. Average correlation is 0.77 for Binet with the Wechsler adult scale (WAIS) and 0.80 for Binet with WISC (Wechsler Intelligence Scale for Children). These high correlations are scarcely surprising since all these tests use similar material, and are constructed with the same end in mind.
>
> On the correlation of IQ with reaction time (an area of Jensen's primary research), Jensen writes [p. 691]: "Neither I nor anyone else, to my knowledge, has been able to get correlations larger than about –0.4 to –0.5 between choice RT [reaction time] and IQ, with typical correlations in the –0.3 to –0.4 range, using reasonable-sized samples." (The correlations are appropriately negative because short reaction time is supposed to accompany more intelligence. But they are much lower than the .77 Wechsler–Binet correlation, not equal to it as Eysenck claims. The correlation coefficient, by the way, is a peculiar statistic with a highly asymmetrical distribution that compresses differences at the upper end and magnifies them at the lower end. Thus, a correlation of 0.4 is not "half as good" as one of 0.8, but substantially less intense.)
>
> Jensen is even more dubious about the literature on evoked potential, for he writes [p. 709]:


Hans Eysenck

> "The AEP average evoked potential and IQ research picture soon becomes a thicket of seemingly inconsistent and confusing findings, confounded variables, methodological differences, statistically questionable conclusions, unbridled theoretical speculation, and, not surprisingly, considerable controversy." The only correlations he cites between IQ tests and evoked potentials average –0.28 with none higher than –0.35 (again, appropriately negative since less time between a stimulus and responding brain waves supposedly records more intelligence—but again vastly less than the Wechsler–Binet correlation, not equal to it). The –0.28 may be statistically "significant," but vernacular and statistical meanings of the word "significant" are quite unrelated. A statistically "significant" correlation is not necessarily a strong one, but only one that can be adequately discriminated from a value of zero, or no correlation.
>
> The relevant measure, in this case, is the coefficient of determination, or r^2 (the correlation coefficient times itself). An r of –0.28 means that variation in evoked potential accounts for a whopping 8 percent (–0.28, or 0.0784) of the variation in measured IQ! Jensen then casts further doubt upon the literature of evoked potentials [p. 709]: "Visual and auditory AEPs seem to yield quite different, even contrary results, visual latencies usually being negatively correlated with IQ, and auditory latencies being positively correlated. The directions of correlations also seem to flip-flop according to whether the IQs of the sample involved in the study are distributed mostly in the below-average range or mostly in the above-average range of IQs."
>
> But even if the correlations were as high as Eysenck claimed, what would it mean? It wouldn't validate a notion of inborn general intelligence. Who can say that childhood nutrition (both gastronomical and educational) does not affect the growing brain and induce variation equally recorded by reaction time and performance on mental tests?
>
> (pp. 52–53)

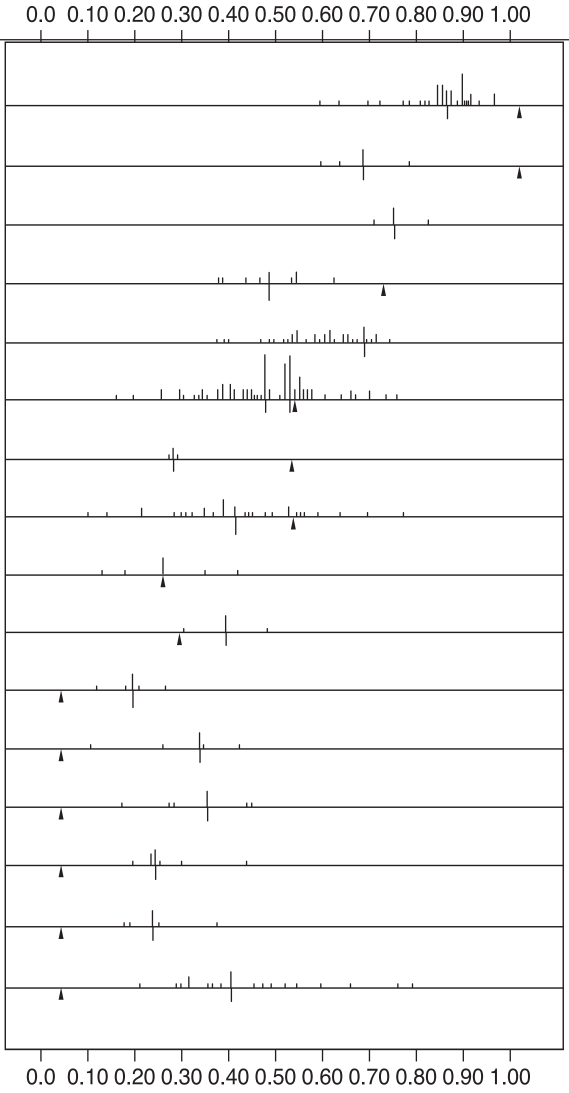
In sum, the contention that some measure of cortical activity provides an index of biological functioning "untainted" by cultural factors is not a sound one. In turn, the purported evidence that measures of evoked cortical potentials are highly correlated with measures of IQ is as illusory as Burt's data about IQ resemblance among MZ twins reared apart. Neither Burt's study (1955, 1958, 1966; Conway, 1958) nor Eysenck's assertions (1979, 1980) provide adequate scientific data to support the hereditarian position.

Reviews of Familial Studies of Intelligence after the Burt "Scandal"

Both before and after the Burt "scandal" surfaced, several summaries were published of the world literature on IQ correlations between relatives. For instance, Erlenmeyer-Kimling and Jarvik (1963) published such a review that, unfortunately, included Burt's reports on MZ twins reared apart as well as his reports on the correlations found for people of other types of relationship (e.g., uncle–nephew correlations). Later reviews were published by Roubertoux and Carlier (1978), Plomin and DeFries (1980), Bouchard and McGue (1981), Herrnstein and Murray (1994), and Rushton (1999). The review by Bouchard and McGue (1981) is comprehensive and was also subjected to peer review prior to its publication in the prestigious scientific journal, *Science*. Thus, to evaluate the quality of the evidence for the genetic determination of intelligence, it is useful to consider this review in some detail.

The results of the Bouchard and McGue (1981) review are presented in Table 11.4. The table summarizes their analysis of 111 studies that reported family resemblances in intelligence. Across these studies there were 526 familial correlations based on 113,942 pairings. Table 11.4 summarizes these correlations between both biological and adoptive relatives by using a vertical bar to indicate the median correlation for each type of relationship (e.g., MZ twins reared together, MZ twins reared apart, etc.). In addition, a small arrow is used to indicate the correlation by a simple genetic model of no dominance, no assortative mating, and no environmental effects. The reason Bouchard and McGue (1981, p. 1055) displayed the predictions derived

Table 11.4 Familial correlations for IQ



	No. of correlations	No. of pairings	Median correlation	Weighted average	x^2 (d.f.)	x^2 d.f.
Monozygotic twins reared together	34	4,672	0.85	0.86	81.29(33)	2.41
Monozygotic twins reared apart	3	65	0.67	0.72	0.92(2)	0.41
Midparent–midoffspring reared together	8	410	0.73	0.72	2.66(2)	1.3
Midparent–offspring reared together	8	992	0.475	0.50	8.11(7)	1.1
Dizygotic twins reared together	41	5,546	0.58	0.60	94.5(40)	2.31
Siblings reared together	69	26.473	0.45	0.47	403.6(64)	6.3
Siblings reared apart	2	203	0.24	0.24	.02(1)	0.02
Single parent–offspring reared together	32	8,133	0.385	0.42	211.0(1)	6.8
Single parent–offspring reared apart	4	814	0.22	0.22	9.61(3)	3.20
Half-siblings	2	200	0.35	0.31	1.55(1)	1.5
Cousins	4	1,176	0.145	0.15	1.02(2)	0.5
Nonbiological sibling pairs (adopted/natural pairings)	5	345	0.29	0.29	1.93(4)	0.41
Nonbiolgical sibling pairs (adopted/adopted pairings)	6	369	0.31	0.34	10.5(5)	2.1
Adopting midparent–offspring	6	758	0.19	0.24	6.8(5)	1.3
Adopting parent–offspring	6	1,397	0.18	0.19	6.64(5)	1.3
Assortative mating	16	3,817	0.365	0.33	96.1(15)	6.4

Note. The vertical bar in each distribution indicates the median correlation; the arrow, the correlation predicted by a simple polygenic model.

Source: Bouchard and McGue (1981, p. 1056).

from such a model is that such a model "provides a noncontroversial pattern against which to compare the results of various familial groupings. Different investigators will undoubtedly fit different models to the data."

The reason it is important to present a statistical model of the inheritance of intelligence is that, without the use of a model, a researcher cannot appraise the extent to which a given set of correlations supports a hereditarian view of intelligence. In other words, a statistical model serves as a template; it specifies the nature of the family correlations that would occur if genetic similarity alone accounted for any observed correlations. Obtained correlations are then compared to what is predicted in the model, and to the extent that the model is useful the empirical findings will match the predictions made by the model. Thus, to support a hereditarian position, it is crucial that the model be a conceptually and technically (i.e., mathematically) appropriate means to portray the role of genes in human intelligence.

To anticipate my forthcoming discussion, the inadequacy of such models is the fatal flaw with using family resemblance data, such as in Table 11.4. However, I should note here that Bouchard and McGue (1981) interpreted the pattern of average correlations displayed in Table 11.4 as being consistent with what would be predicted with a model of "polygenic inheritance. That is, the higher the proportion of genes two family members have in common, the higher the average correlation between their IQs" (p. 1055). But, as Bouchard and McGue (1981) pointed out, there are many features of the information presented in Table 11.4 that diminish the strength of their interpretation. First, "the individual data points are quite heterogeneous" (p. 1055) within any category of family resemblance. This variation means that for a group of relatives of a common type of relationship (e.g., MZs reared together or MZs reared apart), their invariant degree of genetic resemblance is not invariantly related to a given degree of IQ resemblance. Although some of this variability may be produced by differences across studies in the tests used to assess intelligence, Bouchard and McGue (1981, p. 1056) reported, "We do not have sufficient data to determine whether the magnitude of the familial correlation is moderated by the specific test used."

Thus, it remains quite plausible that, within a given category of genetic resemblance, environmental coactions (gene↔environment relations) moderate levels of IQ resemblance. In addition, I should note that the heterogeneity of "within-genetic-resemblance category family member" correlations presents a formidable problem for those who seek to model genetic influences on human intelligence through the use of such data (Goldberger, 1979).

There are additional features of the information in Table 11.4 that diminish the power of a genetic model and that, in turn, suggest the contribution of the environment or, perhaps better, of gene↔environment coactions, that is, "not *genes plus environment*, but rather *genes dynamically interacting* [coacting] *with the environment*" (Moore & Shenk, 2016, p. 5). For instance, Bouchard and McGue (1981) pointed out that DZ twins of the same sex have more similar IQs than do DZ twins of the opposite sex. They saw this as possibly reflecting a social effect wherein same-sex DZ twins are treated more similarly by their parents than are opposite-sex DZ twins.

In addition, Bouchard and McGue (1981) noted that the weighted average correlation for 34 correlations reported on 4,672 MZs *reared together* was 0.86, and that 79% of the 34 correlations were above 0.80. They concluded that such a finding "convincingly demonstrates the remarkable similarity of monozygotic twins" (Bouchard & McGue, 1981, p. 1057). However, it is important to point out that this interpretation conflates the concept of correlation (which means that two sets of scores are related) with the concept of similarity (which means that two entities appear alike; Joseph, 2015). For example, height and weight are two correlated but distinct (dissimilar) physical attributes of people; in turn, the color orange that is seen on a crayon may appear to be similar to the color orange that is seen on the skin of a salamander, but there is no correlation between being a crayon and being a salamander. Moreover, the findings discussed by Bouchard and McGue are misinterpreted in any case. There findings are for MZs reared in the same environment. Not only do these twins share the same genotype but they are also of the same sex (and recall that DZs of the same sex have higher IQ resemblances than do opposite-sex DZs) and are born at the same time. Moreover, the weighted average correlation for MZs reported to have been reared apart (see Joseph, 2015, for a critique of the assertion that these individuals were in fact fully reared apart) is 0.72; Bouchard and McGue (1981, p. 1056) recognized that this value "is much less than that found for the monozygotic twins reared together, the differences suggesting the importance of between-family environmental differences." However, Bouchard and McGue (1981, p. 1056) said that "at the same time, the magnitude of this correlation would be difficult to explain on the basis of any strictly environmental hypothesis." However, Joseph (2015) notes that the reared-apart twin studies discussed by Bouchard and McGue (1981) were the very problematic ones reviewed by Kamin (1974), Taylor (1980), and Farber (1981). These reviews indicate the error in the Bouchard and McGue assertion that there can be no plausible environmental moderator of the findings of these studies. For instance, Joseph (2015) explains that reared-apart MZ pairs share many environmental similarities, including key cohort effects.

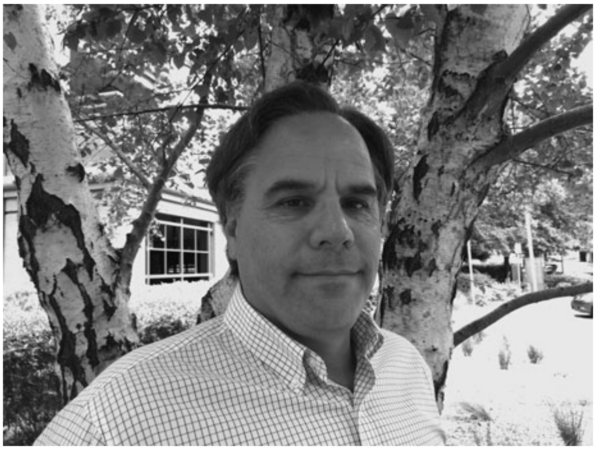
Of course, relational developmental systems (RDS)-based theorists (e.g., Fischer & Bidell, 2006; Gottlieb, 1997; Lickliter, 2016; Magnusson, 1999a, 1999b; Mascolo, 2013; Mascolo & Fischer, 2015; Overton, 2015a; Thelen & Smith, 2006; Witherington & Lickliter, 2016) would agree about the uselessness of a *strictly* environmental hypothesis. In turn, I would assume that, except for the most ardent genetic reductionists, most supporters of the hereditarian position would not dispute the uselessness of a *strictly* genetic hypothesis (e.g., see Plomin, 2000). Nevertheless, I disagree with Bouchard and McGue's (1981) implication that the weighted average of 0.72 for IQ resemblance among separated MZs provides good support for the hereditarian position.

There are two reasons for my disagreement. First, Bouchard and McGue (1981) obtained the weighted correlation of 0.72 from the reports of three separate investigations. That is, Burt's "data" were deleted and they were left with test results from three other studies on 65 pairs of MZs reared apart. Were these the three studies summarized by Kamin (1974) and noted in Table 11.1 of this chapter? Bouchard and McGue (1981) did not indicate in their review the particular three studies they used to identify the 65 cases and obtain the weighted correlation of 0.72. The three studies (other than Burt's) noted in Table 11.1 of this chapter (Juel-Nielsen, 1965; Newman, Freeman, & Holzinger, 1937; Shields, 1962) have a combined sample of 68. It may be that all or some of the data from one or all of these three studies were part of the data of the unknown three studies summarized by Bouchard and McGue (1981). Unfortunately, their review does not provide the information needed to know if this source for the summary is the case. Given the inadequate scientific characteristics (Kamin, 1974) of the studies by Juel-Nielsen (1965), Newman, Freeman, and Holzinger (1937), and Shields (1962), however, little confidence can be placed in the accuracy of the weighted correlation of 0.72 if these studies in any way contributed to the weighted correlation. In turn, given the methodological problems of studies of MZs reared apart (Kamin, 1974), developmental scientists should in any event be particularly cautious about relying on such studies unless all their research procedures are fairly and openly evaluated; such evaluation was not part of the report by Bouchard and McGue (1981; e.g., see footnote 8, pp. 1058–1059).[2]

Indeed, the particular methodological problems of studies of MZ twins reared apart are the second reason for my disagreement with Bouchard and McGue (1981). These problems have been, of course, discussed by Kamin (1974), Taylor (1980), Farber (1981), Gruber (1981), Joseph (e.g., 2010, 2013, 2015), and Richardson (2017). For instance, Farber (1981) reanalyzed all the major studies of MZs reared apart, excluded the doubtful cases (e.g., those of Burt) according to clear and objective criteria, and then attempted to evaluate the information provided by the remaining sample of 95 MZs reared apart. Farber (1981) noted that, across the several studies she reviewed, about 90% of the twin pairs were selected for inclusion in the study on the basis of known similarities between them. In other words, MZs who were not similar (along dimensions of interest to the researcher) were systematically and at times intentionally excluded from the study. Farber estimated that currently there are about 600 pairs of MZ twins being reared apart in the United States alone. Thus, the technique of a priori systematic exclusion of dissimilar separated

MZs is not only incongruent with an unbiased and objective scientific analysis but also the sample so obtained cannot be representative of the larger population of separated MZs that seems to exist. That is, the group that has been studied is probably only a small proportion of the population of separated MZs that exists (i.e., those who were sufficiently similar); the level of correspondence seen with these inappropriately sampled (and, typically, inappropriately assessed; Kamin, 1974) pairs overestimates any actual similarity that exists in this population in general. Simply put, researchers seeking similarity excluded those pairs who did not have it and then, when assessing the remaining pairs for similarity, they "discovered" it, often on the basis of relying on the accuracy of twins' accounts of their separation (Joseph, 2015).

More recently (at this writing), Joseph (2015) analyzed the findings about resemblance on all reared-apart identical twin pairs in the three studies listed in Table 11.1 after the Burt "study" is removed. Over a series of three tables listing all twin pairs studied in the Newman, Shields, and Juel-Nielsen studies, Joseph showed that most of these MZ pairs were reared together for at least some portion of their lives and, as well, that they had regular and, in some cases, frequent contact with each other. Some of the twin pairs shared a close emotional bond as well (Joseph, 2015). Thus, most of these MZ pairs were, in actuality, only partially reared apart.



Jay Joseph

In addition, Joseph (2015) assessed the research of Bouchard and his colleagues. Joseph indicated that this work has failed to provide scientifically sound evidence in support of the hereditarian position regarding IQ and, as well, personality. For instance, Joseph discussed the importance of cohort effects influencing the behavioral similarity of MZs reared apart, and he made the point that the MISTRA researchers suppressed their IQ correlations for DZs reared apart, which have never been published. Pointing to the incomplete DZ reared-apart correlations that were mentioned by Segal (2012), Joseph (2015) indicated that the MZ reared-apart correlations in MISTRA are not significantly higher than DZ reared-apart correlations. Joseph (2015) noted that these findings alone invalidate genetic interpretations of the MISTRA IQ data. Joseph (2015) also notes that his conclusions about the absence of evidence for genetic effects on IQ are consistent with findings at the molecular genetic level (see too Richardson, 2017, for additional evidence of this correspondence).

Findings from Molecular Genetics Research

Geneticist and evolutionary biologist Mae-Wan Ho (e.g., 2010, 2013) reviewed findings pertinent to the genetic level of analysis (see too Charney, 2012, 2013, 2016). Ho (2013) reports the results of several genome-wide association studies (GWAS). These studies:

> involve rapidly scanning markers across the complete genomes of many people to find associations of genetic variants to particular diseases or traits. Typically, thousands or tens of thousands of individuals are scanned, simultaneously for up to 550,000 single nucleotide polymorphisms (SNPs)—common differences in single nucleotides at specific sites across the human genome with frequencies >5%—using DNA microarrays (chips).
>
> (Ho, 2013, p. 70)

Ho (2013, p. 70) notes also that data derived from GWAS

> have identified approximately 2000 genetic variants associated with 165 common diseases and traits; but these variants appear to explain only a tiny fraction of the heritability in most cases . . . (p. 70)
>
> A GWAS on 7000 subjects published in 2008 found only six genetic markers (SNPs, single-nucleotide polymorphisms) associated with cognitive ability, and only one of those remained statistically significant on further tests. Together, the six markers explained barely 1% of the variance in general cognitive ability (Butcher, Davis, Craig, & Plomin, 2008). Recently, the association between 12 specific SNPs and "general intelligence" factor *g* was put to test in an attempt to replicate the associations found in earlier studies, but only one SNP remained significant. The researchers conclude that (Chabris et al., 2012) "most reported genetic associations with general intelligence are probably false positives."
>
> (Ho, 2013, p. 76)

In addition to the presence of facts from GWAS that invalidate the hereditarian view of the genetic basis of intelligence (see too Joseph, 2015; Richardson, 2017), Ho (2013) added more information from genetics research that contradicts the core assumption of the twin studies she noted:

> The fundamental assumption of twin studies—that monozygotic twins share 100% of their genes—is demonstrably false. MZ twins differ, to begin with, in the mitochondrial DNA (mtDNA) complement allocated in cell division of the original oocyte that generated the twins. The oocyte may have had different sets of mtDNA, a condition referred to as heteroplasmy. MZ twins diverge substantially in epigenetic modifications as well as retrotransposition, copy number variations, and aneuploidy throughout life. Although the numerous processes that alter genomes occur in normal development, perhaps as part of "natural genetic engineering" (Ho, 1998, 2003), the same processes are known to be involved in many behavioral, psychiatric, and neurodegenerative diseases, leaving us in no doubt that they have phenotypic consequences (Charney, 2012).
>
> In addition, stochastic nonlinear developmental changes account for substantial divergence in the activities of different brain regions between twins (Molenaar, Smit, Boomsma, & Nesselroade, 2012).
>
> To summarize, no genes for intelligence can be found in the human genome. Instead, common environments, including maternal and rearing environments, along with epigenetic and cultural inheritance create substantial correlations between genetically unrelated individuals, while even "identical twins" diverge genetically and epigenetically throughout life.
>
> (Ho, 2013, p. 82)

Interestingly, Bouchard seems to agree with the conclusion of Ho (2013). He notes that, "In spite of numerous studies with considerable statistical power, we know of very few genes that influence *g* and the effects are very small" (Bouchard, 2014, p. 549).

Mae-wan Ho

Other Problems with Twin Research

Joseph (2013, 2015) also finds fundamental flaws with other key ideas of the twin method. The *equal environments assumption* (EEA) has been used by twin researchers to support the argument that greater IQ resemblance of MZs than of DZs must be due to greater genetic similarity of the former types of twin pairs (Richardson & Norgate, 2005). This argument is predicated on the EEA because it is asserted that, through this assumption, MZs and DZs grow up in essentially equivalent environments. If this assumption is held to be the case, then, with equal environments, the only source of the variance in IQ scores between these two types of twin pairs must be attributed to genes. Joseph (2013) notes that modern twin researchers now concede the point that MZ pairs grow up experiencing much more similar environments than experienced by DZ pairs, but continue to uphold the validity of the EEA and the twin method on the basis of illogical arguments. Joseph (2013) explained that:

> Remarkably, since the 1960s most leading twin researchers have conceded the point that identicals experience more similar environments than fraternals. However, instead of relegating the twin method to a place alongside other discarded pseudosciences, twin researchers have attempted to validate the twin method by subtly redefining the equal environment assumption (EEA). The main way they have done this has been to argue that although identicals do indeed experience more similar environments than fraternals, identical pairs "create" or "elicit" more similar environments for themselves because they are more similar genetically. However, this "twins create their own environment" argument is a circular one, because twin researchers' *conclusion* that identical pairs behave more similarly because they are more similar genetically is based on the *assumption* that identical pairs behave more similarly because they are more similar genetically. This means that twin researchers' position that genetic factors explain the greater behavioral resemblance of identical twin pairs is, illogically, both a conclusion *and a premise* of the twin method. In defending the validity of the twin method, modern twin researchers refer to the premise in support of the conclusion, and then refer back to the conclusion in support of the premise, in a continuously circular loop of faulty reasoning.

In short, Joseph (e.g., 2010, 2013, 2015) significantly extended past scholarship pertinent to the limitations of the twin method (e.g., in his 2015 book, *The Trouble with Twin Studies: A Reassessment of Twin Research in the Social and Behavioral Sciences*) and, in so doing, has elucidated major conceptual and empirical shortcomings of the claims that, through this method, the hereditarian argument about the genetic basis of IQ is advanced.

With these critiques of the twin method and its assumptions in hand, it is useful to return to the Bouchard and McGue (1981) publication and note that they seemed to recognize some of the problems with twin research but, apparently, only for DZ twins. They noted, "The greater similarity of dizygotic twins than of other siblings is most often interpreted as a reflection of greater environmental similarity. It is also likely that bias in the recruitment of dizygotic twins for study is in the direction of increasing psychological similarity" (Bouchard & McGue, 1981, p. 1056). However, a similar recruitment bias may occur with MZs reared apart, and may account for the resemblance in their IQ scores. Bouchard and McGue (1981) failed to indicate this possibility, despite the evidence that existed in support of it at the time of their writing (e.g., Farber, 1981).

For example, Farber (1981) also reported that, even with the separated MZs studied, there is evidence for the ubiquitous role of environmental influences. She noted that greater separation between the MZs tended to be linearly related to greater disparities in their IQ scores. In turn, most separated MZs come from lower-class and lower-middle-class families and are quite often adopted by families of similar socioeconomic standing or by relatives; thus, although separated, the MZs have similar environments. As a consequence, "The range of environments in which the twins develop is thus reduced, and as the geneticist Theodosius Dobzhansky pointed out, reducing the variability of the environment increases estimates of heritability" (Gruber, 1981, p. 22). Thus, as Kamin (in Eysenck & Kamin, 1981b) pointed out, in such a case of comparable rearing environments, the increased estimate of the inheritance of intelligence is an artifact and the score resemblance could be determined as much by environment as by genes.

Other information found in Table 11.4 argues against the hereditarian position. For instance, Bouchard and McGue (1981) noted that DZs correlate higher on IQ than other biological siblings; this difference suggests not only that the DZs' common birth puts them into a socializing environment at the same time, but also that this context may treat them similarly. Bouchard and McGue also pointed out that the IQs of adoptive parents were also consistently related to the IQs of the adopted children; this relation suggests that environmental similarity can lead to some IQ resemblance between people. Thus, Bouchard and McGue (1981) concluded that "the data clearly suggest the operation of environmental effects" (p. 1058).

In turn, they concluded that it is "indisputable" that "the data support the influence of partial genetic determination for IQ" (Bouchard & McGue, 1981, p. 1058). Of course, genes are part of the integrated, relational developmental system (e.g., Ford & Lerner, 1992; Gottlieb, 1997; Horowitz, 2000; Magnusson, 1999a, 1999b; Thelen & Smith, 2006) and, as such, there is no reason to dispute the broad claim that genes are involved in providing some material basis for such human functioning. Indeed, both in my discussion of Anastasi's (1958) position in Chapter 6 and throughout my discussion of RDS-based theories across prior chapters, I have argued that all human behavior derives from (the fusion of) genes and context. However, I have argued that the data reviewed by Bouchard and McGue (1981) provide far less scientifically useful and/or certain support for a strong hereditarian view of the bases of IQ than may be taken from initial inspection of Table 11.4. Indeed, the last statements made by Bouchard and McGue (1981) seem to support my conclusion. First, they noted that it is "dubious" (p. 1058) whether the data are informative about the precise strengths of the genetic effect. Second, they pointed out that "the large amount of unexplained variability within degrees of relationship, while not precluding attempts to model the data, suggests that such models should be interpreted cautiously" (Bouchard & McGue, 1981, p. 1058).

Continuing Misuses of the Heritability Concept in the Discussion of Race Differences in Intelligence

Despite the cautions forwarded by Bouchard and McGue (1981) about the difficulty of modeling the heritability of intelligence, in the decades following their work other hereditarians either ignored their warnings or misunderstood the problems involved in this area of research. For example, Rushton (1999) demonstrated that he either completely misunderstood the meaning of heritability, or simply used the concept in a scientifically inaccurate way. He asserted that:

> Heritability is the amount of variation in a trait due to the genes. A heritability of 1.00 means that the differences are inborn and the environment has no effect. A heritability of zero (0.00) means that the trait is controlled by the environment and not at all by the genes. A heritability of .50 means that the differences come from both the genes and the environment.
>
> (p. 60)

However, behavior geneticists such as Plomin (1986, 2000) and Plomin and colleagues (1980) explained the meaning of heritability in a manner that indicates that Rushton's (1999) interpretation of the concept is egregiously incorrect. Indeed, for more than 60 years, behavior geneticists have understood that, whatever heritability represents, it is not a construct that represents what Rushton (1999) claims.

For instance, the behavior geneticist J. C. DeFries (1967, p. 324) explained that Lush (1940) first defined the term "heritability" "as the fraction of the observed variance which was caused by differences in heredity" (see too Moore & Shenk, 2016, regarding the work of J. L. Lush, e.g., Lush, 1937). DeFries (1967) continued:

> This fraction is a statistic describing a particular population. It can be made larger or smaller if either the numerator or the other ingredients in the denominator can be altered. Thus, it may vary from population to population for the same characteristic and may vary from one characteristic to another even in the same population.
>
> (p. 324)

Thus, it was explicitly stated, more than a half-century prior to Rushton's (1999) claims, that heritability involves the variance associated with both a specific attribute that is being measured and a specific population within which the attribute is measured, a point that has been emphasized by other researchers studying behavior genetics (Hirsch, 1963; Hadler, 1964; Moore & Shenk, 2016; Richardson, 2017).

Given that Rushton's (1999) definition departs so significantly from the one used by the behavior geneticists on whose scientific authority he so heavily relies, one may wonder whether he either failed to consult these hereditarian authors about the correct technical interpretation of heritability, or opted to define this concept in a manner which gives it a significance that fits his theory but is without conceptual or empirical foundation. Luca Cavalli-Sforza, professor in the Department of Genetics at Stanford University, raised a similar question in regard to the sort of "scholarship" represented by the work of Rushton. Cavalli-Sforza and Cavalli-Sforza (1995), in a paper that reviews the research base for statements about the (high) heritability of IQ, noted that:

> Researchers who might be called "IQ hereditarians" are in general reporting high heritabilities for IQ without any information on how these calculations have been obtained, or why the other papers here cited have been ignored. It is unlikely that they were not seen or read; they are published in well-known scientific journals . . . it is possible that the IQ enthusiasts who do not cite these seminal papers are aware of them but did not understand them. This is not as incriminating as the possibility that they did not cite them because they are at odds with their own strongly hereditarian conclusions.
>
> (p. 280)

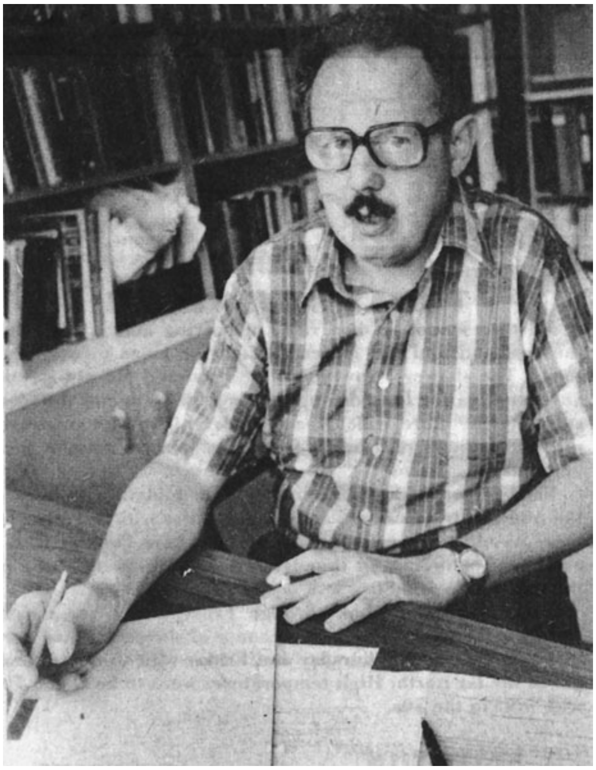
Indeed, Cavalli-Sforza and Cavalli-Sforza (1995) noted that the literature ignored by Rushton (1999) and others (e.g., Herrnstein & Murray, 1994; Jensen, 1969, 1998), including the work of Cavalli-Sforza, found much lower heritability scores than those reported by the "IQ enthusiasts" (Cavalli-Sforza & Cavalli-Sforza, 1995; Hirsch, 1997). Nevertheless, Rushton (1999, p. 69) contended that "Arthur Jensen's 1998 book, *The g Factor*, shows that indeed race difference [sic] are higher on high heritability tests. Such tests show greater White–Black differences even for toddlers." The presence of such a claim, in the face of the widely available evidence against it, provides no assurance that the assertion is based on a complete and even-handed review and analysis of the published literature (cf. Hirsch, 1997).

These problems exist not only with Rushton (1999) but also, as Hirsch (1997, pp. 215–220) documented in precise detail, with the book, *The Bell Curve*, by Herrnstein and Murray (1994), a publication that Hirsch saw as lacking any scientific scrupulosity. To illustrate, Hirsch (1997) pointed out that Herrnstein and Murray (1994, p. 628) contended that there is a *unanimous* conclusion among scholars that "no bias against blacks in educational and occupational prediction has been found." Hirsch noted that Herrnstein and Murray cite Hartigan and Wigdor (1989) as an example of this consensus. However, when Hirsch (1997, p. 219) read this reference he found that Hartigan and Wigdor (1989) reached the entirely opposite conclusion! Hirsch (1997) indicated that their

> Analysis of the impact of selection on minority and nonminority applicants demonstrates that in the absence of score adjustments, minority applicants who could perform successfully on the job will be screened out of the referral group in greater proportions than are equivalent majority applicants. Conversely, majority applicants who turn out not to perform successfully will be included in the referral group in greater proportions than equivalent minority applicants.
>
> (p. 7)

Goldberger and Manski (1995) pointed also to the several conceptual and methodological flaws in the Herrnstein and Murray (1994) book. For instance, Goldberger and Manski (1995) noted that the entire line of argument presented in the book "is tainted by two misconceptions. Heritability is not a measure of parent–child resemblance in IQ, nor is it a biological parameter that sets limits on the effectiveness of policy" (p. 764).

Moreover, Goldberger and Manski (1995) pointed to several fatal flaws in the scientific methods used by Herrnstein and Murray (1994) to support their misconceived argument. To support their ideas about the significance of the heritability of IQ scores for understanding human development, and thus about the genetic basis of intelligence, Herrnstein and Murray (1994) reported analyses from the National Longitudinal Survey of Youth (NLSY) pertinent to trying to determine whether socioeconomic status (SES) or IQ test scores (indexed by the Armed Forces Qualifying Test, AFQT), measured in 1980, are "more important" as determinants of various social behaviors, measured in the NLSY in 1990. Goldberger and Manski indicated several problems with using AFQT scores to measure cognitive ability and SES to index a child's social environment. In addition, Goldberg and Manski (1995) noted that Herrnstein and Murray measure AFQT and SES scores in qualitatively different units and that the statistical procedure used to address this problem (i.e., standardization procedures using statistics termed "beta weights") "accomplishes nothing except to give quantities in noncomparable units the superficial appearance of being in comparable units. This accomplishment is worse than useless—it yields misleading inferences" (p. 769). Accordingly, Goldberger and Manski (1995) concluded that: "We find no substantively meaningful way to interpret the empirical analyses in Part II of *The Bell Curve* as showing that IQ is 'more important' than SES as a determinant of social behaviors" (p. 769).



Arthur Goldberger

Thus, whether one is reading Herrnstein and Murray (1994), Jensen (1969, 1998), or Rushton (1996, 1997, 1999, 2000) about the heritability of IQ and the inherited racial differences in intelligence that are purported to be reflected by such "information," one must be mindful of the problems with their presentations identified by scholars such as Hirsch (1997), Cavalli-Sforza (Cavalli-Sforza & Cavalli-Sforza, 1995), and others (e.g., Kamin, 1974; Lewontin, 2000). In fact, it may be useful and quite appropriate to heed the strong caution explicit in the comment of Wahlsten (1981) that:

> So widespread are errors in this literature that a critical reader now has good reason to doubt every article published on this topic and to check the arithmetic, algebra and original references before seriously considering the "findings" and conclusions. The pitifully low standards of scholarship of many who write on heredity and IQ are scandalous and unforgivable.
>
> (p. 33)

Although Wahlsten presented this view more than three decades ago, the works of Hirsch (1997), Joseph (2015), and Richardson (2017) remind us that Wahlsten's observations are still, alarmingly, true at the time of this writing.

Conclusions

The egregiously poor scientific work associated with the conduct of heritability research in regard to human intelligence involves unreliable and invalid uses of the concept. In addition, technical problems exist in the computation of the heritability statistic itself. In regard to these technical problems, I noted earlier that, in order to use data such as those presented in Table 11.4 to determine the extent to which variation in IQ scores is related to variation in genes, some mathematical model of the role of genes and of environment has to be formulated. Whereas the result of the application of such a model with a given set of data (e.g., those in Table 11.4) enables a heritability coefficient to be computed in regard to data such as those reviewed by Bouchard and McGue (1981), it is in such computation that major conceptual and technical (mathematical) complications arise.

THE CALCULATION OF THE HERITABILITY OF INTELLIGENCE: MODELS AND MISTAKES

Since the publication of Jensen's (1969) article in the *Harvard Educational Review*, there have been repeated discussions in the literature about the concept of heritability, about the applicability of the concept to assessments of the genetic contribution to human intelligence, and about the scientific usefulness of the mathematical models used to estimate heritability (e.g., Eysenck & Kamin, 1981a, 1981b; Feldman & Lewontin, 1975, 1976; Frankel, 1976; Goldberger, 1979; Gottlieb et al., 1998; Gould, 1980, 1981, 1996; Havender, 1976; Hirsch, 1970, 1981, 1997; Ho, 2010, 2013; Jensen, 1973, 1976, 1980; Kamin, 1974; Layzer, 1974; Molenaar, 2010; Molenaar et al., 2012; Moore, 2002, 2006, 2013; Moore & Shenk, 2016; Richardson, 2010, 2017; Richardson & Norgate, 2006). As underscored by the Moore and Shenk (2016) epigraph presented earlier in this chapter, these discussions highlight the points that the concept of heritability is a complex one, and that the concept is not necessarily what it seems to be (i.e., it is *not* an index of the extent to which heredity is a basis of any characteristic *within* a given person; Hirsch, 1997). Thus, as Moore and Shenk (2016) explain:

> *there are simply no such things as gene-only influences*. Our DNA, we now know, does not contain specific blueprint-like instructions about traits; rather, DNA segments merely contribute to the production of different kinds of RNA molecules. These RNA molecules can, in turn, regulate other DNA segments, or contribute to the production of proteins that are constituent parts of cells, cells that are assembled into systems that manifest identifiable traits. This entire process takes place in a developmental context; DNA produces its products under the influence of signals from the environment, as well as from other DNA segments (which are in turn signaled by the environment and other DNA segments, and so on). Rather than spitting out pre-determined creations from programmed instructions, "genes" are more like customized switches that get turned on and off by particular developmental circumstances. Traits are *always* a consequence of this interactive dynamic. After determining that intelligence among poor families was nearly zero-percent "heritable," Eric Turkheimer and colleagues [2003] wrote: "These findings suggest that a model of [genes plus environment] is too simple for the dynamic interaction of genes and real world environments during development."
>
> Patrick Bateson [2001] has echoed this sentiment, stating that heritability studies make "the extraordinary assumption that genetic and environmental influences are independent of one another and do not interact. That assumption is clearly wrong."
>
> (pp. 4–5)

In addition, estimates of heritability are not always formulated appropriately. This problem may be especially the case in regard to human intelligence, as Ken Richardson (2017) makes clear in his book, *Genes, Brains, and Human Potential: The Science and Ideology of Human Intelligence*. Richardson (2017) notes that:

> The more important truth, though, is that even a perfectly accurate heritability estimate of human

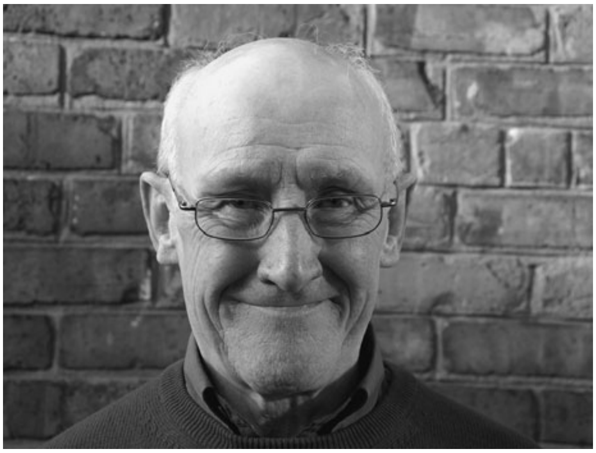
> potential or intelligence—which we certainly do not have—tells us nothing about the "genetic makeup" of individuals or sub-groups reared in different environments. To suggest otherwise is an elementary falsehood . . . Outside of agricultural breeding programs, where environment is carefully controlled, it is a completely pointless statistic—except, that is, for its ideological thrust.
>
> (p. 13)

He goes on to explain that the history of hereditarian research attempting to use heritability analysis to demonstrate the genetic basis of intelligence

> is now littered with unsuccessful attempts to identify such genes. In project after expensive project, no associations have materialized, or a marginal "finding" has not been replicated in repeat investigations. To date, no gene or SNP has been reliably associated with the normal range of human cognitive ability (let alone cause differences in it).
>
> (Richardson, 2017, p. 19)

Earlier in this chapter, I noted that Ho (2013) provides a comparable description of this history of research. However, Richardson (2017) adds that:

> Studies get bigger and bigger, dozens of them combining into ever more-expensive consortia, and papers get published with indecent haste in a few journals. But still they come up with null or miniscule results, with correlations interpreted as causes, almost invariably not replicated in follow up studies.
>
> Even those firmly of the faith have been expressing their disappointments. Erik Turkheimer was prominent among those trumpeting the forthcoming revolution in the discovery of genes for IQ. More recently he has realized how "to the great surprise of almost everyone, the molecular genetic project has foundered on the . . . shoals of developmental complexity" (Turkheimer, 2011). And in 2015 he concludes that, "Scientists have not identified a single gene that would meet any reasonable standard as a 'gene for' schizophrenia, intelligence, depression, or extraversion" (Turkheimer, 2015).
>
> The dilemma is now widely discussed as the "missing heritability" problem.
>
> (pp. 19–20)



Ken Richardson

Simply, the conceptual, methodological, and empirical problems with the hereditarian approach to identifying the genetic basis of intelligence are legion. I discuss some of them in the next sections of this chapter.

Problems of Generalizing Heritability Estimates

After the publication of Jensen's (1969) article, several reviewers identified a problem of inappropriate generalization in Jensen's analysis (e.g., Feldman & Lewontin, 1975; Gould, 1980; Hirsch, 1970; Lewontin, 1976). That is, Gould (1980) made the point that:

> *The value of heritability within either the white or the black population carries no implication whatever about the causes for different average values of IQ between the two populations*. (A group of very short people may have heritabilities for height well above 0.9, but still owe their relative stature entirely to poor nutrition.) *Within and between group variation are entirely different phenomena*; this is a lesson taught early in any basic

> genetics course. Jensen's conflation of these two concepts marked his fundamental error.
>
> (p. 38, italics added)

Similarly, Feldman and Lewontin (1975) noted that:

> We are unable to make any inferences from between-group differences and within-group statistics about the degree of genetic determination of the between-group differences. In other words, the concept of heritability is of no value for the study of differences in measures of human behavioral characters between groups.
>
> (p. 1167)

Moreover, when Lewontin (1973) calculated the genetic diversity within populations, within races between populations, and between races, he found that 85% of human genetic diversity is accounted for by variation between people *within* populations. Only about 6% of human genetic diversity was found to be attributable to variation *between* races (Lewontin, 1973).

In addition to inappropriate attempts to generalize a within-group heritability estimate for use in understanding variation between groups, there is an additional instance of inappropriate generalization. People who attempt to use heritability as an indication of genetic determination appear to be involved in asking and attempting to answer the question "how much of each?" This question is one of the two that Anastasi (1958) rejected as illogical. In essence, they ask, "How much heredity must be *added* to how much environment to determine intelligence?" or "Which of the two is more important in determining intelligence?" However, answering this question is both logically impossible and inappropriate.

This significance of this issue has been explained by Hirsch (1970). He noted that researchers working from a hereditarian perspective typically test the intelligence scores of a sample at a single time in their development. In then making a heritability estimate, all that is being done is a determination of the relative proportions of the variation between the individuals in the sample that can be assigned to genetic and to environmental variation. That is, the reason that a distribution exists is that, by definition, individuals differ from each other in their scores on the test for an attribute. All that heritability does, then, is provide an estimate of how much of these *between-individual differences* is associated with genetic variation and how much is associated with environmental variation.

Some researchers mistakenly use these values, however, to estimate how much of the expression of the tested attribute *within a single individual* of that sample is determined by heredity and by environment. Between-individual data are applied to purported within-individual phenomena. Thus, not only is an attempt to ask "How much of each?" illogical, but also the collected data can in no way be used appropriately to begin to address the question. As Hirsch emphasized, researchers taking this approach "want to know how instinctive is intelligence *(with)in* the development of a certain individual, but instead they measure differences *between* large numbers of fully, or partially, developed individuals" (1970, p. 7, italics added). Indeed, Jensen (1969) agreed with this point, saying that "There is no way of partitioning a given individual's IQ into hereditary and environmental components, as if a person inherited, say, 80 points of IQ and acquired 20 additional points from his environment. This is, of course, nonsense" (p. 42).

Thus, heritability estimates may not be generalized between populations and they are meaningless and irrelevant for determining the roles of heredity and environment within a given individual. There is one final and very important, albeit quite technical, problem with heritability estimates. That is, Bouchard and McGue (1981) warned that the family resemblance data they reviewed (which are summarized in Table 11.4) presented formidable problems for devising useful statistical procedures (models) to appraise the heritability of human intelligence. These problems may be even more severe than Bouchard and McGue (1981) indicated.

Problems with Modeling Heritability

A penetrating analysis of the problems encountered when attempting to devise models with which to estimate heritability from a set of family resemblance data was presented by Goldberger (1979). He addressed these problems by noting:

> About 1970, a consensus had developed among experts that the genetic component of IQ variance was very high, about 80 percent. In part the consensus was based upon common sense. As one correspondent, the geneticist C. O. Carter, later put it, "It is I think evident to any experienced and unprejudiced observer that the major part of the variation in intellectual ability in school children in Britain is genetic" (*The Times*, 3 November 1976). In greater part, however, the consensus was based on the fitting of statistical models to sets of IQ kinship correlations.
>
> I will be concerned with model fitting rather than with common sense.
>
> (p. 327)

To begin to address the issue of model fitting, Goldberger (1979) devised a table wherein each column gave a set of IQ kinship correlations that arose when various pairs of relatives were given IQ tests and their scores were then correlated. Table 11.5 presents this information. In this table the IQ correlation decreases as the biological and/or social "distances" increase.

In order to partition the variance in IQ scores into genetic and environmental components from data such as those in Table 11.5, some instance of a statistical model is needed. The genetic and environmental variance components appear as unknown parameters (statistical portions of the model), and these parameters are adjusted to best fit the data. Table 11.6, which is also taken from Goldberger (1979), presents the estimates of the modeled environmental and genetic variance components that

Table 11.5 IQ kinship correlation sets used in various studies

Type of relationship	Jinks and Fulker (1970) UK	Eaves (1975) UK	Eaves (1975) US	Eaves (1972) US	Jencks (1977) US	Rao, Morton, and Yee (1976) US	Rao and Morton (1978) US
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Identical twins reared together	0.92	0.92	0.97	—	0.91	0.89	0.84
Identical twins reared apart	0.87	0.87	0.75	—	0.67	0.69	0.68
Siblings reared together	0.53	0.53	0.59	0.38	0.53	0.52	0.52
Siblings reared apart	0.44	0.44	—	—	—	—	0.25
Parent and biological child reared together	—	0.49	0.55	—	0.48	0.48	0.48
Parent and biological child reared apart	—	—	0.45	—	0.41	—	0.41
Adoptive siblings	0.27	0.27	0.38	—	0.36	0.25	0.32
Adoptive parents	—	0.19	0.28	—	0.23	0.23	0.23
Grandparents	—	0.33	—	—	—	—	—
Uncles	—	0.34	—	—	—	—	—
First cousins	—	0.28	—	0.14	—	—	—
Second cousins	—	0.16	—	0.07	—	—	—
Third cousins	—	—	—	0.03	—	—	—
Spouses	0.39	0.39	0.57	0.46	0.50	0.50	0.51

Note. Several kinships that were distinguished in the studies are combined here for simplicity: "Sibling" includes fraternal twins along with ordinary siblings. "Adoptive sibling" includes adoptive-adoptive pairs along with adoptive-natural pairs. In column (4) the entries were calculated by Goldberger from the ANOVA tables in Eaves (1977). In column (5) the entries were assembled by Goldberger from various pages in Jencks (1972).

Source: Goldberger (1979, p. 328).

Table 11.6 Components of IQ variance as estimated in various studies

	Jinks and Fulker (1970) UK	Eaves (1975) UK	Eaves (1975) US	Eaves (1977) US	Jencks (1972) US	Rao, Morton, and Yee (1976) US		Rao and Morton (1978) US		Eaves model fitted to Rao and Morton data US
						Children	Adults	Children	Adults	
	(1)	(2)	(3)	(4)	(5)	(6)		(7)		(8)
Genetic	83	85	68	60	45	67	21	69	30	58
Additive	62	65	35	60	32	67	21	69	30	31
Nonadditive	21	20	33	0	13	0	0	0	0	27
Environmental	17	15	32	40	35	19	66	51	70	42
Common	9	7	29	0	—	9	51	16	55	26
Specific	8	8	3	40	—	10	16	15	15	16
Covariance	0	0	0	0	20	14	13	0	0	0
Total	100	100	100	100	100	100	100	100	100	— 100
Kinships	7	14	9	4		11		16		10
No formal fitting										
Parameters	4	4	4	2		8		9		4
X^2/d.f.	2.1	1.4	1.5	0.6		0.9		4.4		1.0

Soure: Goldberger (1979, p. 330).

have been derived from the major attempts at such work. The column numbers in Table 11.6 correspond to those of Table 11.5.

Goldberger (1979) noted that the column headed "Jinks and Fulker (1970) UK" refers to modeling work done in the United Kingdom that uses seven of Cyril Burt's kinship correlations (and that finds heritability to be 0.83). Of course, these figures were recognized by Goldberger (1979) to be inadmissible and scientifically useless, given the errors and dishonesty associated with them. But Goldberger (1979) noted that both Eysenck and Jensen claimed that even after Burt's "data" were discarded, modeling using other data would confirm the estimates derived from Burt's study. For instance, Goldberger (1979, p. 329) noted that Eysenck (1976) wrote in *The Times*, November 12, 1976, that it was "noteworthy that many different approaches have resulted in very similar estimates [of IQ heritability] usually centering around the 80 percent mark, although the range of uncertainty gives a lower bound of something like 60 percent and an upper one of 90 percent or thereabouts," and, in *The Times*, November 8, 1976, Eysenck wrote that:

> There is ample evidence for the genetic determination of intelligence . . . [The] studies are remarkably congruent in the conclusions they allow us to come to. It is typical that opponents are vocal mainly in the pages of newspapers, in the letter columns of popular magazines, or in other non-technical places . . . Critics seem to fight shy of the technical literature, or the essential examination of the totality of the evidence.

On various subsequent occasions, Eysenck restated his scholarly concern in such distinguished "technical" journals as *New Society* (November 11, 1976), *New Scientist* (November 25, 1976), *Encounter* (January 1977), and *The Bulletin of the British Psychological Society* (1977).

Eysenck placed particular weight on the contributions of the Birmingham school of biometrical genetics, represented by J. L. Jinks, D. W. Fulker, and L. J. Eaves. Jensen (1976), on the other hand, placed particular weight on the contributions of the Honolulu school, represented by N. E. Morton and D. C. Rao, writing, in *The Times*, December 9, 1976, that:

> It is noteworthy that a leading American geneticist, Professor Newton Morton, has made a detailed statistical comparison of British kinship correlations (most all of them from Burt's studies) with those of all the parallel studies done by American investigators, and he finds the differences between the two sets of results to be statistically nonsignificant . . . Morton writes: "Whatever errors may have crept into his (i.e., Burt's) material, they do not appear to be systematic." . . . The scientific weight of all the massive and newer evidence and modern quantitative genetic analyses, in numerous studies by independent investigators using somewhat different methods, now far surpass that of Burt's own pioneer research. Yet the evidence *sans* Burt leads *in toto* to essentially the same general conclusions: . . . in accounting for individual differences in IQ, genetic factors considerably outweigh the existing environmental influences.

But, despite Eysenck's and Jensen's claims, Goldberger (1979) showed, in Table 11.6, that, when Burt's "data" are discarded, a picture quite different from the one portrayed by Eysenck emerges. Indeed, the picture is quite blurred (Goldberger, 1979). First, Goldberger noted that in the data sets presented in Table 11.5

> the entry for a kinship is not a correlation observed in a single sample, but rather an average across several samples taken at various times and places. For example, in Column 3, the entry of 0.38 for adoptive siblings is actually an average of the figures found in seven adoption studies, the original correlations having ranged from 0.06 to 0.65.
>
> (Goldberger, 1979, p. 333)

Table 11.4 shows that the family resemblance data reviewed by Bouchard and McGue (1981) had similar evidence for considerable within-category heterogeneity. Given this heterogeneity, the clarity of any of the models' estimates becomes quite dim. Indeed, Goldberger (1979) summarized the usefulness of the modeling attempts represented in Tables 11.5 and 11.6 by indicating that:

> Enough of the structure has been exposed that we can assess the plausibility of the models. Ignorance of genetics need not deter us, because the models involve as much social science theorizing as genetic theorizing. How marriages take place, how adoption agencies operate, how parents raise their children, how brothers and sisters educate one another—all those processes are reflected in the biometrical-genetic models.
>
> I call attention to two such pieces of theorizing that are incorporated in the Birmingham and Honolulu models. (1) Identical twins share just as much IQ-relevant experience as ordinary siblings do, no more. That happens despite the fact that identical twins are of the same age and sex, while ordinary siblings may differ in age and sex. (2) Adoption agencies place children in families randomly drawn from the population at large. That happens even though every adoption study shows that adoptive parents rank high on virtually every socioeconomic and psychological measure.
>
> If those pieces of theorizing are unbelievable, then the parameter estimates provided by Birmingham and Honolulu (and by me in column [8] of Table 11.6) should not be believed either. For the assumptions are critical rather than incidental ones. If ordinary siblings share less IQ-relevant experience than identical twins do, then the difference between the IQ correlations for those two kinships is partly attributable to environment. If adoptive families span a reduced range of environments, then the IQ correlations among adoptive kin understate the common environment variance in the population at large. Both pieces of theorizing tilt the estimated balance from environment to genes. To explain the persistent use of such assumptions, we need only recognize that without them the models would be indeterminate. If less restrictive, and hence more plausible, specifications were made, the number of unknown parameters would approach and soon exceed the number of observations.

> Implausible assumptions are needed to identify the parameters and produce the estimates, and thus to keep the model-fitters happy. However, estimates produced in that manner do not merit the attention of the rest of us.
>
> (p. 336)

Conclusions

Heritability research in general, and as illustrated in the present chapter, particularly as it has been done in relation to IQ, has involved advancing fallacious arguments and misapplying data (see Layzer, 1974). At best, heritability is a concept of extremely narrow utility. If misunderstood and misapplied, it leads to the erroneous assumption that high heritability means developmental fixity—that the expression of a highly heritable attribute cannot be altered through environmental changes, an assertion made by proponents of the purported "Big Five" personality "traits" (e.g., Costa & McCrae, 1980; McCrae et al., 2000). Indeed, McCrae et al. (2000) claim, in regard to each of the five personality traits they discuss, that a trait is simply innate and unavailable to environmental influence.

However, the theory and research discussed across this chapter and previous ones afford the conclusion that no psychological attribute is pre-organized in the genes and unavailable to environmental influence. That is, any alleged genetic difference (or "inferiority") of African Americans based on the high heritability of intelligence would seem to be an attribution built on a misunderstanding of concepts basic to an appropriate conceptualization of the nature–nurture controversy. An appreciation of the coaction of genes and context—of genes↔context relations—within the relational developmental system, and of the meaning, implications, and limitations of the heritability concept, should lead to the conclusion that *the genetic-differences hypothesis of racial differences in IQ makes no scientific sense*. The heritability (in the sense of developmental fixity) of intelligence, or of any other psychological attribute for that matter, must be recognized as a psychological unreality. Such terms have, at best, so little scientific utility as to make them functionally worthless (see Layzer, 1974, 2000; Moore & Shenk, 2016; Richardson, 2017).

For instance, Layzer (1974) stated:

> The definition of IQ has no theoretical context or substratum. Tests of IQ measure what they measure. They are precisely analogous to physical readings made with a black box—a device whose internal working is unknown. Because we do not know what an IQ test or a black box is supposed to measure or how it works, we cannot know to what extent measurements carried out on different subjects are comparable or to what extent they are influenced by extraneous factors. Thus, IQ scores contain uncontrollable, systematic errors of unknown magnitude.
>
> This helps to explain why different investigators frequently report such widely differing estimates of the same IQ correlation. For example, reported estimates of the parent–child correlation range from .2 to .8, whereas estimates of the correlation between same-sex dizygotic twins range from .4 to .9 (Erlenmeyer-Kimling & Jarvik, 1963). According to Jensen (1973), there are no objective criteria (other than sample size) for weighting discrepant estimates of the same correlations.
>
> Because the definition of IQ is purely instrumental, it fails to confer the most essential attribute of a scientific measurement—objectivity. To measure a subject's Stanford-Binet IQ, one must administer a specific test in a specific way under specific conditions. By contrast, a well-equipped physics laboratory does not need to have replicas of the standard meter and the standard kilogram to measure length and mass, and the physicist or biologist is free to devise his own techniques for measuring such quantities. Systematic discrepancies between measurements of the same quantity are never ignored in the physical and biological sciences, because they signal the presence of unsuspected systematic errors or of defects in the theory underlying the measurements.
>
> IQ scores also differ from conventional measurements in that they have no strict quantitative meaning. The IQ is an index of rank order on a standard test, expressed according to a convenient but essentially arbitrary convention (Stevens, 1946). In effect, the intervals of the IQ scale are

> chosen in such a way as to make the frequency distribution of test scores in a reference population approximately normal, but other methods of defining the scale could claim equal prior justification.
>
> These considerations show that IQ scores are not phenotypic measurements in the usual sense. (p. 1262)

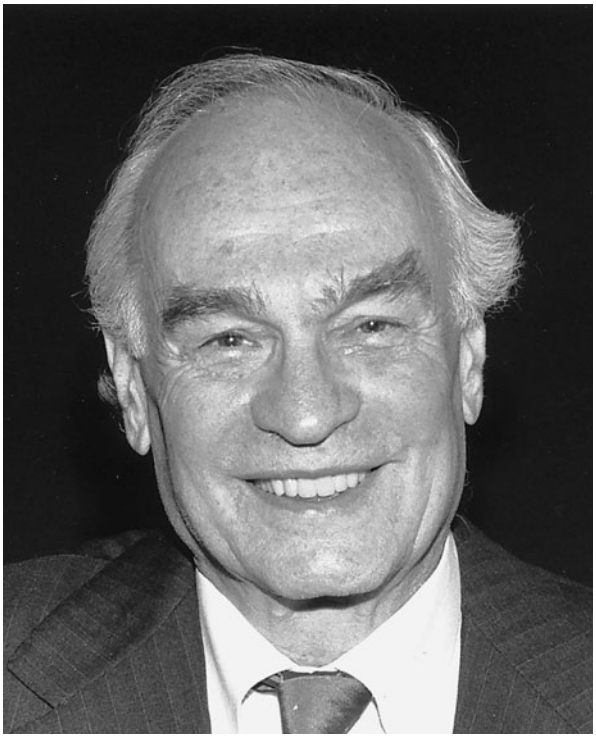
These general problems with IQ tests complicate the issues involved in making any valid statements about the genetic basis of IQ differences between the races. Given, then, the additional problems with the calculation of heritability estimates that I have reviewed, it is clear why Layzer believes that "the only data that might yield meaningful estimates of narrow heritability are phenotypic correlations between halfsibs reared in statistically independent environments. No useful data of this kind are available" (Layzer, 1974, p. 1265). As such, it is appropriate to conclude:

> Under prevailing social conditions, no valid inferences can be drawn from IQ data concerning systematic genetic differences among races or socioeconomic groups. Research along present lines directed toward this end—whatever its ethical status—is scientifically worthless.
>
> Since there are no suitable data for estimating the narrow heritability of IQ, it seems pointless to speculate about the prospects for a hereditary meritocracy based on IQ. (Layzer, 1974, p. 1266)

Moreover, writing about a quarter-century later, Layzer (2000) continued to see theoretical and computational misconceptions of biometrical "IQists." After noting these problems, Layzer (2000) asked: "How have biometricians who analyze IQ scores managed to occupy themselves for so many decades with strictly meaningless questions? The answer—or at least the most polite part of the answer—lies in their failure to formulate their mathematical model correctly" (p. 2). As such, Layzer (2000) in fact concluded that: "Biological, psychological, and sociological considerations make it seem highly implausible that IQ test scores could have the additive structure biometricians posit. For this reason alone, it seems to me that biometric studies of IQ are scientifically bankrupt" (p. 2).

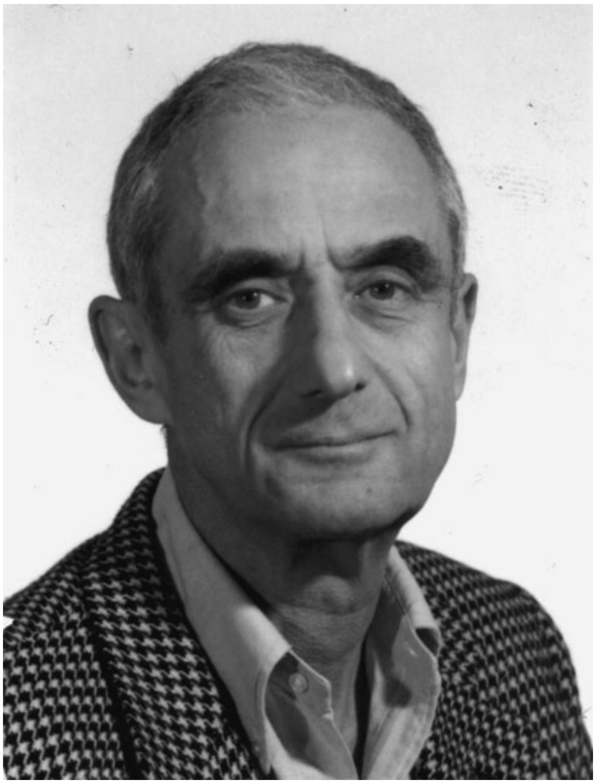
Both Hirsch (1981, 1997) and Feldman and Lewontin (1975) reached congruent conclusions. For instance, Hirsch (1981) noted:

> Failure conceptually to appreciate and to integrate three fundamentals of biology—individuality, interaction and norm of reaction throughout ontogeny—underlies the confusion. Individuality is a consequence of the fact that members of diploid, bisexual, cross-fertilizing species are genotypically unique. Moreover, although it is a platitude to say that heredity and environment interact to produce the phenotype, it is that interaction of unique genotypes with environments which thwarts a simple systems approach . . . Not only do genotypes differ in response to a common environment, but one genotype varies in response to different environments. The latter property is called norm of reaction. Interaction



David Layzer

> and norms of reaction describe aspects of the complex genotype–phenotype relationship. The latter focuses on the developmental outcomes of a single genotype replicated in different environments. The former includes the latter and considers at one time many genotypes and many environments. For an array of genotypes replicated in various sets of environmental conditions, it calls attention to how, out of the variety of possible distributions of phenotypic outcomes, the particular one obtained will depend on which genotype develops under which conditions.
>
> The key to "establishing the relative roles of heredity and environment" has been believed erroneously to be the heritability estimate. But heritability estimates cannot be made for human intelligence measurements, because the heritability coefficient is undefined in the presence of either correlation or interaction between genotype and environment, both of which occur for human intelligence. When correlation exists, either (1) between genetic and environmental contributions to trait expression, or (2) between environmental contributions to trait expression in both members of a parent–child or sib pair, heritability is not defined. Furthermore, when heritability can be defined, for example in well-controlled plant and animal breeding experiments, it has no relevance to measured differences in average values of trait expression between different populations: heritability estimates throw no light upon intergroup comparisons! Also, heritability estimates provide no information about ontogeny and are thus irrelevant to the formulation of public policy on education and social conditions.



Jerry Hirsch

> None of the statements about proportional contributions of heredity and environment to the determination of level of "intelligence" or of many other human traits can be either substantiated or disproven by any conceivable observations.
>
> (p. 33)

In turn, Feldman and Lewontin (1975) indicated:

> The problem we have been examining is the degree to which statistical structures can reveal the underlying biological structure of causation in problems of human quantitative genetics. We must distinguish those problems which are by their nature numerical and statistical from those in which numerical manipulation is a mere methodology. Thus, the breeding structure of human populations, the intensities of natural selection, the correlations between mates, the correlations between genotypes and environments, are all by their nature statistical constructs and can be described and studied, in the end, only by statistical techniques. It is the numbers themselves that are the proper objects of study. It is the numbers themselves that we need for understanding and prediction.
>
> Conversely, relations between genotype, environment, and phenotype are at base mechanical questions of enzyme activity, protein synthesis, developmental movements, and paths of nerve conduction. We wish, both for the sake of understanding and prediction, to draw up the blueprints

Marcus Feldman

> of this machinery and make tables of its operating characteristics with different inputs and in different milieus. For these problems, statistical descriptions, especially one-dimensional descriptions like heritability, can only be poor and, worse, misleading substitutes for pictures of the machinery. There is a vast loss of information in going from a complex machine to a few descriptive parameters. Therefore, there is immense indeterminacy in trying to infer the structure of the machine from those few descriptive variables, themselves subject to error. It is rather like trying to infer the structure of a clock by listening to it tick and watching the hands. At present, no statistical methodology exists that will enable us to predict the range of phenotypic possibilities that are inherent in any genotype, nor can any technique of statistical estimation provide a convincing argument for a genetic mechanism more complicated than one or two Mendelian loci with low and constant penetrance. Certainly the simple estimate of heritability, either in the broad or narrow sense, but most especially in the broad sense, is nearly equivalent to no information at all for any serious problem of human genetics.
>
> (pp. 1167–1168)

CONCLUSIONS ABOUT THE STUDY OF HERITABILITY AND INTELLIGENCE

It seems clear that the evidence for the genetic basis of intelligence and/or for racial/ethnic/social class differences in IQ that is derived from heritability estimates is more "apparent" to hereditarians than it is "real" to scientists using appropriate conceptual and methodological lenses. Given, then, the poor quality of the evidence in support of the hereditarian position, I agree with the conclusion of Kempthorne (1978) that:

> The heredity–IQ controversy has been a "tale full of sound and fury, signifying nothing". To suppose that one can establish effects of an intervention process when it does not occur in the data is plainly ludicrous. Mere observational studies can easily lead to stupidities, and it is suggested that this has happened in the heredity–IQ arena. The idea that there are racial–genetic differences in mental abilities and behavioral traits of humans is, at best, no more than idle speculation.
>
> (p. 1)

Similarly, Jacquard (1983) noted that:

> The need for great rigour exists particularly in the case of research projects which have serious implications for us all; this is the case when psychologists study the "heritability of intellectual aptitudes." They should take the precaution of systematically defining in a precise way the sense in which they use the word "heritability," they should also state whether the assumptions under which this word can be used hold true in

> their studies. It is highly probable that most of the time this exercise in rigour would lead them to the conclusion that none of the three parameters proposed by geneticists can be of any use in solving their problems.
>
> (p. 476)

In sum, then, it is perhaps appropriate to end my presentation of the controversy over the heritability and intelligence by again referencing the views of Moore and Shenk (2016):

> numerous theorists have concluded that "the term 'heritability,' which carries a strong conviction or connotation of something '[in]heritable' in the everyday sense, is no longer suitable for use in *human genetics*, and its use should be discontinued" (Guo, 2000). Reviewing the evidence, we come to the same conclusion. Continued use of the term with respect to human traits spreads the demonstrably false notion that genes have some direct and isolated influence on traits. Instead, scientists need to help the public understand that all complex traits are a consequence of developmental processes. Without such an understanding, we are at risk of underestimating the extent to which environmental manipulations can have profoundly positive effects on development (Moore, 2002, 2015a). Thus, the way "heritability" is used in most discussions of human phenotypes not only perpetuates false ideas; it also blinds us to steps we might otherwise take to improve the human condition.
>
> (pp. 6–7)

THE CONTINUATION OF GENETIC REDUCTIONISM

Unfortunately, claims about genetic determination of human development neither go away nor contribute to improvement of the human condition. Despite the evidence presented about its uselessness, claims about genetic determination based on heritability research continue to be made through the time of this writing (e.g., Plomin et al., 2016; Rimfeld et al., 2016). In addition, other versions of genetic reductionism—for instance, sociobiology and evolutionary developmental psychology—are used to make recommendations about social policy (e.g., Belsky, 2014; Wade, 2014) that can constrain opportunities for social justice, maintain racial, ethnic, and class disparities and, as well, promote new interest in eugenics (Lerner, 2015a, 2015b).

Indeed, at the beginning of the twenty-first century Francine Horowitz (2000) cautioned developmental science that the scholars who, in the twentieth century, promoted reductionist conceptions would not abandon in the twenty-first century their reliance on genes to account for human development. She warned that history teaches that genetic reductionist conceptions do not go away despite being shown repeatedly to be scientifically "fatally flawed."

Horowitz (2000) appears to be correct, at least at this writing. Nevertheless, in the next chapter, I will persevere in discussing the fatal flaws of instantiations of genetic reductionism that continued to be promulgated across the twentieth century and through the first two decades of the twenty-first century.

NOTES

1 I am grateful to Professor Alexander von Eye for pointing out this proverb to me.

2 On October 18, 1983, I wrote Professor Bouchard about the three studies of MZ twins reared apart summarized in his article with McGue. My request for information read:

> I am writing in regard to your paper with Matthew McGue in *Science* (1981, Volume 212, pages 1055–1059), "Familial studies of intelligence: A review." On page 1056 (column 2, paragraph 2) you note that, "After deleting the Burt data we are left with test results on but 65 pairs of monozygotic twins reared apart, as reported in three separate investigations." I would appreciate learning of the references for these three studies and of the numbers of MZ pairs assessed in each study.
>
> Thank you very much for your help in this matter . . .

About a month later my original letter was returned to me. On the bottom of it someone—I assume Professor Bouchard—had handwritten the following message: "See Book reviewed in attached paper—also Taylor, H.

The IQ Myth [sic—the title is actually *The IQ Game*], Rutter 1980. Poor book. I have a critique which will appear in *Intelligence*."

There was no signature. However, reprints of four papers were enclosed: (1) the Bouchard and McGue (1981) paper, about which I had inquired in my October 18 letter to Professor Bouchard; (2) a book review by Duncan (1982) of the Eysenck and Kamin (1981) book, *The Intelligence Controversy*; the review did not identify the three studies by Bouchard and McGue (1981), although Kamin's (1974) reanalysis of the Juel-Nielsen (1965) data was critiqued; (3) a book review by Bouchard (1982) of the Farber (1981) book, *Identical Twins Reared Apart: A Reanalysis*; this review also did not identify the three studies cited by Bouchard and McGue (1981), although Farber's (and Kamin's, 1974) reanalyses of the Newman, Freeman, and Holzinger (1937) and the Shields (1962) data are critiqued; and (4) an article by David T. Lykken (1982), wherein Lykken identifies (Table 1, p. 362) "the four major studies of monozygotic twins reared apart." One of these studies is the Minnesota Study (1981), of which Professor Bouchard is the principal investigator. The other three studies are the ones identified by Kamin (i.e., Newman, Freeman, & Holzinger, 1937; Shields, 1962; and Juel-Nielsen, 1965). However, the numbers of MZ pairs noted by Lykken as being in these studies are 19, 44, and 12, respectively; this listing results in a total number of 75 pairs, which is 10 pairs more than the number cited for the three studies summarized in Bouchard and McGue and 7 pairs more than the number cited by Kamin (1974) for these same three studies.

Although none of the above four articles sent to me explicitly identifies the three articles summarized in the Bouchard and McGue (1981) paper, I presume they are indeed the ones also reviewed by Kamin (1974). In fact, Professor Robert Plomin, a preeminent behavior geneticist, indicated to me (personal communication, January, 1984) that this presumption was the case.

CHAPTER TWELVE

Genetic Reductionism in Developmental Science

Sample Cases from the Twentieth and Twenty-First Centuries

There are many more instances of genetic reductionist accounts of human development than those reviewed in Chapter 11. I noted at the end of that chapter that, at this writing, many of these accounts continue to be promulgated into the twenty-first century. I start my discussion of these genetic reductionist, hereditarian views by returning to the concept of heritability. However, I discuss this concept in relation to the field of research, *behavior genetics* (e.g., Plomin, 2000; Plomin et al., 2016), within which this concept is embedded most often in the literature of human development (Charney, 2016).

I noted at the end of Chapter 11 the conclusion of Moore and Shenk (2016), that the use of the concept of heritability in regard to the study of characteristics of human development falsely implies that genes directly shape these characteristics. However, the computation of heritability is neither restricted to the study of intelligence, which was the principal substantive focus of the discussion of the work of Burt, Jensen, Eysenck, and others in Chapter 11, nor is it the only facet of the work undertaken by behavior geneticists. In addition to the criticisms of the concept of heritability that I discussed in Chapter 11 (e.g., Feldman & Lewontin, 1975; Goldberger, 1979; Hirsch, 1981; Layzer, 1974; Moore & Shenk, 2016; Richardson, 2017), there are other, significant problems with this concept that should be raised in order to thoroughly understand the egregious flaws of this statistic as it continues to be used by behavior geneticists. As well, the field of behavior genetics has flaws that extend beyond the computation of heritability.

THE FIELD OF BEHAVIOR GENETICS

The complexity involved in the depictions of development found in relational developmental systems (RDS)-based theories can be daunting to scholars, both in regard to the conceptual difficulties involved in integratively understanding the multiple levels of organization fused within the relational developmental system and in respect to the methodological challenges involved in using such theories as a frame for research. If challenging to scholars, such theories may often be seen as virtually impossible to grasp by non-specialists (the “person in the street,” to Horowitz, 2000, p. 8) and/or by media representatives. Both groups may gravitate toward “single-variable stories” (Horowitz, 2000, p. 3) about human development—such as “genes cause behavior” (e.g., see Rushton, 1999, 2000)—in order to understand or communicate about people’s lives.

Such a simplistic—a distortedly simplistic—alternative to RDS-based theories of human development is embodied in the field of behavior genetics. As explained by Horowitz (2000):

> Against the media popularity of single-variable stories, the science itself is moving inexorably toward greater and greater data-driven, integrative theoretical complexity. An exception to this is behavioral genetics. In contrast to the dynamic nonlinear interactive models full of reciprocity between and among levels and variables, behavioral genetics presents a relatively non-dynamic linear additive model that tries to assign percentages of variance in behavior and development that can be attributed to genes. The enterprise rests on the assumption that genetic influence can be expressed as a value accounting for a portion of the variance in a nondynamic linear equation for predicting behavioral functioning and furthermore, that the individual experiences of shared and nonshared environments can be assessed inferentially by the degree of biological relatedness of individuals without empirical observations of experience (Hoffman, 1991; Horowitz 1993).
>
> Behavioral genetics involves a relatively simplistic approach when compared with the kinds of dynamic systems theories currently being elaborated. Perhaps that is why, in the mode of wanting simple answers to simple questions, behavior genetic reports are so media attracting. (p. 3)

Frances D. Horowitz

What then is the field of behavior genetics? What is the view of human development it presents? How does it seek to support its views? Does it represent a viable, "nature" alternative to dynamic and integrative developmental systems conceptions of human development in general or of particular facets of human development (e.g., intelligence) more specifically? To address these questions, I turn to a definition of this field.

According to Robert Plomin (2000), arguably the most eminent behavior geneticist in the world, "Behavioural genetics is the genetic study of behaviour, which includes quantitative genetics (twin and adoption studies) as well as molecular genetics (DNA studies) of human and animal behaviour broadly defined to include responses of the organism from responses measured in the brain such as functional neuroimaging to self-report questionnaires" (p. 30). Plomin, DeFries, and McClearn (1980) add that "Behavioral genetics lies at the interface between genetics and the behavioral sciences" (p. 12), and Plomin (1986) noted that "Behavioral geneticists explore the etiology of individuality, differences among individuals in a population" (p. 5). Plomin, DeFries, Knopik, and Neiderhiser (2013) note that the basic methods used in human behavioral genetics are family, twin, and adoption studies and, as well, molecular genetic research.

Across all of these methods, the goal of behavior genetic analysis is to separate (partition, for instance, through biometrical model fitting; Joseph, 2015) the variation in a distribution of scores (e.g., for a personality attribute, temperamental characteristic, or intelligence) into the proportion due to genes and the proportion due to the environment. Although behavior geneticists admit that genes and environments may be correlated and/or may interact (and they use this term in its statistical sense; e.g., see Chapter 10 and my discussion of the work of David Magnusson), they most typically seek to compute a score (termed a *heritability coefficient* or, more commonly, a *heritability estimate*; Joseph, 2015) that (in its most frequently used form) denotes the independent contribution of genetic variance to the overall differences in the distribution of scores for a given individual characteristic.

For such heritability scores to be meaningful, the methodologies of behavior genetics rest on a model of gene function that considers the possibility that genetic contributions are independent of (not correlated or interactive with) the context within which genes exist. However, genes do not work in the way that behavior geneticists imagine. Indeed, Joseph (2015) argues that heritability estimates would not be meaningful even if genes and environment did not interact, which of course they do. Behavioral genetics model fitting is based on the assumption that genetic and environmental influences are "additive." Joseph (2015) points out that this false assumption, by itself, is a "fatal flaw" of the heritability estimates produced through the model fitting procedures of behavioral genetics researchers. Joseph (2015) also indicates that the MISTRA models (e.g., Bouchard et al., 1990), discussed in Chapter 11, were based on this assumption (as well as on other false assumptions).

Fatal Flaws in the Behavior Genetics Model of Gene Function

Strohman (1993a, 1993b), other molecular biologists (e.g., Ho, 1984, 2010, 2013; Müller-Hill, 1988), and biological or comparative psychologists (e.g., McEwen, 1997, 1998, 1999; Meaney, 2010; Meaney et al., 1988) do not place credence in the model of genetic function involved in behavior genetics. In fact, Venter and his colleagues (2001), the group that successfully mapped the sequence of the human genome, emphasize that there are two conceptual errors that should not be made in the face of the advances they and other scientists have made in understanding the structure and functional consequences of the human genome. They stress that:

> There are two fallacies to be avoided: determinism, the idea that all characteristics of the person are "hard-wired" by the genome; and reductionism, the view that with complete knowledge of the human genome sequence, it is only a matter of time before our understanding of gene functions and interactions will provide a complete causal description of human variability.
>
> (Venter et al., 2001, p. 1348)

Developmental scientists conducting their research within RDS-based models would make the same two criticisms (e.g., Overton, 2015a).

Contemporary thought in molecular genetics thus rejects the idea that genes are structures that act *on* supragenetic levels; instead, these scientists adopt the RDS-based, probabilistic-epigenetic view of the role of genes in human development that is epitomized by Gottlieb (e.g., 1970, 1997, 1998). The integration—or fusion—of genes *with* the other levels of organization that comprise the person and his or her context creates the individuality of behavior at and across points in ontogeny (e.g., Charney, 2016; Lickliter, 2016; Moore & Shenk, 2016).

In essence, the field of behavior genetics (e.g., Plomin, 1986, 2000; Plomin et al., 2016; Rowe, 1994) uses a model of genetic structure and function that is specifically rejected by those scientists who study the structure and action of genes directly (e.g., Venter et al., 2001). This rejection occurs not only because the field of behavior genetics employs a counterfactual and scientifically atavistic conception of the role of genes in human development (e.g., Ho, 1984, 2010, 2013; Strohman, 1993a, 1993b), but also because behavior genetics is a viewpoint with a conceptually flawed and empirically deficient view of the developmental process and, as well, involves the conflation of description and explanation.

For instance, in regard to process, the structural account of genetic action offered by behavior genetics suffers from the flaws of all structural accounts of development; that is, as explained by Thelen and Smith (1994, 1998, 2006; Smith & Thelen, 1993), such conceptions are inherently incomplete. These views do not explain individual behavioral performance (actions), other than to express empirically unsubstantiated confidence that in some way genetic structures translate—through the levels of cells, tissues, organs, the individual, and his or her actual context—into real-time actions.

For example, without any specification of the pathways of influence from genes to behaviors, Rowe (1994) asserted that:

> Genes can produce dispositions, tendencies, and inclinations, because people with subtly different nervous systems are differently motivated . . . [and] given enough environmental opportunities [for selection of environments], the ones chosen are those most reinforcing for a particular nervous system created by a particular genotype . . . the direction of the growth curve of development, and the limit ultimately attained, is set in the genes.
>
> (p. 91)

However, because behavior geneticists believe that genetic structure transcends and is independent of real-time actions, an adequate, empirically verifiable account of actual individual-in-context behavior is beyond theoretical range (Smith & Thelen, 1993). Moreover, because of the inability to explain individual performance, that is, actual individual-in-context behavior, behavior genetics, like other structural theories (Smith & Thelen, 1993), cannot explain the global order of behavior or developmental change itself.

In turn, in regard to the conflation of description and explanation, behavior genetics describes variability in *distributions* of a specific attribute in a specific sample, and then explains the distribution it has observed by reference to a label it has applied to one (or the other) of the "sources" of the variability in genes or environment. Not only is this reification an instance of the nominal fallacy, but also—to paraphrase the parody of structural explanations presented by Smith and Thelen (1993, p. 159)—the cause of the distribution of interindividual differences in a distribution of a specific attribute is merely an abstract description of the attribute distribution itself: Behavior genetics describes the variability in a distribution, labels it with a fancy "source" term (i.e., heritability), and then imputes that there is a gene, or set of genes, that explains the distribution.

To illustrate, Rowe (1994) noted that "understanding the growth and development of a single individual has been confused with understanding the origin of different traits in a population" (p. 3). However, this confusion about the distinction between interindividual differences and intraindividual change, as well as the problem of the conflation of description and explanation, exists in behavior genetics. On the basis of heritability data, writers such as Rowe (1994) seamlessly slide from talking about descriptive "sources" of variation within a distribution into talking about the genetic basis of individual development, that is, about the "causal influence on such child outcomes as intelligence, personality, and psychopathology" (p. 1).

The logical and inferential problems with such a statement are enormous. For instance, Charney (2016) explains that:

> It is important to understand that heritability estimates address *similarities/differences* in traits rather than the *causes* of traits themselves, and that the difference here is enormously consequential. The claim, e.g., that lifetime income is 50% heritable is not intended to mean that 50% of a person's income is due to her "genes" and 50% to her "environment." Heritability estimates only apply to populations, not to individuals, and the word "heritability," in this context, must not be confused with "genetic inheritance." Unfortunately, although "correlation does not entail causation," heritability estimates are frequently misinterpreted as showing that genetic similarities *cause* trait similarities in the study population.
>
> (pp. 1–2)

I will note again the problems raised in behavior genetics by the conflation of description and explanation. Here, however, it is necessary to note that one key basis of the lack of an adequate treatment in behavior genetics of performance, developmental sequence and process, as well as the distinction between description and explanation, is that these conceptual problems are coupled in behavior genetics with a lack of an adequate theoretical understanding both of supragenetic intraorganism processes (Gottlieb, 1991, 1992, 1997, 1998; Gottlieb et al., 2006) and of extraorganism contextual or ecological processes (e.g., Bronfenbrenner & Ceci, 1994; Bronfenbrenner & Morris, 2006; Horowitz, 2000; Lewis, 1997; Magnusson, 1999a, 1999b, 2000;

Overton, 2015a; Raeff, 2016; Sameroff, 1983, 2009, 2010; Thelen & Smith, 2006). Accordingly, in behavior genetics, there is a failure to adequately measure the environment or ecology of human development. In short, to paraphrase Goldberger (1980), in his discussion of Hearnshaw's (1979) account of the alleged scientific fraud perpetrated by behavior geneticist Cyril Burt in regard to the study of the heritability of intelligence (see Chapter 11 for a discussion of this sorry episode in the history of social and behavioral science), behavior geneticists have methods that give them a lot of numbers but very little sensible or useful data about human development.

BEHAVIOR GENETICS AS THE EMPEROR'S NEW CLOTHES

That these egregious conceptual and methodological problems of behavior genetics exist is not news. For almost 50 years, at this writing, these problems have been identified by several scholars, including, for example, Bronfenbrenner and Ceci (1994), Charney (2012, 2016), Collins et al. (2000), Ford and Lerner (1992), Gottlieb (e.g., 1970, 1998; Gottlieb et al., 2006), Greenberg (e.g., 2004, 2011, 2013, 2015, 20167), Hirsch (e.g., 1970, 1997, 2004), Horowitz (2000), Joseph (2004), Kuo (e.g., 1967, 1976), Krimsky (e.g., 2013), Lehrman (e.g., 1953, 1970), Lerner (e.g., 1976, 1978, 2015a), Lewis (1997), Magnusson (e.g., 1999a, 1999b; Magnusson & Stattin, 1998), Moore (e.g., 2002, 2006, 2013, 2015a, 2016; Moore & Shenk, 2016), Overton (e.g., 1973, 2015a; Overton & Lerner, 2012), Panofsky (2014), Richardson (e.g., 2013, 2017), Schneirla (e.g., 1956, 1957), Tobach (e.g., 1981; Tobach & Greenberg, 1984), Thelen and Smith (e.g., 1994, 2006), Wahlsten (e.g., 2012, 2013), and Witherington (e.g., 2015; Witherington & Heying, 2013).

Yet, despite criticism by colleagues in the fields of psychology or developmental science, including some colleagues who themselves are leading behavior geneticists (e.g., Turkheimer, 2011), as well as by molecular geneticists (Cole, 2014; Slavich & Cole, 2013), population geneticists (e.g., Feldman, 2014; Feldman & Lewontin, 1975), and evolutionary biologists (e.g., Gould, 1981, Ho, 2010, 2013; Jablonka & Lamb, 2005), many proponents of behavior genetics continue to believe (because of results of twin and adoption studies; Joseph, 2015) that there is a compelling evidence base for the inheritance of behaviors as varied as academic achievement (Selzam et al., 2017), intelligence (Jensen, 1969, 1998), parenting (Scarr, 1992), morality (Wilson, 1975a, 1975c), temperament (Buss & Plomin, 1984), television watching (Plomin, Corley, DeFries, & Faulker, 1990), and, as Charney (2016) lists, "voting in a presidential election, creative dance performance, and utilitarian moral judgments" (p. 1). In addition, there are some behavior genetic accounts claiming that genes shape the role in human development of the "environment" (Harris, 1998; Plomin, 1986, 2000; Plomin & Daniels, 1987; Rowe, 1994)! For example, the researchers associated with the MISTRA project (e.g., Bouchard et al., 1990) base, at least in part, their genetic reductionist interpretation of their data on the assertion that environmental similarities for MZs reared apart should be counted as an effect of genes (Joseph, 2015). This interpretation is made because these researchers forward the circular argument that these twins create their own environments because they are more similar genetically (Joseph, 2015).

The breadth and depth of the continuing criticisms of behavior genetics have been somewhat invisible to or, at least, ignored by Plomin (2000), who claimed that: "The controversy that swirled around behavioural genetics research during the 1970s has largely faded. During the 1980s and especially during the 1990s, the behavioural sciences became much more accepting of genetic influence" (p. 30). Extending this positive view of the contributions of behavior genetics into the second decade of the twenty-first century, Plomin and colleagues presented in 2016 the "top ten" replicated findings from behavior genetics (Plomin, DeFries, Knopik, & Neiderhiser, 2016). They note that:

> On the basis of our decades of experience in the field of behavioral genetics and our experience in writing the major textbook in the field (Plomin, DeFries, Knopik, & Neiderhiser, 2013), we selected these 10 findings because in our opinion they are "big" findings, both in terms of effect size and their potential impact on psychological

Robert Plomin

> science. These findings are not novel precisely because we selected results that have been repeatedly verified. For this reason, each of the findings in our top 10 list has been reviewed elsewhere, and a few have been highlighted previously as "laws" of behavioral genetics, as will be noted later. Although not all of these findings are supported by formal meta-analyses, we expect that most behavioral geneticists will agree with the 10 findings on our list, but we also suspect they may wish to add to the list.
>
> (Plomin et al., 2016, pp. 3–4)

Plomin's assertions about the acceptance and contributions of behavior genetics are incorrect for several reasons.

Failures to Replicate

First, although I would not dispute the claim by Plomin et al. (2016) that behavior geneticists would applaud their "top 10" list and might even want to add additional items to it, there is strong evidence that the replicability of behavior genetics findings that Plomin et al. (2016) claim is simply not the case. Charney (2016) summarizes this evidence. As I noted earlier in this chapter, the computation of heritability coefficients is not the only method used by behavior geneticists to identify the genes that (in their view) determine behavior. One additional method is termed candidate gene association (CGA) studies. This method is used to find polymorphisms associated with specific behaviors. I discussed the concept of *polymorphism* briefly in Chapter 11, but it is useful here to note again that "When a specific variation in DNA sequence in a particular location on a chromosome occurs in >1% of the population, it is referred to as a 'polymorphism'" (Charney, 2016, p. 3). Charney (2016) goes on to explain that:

> In a typical CGA study, a researcher proposes that those individuals with a given polymorphism are more likely to exhibit a given behavior. CGA studies have typically focused upon a small number of polymorphisms on the same regions of a small number of genes (e.g., MAOA, 5-HTT, DRD2, and DRD4) for two reasons: (1) These genes are transcribed to produce proteins involved in the regulation of neurotransmitters and thus are believed to be important for human behavior; and (2) researchers believed, on the basis of mouse and *in vitro* experiments, that they could associate polymorphic differences in regulatory regions of these genes with differences in the level of certain neurotransmitters and thereby with differences in behavior. In fact, attempts to associate these polymorphisms with actual differences in neurotransmitters in the human brain under physiological conditions have been largely unsuccessful. Nonetheless, this did not stop researchers from proposing, and claiming to have demonstrated, associations between the same polymorphisms of these neurotransmitter-related genes and a bewildering array of different behaviors.
>
> (p. 3)

In the face of such failures to replicate, Charney (2016) points out that, in 2010, John K. Hewitt, the then editor of the journal *Behavior Genetics*, wrote an editorial that in part said that:

> The literature on candidate gene associations is full of reports that have not stood up to rigorous replication . . . it now seems likely that many of the published findings of the last decade are wrong or misleading and have not contributed to real advances in knowledge.
>
> (pp. 1–2)

Nevertheless, Charney (2016) points out that through the time of his writing CGA articles continue to be published.

Another method used by behavior geneticists to identify the genes that purportedly determine behavior was also discussed briefly in Chapter 11, that is, genome-wide association studies (GWAS) (see Richardson, 2017). As noted in Chapter 11, this method involves using, typically, thousands of research participants who either manifest a specific behavior of interest (these participants are termed *cases*) or do not manifest this behavior (these participants are termed *controls*), that is, who manifest behaviors that can be "scored" dichotomously (e.g., does the person manifest, say, depression or schizophrenia, yes or no? (Joseph, 2015)). After such an identification is made, researchers then assess "large segments of the genome (a million or more base pairs) . . . in an attempt to find a polymorphism that *cases* . . . have to a greater extent than *controls*" (Charney, 2016, p. 4). In addition, Charney (2016) notes that most GWAS "search for single nucleotide polymorphisms (SNPs), a polymorphism involving the substitution of a single base pair in a given position on the DNA molecule" (p. 4).

Evan Charney

However, here again empirical failure is the main result of such research. Charney (2016) summarized the state of this literature by explaining that these studies:

> have been largely unsuccessful, leading to the so-called "problem of missing heritability": Despite the supposed heritability of all human behavior, behavior geneticists have been unable to find any substantial associations between polymorphisms and behaviors (Turkheimer, 2011).
>
> (p. 4)

Despite such failures, proponents of behavior genetics make claims without any independent empirical evidence that there are, for instance, over nine million SNPs involved in behaviors such as educational attainment (Charney, 2016; see too Richardson, 2017). In sum, then, Plomin's (2000) claims that (1) developmental science has accepted *his specific* view of the role of genes in human development and that, as well, (2) there are several well-replicated findings emanating from the field of behavior genetics (see Plomin et al., 2016) are unfounded.

The Continuing Story of Shortcomings of Heritability Analysis

Coupled with the failures of CGA and GWAS to provide data in support of behavior genetics views about the role of genes in human development are the myriad problems of heritability analysis. Some of the many problems with this method have been discussed in Chapter 11 in relation to purported genetic bases of intelligence (see Richardson, 2017, for a thorough review of this literature). However, there are other deficits of this method that are important to note.

Tabery (2014) explains that heritability analysis represents the essential approach of behavior genetics, that is, *variance partitioning*. As explained by Moore (2015b), this method seeks to divide (partition) the variance in some instance of the behavioral phenotype (e.g., intelligence, personality, character, voting, morality, or television watching) into the variance contributed to the phenotypic variance by genetics versus the variance contributed to the phenotypic variation by environment (which is typically broadly construed). Thus, the variance-partitioning approach harkens back to what Anastasi (1958) termed the "How much of each?" question (see Chapter 6). Tabery (2014) contrasts the variance-partitioning approach to understand the basis of human attributes with the approach associated with RDS-based research, that is, what Tabery (2014) labels *mechanism elucidation*. As explained by Moore (2015b), this approach engages the integrated, developmental system by seeking to understand the co-construction (Moore, 2015a) between the individual and the context (that is, individual↔context relations) involved in the *process* (note, not *mechanism*) through which all attributes of the phenotype (the whole organism) are *developed*.

This process-elucidation approach is aimed at explaining how variation within the holistic, relational development system eventuates in a specific attribute of an individual being developed. At best, the variance-partitioning approach can only describe purported sources of variance associated with differences across individuals in a specific attribute distribution. However, the variance-partitioning approach of behavior genetics cannot even validly fulfill its only purpose, that is, accurately partitioning variance in such a distribution. This approach (1) ignores the fact that heritability is a statistic that pertains only to interindividual differences and not to intraindividual change; (2) uses the statistic in ways that conflate description and explanation (e.g., describing covariation between two constructs—genes and a behavioral attribute, for instance—does not explain the basis of the covariation); and (3) has several important methodological problems.

James Tabery

Calculating Heritability

For the purposes of my discussion of the computational issues associated with heritability analyses, I will symbolize heritability by the term h^2. Typically, this is the symbol for what is termed *narrow heritability*, which is the ratio of *additive* genetic variance to the total phenotypic variance. Narrow heritability may be contrasted with *broad heritability*, which is the ratio of *total* genetic variance to total phenotypic variance (e.g., see Joseph, 2015; Moore & Shenk, 2016; Plomin et al., 2013; Richardson, 2017).

If all the variation in an attribute (e.g., intelligence) could be attributed to the concomitant variation within the gene distribution of the sample under study, then no variation whatsoever in the attribute would be due to environmental variation. In such a case, the value for environmental variation in the population would be zero and, accordingly, h^2 would equal one, or +1.0 (the "+" sign is not actually needed here, in that heritability coefficients range only from 0.0 to 1.0). Note, however, that, although there was no variation in the measure of the environment, and it thus contributed no variance to this particular heritability coefficient, this statistic does *not* mean that environment was not a basis of the attribute (Hebb, 1970; Moore, 2015b). The environment could have had an important impact on the attribute in question, even the *key* impact, although this influence might have been the same for all individuals in the sample.

Donald Hebb (1970) offered a useful example of the problems associated with interpreting high heritability as reflective of low environmental influence. He did so by drawing on a "modest proposal" put forth by Mark Twain:

> Mark Twain once proposed that boys should be raised in barrels to the age of 12 and fed through the bung-hole. Suppose we have 100 boys reared this way, with a practically identical environment. Jensen agrees that environment has *some* importance (20% worth?), so we must expect that the boys on emerging from the barrels will have a mean IQ well below 100. However, the variance attributable to the environment is practically zero, so on the "analysis of variance" argument [that is, the variance-partitioning approach], the environment is not a factor in the low level of IQ, which is nonsense.
>
> (Hebb, 1970, p. 578)

In Hebb's example, environment had no *differential* effect on the boy's IQs; presumably, for all boys, the environment had the same (severely limiting) effect. In having this same effect, environment could contribute nothing to *differences* between the boys. No differences—or variation—existed in the environment, and so the environment could not be said to contribute anything to differences between people. Yet, it is also obvious that environment had a major influence on the boys' IQ scores. Even with heritability equal to 1.0, the intelligence of each of the boys would have been different had he developed in an environment other than a pickle barrel.

As another example of how misleading heritability coefficients can be in regard to understanding the role of environmental influences, consider the following imaginary example. Suppose a society had a law pertaining to eligibility for government office. The law was simply that men could be elected to such positions and women could not. Consider what one would need to know in order to completely and correctly divide a group of randomly chosen people from this society into one of two groups. Group 1 would consist of those who had greater than a zero percent chance of being elected to a leadership post and Group 2 would consist of those who had no chance. All that one would need to know to make this division with complete accuracy was whether a person possessed an XX pair of chromosomes or an XY pair. In the first case, the person would be a female (since possession of the XX chromosome pair leads to female development). In the second case, the person would be a male. One could, thus, correctly place all possessors of the XY pair into the "greater than zero chance" group and all possessors of the XX pair into the "no chance" group.

Joseph (personal communication, May 3, 2017) offers an analogous, real-life example of this situation. At the time of writing, there were laws in Saudi Arabia prohibiting women from driving cars. Therefore, because only men drive cars there, all of the observed variation in car driving is due to whether a person has an XY or XX sex chromosome. This interindividual variation might lead behavioral geneticists to the absurd conclusion that car driving is a genetically determined (or at least a "highly heritable") attribute. Similarly, molecular geneticists might go on to discover that XY chromosomes are "associated with" the driving attribute, and then reach another absurd conclusion, that is, that XY chromosomes *cause* people to drive, and that XX people lack the ability to drive. Of course they are "associated," but the chromosome status does not cause a person to drive or not drive. Oppressive social policies based on gender discrimination do (see too Lerner & Overton, 2008, 2017).

In other words, in these examples, then, all the *variation* between people with respect to the characteristic in question—eligibility for elected office or car driving—can be summarized by genetic differences between them, that is, possession of either the XX or the XY chromosome pair. In these cases, the heritability of "being eligible for elected office" or "being able to drive" would be 1.0. In other words, in the two examples, eligibility for elected office or driving is 100% heritable. But, by any stretch of the imagination, does this mean that the eligibility or driving characteristic is inherited, or that the differences between men and women with respect to these characteristics are genetic in nature? Is there a gene for "eligibility" or "driving," one that men possess and women do not? Would behavior geneticists use the CGA method to search for evidence of an electability or driving gene?

Of course, the answers to all these questions are no, although, frankly, given the plethora of variables that have been the targets of CGA studies (Charney, 2016), I admit I am less certain about whether electability would be excluded from future instances of behavior genetics research using this method. In any case, although heritability in this imaginary case is perfect, it presents an instance analogous to the one Hebb (1970) provided about boys growing up in pickle barrels. The electability example indicates that contextual, social, or cultural environmental variables—laws regarding what men and women can and cannot do—determine whether someone has a chance of being elected. Genes do not determine electability. Indeed, if the law in question was changed, and women were then allowed to hold office, then the heritability of the eligibility characteristic would—probably rather quickly—fall to much less than 1.0.

Thus, h^2 values can range anywhere from 0.0% to 100% and, as the value of the coefficient increases, the proportion of the sample's phenotypic variation associated with genetic variation also increases. Of course, if no attribute variation could be attributed to genetic variation, all the attribute variation in a sample would have to be a function of environmental variation in the sample. Therefore, in this case, h^2 would equal zero, or 0.0.

By definition, the heritability of an attribute falls somewhere within the 0.0% to 100% range and, quite importantly, estimates of heritability of even the same measure of an attribute vary from sample to sample (e.g., Charney, 2016; Hirsch, 1963; Moore, 2002, 2006, 2013; Moore & Shenk, 2016). Therefore, *heritability is a property of samples and not of attributes* (Hirsch, 1963). Thus, an attribute of an individual cannot be appropriately spoken of as being heritable; researchers cannot correctly speak of the attribute of intelligence as being heritable. Rather, heritability only refers to the extent to which genetic variation among the members of a specific sample of individuals accounts for the distribution of scores for an attribute measured in a specific sample. By this same reasoning, an attribute of an individual cannot be appropriately spoken of as being inherited or genetic as a consequence of the computation of heritability estimates; researchers cannot correctly speak of the attribute of intelligence as being inherited or genetic (Joseph, 2015). In fact, Jensen (1969) made this same point. In discussing the appropriateness of applying the concept of heritability to a population of people and the inappropriateness of such application to any individual within that population, Jensen (1969) stated:

> Heritability is a population statistic, describing the relative magnitude of the genetic component (or set of genetic components) in the population variance of the characteristic in question. It has no sensible meaning with reference to a measurement or characteristic in an individual. A single measurement, by definition, has no variance.
>
> (p. 42)

Hence, heritability describes something about a group and not anything about an individual. Heritability relates to the source of differences among people in a sample; it says nothing about a given attribute (e.g., intelligence, personality, etc.) within any individual in that sample. Accordingly, as Moore and Shenk (2016) explain (see too Hirsch, 1963, 1970), the claim that if an attribute is heritable it is therefore inherited seems so obvious as to border on a tautology, or an assertion true by definition. However, nothing could be further from the truth: *The demonstration of heritability says nothing about the extent to which an attribute is inherited* (Lerner, 1992b; Lerner & von Eye, 1992, 1993). In fact, evidence for heritability cannot be taken as evidence for the common possession of a particular set of genes. There is no connection between the concept of heritability and the idea that a human's characteristics are caused by one or more genes (e.g., Moore, 2015b; Moore & Shenk, 2016).

Statistical Problems Associated with Heritability Analysis: Genotype–Environment Correlation

A facet of the problems associated with the calculation of heritability coefficients is the statistical problem of genotype–environment correlations. This problem is worthy of special focus because it represents an Achilles Heel for researchers engaged in making estimates of the heritability of attributes of human development. David Layzer's (1974) ideas have been particularly useful here. Layzer (1974) indicated that he assumes that:

> Some meaningful estimate of IQ heritability—high or low, rough or accurate—can be extracted from the reams of published statistics and that refinements of current techniques for gathering and analyzing test data may be counted on to yield increasingly reliable estimates. These propositions are by no means self-evident, however, and one of my purposes here is to demonstrate that they are actually false.
>
> This conclusion rests upon two arguments:
>
> One concerns the limitations of conventional heritability analysis, the other the validity of IQ scores as phenotypic measurements. Contrary to widely held beliefs, (i) heritability analysis does not require the genotype–environment interaction to be small, and (ii) a high phenotypic correlation between separated monozygotic twins does not, in general, imply that the genotype–environment interaction is small. If genotype–environment interaction does contribute substantially to the phenotypic value of an attribute (as there are strong biological reasons for supposing in the case of phenotypically plastic attributes), then a necessary and sufficient condition for the applicability of heritability analysis is the absence of genotype–environment correlation. This condition is rarely, if ever, met for behavioral attributes in human populations. The second argument is that IQ scores contain uncontrollable, systematic errors of unknown magnitude.
>
> (p. 1259)

Independent of Layzer's (1974) analysis of the errors of measurement found in IQ scores, it is instructive to focus here on his treatment of the genotype–environment correlation problem. Using the label *G* for genotype and *E* for environment, Layzer (1974) argued

> that *G* and *E* can be unambiguously defined if, and only if, genotype–environment correlations are absent. Even then, however, a certain practical ambiguity persists. Genetic differences may influence the development of an attribute in qualitatively distinct ways. For example, the curves labeled x_1, x_2, and x_3 [in Figure 12.1] have different thresholds, different slopes, and different final values. Heritability estimates do not take such qualitative distinctions into account. Thus, if the environmental variable *y* is distributed in a narrow range about the value as illustrated [in Figure 12.1], h^2 is close to unity. Yet in these circumstances the phenotypic variable could reasonably be considered to be largely environmental in origin since it is much greater than the

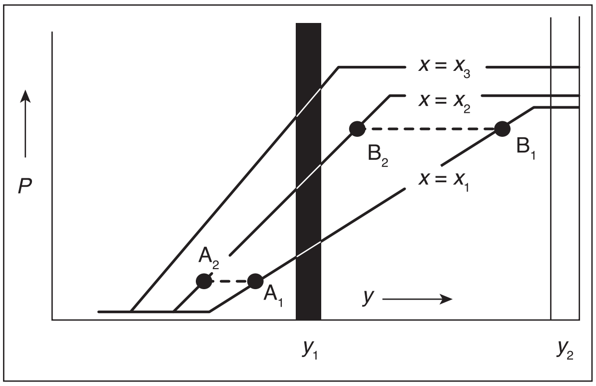


Figure 12.1 Phenotypic value (*P*) of a hypothetical metric trait as a function of an environmental variable (*x*) for three values of a genotypic variable (*y*). A_1 and A_2 (also B_1 and B_2) indicate individuals with a common phenotypic value but distinct genotypes x_1 and x_2, respectively.

Source: Layzer, 1974, p. 1260.

> phenotypic variance that would be measured in an environment ($y = y^2$) that permitted maximum development of the attribute, consistent with genetic endowment.
>
> (p. 1260)

Layzer (1974) then noted that plant and animal geneticists *can* minimize genotype–environment correlation by *randomizing environments*: thus, such researchers can take a step that is, according to Layzer, *indispensable* for the application of heritability analysis, since without such randomization there is just no means to disentangle genetic and environmental contributions to phenotypic variances. But this step is not done in IQ heritability research. Layzer (1974) explained:

> The applicability of heritability analysis does not, as is commonly assumed, hinge on the smallness of the interaction term (R) relative to the terms G and E in Fisher's decomposition of the phenotypic value. In fact, one may reasonably assume on biological grounds that genotype–environment interaction makes a substantial contribution to the phenotypic value of every phenotypically plastic attribute, except in populations where the ranges of genetic and environmental variation are severely restricted. Even so, heritability analysis can be applied to phenotypically plastic attributes, provided that the relevant genetic and environmental variables are statistically uncorrelated. When this condition is not satisfied, the contributions of interaction to phenotypic variances and covariances cannot, in general, be separated from the contributions of genotype and environment, and heritability analysis cannot, therefore, be applied meaningfully.
>
> In adult subpopulations, IQ and environment are well known to be more or less strongly correlated. Since differences in IQ are undeniably related to genetic differences (although not, perhaps, in a very simple way), one may safely assume that genotype–environment correlation is significant in adult subpopulations and in subpopulations composed of children reared by their biological parents or by close relatives. Hence, no valid estimate of IQ heritability can be based on data that refer to such subpopulations.
>
> Yet data of precisely this kind make up the bulk of the available material, and many published heritability estimates have been based on them. Burt (1966), Jensen (1969), and Herrnstein (1971) for example, all cite kinship correlation data as evidence for a high value of h^2.
>
> (p. 1263)

As I noted in Chapter 11, these interpretative problems are linked to others associated with the work of hereditarians when they discuss the heritability of IQ. Here, however, I should note that the existence of genotype–environment correlation presents a seemingly insurmountable problem in studies of the heritability of *any* human characteristic. Analyses of the heritability of human characteristics that rely on data sets wherein such correlations exist, therefore, result in flawed estimates.

Heritability Does *Not* Mean Inherited

As illustrated by the problems associated with the calculation of, and the statistical analyses associated with, heritability coefficients, the methodological problems of behavior genetics are interrelated with problems of inappropriate interpretation of the meaning of such statistics. That is, it is clear that heritability is a far less meaningful, more limited piece of information than most people seem to realize (Hirsch, 1970; Moore & Shenk, 2016). Most importantly, *heritability does not mean genetically determined.* Nevertheless, the assonance between the terms "heritability" and "inherited" (Hirsch, 1997) suggests one reason why the concept may be used in a confused and confusing manner. Indeed, for several reasons heritability *is* a difficult, confusing concept.

At first blush, it would seem to pertain to the extent to which something is inherited, that is, it is based in the genes. For instance, if one were told that "intelligence is 80% heritable" (Jensen, 1969), it might be reasonable to take this to mean that 80% of intelligence was genetically "determined." The seemingly reasonable interpretation that high heritability means that, as I noted Rushton (1999) claimed, "the differences are inborn and the environment has no effect" (p. 60), is a claim often made by some scientists, members of the media, and governmental policy-makers (e.g., see Horowitz, 2000). Nevertheless, this interpretation of heritability is *completely incorrect.*

However, in pertaining only to differences *between* people, heritability refers only to the extent to which differences between people in a specific characteristic can be summarized by genetic differences between these people. Lerner and von Eye (1992) provided a technical explanation of this claim, and noted that, despite the dazzling statistical pyrotechnics often involved in the computation of heritability estimates (e.g., Molenaar, Boomsma, Neeleman, & Dolan, 1990), these statistics, nevertheless, still only describe the extent to which interindividual differences in an attribute distribution, measured at one point in time and under one specific set of environmental conditions, are associated with interindividual differences in gene distributions. These analyses say *nothing* about the attribute per se. They say nothing about the role of genes in causing the interindividual differences in the attribute distribution. As such, Horowitz (2000) concluded that:

> The data reported in behavioral genetics studies involving degrees of relationships among twins, siblings, and biologically unrelated individuals are in themselves interesting, even if it is doubtful that these relationships tell us anything about the direct and unmediated impact of genes.
>
> (p. 3)

Nevertheless, despite the clear fact that heritability does not mean inherited (Hirsch, 1997), people building their scientific careers around the production of heritability analyses cast their work *as if* it, in fact, provided some understanding of the separate *and causal* role of genes in human behavior and development (Hirsch, 1990a, 1997). This "nature as separate from nurture" work cannot achieve such a partition; indeed, the entire idea of this work—of separating the contribution of nature from nurture in the causation of an individual's behavior—is simply counterfactual.

Yet, whatever the motivations of hereditarians for perpetuating the causal misuse of the concept of heritability, they often manifest lapses in language (Keller, 2010)—for example, moving from describing factors associated with interindividual differences in an attribute distribution to advancing genetically causal explanations for the behavior itself (e.g., see Rowe, 1994; Rushton, 1990). Indeed, as noted by Gottlieb (1992), "Although population thinkers tell us that, strictly speaking, h^2 and e^2 refer to sources of individual *differences* among phenotypes, as a matter of fact in actual practice these measures are often applied to a causal understanding of the outcome of individual development as well" (p. 117). Moreover, Gottlieb (1992) goes on to note that:

> If h^2 actually was useful for estimating genetic constraints or limitations on developmental outcomes, it would have some value, but it is widely agreed by geneticists themselves that h^2 cannot be interpreted in that way (Feldman & Lewontin, 1975), although individual scientists may now and again lapse into thinking in those terms (e.g., Gottesman & Shields, 1982).
>
> (p. 118)

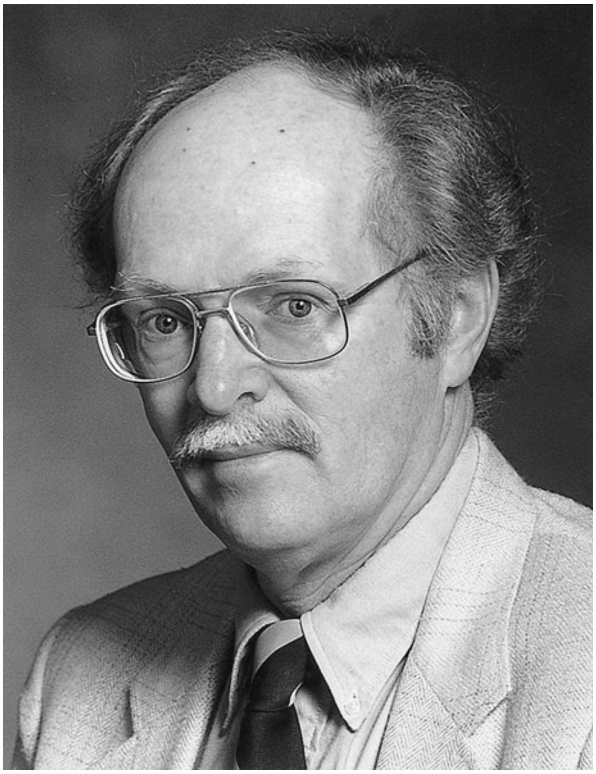
For instance, Plomin and colleagues (1990) conducted a behavior genetic study of individual differences in television viewing in early childhood in order to "explore the etiology of individual differences" (p. 372). Plomin and colleagues contended that their data provide evidence of a "genetic influence" (p. 371) on television viewing, and used phrases that often becloud the distinction between interindividual differences in a behavior and the behavior itself and, invariably, cast their descriptive information as if genetic causality had been demonstrated: "The remarkable result is the evidence for significant genetic influence . . . inherent proclivities of children are in part responsible for differences in the amount of time they choose to watch television" (p. 376).

From the vantage point of the scholarship presented by Gottlieb (e.g., 1992, 1997, 1998, 2004), the interpretation Plomin and colleagues (1990) gave of their data is not correct. Quite simply, it is counterfactual to contend that nature is separable from nurture, and counterproductive (to say the least) to devise statistical methods to model this imaginary (or, at best, hypothetical) situation; but, most important, genes do not, in reality, function in the manner that behavior geneticists must have them work if their "storylines" about the influence of nature are to attain even face validity. As Gottlieb (1992) pointed out:

> The actual role of genes (DNA) is not to produce an arm or a leg or fingers, but to produce protein (through the coactions inherent in the formula DNA → RNA → protein). The protein produced by the DNA → RNA → cytoplasm coaction then differentiates according to coactions with other cells in its surround. Thus, differentiation occurs according to coactions *above the level of DNA → RNA → coactions* (i.e., at the supragenetic level). (pp. 164–165)

Criticisms of Behavior Genetics Have Increased, Not Diminished

I noted that there are other reasons why Plomin's (2000; Plomin et al., 2016) views about the status and stature of behavior genetics are incorrect. These views are contradicted by the fact that the controversy regarding the legitimacy of behavior genetics—both as a conceptual frame for understanding the role of genes in behavioral development and as a methodology for studying the role of genes in behavioral development—has not diminished at all. Indeed, as research on epigenetics has continued to increase and to be extended to behavioral development (e.g., Cole, 2014; Lester, Conradt, & Marsit, 2016; Meaney, 2010, 2014; Moore, 2015a, 2016; Slavich & Cole, 2013), the decades-long criticism of the fundamental flaws of behavior genetics has become amplified. As such, in a review of David S. Moore's (2015) landmark book, *The Developing Genome: An Introduction to Behavioral Epigenetics*, Douglas Wahlsten (2015) concluded that the most important point made by the book was that scholars



Douglas Wahlsten

> need to think of the gene–environment relationship differently and reassess old ideas about development and evolution. He [Moore] advocates a broadened perspective in which gene–environment co-construction of traits is seen as universal and all dreams of simple genetic determination of behavioral phenotypes are finally dismissed. (p. 421)

Indeed, these criticisms of the "dreams" (Wahlsten, 2015) of behavior geneticists that were raised in the second decade of the twenty-first century are only more current instantiations of the criticisms that occurred in the very decades that Plomin (2000) claimed that negative critiques had abated. Scholars need only note the controversy associated with the publication of *The Bell Curve* (Herrnstein & Murray, 1994) or the criticisms leveled at the hereditarian views of J. Philippe Rushton (1996, 1997, 1999), which rely heavily on information derived from behavior genetics, to recognize that Plomin's (2000) "declaration of victory," or the comparable declaration made by Scarr (1987), more than a decade earlier, are inadequate attempts to either ignore or deny the persisting flaws of behavior genetics theory and method identified by scientists from numerous disciplines.

To illustrate, in a critique of the explanatory model and method associated with behavior genetic analyses of parent behaviors and the effects of parenting on child and adolescent development, Collins and colleagues (2000) noted that:

> Large-scale societal factors, such as ethnicity or poverty, can influence group means in parenting behavior—and in the effects of parenting behaviors—in ways that are not revealed by studies of within group variability. In addition, highly heritable traits also can be highly malleable. Like traditional correlational research on parenting, therefore, commonly used behavior-genetic methods have provided an incomplete analysis of differences among individuals.
>
> (p. 220)

Accordingly, Collins and colleagues (2000) concluded:

> Whereas researchers using behavior-genetic paradigms imply determinism by heredity and correspondingly little parental influence (e.g., Rowe, 1994), contemporary evidence confirms that the expression of heritable traits depends, often strongly, on experience, including specific parental behaviors, as well as predispositions and age-related factors in the child.
>
> (p. 228)

Rewriting History

Plomin's (2000) characterization of the growing acceptance of the behavior genetics view of the role of genes in human development in effect rewrites history. His assertion that it was not until the 1990s that behavioral science really came to accept the role of genes in behavioral development is incorrect. For at least a half-century prior to the period of the 1990s, genes had been accepted as part of the developmental system that propels human life across time and place (e.g., Anastasi, 1958; Maier & Schneirla, 1935; Novikoff, 1945a, 1945b; Schneirla, 1956, 1957). The issue is not the one that Plomin points to, then, that is, the issue of accepting that genes are involved in development. Instead, the issue is *how* do genes contribute to development. Plomin's (2000) approach and that of other behavior geneticists (e.g., Rowe, 1994) involves a split, nature-reductionist treatment of this issue. Developmental scientists working with models and concepts framed by the RDS metatheory take an integrated, dynamic view of genes within the relational developmental system (e.g., Lickliter, 2016; Mascolo & Fischer, 2015; Raeff, 2016; Overton, 2015a).

In fact, Plomin (2000) appears to point to the vacuity of the behavior genetics approach, at least as it has been pursued through the twentieth century. Although he maintains that "Twin and adoption research and genetic research using nonhuman animal models will continue to thrive" in the twenty-first century (Plomin, 2000, p. 30), Plomin perhaps admits to the serious flaws in this approach to understanding the role of genes in behavioral development when he acknowledges that, "The greatest need is for quantitative genetic research that goes beyond heritability, that is, beyond asking whether and how much genetic factors are important in behavioral development" (Plomin, 2000, p. 31). Plomin (2000) then continued by asking a series of important questions about the role of genes in behavioral development: "How do genetic effects unfold developmentally? What are the biological pathways between genes and behaviour? How do nature and nurture interact and correlate?" (p. 31). Unfortunately, he was seeking answers to these questions through the flawed model and methods of behavior genetics and, at least through 2016 (e.g., Plomin et al., 2016; Selzam et al., 2017), never explored the potential usefulness of an RDS-based approach. Nevertheless, such exploration would be very useful because Plomin (2000) admitted that it would be a major mistake

> to think that genes determine outcomes in a hardwired, there's-nothing-we-can-do-about-it way. For thousands of rare single-gene disorders, such as the gene on chromosome 4 that causes Huntington's disease, genes do determine outcomes in this hardwired way. However, behavioral disorders and dimensions are complex traits

> influenced by many genes as well as many environmental factors. For complex traits, genetic factors operate in a probabilistic fashion like risk factors rather than predetermined programming.
> (p. 33)

Thus, ultimately, Plomin (2000) admitted that a probabilistic-epigenetic relation is involved in accounting for the role of genes in behavioral development. Still, his views about, for instance, single-gene disorders reflect an ahistorical conception of such problems of human development. That is, in respect to other such single-gene disorders (e.g., as involved with phenylketonuria, PKU), genetic research has found means to counteract the problems produced by genetic inheritance, and has thus shown that a hard-wired genetic influence is not that hard-wired after all (Scriver & Clow, 1980a, 1980b). As such, Plomin (2000) maintains a narrow view of the relational developmental system; it apparently does not include the ingenuity of scholars to capitalize on the relative plasticity within the developmental system and to demonstrate that, what might seem to be hard-wired, is in reality amenable to change as a consequence of its embeddedness within a dynamic system. Nevertheless, in admitting to the importance of a relational development system in behavioral development, Plomin (2000) is, in actuality, defeating his own split approach to the nature and nurture of behavioral development.

Moreover, other scholars are not as convinced as is Plomin (2000) that the various methodologies he associates with behavior genetics will generate useful data. Consistent with the 2016 critique by Charney regarding the failures of the CGA and the GWAS methods, Collins and colleagues (2000) noted that:

> One criticism is that the assumptions, methods, and truncated samples used in behavior-genetic studies maximize the effects of heredity and features of the environment that are different for different children and minimize the effects of shared family environments . . . A second criticism is that estimates of the relative contributions of environment and heredity vary greatly depending on the source of data . . . heritability estimates vary considerably depending on the measures used to assess similarity between children or between parents and children . . . The sizable variability in estimates of genetic and environmental contributions depending on the paradigms and measures used means that no firm conclusions can be drawn about the relative strength of these influences on development.
> (pp. 220–221)

Similarly, and again counter to Plomin's (2000) assertion that the controversy surrounding behavior genetics faded by the 1990s, Horowitz (2000) noted that:

> One sees increasing skepticism about what is to be learned from assigning variance percentages to genes . . . The skepticism is informed by approaches that see genes, the central nervous system and other biological functions and variables as contributors to reciprocal, dynamic processes which can only be fully understood in relation to sociocultural environmental contexts. It is a perspective that is influenced by the impressive recent methodological and substantive advances in the neurosciences.
> (p. 3)

Conclusions

Clearly, many human developmentalists do not believe the causal "storyline" of behavior genetics. Nevertheless, "research" in behavior genetics—studies that, in effect, involve obtaining samples of people with differing degrees of biological relatedness and applying, typically, state-of-the-art measures of "traits" but inadequate measures of the ecology of human development (e.g., Bronfenbrenner & Ceci, 1994; Bronfenbrenner & Morris, 2006; Hoffman, 1991)—continues through this writing to be well funded and widely disseminated, both through articles in the best scientific journals and in books produced through excellent publication houses (e.g., see the critiques by Lerner, 2015b, 2015c, 2016; Moore, 2015b; Panofsky, 2014; Tabery, 2014).

But, behavior genetics is really like the story of the emperor's new clothes. Despite the positive regard some researchers hold for this area, there is actually "nothing there." The naked truth is that conceptual errors and misapplied models—no matter how often repeated or published—do not by dint of their numbers make for an adequate contribution to science. Moreover, behavior genetics cannot therefore be used as a basis for sound applications to social policy. Nevertheless, such pronouncements continue through this writing (e.g., Plomin et al., 2016; Rimfeld, Ayorech, Dale, Kovas, & Plomin, 2016). Accordingly, Greenberg (2005) warns:

> There is, as well, something insidious and unsaid about behavior-genetic analyses of human behavior, especially as they may influence social policy (Hirsch, 2004). Wahlsten (2003) made the point that behavior geneticists "airbrush" this point out of their discussions. He reminded his readers that there were sterilization laws in many states until recently and that successful educational programs such as Head Start have been jeopardized by genetic thinking (Wahlsten, 2003). "The most pernicious application of arguments for genetic influences on behavior has been the rationalization of unequal treatment of different groups—sometimes as horrific as slavery or extermination" (Dickens & Cohen, 2004, p. 151). All of these prominent behavior geneticists (Hirsch, of course, being one of the preeminent behavior geneticists of the twentieth century) cautioned against the misapplication of genetics to the understanding of the development of behavior. Gould (1981) has summarized the racist history of the misapplication of genetic ideas to the concept of intelligence.
>
> (p. 990)

Given the myriad theoretical and methodological problems associated with behavior genetics, little can be gained either for advancing the science of human development or for adequately informing or serving Horowitz's (2000) "person in the street" by continuing to invest resources in the behavior genetics approach. Indeed, there seem to be compelling reasons to make human and financial investments elsewhere, given, on the one hand, the counterfactual view of genetic activity inherent in behavior genetics, the several insurmountable conceptual and computational problems involved in its methods, and the lack of reliable empirical support for the claims or interpretations forwarded by behavior geneticists about the findings derived from their methods. On the other hand, the availability of the theoretically rich and empirically productive RDS-based alternatives to genetic reductionist approaches such as behavior genetics suggests that such work will not be useful in developmental science in the decades following the one in which this book has been written. I will reach similar conclusions as a consequence of my discussion of the other instances of genetic reductionism that I discuss in this chapter, that is, sociobiology and the related genetic-reductionist conception, evolutionary developmental psychology.

SOCIOBIOLOGY

In 1975, E. O. Wilson published a book that announced the "new" scientific discipline of sociobiology. Wilson (1975a) contended that sociobiology would be the "master" synthetic discipline, a field enveloping all of behavioral and social science. The claims made by Wilson (1975a, 1975b, 1980) and others (e.g., Rushton, 1999, 2000) in support of this synthetic role for sociobiology involved ideas pertinent to features of behavior central to human reproduction, parenting, and child caregiving.

The Scientific Goals of Sociobiology

Wilson's (1975a, 1975b, 1980; Lumsden & Wilson, 1981) claims about sociobiology, as well as corresponding assertions made by others (Konner, 1982; MacDonald, 1988), have evoked both approval (e.g., Rushton, 1987, 1988a, 1988b, 1990, 1991a, 1991b, 1995, 1996, 1999) and criticism (e.g., Barlow & Silverberg, 1980; Caplan, 1978; Kitcher, 1985; Lerner, 1992a; Lerner & von Eye, 1992, 1993), even among those who associated themselves with the "new synthesis." For instance, according to Dunbar (1987):

> Wilson created the impression that sociobiology was on the verge of replacing most of the disciplines in the social and behavioral sciences. This, of course, is arrant nonsense since sociobiology

> does not, of itself, deal with much of the subject matter of these disciplines.
>
> (p. 51)

However, Dunbar (1987) went on to defend the importance of Wilson's views for social and behavioral science: "Wilson was, none the less, right to emphasize the importance of sociobiology in relation to these disciplines. What it in fact does . . . is to provide a unifying umbrella under which these disciplines can interact on common ground" (p. 51). It is the process through which this unification is purported to occur that concerns me first. As noted in previous chapters, this process bears on the nature–nurture controversy.

E. O. Wilson

Genetic Determinism as Sociobiology's Key to Interdisciplinary Integration

In Wilson's (1975a) view, the "unifying umbrella" provided by sociobiology is the ubiquitous influence of genes on all facets of individual and social behavior and development. Indeed, Wilson's (1975a, 1975b) views exemplify a key conception of genetic determinism and genetic reductionism. The complexity of all social behavior and development and, indeed, all human culture (Lumsden & Wilson, 1981) can be reduced to a few simple genetic principles.

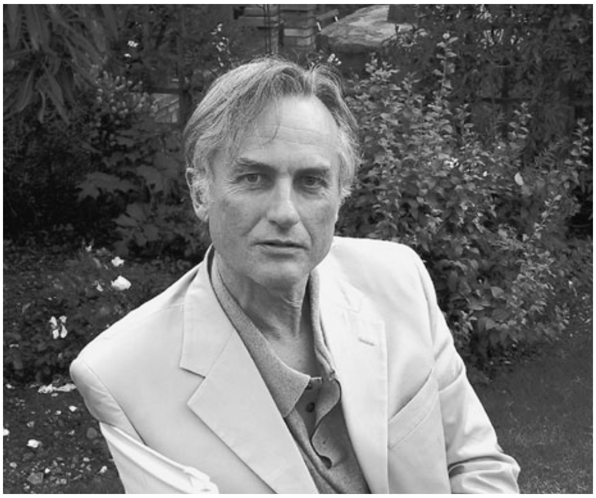
The core idea is one of gene reproduction. To Wilson (1975a, 1980), Dawkins (1976), and other sociobiologists (Barash, 1977; Freedman, 1979; MacDonald, 1988), the essential, core purpose of human life is *only* to reproduce genes. As Konner (1982) put it, "A person is only a gene's way of making another gene" (p. 265). Similarly, Dawkins (1976) sees humans as only "'survival machines'—robot vehicles blindly programmed to preserve the selfish molecules known as genes" (p. ix).

Simply put, in the wake of such anthropomorphism, all of human development reduces ultimately to gene reproduction and, as such, a human organism "does not live for itself" (Wilson, 1975a, p. 3). Instead, the organism's primary function is not even to produce other organisms per se. Rather, Wilson (1975a) claimed that the primary purpose of an organism is to produce genes. In fact, the organism is seen as only the temporary carrier of genes (Wilson, 1975a, p. 3). Through reproduction, the organism "transports" genes to another organism which, in turn, transports the genes to another organism, and so on.

According to this view, humans have not evolved to produce other people, but only to replicate some of their specific complement of genes. Humans' life spans represent only a relatively short period within the vast temporal span of evolution. During this time they provide a temporary "house," or a transport, for the genes carried within them. Given this machine-like view of the "human as transport," it is clear why Dawkins (1976) could see humans merely as "lumbering robots (housing genes that) created us, body and mind; and their preservation is the ultimate rationale for our existence" (p. 21).

On the basis of this core, gene reproduction principle, it is also evident why Dawkins (1976) considered genes to be selfish, although it is less evident why Dawkins decided that it was a conceptually and empirically useful scientific idea to write as if genes possessed intentionality, volition, and cognized, goal-directed purpose. Such attribution reflects the animistic thinking that Piaget (e.g., 1950, 1970) discussed as prototypic of the preoperational child. Interestingly, in the wake of criticisms of the selfish gene idea and Dawkins's (1976) claims about it, Dawkins (1982) admitted that he was doubtful that there could ever be an experiment that could prove his claims (see too Noble, 2015, regarding the shortcomings of these claims). In any event, to Dawkins, genes are "concerned" with nothing other than self-replication, with reproducing themselves over and over, as many times as possible. Simply, then, in this view humans are really just (seemingly complex) duplicating machines. Their mating rituals, their family relationships, and their cultural institutions are all inventions in the service of gene reproduction.

This core genetic principle of life leads to several other ideas about how genes influence individual behavior and the social world. In the terms of sociobiology, the more copies of a person's genes he or she can send out into the world, then, the more the person is increasing his or her *inclusive fitness.* In other words, the more copies of itself a genotype can transport into another generation of "gene reproducers" the greater is its inclusive fitness.



Richard Dawkins

This concept derives from the sociobiological view that natural selection is the essential vehicle through which evolutionary change occurs (Dawkins, 1976; Wilson, 1975a). However, not all genes are able to compete equally in the face of the fierce and rigorous challenges imposed by the natural environment; only the most aggressive genotypes will succeed in this struggle for survival. It is here, in the link between sociobiological ideas regarding inclusive fitness and aggression, that, as discussed in Chapter 11, the ideas of Konrad Lorenz (1940a, 1940b, 1965, 1966, 1974a, 1974b, 1974c 1975) play an important role in the shaping of sociobiological thinking.

Sociobiology and Human Aggression

A parallel arises between the views of sociobiologists (Dawkins, 1976; Konner, 1982) and the ideas of Konrad Lorenz. As discussed in Chapter 11, Lorenz (1966) saw human aggression as both inevitable and inherent in the human genome—to the point of providing for innate "militant enthusiasm." To Lorenz, not only do the genes of humans make them aggressive, but also the "highest form" of humans should be the most aggressive—the most militaristic—because such action reflects the presence of genes that have succeeded most in the struggles of natural selection. Similarly, in the sociobiological world of selfish genes, and the robotic "survival machines" that house them, aggression functions to allow genes to enhance their inclusive fitness; "blind" (i.e., unthinking, and machine-like) aggression allows genes to eliminate anything in the environment that interferes with their reproduction.

Thus, to Lorenz (1966) and to sociobiologists (e.g., Konner, 1982), selfish—indeed, ruthless—human aggression is the cornerstone of genes' control over human functioning. The most successful genes—the "best" of evolution, if you will (Lorenz, 1940b)—will be the most successful at ruthless aggression. They will be the most selfishly directed to maximizing their presence in the gene pool and, at the same time, to minimizing the genes of others. Of this blind, ruthless, militant aggression, Dawkins (1976) said:

> To a survival machine, another survival machine (which is not its own child or another close relative) is part of its environment, like a rock or a river or a lump of food. It is something that gets in the way, or something that can be exploited. It differs from a rock or a river in one important respect: it is inclined to hit back. This is because it too is a machine which holds its immortal genes in trust for the future, and it too will stop at nothing to preserve them. Natural selection favours genes which control their survival machines in such a way that they make the best use of their environments. This includes making the best use of other survival machines, both of the same and other species.
>
> (p. 71)

Similarly, and redolent of Lorenz's (1966) views in his book, *On Aggression*, Konner (1982) explained:

> I believe in the existence of innate aggressive tendencies in humans (p. 203) . . . if we are ever to control human violence we must first appreciate that humans have a natural, biological tendency to react violently as individuals or as groups, in certain situations.
>
> (p. xviii)

According to Lorenz (1966), these innate violent reactions are elicited in a reflex-like manner among either individuals or groups. The reflex occurs when members of an in-group are threatened by members of an out-group. Similarly, as I have noted, Dawkins (1976) contended that selfish genes impel humans to act aggressively against survival machines other than those of their own, close genetic group, and Wilson (1975a; see also Flohr, 1987, p. 199) likewise believes that fear of (and hatred toward) an out-group (xenophobia) is innate in humans. For example, in his book, *On Human Nature*, Wilson (1978) wrote of "hidden biological prime movers," and contended that:

> In all periods of life there is [a] . . . powerful urge to classify other human beings into two artificially sharpened categories. We seem able to be fully comfortable only when the remainder of humanity can be labeled as members versus nonmembers, kin versus nonkin, friend versus foe.
>
> (p. 70)

In sum, all genotypes must struggle arduously to include as many copies of themselves as possible in the gene pool. However, within sociobiological thinking all genotypes are not "created equal." That is, whereas all genotypes strive to maximize their inclusive fitness, genotypes differ in what is termed in sociobiology *gametic potential* (i.e., the potential of a genotype to replicate itself). Differences in gametic potential are associated with the differences that exist between males and females.

Sex Differences in Gametic Potential

Within sociobiology, it is held that men and women differ in their potential for transmitting copies of their genes into the future. For example, as claimed by Konner (1982, p. xviii), "as now seems clearly demonstrated, there are biological reasons why women, like other primate females, have a weaker aggressive tendency than males." According to sociobiologists, because aggression is the key to getting one's genotype reproduced maximally, it follows that women, lacking an aggressive ability sufficient to compete with men, must evolve some other strategy to enhance their inclusive fitness. Sociobiologists contend that the strategy that women use derives completely from the nature of the specialized cells used by women to transmit copies of their genes to future generations.

Genotype copies are contained in gametes, that is, sperms and ova. Both types of gametes function to maximize the inclusive fitness of the genotypes they carry. However, the two types of gametes have a different potential for such reproduction—due to the anatomical and physiological differences between the "lumbering robots"—men and women—housing these gametes. Men, who—it is of more than passing interest to note—were the founders and leading proponents of sociobiology, can generate a large number of genotype copies. Their gametes can be "sent forth to multiply" quite readily—millions can be sent out with each ejaculation. Thus, in the terms of sociobiology, their "gametic potential" is great, given that there is—at least theoretically—a ready, large pool of recipients of their gametes. Freedman (1979) put this idea as follows: "Since mammalian males produce many more sperm than females produce ova, any given male has far greater potential for producing offspring. He is also more inclined to compete with other males over the 'scarce' resource, females" (p. 2).

Simply put, then, any male has a greater potential for enhancing his inclusive fitness than any female, given males' greater gametic potential. Moreover, males must have, in general, a more aggressive genotype than females, because they must compete for access to the female gamete, viewed as a "resource" for the deposit of the males' sperm. Such competition is, of course, highly desirable in the view of sociobiologists, because it ensures that the most aggressive genotypes—those best suited to succeed against the struggles of natural selection—will reproduce most often.

In turn, the genotypes of females impel women to try to reproduce in quite a different way. One might understand the origin and development of this "alternative," female reproductive strategy if one asks these questions:

> Given the vast difference in reproductive potential, and if the point of life is to actualize such potential, is it not reasonable to expect that on the average the male pattern of courtship will differ from the female? Might nature not have arranged it so that men are ready to fecundate almost any female and that selectivity of mates has become the female prerogative?
>
> (Freedman, 1979, p. 12)

In answer to such questions, van den Berghe and Barash (1977) noted:

> Human females, as good mammals who produce a few, costly and therefore precious, offspring, are choosy about picking mates who will contribute maximally to their offspring's fitness, whereas males, whose production of offspring is virtually unlimited, are much less picky.
>
> (p. 813)

Gametic Potential and Social and Sexual Development

What does the sexes' different gametic potential imply for understanding male and female social behavior and development? Given the selfishness of genes and the single-minded direction of the duplicating machines housing them, men develop sexual mores dictating the acceptability (if not the appropriateness) of multiple sexual partners. Indeed, van den Berghe and Barash (1977) argued that the different gametic potential of men and women explains

> the widespread occurrence in human societies of polygamy, hypergamy, and double standards of sexual morality. There is another related reason for the sexual double standard in such things as differential valuation of male and female virginity and differential condemnation of adultery: "marital infidelity of the spouse can potentially reduce the fitness of the husband more than that of the wife. Women stand to lose much less if their husbands have children out of wedlock than vice versa (p. 813) . . . In addition, a woman will, at a maximum, produce some 400 fertile eggs in her lifetime, of which a dozen at most will grow up to reproductive age, while a man produces millions of sperm a day and can theoretically sire hundreds of children. Not surprisingly, females tend to go for quality, and males for quantity.
>
> (p. 814)

Moreover, given the large number of offspring they can potentially produce, a male's parental investment in any one offspring is quite small. Unfortunately for the recipients of males' genetic copies—women—their gametic potential is quite different, and so, too, is their parental investment. They can replicate themselves at most every nine months. Even with multiple births, a woman cannot replicate her genes as much in a lifetime as a man can in a short period of time. Therefore, according to sociobiologists, a woman's investment in her offspring is much greater than is a man's. Moreover, sociobiologists also contend that because women cannot reproduce very frequently, women will not be motivated toward frequent copulation with multiple partners.

Instead, it is believed that women need to protect their offspring and assure their survival, and that this need should motivate them to keep their impregnators bound to them. As a consequence, the view of sociobiologists is that females develop monogamous sexual behaviors and a devotion to childbearing and rearing. Van den Berghe and Barash (1977) argued:

> For a woman, the successful raising of a single infant is essentially close to a full-time occupation for a couple of years, and continues to claim much attention and energy for several more years. For a man, it often means only a minor additional burden . . . [M]ost societies make no attempt to equalize parental care; they leave women holding the babies.
>
> (p. 813)

Lest anyone contend that the different moral, sexual, and social developments of men and women are merely products of socialization, Barash (1977) argued that the sex differences in gene reproduction strategy explain "why women have almost universally found themselves relegated to the nursery, while men derive the greatest satisfaction from their jobs" (p. xv). Van den Berghe and Barash (1977) further noted that "ethnographic evidence points to different reproductive strategies on the part of men and women, and to a remarkable consistency in the institutionalized means of accommodating these biological predispositions" (p. 815). Van den Berghe and Barash (1977), therefore, concluded:

> Men are selected for engaging in male–male competition over resources appropriate to reproductive success, and women are selected for preferring men who are successful in that endeavor. Any genetically influenced tendencies in these directions will necessarily be favored by natural selection.
>
> (p. 814)

Dawkins (1976) embellished these ideas by contending that women's exploitation by men is biologically determined. He argued that the sexes' behavioral developments are differentiated not only by the different number of sex cells that can be used for genotype reproduction but also by the different size of their respective sex cells:

> The sex cells or "gametes" of males are much smaller and more numerous than the gametes of females . . . it is possible to interpret all the other differences between the sexes as stemming from this one basic difference.
>
> (Dawkins, 1976, p. 152)

> Sperms and eggs . . . contribute equal numbers of genes, but eggs contribute far more in the way of food reserves: indeed sperms make no contribution at all, and are simply concerned with transporting their genes as fast as possible to an egg . . . Female exploitation begins here.
>
> (Dawkins, 1976, p. 153)

Sex differences in the gametic potential and size of the gametes result not only in female exploitation in general but, in particular, in the legitimization of extramarital sex for males, but not for females, and the use of violence toward wives who have extramarital sexual relations. To explain these sex differences, Freedman (1979) argued:

> We have to assume that cultural universals reflect those aspects of our species that were evolutionarily derived (evolved). Male promiscuity is universally winked at because there is nothing much we can do about it, and Kinsey's (Kinsey et al., 1953) main findings appear to be descriptions of the species: males must have "frequent outlets" for sex, whether heterosexual or homosexual; whereas many females can go for long periods without copulation or masturbation . . . And this difference appears to hinge on the difference in gametic potential that we have been discussing (p. 19) . . . As in the gelada baboon, in humans female jealousy is based not on the male's sex act with another woman but on his potential attachment to the latter . . . Male jealousy is rather different . . . It does not make evolutionary sense for the male to invest in a child not possessing his genes and the murderous jealousy exhibited by a cuckolded male is biologically sensible. Furthermore, the cuckold's retribution can strike either the female or the male cheater . . . and most legal systems (perhaps all patrilineal systems) wink at the ensuing violence.
>
> (pp. 20–21)

Dawkins (1976) extended across the life span the idea of the biological basis of men's promiscuous sexual interests. He offered both "a possible explanation of the evolution of the menopause in females" (p. 136) and, at the same time, an account of the sociobiological basis of the existence of what are colloquially (and pejoratively) termed "dirty old men":

> The reason why the fertility of males tails off gradually rather than abruptly is probably that males do not invest so much as females in each individual child anyway. Provided he can sire children by young women, it will always pay even a very old man to invest in children rather than in grandchildren.
>
> (p. 136)

Sociobiology, Genetic Determinism, and Human Development

In the theory of sociobiology advanced by Wilson (1975a, 1975b), men—impelled mechanistically by their genes—are oriented to seek sexual relations with as many women as possible, to achieve more and more copies of their genes, and to not be overly devoted to or concerned with any one or any few given "replicates." Women, in contrast, are oriented to remain monogamous in order to maximize the probability that their relatively few replicates will survive. In essence, then, men and women are genetically impelled to differ in ways that are consistent with traditional (i.e., stereotypic) sex-role patterns.

As was the case in regard to Freud (1923) and later Erikson (1968), Wilson (1975a, 1975b) in effect holds that "anatomy is destiny" regarding key features of behavioral development—ones involving reproduction, parenting, child caregiving, and sexuality. In other words, Wilson (1975a, 1975b), Dawkins (1976), Freedman (1979), and other sociobiologists (e.g., Barash, 1977; Konner, 1982; MacDonald, 1988; Rushton, 1999, 2000) built a genetic reductionist-based edifice encompassing the very core of all human behavior and development—the reproduction of men and women, the character of the family, and the survival of the species. Any notions of nurture or of nature↔nurture fusion as sources of key features of human behavior are mere fictions, if genes work in the way that sociobiology requires, that of selfish, goal-directed, and intentional agents. According to sociobiology, after other, more superficial "causes" of human behavior are stripped away (e.g., "causes" involved in an individual's development such as character or moral values), genes provide the ultimate basis for human functioning: the replication of genotypes.

According to this conception, the social world does not coact in a fundamental causal manner with humans' genes, much less act as an alternative source for human development. Instead, according to sociobiologists, the social world—human mores (e.g., regarding sexual permissiveness or monogamy), social institutions (such as marriage and the family), and, indeed, all of human culture—is nothing other than the outcome of strategies laid down by humans' genes for their own replication. In short, sociobiologists have complete faith in the inevitable reducibility of human behavior to the functioning of selfish genes.

Akin to the ideas of Lorenz (1966), this genetic-determinist view has necessarily xenophobic and ruthlessly (if not militantly) selfish implications for society. The faith in genetic determinism and reductionism maintained by sociobiologists is expressed by Dawkins (1976) in his claim that "It can be perfectly proper to speak of 'a gene for behavior so-and-so' even if we haven't the faintest idea of the chemical chain of embryonic causes leading from gene to behaviour" (pp. 65–66). Dawkins (1976) also stated: "Be warned that if you wish, as I do, to build a society in which individuals cooperate generously and unselfishly towards a common good, you can expect little help from biological nature" (p. 3).

To what extent is this sociobiological view of human development, and of society, supported by scientific evidence? Asked another way, what scientific evidence do sociobiologists draw on to legitimate their claims, and how adequate is this evidence?

Evaluating Sociobiological Claims

Given Wilson's (1975a, p. 4) original definition of sociobiology as "the systematic study of the biological basis of all social behavior," it may seem surprising, and perhaps contradictory, to learn that Wilson (1980, p. 296) also contended that "contrary to an impression still widespread among social scientists, sociobiology is not the theory that human behavior has a genetic basis." Perhaps, Wilson (1980) was just playing with words. Perhaps, he meant that sociobiology is not a "theory" but only a "perspective," or merely a rather general framework within which to systematically study the biological and, therefore, ultimately, the genetic basis of all social behavior. Whether his statement pertaining to social scientists' mistaken impressions about sociobiology rests on a difference in meaning between the phrases "the theory that . . ." and "the systematic study of . . .," Wilson's own words show that sociobiology is the study of the role of the connection between genes and human social behavior. Wilson (1980), in fact, used the term *sociobiological theory* to represent this linkage. He claimed:

> Real sociobiological theory allows no less than three possibilities concerning the present status of human social behavior: (a) During the rapid evolution of the human brain, natural selection exhausted any genetic variability of the species affecting social behavior, so that today virtually all human beings are identical with respect to behavioral potential. In addition, the brain has been "freed" from these genes in the sense that all outcomes are determined by culture. The genes, in other words, merely prescribe the capacity for culture. (b) Genetic variability has been exhausted, as in (a). But the resulting uniform genotype predisposes psychological development toward certain outcomes as opposed to others. In an ethological sense, species-specific human attributes exist and, as in animal repertories, they have a genetic foundation. (c) Genetic variability still exists, and, as in (b), at least some human behavioral attributes have a genetic foundation.
>
> Having identified these alternatives, and stressed the freedom of the discipline of sociobiology from the necessity of any particular outcome, I can now add that the evidence appears to lean heavily in favor of alternative (c).
>
> (Wilson, 1980, p. 296)

In the case of each of the given options—(a), (b), and (c)—emphasis is placed on the links among evolution, genetic variability, and human development and society. However, if sociobiologists have spent a good deal of time exploring the first two of the three options, such work has not found its way into the published literature. Hence, Wilson is correct in asserting that, to the extent that "evidence" exists in support of any of the three options, it does so in regard to Option (c). Yet, support for (c) does not exist because the three options have been repeatedly subjected to comparative scientific analyses. Rather, the preponderance of published sociobiological work—at least insofar as the human literature is concerned—has taken as its "working assumption" Option (c). The "evidence" derived from such work constitutes not a test of competing hypotheses but, rather, an attempt to bring empirical observations to bear on a demonstration of a guiding presupposition.

That is, given what are quite well-known facts of genetic variability (e.g., McClearn, 1981), it would be nothing short of preposterous to conduct a scientific investigation predicated on the idea that genetic variability does not exist. As a consequence, I do not believe it plausible that either Wilson or other sociobiologists are not fully aware of this quite basic evidence about the existence of immense human genetic variability. Consequently, it is equally difficult to envision that any serious scientific attention could be paid by sociobiologists to Options (a) or (b). Therefore, these two options cannot be, and, as I indicated, are not treated as, viable counters to (c). Instead, this last conception is the only one actually pursued scientifically by sociobiologists. But, given that no alternatives are really comparatively tested, such pursuit is more a demonstration of how empirical phenomena coincide with a conceptual presupposition than a critical test of theoretical options. How do such demonstrations proceed? Three types of evidence have been invoked.

Comparisons of Humans and Nonhumans: The Concept of Homology

One way in which sociobiologists demonstrate that human social behavior is constrained by evolutionarily shaped genes is to draw parallels between the behaviors of humans and nonhuman animals. If the behaviors of distinct species can be described similarly, it is argued that there must be some evolutionary connection, or continuity, between them. A common evolutionary pathway for a physical structure or a behavioral function in distinct species is termed a *homology.* Simply, then, sociobiologists argue that if the characteristics of two species can be described in a common way, evidence is present of homologous evolution. The positing of such homology is offered as proof that the characteristics in question are controlled, or constrained, by evolutionarily-shaped genes.

The use of such "evidence" is exemplified in the writing of Freedman (1979) and Rushton (1999, 2000). For instance, Freedman (1979) attempted to document his views that human males' gametic potential gives rise to sexually promiscuous behavior—in order to increase their opportunities to garner the "scarce resource" of females' ova—whereas human females' gametic potential makes them more monogamous. In support of his idea, Freedman claimed that he found homologies between fruit flies, rhesus monkeys, and South American jungle-dwelling, polygynous humans. Freedman (1979) argued that in all species:

> Females tend to cluster about an average number of young whereas males form a greatly skewed curve, some very successful, many not successful at all. And, since most mammals are polygynous . . . this tendency may characterize the entire class Mammalia.
>
> (p. 13)

Freedman (1979) carried his argument one step further. By again using what he regarded as common behavioral descriptions across species, he attempted to provide an evolutionary and genetic account for inevitable human male promiscuity and also for the genetically preordained urge to seek sexual relations with other females, even to the point of forcing oneself onto them (i.e., committing rape) (Freedman, 1979). First, Freedman (1979) cites the work of Grzimek (1972) that: "In spring, when the gonads are at the peak of their development, there are attempts to 'rape' strange females in the mallard and pintail and a few other species (Grzimek, 1972, p. 270)" (p. 14).

Second, Freedman made an inference about the "promiscuous, polygynous intentions" of ducks and, finally, drew a conclusion about the insatiable, continuous, and carnal search by human males for females with whom to copulate. Freedman (1979) contended:

> It would appear that if the mallard drake had his way his would be a polygynous species and, in fact, one does occasionally see a consortship of two females and a male. In our own species and our own culture, I am asserting nothing startling when I point out that with sexual maturity, most heterosexual males are in constant search of females, and if inhibited about sexual contact, they fantasize almost continuously and fairly indiscriminately about such contact . . . adolescent males in our culture frequently experience life as a nearly continuous erection—spaced by valleys of depression that accompany sexual disappointment.
>
> (p. 14)

Are these descriptions, and those by other sociobiologists (Barash, 1977; Wilson, 1975a), of purportedly comparable human and nonhuman social behavior, satisfactory proof of the evolutionary and genetic bases of human behavior? Does apparent descriptive similarity establish evolutionary homology? The answer to both of these questions is no, for several reasons, not the least of which is the difficulty of accumulating sound scientific evidence of common evolutionary descent when only physical attributes are being considered (Atz, 1970; Gould, 1980). The task is even more problematic in the case of behavioral characteristics, as even very similar behaviors (a) may be manifestations of quite different processes, and/or (b) may serve different functions (Bitterman, 1965, 1975).

In regard to (a), it is a truism that one can describe similar behaviors across even vastly different species.

For instance, a conclusion that can be drawn from the work of Bitterman (1965, 1975; see Chapter 8) is that insects, fish, rats, and humans all "learn"; that is, in members of each of these species, systematic and relatively permanent changes in behavior occur in relation to experience. Nevertheless, the ways in which these species learn—the processes of learning—vary considerably. For example, it would be difficult to contend that thought processes play a part in the learning of insects at any point in their lives. In turn, it would be equally difficult to argue that cognition does not enter into human learning perhaps for anything other than the earliest weeks of the life span; however, even in this portion of ontogeny cognition may play a role (Piaget, 1970).

Accordingly, although experience-based changes occur in all animals' adjustment to the environment, this similarity is at best evidence for an analogy, not a homology (Atz, 1970; Schneirla, 1957). In other words, different processes may subserve analogous functions. But to claim that such descriptive analogies are indicative of common evolutionary histories is, at best, naive, and, at worst, poor scholarship. Dunbar (1987) was frank in admitting this limitation in sociobiological scholarship:

> Many of those who were influential in promoting the sociobiological perspective . . . (e.g., E. O. Wilson) tend to be unaware of the more sophisticated nature of the behaviour of higher organisms and are apt to regard even advanced mammals simply as scaled-up insects.
>
> (p. 53)

In turn, and in regard to the aforementioned Point (b), the presence of identical behaviors in different organisms does not constitute proof for even common function or purpose. To illustrate, the reasons that male mallard ducks might force copulation upon a female of their species are certainly distinct from those involved when a human male rapes a human female. Indeed, to label both the male duck's behavior and the actions of the human male with the same term (rape) seems to trivialize, through biological reductionism, what is certainly a complex and violent human act, one that may not even be a behavior predicated in any way on sexuality or sexual feelings (Sunday & Tobach, 1985).

Can Freedman (1979), Barash (1977), or other sociobiologists who argue for homology on the basis of such cross-species descriptions, contend that the devaluation of women in many sectors of modern society, and the legitimization of violence as a means of exercising social (and political) control, do not enter into the primary causation of forced copulation by human males and/or that they do enter into the basis of such behaviors in ducks? I think not. Simply stated, the mere portrayal of behaviors in two species as appearing comparable is no proof at all of their common evolutionary heritage. Nor is it any proof at all regarding the extent to which such behaviors are genetically constrained or produced. Indeed, this conclusion seems to have been reached by Wilson (1980) himself. He noted that: "We cannot rest the hypothesis of genetic constraint in human social behavior on the indirect evidences of homology" (p. 297).

If the sociobiologists' behavioral homologies do not constitute adequate proof for the genetic basis of human social behavior, what then does? Two other types of evidence have been offered, ones pertaining to the concepts of heritability and adaptation. I consider heritability first.

Sociobiology and Heritability Analyses

The myriad conceptual and methodological problems associated with heritability analyses of human behavior have been discussed in Chapters 11 and this chapter. I need not reiterate here the information I discussed about the counterfactual view of genetic functioning, the flawed reasoning, and the methodological shortcomings associated with heritability research. I may simply note that, in relying on heritability as a source of support for their hereditarian views, sociobiologists are, in effect, relying on no evidence at all (e.g., Hirsch, 1997).

Nevertheless, Wilson (1980) argued that data from heritability research supported the third of the three possible theoretical options upon which sociobiology rests. The notion was that genetic variability exists and, as such, at least some human behavioral attributes have a genetic foundation. Accordingly, Wilson (1980) saw that heritability research not only supported the presence of genetic variability but also that it did so in a manner supporting the hereditarian claims of sociobiology. This seemingly straightforward perspective evokes, in actuality, a thicket of conceptual confusion.

First, sociobiologists do not have to look to behavior genetics to document the clear fact that genetic variability exists. In fact, behavior genetics and its use of research about heritability do not provide proof about the presence of human genetic variability—molecular genetics and population genetics provide this information. Heritability analysis capitalizes on (begins with the acknowledged fact of) genetic variability and then seeks to partition this variability into hereditary and environmental sources (Tabery, 2014).

Second, however, sociobiologists' reliance on the findings of heritability research as offering support for their views is completely ill-conceived. Sociobiologists wish to talk about behavioral attributes that are common to a species. The task of the sociobiologist is to show scientifically that such attributes uniformly and unequivocally characterize the subgroups of humans in question (e.g., males and females), and do so because of the possession of evolutionarily based genetic "directives" for genotype reproduction.

Stated simply, sociobiologists wish to demonstrate that some human attributes (i.e., ones common to a given group and dealing with that group's reproductive strategy and, hence, inclusive fitness) have a genetic basis. In other words, sociobiologists want to demonstrate the common, or invariant, inheritance of these attributes; but, in relying on evidence from the study of heritability, they are using information that capitalizes not on commonality of inheritance but on its variability!

In essence, then, sociobiologists are trying to claim support for the importance of invariant heredity for human characteristics by pointing to evidence that shows there is variation in heredity. Hirsch made a similar point. Hirsch (1997) noted that "The misleading picture that emerged in *Sociobiology* was that heritability is the very essence of evolution" (p. 210). Hirsch (1997) argued that this depiction of the connection between sociobiology and evolution was flawed because:

> Wilson was downright irresponsible in his failure to emphasize the inherent contradiction in this picture, namely that the important characters have the lowest heritabilities. In the words of his own source "characters with the lowest heritabilities are those most closely connected with reproductive fitness, while the characters with the highest heritabilities are those that might be judged on biological grounds to be the least important determinants of natural fitness" (Falconer, 1960, p. 167).
>
> (p. 210)

Similarly, Collins and colleagues (2000) pointed out that "genetic factors that are highly important in a behavior do not show up in a study of heritability of that behavior because the genetic factor is uniform for all members of a population. Thus, analyzing the variation of a factor within a population does not provide exhaustive information concerning either the genetic or the environmental contributions to the factor.

> (p. 220)

Accordingly, there are insurmountable logical, conceptual, methodological, and empirical problems involved in sociobiologists' reliance on data derived from heritability research for evidence in support of their version of hereditarian claims. As such, neither this line of "evidence" nor that provided by work associated with the concept of homology can be used by sociobiologists to support their ideas about human behavior and development. There is, then, only one possible line of evidence left for them to use to establish the validity of their ideas: adaptation.

Are Adaptations Everywhere?

A cornerstone of the sociobiological "method" is to offer explanations in the vein of Rudyard Kipling's "Just-So Stories" of how particular social behaviors, or differences among people in their social status or roles, came to be (Gould, 1980). As recounted by Gould (1980):

> Rudyard Kipling asked how the leopard got its spots, the rhino its wrinkled skin. He called his

> answers "just-so stories." When evolutionists try to explain form and behavior, they also tell just-so stories—and the agent is natural selection. Virtuosity in invention replaces testability as the criterion for acceptance.
>
> (p. 258)

According to Gould (1980), this unacceptable scientific procedure led the biologist von Bertalanffy (1969) to complain:

> If selection is taken as an axiomatic and a priori principle, it is always possible to imagine auxiliary hypotheses—unproved and by nature unprovable—to make it work in any special case . . . Some adaptive value . . . can always be construed or imagined . . . I think the fact that a theory so vague, so insufficiently verifiable and so far from the criteria otherwise applied in "hard" science, has become a dogma, can only be explained on sociological grounds. Society and science have been so steeped in the ideas of mechanism, utilitarianism, and the economic concept of free competition, that instead of God, selection was enthroned as ultimate reality.
>
> (p. 11)

According to both Gould (1980) and von Bertalanffy (1969), the key feature of sociobiological "just-so stories" is that these current arrangements in society are adaptations; that is, adaptations are changes that enhance fitness, that have been shaped by natural selection over the eons of human evolution to have this function, and that are now represented in the human genotype. Yet, it is the key element in these arguments—the presence of an adaptation, of a change in fitness—that all too often remains a scientifically unverified, post hoc story.

Indeed, as admitted by Dunbar (1987):

> A simple statement that X increases the fitness of those that perform it explains nothing: it is strictly tautologous for improving fitness is what every sociobiological explanation implicitly assumes. What we need to know—and this is the heart of any sociobiological explanation—is: How does it increase fitness?
>
> It is the transparent failure to answer this question that has left so many sociobiologists open to criticisms of "Just-So" story-telling and unscientific practice. Since we necessarily have to rely on comparative observations rather than experimental manipulation when tackling evolutionary problems, we are particularly exposed to this kind of accusation. The only way to avoid it is to provide as watertight a case as is possible by showing that proximate problems of survival or reproduction are in fact resolved when individuals behave in a specified way, and that efficient solutions to these problems will result in increased contributions to the species' future gene pool. This will not always be easy, but, unless it can be done, sociobiological explanations will always be open to skeptical doubts, particularly where these doubts are fuelled by political or religious conviction.
>
> (p. 50)

Despite these explanatory difficulties, sociobiologists see adaptations—changes in fitness "designed" by (or, actually, "resulting" from) natural selection—as being everywhere. In the view of sociobiologists, these changes in fitness, because they are adaptations, are optimizations. That is, as argued as well by nineteenth-century Social Darwinists (Tobach et al., 1974), natural selection results in genetically-based features that are the "time-tested," best possible outcomes of humans' evolutionary history.

According to sociobiologists, then, that which exists *is* an adaptation: Humans' social behaviors and the niches they occupy in the social hierarchy have been shaped by natural selection to take their present form. As claimed succinctly by Konner (1982), "An organism has characteristics, they must have been selected for or they wouldn't be here now" (p. 18).

Given this centrality of the concept of adaptation in sociobiologists' thinking, is there a direct, uniform, and singular pathway that sociobiologists infer from evolution, through natural selection, to adaptation and the present character of people and society? In addition, why is presenting a story—which is a possible scenario of the way natural selection *could have* resulted in a given feature of human behavior—not sufficient to establish scientifically that just such a history transpired?

The Concept of "Exaptation"

The work of Gould and Vrba (1982) is quite relevant to these questions. They provided a new term in evolutionary biology in order to clarify some important, but confusing, uses of the term *adaptation.* Gould and Vrba noted that one meaning of adaptation is the shaping of a feature of the organism (e.g., a physical attribute or a behavior) by natural selection for the function it now performs. A second meaning is a more static one, and refers to the immediate way in which a physical feature or a behavior enhances the organism's current ability to fit its context. This second meaning does not take into account the historical origin of the feature, but only whether the organism's physical or behavioral characteristics help it to meet the current demands of its environment.

Gould and Vrba (1982) cited Williams (1966) as adhering to the first definition of adaptation. Williams (1966) contended that one should speak of adaptation only when one can "attribute the origin and perfection of this design to a long period of selection for effectiveness in this particular role" (p. 6). Bock's (1979) views illustrate the second definition of adaptation. Bock indicated that "an adaptation is . . . a feature of the organism . . . which interacts operationally with some factor of its environment so that the individual survives and reproduces" (p. 39).

Gould and Vrba (1982) claimed that a confusion, therefore, exists regarding a central concept in evolutionary theory—adaptation. This conflict exists because the single term "adaptation" has been used when, in fact, there are different criteria for the historical basis of a given organism's feature and for its current use. Darwin (1859) himself may have seen this potential confusion:

> The sutures in the skulls of young mammals have been advanced as a beautiful adaptation for aiding parturition, and no doubt they facilitate, or may be indispensable for this act; but as sutures occur in the skulls of young birds and reptiles, which have only to escape from a broken egg, we may infer that this structure has arisen from the laws of growth, and has been taken advantage of in the parturition of the higher animals.
>
> (p. 197)

In other words, although Darwin saw the necessity of unfused sutures in the skulls of young mammals, he was uncertain about labeling the unfused sutures as adaptations. This uncertainty occurred because the unfused sutures were not built by selection to function as they now do in mammals (Gould & Vrba, 1982). But if the unfused sutures are not adaptations, if they were not shaped by natural selection, what are they and where did they come from? Clearly, a new term must be used to rectify the confusion, and Gould and Vrba (1982) provided one. They suggested that such characters evolved for other usages (or for no function at all), and were later "coopted" for their current role. They termed such characters *exaptations.* The characters are fit for their current role (i.e., they are *aptus*), but they were not designed by natural selection for this role, therefore, they are not *ad aptus* (i.e., pushed toward fitness by natural selection).

To illustrate, it is useful to consider exaptation pertinent to the features of microevolution. This illustration of exaptation indicates how this concept may account for a feature of the genome that, to those committed to an adaptationist program, might appear anomalous. Gould and Vrba (1982) pointed out that:

> For a few years after Watson and Crick elucidated the structure of DNA, many evolutionists hoped that the architecture of genetic material might fit all their presuppositions about evolutionary processes. The linear order of nucleotides might be the beads on a string of classical genetics: one gene, one enzyme; one nucleotide substitution, one minute alteration for natural selection to scrutinize. We are now, not even 20 years later, faced with genes in pieces, complex hierarchies of regulation and, above all, vast amounts of repetitive DNA. High repetitive, or satellite, DNA can exist in millions of copies: middle-repetitive DNA, with its tens to hundreds of copies, forms about one quarter of the genome in both *Drosophilo* and *Homo.* What is all the repetitive DNA for (if anything)? How did it get there?
>
> (p. 101)

Some of the repeated DNA may be conventional adaptations, selected for a role in regulation (e.g., the repeated copies may bring previously separated parts of the genome into new, aptative interrelation). However, there is too much repetitive DNA for such direct adaptation to account for all of it. A second, traditional basis for the presence of so much repeated DNA has been forwarded (i.e., an adaptationist program-oriented one). This suggestion is that repetitive DNA exists because it is needed for *future* evolution, that is, it exists to provide for a "flexible future"; for instance, non-used, redundant copies are free to alter because their adaptive product is still being produced by the remaining DNA copies (e.g., Cohen, 1976; Kleckner, 1977). However, this second argument is teleological because it permits future needs to determine present circumstances.

Whereas Gould and Vrba (1982) claimed that future uses are quite significant consequences of repeated DNA, the potential future use cannot be held to empirically determine the prior status of the genome. In turn, the concept of exaptation capitalizes on the idea that repeated DNA may, indeed, have a significant future use but does so without recourse to teleological, "final cause" explanations. In making these contributions, the concept of exaptation furthers understanding of how features of the genome provide a basis for plastic microevolutionary processes. Gould and Vrba (1982) explained that:

> Defenders of the second tradition understand how important repetitive DNA is to evolution, but only know the conventional language of adaptation for expressing this conviction. But since utility is a future condition (when the redundant copy assumes a different function or undergoes secondary adaptation for a new role), an impasse in expression develops. To break this impasse, we might suggest that repeated copies are nonapted features available for cooptation later, but not serving any direct function at the moment. When coopted, they will be exaptations in their new role (with secondary adaptive modifications if altered).
>
> What then is the source of these exaptations? According to the first tradition, they arise as true adaptations and later assume their different function. The second tradition, we have argued, must be abandoned. A third possibility has recently been proposed (or rather, better codified after previous hints): perhaps repeated copies can originate for no adaptive reason that concerns the traditional Darwinian level of phenotypic advantage (Orgel & Crick, 1980; Doolittle & Sapienza, 1980). Some DNA elements are transposable: if these can duplicate and move, what is to stop their accumulation as long as they remain invisible to the phenotype (if they become so numerous that they begin to exert energetic constraint upon the phenotype, then natural selection will eliminate them)? Such "selfish DNA" may be playing its own Darwinian game at a gene level, but it represents a true nonaptation at the level of the phenotype. Thus, repeated DNA may often arise as a nonaptation. Such a statement in no ways argues against its vital importance for evolutionary futures. When used to great advantage in that future, these repeated copies are exaptations.
>
> (p. 11)

In other words, and crucial for a synthesis of micro- and macro-evolutionary processes, Gould and Vrba (1982) claimed that there exists an "enormous pool" of nonaptations and that this pool must be the source, the "reservoir," of most evolutionary flexibility. They noted that:

> We need to recognize the central role of "cooptability for fitness" as the primary evolutionary significance of ubiquitous nonaptation in organisms. In this sense, and at its level of the phenotype, this nonaptive pool is an analog of mutation—a source of raw material for further selection.
>
> Both adaptations and nonaptations, while they may have non-random approximate causes, can be regarded as randomly produced with respect to any potential cooptation by further regimes of selection. Simply put: all exaptations originate randomly with respect to their effects. Together, these two classes of characters, adaptations and nonaptations, provide an enormous pool of variability, at a level higher than mutations, for cooptation as exaptations [and provide for] . . . the flexibility of phenotypic characters as a primary enhancer of or damper upon future evolutionary change. Flexibility lies in the pool

> of features available for cooptation (either as adaptations to something else that has ceased to be important in new selective regimes, as adaptations whose original function continues but which may be coopted for an additional role, or as nonaptations always potentially available). The paths of evolution—both the constraints and the opportunities—must be largely set by the site and nature of this pool of potential exaptations. Exaptive possibilities define the internal contribution that organisms make to their own evolutionary future.
>
> (pp. 12–13)

In sum, the concept of exaptation, and the limitations it imposes for the notion of adaptation as the sole process by which evolution occurs, presents formidable conceptual and empirical problems for sociobiological thinking. The existence of exaptive processes indicates that evolution is considerably more plastic than sociobiology would imply. This plasticity is highlighted by those objections to the adaptationist program that involve the specification of the causal role played by the developing organism, and especially by its dynamic coactions with its context, in influencing the course of evolution.

The Role of the Organism in Its Own Evolution

A clear implication of Gould and Vrba's (1982) revised terminology is that not all instances of fitness are adaptations; that is, not all features of an organism's structure and function that are aptational have this character as a consequence of being shaped by natural selection. Such a possibility, if supported, would serve to weaken what Gould and Lewontin (1979) labeled the "adaptationist program," that is, the position, reflected in the earlier quote by Konner (1982, p. 18), that a feature's current aptational character implies historical shaping by natural selection for that character.

Lewontin (1981) discussed the adaptationist "program" and its conventional use of the concept of adaptation. As did Gould and Vrba (1982), Lewontin (1981) saw problems with this view of adaptation; in essence, he saw the view as deficient because it ignores the active, constructive role the organism plays in its own adaptation. The organism shapes the context to which it adapts, and, hence, there is a reciprocal, multilevel (i.e., fused) relation between organism and context, that is, there are organism↔context relations (e.g., Ford & Lerner, 1992; Gottlieb, 1997; Lerner & Walls, 1999; Magnusson, 1999a, 1999b; Thelen & Smith, 2006). Thus, Lewontin's (1981) criticism of the conventional use of the concept of adaptation is associated with a view of the organism compatible with RDS-based conceptions of human development. Specifically, Lewontin (1981) noted:

> Organisms . . . by their own life activities determine which aspects of the outer world make up their environment. Organisms change the environment by their activities . . . they "construct" environments. The problem is that the concept of adaptation has been extended metaphorically from its valid domain of describing individual, short-term, goal-directed behavior to other levels . . . it is pure metaphor, ideologically molded by the progressivism and optimalism of the nineteenth century, to describe numbers of chromosomes, patterns of fertility, migrations, and religious institutions as "adaptations." . . . It is not simply that some evolutionary process can be described as nonadaptive, but that the entire framework is in question. Whether we look at the fossil record or at living species, we do not see them as "adapting," but as "adapted." But how can that be? How is it that, if evolution is a process of adapting, organisms always seem to be adapted. It may be more illuminating to see organisms as changing and, in the process, as reconstructing the elements of the outer world into a new environment that is sufficient for their survival.
>
> (p. 245)

For example, summarizing the literature pertaining to the character of the environment to which organisms adapt, Lewontin and Levins (1978) emphasized that reciprocal processes between organism and environment are involved in human evolution, supporting the view that human functioning is one source of its own evolutionary development. Lewontin and Levins (1978) stated that:

> The activity of the organism sets the stage for its own evolution . . . the labor process by which the human ancestors modified natural objects to make them suitable for human use was itself the unique feature of the way of life that directed selection on the hand, larynx, and brain in a positive feedback that transformed the species, its environment, and its mode of interaction with nature.
>
> (p. 78)

Consistent with the position of Lewontin (1981) and Lewontin and Levins (1978) regarding the problems with the "adaptationist program," Gould and Vrba (1982) contended that recognition of the potential presence of exaptive features leads one to recognize that previously non-adaptive (note, *not* pre-adaptive) features may be present and may be coopted for fitness—a recognition that provides a key for plasticity in evolutionary processes and for the role of individuals' own organismic characteristics in their development. Gould and Vrba (1982) indicated:

> Flexibility lies in the pool of features available for cooptation . . . The paths of evolution—both the constraints and the opportunities—must be largely set by the size and nature of this pool of potential exaptations. Exaptive possibilities define the "internal" contribution that organisms make to their own evolutionary future.
>
> (pp. 12–13)

The concept of exaptation leads to the understanding that the processes involved in evolution are plastic ones, and that plasticity involves organisms' active contributions to their own evolutionary change (e.g., Brandtstädter, 1998, 1999; Gottlieb, 1983, 1997; Lerner, 1982; Lerner & Busch-Rossnagel, 1981b; Lerner & Walls, 1999). As such, exaptation is a concept consistent with a key theme in the RDS-based "alternative" to a hereditarian view of the role of biology in human development (e.g., Bateson, 2015, 2016; Ford & Lerner, 1992; Gottlieb, 1997, 2004; Ho, 2010, 2013, 2014; Lerner & Walls, 1999; Magnusson, 1999a, 1999b; Thelen & Smith, 2006; Witherington, 2015). According to this alternative, it is possible to envision how processes exist that contribute to the plasticity of people's functioning. These processes allow people to play a role in the ontogeny—and, through a concept introduced by Gottlieb (1987), in the phylogeny as well—of their own flexible characteristics.

The Relational Developmental System and the Role of the Concept of "Behavioral Neophenotypes" in Evolutionary Change

As described by Lewontin (1981), it is possible to view the organism as other than just the host of its evolutionarily provided genes and, as I have emphasized throughout this book, it is likewise possible to view the importance of the organism's activity across ontogeny as more than just the maturationally-predetermined unfolding of hereditarily-fixed progressions. The key alternative view is one that sees biological and contextual factors as reciprocally coactive. As such, developmental changes are probabilistic in respect to normative outcomes due to variation in the timing of the biological, psychological, and social factors that provide coactive bases of ontogenetic progressions (e.g., Gottlieb, 1970, 1998, 2004; Schneirla, 1957; Tobach, 1981).

As discussed in previous chapters, this view has been labeled as probabilistic-epigenetic by Gottlieb (1983), and developed by him (e.g., Gottlieb, 1970, 1976, 1991, 1992, 1997, 2004; Gottlieb et al., 2006), and earlier by Schneirla (1956, 1957) and Tobach and Schneirla (1968). Probabilistic epigenesis constitutes a defining feature of RDS-based theories, and the fusions among levels of organization within the system that it reflects provides the basis of plasticity in development across the human life span (e.g., Ford & Lerner, 1992; Gottlieb, 1997; Magnusson, 1999a, 1999b; Thelen & Smith, 2006). As I have just noted, Lewontin (1981) indicated what such plasticity in development may mean for altering the course of evolution. In turn, Gottlieb (1987) also provided a quite intriguing discussion of the role of plastic developmental functioning in shaping evolutionary change.

Although biologists such as Garstang (1922), de Beer (1930), and Goldschmidt (1933) previously argued that developmental changes may lead to evolution, they also believed that a genetic change or a mutation was necessary to create the developmental changes. Gottlieb (1987), however, argued for an evolutionary pathway in which ontogenetic development leads to evolutionary change and, quite significantly, where "genetic change is a secondary or tertiary consequence of enduring behavioral changes brought about by nongenetic alterations of species-typical development" (p. 267). Gottlieb's conception draws on a notion introduced by Kuo (1967), of behavioral neophenotype, that is, a behavioral innovation, or ontogenetic novelty, made possible by the plasticity of the organism and its probabilistic, dynamic coactions with its context.

Gottlieb contended that a behavioral neophenotype is likely the first step in an evolutionary sequence that proceeds from behavioral change, to morphological change, to genetic change (see too Jablonka & Lamb, 2005). More specifically, the emergence of a behavioral neophenotype encourages new environmental relationships that, in turn, bring out latent possibilities for morphological and physiological changes. Gottlieb (1987) noted that somatic mutation, cytoplasmic alteration, or change in gene regulation may also take place at this point; however, an alteration of structural genes need not take place in this secondary stage of the process. However, a change in genes or in gene frequency does occur in the third stage, wherein as a consequence of long-term geographic or behavioral isolation (i.e., separate breeding populations), such alteration takes place.

Because of the plasticity that exists in organisms (and especially ones with larger relative brain size such as humans; Gottlieb, 1987), a plasticity textured by the probabilistic, dynamic coactions organisms have across ontogeny with their context, an evolutionary pathway is created that is inconsistent with the conception of evolutionary change found in evolutionary epistemology and in the associated predetermined-epigenetic view of organism change. Gottlieb's ideas about behavioral neophenotypes have been incorporated by Jablonka and Lamb (2005) as one of the four systemically integrated dimensions of evolution. They note that "many animals transmit information to others by behavioral means, which gives then a third hereditary system" (Jablonka and Lamb, 2005, p. 1), along with genetics, epigenetics, and culture.

Thus, on the basis of both this contribution to evolution by an organism's development, and the implications of the concept of exaptation, it is possible to conclude that the key features of sociobiological thinking are severely scientifically limited. Evolution processes are not, therefore, just comprised of phylogenetically continuous changes that, by virtue of the antecedent and independent effects of the physical world, shape via natural selection particular cognitive structures, reproductive strategies, or parent–child relations. The particular set of behavioral or social features present in a person, social group, or culture cannot be judged as contributing to or diminishing the survival of the human species by virtue of the adaptationist assertion that the features have or have not been shaped and selected for fitness.

Conclusions about the Presence of Evidence in Support of the Sociobiological View of Human Development

As was the case in regard to the lines of evidence relating to the concept of homology and to the use of heritability research data, the third line of evidence relied on by sociobiologists—an adaptationist storyline to explain what are purported to be genetically-based differences in individual and social development—fails. "Just-so stories" (Gould, 1980) about human evolutionary history are used to substitute superficial descriptions for in-depth explanations. Alternative paths to current fitness (or aptation) are excluded from scientific consideration or analysis.

Equally serious problems arise in regard to the other two lines of evidence relied on by sociobiologists—involving the inappropriate postulation of homologies between nonhuman and human animals and the misuse of the concept of heritability, which of course is a fatally flawed idea in regard to understanding human development. Indeed, as noted by Moore (2015b), the variance-partitioning approach epitomized by heritability analyses "black boxes" the study of development. The logical and empirical problems of sociobiology reveal the weak scientific basis of this theory. The severity of these problems suggests that sociobiological thinking has little relevance for the understanding of human behavior and development in general, or of individual or group differences in particular.

Nevertheless, the scientific vacuity of sociobiological ideas about human development did not deter some writers from using these ideas to propose theories about the evolutionary basis of individual and/or group differences in numerous features of human development (e.g., MacDonald, 1988, 1994; Rushton, 1988a, 1988b, 1996, 1999, 2000), for example, sexuality, intelligence, criminality, and parenting. It is important to evaluate an example of this type of work in order to illustrate the quality of the evidence that sociobiologists use to make pronouncements about the hereditary basis of group differences in human development.

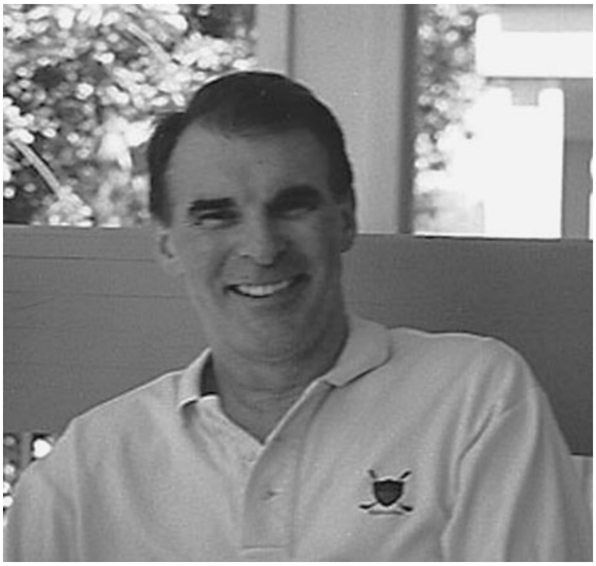
To provide this illustration, I focus on the work of arguably the most visible of the hereditarian writers who used sociobiology to explain group differences in human behaviors: J. Philippe Rushton (e.g., 1999, 2000). I focus on his views about human evolution and the quality of the scientific work he did to support his ideas.

THE WORK OF J. PHILIPPE RUSHTON

Rushton's (1997) work rested on a split view of the nature–nurture issue. In fact, he not only split genes from context in his attempts to explain human development but also he saw a split between the people whose work is associated with hereditarian versus developmental systems conceptions, a split that divides—in his view—good from poor scientists. That is, Rushton (1997) noted that:

> Most of those engaged in the serious study of race today do so from either the "hermeneutical" or the "race-realist" perspective. At one extreme, those I have termed "hermeneuticists" approach race as an epiphenomenon, a mere social construction, with political and economic forces as the real causal agents worthy of study. Rather than research race, hermeneuticists research those who do. At the other end of the forum, those I term the "race-realists" view race as a natural phenomenon to be observed, studied, and explained. Alternative and intermediate positions certainly exist, but the most heated debate currently takes place between advocates of the two polar positions. The hermeneutical approach relies on textual, historical, and political analysis; the race-realist approach is empirical and employs a panoply of scientific methodologies, including surveys, psychometrics, and genetics. Because the hermeneutical viewpoint sees inexorable links between theory and practice, its writings are often prescriptive and assume an advocacy position. The race-realist viewpoint is descriptive and typically avoids prescribing policy. To their opposite numbers, hermeneuticists come across as muddled, heated, and politically committed to "antiracism"; the race-realists come across to their opponents as cold, detached, and suspect of hiding a "racist" agenda.
>
> (p. 78)

In effect, this instance of Rushton's split conception is actually one of labeling hermeneuticists as "obfuscating politically correct ad hominemists" and seeing race realists as "objective crusaders for



J. Philippe Rushton

scientific and social truth." It may be, however, that this characterization hoists Rushton "on his own petard." To see if it does, I turn to a discussion of the ideas and methods Rushton uses to seek and present "truth."

Rushton's Tripartite Theory of Race, Evolution, and Behavior

Rushton (1999, 2000) proposed a tripartite racial view of human evolution, one which purports to show that, in regard to characteristics of human functioning linked to successful development (e.g., high intelligence and occupational achievement, good parenting and caregiving skills, and low criminality), the three racial groups he identified (that he termed "Orientals," "Whites," and "Blacks") differ significantly. Although Rushton (1999) never defined the concept of "race," he noted that there are:

> Three major cases: *Orientals* (East Asians, Mongoloids), *Whites* (Europeans, Caucasoids), and *Blacks* (Africans, Negroids). To keep things simple, I will use these common names instead of scientific ones and will not discuss subgroups within the races.
>
> On average, Orientals are slower to mature, less fertile, and less sexually active, have larger brains and higher IQ scores. Blacks are at the opposite end in each of these areas. Whites fall in the middle, often close to the Orientals.
>
> (p. 18)

There are numerous, and well-known, data sets contradicting Rushton's all-too-facile divisions. For instance, consider the variable that Rushton (1999) considered to be the most clearly linked to the biological and, hence, evolutionary differences between the racial groups he described, that is, reproductive maturation. He noted that "races tend to differ in the age when they reach milestones such as the end of infancy, the start of puberty, adulthood, and old age" (Rushton, 1999, pp. 27–28) and that "Blacks reach sexual maturity sooner than Whites, who in turn mature sooner than Orientals. This is true for things like age at first menstruation, first sexual experience, and first pregnancy" (Rushton, 1999, p. 30). Rushton failed to attend to abundant information which indicates unequivocally that his assertion is simply incorrect.

To illustrate, Rushton (1999) ignored Hiernaux's (1968) data showing that pubertal maturation (i.e., age of menarche, the age of the first menstrual cycle) among Africans can vary from as low as 12.4 years to as high as 18.8 years; as such, the ontogenetic rate of maturation of some Africans is substantially slower than those of many groups of Asians and Europeans studied by Hiernaux (1968). In turn, Tanner (1973, 1991) reported a secular trend wherein the time of pubertal maturation decreased over the course of the twentieth century for numerous groups of Europeans, for European Americans, and for Asians (e.g., Japanese). In fact, the latter group showed the most dramatic decrease in time of maturation for all groups studied by Tanner (1991). In describing trends after World War II, Tanner (1991) noted that "in improving postwar conditions, there was a decline of some 11 months per decade until 1975, when the trend leveled out to practically zero" (p. 638). Thus, pubertal maturation is a quite plastic phenomenon, responsive to the nutritional and medical resources present in the ecology of developing individuals.

The data reflecting such plasticity directly contradict the tripartite differences specified by Rushton (1999). As such, *either* Rushton is open to criticism for weak and inadequate scholarship as a consequence of his not knowing of data sets that had been quite prominent in the biological and human development literatures for several decades at the time of his writing, *or* he is open to criticism for biased and inadequate scholarship as a consequence of failing to acknowledge that his ideas are convincingly contradicted by strong, countervailing data. In either case, Rushton's (1999, p. 96) self-congratulatory assertion that "I have not ignored any important studies" is simply incorrect. Indeed, Winston (1997b) explained that Rushton makes similar "errors" in regard to his claiming support for his tripartite racial theory from data about the brain size of the three "racial groups" Rushton (1997, 1999) described (cf. Peters, 1995a; Winston, 1996).

Rushton's Ideas about Different Reproductive Strategies across Race Groups

Despite the inadequate scholarship that characterizes the evidentiary basis for his claims, Rushton (1999) went on to propose that the bases for the reproductive and associated behavioral differences he associated with the three racial groups he discussed lie in the different "reproductive strategies" characterizing them. He described a continuum of reproductive strategies wherein "At one end of this scale are r-strategies that rely on high reproductive rates. At the other end are K-strategies that rely on high levels of parental care" (p. 24).

The different strategies depicted across this continuum are useful in biology to depict the reproductive rates of separate species (that are trying to survive and reproduce in diverse ecological niches (Johanson & Edey, 1981). For instance, a sponge, living and reproducing on the ocean floor, will produce literally thousands of offspring during a given reproductive cycle, and this level of reproduction will increase the probability of a few offspring withstanding the harsh currents and otherwise dangerous ecology of the ocean bottom for a period sufficient for their survival and eventual perpetuation of the species. In turn, given elephants' enormous nutritional needs during their lengthy prenatal gestation period and postnatal years, the probability of offspring survival is enhanced when a small number of offspring, most typically one, is produced during a reproductive cycle.

Thus, the r–K distinction is useful for describing *differences* between species in how their rate of reproduction fits the ecological niche within which they live. However, there is no validity for applying this concept to differences *within* a species in the reproductive rates of different individuals or groups. Yet, this is an error that Rushton (1999) made, and, in fact, admitted that he did! He noted that the r–K "scale is generally used to compare the life histories of different species of animals. I have used it to explain the small but real differences between the human races" (Rushton, 1999, p. 24).

Hence, Rushton (1999) misapplied the r–K distinction in two ways. First, he took a concept that describes differences between species and applied it to differences within a species *without any biological evidence of the validity of such an application.* Nevertheless, in response to the question of whether his r–K concept applied only to differences between species and not to within-species differences, Rushton (1999) asserted without any documentation that, "It applies to both" (p. 103).

Second, Rushton used a descriptive concept to explain differences within a species—and his explanation was that, basically, the group he called "Blacks" represent an evolutionarily less-advanced form of organism, in that their reproductive strategy is more closely aligned with more "primitive," r-like organisms. Indeed, Rushton (1999) used his r–K explanation to account for purported differences in their investment in their children between "Orientals" and "Whites," who he claimed are more "K-selected," and "Blacks," who he contended are more "r-selected."

He indicated that, "Highly K-selected men invest time and energy in their children rather than the pursuit of sexual thrills. They are 'dads' rather than 'cads'" (Rushton, 1999, p. 24).

Moreover, Rushton (1999) asserted—*without any citation whatsoever to bolster his statements*—that:

> In Africa, the female-headed family is part of an overall social pattern. It consists of early sexual union and the procreation of children with many partners. It includes fostering children away from home, even for several years, so mothers remain sexually active . . . In Black Africa and the Black Caribbean, as in the American underclass ghetto, groups of pre-teens and teenagers are left quite free of adult supervision.
>
> (pp. 35–36)

As I will note in the subsequent sections of this chapter that are devoted to a discussion of evolutionary developmental psychology, the ideas of Belsky, Steinberg, and Draper (1991) and of Ellis, Schlomer, Tilley, and Butler (2012) are redolent of these unfounded ideas presented by Rushton (1999).

Amazingly, Rushton (1999) showed no awareness (e.g., through discussion or even mere citation) of the rich literature pertinent to the African American family (e.g., Demo, Allen, & Fine, 2000; McAdoo, 1977, 1991, 1993a, 1993b, 1995, 1998, 1999;

McCubbin et al., 1998). This literature presents data providing a point-for-point contradiction of Rushton's undocumented assertions. Accordingly, when Rushton (1999) asserted that "scientists have a special duty to examine the facts and tell the truth" (p. 105), one may wonder whether he included himself within the group held to this standard. In any case, it seems clear, from the evaluations that have been made of the quality of the "data" Rushton forwarded regarding his ideas, that the "truth" was not being told by either the data he presented or the interpretations he made of the data.

Evaluations of Rushton's Evidence

It is useful to consider a critique of the breadth of the evidence Rushton presented in regard to his ideas. Cernovsky (1997) noted that Rushton's studies of racial differences (e.g., Rushton, 1988a, 1998b, 1990, 1991a, 1991b, 1995), as well as those of other researchers working to support his findings (e.g., Lynn, 1993),

> are noteworthy for their excessive reliance on very low correlation coefficients from obsolete data sets to postulate causal relationships. When a given method produces findings inconsistent with their . . . views, they conveniently switch to a different method. An independent statistical re-examination of the same source of data by others may produce dramatically different results.
>
> (p. 1)

To illustrate, Cernovsky and Litman (1993) reanalyzed the data that Rushton (1990) used to demonstrate that there were significant race differences involving what Rushton termed "Mongoloid," "Caucasoid," and "Negroid" groups across nations in crime rates (e.g., involving homicide, rape, and serious assault). The data, Rushton (1990) claimed, indicated that the Negroid group had higher rates of crime than did either of the other two groups. However, Cernovsky and Litman (1993) found that the race differences reported by Rushton (1990) were not strong and, in fact, were largely weak and inconsistent. Not only did Rushton (1990) *not* present any evidence why these small differences among races should be considered genetic in origin, but Cernovsky and Litman (1993) also found that, in Rushton's own data, reliance on race to predict an individual's likelihood of committing a crime "would result in an absurdly high rate (99.9%) of false positives" (p. 31).

Similarly, Gorey and Cryns (1995) reassessed some of the data that Rushton (1988a, 1988b, 1990, 1991a, 1991b, 1995) used to illustrate the evolutionary and genetic deficits of "Negroids" in regard to intelligence, rate of physical maturation, personality and temperament, sexuality, and social/familial organization. The results of Gorey and Cryns's (1995) independent analysis of these data contradicted Rushton's characterization of the support provided for his hereditarian views of race differences. Gorey and Cryns found that the "relationships are very close to zero and some are in the opposite direction than postulated by Rushton" (Cernovsky, 1997, p. 2).

To illustrate some of the problems with Rushton's interpretation of the literature, I may note that Rushton (1990) cited the assessment of Beals, Smith, and Dodd (1984) of the relations between brain weight and race as providing support for his contention that "Negroids" have lower brain weights than do the other two race groups Rushton considered. Yet, Cernovsky and Litman (1993) noted that the statistical conclusions of Beals and colleagues (1984) "are the opposite of his own: brain weight is *not* primarily related to race" (p. 35). Cernovsky and Litman (1993) indicated as well that Rushton "selectively reports data confirming his theory . . . this renders the data reported in (his work) *worthless* for generalization" (p. 35).

In addition, Cernovsky (1992) noted that Rushton's (e.g., 1988a, 1988b, 1990, 1991a, 1991b) information suffers from conceptual and methodological flaws, for example, relating to his ignoring environmental effects such as secular trends (e.g., as in Tanner, 1991); statistical problems, associated with interpreting data with restricted ranges or with the overinterpretation of low correlations; and either omitting contradictory information from the literature he reviewed or, as illustrated by the above-noted work of Gorey and Cryns (1995), interpreting contradictory information as supportive of his ideas.

Conclusions about the Quality of Rushton's Hereditarian Views of Race Differences

Given the numerous dimensions of critical scientific problems associated with Rushton's work, I agree with Cernovsky's (1995) view that:

> Although Rushton's writings and public speeches instill the vision of Blacks as small-brained, oversexed criminals who multiply at a fast rate and are afflicted with mental disease, his views are neither based on a bona fide scientific review of literature nor on contemporary scientific methodology. His dogma of bioevolutionary inferiority of Negroids is not supported by empirical evidence.
>
> (p. 677)

In sum, given this quality of work that Rushton (1999) employed to document his views, I believe that an appropriate conclusion about Rushton's scholarship is reflected in Cernovsky's (1997) view, that: "Rushton's racial theory is logically inconsistent, built on methodologically obsolete procedures, and is not supported by credible data sets selected in an objective manner" (p. 4). As is the case with the other lines of evidence that intend to provide sociobiological evidence in support of the genetic basis of human behavior and development (relating to homology, heritability, and adaptation), Rushton's work reduces to no evidence at all.

Zack Z. Cernovsky

CONCLUSIONS: WHY ISN'T NATIVISM "DEAD?"

In this and the preceding chapter I have reviewed several different approaches to nature/hereditarian conceptions of human development. All approaches—behavior genetics theory, the assessment of the heritability of intelligence, the study of instincts, and sociobiology—involve nature–nurture split concepts. Each has been seen to have critical conceptual, methodological, and empirical problems. Although the work of Rushton (e.g., 1999, 2000) may be an exemplar of the bad science associated with these views—combining conceptual problems, including counterfactual assertions, with poor methodology and misinterpretations or misrepresentations of data—instances of such scholarly shortcomings abound in the literatures associated with these instances of genetic reductionism.

I will discuss additional instantiations of genetic reductionist ideas in the remainder of this chapter, but I think that it is appropriate to ask at this point key questions about the persistent presence of these ideas across the history of developmental science. Why, with so many conceptual, methodological, and empirical problems associated with these instances of genetic reductionism, are there still examples of these approaches being presented at this writing? Why, as well, are these approaches still being given scientific attention, for example, through the awarding of research funds or by publication of this work in good scientific journals?

Tabery (2014) offers one answer. He suggests that the variance-partitioning approach of behavior genetics, as an exemplar of genetic reductionist models, and the process-elucidation approach of research associated with RDS-based theories (e.g., Moore, 2015b) address different questions. As a consequence, the data from one approach are not appreciated or seen as relevant to proponents of the other approach. Although this idea has merit—proponents of the two approaches do address different questions—I think that this answer is not compelling for at least two reasons.

First, as Moore (2015b) explains, the flaws of the variance-partitioning approach are so numerous that it cannot be fairly said that the two approaches are scientifically commensurate in regard to their respective capacities to describe, explain, and optimize the diverse intraindividual change trajectories comprising human development within the relational developmental system. Second, Tabery's (2014) answer ignores the psychology of the "person in the street" discussed by Horowitz.

As Horowitz (2000) explained, the simplicity of the hereditarian answer to the questions of the "person in the street" about human development (i.e., "The answer is that it is in your genes") continues to be attractive to people and seen as newsworthy to the media (e.g., see Wade, 2014; but see too Feldman, 2014, for a critique of Wade, 2014). Often, neither the "person in the street" nor the media have patience for more complex answers (e.g., "The answer depends on the particular history of fusions within the developmental system").

Accordingly, given both the theoretical and practical reasons that are associated with attraction to hereditarian ideas, versions of such formulations are likely to continue to be forwarded. As I already noted in regard to the children's game, Whack-A-Mole, as soon as the failures of one instantiation of genetic reductionism are recognized and it is compellingly refuted, other instances of this problem-riddled conception, and another version of this idea, pop up. As a consequence, and as Lewontin cautioned, the "price" developmental scientists must pay for the continued possible use of genetic reductionist conceptions is the need to remain vigilant about their appearance. Developmental scientists must be prepared to discuss the poor science involved in genetic reductionist ideas and to point out the inadequate bases they provide for public policy and for applications pertinent to improving human life (Lerner, 2015b, 2015c; see too Schneirla, 1966; Tobach, 1994). Developmental scientists must be ready to suggest alternatives, such as RDS-based views, to hereditarian views of research about and applications for human development.

I attempt to continue such vigilance as I consider in the remainder of this chapter another example of genetic reductionist ideas, evolutionary developmental psychology. I then return at the end of the chapter to further discuss the action agenda that developmental scientists who are working within RDS-based models might pursue to address the continuing challenges of present and future instances of genetic reductionist models of human development.

EVOLUTIONARY DEVELOPMENTAL PSYCHOLOGY

The evidence about embodiment, plasticity, and epigenetics that accounts for the character of evolutionary and developmental change understandably elicits skepticism about and, even more, the rejection of the "extreme nature" claims of genetic reductionists (Rose & Rose, 2000). For instance, proponents of evolutionary psychology (EP) claim that "everything from children's alleged dislike of spinach to humans' supposed universal preferences for scenery featuring grassland and water derives from [the] mythic human origin in the African savannah" (Rose & Rose, 2000, p. 2). These claims are predicated on the basis of the assertion that one can explain:

> all aspects of human behaviours, and thence culture and society, on the basis of universal features of human nature that found their final evolutionary form during the infancy of our species some 100–600,000 years ago. Thus for EP, what its protagonists describe as the "architecture of the human mind" which evolved during the Pleistocene is fixed, and insufficient time has elapsed for any significant subsequent change. In this architecture there have been no major repairs, no extensions, no refurbishments, indeed nothing to suggest that micro or macro contextual changes since prehistory have been accompanied by evolutionary adaption.
>
> (Rose & Rose, 2000, p. 1)

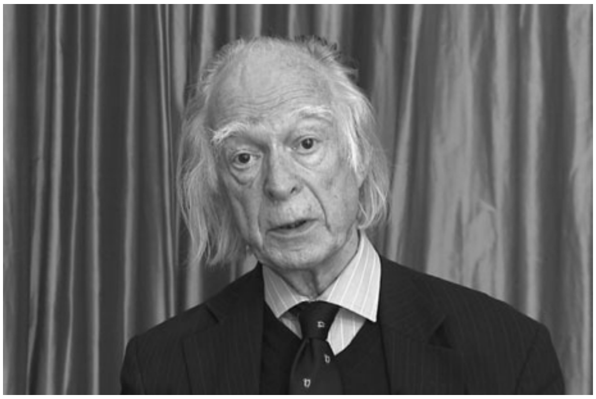
Clearly such assertions are inconsistent with the now quite voluminous evidence in support of the epigenetic character of evolution and ontogeny (Lickliter, 2016; Lickliter & Honeycutt, 2015; Noble, 2015), of the multiple, integrated dimensions of evolution (Jablonka & Lamb, 2005), and of the role of the organism's own agency and of culture in creating change within and across generations (Gottlieb, 1997, 2004; Mistry & Dutta, 2015; Raeff, 2016; Rogoff, 1998, 2003, 2011).

Simply, proponents of evolutionary developmental psychology (EDP) (e.g., Bjorklund, 2015, 2016; Bjorklund & Ellis, 2005; Del Giudice & Ellis, 2016) get the nature of evolution quite wrong (pun intended). They fail to appreciate the autopoietic character of the holistic, dynamic, and integrated relational developmental system, a system that is marked by probabilistic-epigenetic change. Therefore they adhere to an atavistic and incorrect view of the role of genes in this self-constructing system. As Noble (2015) explains, the appropriate understanding of genes within this system involves:

> two fundamental concepts. The first one is the distinction between active and passive causes. Genes are passive causes; they are templates used when the dynamic cell networks activate them. The second concept is that there is no privileged level of causation. In networks, that is necessarily true, and it is the central feature of what I have called the theory of biological relativity, which is formulated in a mathematical context (Noble, 2012).
>
> I will illustrate the second point in a more familiar nonmathematical way. Take some knitting needles and some wool. Knit a rectangle. If you don't knit, just imagine the rectangle. Or use an old knitted scarf. Now pull on one corner of the rectangle while keeping the opposite corner fixed. What happens? The whole network of knitted knots moves. Now reverse the corners and pull on the other corner. Again, the whole network moves, though in a different way. This is a property of networks. Everything ultimately connects to everything else. Any part of the network can be the prime mover, and be the cause of the rest of the network moving and adjusting to the tension. Actually, it would be better still to drop the idea of any specific element as prime mover. It is networks that are dynamically functional.
>
> Now knit a three-dimensional network. Again, imagine it. You probably don't actually know how to knit such a thing. Pulling on any part of the three-dimensional structure will cause all other parts to move . . . It doesn't matter whether you pull on the bottom, the top or the sides. All can be regarded as equivalent. There is no privileged location within the network.
>
> (p. 11)

Noble (2015) adds that this conception of the role of genes within the dynamic, integrated relational developmental system is consonant with the ideas of Ho and Saunders (1979; see too Ho, 2010, 2013). Noble notes their view that:

> The intrinsic dynamical structure of the epigenetic system itself, in its interaction with the environment, is the source of non-random variations which direct evolutionary change, and that a proper study of evolution consists in the working out of the dynamics of the epigenetic system and its response to environmental stimuli as well as the mechanisms whereby novel developmental responses are canalized.
>
> (Ho & Saunders, 1979, p. 573)



Denis Noble

Peter Saunders

Similarly, comparative psychologist Gary Greenberg (e.g., 2004, 2011; Greenberg & Partridge, 2010) explains that:

> Rejection of the atomistic reductionistic approach also entails a rejection of the adaptationist agenda of evolutionary psychology. As many have pointed out, including such notable evolution scientists as Gould (1997), even Darwin suggested that mechanisms other than adaptation are at work in evolution. It is a mistake and a misunderstanding of Darwinism to suppose that there is anything approaching the consensus claimed by evolutionary psychologists. Rather, pluralism of mechanisms is the rule in the still developing paradigm of evolution. For example, we now understand evolution to involve punctuated equilibrium, genetic drift, mutation, and other processes, as well as natural selection. In fact, evolution does not always involve changes in the genome. It is now recognized that not all genes of the human genome get expressed. Evolution can occur if different portions of the genome are expressed, the result perhaps of environmental impact. This would result in new phenotypes (see Honeycutt, 2006).
>
> (Greenberg & Partridge, 2010, p. 122)

Greenberg goes on to note that, whereas

> the concept of epigenesis originated in biology, the usefulness of probabilistic epigenesis was recognized and promoted throughout the twentieth century by psychologists such as Zing-Yang Kuo (1967), Gilbert Gottlieb (1992), and T. C. Schneirla (1957), although Schneirla never specifically employed the term epigenesis in his writing (Aronson et al., 1972). Probabilistic epigenesis has gained support from an exciting set of developments in contemporary science subsumed under the rubric of "dynamic systems theory and relational developmental systems theory," in which complex developmental processes are understood as composed of interrelations among many active system components of the whole developmental system, which I have discussed above. The implication of this position is that in a dynamic and changing environment, rather than genes specifying a particular developmental outcome, be it structural or behavioral, every outcome is an emergent result of the transaction between genes and their cellular, organismic, ecological, and temporal contexts. This view of epigenesis is epitomized by discoveries in biology that even identical genomes in extremely similar environments do not always follow the same developmental pathways. Ko and colleagues (Ko, Yomo, & Urabe, 1994), studying enzyme activity in bacteria, found that despite identical genomes and extremely uniform culture conditions, individual cells developed different levels of enzyme activity and grew into colonies of different size. Ko's studies showed that cell state in bacteria is determined not only by genotype and environment. Rather, "Changes of state can occur spontaneously, without any defined internal or external cause. By definition, these changes are epigenetic phenomena: dynamic processes that arise from the complex interplay of all the factors involved in cellular activities, including the genes" (Solé & Goodwin, 2000, p. 63).
>
> (Greenberg, 2011, pp. 184–185)

Nevertheless, despite biologists and comparative psychologists presenting concepts and data enumerating the errors about evolution and genetics

Gary Greenberg

involved in EDP, examples of misguided scholarship about evolution (phylogeny) and its relation to ontogeny continue to appear in the literature of EDP. An example of the extreme nature of the claims of evolutionary psychologists pointed to by Rose and Rose (2000) occurs in writing about what is termed “paternal investment theory” (Belsky, 2012; Belsky, Steinberg, & Draper, 1991; Draper & Harpending, 1982, 1988). For instance, Ellis, Schlomer, Tilley, and Butler (2012) claim that:

> paternal investment theory links low male parental investment to more aggressive and hypermasculine behavior in sons and more precocious and risky sexual behavior in daughters (Draper & Harpending, 1982, 1988). The assumption is that natural selection has designed boys’ and girls’ brains to detect and encode information about their fathers’ social behavior and role in the family as the basis for calibrating socio-sexual development in gender-specific ways.
>
> (p. 32)

The purported “mechanism” for what Ellis et al. (2012) term this evolutionary-developmental phenomenon is that there is

> a unique role for fathers in regulating daughters’ sexual behavior. The theoretical basis for emphasizing father-effects is (a) that the quality and quantity of paternal investment is—and presumably always has been—widely variable across and within human societies; (b) this variation recurrently and uniquely influenced the survival and fitness of children during our evolutionary history . . . and (c) variability in paternal investment, much more than maternal investment, was diagnostic of the local mating system (degree of monogamy vs. polygyny) and associated levels of male–male competition . . . The mating system is important because more polygynous cultures and subcultures are characterized by heightened male intrasexual competition, dominance-striving, and violence, with concomitant diminution of paternal involvement and investment (Draper & Harpending, 1982, 1988). In turn, female reproductive strategies in this context are biased toward earlier sexual debut, reduced reticence in selecting mates, and devaluation of potential long-term relationships with high-investing males, all of which translate into more RSB [risky sexual behavior].
>
> (p. 32)

However, such embodiment of the individual and of his or her plastic developmental biological, psychological, and behavioral processes within the relational developmental system provides a basis for epigenetic changes across generations (e.g., see Moore, 2015a, 2016), that is, for changes in gene↔context relations within one generation being transmitted to succeeding generations. As such, the “just-so stories” approach to purported behavioral outcomes of evolution (Gould, 1981) used by sociobiology is shared by proponents of EDP; such an approach is conceptually flawed in that it ignores contemporary scholarship about evolutionary processes and their impact on ontogeny (e.g., Gissis & Jablonka, 2011; Ho, 2010, 2013, 2014; Ho & Saunders, 1979; Lickliter, 2016; Lickliter & Honeycutt, 2015; Meaney, 2010, 2014), and is therefore empirically counterfactual. Indeed, in commenting on the counterfactual character of the “just-so stories” told by proponents of evolutionary developmental psychology, Blumberg (2005) points out that:

> Every time that we have closely examined their claims, we have found faulty experiments, far-fetched interpretations, or both. In short, nativists and evolutionary psychologists have draped themselves in the blanket of science, but when all is said and done, they are merely telling bedtime stories for adults.
>
> (p. 205)

Similarly, Gary Greenberg (2004) points to both the lack of attention paid by proponents of evolutionary developmental psychology to the current literature in genetics, molecular biology, or evolutionary biology and, as well, the potentially pernicious implications of this failure in scholarship. He notes that:

> The behavioral sciences in general and evolutionary psychology in particular have tended to ignore, or perhaps not be aware of, contemporary

empirical findings in molecular biology, evolutionary biology, and genetics. Kaplan and Rogers make this point, as do other contemporary critics of evolutionary psychology (e.g., Lickliter & Honeycutt, 2003). For example, it is now known that (a) genes are not directly responsible for phenotypic expression but rather the environmental context of development plays a crucial role in this process, (b) genes not only work from the inside out but that behavior too can influence the expression of genes (referred to as "downward causation" by Campbell, 1990), (c) not all genes of a genome get expressed, (d) natural selection is but one of several mechanisms responsible for evolutionary change, (e) the path from genes to physical or behavioral traits is enormously complex and indirect, and (f) there are other similar crucial facts of the biological sciences that are misunderstood and ignored by evolutionary psychologists. Of course, a common response of evolutionary psychologists to such criticism is that it is they who are misunderstood, and they do not suggest that behavior is genetically determined or innate. However, and unfortunately, these responses amount to little more than lip service, as reference to a newly established Web site (2004) illustrates; this is the Web site of a group known as "The AHRB Project on Innateness and the Structure of the Mind," (the members of which are a veritable Who's Who of the evolutionary psychology elite). One crucial point underscored by recent empirical advances in the biological sciences is that there is much more to biology than genes; for us as psychologists, the message is similar: There is much more to behavior than genes. Determinism is no longer a viable notion in biology, nor is it in any of the sciences, as Kaplan and Rogers point out ". . . even quantum physics does not subscribe to causal determinism" (p. 29). To those still willing to invoke determinism, such explanations "are part of a wider set of constructs that attempt to justify the oppression of races, classes, and minority groups" (p. 31). It is for reasons such as these that Dupré (2003) described evolutionary psychology as "pernicious" (p. 96). Indeed, as Kaplan and Rogers emphasize, genetic determinism and biology have been adopted as a political ideology of the far right in much of the Western world, and is often invoked in discussions of race, gender, homosexuality, and IQ.

> (Greenberg, 2004, pp. 94–95)

The errors of omission and commission by proponents of evolutionary developmental psychology (e.g., see Blumberg, 2005; Kaplan & Rogers, 2003) involve, then, a failure to understand that embodiment provides the basis for epigenetic change within the life span of an individual and for qualitative discontinuity across ontogeny in relations among biological, psychological, behavioral, and social variables. Evidence for the plasticity of human development within the integrated levels of the ecology of human development makes biologically reductionist accounts of parenting, offspring development, or sexuality implausible, at best, and entirely fanciful, at worst.

For instance, Bateson (2016) notes that, "The robust mechanisms [of evolution] that make *species* different from each other also impact processes that make *individuals* distinct from one another. Children both influence their environment and are influenced by it" (p. 1). Bateson (2016) goes on to explain that:

> Recent discoveries in genetics and epigenetics have given us profound new insight into the development of the individual—an understanding marked by the dynamic interplay of *robustness* and *plasticity*. Robustness is profound and real: All humans develop certain predictable traits, and nobody will ever confuse an adult human with an adult howler monkey. At the same time, humans have a remarkable capacity for specialization and change that emerges very early in development in response to individual experiences, educational opportunities, and culture. Importantly, robustness and plasticity cannot be cleanly separated; certainly, one should not think of them in the same way as the discredited dichotomy of innate versus acquired. This is because plasticity in its many forms depends on underlying robust processes—a point illustrated by the history of behavioral biology [p. 1] . . . the key point is that the genotype of an individual can be expressed very differently depending upon the developmental environment (see Lickliter, 2016). (p. 3)

Moreover, as explained by Witherington and Lickliter (2016), the arguments of EDP proponents constitute, in essence, an essentialist approach that sees genes as the provider of the key information determining the substance of robustness of human development; the role of the non-essentialist level—the developmental environment in the terms of Bateson (2016)—is, to proponents of EDP, only the control of the emergence (or not) of genetically-based phenomena. That is, the role in human development of the levels of organization higher than the genetic one is only to manage the expression or release of the information contained in the essential, genetic level. In short, in the essentialist approach of EDP there is a Cartesian-like split between the ultimate cause of development—pre-organism-existing information, shaped by evolution (phylogeny), and inserted into the organism at conception through the content of a gene—and the instantiation of the information, which depends on the vicissitudes of everyday life, the ebb and flow of relations between the organism and its context across ontogeny (Witherington & Lickliter, 2016).

In turn, RDS-based models of human development embrace complexity (and do not reduce it to an essentialist entity), and the autopoietic process of development itself is the source of structure and function of the organism (Noble, 2015). There is no pre-existing information split off from the developmental process, and no essential level of organization to which complex higher levels are to be reduced (Witherington & Lickliter, 2016).

Witherington and Lickliter (2016) emphasize that the concept of emergence is of fundamental importance in understanding RDS-based approaches and how they differ from essentialist approaches, such as EDP (e.g., as in Bjorklund, 2015, 2016; Bjorklund & Ellis, 2005; Del Giudice & Ellis, 2016). They explain that a fundamental idea in the EDP approach is that there are entities, evolved probabilistic cognitive mechanisms (EPCMs), that pre-exist the organism and frame its development, with developmental environments across ontogeny—for example, Bronfenbrenner's conception of nested systems comprising the ecology of human development (e.g., Bronfenbrenner & Morris, 2006)—just determining what is placed within the frame. The purported process is much like how a building contractor frames a person's house but the owner of the house fills in the frame by acting to select paint colors, appliances, floor coverings, etc. In contrast, and as also emphasized by Witherington and Lickliter (2016; see too Bateson, 2015, 2016; Lickliter, 2016; Lickliter & Honeycutt, 2015; Mascolo, 2013; Overton, 2015a; van Geert & Fischer, 2009; Witherington, 2011, 2015), Raeff (2016) explains that in the RDS-based view:

> behavior emerges out of interrelations among "ongoing processes intrinsic to the system" (Lewis, 2000, p. 38). Claiming that human functioning emerges through interrelations among intrinsic constituent processes means that one does not have to involve external, antecedent, or independent factors to explain what people do. In addition, the concept of emergence stands in explicit contrast to any conceptualization of behavior and development as predesigned or predestined by, for example, genetics or how the brain is "hardwired." Rather, what a person does emerges, or is always coming into being, through the ongoing dynamics of constituent processes. (pp. 12–13)

In short, at this writing developmental science includes two very different approaches to the complexity of the integrated, multilevel, interrelated changes that everyone within the field agrees characterizes human ontogeny. One approach is an essentialist, genetic reductionist model and the other approach is RDS-based models.

What are developmental scientists to make of these two approaches to human development and its evolutionary bases? Are both useful frames for the study of human development? If so, then how should research proceed? If not, why? Again, how should research about human development proceed?

Two Approaches to Developmental Complexity

Given the features of the essentialist and the RDS approaches that Witherington and Lickliter (2016) explain in careful detail, a key question must be addressed in evaluating their respective usefulness:

Are the characteristics of an individual (a) features deriving from the constituent processes of the developmental system, or are they (b) an outcome of the developmental system acting on something that preexists and that merely awaits expression, should the organism happen to grow up in an environment "typical" for its species? From an EDP perspective, Del Giudice and Ellis (2016) contend that "while [sic] evolved mechanisms prepare an organism for life in a species-typical environment, they are not preformed or specified in advance by a rigid genetic program" (p. 7).

But where do these "mechanisms" exist and in what form? From an essentialist perspective, they must exist prior to the existence of the organism that houses them during the organism's ontogeny. Presumably these mechanisms *must* be located in the gametes of parents. But how did the information or process constituted by these "mechanisms" come to reside in the gametes?

This information or material—or whatever it is—must have come through the germ line of the parents' parents (so developmental scientists must now go to the grandparental generation for an answer to the question of the origin of the EPCMs in the development of a given, "target" individual). But the same question continues to be needed to be asked of this grandparent generation, of the one prior to it, and so on through an infinite regress that keeps the question being pushed further back in history without any definitive empirical verification. Because of this infinite regress, EDP sets up an argument that cannot be falsified by any developmental data pertinent to a target individual's life span, because there always has to be an appeal made to a former generation as the source of the "whatever."

Moreover, the idea of this whatever—for the purposes of illustration I will label it a homunculus—can only pertain to something that could actually exist *if* genes and context are split entities and, as such, if genes were then conceived of as entities that contained the homunculus, *and*, as well, if modern work in biology pertaining to epigenetics was irrelevant (e.g., Lester et al., 2016; Meaney, 2010, 2014; Moore, 2015a, 2016). Of course, in such a formulation, the homunculus could only be released if the gene was turned on sufficiently, and here proponents of EDP claim that, for such an occurrence, for the homunculus to be instantiated, the "correct" context, a species-typical one, needs to be present for at least some (unspecified) portion of ontogeny (e.g., again, see Del Giudice & Ellis, 2016, p. 7).

But here lie problems of circularity of reasoning becoming coupled with an argument already fatally flawed by the use of the unfalsifiable postulation of an infinite regress: If a developmental scientist sees the homunculus, then it *must* be the case that there was a species-typical environment, because there would be no other way for the homunculus to appear. In turn, if a developmental scientist does not see the homunculus, then it *must* be that it is absent because there was not a species-typical environment within which the organism developed. Ironically, the postulation of the existence of this prior-to-being homunculus cannot be falsified by any direct empirical evidence pertinent to the purported evolutionary (phylogenetic) history that created it. Phylogeny is not studied and, instead, reference is made to an unassessed ontogenetic history that is inferred to have existed because of the presence or absence of some behaviors that are claimed to reflect the also-never-assessed evolutionary history!

Indeed, it is ironic that the only recourse proponents of EDP have to prove their phylogenetic case is to appeal to an ontogenetic developmental process that is regarded by them to have no ultimate causal efficacy, but only the capacity to facilitate expression of an entity caused by a phylogenetic process. The morass of logical problems and appeals to impossible-to-document histories makes the cornerstone idea of EDP—EPCMs—as useful a scientific concept as is the homunculus label I have applied to it.

Importantly, developmental science was subjected to these problematic formulations before the advent of EDP. That is, the logical and empirical shortcomings of the EDP concept of EPCMs are comparable to the fatal flaws associated with the other formulations of essentialist thinking in developmental science. All of these formulations become counterfactual because of not being able to marshal the empirical evidence that is needed to support their claims about scientific usefulness.

For instance, for the nurture-reductionism of Skinner (e.g., 1971) to work as a comprehensive explanation of the behavior of organisms (Skinner, 1938), there must be an *S* for every *R*. However, as pointed out by Bowers (1973), one of the key reasons that Skinner's approach fails is the problem of the missing *S.* Simply, research has failed to identify an *S* (i.e., a discriminative stimulus, which has the status of a secondary reinforcing stimulus) for every *R* that exists. Yet, such an *S* is stipulated by Skinner to be needed to elicit operant behavior. If such stimuli are *the* causes of operant behaviors in any given situation, then how can empiricists hold that the *S*–*R* formulation (S^D–R–S^R) is useful when there are so many *R*s for which there are no *S*s to be seen? They cannot. As such, the radical behaviorism of Skinner (e.g., 1938, 1971) is reduced to a view that must be accepted on the basis of faith (that there must have been an *S* somewhere) and not on empirical evidence.

The concept of the fixed action pattern (FAP) formulated by Lorenz (1937a, 1937b, 1965) is an example of a nature-essentialist formulation, one that is similar to the EPCMs postulated by proponents of EDP (e.g., Bjorklund, 2015, 2016; Bjorklund & Ellis, 2005; Del Giudice & Ellis, 2016). Oddly, these EDP proponents are either unaware of this similarity or have elected to not note it because of the several logical and empirical problems with Lorenz's concept or, perhaps as well, because of Lorenz's own history of problems with his use of nativist ideas (e.g., see Chapter 11, and Lerner, 1992). In any case, Lorenz (1937a, 1937b, 1965) used the FAP to illustrate his concept of instinct, which he claimed was a behavior that could be observed when the individual experienced a specific "releasing" stimulus—that is, when the organism encountered a specific stimulus that "triggered" a given instinct. Lorenz posited the existence of an "innate releasing mechanism" (IRM), a hypothetical mechanism believed to involve a set of receptor cells that released the instinctual behavior pattern when activated by a specific environmental stimulus.

Blumberg (2016) provided an important critique of Lorenz's concept of instinct. He notes that instincts "are not satisfactorily described as inborn, pre-programmed, hardwired, or genetically determined. Rather, research in this area teaches us that species-typical behaviors *develop*—and they do so in every individual under the guidance of species-typical experiences occurring within reliable ecological contexts" (Blumberg, 2016, p. 1). Blumberg (2016) also illustrates the bases for this view by pointing to a 1969 article by Hailman, and explains that:

> Writing in *Scientific American*, in an article cunningly titled "How an instinct is learned," Hailman challenged Lorenz's fundamental notion of instinct: "The term 'instinct,' as it is often applied to animal and human behavior, refers to a fairly complex, stereotyped pattern of activity that is common to the species and is inherited and unlearned. Yet, braking an automobile and swinging a baseball bat are complex, stereotyped behavioral patterns that can be observed in many members of the human species, and these patterns certainly cannot be acquired without experience. Perhaps stereotyped behavior patterns of animals also require subtle forms of experience for development" (p. 241). Hailman meticulously demonstrated the influence of such subtle forms of experience through his investigations of pecking in newly hatched sea gulls.
>
> Hailman's perspective is a forerunner to today's *developmental systems* approach to the origins of abilities, traits, and behaviors. The striking observation that guides the developmental systems approach is that *processes*—sometimes obvious, sometimes subtle—give rise to the emergent properties of each individual's behavior. DNA plays a critical role in these processes, but does not by itself create traits. Accordingly, instincts are not preprogrammed, hardwired, or genetically determined; rather, they emerge each generation through a complex cascade of physical and biological influences.
>
> (p. 2)

Lorenz (1937a, 1965), however, does not agree with the RDS-based recasting of the concept of instinct; instead, Lorenz argues that experience over the course of an organism's life (its ontogeny) had no role in shaping the development of a presumed neural structure that enabled the IRM to occur. Instead the key, innate (instinctual) feature of such a neural structure was "its ability to select, from the range of available possible stimuli, the one which specifically elicits its activity, and thus the response

Mark S. Blumberg

seen by the observer" (Lehrman, 1970, p. 24). The response to the innate structure was an FAP.

As mentioned earlier in the book, the classic example of an FAP involves the male three-spined stickleback fish (Lorenz, 1965). When this fish encounters another male three-spined stickleback with a red belly, the fish displays a set of behaviors indicative of threat. In contrast, when the fish encounters a female with a swollen (but non-red) belly, the male displays the behavior pattern indicative of mating. However, a problem with the foundational argument and definition of the FAP exists. Lorenz admitted that if the appropriate releasing stimulus was not encountered for some period of time, then the FAP could occur spontaneously. That is, it "might go off in vacuo, as if dammed energy burst through containing valves" (Richards, 1987, p. 531).

It seemed obvious to Lorenz (1965) that the FAP with the three-spined sticklebacks was a behavior clearly shaped by evolution, given what he saw as the importance of the threat or mating displays by the male fish for, respectively, warding off competitors for female fish and for engagement with a possible mate if a competitor for the mate swam away in the face of the FAP. However, the problematic facet of this example and other ones of FAPs (Lehrman, 1970; Richards, 1987), which in effect might be termed an evolved probabilistic *behavioral* mechanism (EPBM), is the spontaneous enactment of the behavior. This spontaneous behavior would occur with no evolutionary-relevant stimulus (a male or a female conspecific) in view to engage the purported innate neural structure housing the IRM. Thus, the purported phylogenetic antecedent that explained the EPBM only explained it in some cases, at some times, in some contexts.

Similarly, proponents of EDP can only say that if the EPCM occurs normatively then the context was species-typical. If the EPCM is not expressed normatively then this manifestation of behavior is taken as proof for the existence of a context (and typically a *never measured* context) that was atypical. Like Skinner (e.g., 1971), in regard to the postulation that a relevant (releasing) *S* must have been present if one saw an *R* (Bowers, 1973), the outcome in the formulation of EDP, the appearance (or lack thereof) of the EPCM, explains the antecedent that purportedly explains the outcome!

Thus, the ideas of the proponents of EDP (e.g., Bjorklund, 2015, 2016; Bjorklund & Ellis, 2005; Del Giudice & Ellis, 2016) converge with those involved in other essentialist formulations. Whether developmental scientists are discussing EPCMs or EPBMs, they will confront the same problem: Neither concept is linked to a developmental process that identifies the essentialist "mechanisms" independent of its ontogenetic emergence or their subsequent display in ontogeny after their emergence. Developmental scientists cannot say that these homunculi, EPCMs, EPBMs, FAPs, or whatevers, are always there, independent of context, because the context instantiates them (or does not instantiate them as the case may be). However, the instantiation by the context will be different under different environmental conditions (e.g., species typical versus atypical, or red underbelly present versus red underbelly absent), and therefore the whatever (e.g., the homunculus or the EPCM) cannot be known to exist in any form without the context.

Conclusions about EDP

There is no way of knowing the purported evolutionary-based "whatever" independent of the ontogenetic context. Simply, then, and at best, the foundational concept of EDP (the EPCM), the essentialist approach to the complexity of human development, is entirely nonempirical and gratuitous. At worst, the concept of EPCM is so fraught with logical, conceptual, and empirical problems that using it as a basis for research in human development is a scholarly dead end.

The objections that proponents of EDP have to RDS-based approaches to the complexity of human development (e.g., Bjorklund, 2016) reflect at best a lack of understanding of the dynamics of the relational developmental system (e.g., see Raeff, 2016, and Witherington & Lickliter, 2016, 2017). Whatever the basis of the objections of proponents of EDP, however, their views have resulted in their invention of the equivalent of a homunculus to explain—or, perhaps better, to try to explain away—the holistic and autopoietic features of the relational developmental system.

CONCLUSIONS ABOUT GENETIC REDUCTIONIST THEORIES OF HUMAN DEVELOPMENT

Developmental science may be at a crossroads. Given the irreparable logical and empirical shortcomings of essentialist approaches to human development, there is no scientific value in the continued theoretical or empirical use of these ideas, whether developmental scientists are discussing past instantiations of them, such as those forwarded by Skinner (1971) or Lorenz (1965), or examples of them present in the essentialist and reductionist literatures at the time of this writing, for instance, EDP (e.g., Bjorklund, 2015, 2016; Del Giudice & Ellis, 2016), sociobiological neo-eugenics (e.g., Belsky, 2014), or behavior genetics (e.g., Plomin et al., 2016). Focusing on these flawed ideas as a basis for research or as a means to formulate applications to social policies or programs has the potential to foster applications of developmental science that are derived from seriously mistaken ideas (Lerner, 2015b, 2015c).

For example, Jay Belsky, writing on the Op-Ed page of the Sunday, November 30, 2014 edition of *The New York Times*, posed the question of "why some children come out of development programs with enhanced capabilities and fewer behavioral problems, while others don't seem to be affected very much—or at all," and suggested that, "Eventually, we may be able to identify the children who will benefit the most, and consider investing extra resources in them" (p. 1). He then asked, "What distinguishes children who prove more versus less susceptible—for better and for worse—to developmental experiences?" (p. 1), and he answered this question by asserting that, "There is no single factor, but genetics seems to play a role" (p. 1).

Belsky argued, therefore, that children varied in their susceptibility to profiting from interventions aimed at enhancing their behavior and development and that it was the possession of specific genes that afforded youth the capacity either to gain or not to gain from such interventions. If children had genes that enabled them to be susceptible to interventions aimed at promoting their positive development, then they would profit from experiencing such interventions. If children did not have the "right" genes, exposing them to these interventions would be a waste of money. Given that the capacity to gain from interventions was genetically based, Belsky then asked, "Should we seek to identify the most susceptible children and disproportionately target them when it comes to investing scarce intervention and service dollars?" (p. 3). His reply was, "I believe the answer is yes" (Belsky, 2014, p. 3).

Belsky (2014) then went on to say that:

> Those who value equity over efficacy will object to the notion of treating children differently because of their genes. But if we get to the point where we can identify those more and less likely to benefit from a costly intervention with reasonable confidence, why shouldn't we do this? What is ethical, after all, about providing services to individuals for whom we believe they will not prove effective, especially when spending taxpayers' money?
>
> One might even imagine a day when we could genotype all the children in an elementary school to ensure that those who could most benefit from help got the best teachers. Not only because they would improve the most, but also because they would suffer the most from lower quality instruction. The less susceptible—and

> more resilient—children are more likely to do O.K. no matter what. After six or seven years, this approach could substantially enhance student achievement and well-being.
>
> Let me say clearly that even if targeting can be done effectively, it doesn't mean abandoning those who appear less responsive. Every child deserves a decent quality of life, no matter the cost or long-term payoff. Furthermore, money saved by restricting interventions to those most likely to benefit should be used to explore different and conceivably radical intervention alternatives.
>
> (p. 4)

But what are the radical interventions Belsky envisions? He does not say. However, in Chapter 11 I noted the recommendations made by Lorenz (e.g., 1940a, 1940b) in regard to the interventions that needed to occur to address societal problems caused by people whose genes made them ethically inferior or, even earlier, by Binding and Hoche (1920), who appealed to the German people to sanction the destruction of lives they regarded as unworthy to be lived because of the possession of genes constraining these lives. Scientists and society have witnessed the human devastation caused when fatally flawed and counterfactual genetic reductionist ideas are used to frame social policy. I fear that the policy recommendations of Belsky will take society down a comparable path.

Indeed, given the likely mind sets and political views of politicians who, on the basis of such flawed evidence, enact policies that result in denying money to be spent on children possessing "incorrect" genes, I wonder if it is plausible to suggest, as Belsky did, that the money saved because it was not invested in children with purportedly flawed genotypes would then be invested in radical interventions aimed at promoting their positive development. I fear that the policies and programs that would be likely to be supported by policy-makers would continue the sort of new eugenics initiatives that Belsky suggests.

Accordingly, I think that developmental scientists enacting many of the roles associated with their work—for example, faculty members participating in hiring, tenure, and promotion decisions, teachers, mentors, peer reviewers, editors, and of course researchers—are faced, then, with a decision. Do they embrace the complexity of human development in the enactment of all of their roles and, as such, articulate that essentialist, genetic reductionist approaches are no longer acceptable frames for conducting and applying developmental science? Developmental scientists would likely take such actions if they were faced with evaluating work that used phrenology as the frame for scholarship. Alternatively, do developmental scientists allow egregiously flawed thinking and associated work to fill the minds of their students and the pages of their journals (in the name, perhaps, of academic freedom)?

I articulate the dimensions of this decision with more than a little trepidation, given the range of responses I expect it will elicit. However, more than the quality of developmental science is at stake. Developmental scientists should also recognize that civil society may hang in the balance, given the repeated applications of essentialist thinking finding their way into public policy discourse in the United States and internationally at this writing, for instance, regarding political nativist ideas about racial, ethnic, and religious diversity and about immigration and immigrants (e.g., see Feldman, 2014; Sarason, 1973). The quality of life and the welfare of millions of people may be affected by where developmental scientists stand in regard to these issues and what they may be willing to say publicly about them.

What then might developmental scientists do? As citizens, developmental scientists can act by exercising the rights of any citizen (Lerner, 1976, 2004). However, as scientists, developmental scientists can act to describe, explain, and optimize human development across the life span (of course). In such efforts, developmental scientists would do well to heed the advice of Horowitz (2000) in regard to how, in the face of the simplistically seductive ideas of hereditarianism, they must find the will to act in a manner supportive of social justice. Horowitz (2000) noted that:

> If we accept as a challenge the need to act with social responsibility then we must make sure that we do not use single-variable words like genes or the notion of innate in such a determinative manner as to give the impression that they constitute the simple answers to the simple questions asked

> by the Person in the Street lest we contribute to belief systems that will inform social policies that seek to limit experience and opportunity and, ultimately, development, especially when compounded by racism and poorly advantaged circumstances. Or, as Elman and Bates and their colleagues said in the concluding section of their book *Rethinking Innateness* (Elman et al., 1998), "If our careless, under-specified choice of words inadvertently does damage to future generations of children, we cannot turn with innocent outrage to the judge and say 'But your Honor, I didn't realize the word was loaded.'"
>
> (p. 8)

What developmental scientists can do, then, is to frame their work with terms that reflect the holism, plasticity, and optimism about the possibility of promoting positive human development by theory-predicated integration of individuals and contexts, by promoting *adaptive* individual↔context relations. In addition, they can then couple this language with the appropriate methodology in order to conduct research that provides evidence about the dynamics of the relational developmental system and the health and thriving that can be promoted among diverse individuals by appropriately engaging this system. With such evidence in hand, developmental scientists can then offer evidence-based applications that contribute to policies and programs promoting positive human development. In Chapter 13, I discuss key issues of research methodology and application that developmental scientists will need to address to enact this work.

CHAPTER THIRTEEN

Some Implications of Relational Developmental Systems-Based Theories for Research Methods and for the Application of Developmental Science

Throughout the prior chapters of this book, I have emphasized that developmental science seeks to describe, explain, and optimize within-person (intraindividual) change and between-person (interindividual) differences in intraindividual change. In short, then, to study human development a prime emphasis is placed on within-person change. To study such change, it is clear that developmental methods must include research designs, measures, and data analytic procedures that are change-sensitive, that is, methods that are capable of detecting within-person change, if it in fact exists, must be used.

Of course, in the conduct of good science, the method a researcher uses should derive from the question he or she seeks to address in a specific study and, as suggested by several developmental methodologists (e.g., Collins, 2006; Little, Card, Preacher, & McConnell, 2009), the most useful questions to ask are theory-predicated ones. A key implication of the epigraph that opened the first chapter in this book, is that theory builds the facts of research into a coherent conceptual edifice (von Bertalanffy, 1933).

There may be many different theory-predicated questions that can be asked by researchers. In addition, practical issues may be frames for useful questions (Lerner, 2002; McCandless, 1967, 1970). Whatever the source of questions might be, a researcher should select the methods that are best suited to address his or her question. Researchers should not (in my view) only seek to address questions that can be answered by a specific method (Lerner, Lerner, Urban, & Zaff, 2016).

In addition, among the virtually limitless questions a researcher could ask about human life, not all of them do or necessarily should pertain to within-person change. Therefore, not all questions about human life need to be coupled with change-sensitive designs, measures, or data analysis techniques pertinent to an individual's pathway across his or her life.

For instance, it is completely legitimate to ask questions about how groups differing in age at one point in time behave. As such, there is an important place for cross-sectional designs within the methodological toolbox of researchers interested in such point-in-time questions about differently aged groups. It is also legitimate to ask questions about the "behavior" of variables, and not individuals. Researchers may be interested in assessing how two variables (e.g., a measure of identity and a measure of civic engagement) covary within a specific time of measurement and/or if a measure of one of the variables at one point in time (e.g., identity, measured at the beginning of high school) predicts the other variable at some subsequent time (e.g., civic engagement after high-school graduation).

A researcher asking such a variable-focused question would not need, then, to select measures sensitive to changes within a person across these time periods. It is also legitimate to ask questions about whether it is possible to develop a measure of, say, personality attributes, that can show stability across ontogenetic time periods and across contexts of development (e.g., Costa & McCrae, 1980; McCrae et al., 2000). In such cases, the goal of data analyses would be the computation of statistics reflecting such constancy (e.g., stability coefficients).

In short, developmental scientists should have numerous "arrows" in their methodological "quiver" (Overton, 2014), and no method should be either tossed aside or elevated to exclusive primacy (Lerner & Callina, 2014b). Every method can play an important role in advancing knowledge about human life and development, depending on the question asked by a researcher. However, when asking questions about the description, explanation, and optimization of *intraindividual* change, change-sensitive methods will be essential to use. These methods are the only ones suited for understanding within-the-individual pathways (trajectories) (Molenaar & Nesselroade, 2014, 2015; Rose, 2016).

There is a "catch" here, however, in making this argument. Many researchers working in the field of human development, although recognizing that this domain of scholarship pertains to within-person change, recognize as well that using change-sensitive designs, measures, and data analytic techniques is quite difficult. Accordingly, they are drawn to the belief that, whereas cross-sectional designs, variable-centered measurement models, and data analysis techniques that focus on group averages are not equivalent to methods indexing within-person change, they provide "good enough" approximations of such change to legitimate their use. A key purpose of this chapter, and indeed of this book, is to show that such "good enough" approximations are not good enough at all. They are in fact inadequate.

Moreover, when questions about intraindividual change are theory-based, and when these questions are linked to relational developmental systems (RDS)-based ideas about individual↔context coactions, then such change-sensitive methods may be uniquely useful. Nevertheless, other methods should not be abandoned or regarded as somehow inferior. Within the three moments of RDS-based research programs discussed by Overton (2015a), that is, the identity of opposites, the opposites of identity, and the synthesis of wholes, other methods (e.g., cross-sectional studies or variable-centered analyses) may play vital roles in adequately addressing a specific question. However, when questions pertain to changes in an individual across ontogenetic time and place, change-sensitive methods need to be used to depict changes within a person (Elder et al., 2015).

USING RDS-BASED IDEAS AS A FRAME FOR DEVELOPMENTAL METHODS

Across the prior chapters of this book, I pointed to some of the implications of RDS metatheory and RDS-based concepts and theories for research methods and for applications to policies and programs aimed at promoting positive and healthy human development. In this chapter my goal is not to reiterate these discussions. Instead, my intent is to extend my prior points to more general facets of research methods and applications of developmental science.

As framed by RDS-based concepts and theories, the superordinate methodological issue in developmental science is how to best conduct research that validly assesses theoretically-predicted changes pertinent to an individual, that is, the specific changes in individual↔context relations that comprise a person's pathway across his or her life (Bornstein, 2017; Rose, 2016). As well, RDS-based studies of change may involve appraisal of differences between individuals in their trajectories of individual↔context relations. In addition, when developmental science is framed by concepts associated with RDS metatheory—for instance, by ideas such as relative plasticity, holism, agency, and autopoiesis—I believe that the superordinate issue for the application of developmental science is: How can evidence derived from RDS-based research be transformed into actions enhancing the life chances of each individual and, through the systematic accumulation and integration of such person-specific information, provide evidence that might be useful in creating a world marked by social justice and equity? I turn first to some of the methodological problems that I believe must be addressed to study specific, theoretically-predicted change within the relational developmental system.

Problematics in the Study of Change within RDS-Based Models

Change is difficult to study empirically. This challenge may be especially the case in regard to developmental change, in that (as I have noted throughout this book) development is primarily a theoretical construct and not an empirical one (Lerner & Nesselroade, 1991). Indeed, the study of changes across the life span has been beset by both logical and methodological problems.

For instance, one logical problem involved in developmental research occurs in regard to what are termed follow-up versus follow-back designs. The former design is also termed a prospective design and the latter design is also termed a retrospective design. In a follow-back design, a researcher identifies a contemporaneous instance of behavior among a sample and tries to ascertain whether the members of the sample had some common, earlier experience. For example, suppose a researcher finds that, among 100 men incarcerated for sexually violent crimes, all had been physically abused in the first five years of life. If the researcher concludes, therefore, that early abuse is a general antecedent (e.g., a cause) of later sexual violence, he or she is making a logical error.

That is, follow-back (retrospective) data do not reveal how many people who have an experience at one point in development show a given behavior at a later time in life. Such data are provided through follow-up (prospective) designs. For instance, in a follow-up (prospective) design, children who experience early physical abuse would be followed to adulthood, and the proportion of abused children who eventually became incarcerated for sexual violence would be determined. It is not logical to assume that, because most men incarcerated for sexual violence were abused as children, as a consequence most men who were abused as children end up as people who commit sexually violent acts. To illustrate the lack of logic that may occur with follow-back designs, consider the case of individuals who have earned a Ph.D. degree. It is reasonable to assume that all people who end up with Ph.D. degrees graduated from high school at an earlier point in their lives. However, not all people who graduate from high school go on to earn a Ph.D. degree.

Another logical problem associated with interpreting longitudinal data may occur when sequential changes are interpreted causally. That is, when a researcher assumes that *post hoc ergo propter hoc* (a Latin phrase that means after this, therefore because of this), he or she is making a logically inappropriate causal inference. Suppose, for instance, that a farmer arose every morning at 4:30 a.m. to begin his or her daily chores and then, invariably, an hour or so later (depending on the time of year of course) the sun then rose. It would be fallacious to infer that the rising of the farmer was the cause of the sun's then rising, and no developmental scientist would make this inference. However, it may be more likely that such a logically flawed inference might be made when sequential events within a person's life are studied. For example, say a developmental scientist observes a specific pattern of child↔parent relations (e.g., authoritative parenting; Bornstein, 2015) at Age 1 in a child's life and then observes a specific behavior at Age 2 in the child's life (e.g., prosocial behavior). If the researcher then infers that the events observed at Age 1 caused the behavior observed at Age 2, he or she would be making the logical error of *post hoc ergo propter hoc*.

Of course, events in ontogeny may be causally linked. However, causality may be inferred only when theory is coupled with data from well-designed longitudinal research that are then analyzed in manners appropriate for testing for causal relations (e.g., through procedures that address selection effects, or endogeneity, and/or that model causal pathways; Little et al., 2009; Laursen, Little, & Card, 2012; Teti, 2005). Again, then, as emphasized throughout this book, theory is, or at least should be, the starting point for the use of developmental methods. Indeed, as I have already noted, developmental methodologists (e.g., Collins, 2006; Little et al., 2009) maintain that theory is (or, at least, should be) the major and, as well, the primary tool of the developmental scientist.

How, then, should theory be used to frame subsequent methodological choices? Methods used to study developmental change should be predicated on theoretical specification of the nature of development and on the assumptions one derives from theory about:

1. *Units of analysis* (e.g., individual-psychological variables or, from an RDS-based perspective, *relations* among variables from different levels of analysis, as they are evolving across ontogeny within an individual; e.g., Mascolo & Fischer, 2015; Molenaar & Nesselroade, 2015; Nesselroade & Molenaar, 2010; Rose, 2016; von Eye, Bergman, & Hsieh, 2015).
2. *Levels of organization* involved in developmental change (e.g., genes, organism, social relationships, or culture; e.g., Fischer & Bidell, 2006; Gottlieb, 1997, 1998; Mascolo & Fischer, 2015; Rogoff, 2003, 2011).
3. The role of *time and temporality* (history) in indexing such change (Baltes, Reese, & Nesselroade, 1977; Bronfenbrenner & Morris, 2006; Elder, 1998; Elder & Shanahan, 2006; Elder et al., 2015; Schaie, 1965).

In other words, developmental theories vary in regard to the units of analysis and the levels of organization used to study individuals across time and place.

This variation is linked to differences in the approach to research taken by scholars following different theories of development. The conceptual pathway from paradigm, through metatheory and theory, to method, and then, eventually, to data, is illustrated in the funnel diagram presented in Figure 1.1 (in Chapter 1).

What are some of the specific implications of RDS-based theories for research? Entire handbooks (e.g., Molenaar et al., 2014), major sections within handbook volumes (e.g., Overton & Molenaar, 2015), and monographs (e.g., Molenaar & Newell, 2010) have been devoted to addressing this question, that is, to elucidating the connections between RDS-based concepts and theories and specific methodological issues and options. Therefore, my goal in this chapter is much more modest than the scholarship in these other works. My goal is only to illustrate the sort of methodological thinking a developmental scientist might use when his or her scholarship is predicated on RDS-based models (see too Baltes et al., 1977; Fischer & Bidell, 2006; Mascolo & Fischer, 2015; Molenaar & Nesselroade, 2012, 2014, 2015; Nesselroade & Molenaar, 2010; Ram & Grimm, 2015; Rose, 2016, Rose et al., 2013; Tolan & Deutsch, 2015; von Eye et al., 2015).

Variable versus Person-Centered Foci

RDS-based models emphasize that reciprocal changes within and among levels of organization are both products and producers of other reciprocal changes within and among levels. When these models are aimed at elucidating ontogenetic/within-person variation, individual↔context relations should be the units of analysis in research seeking to describe, explain, and optimize both intraindividual change and interindividual differences in intraindividual change. Indeed, the specificity principle (Bornstein, 2017) that I discussed in prior chapters provides a frame for asking RDS-based questions about the relations between individuals and their contexts that result in specific pathways across ontogeny. Bornstein (2017) explains why the specificity principle is an essential frame for designing, conducting, and interpreting data from developmental research. He notes that:

> In life-span human development, some characteristics and experiences have broad implications. Where one is born, how much education one accrues, one's gender, as examples, doubtless have pervasive consequences over the life course. Even so, as life proceeds, advantages and disadvantages cumulate to heterogeneity—so much so that variability and therefore specificity are inevitable. That is, the life-span development of specific characteristics in specific individuals is affected by specific experiences in specific ways at specific times—this is the specificity principle. To complement universals, understanding often depends on what is studied, in whom, how, and when.
>
> The specificity principle therefore differs from many common assumptions, for example that

> overall stimulation influences overall development or better nutrition ensures better health. Some such generalities may be valid. However, it is not the case necessarily that a monolithic global shared experience affects performance in all areas of life, is adequate to adaptive functioning, or compensates for selective deficiencies. Familial love, financial well-being, or a stimulating environment do not guarantee, or even speak to, lifespan development of specific characteristics, such as a healthy diet, empathic personality, verbal competence, sports prowess, ethical action, or myriad others. Rather, contemporary science indicates that specificities are often at play. The specificity principle advances a theory that is particularistic in nature, such that development depends on several separate identifiable factors, including the experience involved, who experiences and who generates the experience, when in life the experience occurs, how the experience occurs, and the domain of development affected by the experience.
>
> (Bornstein, 2017, p. 5)

Given, then, the specificity of an individual's development, RDS-based models emphasize the idiographic character of intraindividual change (e.g., Molenaar & Nesselroade, 2015; Rose, 2016) and, as such, the importance of using methods that illuminate the person's specific trajectory across life. Developmental science is, then, a science of the individual's development and not a field focused on the tracing of changes in a variable or among variables across points in time (or across people).

Thus, at the least, methods in RDS-based research about intraindividual change should be person-centered and not variable-centered. However, person-centered is understood to involve more than a group of people (or a subgroup of a sample) who have patterns of covariation among variables that place them in a class or a trajectory different from that of another group of people. Given that the unit of analysis in RDS-based models is the individual↔context relation, a specific (idiographic) conception of person-centered is of prime theoretical interest in understanding human development within the autopoietic relational developmental system (Witherington, 2014, 2015). It is the single person, with all the details of his or her specificity noted by Bornstein (2017), that is at the core of the meaning of person-centered. This conception of the individual is consistent with RDS-based ideas (Molenaar & Nesselroade, 2014, 2015; Rose, 2016; Stafford-Brizard, Cantor, & Rose, in press). Thus, as may be inferred from my discussion in Chapter 5, person-centered approaches that pertain to specific subgroups of individuals are more associated with differential approaches than with the idiographic ones. Moreover, because of temporality, change is constant within the relational developmental systems, and developmental science questions derived from this perspective should *not* ask, therefore, whether there is change in the specific trajectories of individuals but, rather, if and how one instance of a specific change in a specific trajectory (for a specific person) matters for another specific instance of change in that trajectory (Lerner, Agans, DeSouza, & Hershberg, 2014).

However, as I noted in prior chapters, Molenaar (e.g., 2014; see too Nesselroade & Molenaar, 2010; Molenaar & Nesselroade, 2012, 2014, 2015) explained that the standard approach to statistical analysis in the social and behavioral sciences is not focused on change but, instead, derived from mathematical assumptions regarding the constancy of phenomena *across* people and, critically, time. He noted that these assumptions, the ergodic theorems, lead to statistical analyses placing prime interest on the population level. Interindividual variation, and not intraindividual change, is the source of this population information (e.g., Molenaar, 2014; Molenaar & Nesselroade, 2012, 2014, 2015).

Within the process-relational paradigm (Overton, 2015a), development is nonlinear and characterized by autopoietic (self-constructing) and hence idiographic intraindividual change, features of human functioning that violate the ideas of ergodicity. Accordingly, use of the RDS metatheory as a frame for research requires a rejection of primary (or initial) use of data analytic tools predicated on the ergodic theorems that constitute the bases of traditional statistical procedures (e.g., Molenaar & Nesselroade, 2012, 2014; Nesselroade & Molenaar, 2010), procedures that describe properties of variation across individuals in a sample (through the computation of sample statistics such as averages and standard deviations (Rose, 2016). As discussed in prior chapters, a normally distributed ergodic process has to be stationary, or constant, across time, and homogeneous across individuals (Molenaar, 2014; Molenaar & Nesselroade, 2012, 2014, 2015). However, developmental processes vary across time, place, and individuals. Thus, depicting variation across people is not equivalent to variation at the level of the individual (Molenaar, 2014). Intraindividual developmental processes are therefore non-ergodic.

As a consequence, to obtain valid information about developmental processes it is necessary to study intraindividual variation within single individuals, and Molenaar and Nesselroade (e.g., 2012, 2015; Nesselroade & Molenaar, 2010) have developed such procedures (e.g., the Idiographic Filter, which first analyzes variation at the individual level and then appraises if it is empirically appropriate to aggregate data across individuals) (see too Ram & Grimm, 2015). Through use of such "analyze (at the individual level) and then (if appropriate) aggregate" procedures, developmental scientists can capture the non-ergodic nature of intraindividual change, and may be able to produce generalities about groups that apply as well to the individuals within them.

How, then, may research proceed? Consistent with the Bornstein (2017) specificity principle, I suggest formulating and addressing a version of the sort of multi-part "what" question, that I illustrated in prior chapters, in order to conduct programmatic research about the function, structure, and content of development across the life span. That is, to test RDS-based ideas about the ontogenetically changing structure of development across the life span—to test empirically the process-relational conception of intraindividual change (e.g., Overton, 2015a; Sokol, Hammond, & Berkowitz, 2010; Sokol et al., 2015)—the task for developmental researchers is to undertake programs of research to ascertain answers to a multi-part "what" question of the following sort:

1. What specific structure–content relations emerge; that are linked to
2. What specific antecedent and consequent adaptive developmental regulations (to what specific trajectory of individual↔context relations); at
3. What specific points in development; for
4. What specific individuals; living in
5. What specific contexts; across
6. What specific historical periods?

Collecting data addressing these questions is an arduous—and often an expensive—task, one requiring longitudinal studies involving sufficient power for conducting quantitative analyses of several specific individuals. As such, issues of both research design and analysis are raised. These issues are of course interrelated. Thus, discussions of them often need to involve shifting emphases between design and analysis. Nevertheless, I will begin my discussion of these issues by first foregrounding design issues.

Designs of Developmental Research: Problematics of Time and Timing in the Longitudinal Study of Human Development

To study the development of an individual, a developmental scientist needs to use a design of research that enables within-person changes to be appraised. The developmental scientist must, therefore, conduct research that is longitudinal in design (Baltes et al., 1977; Collins, 2006; Little et al., 2009; Nesselroade & Baltes, 1979; von Eye, 1990a, 1990b). However, the term *longitudinal design* is a generic label pointing to an array of different temporal designs used to include multiple time points at which to observe

Linda Collins

change. Just as there are numerous designs for controlled experiments (e.g., the two-group, test and control design versus the Solomon four-group design; Solomon & Lessac, 1968), there are also various longitudinal or temporal designs (Collins, 2006).

For instance, Collins (2006) differentiated between panel and intensive longitudinal designs and, in turn, Nesselroade (Corneal & Nesselroade, 1991; Nesselroade & Ford, 1985) and Collins provided different specifications for the features of intensive longitudinal designs. Schaie (1965), Schaie and Strother (1968), and Baltes et al. (1977) contrasted single-cohort longitudinal designs with sequential design strategies and, for instance, described several variations of the latter methods (e.g., cohort sequential, time sequential, and cross-sectional sequential designs). In addition, Collins discussed accelerated longitudinal designs, which are special cases of cohort sequential methods. Table 13.1 provides some of the defining features of key instances of longitudinal designs.

However, my purpose is not to review these designs, a task addressed in numerous textbooks over the course of the last 40-plus years (e.g., Baltes et al., 1977; Laursen et al., 2012; Lerner, 2002; Teti, 2005). Instead, my purpose is to explore several important issues that arise when *any* temporal or longitudinal design is selected on the basis of fitting *any* theory-derived developmental question. I focus on issues that arise when investigators are making decisions about theory–design fit. These issues involve the treatment of time and the timing of observations that developmental scientists face when seeking to chart in change-sensitive ways variation in developmental processes across life. These issues are generic to temporal designs and are not specific to any particular longitudinal design.

A key reason why consideration of these issues is important is that decisions about which temporal design to use should not be based on what Lerner and Overton (2008) discussed as mindless methodologism, that is, allowing the preference for one or another research design to dictate or constrain the questions one asks about the substance of human development. To reiterate my earlier point in this chapter, in good science, method selection should follow from theory-predicated questions. Thus, in an ideal world, decisions about the features of

Table 13.1 Features of key instances of longitudinal designs

Single-cohort designs
A group of individuals (e.g., members of the same birth cohort, people born in 1990) is studied repeatedly, that is, at multiple occasions (2 or more times, e.g., 2000 and 2001).

Panel designs
A cohort (e.g., people born in 1990) is studied at 3 or more times (e.g., 2000, 2001, and 2002). Collins [2006] defines these designs as involving 8 or fewer times of measurement that are separated by at least 6 months.

Intensive designs
A person or group is studied on a large number of occasions. Collins [2006] defines these designs as involving at least 20, relatively closely spaced times of measurement. However, Nesselroade [e.g., Nesselroade & Ford, 1987] indicates that, when a single person is studied, there may be 100 or more times of measurement.

Sequential designs
Multiple cohorts of individuals are studied repeatedly, typically at 3 or more times. Attrition and retest control groups are often part of sequential designs.

Accelerated designs
Multiple cohorts of individuals are studied repeatedly, and the cohorts differ in age at time I of measurement. However, the initial ages of the cohorts (e.g., 10, 12, 14, and 16 years of age), and the points in time at which they are measured (e.g., 2000, 2001, and 2002), are selected to assure that members of the different cohorts will be of at least one identical age at different time points. The overlap of ages enables change across years to be estimated in less than elapsed calendar time.

methodology to be used in a specific study should not be based on convenience, staff resources, or funding. However, often, in the practical world of conducting research with human participants, these issues may in fact determine to great extents the schedule of repeated testing in longitudinal studies and how many participants a researcher can afford to study. A developmental scientist may have good, theory-predicated reasons to want to retest a sample of participants every six months across a specific portion of the life span (e.g., say in the transition from elementary school to middle school and then from middle school to high school; e.g., Simmons & Blyth, 1987). However, issues of the availability of funding for conducting all these waves of testing, participant fatigue in the face of frequent testing, and the aversion of schools to having their regular class schedule interrupted repeatedly by researchers may be among the factors making annual testing the only feasible design option.

Three Bases of Methodological Decisions in Longitudinal Research

Many developmental methodologists (e.g., Collins, 2006; Little et al., 2009; Nesselroade & Baltes, 1979; von Eye, 1990a, 1990b) emphasize that substantive decisions about research designs (as compared to practical decisions) should be based on three considerations. First, and as already emphasized, *methodological choices should be based on theory*. For instance, Collins (2006) noted that only when there is a "well-articulated theoretical model of change" (p. 507) can longitudinal research begin to approach the ideal of providing "clear and unambiguous" answers to questions about the nature of human development. In addition, Little et al. (2009) emphasized that "longitudinal data are best suited for testing hypotheses derived from well-articulated models of change. In this regard, theory is the [data] analyst's best friend" (p. 6). In short, the developmental question a developmental scientist is asking and the theoretical conception of the substantive phenomenon/process he or she is studying should be the basis of any decisions about methodology (Lerner et al., 2009).

Todd Little

The second key consideration that needs to be addressed in making decisions about research involves the question of "What design will best elucidate the theoretical issues being addressed?" That is, the investigator must seek to *optimally match the theory about the substantive phenomenon of interest with the design that will best enable observation of changes in the phenomenon*. In developmental science, this question becomes "What temporal (longitudinal) design affords optimal fit with the theory-predicated question of the developmental process being addressed?'

Third, the investigator must consider *how best to match the design that is selected with measures that have been designed to be change-sensitive*; that is, the researcher must ask if the measure he or she is using can detect change if it occurs. As well, the investigator must use statistical procedures that will best exploit the data that will be collected, in order to assess change if, in fact, it is in evidence. The results of such statistical analyses, and the resulting conclusions drawn about the study hypothesis or question, can then be used to refine or extend theory or to design intervention programs to promote successful development. The study of development therefore requires a dynamic interplay among theory, research design, measurement, and statistical analysis (Baltes et al., 1977; Collins, 2006), which is why discussions of these facets of developmental methodology need to be understood (and often discussed) in an interrelated manner.

These ideas are not new, as evidenced by the dates of many of the above-cited works. For instance, chapters by Collins (2006) and Little et al. (2009) highlighted the need to continue to push these ideas to the forefront of developmental science. I hope to support this effort by highlighting a set of issues that must be addressed in longitudinal research if it is going to be able to serve the three important functions of developmental science, that is, describing, explaining, and optimizing developmental trajectories.

Accordingly, I will build on the work of Paul Baltes, Linda Collins, Todd Little, John Nesselroade, Peter Molenaar, Todd Rose, and Alexander von Eye, and others and discuss the second issue that must be addressed in conducting longitudinal studies of human development, that is, the fit between theory and type of longitudinal design. By focusing on theory–design fit I hope to illuminate several issues that must be addressed when selecting a temporal design to observe change phenomena, issues that arise independent of whether one is conducting research framed by theoretical models that focus on either qualitative change (e.g., stage theories of development; Piaget, 1970), quantitative change (e.g., in scores for fluid or crystallized mental abilities; Horn, 1968), or both quantitative and qualitative changes (e.g., dynamic system models of development; Mascolo & Fischer, 2015).

A key issue here is one of conceptualizing the timing of observations. This issue must be addressed during the decision-making process of matching theory about the specific substantive phenomenon with the temporal design used to observe changes in the phenomenon.

Alexander von Eye

Dividing the x-Axis: The Temporal Spacing of Observations

I believe that the temporal division (spacing) of the x-axis in longitudinal research is the most unrecognized problematic in developmental science (Lerner et al., 2009). Both researchers and the funding agencies that support their work have shown little recognition that the use of calendar time to divide the axis is not necessary, that age may not be the best way to represent time, and that equal interval spacing along the axis may not be appropriate in all situations (Collins, 2006; Little et al., 2009). There has not been awareness that annual, semi-annual, or even shorter temporal divisions may not be appropriate conceptually. In other words, time-related changes in developmental processes may occur at the same rate across units of time arrayed across the x-axis or, alternatively, at different rates across adjacent or nonadjacent demarcations of the x-axis.

As such, when, and how often, to measure (observe) a specific developmental process (or an indicator of the process) should be the primary questions in making decisions about how the x-axis should be divided—and, ideally, theoretical understanding of the process of change should be used as the frame for addressing these questions. That is, the rate of change specified by theory, and/or theoretically-specified ontogenetic "tipping points," when transformational changes in a process are expected to occur, should be used to guide the selection of assessment points for any study of a specific developmental process.

If a specific developmental process may not change at a constant rate across all of ontogeny, then it is also the case that the variation observed across x-axis divisions could occur for both qualitative and quantitative facets of development. Qualitative change may emerge in gradual and continuous ways (e.g., as in the notion of stage mixture discussed by Turiel, 1969, when the transition between developmental stages is marked by the increasing presence of the structures, or qualities, of a new stage and the decreasing presence of the structures of prior stages), or may be marked by an all-or-none-type change, as in Anna Freud's (1969) idea that the rapid emergence of the sex drive creates a developmental disturbance in early adolescence. In turn, quantitative change is continuous when the slope of change remains the same across a set of sampled points in ontogeny, and is discontinuous when the slope changes across adjacent ontogenetic points (a phenomenon that Werner, 1957, labeled abruptness; see Chapter 8). For either qualitative or quantitative changes, then, the developmental process may be faster, slower, or take different forms across a set of sampled points in ontogeny.

For instance, feelings pertinent to emotional well-being (e.g., self-esteem) may fluctuate rapidly among adolescent girls during the transition from elementary school to middle school (e.g., Simmons & Blyth, 1987). In the spring of the pre-transition school year (i.e., fifth grade) self-esteem may be high, but it may decrease precipitously in the fall of the post-transition school year (sixth grade), show some positive increase by mid-year (but still be substantially lower than the pre-transition point in time) and, by the spring of the post-transition year, self-esteem may return to the pre-transition level. Whereas annual assessments of self-esteem may have been useful to mark the level of this construct during the elementary school years, and annual spacing along the x-axis may again be useful for the second and third years after transition, such a division during the first post-transition school year would mask the curvilinear character of the course of self-esteem change during this period.

Within this transition period, finer divisions of the x-axis would be needed to index with sufficient sensitivity the actual course or form of change in this construct. Accordingly, divisions of the x-axis do not need to be spaced evenly, and spacing them evenly may actually misrepresent the developmental process being investigated (Lerner et al., 2009). Inappropriate aggregation can also result in misrepresenting the form of the trajectory, such as fitting a linear slope to a nonlinear trajectory. Although this error can happen regardless of how time points are spaced along the x-axis, arbitrarily deciding to evenly space time points (which is the most common x-axis spacing decision in longitudinal research) represents one way in which this problem can be created.

As I emphasized, spacing should be dictated by theoretical understanding of the form of the developmental process. Such understanding may involve specification of the ontogenetic periods within which spurts may be expected in the growth of specific variables or of indicators of a process. That is, there may be increases in y-axis scores for a variable that are greater than prior y-axis levels of increase; such variation would, then, be associated with a marked change—or, in the terms of Werner (1957), with a quantitative discontinuity—in the slope, and thus in the y intercept, of a developmental function.

This variation is illustrated by the findings of Simmons and Blyth (1987) about grade-associated changes in self-esteem that I noted above. Such data enable the formulation of empirical generalizations regarding when quantitative discontinuities in self-esteem are likely to occur. This sort of information could be used to divide the x-axis.

No statistical method used to depict changes in a developmental process will yield better results than would be derived from a research design that appropriately placed data observation (collection) points at locations along the x-axis based on theoretically-guided decisions or, at the least, decisions derived from robust generalizations from prior research. Placing data collection points either too far apart or too closely together holds the danger of inaccurately depicting a developmental process.

For instance, if the theoretically-consistent trend in the data is linear, but the data collection intervals are too fine, a discontinuous trend might be suggested by the results—regardless of the analytic techniques used. Similarly, if the data are not collected frequently enough, then the true patterns in the data may be missed, and no statistical technique will be able to recover the true pattern. As a result, theory should be consulted first during the design phase of a study. In addition, unless a researcher empirically compares the trajectory of development derived for a study that used different x-axis divisions (e.g., comparing self-esteem data from, say, a burst of daily-collected data points surrounding the transition from elementary school versus annually-collected data in, say, Grades 4, 5, 6, 7, and 8), statements about which division of the x-axis in fact provides a more veridical representation of a developmental trajectory for a specific process are just speculations.

Selection of Appropriate Ontogenetic Points to Index a Change Process

What does theory tell developmental scientists about when in ontogeny a particular process may unfold? Unfortunately, there are few statements we can derive from extant theory and few attempts to derive empirical generalizations from research to specify the precise timing of the tipping points in ontogeny for use in dividing the x-axis in developmental research (Bornstein, 2017; Wohlwill, 1970; Lerner, 2002). Moreover, the non-ergodicity of intraindividual change (e.g., Molenaar & Nesselroade, 2015; Rose, 2016) complicates the task of making decisions about such divisions that will be applicable to all individuals. Nevertheless, independent of how times of observations are spaced along the x-axis, developmental scientists need to address the issue of selecting the correct points in the life span to optimize the identification of changes in a specific process. The key idea here is that atheoretical selection of ontogenetic observational points may lead to unrepresentative—if not distorted—conclusions about the presence or form of change (Lerner, Bornstein, & Smith, 2003; Lerner et al., 2009).

A prime example of such a potential error is the literature on identity development.

Based on his clinical observations, Erikson (1950, 1968) theorized that the majority of identity formation occurs during the adolescent years. Moreover, Erikson argued that the emergence of the identity crisis is coupled with the advent of puberty and, as such, ontogenetic points within the early portion of the adolescent period should be selected for observation. However, the vast majority of identity research studies have been conducted using college student samples (Schwartz, 2005).

As illustrated by the discussion of the research of Simmons and Blyth (1987), in the absence of theory, selection of appropriate ontogenetic observation points could be rationalized on the basis of empirical generalizations. For example, if a longitudinal study of the initiation of alcohol use were to be conducted in the United States, it would be necessary to plan to have an initial observation point prior to entrance into about eighth grade. By this grade level, about 23% of youth have begun to use alcohol (Johnston, O'Malley, Miech, Bachman, & Schulenberg, 2016). Similarly, observation points after the end of high school would be of relatively little value for the collection of information about the initiation of alcohol use, given that 61% of youth have initiated use by this time (Johnston et al., 2016).

In short, theory or, at the least, inferences from past research, should be used to select the particular times in life when observations of development are made and, as well, the spacing—that is, the intervals between, or the density—of times of observation. Innovations in longitudinal designs can be useful in selecting these intervals. For instance, using the 4-H Study data base (see Chapter 9) and, as well, the broader literature of PYD (Lerner, Lerner, Bowers, & Geldhof, 2015) as an empirical sample case, Geldhof et al. (2014a) discussed the use of the above-noted *burst designs* within longitudinal studies. These designs involve the use of a series (often numbering several dozen) of closely spaced x-axis divisions across theoretically-specified portions of ontogeny. An example may be a purported transition time in the course of a specific process, for instance, involving early pubertal change and identity development (e.g., Erikson, 1950, 1959, 1968). Geldhof and colleagues noted that the lack of theory sufficient to make specific divisions of the x-axis is coupled with the exigencies of longitudinal research (e.g., the costs of repeated testing, the availability of sufficient staff, and potential fatigue or attrition among participants because of frequencies of repeated testing). As noted earlier in this chapter, Geldhof et al. (2014a) concluded that future enactments of large-scale longitudinal research will use x-axis divisions of many months or even years (e.g., Lerner et al., 2015; Nesselroade & Baltes, 1974; Schaie, Labouvie, & Buech, 1973; Wang et al., 2015).

Nevertheless, hypotheses about ontogenetic tipping points or interest in identifying idiographic trajectories among subsamples of participants (selected either for theoretical or for exploratory, hypothesis-generating reasons) can be addressed by intensively measuring a subsample of participants prior to an observation point used for the overall sample. That is, a burst of, say, daily observations could be made for a subsample of participants (e.g., measuring the subsample on each of 30 or 40 days both prior to and after an observation point, say Age 10 years, used for the overall sample). Using the resulting data base, researchers could then employ methods described by Molenaar and Nesselroade (2014, 2015), that is, dynamic factor analysis and the Idiographic Filter, to both identify the individual trajectories of intraindividual change involved in the subsample and, as well, assess the relations between these idiographic patterns and the change trajectories that are identified for the overall sample across the more widely separated x-axis points. As I just noted, however, at this writing there is little evidence about the comparability of trajectories of intraindividual change across data sets differentiated by these two formats of x-axis divisions.

Moreover, even when a common group observation point is used, for example, Grade 5, it may not be that, in large-scale longitudinal studies, all participants in the grade will be tested on the same date. Thus, "age at testing" will be different across participants. As such, it is advisable to avoid using "wave" (of testing) as a marker of age of participants. Age should be used as a marker of age of participants. To illustrate, if, say, Age 10 years was the target age of data collection for a specific wave of testing in a longitudinal study, it is likely be the case that, on the days allocated to data collection, researchers would not be able to assess all participants on precisely their tenth birthday. Some participants might be a bit older and some a bit younger than 10 years. Accordingly, age at testing would vary across participants. In such cases, the temporal variation in age at testing for a wave within a longitudinal study could be used to assess any possible moderating effects of age at testing on developmental trajectories (Little, 2013). In other words, wave of testing would be a poor way to index intraindividual changes that are believed to be indexed by age.

In short, then, depending on theoretical understanding of the process under study and/or empirical knowledge of rates of change in the process, interobservational points may involve any ontogenetic time scale, for example, days, weeks, months, or years. As well, theory or prior research should influence the density of observational points along the temporal axis, for example, precise descriptions of nonlinear trajectories may require greater density of measurement during periods of curvature. Here again, then, burst designs may be particularly useful.

These bases of the selection of observation points and of the spacing of these points along the temporal axis may also suggest that during some ontogenetic periods (e.g., early adolescence in the case of identity development) several different times of observation (separated perhaps by intervals of six months) should be considered because nonlinearity and rapid fluctuation are likely. However, at other times in ontogeny (e.g., middle adulthood) fewer points in time (with longer, say annual, separations between them) or, perhaps, the aggregation of data points, would suffice to chart developmental change, because it is more likely to be linear and/or to change less rapidly. These recommendations would likely apply regardless of the specific longitudinal design used.

The Nonequivalent Temporal Metric: Relations among Levels within the Relational Developmental System

Across the chapters of this book, I have argued that, at this writing, RDS-based theories represent the superordinate theoretical frame for the study of human development (Overton, 2015a). Although the basic processes of development within these theories always involve links between intraindividual changes and changes occurring at the other levels of organization within the ecology of human development, the temporal metric of change may vary across these levels (Lerner, Schwartz, & Phelps, 2009; Lerner, Skinner, & Sorell, 1980). Such interlevel relations may involve covariance among variables from temporally commensurate levels of organization (e.g., pubertal change and cognitive change are both individual-level processes and may change across x-axis divisions that are closely aligned, e.g., months or years). However, such relations may involve covariance among variables from temporally incommensurate levels of organization, for example, the individual (weeks, months, or a year) and the society or culture (e.g., decades).

For example, in Bronfenbrenner's (1979, 2005; Bronfenbrenner & Morris, 2006) instance of an RDS-based theory, each system within the bioecology of human development is both inextricably interconnected and, at the same time, changing in accordance with its own facet of the chronosystem; that is, the temporal metric with which changes in the system can be detected will vary across the nested levels of the ecology of human development (see too Elder, 1998; Elder & Shanahan, 2006; Elder et al., 2015). For instance, to index changes in infant sensorimotor or neuromuscular development (which involves changes in the microsystem), the x-axis (the chronosystem) may need to be divided by weeks. In turn, to index changes in the impact of U.S. government policies aimed at improving infants' health and neuromuscular development (which involves changes in the macrosystem), x-axis divisions should arguably be no finer than one year and, more likely, two, four, six or more years apart, depending on the nature of governmental changes being indexed.

As another example, Elder (1998; Elder & Shanahan, 2006; Elder et al., 2015) illustrated the impact of such chronosystem variation in describing the differences among individual, family, and generational change. The individual chronosystem is typically gauged in years, because the person measures his or her time through counting years since birth. However, family time may be indexed by a chronosystem involving life events (marriage, birth of a first child, or the occurrence of an *empty nest*, that is, all children leaving the home of origin). In turn, generational time involves a chronosystem where, as family members are born, marry, or die, cohorts may change from being, for instance, the children, to the parents, to the grandparents.

To illustrate the methodological challenges of indexing the different meanings of time in order to understand the coactions among the levels of organization within the dynamic, relational developmental system, it is useful to return to the example of changes in infant neuromuscular development. As I noted, such changes might best be measured in weeks. However, changes in the factors and processes that coact with infant neuromuscular development may have very different temporal metrics. For example, changes in parenting styles or practices in infancy might best be measured in months whereas, as noted, changes in government policies toward young children might best be measured in divisions of several years. In cases such as these, when research involves measures of all of these constructs using the same temporal metric to chart changes, a clear limitation exists in the potential to identify relations among infant neuromuscular development and the potential influences on this development that are occurring at other levels of the bioecological system described by Bronfenbrenner. Measuring all variables using weeks as the temporal metric would result in parenting practices and government policies remaining constant (or fluctuating randomly) as infant neuromuscular development changes along its appropriate metric. Conversely, measuring all variables using months or years as the temporal metric would result in the trajectory of infant neuromuscular development being misestimated (or "falling between the cracks" of the x-axis divisions) (Lerner et al., 2009).

The non-commensurate nature of time across these levels of organization within the bioecology of human development makes the study of trajectories of intraindividual changes difficult to index empirically. In such circumstances, relationships between intraindividual development and its influences that exist at more macro levels may not be able to be directly charted. As such, developmental scientists may need to utilize cohort comparative designs, such as cross-sectional sequential methods (Baltes et al., 1977; Schaie, 1965).

However, there may be ways in which other temporal designs may be used to understand the role of macrolevel events or changes on intraindividual change. One possibility is to use multiple panels, or multiple birth cohorts in an accelerated longitudinal design, that are selected on the basis of the presence or absence of a major historical event, for example, the advent of the Great Depression (Elder, 1974), or the advent of the Vietnam War (Nesselroade & Baltes, 1974), differentiating the experiences of a specific birth cohort during specific times in life (e.g., childhood, adolescence). Another possibility is to use an intensive measurement design (e.g., P-technique; Molenaar & Nesselroade, 2015; Nesselroade & Baltes, 1979), where sufficient data points are gathered prior to or after some expected macrolevel event (e.g., new social policies affecting support to parents with dependent children, or the level of retirement benefits provided to adults ending their work lives) to assure sufficient measurement occasions to judge whether there has been a change in the developmental trajectory.

In P-technique, the person is held constant, and the covariance among variables is modeled across time. That is, within P-analysis designs, the source of variation in scores for variables is time of measurement. In contrast, the more typically used R-technique design involves time being held constant, with covariance among variables modeled across persons within time (Cattell, 1957). That is, within R-analysis designs, the source of variation in scores for variables is individuals. In other words, in P-technique designs intraindividual change is assessed by considering how variables covary across time within a person, whereas in R-technique designs analysis is focused on how variables covary across individuals at one time of measurement. Because the person-centered, P-technique design may involve as many as 100 or more times of measurement (Corneal & Nesselroade, 1991; Nesselroade & Ford, 1985), it constitutes an instance of an intensive longitudinal design (Collins, 2006). However, R-technique designs, even when reiterated at multiple times with the same sample, are not longitudinal designs; they are variable-centered in nature, and assess how variables covary across people within time.

In short, to study the relations within the relational developmental system that moderate the rate and form (trajectory shape) of developmental processes, developmental scientists must be sensitive to the different meanings of time across levels of organization or analysis within the system. Developmental scientists may need to pursue programs of research that include combinations of intraindividual and interindividual research designs, such as P-technique, and/or that use macrolevel events or changes to identify idiographic developmental trajectories in order to fully elucidate the course of developmental change (Rose, 2016).

Complicating this challenge, however, is that in any design used in such a developmental research program, decisions must be made about the specific ontogenetic observation points that will be used for each variable or each level of analysis. Whether a researcher chooses to divide the x-axis according to a fixed or variable number of weeks, months, or years, questions may be raised about whether the selections are theoretically optimal for elucidating the “true” rate and form of developmental change for each of the developmental processes being studied. When several variables are assessed in a given study—and especially when the variables being assessed are at different levels of analysis—the decisions regarding research design and, as well, data analysis strategy can become quite complex. This point enables me to turn to a discussion of some ideas pertinent to data analysis within developmental research. The point also allows me to discuss some of the contributions of Joaquim Wohlwill.

SOME DATA ANALYSIS ISSUES IN DEVELOPMENTAL RESEARCH

Issues of stasis, of constancy in the rate of change, or of irregularly timed spurts in growth are not documented or even theoretically conceptualized for many developmental processes. Similarly, there is very little information about whether intraindividual changes assessed in “bursts” (that is, in closely temporally proximal points—for instance, consecutive day, ones spanning, say, two or three months) correspond to changes assessed across x-axis points more widely separated (e.g., by years, instead of days). How can these issues be addressed?

One solution is to explore the usefulness of the idea of Joaquim Wohlwill (1970), that time be included in the definition of the dependent variable in developmental research. Wohlwill suggested that researchers place time on the y-axis, that is, make time the dependent variable. He indicated that changing the placement of time (from the x-axis to the y-axis) could help test the assumption that developmental processes unfold in equally timed intervals. Wohlwill noted that this assumption was rarely, if ever, tested. However, if the levels, phases, or stages of a process did unfold in a manner commensurate with a linear and quantitatively continuous progression of time, then one could invert the x- and y-axes and place the levels of the process along the x-axis and see whether, in fact, the assumption of linear and quantitatively continuous time progression was warranted. That is, the traditional plotting method requires specifying the mean scores for each demarcation of time. However, in turn, Wohlwill (1970) suggests that researchers plot the mean age/time for each score on the variable in question.

To illustrate the application of Wohlwill's (1970) ideas for the analysis of longitudinal data, consider the imaginary data for a study of a developmental process that are presented in Table 13.2. Figure 13.1a presents a graph showing the typical way in which these data are depicted in developmental analyses. Differences in scores for the developmental process are presented as a function of equal-interval age differences. As shown in Figure 13.1a, the data suggest that the process linearly increases across Ages 5–15 years. However, if the developmental process actually has the time-ordered characteristics depicted in Figure 13.1a, then the process should appear the same when scores for age are presented as a function of different scores for the developmental process.

Table 13.2 Scores for an indicator of a developmental process

Participant	*Age*			Mean
	5	*10*	*15*	
1	8	9	10	9.0
2	0	4	5	3.0
3	1	4	7	4.0
4	1	2	6	3.0
5	0	1	2	1.0
Mean	2.0	4.0	6.0	

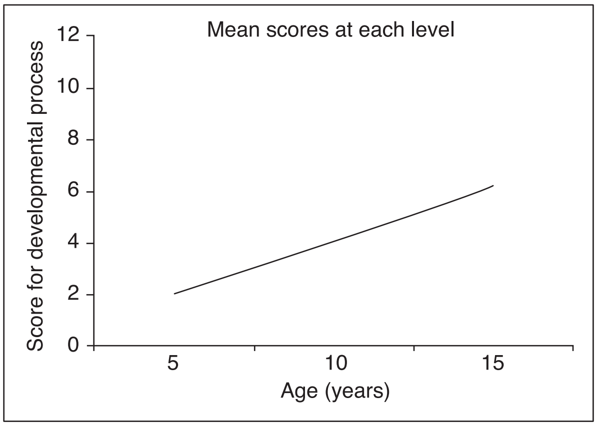


Figure 13.1a Scores for a developmental process as a function of equal interval age differences.

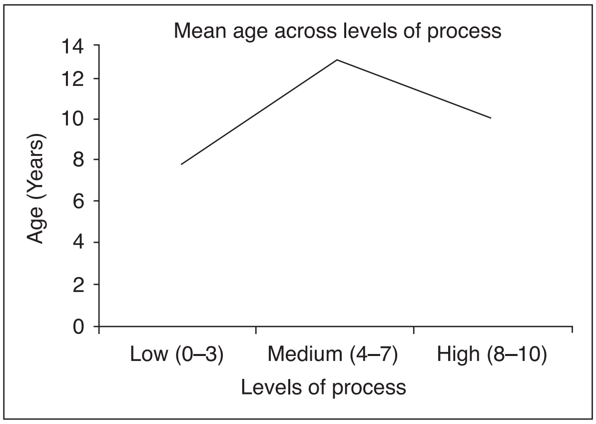


Figure 13.1b Age as a function of different levels of a developmental process.

The data represented in the figure pertain to an aggregate growth curve. In developmental science, views about best practice in growth curve analysis involve discussion of advantages of methods of individual growth curve fitting (Newell & Molenaar, 1998; Ram & Grimm, 2015). However, the purpose of this example is not related to a discussion of these methods or of their advantages in comparison to aggregate growth curve analysis. The purpose is more fundamental, that is, to discuss the issue that theory affects the treatment of data and thus the findings (the curves) one observes. That is, whether a developmental scientist is fitting an individual or an aggregate curve, he or she may in some way code, organize, aggregate, etc. in a manner that is shaped primarily by theory and not by the empirical array with which he or she is working (Lerner et al., 2009). Simply, then, the purpose of this illustration is to show that in any of these steps theory affects data treatment.

As I have noted, if divisions of the (inverted) x-axis cannot be established on the basis of theory or past research, then, with recognition of cautions about generalizability beyond a given sample, x-axis divisions could be made empirically (e.g., on the basis of quartile scores). In any case, scores for the developmental process should be arranged in ascending order, and the participant ages that correspond to each score (or set of scores) should be plotted on the y-axis. If the same curve emerges for the inverted and non-inverted forms of the plot, then equal intervals of chronological age may well serve as an adequate index of time. However, it is also possible that when such inversion of the x- and y-axes is done, a very different form for developmental change will be evident. This difference is illustrated in Figure 13.1b. The data presented in Table 13.2 are again used. However, the developmental process appears curvilinear, as an inverted U-shaped function.

Which depiction of the process is correct? To answer this question, theory, once again, should take precedence. In cases where theory is weak or even absent, developmental scientists can pursue another, iterative path based again on Wohlwill's (1970) work. Data sets can be explored for divisions of both time and process until the fit between the inverted and non-inverted (traditional) depictions of the developmental function is maximized (Lerner et al., 2009). Specifically, a researcher could create an index of fit between the inverted and non-inverted curves, and the x-axis partitioning scheme for which this fit index is maximized would be selected tentatively as the correct metric and scaling for time. Note that this maximization of fit does not necessarily imply that the original and inverted curves are equivalent—just that they can be reconciled with one another in a logical and meaningful way. Of course, scaling decisions reached through this method would need to be cross-validated with another sample.

Nevertheless, through such iteration and cross-validation, the nuanced understanding of developmental change may be advanced and, as well, the extent to which time in general, or specific portions of ontogenetic time in particular, represent important parameters of the form of change may be identified. Moreover, as is implicit in the above imaginary example, the x- and y-axis inversion procedures I illustrated can be used to test whether theoretical ideas about the time-ordered character of a process are correct.

In short, then, not only may the x-axis be divided in diverse ways when time (age, stage, etc.) is placed on this axis but, as well, there can be benefits for exploring the use of time as a dependent (y-axis) variable (Lerner et al., 2009; Wohlwill, 1970). Consistent with the links between method and theory stressed by Collins (2006) and Little et al. (2009), such inversion of the x- and y-axes can have important implications for testing or extending developmental theories. Other issues pertinent to time also illustrate the significance of the theory–method link.

Problems with Aggregating Observational Points

One way in which developmental scientists have dealt with differing temporal metrics across distinct substantive phenomena (e.g., infant development and parent behaviors), or sought to maintain equal intervals of change across the x-axis, has been to aggregate data across two or more observational points. Aggregation is discussed here as a method of handling and understanding data, rather than as a statistical strategy for summing over different trajectories that may be present within a given sample (e.g., see Molenaar, 2014; Molenaar & Nesselroade, 2014, 2015; Muthén & Muthén, 2000; Nagin, 2005; Ram & Grimm, 2009).

Although aggregation may seem to be an appropriate way of reliably handling variables with differing temporal metrics (see Rushton, Brainerd, & Pressley, 1983), it has the effect of summing over potential idiographic developmental variations (Molenaar & Nesselroade, 2014, 2015; Rose, 2016). In Chapter 8, I provided some examples of how such a problem might occur in tests of the continuity or discontinuity of developmental processes. Aggregation, then, may have the effect of recasting into different forms a pattern characterized at the individual level by fluctuations (e.g., taking the form of a sine curve; Ram, Chow, Bowles, Wang, Grimm, Fujita, & Nesselroade, 2005). For instance, a pattern that is sinusoidal at the individual level may appear more linear when daily variation at the intraindividual level is recast as weekly or monthly variation at this level (Ram et al., 2005). As a result, a complex nonlinear trajectory may be mistaken for a simple linear trajectory, and the conclusions drawn about the developmental process in question are likely to be incorrect. To avoid such distortions of data, whether and how one should aggregate should be determined by explicit specification of the theory of the process under study.

As noted in Chapter 8 as well, this issue highlights the need to be aware of how depictions of data elucidating developmental processes relate primarily to theoretical, rather than to empirical, issues. An example from the middle decades of the twentieth century provides a vivid example of this point. During this period, psychological science was dominated by research framed by theories of learning and, arguably the two most famous textbooks across the history of the psychological study of learning were Hilgard and Marquis's *Conditioning and Learning* (1940), which summarized *all* research on the learning of organisms through 1940, and the 1961 revision of this book by Gregory Kimble, which he entitled *Hilgard and Marquis' Conditioning and Learning*. Kimble's revision was a thorough updating of, again, *all* that was known to that date about the learning of ALL organisms (at least as it was known within psychological science).

In a chapter on "Practice and the strength of conditioning" (conditioning was believed then to be *the* key means through which learning occurred), Kimble (1961) included a section on "The problem of averaging" (pp. 114–117). The "problem" that Kimble identified was that the one, universal learning curve that was presupposed to characterize the learning of *all* organisms was not being found when learning curves were being constructed by plotting the average score across organisms on each of the successive trials to criterion in learning experiments. The behavior of organisms was averaged for each trial, but something was not working. Here is what Kimble (1961) wrote:

> Most learning curves are for groups of subjects, rather than individuals. For many purposes this focus creates a problem, especially in experiments where subjects are tested until they reach some criterion, such as 100 percent conditioning in a block of trials. Different subjects will take different numbers of trials to reach the criterion, and it becomes difficult to find a base line against which to plot the response measures to represent the course of acquisition.
>
> (p. 114)

Simply, the smooth, negative accelerated learning (or conditioning) curve that was presumed to hold for the behavior of all organisms was not being produced because—admittedly—there was individual variation. After all, in Skinner's classic 1938 book, *The Behavior of Organisms*, he discussed the universal laws of learning (to him, operant and respondent conditioning) that were "known" to apply to all organisms (although, in his 1938 book, the only organisms that Skinner had data from were rats and pigeons).

Of course, Kimble, and other experts in learning of this era, could have gone down a theoretical path that led them to the concepts of non-ergodicity and idiography that Rose (2016) championed. After all, these researchers say, empirically, that the learning curves of individuals differ. However, they did not. Their theoretical views so strongly shaped the way they interpreted what Kimble stated was empirically the case, they assumed that their theory and not the observed behavior of participants was true. As such, to save their theory, researchers needed to find a way to "correct" the non-ergodic behavior of participants. Reflecting the character from Greek mythology, Procrustes (who either stretched people or cut off their legs to force them to fit the size of a bed), researchers needed to find a way to get the "unruly" behavior (that is, the personalized, or idiographic, learning pathways/trajectories) to fit into the universal learning curves that their theory specified. Kimble (1961) reported a means to create just such a fit. The means he suggested to "salvage" the "one learning curve fits all" presupposition of the nomothetic learning theory he and others followed was to use a method recommended first by Vincent (1912). Kimble suggested the use of an aggregation method termed *Vincentized curves*. Here is what Kimble (1961) wrote:

> One solution to this problem [of idiographic trajectories of learning] is the use of the *Vincent curve* (Vincent, 1912; Kjerstad, 1919). The total number of trials for each subject to reach criterion is divided into fractional parts such as tenths, and measures are plotted for these portions. (For a subject requiring 35 trials the units would be 3.5 trials long; for a subject requiring 40 trials, they would be 4.0 trials long, and so on.) This method makes it possible to combine data for subjects whose performances are widely different.
>
> (pp. 114–115)

Kimble then included a figure (reproduced here as Figure 13.2) showing that when Vincent curves were used, the "noise" produced by the individual learning pathways to criterion for each organism could be eliminated, and the universal curve was thereby produced!

This historical account indicates that, for more than 100 years, the scientists studying the psychology of learning knew that individuals did not follow the same pathway to reaching a criterion, that individuals differed in their learning "curves." However, the theoretical commitment to ergodicity, to nomothetic laws of development and learning, was so great that they chose to believe that the use of averages was the way to go beyond unruly individuals and confirm their belief in the universality of the process of learning. They chose to believe their supposition rather than what they appear to have regarded as their "lying eyes."

Unfortunately, and as Rose (2016) eloquently and convincingly demonstrated, this commitment to averages continues. For instance, the use of Vincentized curves to get around individuality of

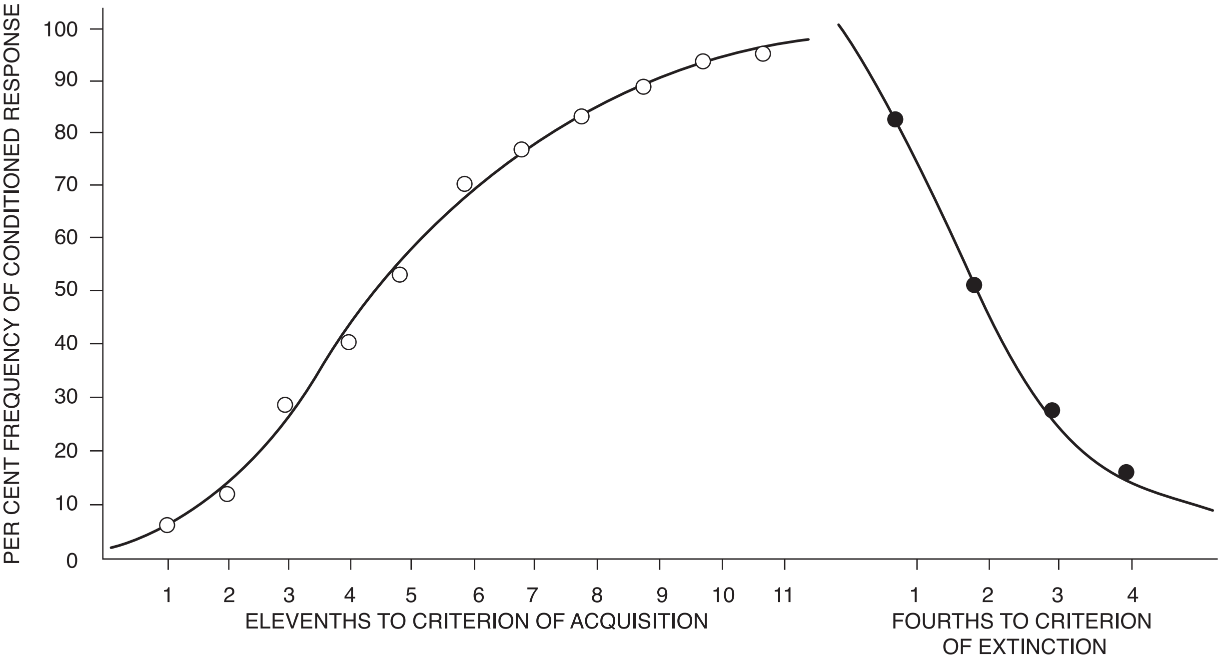


Figure 13.2 Vincent curves of acquisition and extinction of an instrumental avoidance response in dogs (from Kimble, 1961).

learning and, thus, its idiographic character, continued through the twentieth century and into the twenty-first. For instance, reflecting the commitment to the ergodic theorem-based ideas of homogeneity and stationarity, Kendler (1995) argued that:

> Stationarity can be measured in a variety of ways, but the least biased method depends on constructing Vincentized learning curves (Suppes & Ginsberg, 1963). Vincentized curves are based on the averaged proportion of correct responses over percentiles of trials. Therefore this kind of learning curve has the virtue of equating the weight contributed by the fast and slow learners. (p. 66)

Clearly, Kendler (1995) is committed to a theory that specified that learning, or in the case of Kendler (1995) cognitive development, is a homogeneous and stationary phenomenon that can be represented by ergodic theorem-based ideas involving the computation of averages across diverse individuals ("learners," in Kendler's terms). This commitment appears to have resulted in Kendler (1995) ignoring the individual pathways of learning and, remarkably to me, her doing so through the use of a method that she characterized as the *least biased* method of data analysis that is available to researchers! Moreover, the reliance on Vincentized curves to resolve what were regarded as continuing problems associated with finding evidence for a universally applicable, that is, nomothetic, learning curve, continued into at least the first decade of the twenty-first century (Addis & Kahana, 2004).

However, if a researcher adopts RDS-based theories of human development, aggregation should not be pursued merely because it affords ease and clarity in the analysis and presentation of data or because it may lead to more reliable estimation of data points. Instead, from the perspective of these individual↔context models of human development, the data analytic steps recommended by Kimble (1961) and Kendler (1995) would have been obviated, in that the approach I have described as "analyze and then aggregate" would have been followed. Accordingly, the example of Kimble's (1961) use of Vincentized curves to recast a data set replete with evidence of idiographic change into one showing nomothetic functioning underscores the point I made in Chapter 8. That is, the same data set can be analyzed in different ways and, as such, theory, more so than the empirical data collected in a study, *may* lead a researcher to adopt a specific data analysis method. Thus, vigilance in regard to theoretical biases regarding aggregation, *or any other approach to data analysis*, needs to be integrated with attention to the potential ways in which a data analysis method may mask systematicity in the data set that is associated with non-preferred theoretical ideas (Lerner et al., 2009).

Developmental scientists should remain aware that their theoretical position might lead them to interpret a specific data set in one way (e.g., as consistent with a continuity position), whereas other developmental scientists, with different theoretical positions, might interpret that same data set in another way (e.g., as consistent with a discontinuity position). Indeed, as emphasized by both Collins (2006) and Little et al. (2009), developmental scientists would do well to attend to the role of theory in their choice of longitudinal design, in their selection, spacing, and treatment of the multiple times of observation involved in the design they elect to use, and in their selection of the procedures used to analyze data. Simply, statistical analyses cannot replace theory in being the primary tool used in making these decisions.

The Importance of Qualitative Methods and of Mixed-Methods Research

As already explained, any method—any research design, measurement approach, or data analytic technique—may be useful within developmental science, depending on the theoretically-predicated question asked by a developmental scientist. Indeed, depending on a given theory-predicated question, there may be multiple methods that might be appropriate to use to address the question. These methods may be either quantitative or qualitative. Moreover, to reduce the challenges associated with disentangling method variance and substantive variance, triangulating across multiple methods is always advisable in good scientific practice (Campbell

& Fiske, 1959). At this writing, such triangulation increasingly involves mixed-method research, that is, research employing both quantitative and qualitative methods in a mutually informative manner (Tolan & Deutsch, 2015). This use of mixed methods is consistent with Overton's (2014) point, that developmental scientists interested in testing RDS-based ideas about the plasticity of individual↔context relations need to add new features to their methodological "toolboxes." Moreover, as I will emphasize below, qualitative research, independent of its essential role in mixed-methods research, makes a unique and essential contribution to developmental science.

Quantitative Innovations

As represented by the chapters on developmental methodology in several major handbooks in developmental science (e.g., Molenaar et al., 2014; Overton & Molenaar, 2015), the invention or refinement of quantitative data analytic methods is burgeoning. In this section, I point to a few instances of this work that are pertinent to RDS-based ideas.

Non-Ergodic Methods

As I have already discussed, the "analyze then aggregate" methods suggested by Molenaar and Nesselroade (2012, 2014, 2015; Nesselroade & Molenaar, 2010) to quantitatively chart non-ergodic, idiographic trajectories (that is, dynamic factor analysis and the Idiographic Filter; see too Ram & Grimm, 2015) are one instance of such additions to an RDS-based methodological toolbox. These data analysis methods provide one means to ascertain, first, what, if anything, is common across different individuals' intraindividual change trajectories, and then to attempt to build generalizations on that information. This approach stands in marked contrast to initially aggregating the individual-level information and extracting generality from it in the form of average tendencies—the approach of traditional differential psychology—and replacing it with an approach that embraces development and complexity (see Rose, 2016).

Systems Science Methods

The work of Molenaar and Nesselroade (e.g., 2014, 2015) is an example of the application of systems science methods to developmental science framed by RDS-based theories (see also Molenaar et al., 2014). For instance, dynamic factor analysis is an example of a state space model, in that it integrates a model of the dynamic evolution of the state process and another model linking the state process at each time point to the observed process at that time.

Systems science "methods are designed to address complexity, that is, change . . . nonlinear relationships, bidirectional relationships (feedback loops), time-delayed effects, and emergent properties of the system—phenomena that are observed at the system level but cannot be linked to a specific individual component of the system" (Mabry & Kaplan, 2013, p. 9S).

Examples of systems science methods are computational/mathematical modeling, agent-based modeling, system dynamics modeling, and network analysis (Urban, Osgood, & Mabry, 2011).

The use of systems science methods in developmental science is a sample case of the opening of the field to innovations in methodology, perhaps especially those associated with other disciplines. Econometric methods are another example of such innovations in quantitative methods.

Econometric Methods

I emphasized that the *sine qua non* of developmental analyses is the study of intraindividual change. As such, longitudinal designs continue to be the key approach to the study of such change (Molenaar & Nesselroade, 2015; von Eye, Bergman, & Hsieh, 2015). However, the problem of selection—of what economists term "endogeneity" (e.g., Heckman, Ichimura, & Todd, 1997, 1998)—besets longitudinal studies, given that, even if representative samples are present at the beginning of a longitudinal study, selective attrition will increasingly bias the sample. People who stay in a study, perhaps especially a long-term one, have been found to have "something about them" (something endogenous to them) that differs from participants who drop out of a study (e.g., see Schaie & Strother, 1968; Siegler & Botwinick, 1979). Are changes seen in the remaining participants due, therefore, to something about the nature of developmental process or to what may have been a pre-existing endogenous factor (e.g., the tenacity needed to stay at a task, obedience to authority, or trust in institutions)? The issue of endogeneity is particularly problematic when longitudinal studies are used to assess whether particular experiences of one group (e.g., participation in a community-based, youth development program) are associated with developmental changes that differ from those seen within members of a group not participating in the experience (program). Here, the researcher may not be able to infer that the program was causally associated with any differences between participating groups because it may be that there were pre-existing, endogenous factors that led some individuals to participate in (self-select into) the experience.

As a consequence, because of the problem of endogeneity, randomized control trials (RCTs) have been regarded as the "gold standard" design to test for causality (McCall & Green, 2004). Many potential funders of developmental science research have eschewed longitudinal studies because of the inability to demonstrate causality due to selection effects. However, the landscape of research aimed at causal analysis has changed. Econometric methods are being used in developmental science research to address endogeneity in longitudinal research. Among the important tools provided by econometricians are propensity score analyses, instrumental variable (IV) analyses, and regression discontinuity designs.

James Heckman

A propensity score is the probability of a unit (e.g., person, classroom, school) being assigned to a particular treatment given a set of observed covariates. Propensity scores are used to reduce selection bias by equating groups based on these covariates (e.g., Heckman et al., 1997, 1998; Rosenbaum & Rubin, 1983). An instrument is a variable that does not itself belong in the explanatory equation but is correlated with the endogenous explanatory variables. In attempting to estimate the causal effect of some variable x on another y, an instrument is a third variable z which affects y only through its effect on x (Angrist, Imbens, & Rubin, 1996; Martens, Pestman, de Boer, Belitser, & Klungel, 2006; Staiger & Stock, 1997). In regression discontinuity designs, a pretest cutoff score is used to assign participants to either the program or comparison group. The assumption is that in the absence of the program the pre–post relationship would be equivalent for the two groups (Trochim, 2005).

The Decline of the RCT "Gold Standard"

The presence of these econometric tools indicates that randomized control trials are not the only means through which to assess causality in studies of the effects of programs on participants. When econometric methods are part of the methodological tools of developmental scientists conducting longitudinal research, they provide researchers with a rich set of resources to use in the study of ontogenetic change. In addition, RCTs are not the "gold standard" for identifying causality. Indeed, as implemented from the latter decades of the twentieth century through the first decades of the twenty-first century, many RCTs were "fools' gold."

In the developmental science of the 1950s through the 1970s (e.g., Reese & Lipsitt, 1970), experimentation in the study of human development involved a focus on internally valid designs. In such designs, researchers seek to eliminate threats to their being able to attribute the variance in the dependent variable to the manipulated variance in the independent variable. Such threats are ruled out through controls. However, many of the experiments actually conducted during the decades wherein experimental child psychology was a predominant approach taken by developmental researchers (i.e., from the 1940s into the early 1970s; see White, 1970) did not have adequate controls to rule out threats to internal validity. That is, the three control groups in the Solomon and Lessac (1968) formulation of experimental designs were rarely used in studies of human development.

Solomon and Lessac (1968) explained that the typical experiment included only one control group (pre-test, no manipulation, post-test), a group included to account for the predicted differential variance associated with experiencing the manipulation as compared to only the pre- and the post-tests. However, this design did not control for the variance that may be associated with the reactive effects of the pre-test (and thus a second control group, involving no pre-test but a manipulation and a post-test, was needed), or for the variance that may be associated with maturation/development (and thus a third control group, involving neither a pre-test nor a manipulation, but only a post-test, was needed). Despite the absence of all three control groups, and thus the presence of threats to internal validity, a focus on experimental designs continued to be a method of choice of many researchers (Reese & Lipsitt, 1970).

Much research continues to employ the two-group design, with only the first of the three control groups noted by Solomon and Lessac (1968) used. Oddly, this approach is used in RCT research with samples across the life span and, as I have noted, is mistakenly called the "gold standard" of experimental designs despite the lack of appropriate controls. Nevertheless, even in the 1970s developmental scientists were moving away from a focus on issues of internal validity.

Hultsch and Hickey (1978) pointed out that issues of external validity were important if one took theoretical positions that were attentive to the contextual conditions associated with time and place (Elder, 1998; Elder et al., 2015). By external validity, Hultsch and Hickey meant features of experimental design that would allow generalization to other samples, to studies that employed similar constructs but different measures, or findings that might be generalized to different historical periods and places.

Although such research is important to conduct if one is attentive to many of the ideas derived from RDS-based metatheory, Freund and Isaacowitz (2014) noted that a third type of validity must be attended to in order to fully embrace the implications of the process-relational paradigm and the relational developmental systems metatheory derived from it. Inspired by the ideas of Brunswik (1955), and legitimated as a focus of such scholarship by the moment of the opposites of identity discussed by Overton (2015a), Freund and Isaacowitz noted that an important tool of developmental scientists is ecologically valid experiments. Such research involves the use of conditions that veridically reflect the actual, lived experiences of the participants. Such studies assess individuals in contexts that are fully representative of the settings within which the sample live.

Freund and Isaacowitz (2014) contend that ecologically valid experiments should be used as a method when a researcher wants to elucidate contextual sources of variance in the individual–context relation that reflect the lived lives of people in particular places, developing within particular historical periods. Indeed, when such research is conducted, especially when it is conducted with sensitivity to the other types of validity, it can be an important asset in the methodological armamentarium of developmental scientists conducting research predicated on RDS-based theoretical models.

Qualitative Innovations

Across the last half-century, developmental science has passed a tipping point—it has undergone a transformational (qualitative) change (Overton, 2015a), in that there emerged a recognition of the contributions to theory and methodology that can be made by qualitative research; such methods may be particularly useful for elucidating the mutually influential relations between individuals and contexts that constitute the fundamental analytic target of research in RDS-based models (e.g., Burton, Garrett-Peters, & Eaton, 2009; Damon & Colby, 2016; Lerner & Tolan, 2016; Tolan & Deutsch, 2015; Yoshikawa et al., 2008). As explained by Lerner and Tolan (2016), prior to the occurrence of this tipping point, scholarship in developmental science was characterized by interest in testing Cartesian split conceptions of what were termed developmental "mechanisms"; there was a search for universals and an assumption of singular or fundamental causes that could be isolated, with differentiation into primarily nature or nurture variables (Overton, 2015a; Tolan, Chertok, Keys, & Jason, 1990). The chapters in the 1970, third edition of the *Handbook of Child Psychology* (Mussen, 1970) illustrated the split approach to the description, the explanation, and (in the 1970s, occasionally) the optimization of human development (Damon, 2015; Lerner, 2015a).

Methods used to elucidate these purported mechanisms were almost exclusively quantitative ones, with the numbers derived from this research coming from either the results of experiments or from findings derived from interrelational (e.g., correlational) studies using questionnaires, surveys, psychometrically refined tests, or quantitative codes from structured interviews. Cronbach (1957, 1975) characterized this split approach to methodology as the two disciplines (or worlds) of psychology: experimental versus correlational. Even when recognized as inherently limited in capturing the richness of variation of complex interrelations, the prevailing view was that quantitative methods were superior in reliability, validity, and utility to other approaches (e.g., case studies, narrative analyses) (Lerner & Tolan, 2016).

There was no "third world" of qualitative research in developmental science and barely any reference to anthropological studies or to sociological research. There was a burgeoning influence of Piaget, beginning in the 1960s (e.g., Flavell, 1963; Piaget, 1970), and, as well, a growing reliance on case methods in psychoanalytic formulations (e.g., Erikson, 1968; Freud, 1965). However, when incorporated into the developmental science of the era, these theoretical orientations were reduced (or, perhaps better, assimilated) to secondary value because of the privileging in the United States of quantification in developmental scholarship.

In particular, objectivity was valued and was seen as more a part of quantitative methods (Lerner & Tolan, 2016). When and if any qualitative research was published in the journals that were regarded as top-tier ones during this period (e.g., *Child Development*, *Developmental Psychology*, or—perhaps—the *Journal of Experimental Child Psychology*), it was typically anchored with some quantitative basis (e.g., chi-square analyses). Recalling what it was like to be immersed in this era, Lerner and Tolan (2016) reported that it would have been hard to envision change from that status to the rich diversity and sophisticated applications of qualitative methods used in some of the research appearing in the literature of developmental science at the time of the present writing.


Patrick Tolan

For instance, nowhere in the top-tier research journals in the developmental science of this past era were there studies that used participatory action research (Katsiaficas et al., 2016), thematic analysis (Arbeit, Hershberg, Rubin, DeSouza, & Lerner, 2016), interpretive phenomenological analysis (Zaff et al., 2016), narrative analysis (Futch Ehrlich, Deutsch, Fox, Johnson, & Varga, 2016), critical youth study approaches (Fox, 2016), or retrospectively constructed graphical representations (Griffith, 2016) to describe and explain individual development. Nevertheless, such studies generate understanding of development that would be difficult if not impossible to capture richly and scientifically with quantitative methods (Lerner & Tolan, 2016).

At this writing, such qualitative methods are more than accepted. They are seen as contributing in at least two ways to advancing developmental science. First, as noted, they provide important contributions to advancing understanding of an individual's specific coactions with his or her context, contributions often not readily accessible through quantitative methods. Thus, qualitative research may make singular contributions to RDS-based models of individual↔context relations. Second, qualitative methods enhance the validity of developmental science by providing an essential part of mixed-methods approaches to developmental science.

One illustration of the first contribution of qualitative methods derives from research on positive youth development (see too Chapter 9). Lerner and Tolan (2016) note that the nuances of how agentic youth contribute to their worlds at the same time that they are influenced by their settings may not be able to be fully understood alone by quantitative estimates of the strength of pathways using quantitative statistical tools such as structural equation modeling. Moreover, RDS metatheory requires understanding of the meaning-making processes and purposes of the person, and of the phenomenological experience of the ecology within which he or she is developing (Overton, 2015a). These research foci may be explored with nuance through qualitative methods (e.g., Damon, 2008; Damon & Colby, 2015; Spencer, Swanson, & Harpalani, 2015).

For example, Anne Colby and William Damon (e.g., Colby & Damon, 1992; Damon & Colby, 2015) used innovative qualitative methodology to provide developmental science with extraordinarily important, and indeed unique, knowledge of moral development and of the character of people who exemplify lives of moral commitment and contribution. This scholarship illustrates as well the idiographic character of human development, in that it illuminates the importance of a focus on the specificity of individual↔context relations in order to understand how lives of moral commitment develop and are sustained across life. It is accurate to say that the knowledge that developmental science now possesses about the course of moral development and extraordinary moral courage and commitment would have not been obtained had Colby and Damon used the quantitative tools available to them as they began their research program.

Often, as well, the meaning, purpose, and phenomenological understanding of the key people in the context of a person (e.g., in the case of adolescents, parents, peers, teachers, coaches, mentors, and faith leaders; King et al., 2005; Lerner et al., 2015) may also be part of the assessments usefully included in qualitative research aimed at understanding the relational developmental system. For instance, Lerner and Tolan (2016) noted that qualitative research has provided significant portions of the evidence base pointing to the fundamental importance of agency in understanding the mutually beneficial individual↔context and, as well, individual↔individual coactions that comprise the process of positive development. Moreover, qualitative research provided some of the earliest elucidation about how such bidirectional relations could also be mutually beneficial to both person and context, and about how adaptive exchanges regulated the ways in which individuals could thrive

Anne Colby

through their impacts on the settings that were influencing them (Brandtstädter, 1998).

Continuing with the example of the use of PYD research, Larson (2000) and Damon (2004), in particular, shaped this area of developmental science scholarship through conducting qualitative research about the individual↔context relations involved in the process of youth thriving (e.g., Ballard, Malin, Porter, Colby, & Damon, 2015; Colby & Damon, 1992; Damon, 1990; Damon & Colby, 2015; Larson & Angus, 2011; Larson & Brown, 2007; Larson, Rickman, Gibbons, & Walker, 2009; Larson, Walker, & Pearce, 2005). In fact, it is possible to argue that the study of PYD served as a key sample case for enhancing the role of qualitative methodology in elucidating the mutually beneficial individual↔context relations brought to the fore of scientific concern by RDS-based models of the thriving process.

This foundational work was joined by the more quantitatively-oriented colleagues studying PYD (e.g., Benson et al., 1998; Callina, Mueller, Napolitano, Lerner, & Lerner, 2016; Lerner et al., 2015; Roth & Brooks-Gunn, 2003), and by prescient colleagues who represented mixed-methods approaches to PYD (e.g., Spencer, 2006; Spencer et al., 2015; see too Ballard et al., 2015). Together, these efforts provided an intellectual foundation for elucidating the PYD process and having it regarded as a significant scientific advance within the mainstream of developmental science (Lerner & Tolan, 2016).

Indeed, the vibrancy and productivity of developmental science, as it exists at this writing (e.g., Damon, 2015; Lerner, 2015a), is attributable at least in part to the contributions of scholars conducting qualitative research. Lerner and Tolan (2016) note that they came of age professionally when the hegemony of quantitative research was not even open to debate. As such they indicated that it seemed to them remarkable that developmental science has so thoroughly and significantly advanced and been transformed by the contributions of colleagues conducting qualitative research (see Tolan & Deutsch, 2015). This transformation reflects an incalculable benefit for developmental science.

This contribution is enhanced because of the importance of transcending the limits of knowledge linked only to one method (that is, the importance of disentangling "true" variance about a phenomenon from "method" variance, that is, variance associated with the method used to gather information about the phenomenon). Thus, in this second major contribution of qualitative research, that is, its place in mixed-methods research, qualitative methods enable triangulation across multiple methods and provide a means to build an evidence base for establishing the validity of findings (e.g., Campbell & Fiske, 1959; Cronbach & Meehl, 1955).

In sum, the contribution that the use of qualitative methods makes within the methodological toolbox of developmental science is to provide innovative means to advance understanding of what is meaningful and what matters to the individuals who, through their specific, idiographic efforts, positively engage their world, act to enhance themselves, and strive to make the lives of their families, communities, and societies better. Qualitative methods may be regarded as defining the cutting-edge of methodological innovations needed to understand intraindividual change across the life span (Tolan & Deutsch, 2015). Indeed, doctoral training programs would be seen as inadequate if they excluded from the methodological training of students education in some or all of the instances of qualitative methods noted above, or if these training programs failed to instruct students on how to productively integrate qualitative and quantitative techniques in mixed-methods research (Lerner & Tolan, 2016).

Conclusions: From Research Methods to Application

The study of intraindividual change across the human life span is an arduous enterprise. Good developmental theory must be coupled with change-sensitive measures, longitudinal designs, and change-sensitive data analytic methods to conduct good developmental science. For more than four decades, developmental scientists involved in instances of RDS-based models have discussed the links between theory and the methodological choices involved in longitudinal research (Baltes et al., 1977; Collins, 2006; Little et al., 2009; Nesselroade & Baltes, 1979; Molenaar et al., 2014; Molenaar & Nesselroade, 2012, 2014, 2015; von Eye, 1990a, 1990b; von Eye et al., 2015). However, it is accurate to state that there is not a complete set of recommendations to address all of the problems facing developmental scientists interested in advancing understanding of the links among RDS-based theories and developmental methods. Nevertheless, the essential point to be derived from my discussion is that theory should guide any developmental science research endeavor seeking to describe, explain, or optimize the course of individual↔context relations across the life span.

A key implication of this fundamental point is one that certainly reflects the RDS-based mantra "to avoid all splits" (Overton, 2015a), that is, in RDS-based scholarship there is a collapse of the distinction between basic and applied developmental science (Birkel, Lerner, & Smyer, 1989; Lerner, 1995). That is, individuals develop within a multilevel context—within the bioecology of human development, to use the terminology of Bronfenbrenner (2005; Bronfenbrenner & Morris, 2006). Theory-predicated and methodologically-rigorous developmental science research that enhances understanding of the process through which individual↔context relations eventuate in healthy and positive development is both explanatory research *and* research that illuminates how to optimize the course of human development. In other words, there is an explanatory research↔optimization research relation within approaches to developmental science framed by RDS-based ideas.

This recognition affords a transition from a focus on research methods to a focus on the application of developmental science. That is, it is important to appreciate that the methodological issues raised about the study of developmental processes are not simply theoretical abstractions. Theoretically-sound and methodologically-rigorous research on human development should be used to shape public policies, prevention and treatment programs, classroom curricula, and community-based programs for children, adolescents, and adults across the life span. More nuanced theoretically-predicated study of developmental change processes along the lines I have discussed will enable developmental scientists to suggest how such applications of developmental science might be better designed, implemented, and assessed or evaluated, in regard to their effectiveness in enhancing human development. Moreover, RDS-based concepts can also directly shape the ways in which research evaluating such applications of development science can be designed. This point leads to a discussion of the implications of RDS-based research for applications.

APPLIED DEVELOPMENTAL SCIENCE

. . . the very meaning of things known is wrapped up in relationships beyond themselves. Thus, unapplied knowledge is knowledge shorn of its meaning.
Alfred North Whitehead (1936, p. 267)

Among the many split conceptions maintained by viewing the study of development through a Cartesian lens is the split between basic and applied research (Overton, 2015a). However, within models of human development derived from the ideas of the process-relational paradigm, this split joins other ones (e.g., nature–nurture or continuity–discontinuity) in being rejected. Indeed, transcending this split may have enormous benefits for both science and application. For instance, in discussing the requirements of building and traversing a useful bridge between developmental and educational science and applications to practice, Stafford-Brizard et al. (in press) noted that:

> Building and activating this bridge between science and practice has the potential to transform educational practice, science and the systems that influence both. This potential involves a fundamental shift in education from the current focus on academic outputs to a demand for outcomes framed by comprehensive human development. The most profound results of such a paradigm shift will lie not in a journal article or a specific education policy, but in the experiences of children—experiences that will contribute to the healthy, rigorous development of engaged, informed and productive citizens who will innovate, contribute to and transform our society.

To illustrate, when one studies the embodied individual within the relational developmental system, then explanations of how changes in the individual↔context relation at Time 1 may eventuate in subsequent changes in this relation at Time 2, Time 3, etc. are tested by altering the Time 1 person↔context relation. When such alterations are conducted in the ecologically valid setting of the individual, these assessments constitute tests of the basic, relational process of human development and, at the same time, applications—interventions—into the course of human development (Lerner, 2002). Indeed, depending on the level of analysis, aggregation, and time scale at which these interventions are implemented, such changes in the ecology of the individual↔context relation may involve a relationship between an individual and another person (e.g., mentoring relationships); an individual's relationships within community-based programs (e.g., a young person's engagement with other youth and adult leaders in out-of-school-time programs, such as 4-H, scouting, athletics, or a community-service organization); or an individual's development in the context of specific social policies (e.g., regarding military service, eligibility for voting, or opportunities for financial support for higher education) (e.g., Bronfenbrenner, 2005).

As I noted in prior chapters, the rationale for applying developmental science to enhance the lives of individuals or groups is predicated on the presence of relative plasticity in human development, a concept that is derived from RDS-based ideas, such as mutually influential individual↔context relations, autopoiesis, and embodiment. The relative plasticity of human development is a fundamental strength in, and the basis of optimism about, human development. Developmental scientists can be hopeful that there are combinations of person and context that can be identified or created (through programs or policies) to enhance the lives of all individuals and groups. In other words, developmental scientists may act to change the course of developmental regulations, of individual↔context relations, in manners aimed at optimizing the opportunities for individual and group trajectories across life to reflect health and thriving.

As I just noted, these actions by developmental scientists may involve engagement with programs or policies. Programs are planned and systematic attempts to either (a) reduce (or ameliorate) the presence of an emotional, behavioral, or social problem, (b) prevent such problems from occurring, or (c) promote positive, healthy behaviors among people. Policies represent standards, or rules, for the conduct of individuals, organizations, and institutions (Lerner, 1995, 2004; Lerner et al., 1999). As such, policies structure people's actions; the presence of policies indicates how individuals, groups, or institutions may be expected to function in regard to particular substantive issues, for instance, regarding education, public health, environmental stewardship, commerce, or participation in government (e.g., voting, military service, or service in public office). Policies reflect what people value or believe (e.g., policies regarding voting rights or policies barring discrimination or decreasing disparities across racial, ethnic, social class, gender, or sexual preference groups) and what people think is in their best interest (e.g., regarding climate change, immigration, or international relations). Simply, policies indicate the topics or actions in which people are invested and about which they care.

As I will elaborate later in this chapter, developmental scientists may apply their skills to help design or implement new programs or policies pertinent to development across the life span; such planned actions, actions that alter the ways things are (e.g., elementary school education, funding of out-of-school-time activities for adolescents, or access to in-home care for aged, infirm individuals) in an attempt to make things better, may be termed *interventions*. Developmental scientists may engage in interventions in order to decrease (ameliorate) problematic conditions in individuals or their settings, to prevent such conditions from occurring, or to promote valued behaviors or conditions among individuals (e.g., academic achievement among school-age children, entrepreneurship or vocational success among young adults, or enhancing parenting skills among new parents) or their settings (e.g., enhancing neighborhood safety or improving access to medical and mental-health services). As well, developmental scientists working from an RDS-based, relational perspective might develop interventions to enhance specific instances of individual↔context relations. For example, programs or policies could be instituted to promote empathic peer relations among school-age youth in order to reduce or prevent bullying (e.g., Hilliard et al., 2018), or to improve communication about standards of honor and respect among military officers and cadets in military training in educational institutions (such as the United States Military Academy at West Point) in order to enhance the development of new officers in their quest to become leaders of character (Callina et al., 2017).

In addition to designing or implementing interventions, developmental scientists can work to devise tools (e.g., measures) to be used in assessments of specific programs or policies, for example, the researchers may develop questionnaires, interviews, surveys, behavioral observation conditions, or electronic media to assess if programs or policies are working as intended (e.g., Geldhof et al., 2014b; Wang et al., 2015). Moreover, developmental scientists may conduct evaluation research, that is, they may design a research project to ascertain—to evaluate—if, or the extent to which, a specific program or policy is effective, or working as hypothesized or intended.

Evaluations are empirical procedures used to ascertain whether programs are effective, and if programs attain their goals (Connell, Kubisch, Schorr, & Weiss, 1995; Fetterman, Kaftarian, & Wandersman, 1996; Jacobs, 1988; Jacobs & Kapuscik, 2000; Millett, 1996; Lerner, Ostrom, & Freel, 1995; Ostrom, Lerner, & Freel, 1995). For example, an evaluation research project might be aimed at answering questions such as "Does a program accomplish its aims?" "Does it reduce youth violence?" "Does it prevent unsafe sexual behaviors and promote sexual respect among college students?" "Does it enhance self-esteem among children?" If a program achieves what it is intended to achieve, it is effective. It is a program that is valid for its intended purpose. Evaluations try to prove that any changes individuals experience over the course of their participation in a program are due to the program itself and not due, for example, to pre-existing, exogenous factors, that is, to endogeneity (see the discussion of econometric methods, earlier in this chapter, regarding designs useful for addressing the question of endogeneity—of pre-existing, or selection, effects).

Evaluations aimed at proving that a program is effective are often termed outcome or *summative* evaluations. Evaluations also try to improve the quality of the program as it is being conducted. Such evaluations are termed *formative* evaluations. Here, the evaluator will try to determine whether the program can be improved—whether mid-course corrections can be made to the program so that better efforts can be made to promote self-esteem or prevent violence.

For instance, if the "theory" of developmental change used by program planners involves the idea that more police need to be present on the streets of the community in order to reduce youth violence, the evaluator may monitor whether such presence is, in fact, increasing; if not, then he or she may work with the community to create conditions that would allow greater community policing. The evaluator would not wait for a final determination of violence reduction to be made prior to taking the step of involving more police in the community. Rather, before any outcomes are seen, efforts would be made to improve the work involved in the delivery of the program. Because they seek to enhance the process through which a program provides its services, formative evaluations may also be termed *process* evaluations.

Evaluations also try to empower the people who are delivering the program and, as well, the individuals who are participating in it. For example, a key goal of evaluators of youth programs—especially of those programs that are located in communities and that were begun, and are continued, through the efforts of members of the community (as compared to trained professionals, e.g., psychologists, social workers, nurses, or physicians)—is to increase capacity among community members to both prove and improve the program (Connell et al., 1995; Fetterman et al., 1996; Jacobs, 1988; Jacobs & Kapuscik, 2000; Millett, 1996; Ostrom et al., 1995). These *empowerment* evaluations are seen as critical to enact if the community is to use evidence of program effectiveness to bring the program to all the individuals who need it and to maintain the program (Fetterman et al., 1996).

At this writing, the applications of developmental science include the sorts of assessment and evaluation research activities I have described. In addition, these applications involve providing direct services to individuals, families, or communities based on evidence derived from theoretically-predicated, rigorous developmental research; that is, engaging in evidence-based practice. Such applications of developmental science are integral parts of the optimization component of the field. In other words, the split between, on the one hand, the descriptive and explanatory goals of developmental science and, on the other hand, the optimization goal, has been overcome.

However, to adapt a phrase from Hermann Ebbinghaus (Boring, 1950), applied developmental science has a long history but a short past. It is useful to review the relatively brief history of the current (at this writing) instantiation of applied developmental science (Fisher et al., 1993; Lerner & Fisher, 1994). This discussion will illustrate the ideas and the organizations that were involved in providing this approach to promoting healthy and positive development across the life span. This discussion will also enable me to indicate the central role of RDS-based concepts in shaping the facets of this approach to optimizing human life.

The Emergence and Definition of Applied Developmental Science

In the late 1980s, scholars from several disciplines (ones associated with the American Psychological Association, the Society for Research in Child Development, the Society for Research on Adolescence, the International Society for Infant Studies, the Gerontological Society of America, the National Black Child Development Institute, and the National Council on Family Relations) came to the realization that issues of child and youth development, family structure and function, economic competitiveness, environmental quality, and health care were interdependent and, thus, required creative and integrative research to understand. As well, such research was needed to provide the evidence required to design, deploy, and evaluate innovative public policies and intervention programs. Moreover, as a consequence of the presence of the interrelated problems confronting global society (e.g., climate change, economic and opportunity disparities between the majority and minority worlds) (e.g., Lerner et al., in press; USAID, 2013), there has been an increasing societal pressure for universities and for the scholars within them to design and deliver knowledge applications addressing the problems of individuals and communities across the life span (Boyer, 1990, 1994; Chibucos & Lerner, 1999; Ettekal et al., 2017; Ralston et al., 1999).

These applications involve the ability to understand and assist the development of individuals who vary with respect to cultural and ethnic backgrounds, economic and social opportunities, physical and cognitive abilities, life style and relationship preferences, and conditions of living (e.g., in regard to their family, neighborhood, community, and physical settings). Moreover, infants at biological or social risk (e.g., due to being born into conditions of poverty), gifted children or those with developmental disabilities, adolescents considering health-compromising behaviors, single- and dual-worker parents, the frail elderly, and ethnic minority individuals experiencing discrimination or disparities in health care, employment, or educational opportunities are just some of the groups that may be enhanced by applications of knowledge based on the work of diversely-trained scholars. Scholars in fields such as psychology, sociology, nursing, human ecology/human development, social work, criminology, political science, medicine, biology, anthropology, and economics may integrate their efforts to enhance the lives of these individuals and groups, perhaps especially if their collaboration is framed by an RDS-based perspective to their work.

The multiplicity of disciplines called on to apply their scientific expertise in the service of enhancing the development of individuals, families, and communities resulted in a collaboration among the above-noted scholarly societies. These groups organized a "National Task Force on Applied Developmental Science" in order to synthesize research and applications aimed at describing, explaining, and promoting optimal developmental outcomes across the life cycle of individuals, families, and communities.

To accomplish these objectives, the National Task Force defined the nature and scope of applied developmental science (ADS). The Task Force forwarded these definitions in the context of convening a national conference (at Fordham University, in October 1991) on "Graduate Education in the Applications of Developmental Science across the Life Span." The conference, organized and led by Celia B. Fisher, inaugurated ADS as a formal program of graduate study and specified the key components involved in graduate education in ADS (Fisher et al., 1993).

Celia B. Fisher

Reflecting the influence of RDS-based ideas across the disciplines and scholarly organizations involved in the National Task Force, the definition of ADS that was derived from the work of the group indicated that this field involved the systematic synthesis of research and applications to describe, explain, and promote optimal developmental outcomes in individuals and families as they develop across the life cycle (Fisher et al., 1993). In addition, the Task Force defined the term "applied" as having direct implications for what individuals, families, practitioners, and policy-makers do. The term "developmental" was defined by the group as systematic and successive changes within human systems that occur across the life span. Finally, the Task Force defined "science" as an endeavor grounded in a range of research methods designed to systematically collect reliable and objective information that can be used to test the validity of theory and applications (Fisher et al., 1993).

Similarly, the set of activities involved in ADS that was enumerated by the Task Force reflects as well the RDS-based research and methodological agenda that I discussed both earlier in this chapter and in previous chapters. That is, the Task Force noted that activities of ADS span a continuum of knowledge generation to knowledge application which includes, but is not limited to:

1. Research on the applicability of scientific theory to growth and development in "natural" (i.e., ecologically valid) contexts.
2. The study of developmental correlates of phenomena of social import.
3. The construction and utilization of developmentally and contextually sensitive assessment instruments.
4. The design and evaluation of developmental interventions and enhancement programs.
5. The dissemination of developmental knowledge to individuals, families, communities, practitioners, and policy-makers through developmental education, written materials, the mass media, expert testimony, and community collaborations.

I should note that, in addition to reflecting conceptual and empirical themes embodied within the set of ideas associated with RDS metatheory, the articulation by the Task Force of ADS activities reflected also the philosophy, the scholarly interests, and the outreach (application) agenda of the land-grant university system in the United States (Bonnen, 1998; Kellogg Commission on the Future of State and Land-Grant Universities, 1999; Lerner & Miller, 1993; Lerner & Simon, 1998; Miller & Lerner, 1994; Ralston et al., 1999).

In sum, and also consistent with an RDS-based perspective, applied developmental scientists seek to synthesize research and application in order to describe, explain, and optimize development in individuals and families across the life span (Fisher & Lerner, 1994). Celia B. Fisher and her colleagues (Fisher et al., 1993) characterized the principles, or core substantive features, of ADS in regard to the following five conceptual themes:

1. *The temporality of change*. There is a temporal component to individuals, families, institutions, and community experiences. Some components remain constant across time; other components may change. The temporality of change has important implications for research design, service provision, and program evaluation.

2. *Sensitivity to individual differences and within-person change*. Interventions must take into account between-person differences in within-person change. Therefore, attention must be paid to the diversity of racial, ethnic, social class, and gender groups.
3. *The centrality of context*. Contextual influences exist at all levels of organization within the ecology of human development—biological, physical-ecological, sociocultural, political, economic, etc.—and need to be considered in integrative and systemic approaches to research and to program design, implementation and evaluation.
4. *The importance of an emphasis on (descriptively) normative developmental processes, and on primary prevention and optimization—on the promotion of positive development—rather than on remediation*. The diversity of intraindividual development and of variation among individuals and across groups in intraindividual development means that there is no, one norm for positive or healthy development. Groups and the individuals within them do not develop in accord with universal (nomothetic) norms or in accord with any single group (e.g., European Americans) purported to be the standard against which other groups are compared. Applied developmental scientists must focus on the idiographic character of development and, as such, must seek individualized approaches to preventing problems and to promoting positive or healthy development.
5. *Respect for the bidirectional relationship between knowledge generation and knowledge application*. There is a mutually influential relation between generating knowledge of human development and applying knowledge of human development. There is a knowledge generation↔knowledge application relation.

Moreover, given the RDS-based perspective involved in ADS, scholars in this field assume that there is a coactive

> relationship between science and application. Accordingly, the work of those who generate empirically based knowledge about development and those who provide professional services or construct policies affecting individuals and families is seen as reciprocal in that research and theory guide intervention strategies and the evaluation of interventions and policies provides the bases for reformulating theory and future research . . . As a result, applied developmental [scientists] not only disseminate information about development to parents, professionals, and policy makers working to enhance the development of others, they also integrate the perspectives and experiences of these members of the community into the reformulation of theory and the design of research and interventions.
>
> (Lerner & Fisher, 1994, p. 7)

The RDS-based ideas that frame ADS promote a revised view of the optimization component of the tripartite goals of developmental science, one predicated on seeing the world as an integrated, dynamic system. Enacting the activities proposed to be involved in ADS, as enumerated by the National Task Force (Fisher et al., 1993), requires that developmental scientists must see beyond the framework for the study of human development that may be associated with any one discipline. Developmental scientists seeking to describe, explain, and optimize human development must strive to understand the changing interrelations among levels of organization that comprise the integrated, relational developmental system.

In turn, merely additively assembling information across different disciplines typically just results in a simple layering of knowledge. Such an approach, then, falls short of understanding the dynamics across levels of organization that constitute the holistic process of human health and positive development (Halfon, Forrest, Lerner, & Faustman, 2018). As such, applied work that is not framed within the RDS-based conceptions used by ADS lacks ecological validity; it does not involve the essence of the integrative individual↔context processes involved in healthy and positive development across the life course (Halfon et al., 2018).

Given the focus on the holistic and integrated relational developmental system within which human life is embodied, ecological validity is of fundamental concern in RDS-based approaches to research and application. As such, within an ADS approach to using developmental science evidence for applications to policies and programs, developmental scientists seek to integratively better their understanding of human development and their service to society. As such, humility is an important character virtue in ADS work, in that, to serve the diverse individuals, families, and communities within the relational developmental system, developmental scientists must collaborate with these individuals and groups to enhance their understanding of the lives, perspectives, and cultures that moderate the development of these people and groups (e.g., Mistry & Dutta, 2015; Rogoff, 1998, 2003, 2011).

For instance, in enacting ADS activities in specific communities, developmental scientists would collaborate with the individuals, families, and organizations within the community to co-define the nature of research and program design, delivery, and evaluation endeavors. Lerner and Miller (1998) termed this researcher↔community approach to application *outreach scholarship*; as noted above, this approach to application is a hallmark of the mission enacted by U.S. land-grant universities (Lerner & Simon, 1998; McHale & Lerner, 1996). Thus, applied developmental scientists seek ways to apply their scientific expertise to collaborate with, and promote the life chances of, the individuals, social groups, and communities participating in developmental scholarship. The key challenge in such efforts is to generate scientifically rigorous evaluations of the usefulness of the policies and the programs associated with such ADS and, as well, to use such information in the day-to-day operation of programs (Fetterman, Kaftarian, & Wandersman, 1996; Higgins-D'Alessandro et al., 1998; Jacobs, 1988; Ostrom, Lerner, & Freel, 1995; Weiss & Greene, 1992).

To understand how such community-collaborative scholarship may be enacted, it is useful to discuss some examples of application of ADS ideas. Accordingly, I discuss ADS-related efforts in regard to program evaluation and, in turn, at a more macrosystem level, I discuss how ADS may influence policies pertinent to enhancing civil society and social justice.

RDS-Based Approaches to the Design of Program Evaluation: The Sample Case of Evolutionary Evaluation

Urban, Hargraves, and Trochim (2014) presented an approach to program evaluation that is derived from systems science and reflects the RDS-based ideas we have been discussing. Termed "evolutionary evaluation" (EE), Urban et al. note that this approach integrates evolutionary theory, RDS-based theories, and evolutionary epistemology to "provide deep theoretical foundations for understanding programs, their development over time, and the role of evaluation . . . [and that EE] offers a way of thinking about program development that has deep theoretical foundations and casts new light on some of the major controversies in evaluation and applied social research" (2014, p. 127). Used in evaluations of 4-H youth development programs in New York State and in youth character development programs in Scotland, the EE approach provides empirical justification for arguing that "the evidence-based label is being applied to programs prematurely and that the definition of [evidence-based practices, or] EBPs needs to consider multiple types of validity and the importance of methodological pluralism" (Urban et al., 2014, p. 128).

EE integrates the assessment of the evolution of programs with the aligned evolution of evaluation methods. That is, EE conceptualizes four phases of program evolution—initiation, development, stability, and dissemination—and couples these phases with evaluation methods that involve, respectively, process and response examination, examination of change, comparison and control, and generalizability (see Figure 1 in Urban et al., 2014, p. 129). Urban et al. (2014) underscore "the importance of appropriately yoking method to program phase" and explain that there should be "a sequence of evaluation efforts that begins by assessing the viability of an intervention (i.e., the degree to which it is practical, affordable, helpful, etc.) before trying to assess effectiveness or using efficacy methods such as an RCT" (p. 134). Moreover, they warn:

> Decisions based on findings from "premature experimentation" risk discontinuing an otherwise

> potentially effective program that has not yet reached a level of stability that would allow for the detection of positive effects, or the promotion of an otherwise poor program that happened to demonstrate positive results (but which may not be replicable over subsequent rounds of implementation.
>
> (Urban et al., 2014, p. 135)

Within the EE approach, it is useful to distinguish between top-down and bottom-up approaches to program development and evaluation. Urban et al. explain that top-down programs are typically developed by researchers and emphasize internal validity as the key focus of program evaluation. RCTs are the method prototypically associated with internal validity and, as well, with the prioritization of such validity over other forms of validity, e.g., external validity (as manifested, for example, by generalizability to other settings or samples) or ecological validity (the veridicality of the program in regard to the way people within a specific setting actually live their lives; Brunswik, 1955; Lerner & Callina, 2014b). Bottom-up programs are typically developed by practitioners and emphasize construct validity and viable validity; this type of validity is an instance of ecological validity, and it pertains to whether the program is actually possible to implement and sustain in a specific community (see Chen, 2010). Consistent with the RDS-based ideas of integration and holism, within the EE approach there is a focus on all forms of validity (including construct, content, internal, external, and viable validity) and, in particular, an emphasis on the need to address all validity types before labeling a program as "evidence-based."

Thus, EE integrates both the bottom-up (practitioner-driven) and the top-down (researcher-driven) approaches and elucidates their respective strengths and shortcomings. EE also reflects the bidirectional and mutually influential relation between research and application that is of focal concern in ADS. In turn, Urban et al. (2014) explain that evaluation research has ordinarily termed programs that have been assessed by a top-down approach, particularly RCTs, as being "evidence-based." However, they argue that "It would be premature to label such programs as 'rigorous' or 'evidence-based' when they have not addressed viability, whether the program reflects what was intended, whether the measures accurately reflect the outcomes, or whether they can work in any but the original testing contexts" (Urban et al., 2014, p. 136). That is, there must be evidence that programs work in the ecologically valid settings of the individuals and families who are intended to benefit from them.

In sum, proponents of EE argue that many potentially viable programs may be lost when evaluations associated with bottom-up methods are ignored or undervalued. Proponents of EE argue as well that one must establish all the above-noted forms of validity, as well as other forms (e.g., construct and conclusion validity), in order to maximize the chances that good programs will survive and be successful. Accordingly, from the perspective of proponents of EE, only when as many sources of variation as possible are drawn on within an integrative, community-collaborative approach as, for instance, marked by research↔program practitioner relations, including variation associated with both bottom-up and top-down approaches, should a program be regarded as possessing rigorous evaluation data. As a consequence, only in the context of such a thorough approach to program evaluation (see Figure 2 in Urban et al., 2014, p. 133, for an illustration of the breadth of content of the integrative approach involved in EE) can program evaluators assure that there is the necessary, appropriate alignment between program phases and EE evaluation methods.

Only with such information in hand can community members, evaluators, and funders make fully informed choices about the destiny of a program, that is, to sustain, end, or change it. In addition, only with such information can a community be empowered to continue to enact programs that fit with their needs, values, beliefs, and culture.

Applied Developmental Science, the Future of Civil Society, and the Promotion of Social Justice

Civil society rests on integrative contributions by all sectors and institutions of a nation in support of social justice (Lerner, 2004). Such contributions to civil society would assure that there is a "level playing field" for individuals to pursue lives marked by positive and healthy contributions to self, family, and community. To maintain and perpetuate such actions, social functioning that supports civil society must be transformed into public policy. ADS may act as an instrument for the promotion of civil society by (a) ADS-oriented scholars conducting research that engages public policy and (b) ADS scholars working to promote in their institutions a sustained commitment to engaging their communities in collaborative actions that merge research and service in support of civil society, that is, actions involving outreach scholarship (Kellogg Commission, 1999; Lerner & Simon, 1998; Lerner & Miller, 1998).

ADS research may engage public policy by ascertaining whether current local, state, and federal policies are supported by or run counter to research evidence and by providing empirical grounding for policies (Jensen et al., 1999). Studies can be made of programs and policies already in place or of the likely impacts of actions that may be developed into policies (Jensen et al., 1999). If such ADS scholarship and the institutions within which such work is conducted are to contribute to the enhancement and future maintenance of civil society, they must aid policy-makers in developing principles or strategies (i.e., policies) that enable all families to produce children capable of, and committed to, contributing to self and to society in a positive and integrated way.

In other words, in the superordinate sense of enabling civil society to be maintained and perpetuated, all families with children—no matter what their particular structure may be (e.g., families wherein two biological parents rear children, families wherein stepparents are involved in childrearing, families with adopted children, single-parent families, or families involving gay or lesbian parents)—have the responsibility of socializing the next generation in ways that allow children to become productive and committed members of society (Ganong, Coleman, & Russell, 2015; Gore & Gore, 2002; Sarason, 1973). Any society, then, needs to develop rules (policies) that enable such contributions to be made by the diverse families that exist within it (Lerner et al., 1999).

Across the chapters in this book, I indicated that developmental scientists working within an RDS-based perspective have in the repertoire of models and of methods in their intellectual "toolbox" the means to act in the service of promoting a better life for all people, to give diverse individuals the requisite chances needed to maximize their aspirations and actions aimed at being active producers of their positive development, and to promote a more socially just world (Lerner, 2002, 2004; Lerner & Overton, 2008). In this regard, Lerner and Overton (2008) noted that theoretically predicted changes in the RDS need to be evaluated in regard to how positive development may be promoted among individuals whose ecological characteristics (e.g., socioeconomic circumstances, neighborhood conditions, racial, ethnic, gender, or sexual preference characteristics) might lower the probability of such development (e.g., Bradley, 2015; Duncan, Magnuson, & Votruba-Drzal, 2015; Leventhal, Dupéré, & Shuey, 2015; Masten, Narayan, Silverman, & Osofsky, 2015; Murry, Hill, Witherspoon, Berkel, & Bartz, 2015; Sampson, 2016). To contribute significantly to creating a developmental science aimed at promoting social justice, scholars need to identify the means to change individual↔context relations in manners that enhance the probability that all individuals, no matter their individual characteristics or contextual circumstances, have greater opportunity to experience positive development (e.g., see Cauffman, Shulman, Bechtold, & Steinberg, 2015; Dodge & Haskins, 2015; Fisher, Busch, Brown, & Jopp, 2013).

Indeed, Fisher and Lerner (2013) noted that social justice focuses on the rights of all groups in a society to have fair access to, and a voice in policies governing, the distribution of resources essential to their physical and psychological well-being. Social justice focuses also on social inequities, characterized as avoidable and unjust social structures and policies that limit access to resources based solely on group or individual characteristics such as race/ethnicity, age, gender, sexual orientation, physical or developmental ability status, and/or immigration status, among others. Developmental science framed by the process-relational paradigm and RDS metatheory has a clear agenda involving such scholarship.

For instance, Fisher et al. (2013) provided a vision for social justice-relevant research in developmental science. Some of the research foci they discuss include addressing the pervasive systemic disparities in opportunities for development; investigating the origins, structures, and consequences of social inequities in human development; identifying societal barriers to health and well-being; identifying barriers to fair allocation of and access to resources essential to positive development; identifying how racist and other prejudicial ideologies and behaviors develop in majority groups; studying how racism, heterosexism, classism, and other forms of chronic and acute systemic inequities and political marginalization may have a "weathering" effect on physical and mental health across the life span; enacting evidence-based prevention and policy research aimed at demonstrating if systemic oppression can be diminished and psychological and political liberation can be promoted; taking a systems-level approach to reducing unjust institutional practices and to promoting individual and collective political empowerment within organizations, communities, and local and national governments; evaluating programs and policies that alleviate developmental harms caused by structural injustices; and creating and evaluating empirically-based interventions that promote a just society that nurtures lifelong healthy development in all of its members.

ADS provides an RDS-based conceptual frame for a set of key issues that can be addressed by researchers seeking to integratively enhance understanding of the diversity of paths that may be promoted in the service of enhancing individual development and, at the same time, contributing to civil society and social justice. Such contributions may be made by ADS-oriented scholars engaging collaboratively with communities to translate research into action and service and to promote ecologically valid programs and policies. Integratively advancing the quality of developmental science and advancing the presence of a civil society that is socially just can be the legacy that derives from such ADS activities.

CONCLUSIONS: TOWARDS THE FUTURE OF DEVELOPMENTAL SCIENCE

The probabilistic-epigenetic, plastic, autopoietic, and embodied developmental changes that characterize individual↔context relations within the autopoietic relational developmental system provide a rationale for, and optimism that developmental scientists can be successful in, applications aimed at promoting thriving and social justice for all people. However, as I have argued and, I hope, illustrated throughout this book, such work rests upon framing developmental science within models derived from RDS metatheory. If this approach to scholarship is pursued, what, then, may be the future trajectory of developmental science?

The theoretical orientations and interests of contemporary cohorts of developmental scientists, the aspiration to produce scholarship that matters in the real world, and the need for evidence-based means to address the challenges of the twenty-first century have coalesced to create a challenge and an opportunity for developmental scientists. The challenge is the immensity of the task of promoting thorough and sustained systems change. The opportunity is to contribute in meaningful and enduring manners to the enhancement of the lives of diverse individuals, their families, and their communities in ways that transform for the better lives of people around the world.

The scientific and societal value on which the developmental science of the future will be judged is whether the theoretical and methodological tools of the field describe and explain the diversity and dynamism of human development and, as well, are effectively promoting individual thriving and social justice. The most fundamental task, then, of developmental scientists working to understand and to promote the positive development of individuals and social justice in a civil society is to conduct good science in the service of devising and using theoretical and methodological tools that are integratively productive at promoting these individual and contextual goals for the diverse individuals of the world. Building on the scholarship of developmental scientists working within RDS-based models, developmental science of the future can act as a producer and a product of a more just world, one wherein every individual is born within a social context wherein science and society collaborate to enable humans and humanity to flourish.

References

Aboud, F. E. (2005). The development of prejudice in childhood and adolescence. In J. F. Dovidio, P. Glick, & L. A. Rudman (Eds.), *On the nature of prejudice: Fifty years after Allport* (pp. 310–326). New York: Blackwell.

Abravanel, E. (1968). The development of intersensory patterning with regard to selected, spatial dimensions. *Monographs of the Society for Research in Child Development, 33, 2*, 118.

Addis. K. M., & Kahana, M. J. (2004). Decomposing serial learning. What is missing from the learning curve? *Psychonomic Bulletin & Review, 11, 1*, 118–124.

Alcock, J. (2001). *The triumph of sociobiology*. New York: Oxford University Press.

Alexander, C. N., & Langer E. J. (Eds.). (1990). *Higher stages of human development*. New York: Oxford University Press.

Allister, L., Lester, B. M., Carr, S., & Liu, J. (2001). The effects of maternal depression on fetal heart rate response to vibroacoustic stimulation. *Developmental Neuropsychology, 20*, 639–651.

Allport, G. W. (1937). *Personality: A psychological interpretation*. New York: Holt.

Allport, G. W. (1954). *On the nature of prejudice*. Reading, MA: Addison-Wesley.

Altman, I., & Rogoff, B. (1987). World views in psychology: Trait, interactional, organismic and transactional perspectives. In D. Stokols & I. Altman (Eds.), *Handbook of environmental psychology* (pp. 7–40). New York: Wiley.

Anastasi, A. (1958). Heredity, environment, and the question "how?" *Psychological Review, 65*, 197–208.

Anderson, P. W. (1972). More is different. *Science, 177*, 393–396.

Angrist, J. D., Imbens, G. W., & Rubin, D. B. (1996). Identification of causal effects using instrumental variables. *Journal of the American Statistical Association*, *91*, 444–455.

Anthony, E. J. (1969). The reactions of adults to adolescents and their behavior. In G. Caplan & S. Lebovici (Eds.), *Adolescence: Psychosocial perspectives* (p. 77). New York: Basic Books.

Antonucci, T. C., & Jackson, J. S. (1990). The role of reciprocity in social support. In I. G. Sarason, B. R. Sarason, & G. R. Pierce (Eds.), *Social support: An interactional view* (pp. 111–128). New York: John Wiley & Sons.

Arbeit, M. R., Hershberg, R. M., Rubin, R. O., DeSouza, L. M., & Lerner, J. V. (2016). "I'm hoping that I can have better relationships": Exploring interpersonal connection for young men. *Qualitative Psychology, 3*, 79–97.

Ardelt, M. (2000). Still stable after all these years? Personality stability theory revisited. *Social Psychology Quarterly*, *63*, 392–405.

Aristotle. (1999). *Nicomachean Ethics* (T. H. Irwin, Trans.). Indianapolis, IN: Hackett Publishing Company.

Aronson, L. R., Tobach, E., Lehrman, D. S., & Rosenblatt, J. S. (Eds.). (1970). *Development and evolution of behavior: Essays in memory of T. C. Schneirla*. San Francisco: W. H. Freeman.

Aronson, L. R., Tobach, E., Rosenblatt, J. S, & Lehrman, D. S. (Eds.). (1972). *Selected writings of T. C. Schneirla*. San Francisco: W. H. Freeman.

Atz, J. W. (1970). The application of the idea of homology to behavior. In L. R. Aronson, E. Tobach, D. S. Lehrman, & J. S. Rosenblatt (Eds.), *Development and evolution of behavior: Essays in memory of T. C. Schneirla* (pp. 53–74). San Francisco: W. H. Freeman.

Baer, D. (1970). An age-irrelevant concept of development. *Merrill-Palmer Quarterly of Behavior and Development, 16*, 238–245.

Baer, D. M. (1976). The organism as host. *Human Development, 19*, 87–89.

Baer, D. M. (1982). Behavior analysis and developmental psychology: Discussant comments. *Human Development, 25*, 357–361.

Baldwin, J. M. (1897a). *Mental development in the child and the race*. New York: Macmillan.

Baldwin, J. M. (1897b). *Social and ethical interpretations in mental development: A case study in social psychology*. New York: Macmillan.

Ballard, P. J., Malin, H., Porter, T., Colby, A., & Damon, W. (2015). Motivations for civic participation among diverse youth: More similarities than differences. *Research in Human Development, 12, 1–2*, 63–83.

Baltes, M. M. (1987). Erfolgreiches altern als ausdruck von verhaltenkompetenz und unweltqualität. In C. Niemitz (Ed.), *Der mensch im zusammenspiel von anlage und umwelt* (pp. 353–377). Frankfurt: Suhrkamp.

Baltes, M., & Carstensen, L. L. (1996). The process of successful ageing. *Ageing and Society, 16*, 397–422.

Baltes, M. M., & Carstensen, L. L. (1998). Social-psychological theories and their applications to aging: From individual to collective. In V. L. Bengtson & K. W. Schaie (Eds.), *Handbook of theories of aging* (pp. 209–226). New York: Springer.

Baltes, P. B. (1968). Longitudinal and cross-sectional sequences in the study of age and generation effects. *Human Development, 11*, 145–171.

Baltes, P. B. (Ed.). (1978). *Life-span development and behavior* (Vol. 1). New York: Academic Press.

Baltes, P. B. (1979a). Life-span developmental psychology: Some converging observations on history and theory. In P. B. Baltes & O. G. Brim, Jr. (Eds.), *Life-span development and behavior* (Vol. 2, pp. 255–279). New York: Academic.

Baltes, P. B. (1979b). On the potential and limits of child development: Life-span developmental perspectives. *Newsletter of the Society of Research in Child Development*, 1–4.

Baltes, P. B. (1983). Life-span developmental psychology: Observations on history and theory revisited. In R. M. Lerner (Ed.), *Developmental psychology: Historical and philosophical perspectives* (pp. 79–111). Hillsdale, NJ: Erlbaum.

Baltes, P. B. (1987). Theoretical propositions of life-span developmental psychology: On the dynamics between growth and decline. *Developmental Psychology, 23*, 611–626.

Baltes, P. B. (1997). On the incomplete architecture of human ontogeny: Selection, optimization, and compensation as foundations of developmental theory. *American Psychologist, 52*, 366–380.

Baltes, P. B., & Baltes, M. M. (1980). Plasticity and variability in psychological aging: Methodological and theoretical issues. In G. E. Gurski (Ed.), *Determining the effects of aging on the central nervous system* **(**pp. 41–66**)**. Berlin: Schering AG (Oraniendruck).

Baltes, P. B., & Baltes, M. M. (1990). Psychological perspectives on successful aging: The model of selective optimization with compensation. In P. B. Baltes & M. M. Baltes (Eds.), *Successful aging: Perspectives from the behavioral sciences* (pp. 1–34). New York: Cambridge University Press.

Baltes, P. B., Baltes, M. M., Freund, A. M., & Lang, F. R. (1995). *Measurement of selective optimization with compensation by questionnaire*. Berlin: Max Planck Institute for Human Development and Education.

Baltes, P. B., Baltes, M. M., & Reinert, G. (1970). The relationship between time of measurement and age in cognitive development of children: An application of cross-sectional sequences. *Human Development, 13*, 258–268.

Baltes, P. B., & Brim, O. G., Jr. (Eds.). (1984). *Life-span development and behavior* (Vol. 6). New York: Academic Press.

Baltes, P. B., Cornelius, S. W., & Nesselroade, J. R. (1978). Cohort effects in behavioral development: Theoretical and methodological perspectives. In W. A. Collins (Ed.), *Minnesota symposia on child psychology* (Vol. 11, pp. 1–63). Hillsdale, NJ: Erlbaum.

Baltes, P. B., Cornelius, S. W., & Nesselroade, J. R. (1979). Cohort effects in developmental psychology. In J. R. Nesselroade & P. B. Baltes (Eds.), *Longitudinal research in the study of behavior and development* (pp. 61–87). New York: Academic Press.

Baltes, P. B., Dittmann-Kohli, F., & Dixon, R. A. (1984). New perspectives on the development of intelligence in adulthood: Toward a dual-process conception and a model of selective optimization with compensation. In

P. B. Baltes & O. G. Brim, Jr. (Eds.), *Life-span development and behavior* (Vol. 6). New York: Academic Press.

Baltes, P. B., Featherman, D. L., & Lerner, R. M. (Eds.). (1986). *Life-span development and behavior* (Vol. 7). Hillsdale, NJ: Erlbaum.

Baltes, P. B., Lindenberger, U., & Staudinger, U. M. (1998). Life-span theory in developmental psychology. In W. Damon (Series Ed.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (5th ed., pp. 1029–1144). New York: Wiley.

Baltes, P. B., Lindenberger, U., & Staudinger, U. M. (2006). Life span theory in developmental psychology. In R. M. Lerner (Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (6th ed., pp. 569–664). Editors-in-chief: W. Damon & R. M. Lerner. Hoboken, NJ: John Wiley & Sons.

Baltes, P. B., & Nesselroade, J. R. (1973). The developmental analysis of individual differences on multiple measures. In J. R. Nesselroade & H. W. Reese (Eds.), *Life-span developmental psychology: Introduction to research methodological issues* (pp. 219–251). New York: Academic Press.

Baltes, P. B., Reese, H. W., & Lipsitt, L. P. (1980). Life-span developmental psychology. *Annual Review of Psychology*, *31*, 65–110.

Baltes, P. B., Reese, H. W., & Nesselroade, J. R. (1977). *Life-span developmental psychology: Introduction to research methods.* Monterey, CA: Brooks/Cole.

Baltes, P. B., & Schaie, K. W. (Eds.). (1973). *Life-span developmental psychology: Personality and socialization*. New York: Academic.

Baltes, P. B., & Schaie, K. W. (1974). Aging and IQ: The myth of the twilight years. *Psychology Today*, *7*, 35–40.

Baltes, P. B., & Schaie, K. W. (1976). On the plasticity of intelligence in adulthood and old age: Where Horn and Donaldson fail. *American Psychologist*, *31*, 720–725.

Baltes, P. B., & Staudinger, U. M. (Eds.). (1996). *Interactive minds: Life span perspectives on the social foundation of cognitio*n. New York: Cambridge University Press.

Baltes, P. B., Staudinger, U. M., & Lindenberger, U. (1999). Life span psychology: Theory and application to intellectual functioning. In J. T. Spence, J. M. Darley, & D. J. Foss (Eds.), *Annual Review of Psychology* (Vol. 50, pp. 471–507). Palo Alto, CA: Annual Reviews.

Baltes, P. B., & Willis, S. L. (1982). Plasticity and enhancement of intellectual functioning in old age: Penn State's Adult Development and Enrichment Project (ADEP). In F. I. M. Craik & S. E. Trehub (Eds.), *Aging and cognitive processes* (pp. 353–389). New York: Plenum Press.

Bandura, A. (1964). The stormy decade: Fact or fiction? *Psychology in the School, 1*, 224–231.

Bandura, A. (1977). Self-efficacy: Toward a unifying theory of behavioral change. *Psychological Review, 84,* 191–215.

Bandura, A. (1978). The self system in reciprocal determinism. *American Psychologist, 33*, 344–358.

Bandura, A. (2001). The changing face of psychology at the dawning of a globalization era. *Canadian Psychology, 42, 1*, 12–24.

Barash, D. P. (1977). *Sociobiology and behavior*. New York: Elsevier.

Barlow, G. W. (1980). The development of sociobiology: A biologist's perspective. In G. W. Barlow & J. Silverberg (Eds.), *Sociobiology: Beyond nature/nurture* (pp. 3–34). Boulder, CO: Westview.

Barlow, G. W., & Silverberg, J. (Eds.). (1980). *Sociobiology: Beyond nature/nurture? Reports, definitions, and debate*. Boulder, CO: Westview.

Basser, L. S. (1962). Hemiplegia of early onset and the faculty of speech with special reference to the effects of hemispherectomy. *Brain, 85*, 427–460.

Bateson, P. (1966). The characteristics and context of imprinting. *Biological Reviews, 41*, 177–220.

Bateson, P. (1979). How do sensitive periods arise and what are they for? *Animal Behavior, 27*, 470–486.

Bateson, P. (1983). The interpretation of sensitive periods. In A. Oliverio & M. Zapella (Eds.), *The behavior of human infants* (pp. 57–70). New York: Plenum.

Bateson, P. (2004). The active role of behavior in evolution. *Biology and Philosophy, 19*, 283–298.

Bateson, P. (2015) Ethology and human development. In W. F. Overton & P. C. Molenaar (Eds.), *Handbook of child psychology and developmental science: Vol. 1 Theory and method* (7th ed., pp. 208–243). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Bateson, P. (2016). Robustness and plasticity. *WIREs Cogn Sci* 2016. doi: 10.1002/wcs.1386.

Bateson, P., & Gluckman, P. (2011). *Plasticity, development and evolution*. Cambridge, UK: Cambridge University Press.

Bauer, E., Fischer, E., & Lenz, F. (1927). *Grundriss der menschlichen Erblicheitslehre und Rassenhygiene* (Vol. 1, 3rd ed.). Munich, Germany: J. F. Lehmann.

Baumrind, D. (1971). Current patterns of parental authority. *Developmental Psychology Monographs, 4, 1*, Part 2.

Baumrind, D. (1972). Some thoughts about child rearing. In U. Bronfenbrenner (Ed.), *Influences on human development* (pp. 270–281). Hinsdale, IL: Dryden.

Baxter, B. (1966). Effect on visual deprivation during postnatal maturation of the electroencephalogram of the cat. *Experimental Neurology, 14*, 224–237.

Beach, F. A. (1950). The Snark was a Boojum. *American Psychologist, 5*, 115–124.

Beals, K. L., Smith, C. L., & Dodd, S. M. (1984). Brain size, cranial morphology, climate, and time machines. *Current Anthropology, 25*, 301–330.

Bearer, E. (2004). Behavior as influence and result of the genetic program: Non-kin rejection, ethnic conflict, and issues in global health care. In C. Garcia Coll, E. Bearer, & R. M. Lerner (Eds.), *Nature and nurture: The complex interplay of genetic and environmental influences on human behavior and development* (pp. 171–199). Mahwah, NJ: Erlbaum.

Bell, R. Q. (1968). A reinterpretation of the direction of effects in studies of socialization. *Psychological Review, 75*, 81–95.

Bell, R. Q., & Harper, L. V. (1977). *Child effects on adults*. Hillsdale, NJ: Erlbaum.

Belsky, J. (2012). The development of human reproductive strategies: Progress and prospects. *Current Directions in Psychological Science*, *21, 5*, 310–316.

Belsky, J. (2014, November 30). The downside of resilience. *New York Times*, *Sunday Review*, p. SR4.

Belsky, J., Steinberg, L., & Draper, P. (1991). Childhood experience, interpersonal development, and reproductive strategy: An evolutionary theory of socialization. *Child Development, 62*, 647–670.

Belsky, J., & Tolan, W. J. (1981). Infants as producers of their own development: An ecological analysis. In R. M. Lerner & N. A. Busch-Rossnagel (Eds.), *Individuals as producers of their own development: A life-span perspective* (pp. 87–116). New York: Academic Press.

Benson, P. L. (2008). *Sparks: How parents can help ignite the hidden strengths of teenagers.* San Francisco: Jossey-Bass.

Benson, P. L., Leffert, N., Scales, P. C., & Blyth, D. A. (1998). Beyond the "village" rhetoric: Creating healthy communities for children and adolescents. *Applied Developmental Science, 2, 3*, 138–159.

Benson, P. L., Scales, P. C., Hamilton, S. F., & Semsa, A., Jr. (2006). Positive youth development: Theory, research, and applications. In R. M. Lerner (Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (6th ed., pp. 894–941). Editors-in-chief: W. Damon & R. M. Lerner. Hoboken, NJ: Wiley.

Benson, P. L., Scales, P. C., & Syvertsen, A. K. (2011). The contribution of the developmental assets framework to positive youth development theory and practice. In R. M. Lerner, J. V. Lerner, & J. B. Benson (Eds.), *Advances in child development and behavior: Positive youth development: Research and applications for promoting thriving in adolescence* (pp. 195–228). London: Elsevier.

Berg, P. (1981). Dissections and reconstructions of genes and chromosomes. *Science, 213*, 296–303.

Berger, P. L., Berger, P. L., & Kellner, H. (1967). *The homeless mind: Modernization and unconsciousness.* New York: Random House.

Berkowitz, M. W. (2012). Moral and character education. In K. R. Harris, S. Graham, & T. Urdan (Eds.), *APA educational psychology handbook. Vol. 2 Individual differences and contextual factors* (pp. 247–264). Washington, DC: American Psychological Association.

Berndt, T. J. (1978). Stages as descriptions, explanations, and testable constructs. *The behavioral and brain sciences, 2*, 183–184.

Bigler, R. S. (1995). The role of classification skill in moderating environmental influences on children's gender stereotyping: A study of the functional use of gender in the classroom. *Child Development, 66*, 1072–1087.

Bigler, R. S., & Liben, L. S. (2006). A developmental intergroup theory of social stereotypes and prejudice. In R. V. Kail (Ed.), *Advances in child development and behavior* (Vol. 34, pp. 39–89). San Diego, CA: Elsevier.

Bijou, S. W. (1976). *Child development: The basic stage of early childhood*. Englewood Cliffs, NJ: Prentice-Hall.

Bijou, S. W., & Baer, D. M. (1961). *Child development: A systemic and empirical theory* (Vol. 1). New York: Appleton-Century-Crofts.

Binding, K., & Hoche, A. (1920). *Die Freigabe der vernichtung lebensunwerten lebens* (The sanctioning of the destruction of lives unworthy to be lived). Leipzig: F. Meiner.

Binet, A., & Simon, T. (1905a). Sur la nécessité d'établir un diagnostic scientifique des états inférieurs de l'intelligence. *L'Année Psychologique, 11*, 162–190.

Binet, A., & Simon, T. (1905b). Methodes nouvelles pour le diagnostic du niveau intellectuel des anormaux. *L'Année Psychologique, 11*, 191–244.

Birch, H. G., & Lefford, A. (1963). Intersensory development in children. *Monographs of the Society for Research in Child Development, 28, 5*, 89.

Birch, H. G., & Lefford, A. (1967). Visual differentiation, intersensory integration, and voluntary motor control. *Monographs of the Society for Research in Child Development, 32, 2*, 110.

Birkel, R., Lerner, R. M., & Smyer, M. A. (1989). Applied developmental psychology as an implementation of a life-span view of human development. *Journal of Applied Developmental Psychology, 10*, 425–445.

Bitterman, M. E. (1960). Toward a comparative psychology of learning. *American Psychologist, 15*, 704–712.

Bitterman, M. E. (1965). Phyletic differences in learning. *American Psychologist, 20*, 396–410.

Bitterman, M. E. (1975). The comparative analysis of learning. *Science, 188*, 699–709.

Bjorklund, D. F. (1990). *Children's strategies: Contemporary views of child development*. Hillsdale, NJ: Erlbaum.

Bjorklund, D. F. (2015). Developing adaptations. *Developmental Review*, *38*, 13–35.

Bjorklund, D. F. (2016). Prepared is not preformed: Commentary on Witherington and Lickliter. *Human Development, 59*, 235–241.

Bjorklund, D. F., & Ellis, B. J. (2005). Evolutionary psychology and child development: An emerging synthesis. In B. J. Ellis & D. F. Bjorklund (Eds.), *Origins of the social mind: Evolutionary psychology and child development* (pp. 3–18). New York: Guilford.

Bjorklund, D. F., & Shackelford, T. K. (1999). Differences in parental investment contribute to important differences between men and women. *Current Directions in Psychological Science*, *8, 3*, 86–89.

Blakemore, C., & Van Sluyters, R. C. (1974). Reversal of the physiological effects of monocular deprivation in kittens: Further evidence for a sensitive period. *Journal of Physiology, 248*, 663–716.

Blaze, J., & Roth, T. L. (2015). Evidence from clinical and animal model studies of the long-term and transgenerational impact of stress on DNA methylation. *Seminars in Cell & Developmental Biology*, *43*, 76–84.

Blaze, J., & Roth, T. L. (2017). Caregiver maltreatment causes altered neuronal DNA methylation in female rodents. *Development and Psychopathology*, *29*, 477–489.

Block, J. (1971). *Lives through time*. Berkeley, CA: Bancroft.

Block, J. (1982). Assimilation, accommodation, and the dynamics of personality development. *Child Development, 53*, 281–295.

Block, J. (1995). A contrarian view of the five-factor approach to personality description. *Psychological Bulletin*, *117*, 187–215.

Block, J. (2010). The five-factor framing of personality and beyond: Some ruminations. *Psychological Inquiry*, *21*, 2–25.

Block, J. H., & Block, J. (1980). The role of ego-control and ego-resiliency in the organization of behavior. In W. A. Collins (Ed.), *Minnesota symposia on child psychology* (Vol. 13, pp. 89–101). Hillsdale, NJ: Erlbaum.

Bloom, B. S. (1964). *Stability and change in human characteristics*. New York: Wiley.

Bloom, L. (1998). Language acquisition in its developmental context. In W. Damon (Series Ed.), D. Kuhn & R. S. Siegler (Vol. Eds.), *Handbook of child psychology: Vol. 2 Cognition, perception, and language* (5th ed., pp. 309–370). New York: Wiley.

Blum, R. W., & Nelson-Mmari, K. (2004). Adolescent health from an international perspective. In R. M. Lerner & L. Steinberg (Eds.), *Handbook of Adolescent Psychology* (2nd ed., pp. 553–586). Hoboken, NJ: Wiley.

Blumberg, M. S. (2005). *Basic instinct. The genesis of behavior*. New York: Thunder's Mouth Press.

Blumberg, M. S. (2016). Development evolving: The origins and meanings of instinct. *WIREs Cogn Sci* 2016. doi: 10.1002/wcs.1371.

Bock, W. J. (1979). A synthetic explanation of macroevolutionary change—a reductionistic approach. *Bulletin of the Carnegie Museum of Natural History, 13*, 20–69.

Bodmer, W. F., & Cavalli-Sforza, L. L. (1976). *Genetics, evolution and man*. San Francisco: Freeman.

Boelsche, W. (1906). *Die Schopfungstage: Umrisse zu einer Entwicklungsgeschichte der Natur.* Dresden: Reissner

Bolles, R. C. (1967). *Theory of motivation*. New York: Harper & Row.

Bonnen, J. T. (1998). The land-grant idea and the evolving outreach university. In R. M. Lerner & L. A. K. Simon (Eds.), *University–community collaborations for the twenty-first century: Outreach scholarship for youth and families* (pp. 25–71). New York: Garland.

Boring, E. G. (1950). *A history of experimental psychology* (2nd ed.). New York: Appleton-Century-Crofts.

Bornstein, M. H. (Ed.). (1995a). *Handbook of parenting: Vol. 3 Status and social conditions of parenting*. Mahwah, NJ: Erlbaum.

Bornstein, M. H. (1995b). Parenting infants. In M. H. Bornstein (Ed.), *Handbook of parenting* (Vol. 1, pp. 3–39). Mahwah, NJ: Erlbaum.

Bornstein, M. H. (2006). Parenting science and practice. In K. A. Renninger & I. E. Sigel (Eds.), *Handbook of child psychology: Vol. 4 Child psychology in practice* (6th ed., pp. 893–949). Editors-in-chief: W. Damon & R. M. Lerner. Hoboken, NJ: Wiley.

Bornstein, M. H. (Ed.). (2010). *Handbook of cultural developmental science*. New York: Psychology Press.

Bornstein, M. H. (2015). Children's parent. In M. H. Bornstein and T. Leventhal (Eds.), *Handbook of child psychology and developmental science: Vol. 4 Ecological settings and processes* (7th ed., pp. 55–132). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Bornstein, M. H. (2017). The specificity principle in acculturation science. *Perspectives in Psychological Science, 12, 1*, 3–45.

Bornstein, M. H., & Lamb, M. E. (Eds.). (1999). *Developmental psychology: An advanced textbook* (4th ed.). Mahwah, NJ: Lawrence Erlbaum.

Bornstein, M. H., & Lamb, M. E. (Eds.). (2005). *Developmental science: An advanced textbook* (5th ed.). Mahwah, NJ: Lawrence Erlbaum.

Bornstein, M. H., & Tamis-LeMonda, C. S. (1990). Activities and interactions of mothers and their first-born infants in the first six months of life: Covariation, stability, continuity, correspondence, and prediction. *Child Development*, *61, 4*, 1206–1217.

Bouchard, T. J., Jr. (1982). Identical twins reared apart: Reanalysis or pseudoanalysis. Review of S. L. Farber (1981). *Identical twins reared apart: A reanalysis*. New York: Basic Books. *Contemporary Psychology, 27*, 190–191.

Bouchard, T. J., Jr. (1995). Breaking the last taboo. [Review of *The Bell Curve*, by R. J. Herrnstein & C. Murray], *Contemporary Psychology, 40*, 415–421.

Bouchard, T. J., Jr., (2014), Genes, evolution and intelligence. *Behavior Genetics, 44*, 549–577.

Bouchard, T. J., Jr., Lykken, D. T., McGue, M., Segal, N. L., & Tellegen, A. (1990). Sources of human psychological differences: The Minnesota Study of Twins Reared Apart. *Science, 250*, 223–228.

Bouchard, T. J., Jr., & McGue, M. (1981). Familial studies of intelligence: A review. *Science, 212*, 1055–1059.

Bowers, E. P., Geldhof, G. J., Johnson, S. K., Lerner, J. V., & Lerner, R. M. (2014). Elucidating the developmental science of adolescence: Lessons learned from the 4-H Study of Positive Youth Development. *Journal of Youth and Adolescence, 43, 6* (Whole Issue).

Bowers, E. P., Hilliard, L. J., Stacey, D. C., Wartella, J., Tirrell, J. M., Greenman, K. N., Doering, C., Johnson, H. L., & Lerner, R. M. (2015). The Arthur Interactive Media Study: Usability and child engagement with a digital media-based program to promote conversations about character. *Journal of Youth Development, 10*(3), 46–63.

Bowers, E. P., Li, Y., Kiely, M. K., Brittian, A., Lerner, J. V., & Lerner, R. M. (2010). The Five Cs Model of Positive Youth Development: A longitudinal analysis of confirmatory factor structure and measurement invariance. *Journal of Youth and Adolescence, 39*(7), 720–735.

Bowers, K. S. (1973). Situationism in psychology: An analysis and a critique. *Psychological Review*, *80*, 307–336.

Bowlby, J. (1969). *Attachment and loss: Vol. 1 Attachment*. New York: Basic Books.

Boyce, W. T., & Hertzman, C. (2018). Early childhood and the life course: The state of the science and proposed research priorities. In N. Halfon, C. B. Forrest, R. M. Lerner, & E. Faustman (Eds.), *Handbook of life course health development* (pp. 61–93). New York: Springer.

Boyer, E. L. (1990). *Scholarship reconsidered: Priorities of the professoriate*. Princeton, NJ: The Carnegie Foundation for the Advancement of Teaching.

Boyer, E. L. (1994, March 9). Creating the new American college. *The Chronicle of Higher Education*, A48.

Bradley, R. H. (2015). Children's housing and physical environments. In M. H. Bornstein and T. Leventhal (Eds.), *Handbook of child psychology and developmental science: Vol. 4 Ecological settings and processes* (7th ed., pp. 455–492). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Brainerd, C. J. (1978). The stage question in cognitive-developmental theory. *The Behavioral and Brain Sciences, 2*, 173–182.

Brainerd, C. J. (1979). Further replies on invariant sequences, explanation and other stage criteria. *The behavioral and brain sciences, 2*, 149–152.

Brandtstädter, J. (1998). Action perspectives on human development. In W. Damon (Series Ed.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology*: *Vol. 1 Theoretical models of human development* (5th ed., pp. 807–863). New York: Wiley.

Brandtstädter, J. (1999). The self in action and development: Cultural, biosocial, and ontogenetic bases of intentional self-development. In J. Brandtstädter & R. M. Lerner (Eds.), *Action and self-development: Theory and research through the life-span* (pp. 37–65). Thousand Oaks, CA: Sage.

Brandtstädter, J. (2006). Action perspectives on human development. In R. M. Lerner (Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (6th ed., pp. 516–568). Editors-in-chief: W. Damon & R. M. Lerner. Hoboken, NJ: Wiley.

Brandtstädter, J. & Lerner, R. M. (Eds.). (1999). *Action and self-development: Theory and research through the life-span*. Thousand Oaks, CA: Sage.

Brandtstädter, J., & Wentura, D. (1994). Veränderungen der Zeit- und Zukunftsperspektive in Übergang zum höheren Erwachsenenalter: Entwicklungspsychologische und differentielle Aspekte. *Zeitschrift für Entwicklungspsychologie und Pädagogische Psychologie, 26*, 2–21.

Brazelton, T. B., Koslowski, B., & Main, M. (1974). The origins of reciprocity: The early mother–infant interaction. In M. Lewis & L. A. Rosenblum (Eds.), *The effect of the infant on its caregiver.* New York: Wiley.

Breland, K., & Breland, M. (1961). The misbehavior of organisms. *American Psychologist, 16*, 681–684.

Brent, S. B. (1978). Individual specialization, collective adaptation and rate of environment change. *Human Development, 21*, 21–33.

Brim, O. G., Jr. (1966). Socialization through the life cycle. In O. G. Brim, Jr. & S. Wheeler (Eds.), *Socialization after childhood: Two essays* (pp. 1–49). New York: Wiley.

Brim, O. G., Jr., & Kagan, J. (Eds.). (1980a). *Constancy and change in human development*. Cambridge, MA: Harvard University Press.

Brim, O. G., Jr., & Kagan, J. (1980b). Constancy and change: A view of the issues. In O. G. Brim, Jr. & J. Kagan (Eds.), *Constancy and change in human development* (pp. 1–25). Cambridge, MA: Harvard University Press.

Brim, O. G., Jr., & Ryff, C. D. (1980). On the properties of life events. In P. B. Baltes & O. G. Brim, Jr. (Eds.), *Life-span development and behavior* (Vol. 3, pp. 367–388). New York: Academic Press.

Brim, O. G., Jr. & Wheeler, S. (1966). *Socialization after childhood: Two essays*. New York: Wiley.

Broca, P. (1861). Sur le volume et la forme du cerveau suivant les individus et suivant les races. *Bulletin de la Société de Anatomie de Paris, 2*, 139–207, 301–321, 441–446.

Broca, P. (1862a). Sur les proportions relatives du bras, de l'avant bras et de la clavicule chez les nègres et les européens. *Bulletin de la Société de Anatomie de Paris, 3*, Part 2, 13pp.

Broca, P. (1862b). Sue la capacité des crânes parisiens des diverses époques. *Bulletin Société d'Anthropologie Paris, 3*, 102–116.

Broca, P. (1862c). Sur les projections de la tête et sur un nouveau procédé de céphalométrie. *Bulletin Société d'Anthropologie Paris, 3*, 32pp.

Bronfenbrenner, U. (1963). Developmental theory in transition. In H. W. Stevenson (Ed.), *Child psychology*. Sixty-second yearbook of the National Society for the Study of Education, Part 1 (pp. 517–542). Chicago: University of Chicago Press.

Bronfenbrenner, U. (1977). Toward an experimental ecology of human development. *American Psychologist, 32*, 513–531.

Bronfenbrenner, U. (1979). *The ecology of human development: Experiments by nature and design*. Cambridge, MA: Harvard University Press.

Bronfenbrenner, U. (1983). The context of development and the development of context. In R. M. Lerner (Ed.), *Developmental psychology: Historical and philosophical perspectives*. Hillsdale, NJ: Erlbaum.

Bronfenbrenner, U. (1989). Ecological systems theory. In R. Vasta (Ed.), *Annals of child development: Six theories of child development—Revised formulations and current issues* (pp. 187–249). London: JAI Press.

Bronfenbrenner, U. (1994). Ecological models of human development. In T. Husen & T. N. Postletwaite (Eds.), *International encyclopedia of education* (2nd ed., Vol. 3, pp. 1643–1647). Oxford: Pergamon Press/Elsevier Science.

Bronfenbrenner, U. (Ed.). (2005). *Making human beings human: Bioecological perspectives on human development*. Thousand Oaks, CA: Sage.

Bronfenbrenner, U., & Ceci, S. J. (1993). Heredity, environment, and the question “How?” A new theoretical perspective for the 1990s. In R. Plomin & G. E. McClearn (Eds.), *Nature, nurture, and psychology* (pp. 313–324). Washington, DC: American Psychological Association.

Bronfenbrenner, U., & Ceci S. J. (1994). Nature–nurture reconceptualized: A bioecological model. *Psychological Review, 101*, 568–586.

Bronfenbrenner, U., & Crouter, A. C. (1983). The evolution of environmental models in developmental research. In W. Kessen (Series Ed.) & P. H. Mussen (Vol. Ed.), *Handbook of child psychology: Vol. 1 History, theory, and methods* (4th ed., pp. 357–414). New York: Wiley.

Bronfenbrenner, U., & Morris, P. A. (1998). The ecology of developmental process. In W. Damon (Series Ed.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (5th ed., pp. 993–1028). New York: Wiley.

Bronfenbrenner, U., & Morris, P. A. (2006). The bioecological model of human development. In R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (6th ed., pp. 793–828). Editors-in-chief: W. Damon & R. M. Lerner. Hoboken, NJ: Wiley.

Brooks-Gunn, J., & Petersen, A. C. (1983). *Girls at puberty: Biological and psychosocial perspectives*. New York: Plenum.

Brown, D. A., & Lamb, M. E. (2017). A contextually and developmentally sensitive view of children’s memory development: Between the laboratory and the field. In A. S. Dick & U. Müller (Eds.), *Advancing developmental science: Philosophy, theory, and method* (pp. 119–132). New York: Routledge.

Brown, D. D. (1981). Gene expression in eukaryotes. *Science, 211*, 667–674.

Brown, R. T. (1974). Following and visual imprinting in ducklings across a wide age range. *Developmental Psychobiology, 8*, 27–33.

Bruer, J. T. (1999). *The myth of the first three years*. New York: Free Press.

Bruner, J. (1964). The course of cognitive growth. *American Psychologist, 19*, 1–15.

Bruner, J. (1990). *Acts of meaning*. Cambridge, MA: Harvard University Press.

Brunswik, E. (1955). Representative design and probabilistic theory in a functional psychology. *Psychological Review, 62*, 193–121.

Buhler, C. (1928). *Kindheit und jugend*. Leipzig: Hirzel.

Buhler, C. (1933). *Der menschliche Lebenslauf als psychologisches Problem*. Leipzig: Hirzel.

Bundick, M., Yeager, D., King, P. E., & Damon, W. (2010). Thriving across the life span. In W. F. Overton (Ed.), *Handbook of lifespan development: Vol. 1 Cognition, biology, and methods* (pp. 882–923). Editor-in-chief: Richard M. Lerner. New York: John Wiley and Sons.

Burt, C. (1955). The evidence of the concept of intelligence. *British Journal of Educational Psychology, 25*, 159–177.

Burt, C. (1958). The inheritance of mental ability. *American Psychologist, 13*, 1–15.

Burt, C. (1966). The genetic determination of differences in intelligence: A study of monozygotic twins reared together and apart. *British Journal of Psychology, 57*, 137–153.

Burton, L. M., Garrett-Peters, R., Eaton, S. C. (2009) “More than good quotations”: How ethnography informs knowledge on adolescent development and context. In R. M. Lerner & L. Steinberg (Eds.), *Handbook of adolescent psychology* (3rd ed., pp. 55–92). Hoboken, NJ: Wiley.

Buss, A. R. (1979). On the four kinds of causality. *Behavioral and Brain Sciences, 2*, 139.

Buss, A. H., & Plomin, R. (1984). *Temperament: Early developing personality traits*. Hillsdale, NJ: Erlbaum.

Butcher, L. M., Davis, O. S., Craig, I. W., & Plomin, R. (2008). Genome-wide quantitative trait locus association scan of general cognitive ability using pooled

DNA and 500K single nucleotide polymorphism microassays. *Genes, Brain and Behavior, 7*(4), 435–436.

Cairns, R. B. (1998). The making of developmental psychology. In W. Damon (Series Ed.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (5th ed., pp. 419–448). New York: Wiley.

Cairns, R. B., & Cairns, B. D. (1994). *Lifelines and risks: Pathways of youth in our time*. New York: Cambridge University Press.

Cairns, R. B., & Cairns, B. D. (2006). The making of developmental psychology. In R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (6th ed., pp. 89–165). Editors-in-chief: W. Damon & R. M. Lerner. Hoboken, NJ: Wiley.

Cairns, R. B., & Hood, K. E. (1983). Continuity in social development: A comparative perspective on individual difference prediction. In P. B. Baltes & O. G. Brim, Jr. (Eds.), *Life-span development and behavior* (Vol. 5, pp. 301–358). New York: Academic Press.

Callina, K. S., Mueller, M. K., Napolitano, C. M., Lerner, J. V., & Lerner, R. M. (2016). Positive youth development: Relational developmental systems approaches to thriving from childhood to early adulthood. In S. J. Lopez, L. M. Edwards, & S. C. Marques (Eds.), *The Oxford handbook of positive psychology* (3rd ed.). New York: Oxford University Press. doi: 10.1093/oxfordhb/9780199396511.013.11.

Callina, K. S., Ryan, D., Murray, E. D., Colby, A., Damon, W., Matthews, M., & Lerner, R. M. (2017). Developing leaders of character at the United States Military Academy: A relational developmental systems analysis. *Journal of College and Character*, *18*(1), 9–27.

Campbell, D. (1990). Level of organization, downward causation, and the selection-theory approach to evolutionary epistemology. In G. Greenberg & E. Tobach (Eds.), *Theories of the evolution of knowing: The T. C. Schneirla Conference Series* (Vol. 4, pp. 1–18). Hillsdale, NJ: Erlbaum Associates.

Campbell, D. T., & Fiske, D. W. (1959). Convergent and discriminant validation by the multitrait-multimethod matrix. *Psychological Bulletin, 56*(2), 81–105.

Campbell, J. J., Lamb, M. E., & Hwang, C. P. (2000). Early child care experiences and children's social competence between 1.5 and 15 years of age. *Applied Developmental Science, 4*, 166–175.

Campbell, R. L., & Richie, D. M. (1983). Problems in the theory of developmental sequences. *Human Development, 26*, 156–172.

Capaldi, E. J., & Proctor, R. W. (1999). *Contextualism in psychological research? A critical review*. Thousand Oaks, CA: Sage.

Capitanio, J. P., & Leger, D. W. (1979). Evolutionary scales lack utility: A reply to Yarczower and Hazlett. *Psychological Bulletin, 86*, 876–879.

Caplan, A. L. (1978). *The sociobiology debate*. New York: Harper & Row.

Carmichael, L. (Ed.). (1946). *Manual of child psychology*. New York: Wiley.

Carmichael, L. (Ed.). (1954). *Manual of child psychology* (2nd ed.). New York: Wiley.

Carmichael, S. L., & Shaw, G. M. (2000). Maternal life event stress and congenital anomalies. *Epidemiology, 11*(1), 30–35.

Carpendale, J. I. M., & Lewis, C. (2015). The development of social understanding. In L. S. Liben & U. Mueller (Eds.), *Handbook of child psychology and developmental science: Vol. 2 Cognitive processes* (7th ed., pp. 381–424). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Carstensen, L. L., Hanson, K. A., & Freund, A. (1995). Selection and compensation in adulthood. In R. A. Dixon & L. Bäckman (Eds.), *Compensating for psychological deficits and declines: Managing losses and promoting gains* (pp. 107–126). Hillsdale, NJ: Lawrence Erlbaum Associates.

Case, R. (1984). The process of stage transition: A neo-Piagetian development. In R. J. Sternberg (Ed.), *Mechanisms of change* (pp. 19–44). San Francisco: W. H. Freeman.

Case, R. (1992a). Neo-Piagetian theories of intellectual development. In H. Beilin & P. B. Pufall (Eds.), *Piaget's theory: Prospects and possibilities* (pp. 61–107). Hillsdale, NJ: Lawrence Erlbaum.

Case, R. (1992b). *The mind's staircase: Exploring the conceptual underpinnings of children's thought and knowledge*. Hillsdale, NJ: Lawrence Erlbaum.

Catalano, R. F., Fagan, A. A., Gavin, L. E., Greenberg, M. T., Irwin, C. E., Jr., Ross, D. A., & Shek, D. T. (2012). Worldwide application of prevention science in adolescent health. *Lancet, 379* (9826), 1653–1664.

Catalano, R. P., Hawkins, J. D., Berglund, M. L., Pollard, J. A., & Arthur, M. W. (2002). Prevention science and positive youth development: Competitive or cooperative frameworks? *Journal of Adolescent Health*, *31*, 230–239.

Cattell, R. B. (1957). *Personality and motivation: Structure and measurement*. New York: World.

Cattell, R. B. (1966). Psychological theory and scientific method. In R. B. Cattell (Ed.), *Handbook of multivariate experimental psychology* (pp. 1–18). Chicago: Rand McNally.

Cauffman, E., Shulman, E., Bechtold, J., & Steinberg, L. (2015). Children and the law. In M. H. Bornstein and T. Leventhal (Eds.), *Handbook of child psychology and developmental science: Vol. 4 Ecological settings and processes* (7th ed., pp. 616–653). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Cavalli-Sforza, L. L., & Cavalli-Sforza, F. (1995). *The great human diasporas: The history of diversity and evolution* (first published in 1993 in Italy by Arnoldo Mondadori Editore Spa, Milano, as *Chi siamo: La storia della diversita umana*, translated into English by Sarah Horne, English Postscript 1995 by Luigi Luca Cavalli-Sforza). Menlo Park, CA: Addison-Wesley.

Cernovsky, Z. Z. (1992). J. P. Rushton on negroids and caucasoids: Statistical concepts and disconfirmatory evidence. *The International Journal of Dynamic Assessment and Instruction, 2*(2), 55–67.

Cernovsky, Z. Z. (1995). On the similarities of American blacks and whites. *Journal of Black Studies, 25*(6), 672–679.

Cernovsky, Z. Z. (1997). Statistical methods and behavioral similarities of blacks and whites. Paper presented at the 58th Annual Convention of the Canadian Psychological Association, Toronto, Canada.

Cernovsky, Z. Z. & Litman, L. C. (1993). Re-analyses of J. P. Rushton's crime data. *Canadian Journal of Criminology, 35*(1), 31–36.

Cervone, D., Shoda, Y., & Downey, G. (2007). Construing persons in context: On building a science of the individual. In Y. Shoda, D. Cervone, & G. Downey (Eds.), *Persons in context: Building a science of the individual* (pp. 3–15). New York: Guilford.

Chabris, C. F., Hebert, B. M., Benjamin, D. J., Beauchamp, J., Cesarini, D., van der Loos, M., Johannesson, M., Magnusson, P. K., Lichtenstein, P., Atwood, C. S., Freese, J., Hauser, T. S., Hauser, R. M., Christakis, N., & Laibson, D. (2012). Most reported genetic associations with general intelligence are false positives. *Psychological Science, 23*(11), 1314–1323.

Chandler, M. J., Lalonde, C. E., Sokol, B. W., & Hallett, D. (2003). Personal persistence, identity, and suicide: A study of Native and non-Native North American adolescents. *Monographs of the Society for Research in Child Development, 68*(2), vii–viii, 1–130, Discussion, 131–138.

Chapman, M. (1988a). *Constructive evolution: Origins and development of Piaget's thought*. Cambridge, UK: Cambridge University Press.

Chapman, M. (1988b). Contextuality and directionality of cognitive development. *Human Development, 31*, 92–106.

Charney, E. (2012). Behavior genetics and post genomics. *Behavioral and Brain Sciences, 35*, 331–410.

Charney, E. (2013). Cytoplastic inheritance redux. In R. M. Lerner & J. B. Benson (Eds.), *Advances in child development and behavior: Embodiment and epigenesis: Theoretical and methodological issues in understanding the role of biology within the relational developmental system. Part A. Philosophical, theoretical, and biological dimensions* (pp. 225–255). London: Elsevier.

Charney, E. (2016). Genes, behavior, and behavior genetics. *WIREs Cogn Sci* 2016. doi: 10.1002/wcs.1405.

Chen, H. (2010). The bottom-up approach to integrative validity: A new perspective for program evaluation. *Evaluation and Program Planning, 33*, 205–214.

Chess, S., & Thomas, A. (1984). *The origins and evolution of behavior disorders: Infancy to early adult life*. New York: Brunner/Mazel.

Chess, S., & Thomas, A. (1996). *Temperament: Theory and practice*. New York: Brunner/Mazel.

Chess, S., & Thomas, A. (1999). *Goodness of fit: Clinical applications from infancy through adult life*. Philadelphia, PA: Brunner/Mazel.

Chibucos, T., & Lerner, R. M. (Eds.). (1999). *Serving children and families through community–university partnerships: Success stories*. Norwell, MA: Kluwer.

Chomsky, N. A. (1965). *Aspects of the theory of syntax*. Cambridge, MA: MIT Press.

Chomsky, N. A. (1966). *Cartesian linguistics*. New York: Harper & Row.

Chorover, S. L. (1979). *From genesis to genocide*. Cambridge, MA: MIT Press.

Chow, K. L., & Stewart, D. L. (1972). Reversal of structural and functional effects of long-term visual deprivation in the cat. *Experimental Neurology, 34*, 409–433.

Clark, G. H. (1957). *Thales to Dewey*. Boston: Houghton Mifflin.

Clarke, A. M., & Clarke, A. D. B. (Eds.). (1976). *Early experience: Myth and evidence*. New York: Free Press.

Clausen, J. A. (1972). The life-course of individuals. In M. W. Riley, M. Johnson, & A. Foner (Eds.), *Aging and society: Vol. 3 A sociology of age stratification*. New York: Russell Sage Foundation.

Cohen, S. (1976). Transposable genetic elements and plasmid evolution. *Nature, 263*, 731–738.

Colby, A., & Damon, W. (1992). *Some do care: Contemporary lives of moral commitment*. New York: Free Press.

Cole, M. (1985). The zone of proximal development: Where culture and cognition create each other. In J. W. Wertsch (Ed.), *Culture, communication, and cognition* (pp. 146–162). Cambridge, UK: Cambridge University Press.

Cole, M. (2006). Cultural and cognitive development in phylogenetic, historical, and ontogenetic perspective. In D. A. Kuhn & R. S. Siegler (Eds.), *Handbook of child psychology: Vol. 2 Cognition, perception, and language* (6th ed., pp. 636–683). Editors-in-chief: W. Damon & R. M. Lerner. Hoboken, NJ: Wiley.

Cole, S. W. (2014). Human social genomics. *PLOS Genetics*, *10*(8), 1–7.

Collins, L. M. (2006). Analysis of longitudinal data: The integration of theoretical model, temporal design, and statistical model. *Annual Review of Psychology, 57*, 505–528.

Collins, W. A. (1982). *The concept of development: The Minnesota symposia on child psychology* (Vol. 15). Hillsdale, NJ: Erlbaum.

Collins, L. M., & Graham J. W. (2002). The effect of the timing and temporal spacing of observations in longitudinal studies of tobacco and other drug use: Temporal design considerations. *Drug and Alcohol Dependence, 68*, S85–S96.

Collins, W. A., Maccoby, E. E., Steinberg, L., Hetherington, E. M., & Bornstein, M. H. (2000). Contemporary research on parenting: The case of nature and nurture. *American Psychologist, 55*, 218–232.

Colombo, J. (1982). The critical period concept: Research, methodology and theoretical issues. *Psychological Bulletin, 91*, 260–275.

Connell, J. P., Kubisch, A. C., Schorr, L. B., & Weiss, C. H. (1995). *New approaches to evaluating community initiatives: Concepts, methods, and contexts*. Washington, DC: The Aspen Institute.

Connolly, K. (1972). Learning and the concept of critical periods in infancy. *Developmental Medicine and Child Neurology, 14*, 705–714.

Conway, J. (1958). The inheritance of intelligence and its social implications. *British Journal of Statistical Psychology, 11*, 171–190.

Cooper, C. R. (1980). Development of collaborative problem solving among preschool children. *Developmental Psychology, 16*, 433–440.

Corneal, S. E., & Nesselroade, J. R. (1991). Multivariate, replicated, single-subject, repeated measures design: Studying change in the adolescent. In R. M. Lerner, A. C. Petersen, & J. Brooks-Gunn (Eds.), *Encyclopedia of adolescence* (Vol. 2, pp. 681–687). New York: Garland.

Costa, P. T., Jr., & McCrae, R. R. (1980). Still stable after all these years: Personality as a key to some issues in adulthood and old age. In P. B. Baltes & O. G. Brim, Jr. (Eds.), *Life span development and behavior* (Vol. 3, pp. 65–102). New York: Academic Press.

Costa, P. T., Jr., & McCrae, R. R. (2006). Age changes in personality and their origins: Comment on Roberts, Walton, and Viechtbauer (2006). *Psychological Bulletin*, *132*, 28–30.

Costa, P. T., Jr., McCrae, R. R., & Siegler, I. C. (1999). Continuity and change over the adult life cycle: Personality and personality disorders. In C. R. Cloninger (Ed.), *Personality and psychopathology* (pp. 129–154). Washington, DC: American Psychiatric Press.

Côté, J. E. (2009). Identity formation and self-development in adolescence. In R. M. Lerner & L. Steinberg (Eds.), *Handbook of adolescent psychology: Vol. 1 Individual bases of adolescent development* (3rd ed., pp. 266–304). Hoboken, NJ: Wiley.

Cox, V. (1974). A prize for the Goose Father. *Human Behavior, 3*, 17–22.

Crawford, M. L., Blake, R., Cool, S. J., & von Noorden, G. K. (1975). Physiological consequences of unilateral and bilateral eye closure in macaque monkeys: Some further observations. *Brain Research, 84*, 150–154.

Cronbach, L. J. (1957). The two disciplines of psychology. *American Psychologist, 12*, 671–684.

Cronbach, L. J. (1975). Beyond the two disciplines of scientific psychology. *American Psychologist, 30*, 116–127.

Cronbach, L. J., & Meehl, P. E. (1955). Construct validity in psychological tests. *Psychological Bulletin, 52*, 281–302.

Crosnoe, R., & Benner, A. D. (2015). Children at school. In M. H. Bornstein & T. Leventhal (Eds.), *Handbook of child psychology and developmental science: Vol. 4 Ecological settings and processes* (7th ed., pp. 268–204). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Cumming, E., & Henry, W. E. (1961). *Growing old: The process of disengagement*. New York: Basic Books.

Cummings, E. M., & Valentino, K. (2015). Developmental psychopathology. In W. F. Overton & P. C. Molenaar (Eds.), *Handbook of child psychology and developmental science: Vol. 1 Theory and method* (7th ed., pp. 566–606). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Cunha, F., & Heckman, J. (2014). Estimating the technology of cognitive and noncognitive skill formation: The linear case. In P. C. M. Molenaar, R. M. Lerner, & K. Newell (Eds.), *Handbook of developmental systems theory and methodology* (pp. 221–269). New York: Guilford.

Cunha, F., Heckman, J. J., & Schennach, S. M. (2010). Estimating the technology of cognitive and noncognitive skill formation. *Econometrica, 78*(3), 883–931.

Cunningham, M., & Spencer, M. B. (1996). The Black male experiences measure. In R. L. Jones (Ed.), *Handbook of tests and measurements for black populations* (pp. 301–307). Hampton, VA: Cobb & Henry.

Curie, E. (1937). *Madame Curie*. Vienna: Baumann-Fischer Verlag.

Curtiss, S. (1977). *Genie: A psycholinguistic study of a modern day "wild child."* New York: Academic Press.

Curtiss, S., Fromkin, V., Rigler, M., Rigler, D., & Krashen, S. (1975). An update on the linguistic development of Genie. In D. Dato (Ed.), *Developmental psycholinguistics: Theory and applications* (pp. 145–157). Washington, DC: Georgetown University Press.

Cynader, M., Berman, N., & Hein, A. (1976). Recovery of function in cat visual cortex following prolonged deprivation. *Experimental Brain Research, 25*, 139–156.

Damasio, A. R., & Damasio, H. (1996). Making images and creating subjectivity. In R. R. Llinas & P. S. Churchland (Eds.), *The mind–brain continuum: Sensory processes* (pp. 19–27). Cambridge, MA: MIT Press.

Damon, W. (1984). Self-understanding and moral development from childhood to adolescence. In J. Gewirtz & W. Kurtines (Eds.), *Morality, moral behavior and moral development* (pp. 109–127). New York: Wiley.

Damon, W. (1990). *The moral child: Nurturing children's natural moral growth*. New York: Free Press.

Damon, W. (Ed.). (1998). *Handbook of child psychology* (5th ed.). New York: Wiley.

Damon, W. (2004). What is positive youth development? *Annals of the American Academy of Political and Social Science, 591*, 13–24.

Damon, W. (2006). Preface. In W. Damon & R. M. Lerner (Eds.), *Handbook of child psychology* (6th ed., pp. xi–xix). Hoboken, NJ: Wiley.

Damon, W. (2008). *The path to purpose: Helping our children find their calling in life.* New York: Simon & Schuster.

Damon, W. (2015). Foreword to the *Handbook of child psychology and developmental science*. In R. M. Lerner (Editor-in-chief), *Handbook of child psychology and developmental science* (7th ed., pp. vii–xiii). Hoboken, NJ: Wiley.

Damon, W., & Colby, A. (2015). *The power of ideals: The real story of moral choice*. New York: Oxford University Press.

Damon, W., & Hart, D. (1988). *Self-understanding in childhood and adolescence*. New York: Cambridge University Press.

Damon, W. (Editor-in-chief) & Lerner, R. M. (Vol. Ed). (1998). *Handbook of child psychology: Vol. 1 Theoretical models of human development* (5th ed.). New York: Wiley.

Damon, W., & Lerner, R. M. (Editors-in-chief). (2006). *Handbook of child psychology: Vols. 1–4* (6th ed.). Hoboken, NJ: Wiley.

Damon, W., Menon, J. L., & Bronk, K. C. (2003). The development of purpose during adolescence. *Applied Developmental Science, 7*(3), 119–128.

D'Andrade, R. G. (1984). Cultural meaning systems. In R. A. Shweder & R. A. Le Vine (Eds.), *Culture theory: Essays on mind, self, and emotion* (pp. 88–119). Cambridge, UK: Cambridge University Press.

Dannefer, D. (1984). Adult developmental and socialization theory: A paradigmatic reappraisal. *American Sociological Review, 49*, 100–116.

Darwin, C. (1859). *The origin of species by means of natural selection or the preservation of favoured races in the struggle for life*. London: J. Murray.

Darwin, C. (1872). *The expression of emotions in man and animals*. London: J. Murray.

Datan, N., & Ginsberg, L. H. (Eds.). (1975). *Life-span developmental psychology: Normative life crises.* New York: Academic Press.

Datan, N., & Reese, H. W. (Eds.). (1977). *Life-span developmental psychology: Dialectical perspectives on experimental research*. New York: Academic Press.

Davis, A. (1944). Socialization and the adolescent personality. *Forty-third yearbook of the National Society for the Study of Education* (Vol. 43, Part 1). Chicago: University of Chicago Press.

Davison, M. L., King, P. M., Kitchener, K. S., & Parker, C. A. (1980). The stage sequence concept in cognitive and social development. *Developmental Psychology, 14*, 137–146.

Dawidowicz, L. S. (1975). *The war against the Jews, 1933–1945*. New York: Holt, Rinehart & Winston.

Dawkins, R. (1976). *The selfish gene*. New York: Oxford University Press.

Dawkins, R. (1982). *The extended phenotype*. London: Freeman.

de Beer, G. R. (1930). *Embryology and evolution*. Oxford: Clarendon Press.

de Beer, G. R. (1958). *Embryos and ancestors* (3rd ed.). Oxford: Clarendon Press.

de Beer, G. R. (1959). Paedomorphosis. *Proceedings of the XV International Congress of Zoology, 15*, 927–930.

DeFries, J. C. (1967). Quantitative genetics and behavior: Overview and perspective. In J. Hirsch (Ed.), *Behavior-genetic analysis*. New York: McGraw-Hill.

Deichmann, U. (1996). *Biologists under Hitler*. Cambridge, MA: Harvard University Press.

Del Giudice, M., & Ellis, B. J. (2016). Evolutionary foundations of developmental psychopathology. In D. Cicchetti (Ed.), *Developmental psychopathology: Vol. 12 Developmental neuroscienc*e (3rd ed., pp. 1–58). New York: Wiley.

Demo, D. H., Allen, K. R., & Fine, M. A. (2000). *Handbook of family diversity*. New York: Oxford University Press.

de Santillana, G., & von Dechend, H. (1977). *Hamlet's mill: An essay investigating the origins of human knowledge and its transmission through myth*. Boston: Godine.

Descartes, R. (1969). *The philosophical works of Descartes* (E. S. Haldane & G. R. T. Ross, Trans.). Cambridge, UK: Cambridge University Press.

DeSouza, L. M. (2017). Success in a career-technical education program: The role of individual strengths and contextual assets. Unpublished dissertation. Medford, MA: Tufts University.

Dewey, J. (1916). *Democracy and education: An introduction to the philosophy of education*. New York: Macmillan.

Dewey, J., & Bentley, A. F. (1948). *Knowing and the known*. Boston: Beacon Press.

Diamond, L. M., Bonner, S. B., & Dickenson, J. (2015). The development of sexuality. In M. Lamb (Ed.), *Handbook of child psychology and developmental science: Vol. 3 Socioemotional processes* (7th ed., pp. 888–931). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Dick, A. S., & Müller, U. (Eds.). (2017). *Advancing developmental science: Philosophy, theory, and method*. New York: Routledge.

Dickens, W. T., & Cohen, J. L. (2004). Instinct and choice: A framework for analysis. In C. Garcia Coll, E. L. Bearer, & R. M. Lerner (Eds.), *Nature and nurture: The complex interplay of genetic and environmental influences on human behavior and development* (pp. 145–170). Mahwah, NJ: Erlbaum.

Dixon, R. A. (2000). The concept of metamemory: Cognitive, developmental, and clinical issues. In G. E. Berrios & J. R. Hodges (Eds.), *Memory disorders in psychiatric practice* (pp. 47–57). New York: Cambridge University Press.

Dixon, R. A., & Lerner, R. M., (1988). A history of systems in developmental psychology. In M. H. Bornstein & M. E. Lamb (Eds.), *Developmental psychology: An advanced textbook* (2nd ed., pp. 3–50). Hillsdale, NJ: Erlbaum.

Dixon, R. A., & Lerner, R. M. (1999). History and systems in developmental psychology. In M. H. Bornstein & M. E. Lamb (Eds.), *Developmental psychology: An advanced textbook* (4th ed., pp 3–45). Mahwah, NJ: Erlbaum.

Dixon, R. A., Lerner, R. M., & Hultsch, D. F. (1991a). Maneuvering among models of developmental psychology. In P. van Geert & L. P. Mos (Eds.), *Annals of theoretical psychology* (Vol. 7, pp. 357–368). New York: Plenum.

Dixon, R. A., Lerner, R. M., & Hultsch, D. F. (1991b). The concept of development in the study of individual and social change. In P. van Geert & L. P. Mos (Eds.), *Annals of theoretical psychology* (Vol. 7, pp. 279–323). New York: Plenum.

Dixon, R. A., & Nesselroade, J. R. (1983). Pluralism and correlation analysis in developmental psychology: Historical commonalities. In R. M. Lerner (Ed.), *Developmental psychology: Historical and philosophical perspectives* (pp. 113–145). Hillsdale, NJ: Erlbaum.

Dobzhansky, T., Ayala, F. J., Stebbings, G. L., & Valentine, J. W. (1977). *Evolution.* San Francisco: Freeman.

Dodge, K. A., & Haskins, R. (2015). Children and government. In M. H. Bornstein and T. Leventhal (Eds.), *Handbook of child psychology and developmental science: Vol. 4 Ecological settings and processes* (7th ed., pp. 654–703). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Doerr, A. (2009). Three generations of imbeciles are enough. *Genomics Law Report*. Retrieved from www.genomicslawreport.com/index.php/2009/06/25/three-generations-of-imbeciles-are-enough/.

Doherty, T. S., & Roth, T. L. (2016). Insight from animal models of environmentally driven epigenetic changes in the developing and adult brain. *Development and Psychopathology*, *28*, 1229–1243.

Dollard, J., Doob, L. W., Miller, N. E., Mowrer, O. H., & Sears, R. R. (1939). *Frustration and aggression*. New Haven: Yale University Press.

Dollard, J., & Miller, N. E. (1950). *Personality and psychotherapy*. New York: McGraw-Hill.

Doolittle, W. F., & Sapienza, C. (1980). Selfish genes, the phenotype paradigm, and genome evolution. *Nature, 284*, 601–603.

Dorfman, D. D. (1978). The Cyril Burt question: New findings. *Science, 201*, 1177–1186.

Dorfman, D. D. (1980). Intelligence for beginners? (Review of Hans J. Eysenck's *The Structure and Measurement of Intelligence*). *Nature, 284*, 645.

Douvan, J. D., & Adelson, J. (1966). *The adolescent experience*. New York: Wiley.

Draper, P., & Harpending, H. (1982). Father absence and reproductive strategy: An evolutionary perspective. *Journal of Anthropological Research, 38*, 255–273.

Draper, P., & Harpending, H. (1988). A sociobiological perspective on the development of human reproductive strategies. In K. B. MacDonald (Ed.), *Sociobiological perspectives on human development* (pp. 340–372). New York: Springer-Verlag.

Dunbar, R. I. M. (1987). Sociobiological explanations and the evolution of ethnocentrism. In V. Reynolds, V. Falger, & I. Vine (Eds.), *The sociobiology of ethnocentrism* (pp. 48–59). London: Croom Helm.

Duncan, C. P. (1982). Book reviews: The intelligence controversy. *American Journal of Psychology, 95*, 346–349.

Duncan, G. J., Magnuson, K., & Votruba-Drzal, E. (2015). Children and socioeconomic status. In M. H. Bornstein and T. Leventhal (Eds.), *Handbook of child psychology and developmental science: Vol. 4 Ecological settings and processes* (7th ed., pp. 534–573). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Dunn, L. C. (1965). *A short history of genetics*. New York: McGraw-Hill.

Dupré, J. (2003). *Darwin's legacy: What evolution means today*. Oxford: Oxford University Press.

Dweck, C. S. (2006). *Mindset*. New York: Random House.

Dweck, C. S. (2016). *Mindset: The new psychology of success*. New York: Ballantine Books.

Dylan, B. (1964). My back pages. *Another side of Bob Dylan*. New York: Columbia Records.

Eacker, J. N. (1972). On some elementary philosophical problems of psychology. *American Psychologist, 27*, 553–565.

Eaves, L. J. (1975). Testing models for variation in intelligence. *Heredity, 34*, 132–136.

Eaves, L. J. (1977). Inferring the causes of human variation. *Journal of the Royal Statistical Society, Series A, 140*, 324–348.

Eccles, J. S. (1991). Academic achievement. In R. M. Lerner, A. C. Petersen, & J. Brooks-Gunn (Eds.), *Encyclopedia of adolescence* (Vol. 1, pp. 1–9). New York: Garland.

Eccles, J. S. (2004). Schools, academic motivation, and stage–environment fit. In R. M. Lerner & L. Steinberg (Eds.), *Handbook of adolescent psychology* (Vol. 2, pp. 125–153). Hoboken, NJ: Wiley.

Eccles, J. S., & Gootman, J. A. (Eds.). (2002). *Community programs to promote youth development/committee on community-level programs for youth*. Washington, DC: National Academy Press.

Eccles, J. S., & Harold, R. D. (1996). Family involvement in children's and adolescents' schooling. In A. Booth & J. F. Dunn (Eds.), *Family–school links: How do they affect educational outcomes*? (pp. 3–34). Mahwah, NJ: Erlbaum.

Eccles, J. S., Lord, S., & Buchanan, C. M. (1996). School transitions in early adolescence: What are we doing to your young people? In J. A., Graber, J. Brooks-Gunn, & A. C. Petersen (Eds.), *Transitions through adolescence* (pp. 251–284). Mahwah, NJ: Erlbaum.

Eccles, J. S., & Midgley, C. (1989). Stage–environment fit: Developmentally appropriate classrooms for young adolescents. In C. Ames & R. Ames (Eds.), *Research on motivation in education. Goals and cognitions* (Vol. 3, pp. 139–186). New York: Academic Press.

Eccles, J. S., Midgley, C., Wigfield, A., Buchanan, C. M., Reuman, D., Flanagan, C., & MacIver D. (1993). Development during adolescence: The impact of stage–environment fit on young adolescents' experiences in schools and in families. *American Psychologist, 48*, 90–101.

Eckensberger, L. H. (1989). A bridge between theory and practice, between general laws and contexts? *Psychology and Developing Societies, 1*, 21–35.

Edelman, G. M. (1987). *Neural Darwinism: The theory of neuronal group selection*. New York: Basic Books.

Edelman, G. M. (1988). *Topobiology: An introduction to molecular biology.* New York: Basic Books.

Edelman, G. M. (1992). *Bright air, brilliant fire: On the matter of the mind*. New York: Basic Books.

Eichorn, D. H., Clausen, J. A., Haan, N., Honzik, M. P., & Mussen, P. H. (1981). *Present and past in middle life*. New York: Academic Press.

Eisenberg, L. (1972). The *human* nature of human nature. *Science, 176*, 123–128.

Elder, G. H., Jr. (1974). *Children of the Great Depression*. Chicago: University of Chicago Press.

Elder, G. H., Jr. (1975). Age differentiation and the life course. In A. Inkeles, J. Coleman, & N. Smelser (Eds.), *Annual review of sociology.* Palo Alto, CA: Annual Review.

Elder, G. H., Jr. (1977). Family history and the life course. *Journal of Family History, 2*, 279–304.

Elder, G. H., Jr. (1979). Historical change in life patterns and personality. In P. B. Baltes & O. G. Brim, Jr. (Eds.), *Life-span development and behavior* (Vol. 2, pp. 117–159). New York: Academic Press.

Elder, G. H., Jr. (1980). Adolescence in historical perspective. In J. Adelson (Ed.), *Handbook of adolescent psychology* (pp. 3–46). New York: Wiley.

Elder, G. H., Jr. (1998). The life course and human development. In W. Damon (Series Ed.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (5th ed., pp. 939–991). New York: Wiley.

Elder, G. H., Jr. (1999). *Children of the Great Depression: Social change in life experience* (25th anniversary ed.). Boulder, CO: Westview.

Elder, G. H., Jr., Modell, J., & Parke, R. D. (1993). Studying children in a changing world. In G. H. Elder, Jr., J. Modell, & R. D. Parke (Eds.), *Children in time and place: Developmental and historical insights* (pp. 3–21). New York: Cambridge University Press.

Elder, G. H., Jr., & Shanahan, M. J. (2006). The life course and human development. In R. M. Lerner (Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (6th ed., pp. 665–715). Editors-in-chief: W. Damon & R. M. Lerner. Hoboken, NJ: Wiley.

Elder, G. H., Jr., Shanahan, M. J., & Jennings, J. A. (2015). Human development in time and place. In M. H. Bornstein and T. Leventhal (eds), *Handbook of child psychology and developmental science. Vol. 4 Ecological settings and processes in developmental systems* (7th ed., pp. 6–54). Hoboken, NJ: Wiley.

Ellis, B. J., Schlomer, G. L., Tilley, E. H., & Butler, E. A. (2012). Impact of fathers on risky sexual behavior in daughters: A genetically and environmentally controlled sibling study. *Development and Psychopathology, 24*, 317–332.

Emmerich, W. (1968). Personality development and concepts of structure. *Child Development 39*, 671–690.

Entringer, S., Buss, C., Kumsta, R., Hellhammer, D. H., Wadhwa, P. D., & Wüst, S. (2009). Prenatal psychosocial stress is associated with subsequent working memory performance in young women. *Behavioral Neuroscience, 123*(4), 886–893.

Entringer, S., Buss, C., & Wadhwa, P. D. (2010). Prenatal stress and developmental programming of human health and disease risk: Concepts and integration of empirical findings. *Current Opinion in Endocrinology, Diabetes and Obesity, 17*(6), 507–516.

Entringer, S., Kumsta, R., Nelson, E. L., Hellhammer, D. H., Wadhwa, P. D., & Wüst, S. (2008). Influence of prenatal psychosocial stress exposure on immune function in adult women. *Developmental Psychobiology, 50*(6), 579–587.

Entringer, S., Wüst, S., Kumsta, R., Layes, I. M., Nelson, E. L., Hellhammer, D. H., & Wadhwa, P. D. (2008). Prenatal psychosocial stress exposure is associated with insulin resistance in young adults. *American Journal of Obstetrics and Gynecology, 199*(5), 498.e1–7.

Erentaitė, R., & Raižienė, S. (2015). Lithuanian version of measure of positive youth development based on the Five Cs model. *European Journal of Developmental Psychology, 12*, 701–717.

Erikson, E. H. (1950). *Childhood and society*. New York: Norton.

Erikson, E. H. (1959). Identity and the life cycle. *Psychological Issues, 1*, 50–100.

Erikson, E. (1963). *Childhood and society* (2nd ed.). New York: Norton.

Erikson, E. (1964). Inner and outer space: Reflections on womanhood. In R. J. Lifton (Ed.), *The woman in America* (pp. 1–26). Boston: Beacon Press.

Erikson, E. (1968). *Identity, youth, and crisis*. New York: Norton.

Erlenmeyer-Kimling, L., & Jarvik, L. F. (1963). Genetics and intelligence. *Science, 142*, 1477–1479.

Esposito, N. J. (1975). Review of discrimination shift learning in young children. *Psychological Bulletin, 82*, 432–455.

Ettekal, A. V., Callina, K. S., & Lerner, R. M. (2015). The promotion of character through youth development programs: A view of the issues. *Journal of Youth Development, 10*(3), 6–13.

Ettekal, A. V., Konowitz, L. S., Agans, J. P., Syer, T., & Lerner, R. M. (2017). Researcher–practitioner collaborations: Applying developmental science to understand sport participation and positive youth development. *The Journal of Community Engagement in Higher Education, 9*(2), 29–45.

Evans, R. I. (1974). Interview with Konrad Lorenz. *Psychology Today, 8*, 26ff.

Eysenck, H. J. (1979). *The structure and measurement of intelligence*. Berlin: Springer-Verlag.

Eysenck, H. J. (1980). Jensen and bias: An exchange. *The New York Review of Books, 27*, 52.

Eysenck, H. J., & Kamin, L. (1981a). *The intelligence controversy*. New York: Wiley.

Eysenck, H. J., & Kamin, L. (1981b). *Intelligence: The battle for the mind*. New York: Macmillan.

Falconer, D. S. (1960). *Quantitative genetics*. Edinburgh: Oliver & Boyd.

Farber, S. L. (1981). *Identical twins reared apart: A reanalysis.* New York: Basic Books.

Featherman, D. L. (1980). Schooling and occupational careers: Constancy and change in worldly success. In O. G. Brim, Jr., & J. Kagan (Eds.), *Constancy and change in human development* (pp. 675–738). Cambridge, MA: Harvard University Press.

Featherman, D. L. (1983). Life-span perspectives in social science research. In P. B. Baltes & O. G. Brim, Jr. (Eds.), *Life-span development and behavior* (Vol. 5, pp. 1–57). New York: Academic Press.

Featherman, D. L. (1985). Individual development and aging as a population process. In J. R. Nesselroade & A. von Eye (Eds.), *Individual development and social change: Explanatory analysis* (pp. 213–241). New York: Academic Press.

Featherman, D. L., & Lerner, R. M. (1985). Ontogenesis and sociogenesis: Problematics for theory about development across the lifespan. *American Sociological Review, 50*, 659–676.

Featherman, D. L., Lerner, R. M., & Perlmutter, M. (Eds.). (1994). *Life-span development and behavior*. Hillsdale, NJ: Erlbaum.

Feldman, B. (1995). The search for identity in late adolescence. In M. Sidoli & G. Bovensiepen (Eds.), *Incest fantasies and self-destructive acts: Jungian and Post-Jungian psychotherapy in adolescence* (pp. 153–166). New Brunswick, NJ: Transaction Books.

Feldman, D. H. (1980a). *Beyond universals in cognitive development*. Norwood, NJ: Ablex Publishing Corp.

Feldman, D. H. (1980b). Stage and sequence: Getting to the next level. *The Genetic Epistemologist, 9*, 1–6.

Feldman, D. H. (1994). *Beyond universals in cognitive development* (2nd ed.). Norwood, NJ: Ablex.

Feldman, D. H. (1995). Learning and developmental in nonuniversal theory. *Human Development, 38*, 315–321.

Feldman, D. H. (2000). Piaget's stages: The unfinished symphony. Unpublished manuscript. Medford, MA: Eliot-Pearson Department of Child Development, Tufts University.

Feldman, D. H. (2007). Vertical and horizontal décalage in Piaget's theory. In *CD 151 Intellectual development*, Fall, 2007. Tufts Open Courseware (p. 17). Medford, MA: Tufts University. http://ocw.tufts.edu/Course/53/Coursehome.

Feldman, M. (2014). Echoes of the past: Hereditarianism and a troublesome inheritance. *PLoS Genetics, 10*, e1004817.

Feldman, M., and Laland, K. N. (1996). Gene-culture coevolutionary theory. *Trends in Ecology and Evolution, 11*, 453–457.

Feldman, M., & Lewontin, R. C. (1975). The heritability hang-up. *Science, 190*, 1163–1168.

Feldman, M., & Lewontin, R. C. (1976). Letters: Heritability of IQ. *Science, 194*, 12–14.

Ferris, K. A., Ettekal, A. V., Agans, J. P., & Burkhard, B. M. (2015). Character development through youth sport: High school coaches' perspectives on a character-based education program. *Journal of Youth Development, 10*, 127–140.

Fetterman, D. M., Kaftarian, S. J., & Wandersman, A. (Eds.). (1996). *Empowerment evaluation: Knowledge and tools for self-assessment and accountability*. Thousand Oaks, CA: Sage.

Fischer, K. W. (1980). A theory of cognitive development: The control and construction of hierarchies of skills. *Psychological Review, 87*, 477–531.

Fischer, K. W. (1987). Relations between brain and cognitive development. *Child Development, 57*, 623–632.

Fischer, K. W., & Bidell, T. R. (1991). Constraining nativist inferences about cognitive capacities. In S. Carey & R. Gelman (Eds.), *The epigenesis of mind: Essays on biology and knowledge* (pp. 199–235). Hillsdale, NJ: Erlbaum.

Fischer, K. W., & Bidell, T. (1998). Dynamic development of psychological structures in action and thought. In W. Damon (Series Ed.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (5th ed., pp. 467–561). New York: Wiley.

Fischer, K. W., & Bidell, T. R. (2006). Dynamic development of action and thought. In R. M. Lerner (Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (6th ed., pp. 313–399). Editors-in-chief: W. Damon & R. M. Lerner. Hoboken, NJ: Wiley.

Fischer, K. W., & Pipp, S. L. (1984). Process of cognitive development: Optimal level and skill acquisition. In R. J. Sternberg (Ed.), *Mechanisms of cognitive development* (pp. 45–81). New York: W. H. Freeman.

Fischer, K. W., & Rose, S. P. (1994). Dynamic development of coordination of components in brain and behavior: A framework for theory and research. In G. Dawson & K. W. Fischer (Eds.), *Human behavior and the developing brain* (pp. 3–66). New York: Guilford Press.

Fisher, C. B., & Lerner, R. M. (Eds.). (1994). *Applied developmental psychology*. New York: McGraw-Hill.

Fisher, C. B., & Lerner, R. M. (2013). Promoting positive development through social justice: An introduction to a new ongoing section of *Applied Developmental Science*. *Applied Developmental Science, 17*(2), 57–59.

Fisher, C. B., Busch, N. A., Brown, J. L., & Jopp, D. S. (2013). Applied developmental science: Contributions and challenges for the 21st century. In R. M. Lerner, M. A. Easterbrooks, & J. Mistry (Eds.), *Handbook of psychology*: *Vol. 6 Developmental psychology* (2nd ed., pp. 516–546). Editor-in-chief: I. B. Weiner. Hoboken, NJ: Wiley.

Fisher, C. B., Murray, J. P., Dill, J. R., Hagen, J. W., Hogan, M. J., Lerner, R. M., Rebok, G. W., Sigel, I., Sostek, A. M., Smyer, M. A., Spencer, M. B., & Wilcox, B. (1993). The national conference on graduate education in the applications of developmental science across the life span. *Journal of Applied Developmental Psychology, 14*, 1–10.

Fisher, H. E. (1982a). Of human bonding. *The Sciences, 22*, 18–23, 31.

Fisher, H. E. (1982b). Is it sex? Helen E. Fisher replies. *The Sciences, 22*, 2–3.

Flavell, J. H. (1963). *The developmental psychology of Jean Piaget*. New York: Van Nostrand.

Flavell, J. H. (1971). Stage related properties of cognitive development. *Cognitive psychology, 2*, 421–453.

Flavell, J. H. (1972). An analysis of cognitive developmental sequences. *Genetic Psychology Monographs, 86*, 279–350.

Flavell, J. H. (1980). Structures, stages and sequences in cognitive development. Paper presented at the 1980 Minnesota Symposium on Child Psychology, October.

Flavell, J. H., & Wohlwill, J. F. (1969). Formal and functional aspects of cognitive development. In D. Elkind, & J. H. Flavell (eds.), *Studies in cognitive development.* New York: Oxford University Press.

Flay, B. R. (2002). Positive youth development requires comprehensive health promotion programs. *American Journal of Health Behavior*, *26*(6), 407–424.

Flohr, H. (1987). Biological bases of social prejudices. In V. Reynolds, V. Falger, & I. Vine (Eds.), *The sociobiology of ethnocentrism* (pp. 190–207). London: Croom Helm.

Ford, D. H. (1987). *Humans as self-constructing living systems*. Hillsdale, NJ: Erlbaum.

Ford, D. H., & Lerner, R. M. (1992). *Developmental systems theory: An integrative approach*. Newbury Park, CA: Sage.

Fox, M. (2016). On research, art, and transformation: Multi-generational participatory research, critical positive youth development, and structural change. *Qualitative Psychology,* 3, 46–58.

Fraiberg, S. (1977). *Every child's birthright: In defense of mothering*. New York: Basic Books.

Frankel, J. (1976). Controversial areas of research. *Science, 190*, 12.

Fredricks, J. A., Alfeld, C., & Eccles, J. S. (2010). Developing and fostering passion in academic and nonacademic domains. *Gifted Child Quarterly, 54*(1), 18–30.

Freedman, D. G. (1979). *Human sociobiology: A holistic approach*. New York: Free Press.

French, V. (1977). History of the child's influence: Ancient Mediterranean civilizations. In R. Q. Bell & L. V. Harper (Eds.), *Child effects on adults* (pp. 3–29). Hillsdale, NJ: Erlbaum.

Freud, A. (1969). Adolescence as a developmental disturbance. In G. Caplan & S. Lebovici (Eds.), *Adolescence* (pp. 5–10). New York: Basic Books.

Freud, S. (1923). *The ego and the id*. London: Hogarth Press.

Freud, S. (1949). *Outline of psychoanalysis*. New York: Norton.

Freud, S. (1954). *Collected works, standard edition*. London: Hogarth.

Freud, S. (1965). *The psychopathology of everyday life* (Alan Tyson, Trans.). New York: Norton.

Freund, A. M., & Baltes, P. B. (1998). Selection, optimization, and compensation as strategies of life-management: Correlations with subjective indicators of successful aging. *Psychology and Aging, 13*, 531–543.

Freund, A. M., & Baltes, P. B. (2000). The orchestration of selection, optimization, and compensation: An action-theoretical conceptualization of a theory of developmental regulation. In W. J. Perrig & A. Grob (Eds.), *Control of human behavior, mental processes and consciousness: Essays in honor of the 60th birthday of August Flammer* (pp. 35–58). Mahwah, NJ: Lawrence Erlbaum Associates.

Freund, A. M., & Baltes, P. B. (2002). Life-management strategies of selection, optimization, and compensation: Measurement by self-report and construct validity. *Journal of Personality and Social Psychology*, *82*(4), 642–662.

Freund, A. M., & Isaacowitz, D. M. (2014). Beyond age comparisons: A plea for the use of a modified Brunswikian approach to experimental designs in the study of adult development and aging. *Human Development, 56*, 351–371.

Freund, A., Li, K. Z. H., & Baltes, P. B. (1999). The role of selection, optimization, and compensation in successful aging. In J. Brandtstädter & R. M. Lerner (Eds.), *Action and self-development: Theory and research through the life-span* (pp. 401–434). Thousand Oaks, CA: Sage.

Fromkin, V., Krashen, S., Curtiss, S., Rigler, D., & Rigler, M. (1974). The development of language in Genie: A case of linguistic isolation beyond the "critical period." *Brain and Language, 1*, 81–107.

Fuligni, A. J., Eccles, J. S., & Barber, B. (1995). The long-term effects of seventh-grade ability groupings in mathematics. *Journal of Early Adolescence, 15*, 58–89.

Furrow, J. L., King, P. E., & White, K. (2004). Religion and positive youth development: Identity, meaning, and prosocial concerns. *Applied Developmental Science, 8*, 17–26.

Futch Ehrlich, V. A., Deutsch, N. L., Fox, C. V., Johnson, H. E., & Varga, S. M. (2016). Leveraging relational assets for adolescent development: A qualitative investigation of youth–adult 'connection' in positive youth development. *Qualitative Psychology, 3*(1), 59–78.

Gagné, R. M. (1968). Contributions of learning to human development. *Psychological Review, 75*, 177–191.

Gallatin, J. E. (1975). *Adolescence and individuality*. New York: Harper & Row.

Galton, F. (1869/1892). *Hereditary genius: An inquiry into its laws and consequences*. London: Macmillan.

Galton, F. (1975). *English men of science: Their nature and nurture*. New York: D. Appleton.

Ganong, L., Coleman, M., & Russell, L. T. (2015). Children in diverse families. In M. H. Bornstein & T. Leventhal (Eds.), *Handbook of child psychology and developmental science: Vol. 4 Ecological settings and processes* (7th ed., pp. 133–174). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Garbarino, J. (1992). *Children and families in the social environment* (2nd ed.). New York: Aldine de Gruyter.

Garcia-Coll, C. (2013). Minority children: The future majority of the USA. *Social Policy Report, 27*(2), 23.

Garcia Coll, C., Bearer, E., & Lerner, R. M. (Eds.). (2004). *Nature and nurture: The complex interplay of genetic and environmental influences on human behavior and development.* Mahwah, NJ: Lawrence Erlbaum Associates.

Gardner, H., Kornhaber, M., & Wake, W. (1996). *Intelligence: Multiple perspectives*. Fort Worth, TX: Harcourt Brace.

Garstang, W. (1922). The theory of recapitulation: A critical restatement of the biogenetic law. *Journal of the Linnean Society of London. Zoology, 35*, 81–101.

Gartner, L. M., Morton, J., Lawrence, R. A., Naylor, A. J., O'Hare, D., Schanler, R. J., Eidelman, A. I.; American Academy of Pediatrics Section on Breastfeeding (2005). Policy statement: Breastfeeding and the use of human milk. *Pediatrics, 115*(2), 496–506.

Gasman, D. (1971). *The scientific origins of natural socialism: Social Darwinism in Ernst Haeckel and the German Monist League*. New York: Elsevier.

Gauvain, M., & Perez, S. (2015). Cognitive development and culture. In L. S. Liben & U. Mueller (Eds.), *Handbook of child psychology and developmental science: Vol. 2 Cognitive processes* (7th ed., pp. 854–896). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Geldhof, G. J., Bowers, E. P., & Lerner, R. M. (2013). Special section introduction: Thriving in context: Findings from the 4-H Study of Positive Youth Development. *Journal of Youth and Adolescence, 42*(1), 1–5.

Geldhof, G. J., Bowers, E. P., Boyd, M. J., Mueller, M. K., Napolitano, C. M., Schmid, K. L., Lerner, J. V., & Lerner, R. M. (2014a). Creation of short and very short measures of the Five Cs of Positive Youth Development. *Journal of Research on Adolescence*, *24*(1), 163–176.

Geldhof, G. J., Bowers, E. P., Mueller, M. K., Napolitano, C. M., Schmid, K. L., & Lerner, R. M. (2014b). Longitudinal analysis of a very short measure of Positive Youth Development. *Journal of Youth and Adolescence*, *43*(6), 933–949.

Geldhof, G. J., Bowers, E. P., Hull, D. M., Lopes, S. W. M. S., Powell, M. G., & Näslund-Hadley, E. I. (2016). Invariance of the Five Cs of Positive Youth Development across three countries. Unpublished manuscript. Corvallis, OR: Oregon State University.

Geldhof, G. J., Little, T. D., & Colombo, J. (2010). Self-regulation across the life span. In M. E. Lamb & A. M. Freund (Vol. Eds.), *Handbook of life-span development: Vol. 2 Social and emotional development* (pp. 116–157). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley and Sons.

Gengerelli, J. A. (1976). Graduate school reminiscence: Hull and Koffka. *American Psychologist, 31*, 685–688.

Gergen, K. J. (1973). Social psychology and history. *Journal of Personality and Social Psychology, 26*, 309–320.

Gesell, A. L. (1928). *Infancy and human growth*. New York: Macmillan.

Gesell, A. L. (1929). Maturation and infant behavior pattern. *Psychological Review, 36*, 307–319.

Gesell, A. L. (1931). Maturation and the patterning of behavior. In C. Murchison (Ed.), *Handbook of child psychology* (pp. 209–235). Worcester, MA: Clark University Press.

Gesell, A. L. (1934). *An atlas of infant behavior*. New Haven: Yale University Press.

Gesell, A. L. (1939). Charles Darwin and child development. *Scientific Monthly, 49*, 548–553.

Gesell, A. L. (1946). The ontogenesis of infant behavior. In L. Carmichael (Ed.), *Manual of child psychology* (1st ed., pp. 295–331). New York: Wiley.

Gesell, A. L. (1948). *Studies in child development*. Westport, CT: Greenwood.

Gesell, A. L. (1954). The ontogenesis of infant behavior. In L. Carmichael (Ed.), *Manual of child psychology* (2nd ed., pp. 335–373). New York: Wiley.

Gestsdottir, S., Bowers, E. P., von Eye, A., Napolitano, C. M., & Lerner, R. M. (2010). Intentional self regulation in middle adolescence: The emerging role of loss-based selection in Positive Youth Development. *Journal of Youth and Adolescence, 39*(7), 764–782.

Gestsdóttir, S., Geldhof, G. J., Lerner, J. V., & Lerner, R. M. (2017). What drives positive youth development? Assessing intentional self-regulation as a central adolescent asset. *International Journal of Developmental Science*. http://content.iospress.com/articles/international-journal-of-developmental-science/dev160207.

Gestsdóttir, S., & Lerner, R. M. (2008). Positive development in adolescence: The development and role of intentional self regulation. *Human Development, 51*, 202–224.

Gestsdóttir, S., Lewin-Bizan, S., von Eye, A., Lerner, J. V. & Lerner, R. M. (2009). The structure and function of selection, optimization, and compensation in middle adolescence: Theoretical and applied implications. *Journal of Applied Developmental Psychology, 30*(5), 585–600.

Gewirtz, J. L. (1961). A learning analysis of the effects of normal stimulation, privation and deprivation on the acquisition of social motivation and attachment. In B. M. Foss (Ed.), *Determinants of infant behavior* (pp. 213–303). New York: Wiley.

Gewirtz, J. L., & Stingle, K. G. (1968). Learning of generalized imitation as the basis for identification. *Psychological Review, 75*, 374–397.

Gibson, E. J. (1969). *Principles of perceptual learning and development*. New York: Appleton-Century-Crofts.

Gibson, E. J., & Walk, R. D. (1960). The "Visual Cliff." *Scientific American, 202*, 64–71.

Gillie, O. (1976, October 24). Crucial data was faked by eminent psychologist. *The Sunday Times* (London), 1–2.

Gillie, O. (1979). Burt's missing ladies. *Science, 204*, 1035–1037.

Gillie, O. (1980). Burt: The scandal and the cover-up. *Supplement to the Bulletin of British Psychological Society, 33*, 9–16.

Gissis, S. B., & Jablonka, E. (Eds.). (2011). *Transformations of Lamarchism: From subtle fluids to molecular biology*. Cambridge, MA: MIT Press.

Glasersfeld, E. von, & Kelley, M. F. (1982). On the concepts of period, phase, stage and level. *Human Development, 25*, 152–160.

Gleick, J. (1987). *Chaos: Making a new science*. New York: Viking.

Goddard, H. H. (1912). *The Kallikak family: A study in the heredity of feeble-mindedness*. New York: Macmillan.

Goddard, H. H. (1914). *Feeble-mindedness: Its causes and consequences*. New York: Macmillan.

Goldberg, L. R. (1993). The structure of phenotypic personality traits. *American Psychologist, 48*, 26–34.

Goldberger, A. S. (1979). Heritability. *Economica, 46*, 327–347.

Goldberger, A. S. (1980). Review of "Cyril Burt, Psychologist." *Challenge: The Magazine of Economic Affairs, 23*, 61–62.

Goldberger, A. S., & Manski, C. F. (1995). Review Article: *The Bell Curve* by Herrnstein and Murray. *Journal of Economic Literature, 33*, 762–776.

Goldschmidt, R. (1933). Some aspects of evolution. *Science, 78*, 539–547.

Gollin, E. S. (1965). A developmental approach to learning and cognition. In L. P. Lipsitt & C. C. Spiker (Eds.), *Advances in child development and behavior* (Vol. 2, pp. 159–186). New York: Academic Press.

Gollin, E. S. (1981). Development and plasticity. In E. S. Gollin (Ed.), *Developmental plasticity: Behavioral and biological aspects of variations in development* (pp. 231–251). New York: Academic Press.

Goodnow, J. J., & Lawrence, J. A. (2015) Children and cultural context. In M. H. Bornstein and T. Leventhal (Eds.), *Handbook of child psychology and developmental science: Vol. 4 Ecological settings and processes* (7th ed., pp. 746–786). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Gore, A., & Gore, T. (2002). *Joined at the heart: The transformation of the American family*. New York: Macmillan.

Gorey, K. M., & Cryns, A. G. (1995). Lack of racial differences in behavior: A quantitative replication of Rushton's (1988) review and an independent meta-analysis. *Personality and Individual Differences, 19*, 345–353.

Gottesman, I. L., & Shields, J. (1982). *Schizophrenia: The epigenetic puzzle*. Cambridge: Cambridge University Press.

Gottlieb, G. (1970). Conceptions of prenatal behavior. In L. R. Aronson, E. Tobach, D. S. Lehrman, & J. S. Rosenblatt (Eds.), *Development and evolution of behavior: Essays in memory of T. C. Schneirla* (pp. 111–137). San Francisco: Freeman.

Gottlieb, G. (1976a). The roles of experience in the development of behavior and the nervous system. In G. Gottlieb (Ed.), *Neural and behavioral specificity* (pp. 25–54). New York: Academic Press.

Gottlieb, G. (1976b). Conceptions of prenatal development: Behavioral embryology. *Psychological Review, 83*, 215–234.

Gottlieb, G. (1983). The psychobiological approach to developmental issues. In M. M. Haith & J. Campos (Eds.), *Handbook of child psychology: Infancy and biological bases* (Vol. 2, pp. 1–26). New York: Wiley.

Gottlieb, G. (1991). The experiential canalization of behavioral development: Theory. *Developmental Psychology, 27*, 4–13.

Gottlieb, G. (1992). *Individual development and evolution: The genesis of novel behavior*. New York: Oxford University Press.

Gottlieb, G. (1997). *Synthesizing nature–nurture: Prenatal roots of instinctive behavior*. Mahwah, NJ: Erlbaum.

Gottlieb, G. (1998). Normally occurring environmental and behavioral influences on gene activity: From central dogma to probabilistic epigenesis. *Psychological Review, 105*, 792–802.

Gottlieb, G. (2003). Probabilistic epigenesis of development. In J. Valsiner & K. J. Connolly (Eds.), *Handbook of developmental psychology* (pp. 3–17). London: Sage.

Gottlieb, G. (2004). Normally occurring environmental and behavioral influences on gene activity: From central dogma to probabilistic epigenesis. In C. Garcia Coll, E. Bearer, & R. M. Lerner (Eds.), *Nature and nurture: The complex interplay of genetic and environmental influences on human behavior and development* (pp. 85–106). Mahwah, NJ: Erlbaum.

Gottlieb, G., Wahlsten, D., & Lickliter, R. (1998). The significance of biology for human development: A developmental psychobiological systems view. In W. Damon (Series Ed.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (5th ed., pp. 233–273). New York: Wiley.

Gottlieb, G., Wahlsten, D., & Lickliter, R. (2006). The significance of biology for human development: A developmental psychobiological systems view. In R. M. Lerner (Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (6th ed., pp. 210–257). Editors-in-chief: W. Damon & R. M. Lerner. Hoboken, NJ: Wiley.

Gould, S. J. (1976). Grades and clades revisited. In R. B. Masterton, W. Hodos, & H. Jerison (Eds.), *Evolution, brain, and behavior: Persistent problems* (pp. 115–122). New York: Lawrence Erlbaum Associates.

Gould, S. J. (1977). *Ontogeny and phylogeny*. Cambridge, MA: Belknap Press of Harvard University Press.

Gould, S. J. (1980). Jensen's last stand. *New York Review of Books, 27*, 38–44.

Gould, S. J. (1981). *The mismeasure of man*. New York: Norton.

Gould, S. J. (1996). *The mismeasure of man* (revised/expanded ed.). New York: Norton.

Gould, S. J. (1997). Darwinian fundamentalism. *New York Review of Books, 44*(10), 34–37.

Gould, S. J., & Lewontin, R. C. (1979). The spandrels of San Marco and the panglossian paradigm: A critique of the adaptionist programme. In J. Maynard Smith & R. Holliday (Eds.), *The evolution of adaptation by natural selection* (pp. 581–598). London: Royal Society of London.

Gould, S., & Vrba, E. (1982). Exaptation: A missing term in the science of form. *Paleobiology, 8*, 4–15.

Goulet, L. R. & Baltes, P. B. (Eds.). (1970). *Life-span developmental psychology: Theory and research*. New York: Academic Press.

Graham, S. (1992). "Most of the subjects were white and middle class": Trends in published research on African

Americans in selected APA journals, 1970–1989. *American Psychologist, 47*, 629–639.

Greenberg, G. (1983). Psychology without the brain. *The Psychological Record, 33*, 49–58.

Greenberg, G. (2004). R. I. P. Genetic determinism: Please. Review of G. Kaplan and L. J. Rogers (2003). *Gene worship*. New York: Other Press. *Developmental Psychobiology, 46*, 93–96.

Greenberg, G. (2005). The limitations of behavior-genetic analyses: Comment on McGue, Elkins, Walden, and Iacono (2005). *Developmental Psychology, 41*(6), 989–992.

Greenberg, G. (2011). The failure of biogenetic analysis in psychology: Why psychology is not a biological science. *Research in Human Development, 8*, 173–191.

Greenberg, G. (2013). A long way from genes to behavior. Commentary on: With Gottlieb beyond Gottlieb: The role of epigenetics in psychobiological development. *International Journal of Developmental Science, 7*, 83–86.

Greenberg, G. (2014). How new ideas in physics and biology influence developmental science. *Research in Human Development, 11*(1), 5–21.

Greenberg, G. (2015). The case against behavioral genetics. Review of A. Panofsky (2014). *Misbehaving science: Controversy and the development of behavior genetics*. Chicago: University of Chicago Press. *Developmental Psychobiology, 57*, 854–857.

Greenberg, G. (2017). How many nails does it take to seal the coffin? Review of R. J. Perry (2015). Killer apes, naked people & Just plain nasty people: The misuse and abuse of science in political discourse. *Developmental Psychobiology, 59*(1), 133–134.

Greenberg, G., & Haraway, M. H. (2002). *Principles of comparative psychology*. Boston: Allyn & Bacon.

Greenberg, G., & Lambdin, C. (2007). Psychology is a behavioral science, not a biological science. A discussion of the issue and a review of *Neural theories of mind: why the mind-brain problem may never be solved*, by William Uttal. *The Psychological Record,* 57, 457–475.

Greenberg, G., & Partridge, T. (2000). Prologemena to Praxiology redux: The psychology of Zing-Yang Kuo. Frances L. Hiatt School of Psychology. *From Past to Future: Clark Papers on the History of Psychology*. Vol 2(2). *From instinct to epigenesis: Lessons from Zing-Yang Kuo* (pp. 13–37). Worchester, MA: Clark University.

Greenberg, G., & Partridge, T. (2010). Biology, evolution, and development. In W. F. Overton (Ed.), *Handbook of life-span development: Vol. 1 Cognition, biology, and methods* (pp. 115–148). Hoboken, NJ: Wiley.

Greenberg, G., Partridge, T., Weiss, E., & Haraway, M. M. (1999). Integrative levels, the brain, and the emergence of complex behavior. *Review of General Psychology, 3*(3), 168–187.

Greenberg, G., & Tobach, E. (Eds.). (1984). *Behavioral evolution and integrative levels.* Hillsdale, NJ: Erlbaum.

Greene, B. (1993). *Hang time: Days and dreams with Michael Jordan*. New York: St. Martin's Press.

Greenfield, P. M., Suzuki, L. K., & Rothstein-Fisch, C. (2006). Cultural pathways through human development. In K. A. Renninger & I. E. Sigel (Eds.), *Handbook of child psychology: Vol. 4 Child psychology in practice* (6th ed., pp. 655–699). Editors-in-chief: W. Damon & R. M. Lerner. Hoboken, NJ: Wiley.

Greenough, W. T., & Green, E. J. (1981). Experience and the changing brain. In J. L. McGaugh, J. G. March, & S. B. Kiesler (Eds.), *Aging: Biology and behavior* (pp. 159–200). New York: Academic Press.

Griffith, A. N. (2016). Trajectories of trust within the youth program context. *Qualitative Psychology, 3*, 98–119.

Griffiths, P. E., & Tabery, J. (2008). Behavioral genetics and development: Historical and conceptual causes of controversy. *New Ideas in Psychology, 26*, 332–352.

Griffiths, P. E., & Tabery, J. (2013). Developmental systems theory: What does it explain, and how does it explain it? In R. M. Lerner & J. B. Benson (Eds.), *Advances in child development and behavior: Embodiment and epigenesis: Theoretical and methodological issues in understanding the role of biology within the relational developmental system. Part A: Philosophical, theoretical, and biological dimensions* (pp. 65–94). London: Elsevier.

Grouse, L. D., Schrier, B. K., Bennett, E. L., Rosenzweig, M. R., & Nelson, P. G. (1978). Sequence diversity studies of rat brain RNA: Effects of environmental complexity and rat brain RNA diversity. *Journal of Neurochemistry, 30*, 191–203.

Grouse, L. D., Schrier, B. K., & Nelson, P. G. (1979). Effect of visual experience on gene expression during the development of stimulus specificity in cat brain. *Experimental Neurology, 64*, 354–359.

Grouse, L. D., Schrier, B. K., Letendre, C. H., & Nelson, P. G. (1980). RNA sequence complexity in central nervous system development and plasticity. *Current Topics in Developmental Biology, 16*, 381–397.

Gruber, H. E. (1981, March 1, 7, 8). Nature versus nurture: A natural experiment. *The New York Times Book Review*, 22–23.

Grzimek, B. (Ed.). (1972). *Animal life encyclopedia*. New York: Van Nostrand Reinhold.

Gump, P. V. (1975). Ecological psychology and children. In E. M. Hetherington (Ed.), *Review of child development research* (pp. 75–126). Chicago: University of Chicago Press.

Gunnar, M. R., Doom, J. R., & Esposito, E. A. (2015). Psychoneuroendocrinology of stress: Normative development and individual differences. In M. E. Lamb (Eds.), *Handbook of child psychology and developmental science: Vol. 3 Socioemotional processes* (7th ed., pp. 106–151). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Guo, S. W. (2000). Gene–environment interaction and the mapping of complex traits: Some statistical models and their implications. *Human Heredity, 50*, 286–303.

Haan, N., & Day, D. (1974). A longitudinal study of change and sameness in personality development: Adolescence to later adulthood. *International Journal of Aging and Human Development, 5*, 11–39.

Hadler, N. M. (1964). Heritability and phototaxis in Drosophila melanogster. *Genetics, 50*, 1269–1277.

Haeckel, E. (1868). *Naturliche Schopfungsgeschichte*. Berlin: Georg Reimer.

Haeckel, E. (1876). *The history of creation; or the development of the earth and its inhabitants by the action of natural causes*. New York: Appleton.

Haeckel, E. (1891). *Anthropogenie oder Entwickelungsgeschichte des Menschen* (4th rev. and enlarged ed.). Leipzig: Wilhelm Engelmann.

Haeckel, E. (1905). *The wonders of life*. New York: Harper.

Hagen, J. W., Paul, B., Gibb, S., & Wolters, C. (1990). Trends in research as reflected by publications in *Child Development*: 1930–1989. Paper presented at the Biennial meeting of the Society for Research on Adolescence, Atlanta, GA.

Hagestad, G. O. (1982). Parent and child: Generations in the family. In T. M. Field, A. Huston, H. C. Quay, L. Troll, & G. E. Finley (Eds.), *Review of human development* (pp. 485–499). New York: Wiley.

Hailman, J. P. (1969). How an instinct is learned. *Scientific American, 221*, 98–106.

Halfon, N., & Forrest, C. B. (2018). The emerging theoretical framework of life course health development. In N. Halfon, C. Forrest, R. M. Lerner, & E. Faustman (Eds.), *The handbook of life course health development science* (pp. 19–43). New York: Springer.

Halfon, N., Forrest, C. B., Lerner, R. M., & Faustman, E. (Eds.). (2018). *Handbook of life course health development*. New York: Springer.

Hall, G. S. (1883). The contents of children's minds. *Princeton Review, 2*, 249–272.

Hall, G. S. (1904). *Adolescence: Its psychology and its relations to physiology, anthropology, sociology, sex, crime, religion, and education* (Vol. 1 & 11). New York: Appleton.

Hall, G. S. (1922). *Senescence: The last half of life*. New York: Appleton.

Halpern, C. T., Hood, K. E., & Lerner, R. M. (2007). The scientific contributions of Gilbert Gottlieb: An overview. *European Journal of Developmental Science, 1*(2), 97–101.

Hamburger, V. (1957). The concept of development in biology. In D. B. Harris (Ed.), *The concept of development* (pp. 49–58). Minneapolis: University of Minnesota Press.

Hamilton, S. F. (1994). Employment prospected as motivation for school achievement: Links and gaps between school and work in seven countries. In R. K. Silbereisen & E. Todt (Eds.), *Adolescence in context: The interplay of family, school, peers, and work in adjustment* (pp. 267–303). New York: Springer.

Hamilton, S. F., & Hamilton, M. A. (2009). The transition to adulthood: Challenges of poverty and cultural lag. In R. M. Lerner & L. Steinberg (Eds.), *Handbook of adolescent psychology: Vol. 2 Contextual influences on adolescent development* (3rd ed., pp. 492–526). Hoboken, NJ: Wiley.

Harley, D. (1982). Models of human evolution. *Science, 217*, 296.

Harlow, H. F. (1959). Love in infant monkeys. *Scientific American, 200*, 68–74.

Harlow, H. F. (1965). Total isolation: Effects on macaque monkey behavior. *Science, 148*, 666.

Harper, L. V. (2005). Epigenetic inheritance and the intergenerational transfer of experience. *Psychological Bulletin, 131*, 340–360.

Harper, L. V. (2010). Trans-generational epigenetic inheritance. In K. E. Hood, C. T. Halpern, G. Greenberg, & R. M. Lerner (Eds.). *Handbook of developmental systems, behavior and genetics* (pp. 434–465). Malden, MA: Wiley Blackwell.

Harper, L. V. (2013). Pathways by which the interplay of organismic and environmental factors lead to phenotypic variation within and across generations. In R. M. Lerner & J. B. Benson (Eds.), *Advances in child development and behavior: Embodiment and epigenesis: Theoretical and methodological issues in understanding the role of biology within the relational developmental system. Part A. Philosophical, theoretical, and biological dimensions*. (pp. 325–354). London: Elsevier.

Harrington, G. (1975). Intelligence test may favour the majority groups in a population. *Nature, 258*, 708–709.

Harrington, G. M. (1988). Two forms of minoritygroup test bias as psychometric artifacts with an animal model (*Rattus norvegicus*). *Journal of Comparative Psychology*, *102*, 400–407.

Harris, D. B. (1957). *The concept of development*. Minneapolis: University of Minnesota Press.

Harris, J. R. (1998). *The nurture assumption: Why children turn out the way they do*. New York: Free Press.

Harris, P. L. (2006). Social cognition. In D. Kuhn & R. S. Siegler (Eds.), *Handbook of child psychology*: *Vol. 2 Cognition, perception, and language* (6th ed., pp. 811–858). Hoboken, NJ: Wiley.

Hartigan, J. A., & Wigdor, A. K. (Eds.). (1989). *Fairness in employment testing: Validity generalization, minority issues, and the General Aptitude Test Battery*. Washington, DC: National Academy Press.

Havender, W. R. (1976). Heritability of IQ. *Science, 194*, 8–9.

Havighurst, R. J. (1948). *Developmental tasks and education*. New York: David McKay.

Havighurst, R. J. (1951). *Developmental tasks and education*. New York: Longmans.

Havighurst, R. J. (1953). *Human development and education*. London: Longmans.

Havighurst, R. J. (1956). Research on the developmental task concept. *School Review, 64*, 214–223.

Havighurst, R. J. (1973). Social roles, work, leisure, and education. In C. Eisdorfer & M. P. Lawton (Eds.), *The psychology of adult development and aging* (pp. 598–618). Washington, DC: American Psychological Association.

Havighurst, R. J., Neugarten, B. L., & Tobin, S. S. (1968) Disengagement and patterns of aging. In B. L. Neugarten (Ed.), *Middle age and aging: A reader in social psychology* (pp. 161–172). Chicago: University of Chicago Press.

Hayes, S. C., Hayes, L. J., Reese, H. W., & Sarbin, T. R. (Eds.). (1993). *Varieties of scientific contextualism*. Reno, NV: Context Press.

Hearnshaw, L. S. (1979). *Cyril Burt, psychologist*. New York: Cornell University Press.

Hebb, D. O. (1949). *The organization of behavior*. New York: Wiley.

Hebb, D. O. (1970). A return to Jensen and his social critics. *American Psychologist, 25*, 568.

Heck, K. E., & Subramaniam, A. (2009). *Youth development frameworks*. [Monograph]. Davis, CA: 4-H Center for Youth Development, University of California.

Heckhausen, J. (1999). *Developmental regulation in adulthood: Age-normative and sociocultural constraints as adaptive challenges*. New York: Cambridge University Press.

Heckhausen, J. (2003). The future of life-span developmental psychology: Perspectives from control theory. In U. M. Staudinger & U. Lindenberger (Eds.), *Understanding human development: Lifespan psychology in exchange with other disciplines* (pp. 383–400). Dordrecht, Netherlands: Kluwer.

Heckhausen, J. (2011). Agency and control striving across the lifespan. In K. Fingerman, C. Berg, T. Antonucci, & J. Smith (Eds.), *Handbook of lifespan psychology* (pp. 183–212). New York: Springer.

Heckhausen, J., Dixon, R. A., & Baltes, P. B. (1989). Gains and losses in development through adulthood as perceived by different adult age groups. *Developmental Psychology, 25*, 109–121.

Heckhausen, J., & Krueger, J. (1993). Developmental expectations for the self and most other people: Age grading in three functions of social comparison. *Developmental Psychology, 29*, 539–548.

Heckhausen, J., & Schulz, R. (1995). A life-span theory of control. *Psychological Review, 102*, 284–304.

Heckhausen, J., & Wrosch, C. (2016). Challenges to developmental regulation across the life course: What are they and which individual differences matter? *International Journal of Behavioral Development, 40*, 145–150.

Heckhausen, J., Wrosch, C., & Schulz, R. (2010). A motivational theory of life-span development. *Psychological Review, 117*, 32–60.

Heckman, J. J., Humphries, J. E., & Kautz, T. (Eds.). (2014). *The myth of achievement tests: The GED and the role of character in American life*. Chicago and London: University of Chicago Press.

Heckman, J. J., Ichimura, H., & Todd, P. E. (1997). Matching as an econometric evaluation estimator: Evidence from evaluating a job training programme. *Review of Economic Studies, 64*(4), 605–654.

Heckman, J. J., Ichimura, H., & Todd, P. (1998). Matching as an econometric evaluation estimator. *Review of Economic Studies, 65*(2), 261–294.

Heckman, J. J., John, E. H., & Kautz, T. (Eds.). (2014). *The myth of achievement tests: The GED and the role of character in American life*. Chicago and London: University of Chicago Press.

Hegel, G. W. F. (1830/1874). *Hegel's logic: Being part one of the encyclopedia of the philosophical sciences* (W. Wallace, Trans.). Oxford: Oxford University Press.

Held, R., & Hein, A. V. (1963). Movement-produced stimulation in the development of visually guided behavior. *Journal of Comparative and Physiological Psychology, 56*, 872–876.

Hempel, C. G. (1966). *Philosophy of natural science*. Englewood Cliffs, NJ: Prentice-Hall.

Henle, M. (1977). The influence of Gestalt Psychology in America. In R. W. Rieber & F. Salzinger (Eds.), The roots of American psychology: Historical influences and implications for the future. *Annals of the New York Academy of Sciences, 291*, 3–12.

Hernandez, D. J. (1993). *America's children: Resources for family, government, and the economy*. New York: Russell Sage Foundation.

Herrnstein, R. J. (1971). I.Q. *Atlantic Monthly, 228*, 43–64.

Herrnstein, R. J. (1977). The evolution of behaviorism. *American Psychologist, 32*, 593–603.

Herrnstein, R. J., & Murray, C. (1994). *The bell curve: Intelligence and class structure in American life*. New York: Free Press.

Hershberg, R. M., Chase, P. A., Champine, R. B., Hilliard, L. J., Wang, J., & Lerner, R. M. (2015). "You can quit me but I'm not going to quit you": A focus group study of leaders' perceptions of their positive influences on youth in Boy Scouts of America. *Journal of Youth Development 10*(2), 5–30.

Hershberg, R. M., Rubin, R. O., Johnson, S. K., Callina, K. S., & Lerner, R. M. (2016). Perceptions of character development at a trade college: Triangulating student, alumni, administrator, and teacher perspectives. *Research in Human Development*, *13*(2), 157–173.

Hess, E. H. (1973). *Imprinting*. New York: Van Nostrand Reinhold.

Hetherington, E. M., & Baltes, P. B. (1988). Child psychology and life-span development. In E. M. Hetherington, R. M. Lerner, & M. Perlmutter (Eds.), *Child development in life-span perspective* (pp. 1–19). Hillsdale, NJ: Lawrence Erlbaum.

Hetherington, E. M., Lerner, R. M., & Perlmutter, M. (Eds.). (1988). *Child development in life-span perspective.* Hillsdale, NJ: Erlbaum.

Hewitt, J. K. (2012). Editorial policy on candidate gene association and candidate gene-by-environment interaction studies of complex traits. *Behavior Genetics, 42*, 1–2.

Hiernaux, J. (1968). Ethnic differences in growth and development. *Eugenics Quarterly, 15*, 12–21.

Higgins-D'Alessandro, A., Fisher, C. B., & Hamilton, M. G. (1998). Educating the applied developmental psychologist for university–community partnerships. In R. M. Lerner & L. A. K. Simon (Eds.), *University–community collaborations for the twenty-first century: Outreach scholarship for youth and families* (pp. 157–183). New York: Garland.

Hilgard, E. R. (1956). *Theories of learning* (2nd ed.). New York: Appleton-Century-Crofts.

Hilgard, E. R., & Marquis, D. G. (1940). *Conditioning and learning*. New York: Appleton-Century-Crofts.

Hilliard, L. J., Buckingham, M. H., Geldhof, G. J., Gansert, P., Stack, C., Gelgoot, E., Bers, M. U., & Lerner, R. M. (2018). Perspective-taking and decision-making in educational game play: A mixed-methods study. *Applied Developmental Science*, *22*(1), 1–13.

Hilliard, L. J., & Liben, L. S. (2010). Differing levels of gender salience in preschool classrooms: Effects on children's gender attitudes and group bias. *Child Development, 81*, 1787–1798.

Hinde, R. A. (1962). Sensitive periods and the development of behavior. In S. A. Barnett (Ed.), *Lessons from animal behavior for the clinician*. London: National Spastics Society.

Hines, M. (2015). Gendered development. In M. Lamb (Ed.), *Handbook of child psychology and developmental science: Vol. 3 Socioemotional processes* (7th ed., pp. 842–887). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Hirsch, J. (1963). Behavior genetics and individuality understood. *Science, 142*, 1436–1442.

Hirsch, J. (1970). Behavior-genetic analysis and its biosocial consequences. *Seminars in Psychiatry, 2*, 89–105.

Hirsch, J. (1975). Jensenism: The bankruptcy of "science" without scholarship. *Educational Theory*, *25*, 3–27, 102.

Hirsch, J. (1976a). Jensenism: The bankruptcy of "science" without scholarship. *United States Congressional Record*, *Vol. 122*: no. 73, E26712; no. 74, E26935; no. 75, E27035, E27168, E27212.

Hirsch, J. (1976b). Review of "Sociobiology," by E. O. Wilson. *Animal Behavior, 24*, 707–709.

Hirsch, J. (1981). To "unfrock the charlatans." *Sage Race Relations Abstracts, 6*, 1–65.

Hirsch, J. (1990a). Correlation, causation, and careerism. *European Bulletin of Cognitive Psychology, 10*, 647–652.

Hirsch, J. (1990b). A nemesis for heritability estimation. *Behavioral and Brain Sciences, 13*, 137–138.

Hirsch, J. (1997). Some history of heredity-vs-environment, genetic inferiority at Harvard (?), and The (incredible) Bell Curve. *Genetica, 99*, 207–224.

Hirsch, J. (1999, February 12). The pitfalls of heritability. *The Times Literary Supplement*, 33.

Hirsch, J. (2004). Uniqueness, diversity, similarity, repeatability, and heritability. In C. Garcia Coll, E. Bearer, & R. M. Lerner (Eds.), *Nature and nurture: The complex interplay of genetic and environmental influences on human behavior and development* (pp. 127–138). Mahwah, NJ: Erlbaum.

Hitler, A. (1925 [1927/1943]). *Mein kampf* (R. Manheim, Trans.). Boston: Houghton Mifflin.

Ho, M. W. (1984). Environment and heredity in development and evolution. In M. W. Ho & P. T. Saunders (Eds.), *Beyond neo-Darwinism: An introduction to the new evolutionary paradigm* (pp. 267–289). London: Academic Press.

Ho, M. W. (1998). *Genetic engineering dream or nightmare? The brave new world of bad science and big business*. New York: Third World Network, Gateway Books, Macmillan, Continuum.

Ho, M. W. (2010). Development and evolution revisited. In K. E. Hood, C. T. Halpern, G. Greenberg, & R. M. Lerner (Eds.), *Handbook of developmental systems, behavior and genetics* (pp. 61–109). Malden, MA: Wiley Blackwell.

Ho, M. W. (2013). No genes for intelligence in the fluid genome. In R. M. Lerner & J. B. Benson (Eds.), *Advances in child development and behavior: Embodiment and epigenesis: Theoretical and methodological issues in understanding the role of biology within the relational developmental system. Part B. Ontogenetic dimensions* (pp. 67–92). London: Elsevier.

Ho, M. W. (2014). How development directs evolution. In P. C. M. Molenaar, R. M. Lerner, & K. Newell (Eds.), *Handbook of developmental systems theory and methodology* (pp. 131–153). New York: Guilford.

Ho, M. W., & Saunders, P. T. (1979). Beyond neo-Darwinism – an epigenetic approach to evolution. *Journal of Theoretical Biology, 78*(4), 573–591.

Ho, M. W., & Saunders, P. T. (Eds.). (1984). *Beyond neo-Darwinism: An introduction to the new evolutionary paradigm*. London: Academic Press.

Hodos, W., & Campbell, C. B. G. (1969). Scala Naturae: Why there is no theory in comparative psychology. *Psychological Review, 76*, 337–344.

Hoffman, L. W. (1991). The influence of family environment on personality: Accounting for sibling differences. *Psychological Bulletin, 110*, 187–203.

Hoffman, R. F. (1978). Developmental changes in human infant visual-evoked potentials to patterned stimuli recorded at different scalp locations. *Child Development*, *49*(1), 110–118.

Hogan, R., Johnson, J. A., & Emler, N. P. (1978). A socioanalytical theory of moral development. *New Directions for Child Development, 2*, 1–18.

Hollingworth, H. L. (1927). *Mental growth and decline: A survey of developmental psychology*. New York: Appleton.

Holsen, I., Geldhof, G. J., Larsen, T., & Aardal, E. (2017). The Five Cs of Positive Youth Development in Norway: Assessment and associations with positive and negative outcomes. *International Journal of Behavioral Development*, *41*, 559–569.

Holt, N. L. (Ed.). (2015). *Positive youth development through sport* (2nd ed.). New York: Routledge.

Homans, G. C. (1961). *Social behavior: Its elementary forms*. New York: Harcourt, Brace & World.

Hood, K. E., Halpern, C. T., Greenberg, G., & Lerner, R. M. (2010). Developmental systems, nature–nurture, and the role of genes in behavior and development: On the legacy of Gilbert Gottlieb. In K. E. Hood, C. T. Halpern, G. Greenberg, & R. M. Lerner (Eds.). *Handbook of developmental systems, behavior and genetics*. Malden, MA: Wiley Blackwell.

Horn, J. L. (1968). The organization of abilities and the development of intelligence. *Psychological Review, 75*, 242–259.

Horowitz, F. D. (1993). Bridging the gap between nature and nurture. A conceptually flawed issue and the need for a comprehensive and new environmentalism. In R. Plomin & G. E. McClearn (Eds.), *Nature, nurture and psychology* (pp. 341–354). Washington, DC: APA Books.

Horowitz, F. D. (2000). Child development and the PITS: Simple questions, complex answers, and developmental theory. *Child Development, 71*, 1–10.

Huang-Pollock, C. L., Nigg, J. T., & Carr, T. H. (2005). Deficient attention is hard to find: Applying the perceptual load model of selective attention to attention deficient hyperactivity disorder. *Journal of Child Psychology and Psychiatry*, *46*, 1211–1218.

Hubbard, R. (1951). The mechanism of rhodopsin synthesis. *Proceedings of the National Academy of Sciences of the United States of America*, *37*(2), 69–79.

Hubbard, R. (1956). Retinene isomerase. *The Journal of General Physiology, 39*(6), 935–956.

Hubbard, R. (1958). Bleaching of rhodopsin by light and by heat. *Nature, 181*(46160), 1126.

Hubbard, R. (1990). *The politics of women's biology*. New Brunswick, NJ: Rutgers University Press.

Hubbard, R. (1995). *Profitable promises: Essays on women, science and health*. Monroe, ME: Common Courage Press.

Hubbard, R., & Lewontin, R. C. (1996). Pitfalls of genetic testing. *New England Journal of Medicine, 334*(18), 1192–1194.

Hubbard, R., & St. George, R. C. C. (1958). The rhodopsin system of the squid. *The Journal of General Physiology, 41*(3), 501–528.

Hubbard, R., & Wald, E. (1999). *Exploding the gene myth: How genetic information is produced and manipulated by scientists, physicians, employers, insurance companies, educators, and law enforcers*. Boston: Beacon Press.

Hubbard, R., & Wald, G. (1952). Cis-trans isomers of vitamin A and retinene in the rhodopsin system. *The Journal of General Physiology, 36*(2), 269–315.

Hubel, D. H., & Wiesel, T. N. (1970). The period of susceptibility to the physiological effects of unilateral eye closure in kittens. *Journal of Physiology, 206*, 419–436.

Hull, C. L. (1929). A functional interpretation of the conditional reflex. *Psychological Review, 36*, 498–511.

Hultsch, D. F., & Hickey, T. (1978). External validity in the study of human development: Theoretical and methodological issues. *Human Development, 21*, 76–91.

Hunt, J. McV. (1961). *Intelligence and experience*. New York: Ronald Press.

Hunziker, U. A., & Barr, R. G. (1986). Increased carrying reduces infant crying: A randomized controlled trial. *Pediatrics, 77*(5), 641–648.

Hyson, M., Copple, C., & Jones, J. (2006). Early childhood development and education. In K. A. Renninger & I. E. Sigel (Eds.), *Handbook of child psychology: Vol. 4 Child psychology in practice* (6th ed., pp. 3–47). Editors-in-chief: W. Damon & R. M. Lerner. Hoboken, NJ: Wiley.

Isaac, G. L. (1982). Models of human evolution. *Science, 217*, 295.

Jablonka, E., & Lamb, M. (2005). *Evolution in four dimensions: Genetic, epigenetic, behavioral, and symbolic variation in the history of life*. Cambridge, MA: MIT Press.

Jacob, F., & Monod, J. (1961). On the regulation of gene activity. *Cold Spring Harbor Symposia on Quantitative Biology, 26*, 193–209.

Jacobs, F. (1988). The five-tiered approach to evaluation: Context and implementation. In H. B. Weiss & F. Jacobs (Eds.), *Evaluating family programs* (pp. 37–68). Hawthorne, NY: Aldine.

Jacobs, F., & Kapuscik, J. (2000). *Making it count: Evaluating family preservation services*. Medford, MA: Eliot-Pearson Department of Child Development, Tufts University.

Jacquard, A. (1983). Heritability: One word, three concepts. *Biometrics, 39*, 465–477.

Jencks, C. (1972). *Inequality: A reassessment of the effect of family and schooling in America*. New York: Basic Books.

Jenkins, J. J. (1974). Remember that old theory of memory? Well forget it. *American Psychologist, 29*, 785–795.

Jensen, A. R. (1969). How much can we boost IQ and scholastic achievement? *Harvard Educational Review, 39*, 1–123.

Jensen, A. R. (1972). A reply to Gage: The causes of twin differences in IQ. *Phi Delta Kappan*, March, 420–421.

Jensen, A. R. (1973). *Educability and group differences*. New York: Harper & Row.

Jensen, A. R. (1974). How biased are culture-loaded tests? *Genetic Psychology Monographs, 90*, 185–244.

Jensen, A. R. (1976). Letters: Heritability of IQ. *Science, 194*, 6–14.

Jensen, A. R. (1980). *Bias in mental testing*. New York: Free Press.

Jensen, A. R. (1998). Jensen on "Jensenism." *Intelligence, 26*, 181–208.

Jensen, P., Hoagwood, K., & Trickett, E. (1999). Ivory towers or earthen trenches? Community collaborations to foster "real world" research. *Applied Developmental Science, 3*(4), 206–212.

Jerison, H. J. (1978). Smart dinosaurs and comparative psychology. Paper presented at the meeting of the American Psychological Association, August, Toronto, Canada.

Jinks, J. L., & Fulker, D. W. (1970). Comparison of the biometrical genetical, MAVA and classical approaches to the analysis of human behavior. *Psychological Bulletin, 73*, 311–349.

Johanson, D. C., & Edey, M. A. (1981). *Lucy: The beginnings of humankind*. New York: Simon & Schuster.

John, O. P., & Naumann, L. P. (2010). Surviving two critiques by Block? The resilient Big Five have emerged as the paradigm for personality trait psychology. *Psychological Inquiry*, *21*(1), 44–49.

Johnson, C. (2013). California was sterilizing its female prisoners as late as 2010. *The Guardian* (U.S. edition). www.theguardian.com/commentisfree/2013/nov/08/california-female-prisoner-sterilization.

Johnson, S. K., Buckingham, M. H., Morris, S. L., Suzuki, S., Weiner, M. B., Hershberg, R. M., Fremont, E. R., Batanova, M., Aymong, C. C., Hunter, C. J., Bowers, E. P., Lerner, J. V., & Lerner, R. M. (2016) Adolescents' character role models: Exploring who young people look up to as examples of how to be a good person. *Research in Human Development, 13*(2), 126–141.

Johnson, S. K., Hershberg, R. M., Arbeit, M., DeSouza, L. M., Callina, K. S., Gutierrez, A. S., Warren, D. J. A., Harris, E. M., Rubin, R. O., Lerner, J. V., & Lerner, R. M. (2014). Exploring characteristics of young adult men: Initial findings from a mixed methods evaluation of an all-male character-focused trade school. *Journal of Character Education, 10*(2), 129–154.

Johnston, L. D., O'Malley, P. M., Bachman, J. G., & Schulenberg, J. E. (2007). *Monitoring the future national results on adolescent drug use: Overview of key findings, 2006*. Bethesda, MD: National Institute on Drug Abuse.

Johnston, L. D., O'Malley, P. M., Miech, R. A., Bachman, J. G., & Schulenberg, J. E. (2016). *Monitoring the future: National survey results on drug use, 2015 Overview. Key findings on adolescent drug use*. Ann Arbor: Institute for Social Research, University of Michigan.

Joseph, J. (2003). *The gene illusion*. Ross-on-Wye: PCCS Books.

Joseph, J. (2004). *The gene illusion*. New York: Algora.

Joseph, J. (2010). Genetic research in psychiatry and psychology: A critical overview. In K. E. Hood, C. T. Halpern, G. Greenberg, & R. M. Lerner (Eds.), *Handbook of developmental systems, behavior and genetics* (pp. 557–625). Malden, MA: Wiley Blackwell.

Joseph, J. (2013). The lost study: A 1998 adoption study of personality that found no genetic relationship between birthparents and their 240 adopted-away biological offspring. In R. M. Lerner & J. B. Benson (Eds.), *Advances in child development and behavior: Embodiment and epigenesis: Theoretical and methodological issues in* *understanding the role of biology within the relational developmental system. Part B. Ontogenetic dimensions* (pp. 93–124). London: Elsevier.

Joseph, J. (2015). *The trouble with twin studies: A reassessment of twin research in the social and behavioral sciences*. New York: Routledge.

Jovanovic, J., Lerner, R. M., & Lerner, J. V. (1989). Objective and. subjective attractiveness and early adolescent adjustment. *Journal of Adolescence, 12*, 225–229.

Juel-Nielsen, N. (1965). Individual and environment: A psychiatric-psychological investigation of monozygotic twins reared apart. *Acta Psychiatrica et Neurologica Scandinavica*, Monograph Supplement 183.

Jung, T., & Wickrama, K. A. S. (2008). An introduction to latent class growth analysis and growth mixture modeling. *Social and Personality Psychology Compass, 2*, 302–317.

Kagan, J. (1969). Inadequate evidence and illogical conclusions. *Harvard Educational Review, 39*, 274–277.

Kagan, J. (1980). Perspectives on continuity. In O. G. Brim, Jr., & J. Kagan (Eds.), *Constancy and change in human development*. Cambridge, MA: Harvard University Press.

Kagan, J. (1983). Developmental categories and the premise of connectivity. In R. M. Lerner (Ed.), *Developmental psychology: Historical and philosophical perspectives*. Hillsdale, NJ: Erlbaum.

Kalikow, T. J. (1978a). Konrad Lorenz's ethological theory: Explanation and ideology, 1938–1943. *Journal of the History of Biology, 16*, 39–73.

Kalikow, T. J. (1978b). Review of civilized man's eight deadly sins, by Konrad Lorenz. *Philosophy of the Social Sciences, 8*, 99–108.

Kalikow, T. J. (1983). Konrad Lorenz's ethological theory: Explanation and ideology. *Journal of the Social Sciences, 8*, 39–73.

Kamin, L. J. (1974). *The science and politics of IQ*. Potomac, MD: Wiley.

Kamin, L. J. (1980). Jensen's last stand. A review of Arthur Jensen *Bias in Mental Testing. Psychology Today*, February, 117–118, 120, 123.

Kantor, J. R. (1959). *Interbehavioral psychology* (2nd ed.). Chicago: Principia Press.

Kaplan, B. (1966). The comparative-developmental approach and its application to symbolization and language in psychopathology. In T. Arieti (Ed.), *American handbook of psychiatry* (Vol. 3). New York: Basic Books.

Kaplan, B. (1983). A trio of trials. In R. M. Lerner (Ed.), *Developmental psychology: Historical and philosophical perspectives* (pp. 185–228). Hillsdale, NJ: Erlbaum.

Kaplan, G., & Rogers, L. J. (2003). *Gene worship*. New York: Other Press.

Katsiaficas, D., Alcantar, C. M., Hernandez, E., Samayoa, E., Gutierrez, M. N., Texis, O. R., & Williams, Z. (2016). Important theoretical and methodological turning points for understanding contribution with undocumented undergraduates. *Qualitative Psychology, 3*(1), 7–25.

Keil, F. C. (1998). Cognitive science and the origins of thought and knowledge. In W. Damon (Series Ed.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (5th ed., pp. 341–413). New York: Wiley.

Keil, F. (2006). Cognitive science and cognitive development. In D. A. Kuhn & R. S. Siegler (Eds.), *Handbook of child psychology: Vol. 2 Cognition, perception, and language* (6th ed., pp. 609–635). Editors-in-chief: W. Damon & R. M. Lerner. Hoboken, NJ: Wiley.

Keller, E. F. (2010). *The mirage of a space between nature and nurture*. Durham, NC: Duke University Press.

Kellogg Commission on the Future of State and Land-Grant Colleges. (1999). *Returning to our roots: The engaged institution*. Washington, DC: National Association of State Universities and Land-Grant Colleges.

Kellogg, W. N., & Kellogg, L. A. (1933). *The ape and the child*. New York: McGraw-Hill.

Kempthorne, O. (1978). Logical, epistemological and statistical aspects of nature–nurture data interpretation. *Biometrics, 34*, 1–23.

Kendall, P. C., Lerner, R. M., & Craighead, W. E. (1984). Human development intervention in childhood psychopathology. *Child Development, 55*, 71–82.

Kendler, H. H., & Kendler, T. S. (1962). Vertical and horizontal processes in human concept learning. *Psychological Review, 69*, 1–16.

Kendler, T. S. (1986). World views and the concept of development: A reply to Lerner and Kauffman. *Developmental Review, 6*(1), 80–95.

Kendler, T. S. (1995). *Levels of cognitive development*. New York: Psychology Press.

Kessen, W. (1962). "Stage" and "structure" in the study of children. In W. Kessen & C. Kuhlman (Eds.), *Thought in the young child* (pp. 65–86). *Monographs of the Society for Research in Child Development, 27*.

Kessen, W. (1965). *The child*. New York: John Wiley.

Killen, M., & Smetana, J. G. (Eds.). (2014). *Handbook of moral development* (2nd ed.). New York: Taylor & Francis.

Killen, M., & Smetana, J. G. (2015). Origins and development of morality. In M. E. Lamb (Ed.), *Handbook of child psychology and developmental science: Vol. 3 Socioemotional processes* (7th ed., pp. 701–749). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Kim, P., Evans, G. W., Chen, E., Miller, G., & Seeman, T. (2018). How socioeconomic disadvantages get under the skin and into the brain to influence health development across the lifespan. In N. Halfon, C. B. Forrest, R. M. Lerner, & E. Faustman (Eds.), *Handbook of life course health development* (pp. 463–497). New York: Springer.

Kimble, G. A. (1961). *Hilgard and Marquis' conditioning and learning*. New York: Appleton-Century-Crofts.

King, P. E., & Boyatzis, C. J. (2015). Religious and spiritual development. In M. E. Lamb (Ed.), *Handbook of child psychology and developmental science: Vol. 3 Socioemotional processes* (7th ed., pp. 975–1022). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

King, P. E., Carr, D., & Boitor, C. (2011). Religion, spirituality, positive youth development, and thriving. In R. M. Lerner, J. V. Lerner, & J. B. Benson, (Eds.), *Advances in child development and behavior: Positive youth development: Research and applications for promoting thriving in adolescence* (pp. 161–195). Amsterdam: Elsevier Publishing.

King, P. E., Dowling, E. M., Mueller, R. A., White, K., Schultz, W., Osborn, P., Dickerson, E., Bobek, D. L., Lerner, R. M., Benson, P. L., & Scales, P. C. (2005). Thriving in adolescence: The voices of youth-serving practitioners, parents, and early and late adolescents. *Journal of Early Adolescence, 25*(1), 94–112.

Kinsey, A. C., Pomeroy, W. B., Martin, C. E., & Gebhard, P. H. (1953). *Sexual behavior in the human female*. Philadelphia, PA: Saunders.

Kitcher, P. (1985). *Vaulting ambition*. Cambridge, MA: MIT Press.

Kjerstad, C. L. (1919). The form of the learning curves for memory. *Psychological Monographs, 26*, 116.

Klaus, M., & Kennell, J. (1976). *Maternal-infant bonding*. St. Louis: C. V. Mosby.

Kleckner, J. H. (1977). Alcoholics can drink again, or can they? *Psychology, 14*(2), 6–8.

Kluckhohn, C., & Murray, H. (1948). *Personality in nature, society, and culture* (pp. 3–32). New York: Knopf.

Ko, E. P., Yomo, T., & Urabe, I. (1994). Dynamic clustering of bacterial populations. *Physica D: Nonlinear Phenomena, 75*, 81–88.

Kohlberg, L. (1958). The development of modes of moral thinking and choice in the years ten to sixteen. Unpublished doctoral dissertation, University of Chicago.

Kohlberg, L. (1963a). The development of children's orientations toward a moral order: Sequence in the development of moral thought. *Vita Humana, 6*, 11–33.

Kohlberg, L. (1963b). Moral development and identification. In H. W. Stevenson (Ed.), *Child psychology: 62nd yearbook of the National Society of Education* (pp. 277–332). Chicago: University of Chicago Press.

Kohlberg, L. (1968). Early education: A cognitive-developmental view. *Child Development, 39*, 1014–1062.

Kohlberg, L. (1978). Revisions in the theory and practice of moral development. *New Directions for Child Development, 2*, 93–120.

Kollar, E. J., & Fisher, C. (1980). Tooth induction in chick epithelium: Expression of quiescent genes for enamel synthesis. *Science, 207*, 993–995.

Koller, S. H., Motti-Stefanidi, F., Petersen, A. C., & Verma, S. (2017). Achieving positive development for youth globally: How far have we come and what is yet needed? In A. C. Petersen, S. H. Koller, F. Motti-Stefanidi, & S. Verma (Eds.), *Positive Youth Development in global contexts of social and economic change* (pp. 301–310). New York: Routledge.

Konner, M. (1982). *The tangled wing*. New York: Holt, Rinehart & Winston.

Korn, S. J., Chess, S., & Fernandez, P. (1978). The impact of children's physical handicaps on marital quality and family interactions. In R. M. Lerner & G. B. Spanier (Eds.), *Child influences on marital and family interaction: A life-span perspective* (pp. 299–326). New York: Academic Press.

Krashen, S. D. (1975). The critical period for language and its possible bases. *Annals of the New York Academy of Sciences, 263*, 211–224.

Kreppner, K. (1994). William L. Stern: A neglected founder of developmental psychology. In R. D. Parke, P. A. Ornstein, J. J. Rieser, & C. Zahn-Waxler (Eds.), *A century of developmental psychology* (pp. 311–331). Washington. DC: American Psychological Association.

Krimsky, S. (2013). Genetic causation: A cross disciplinary inquiry. In R. M. Lerner & J. B. Benson (Eds.), *Advances in child development and behavior: Embodiment and epigenesis: Theoretical and methodological issues in understanding the role of biology within the relational developmental system. Part A: Philosophical, theoretical, and biological dimensions* (pp. 307–323). London: Elsevier.

Krimsky, S., & Gruber, J. (Eds.). (2013). *Genetic explanations: Sense and nonsense*. Cambridge, MA: Harvard University Press.

Kuczynski, L., & De Mol, J. (2015). Dialectical models of socialization. In W. F. Overton & P. C. Molenaar (Eds.), *Handbook of child psychology and developmental science: Vol. 1 Theory and method* (7th ed., pp. 323–368). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Kuhn, D. (1995). Microgenetic study of change: What has it told us? *Psychological Science, 6*, 133–139.

Kuhn, D. (1998). Afterword to Volume 2: Cognition, perception, and language. In D. Kuhn & R. S. Siegler (Eds.), *Handbook of child psychology: Cognition, perception, and language* (Vol. 2, pp. 979–981). Editor-in-Chief: William Damon. New York: John Wiley & Sons, Inc.

Kuhn, D. (2009). Adolescent thinking. In R. M. Lerner & L. Steinberg (Eds.), *Handbook of adolescent psychology: Vol. 1 Individual bases of adolescent development* (3rd ed., pp. 152–186). Hoboken, NJ: Wiley.

Kuhn, D., & Franklin, S. (2006). The second decade: What develops (and how). In D. A. Kuhn & R. S. Siegler (Eds.), *Handbook of child psychology: Vol. 2 Cognition, perception, and language* (6th ed., pp. 953–993). Editors-in-chief: W. Damon & R. M. Lerner. Hoboken, NJ: Wiley.

Kuhn, T. S. (1962). *The structure of scientific revolutions*. Chicago: University of Chicago Press.

Kuhn, T. S. (1970). *The structure of scientific revolutions* (2nd ed.). Chicago: University of Chicago Press.

Kuo, Z.-Y. (1930). The genesis of the cat's response to the rat. *Journal of Comparative Psychology, 11*, 1–35.

Kuo, Z.-Y. (1967). *The dynamics of behavior development*. New York: Random House.

Kuo, Z.-Y. (1970). The need for coordinated efforts in developmental studies. In A. Aronson, E. Tobach, D. S. Lehrman, & J. S. Rosenblatt (Eds.), *Development and evolution of behavior: Essays in memory of T. C. Schneirla* (pp. 181–193). San Francisco: W. H. Freeman.

Kuo, Z.-Y. (1976). *The dynamics of behavior development: An epigenetic view*. New York: Plenum.

Kurtines, W. M., Azmitia, M., & Gewirtz, J. L. (Eds.). (1992). *The role of values in psychology and human development*. New York: Wiley.

Labouvie-Vief, G. (1981). Proactive and reactive aspects of constructivism: Growth and aging in life-span perspective. In R. M. Lerner & N. A. Busch-Rossnagel (Eds.), *Individuals as producers of their development: A life-span perspective* (pp. 197–230). New York: Academic Press.

Lachman, M. E., & James, J. B. (Eds.). (1997). *Multiple paths of midlife development*. Chicago: University of Chicago Press.

Lamb, M. E. (1977a). A reexamination of the infant social world. *Human Development, 20*, 65–85.

Lamb, M. E. (1977b). Father–infant and mother–infant interaction in the first year of life. *Child Development, 48*, 167–181.

Lamb, M. E. (1977c). The development of mother–infant and father–infant attachments in the second year of life. *Developmental Psychology, 13*, 637–648.

Lamb, M. E. (1978a). Qualitative aspects of mother– and father–infant attachments. *Infant Behavior and Development, 1*, 265–275.

Lamb, M. E. (1978b). Influence of the child on marital quality and family interaction during the prenatal, perinatal, and infancy periods. In R. M. Lerner & G. B. Spanier (Eds.), *Child influences on marital and family interaction: A life-span perspective* (pp. 137–163). New York: Academic Press.

Lamb, M. E. (1998). Nonparental child care: Context, quality, correlates, and consequences. In W. Damon (Series Ed.), I. Sigel, & K. A. Renninger (Vol. Eds.), *Handbook of child psychology*: *Vol. 4 Child psychology in practice* (5th ed., pp. 73–133). New York: Wiley.

Lamb, M. E. (2000). The effects of quality of care on child development. *Applied Developmental Science, 4*, 1112–1115.

Lamb, M. E., Malloy, L. C., Hershkowitz, I., & La Rooy, D. (2015) Children and the law. In M. E. Lamb (Ed.), *Handbook of child psychology and developmental science: Vol. 3 Socioemotional processes* (7th ed., pp. 464–512). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Lamb, M. E., Thompson, R. A., Gardner, W. P., & Charnov, E. L. (1985). *Infant–mother attachment*. Hillsdale, NJ: Lawrence Erlbaum Associates.

Landsell, H. (1969). Verbal and nonverbal factors in right-hemisphere speech: Relation to early neurological history. *Journal of Comparative and Physiological Psychology, 69*, 734–738.

Langer, J. (1969). *Theories of development*. New York: Holt, Rinehart & Winston.

Langer, J. (1970). Werner's theory of development. In P. H. Mussen (Ed.), *Carmichael's manual of child psychology* (Vol. 1, pp. 733–772). New York: Wiley.

Langlois, J. H., & Stephan, C. W. (1981). Beauty and the beast: The role of physical attraction in peer relationships and social behavior. In S. S. Brehm, S. M. Kassin, & S. X. Gibbons (Eds.), *Developmental social psychology: Theory and research* (pp. 152–168). New York: Oxford University Press.

Larson, K., Russ, S. A., Kahn, R. S., Flore, G., Doodman, E., Cheng, T. L., & Halfon, N. (2018). Healthy disparities: A life course health development perspective and future research directions. In N. Halfon, C. B. Forrest, R. M. Lerner, & E. Faustman (Eds.), *Handbook of life course health development* (pp. 499–520). New York: Springer.

Larson, R. W. (2000). Toward a psychology of positive youth development. *American Psychologist, 55*(1), 170–183.

Larson, R. W., & Angus, R. M. (2011). Adolescents' development of skills for agency in youth programs: Learning to think strategically. *Child Development, 82*(1), 277–294.

Larson, R. W., & Brown, J. R. (2007). Emotional development in adolescence: What can be learned from a high school theater program? *Child Development, 78*(4), 1083–1099.

Larson, R. W., Rickman, A. N., Gibbons, C. M., & Walker, K. C. (2009). Practitioner expertise: Creating quality within the daily tumble of events in youth settings. *New Directions for Youth Development, 121*, 71–88.

Larson, R. W., Walker, K., & Pearce, N. (2005). A comparison of youth-driven and adult-driven youth programs: Balancing inputs from youth and adults. *Journal of Community Psychology, 33*(1), 57–74.

Latour, B. (1993). *We have never been modern* (Catherine Porter, Trans.). Cambridge, MA: Harvard University Press.

Laursen, B., Little, T. D., & Card, N. A. (Eds.). (2012). *Handbook of developmental research methods*. New York: Guilford.

Layzer, D. (1974). Heritability analyses of IQ scores: Science or numerology? *Science, 183*, 1259–1266.

Layzer, D. (2000). Comment on "Misconceptions of biometrical IQists" by C. Capron et al. Unpublished manuscript. Cambridge, MA: Harvard University.

Leaper, C. (2015). Gender and social-cognitive development. In L. S. Liben & U. Mueller (Eds.), *Handbook of child psychology and developmental science: Vol. 2 Cognitive processes* (7th ed., pp. 806–853). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Lee, C. D., Spencer, M. B., & Harpalani, V. (2003). "Every shut eye ain't sleep": Studying how people live culturally. *Educational Researcher, 32*(5), 6–13.

Lehrman, D. S. (1953). A critique of Konrad Lorenz's theory of instinctive behavior. *Quarterly Review of Biology, 28*, 337–363.

Lehrman, D. S. (1970). Semantic and conceptual issues in the nature–nurture problem. In L. R. Aronson, E. Tobach, D. S. Lehrman, & J. S. Rosenblatt (Eds.), *Development and evolution of behavior: Essays in memory of T. C. Schneirla* (pp. 17–52). San Francisco: Freeman.

Lenneberg, E. (1967). *The biological basis of language*. New York: Academic Press.

Lenneberg, E. (1969). On explaining language. *Science, 165*, 635–643.

Lenneberg, E., Nichols, I., & Rosenberger, E. (1964). Primitive stages of language development in Mongolism. In *Disorders of communication: Vol. 42 Research publications*. Baltimore, MD: Williams & Wilkins.

Lenz, F. (1930). Alfred Ploetz zum 70. Geburtstag am 22 August. *Archiv für Rassen- und Gesellschaftsbiologie, 24*.

Leont'ev, A. N. (1981). The problem of activity in psychology. In J. V. Wertsch (Ed.), *The concept of activity in Soviet psychology* (pp. 37–71). Armonk, NY: Sharpe.

Lerner, J. V. (1983). A "goodness of fit" model of the role of temperament in psychosocial adaptation in early adolescents. *Journal of Genetic Psychology, 143*, 149–157.

Lerner, J. V. (1984). The import of temperament for psychosocial functioning: Test of a "goodness of fit" model. *Merrill-Palmer Quarterly, 30*, 177–188.

Lerner, J. V., & Callina, K. S. (2018). Positive youth development: Applying relational developmental systems metatheory to promote thriving. In A. S. Dick & U. Müller (Eds.), *Advancing developmental science: Philosophy, theory, and method* (pp. 185–198). New York: Routledge.

Lerner, J. V., & Lerner, R. M. (1983). Temperament and adaptation across life: Theoretical and empirical issues. In P. B. Baltes & O. G. Brim, Jr. (Eds.), *Life-span development and behavior* (Vol. 5, pp. 197–231). New York: Academic Press.

Lerner, J. V., & Lerner, R. M. (1989). Introduction: Longitudinal analyses of biological, psychological, and social interactions across the transitions of early adolescence. *Journal of Early Adolescence, 9*, 175–180.

Lerner, J. V., Phelps, E., Forman, Y. E., & Bowers, E. (2009). Positive youth development. In. R. M. Lerner & L. Steinberg (Eds.), *Handbook of adolescent psychology* (3rd ed., pp. 524–558). Hoboken, NJ: Wiley.

Lerner, R. M. (1972). "Richness" analyses of body build stereotype development. *Developmental Psychology, 7*, 219.

Lerner, R. M. (1973). The development of personal space schemata toward body build. *Journal of Psychology, 84*, 229–235.

Lerner, R. M. (1976). *Concepts and theories of human development*. Reading, MA: Addison-Wesley.

Lerner, R. M. (1977). Biographies of DeSanctis, S., Dewey, J., Gesell, A., Goodenough, F., Locke, J., Terman, L. M., Werner, H., and Witmer, L. In B. B. Wolman (Ed.), *International encyclopedia of neurology, psychiatry, psychoanalysis, and psychology*. New York: Van Nostrand Reinhold.

Lerner, R. M. (1978). Nature, nurture, and dynamic interactionism. *Human Development, 21*, 1–20.

Lerner, R. M. (1979). A dynamic interactional concept of individual and social relationship development. In R. L. Burgess & T. L. Huston (Eds.), *Social exchange in developing relationships* (pp. 271–305). New York: Academic Press.

Lerner, R. M. (1980). Concepts of epigenesis: Descriptive and explanatory issues. A critique of Kitchner's comments. *Human Development, 23*, 63–72.

Lerner, R. M. (1982). Children and adolescents as producers of their own development. *Developmental Review, 2*, 342–370.

Lerner, R. M. (1984). *On the nature of human plasticity*. New York: Cambridge University Press.

Lerner, R. M. (1985). Individual and context in developmental psychology: Conceptual and theoretical issues. In J. R. Nesselroade & A. von Eye (Eds.), *Individual development and social change: Explanatory analysis* (pp. 155–187). New York: Academic Press.

Lerner, R. M. (1986). *Concepts and theories of human development* (2nd ed.). New York: Random House.

Lerner, R. M. (1988). Personality development: A life-span perspective. In E. M. Hetherington, R. M. Lerner, & M. Perlmutter (Eds.), *Child development in life-span perspective* (pp. 21–46). Hillsdale, NJ: Erlbaum.

Lerner, R. M. (1991). Changing organism–context relations as the basic process of development: A developmental contextual perspective. *Developmental Psychology, 27*, 27–32.

Lerner, R. M. (1992a). *Final solutions: Biology, prejudice, and genocide*. University Park, PA: Penn State Press.

Lerner, R. M. (1992b). Nature, nurture and mass murder. *Readings: A Journal of Reviews and Commentary on Mental Health, 7*(3), 8–15.

Lerner, R. M. (1993a). Plasticity: Genetic program or developmental process? [Review of Brauth, S. E., Hall, W. S., & Dooling, R. J. (Eds.). (1991). *Plasticity of development*. Cambridge, MA: MIT Press.] *Contemporary Psychology, 11*, 1192–1194.

Lerner, R. M. (1993b). Investment in youth: The role of home economics in enhancing the life chances of America's children. *AHEA Monograph Series, 1*, 5–34.

Lerner, R. M. (1995a). Toward enhancing knowledge about, and service to, America's poor children: Models and methods for integrating research, policy, and programs in child development research. In

H. E. Fitzgerald, B. M. Lester, & B. Zuckerman (Eds.), *Children of poverty: Research, health, and policy issues* (pp. 327–345). New York: Garland.

Lerner, R. M. (1995b). The place of learning within the human development system: A developmental contextual perspective. *Human Development, 38*, 361–366.

Lerner, R. M. (1995c). *America's youth in crisis: Challenges and options for programs and policies*. Thousand Oaks, CA: Sage.

Lerner, R. M. (1996). Relative plasticity, integration, temporality, and diversity in human development: A developmental contextual perspective about theory, process, and method. *Developmental Psychology, 32*(4), 781–786.

Lerner, R. M. (Ed). (1998a). *Handbook of child psychology: Vol. 1 Theoretical models of human development* (5th ed.). Editor-in-Chief: William Damon. New York: Wiley.

Lerner, R. M. (1998b). Theories of human development: Contemporary perspectives. In W. Damon (Series Ed.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (5th ed., pp. 1–24). New York: Wiley.

Lerner, R. M. (2002). *Concepts and theories of human development* (3rd ed.). Mahwah, NJ: Lawrence Erlbaum Associates.

Lerner, R. M. (2004). *Liberty: Thriving and civic engagement among America's youth*. Thousand Oaks, CA: Sage.

Lerner, R. M. (2005). Foreword: Urie Bronfenbrenner: Career contributions of the consummate developmental scientist. In U. Bronfenbrenner, *Making human beings human* (pp. ix–xxvi). Thousand Oaks, CA: Sage.

Lerner, R. M. (Ed.). (2006a). *Handbook of child psychology: Vol. 1 Theoretical models of human development* (6th ed.). Editors-in-chief: W. Damon & R. M. Lerner. Hoboken, NJ: Wiley.

Lerner, R. M. (2006b). Developmental science, developmental systems, and contemporary theories of human development. In R. M. Lerner (Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (6th ed., pp. 1–17). Editors-in-chief: W. Damon & R. M. Lerner. Hoboken, NJ: Wiley.

Lerner, R. M. (2006c). Another nine-inch nail for behavioral genetics! A commentary on Richardson and Norgate. *Human Development, 49*(6), 336–342.

Lerner, R. M. (2009). The positive youth development perspective: Theoretical and empirical bases of a strength-based approach to adolescent development. In C. R. Snyder and S. J. Lopez (Eds.), *Oxford handbook of positive psychology* (2nd ed., pp. 149–163). Oxford: Oxford University Press.

Lerner, R. M. (2010). Preface. In R. M. Lerner (Editor-in-chief), *Handbook of life-span development* (Vol. 1: pp. ix–xvii; Vol. 2: pp. viii–xvi). Hoboken, NJ: Wiley.

Lerner, R. M. (2011). Structure and process in relational, developmental systems theories: A commentary on contemporary changes in the understanding of developmental change across the life span. *Human Development, 54*, 34–43.

Lerner, R. M. (2012). Developmental science and the role of genes in development. *GeneWatch, 25*(1–2), 34–35. www.councilforresponsiblegenetics.org/genewatch/GeneWatchPage.aspx?pageId=413.

Lerner, R. M. (2014). Taking the boy out of Brooklyn: Time, place, and people in the development of a developmental scientist. In R. M. Lerner, A. C. Petersen, R. K. Silbereisen, & J. Brooks-Gunn (Eds.), *The developmental science of adolescence: History through autobiography* (pp. 277–308). New York: Psychology Press.

Lerner, R. M. (2015a). Preface. *Handbook of child psychology and developmental science* (7th ed.). Editor-in-chief: R. M. Lerner (pp. xv–xxi). Hoboken, NJ: Wiley.

Lerner, R. M. (2015b). Promoting social justice by rejecting genetic reductionism: A challenge for developmental science. *Human Development, 58*, 67–69.

Lerner, R. M. (2015c). Eliminating genetic reductionism from developmental science. *Research in Human Development, 12*, 178–188.

Lerner, R. M. (2015d). Promoting positive human development and social justice: integrating theory, research, and application in contemporary developmental science. *International Journal of Psychology*, *50*(3), 165–173.

Lerner, R. M. (Ed.). (2015e). *Handbook of child psychology and developmental science* (7th ed.). Hoboken, NJ: Wiley.

Lerner, R. M. (2016). Complexity embraced and complexity reduced: A tale of two approaches to human development. *Human Development, 59*, 242–249.

Lerner, R. M. (2017). Promoting positive development, health, and social justice through dismantling genetic determinism. In K. Bogard, V. M. Murry, & C. Alexander (Eds.), *Perspectives on health equity and social determinants of health* (pp. 57–76). Washington, DC: National Academy of Medicine.

Lerner, R. M., Agans, J. P., DeSouza, L. M., & Gasca, S. (2013). Describing, explaining, and optimizing intraindividual change across the life span: A relational developmental systems perspective. *Review of General Psychology, 17*(2), 179–183.

Lerner, R. M., Agans, J. P., DeSouza, L. M., & Hershberg, R. M. (2014). Developmental science in 2025: A predictive review. *Research in Human Development, 11*, 255–272.

Lerner, R. M., & Benson, J. B. (Eds.). (2013a). *Embodiment and epigenesis: Theoretical and methodological issues in understanding the role of biology within the relational developmental system. Vol. 1: Philosophical, theoretical, and biological dimensions. Advances in child development and behavior, 44*. London: Elsevier.

Lerner, R. M., & Benson, J. B. (Eds.). (2013b). *Embodiment and epigenesis: Theoretical and methodological issues in understanding the role of biology within the relational developmental system. Vol. 2: Ontogenetic dimensions. Advances in child development and behavior, 45*. London: Elsevier.

Lerner, R. M., & Benson, J. B. (2013c). Introduction: Embodiment and epigenesis: A view of the issues. In R. M. Lerner & J. B. Benson (Eds.), *Advances in child development and behavior: Embodiment and epigenesis: Theoretical and methodological issues in understanding the role of biology within the relational developmental system* (pp. 1–20). London: Elsevier.

Lerner, R. M., & Benson, P. L. (Eds.). (2003). *Developmental assets and asset-building communities: Implications for research, policy, and practice*. New York: Kluwer.

Lerner, R. M., Bornstein, M. H., & Smith C. (2003). Child well-being: From elements to integrations. In M. H. Bornstein, L. Davidson, C. M. Keyes, K. Moore, & The Center for Child Well-Being. *Well-being: Positive development across the life course* (pp. 501–523). Mahwah, NJ: Lawrence Erlbaum Associates.

Lerner, R. M., & Brackney, B. E. (1978). The importance of inner and outer body parts attitudes in the self-concept of late adolescents. *Sex Roles, 4*, 225–238.

Lerner, R. M., & Busch-Rossnagel, N. A. (Eds.). (1981a). *Individuals as producers of their development: A life-span perspective*. New York: Academic Press.

Lerner, R. M., & Busch-Rossnagel, N. A. (1981b). Individuals as producers of their development: Conceptual and empirical bases. In R. M. Lerner & N. A. Busch-Rossnagel (Eds.), *Individuals as producers of their development: A life-span perspective* (pp. 1–36). New York: Academic Press.

Lerner, R. M., & Callina, K. S. (2014a). The study of character development: Towards tests of a relational developmental systems model. *Human Development, 57*(6), 322–346.

Lerner, R. M., & Callina, K. S. (2014b). Relational developmental systems theories and the ecological validity of experimental designs: Commentary on Freund and Isaacowitz. *Human Development, 56*, 372–380.

Lerner, R. M., Castellino, D. R., Terry, P. A., Villarruel, F. A., & McKinney, M. H. (1995). Developmental contextual perspective on parenting. In M. H. Bornstein (Ed.), *Handbook of parenting: Vol. II Biology and ecology of parenting* (pp. 285–309). Hillsdale, NJ: Erlbaum.

Lerner, R. M., Easterbrooks, M. A., & Mistry, J. (2013). Foundations of development across the life span. In R. M. Lerner, M. A. Easterbrooks, & J. Mistry (Eds.), *Handbook of psychology: Vol. 6 Developmental psychology* (2nd ed.). Editor-in-chief: I. B. Weiner (pp. 3–17). Hoboken, NJ: Wiley.

Lerner, R. M., & Fisher, C. B. (1994). From applied developmental psychology to applied developmental science: Community coalitions and collaborative careers. In C. B. Fisher & R. M. Lerner (Eds.), *Applied developmental psychology* (pp. 505–522). New York: McGraw-Hill.

Lerner, R. M., Fisher, C. B., & Weinberg, R. A. (1997). Applied developmental science: Scholarship for our times. *Applied Developmental Science, 1*, 2–3.

Lerner, R. M., Fisher, C. B., & Weinberg, R. A. (2000a). Toward a science for and of the people: Promoting civil society through the application of developmental science. *Child Development, 71*, 11–20.

Lerner, R. M., Fisher, C. B., & Weinberg, R. A. (2000b). Applying developmental science in the twenty-first century: International scholarship for our times. *International Journal of Behavioral Development*, *24*, 24–29.

Lerner, R. M., Freund, A. M., De Stefanis, I., & Habermas, T. (2001). Understanding developmental regulation in adolescence: The use of the selection, optimization, and compensation model. *Human Development*, *44*, 29–50.

Lerner, R. M., & Gellert, E. (1969). Body build identification, preference, and aversion in children. *Developmental Psychology*, *1*, 456–462.

Lerner, R. M., Harris, E. M., Agans, J. P., Arbeit, M. R., & DeSouza, L. M. (2014). In the wake of Michael Brown and Eric Garner: Applying developmental science to promote social justice. *Society for Research on Adolescence Newsletter*. www.s-r-a.org/announcements/online-newsletter/2014-12-11-wake-michael-brown-and-eric-garner-applying-developmental.

Lerner, R. M., Hultsch, D. F., & Dixon, R. A. (1983). Contextualism and the character of developmental psychology in the 1970s. *Annals of the New York Academy of Sciences, 412*, 101–128.

Lerner, R. M., Johnson, S. K., & Buckingham, M. H. (2015a). Relational developmental systems-based theories and the study of children and families: Lerner and Spanier (1978) revisited. *The Journal of Family Theory and Review, 7*, 83–104.

Lerner, R. M., Johnson, S. K., & Buckingham, M. H. (2015b). It seems like déjà vu all over again: A (friendly) rejoinder. *The Journal of Family Theory and Review, 7*, 118–125.

Lerner, R. M., & Karabenick, S. A. (1974). Physical attractiveness, body attitudes, and self-concept in late adolescents. *Journal of Youth and Adolescence, 3*, 307–316.

Lerner, R. M., Karabenick, S. A., & Meisels, M. (1975a). Effects of age and sex on the development of personal space schemata towards body build. *Journal of Genetic Psychology, 127*, 91–101.

Lerner, R. M., Karabenick, S. A., & Meisels, M. (1975b). One-year stability of children's personal space schemata towards body build. *Journal of Genetic Psychology, 127*, 151–152.

Lerner, R. M., Karabenick, S. A., & Stuart, J. L. (1973). Relations among physical attractiveness, body attitudes, and self-concept in male and female college students. *Journal of Psychology, 85*, 119–129.

Lerner, R. M., & Kauffman, M. B. (1985). The concept of development in contextualism. *Developmental Review, 5*, 309–333.

Lerner, R. M., & Kauffman, M. B. (1986). On the metatheoretical relativism of analyses of metatheoretical analyses: A critique of Kendler's comments. *Developmental Review, 6*, 96–106.

Lerner, R. M., Knapp, J. R., & Pool, K. B. (1974). Structure of body-build stereotypes: A methodological analysis. *Perceptual and Motor Skills, 39*, 719–729.

Lerner, R. M., & Korn, S. J. (1972). The development of body build stereotypes in males. *Child Development, 43*, 908–920.

Lerner, R. M., & Lerner, J. V. (1977). Effects of age, sex, and physical attractiveness on child-peer relations, academic performance, and elementary school adjustment. *Developmental Psychology, 13*, 585–590.

Lerner, R. M., & Lerner, J. V. (1987). Children in their contexts: A goodness-of-fit model. In J. B. Lancaster, J. Altmann, A. S. Rossi, & L. R. Sherrod (Eds.), *Parenting across the life span: Biosocial dimensions* (pp. 377–404). New York: Aldine.

Lerner, R. M., & Lerner, J. V. (1989). Organismic and social contextual bases of development: The sample case of early adolescence. In W. Damon (Ed.), *Child development today and tomorrow* (pp. 69–85). San Francisco, CA: Jossey-Bass.

Lerner, R. M., & Lerner, J. V. (Eds.). (1999). *Theoretical foundations and biological bases of development in adolescence.* New York: Garland.

Lerner, R. M., Lerner, J. V., Almerigi, J. B., Theokas, C., Phelps, E., Gestsdottir, S., et al. (2005). Positive youth development, participation in community youth development programs, and community contributions of fifth-grade adolescents: Findings from the first wave of the 4-H Study of Positive Youth Development. *Journal of Early Adolescence*, *25*(1), 17–71.

Lerner, R. M., Lerner, J. V., Bowers, E., & Geldhof, G. J. (2015). Positive youth development and relational developmental systems. In W. F. Overton & P. C. Molenaar (Eds.), *Handbook of child psychology and developmental science: Vol. 1 Theory and method* (7th ed., pp. 607–651). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Lerner, R. M., Lerner, J. V., Geldhof, G. J., Gestsdottir, S., King, P. E., Sim, A. T. R., Batanova, M., Tirrell, J.,

& Dowling, E. (in press). Studying positive youth development in different nations: Theoretical and methodological issues. In J. J. Lansford & P. Banati (Eds.), *Handbook of adolescent development research and its impact on global policy*. New York: Oxford University Press.

Lerner, R. M., Lerner, J. V., & Jovanovic, J. (1996). Constitutional psychology. In A. M. Kuper & J. Kuper (Eds.), *The social science encyclopedia* (2nd ed., pp. 132–133). London: Routledge.

Lerner, R. M., Lerner, J. V., Urban, J. B., & Zaff, J. F. (2016). Evaluating programs aimed at promoting positive youth development: A relational developmental systems-based view. *Applied Developmental Science, 20*(3), 175–187.

Lerner, R. M., & Miller, J. R. (1993). Integrating human development research and intervention for America's children: The Michigan State University model. *Journal of Applied Developmental Psychology*, *14*, 347–364.

Lerner, R. M., & Miller, J. R. (1998). Developing multidisciplinary institutes to enhance the lives of individuals and families: Academic pitfalls and potentials. *Journal of Public Service & Outreach, 3*(1), 64–73.

Lerner, R. M., & Murray, E. D. (2016). Finally! A developmental science that privileges development: A review of Catherine Raeff, Exploring the Dynamics of Human Development, New York: Oxford University Press, 2016. *Human Development, 59*, 377–385.

Lerner, R. M., & Nesselroade, J. R. (1991). Theory and method in the study of behavioral development: On the legacy of Joachim F. Wohlwill. In P. van Geert & L. P. Mos (Eds.), *Annals of theoretical psychology* (Vol. 7, pp. 177–189). New York: Plenum.

Lerner, R. M., Orlos, J. B., & Knapp, J. R. (1976). Physical attractiveness, physical effectiveness, and self-concept in late adolescents. *Adolescence, 11*, 313–326.

Lerner, R. M., Ostrom, C. W., & Freel, M. A. (1995). Promoting positive youth and community development through outreach scholarship: Comments on Zeldin and Peterson. *Journal of Adolescent Research, 10*, 486–502.

Lerner, R. M., & Overton, W. F. (2008). Exemplifying the integrations of the relational developmental system: Synthesizing theory, research, and application to promote positive development and social justice. *Journal of Adolescent Research, 23*(3), 245–255.

Lerner, R. M., & Overton, W. F. (2017). Reduction to absurdity: Why epigenetics invalidates all models of genetic reductionism. *Human Development, 60*(2–3), 107–123.

Lerner, R. M., & Schroeder, C. (1971a). Kindergarten children's active vocabulary about body build. *Developmental Psychology, 5*, 179.

Lerner, R. M., & Schroeder, C. (1971b). Physique identification, preference, and aversion in kindergarten children. *Developmental Psychology, 5*, 538.

Lerner, R. M., Schwartz, S. J., & Phelps, E. (2009). Problematics of time and timing in the longitudinal study of human development: Theoretical and methodological issues. *Human Development, 52*, 44–68.

Lerner, R. M., & Simon, L. A. K. (Eds.). (1998). *University–community collaborations for the twenty-first century: Outreach scholarship for youth and families*. New York: Garland.

Lerner, R. M., Skinner, E. A., & Sorell, G. T. (1980). Methodological implications of contextual/dialectic theories of development. *Human Development, 23*, 225–235.

Lerner, R. M., Sorell, G. T., & Brackney, B. E. (1981). Sex differences in self-concept and self-esteem of late adolescents: A time-lag analysis. *Sex Roles, 7*, 709–722.

Lerner, R. M., & Spanier, G. B. (Eds.). (1978a). *Child influences on marital and family interaction: A life span perspective*. New York: Academic Press.

Lerner, R. M., & Spanier, G. B. (1978b). A dynamic interactional view of child and family development. In R. M. Lerner & G. B. Spanier (Eds.), *Child influences on marital and family interaction: A life-span perspective* (pp. 1–22). New York: Academic Press.

Lerner, R. M., & Spanier, G. B. (1980). *Adolescent development: A life-span perspective*. New York: McGraw-Hill.

Lerner, R. M., Sparks, E. S., & McCubbin, L. (1999). *Family diversity and family policy: Strengthening families for America's children*. Boston: Kluwer.

Lerner, R. M., Sparks, E. S., & McCubbin, L. (2000). Family diversity and family policy. In D. Demo, K. Allen, & M. Fine (Eds.), *Handbook of family diversity* (pp. 380–401). New York: Oxford University Press.

Lerner, R. M., Theokas, C., & Jelicic, H. (2005). Youth as active agents in their own positive development: A developmental systems perspective. In W. Greve, K.

Rothermund, & D. Wentura (Eds.), *The adaptive self: Personal continuity and intentional self-development* (pp. 31–47). Göttingen, Germany: Hogrefe & Huber Publishers.

Lerner, R. M., & Tolan, P. (2016). On the qualitative transformation of developmental science: The contributions of qualitative methods. *Qualitative Psychology, 3*(1), 120–124.

Lerner, R. M., & von Eye, A. (1992). Sociobiology and human development: Arguments and evidence. *Human Development, 35*, 12–33.

Lerner, R. M., & von Eye, A. (1993). Why Burgess and Molenaar "just don't get it." *Human Development*, *36*, 55–56.

Lerner, R. M., & Walls, T. (1999). Revisiting individuals as producers of their development: From dynamic interactionism to developmental systems. In J. Brandtstädter & R. M. Lerner (Eds.), *Action and self-development: Theory and research through the life-span* (pp. 3–36). Thousand Oaks, CA: Sage.

Lerner, R. M., Wang, J., Hershberg, R. M., Buckingham, M. H., Harris, E. M., Tirrell, J., Bowers, E. P., & Lerner, J. V. (2017). Positive youth development among minority youth: A relational developmental systems model. In N. J. Cabrera & B. Leyendecker (Eds.), *Positive development of minority children* (pp. 5–18). Dordrecht, Netherlands: Springer.

Lerner, R. M., Wiatrowski, M., Kiely, M. K., Napolitano, C. M., Schmid, K.L., & Pritchard, A. (2011). The Positive Youth Development perspective as a new framework for the juvenile justice system: Issues and answers. In F. Sherman & F. Jacobs (Eds.), *Health and wellbeing in the juvenile justice system: Youth in context* (pp. 92–108). Hoboken, NJ: Wiley.

Lester, B. M., Conradt, E., & Marsit, C. (2016). Introduction to the special section on epigenetics. *Child Development, 87*, 29–37.

Leventhal, T., Dupéré, V., & Shuey E. (2015). Children in neighborhoods. In M. H. Bornstein and T. Leventhal (Eds.), *Handbook of child psychology and developmental science: Vol. 4 Ecological settings and processes* (7th ed., pp. 493–533). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Levinson, D. J. (1978). Eras: The anatomy of the life cycle. *Psychiatric Opinion, 15*, 10–11, 39–48.

Levinson, D. J., Darrow, C. N., Klein, E. B., Levinson, M. H., & Mckee, B. (1978). *Seasons of a man's life*. New York: Knopf.

Lewin, K. (1935). *A dynamic theory of personality*. New York: McGraw-Hill.

Lewin, K. (1952). *Field theory in social science: Selected theoretical papers*. London: Tavistock.

Lewin, K. (1954). Behavior and development as a function of the total situation. In L. Carmichael (Ed.), *Manual of child psychology* (2nd ed.). New York: Wiley.

Lewin-Bizan, S., Bowers, E. P., & Lerner, R. M. (2010). One good thing leads to another: Cascades of positive youth development among American adolescents. *Development and Psychopathology*, *22*, 759–770.

Lewis, M. (1972). State as an infant-environment interaction: An analysis of mother–infant behavior as a function of sex. *Merrill-Palmer Quarterly, 18*, 95–121.

Lewis, M. (Ed.). (1983). *Origins of intelligence: Infancy and early childhood* (2nd ed.). New York: Plenum.

Lewis, M. (1990). Intention, consciousness, desires and development. *Psychological Inquiry, 1*(3), 278–283.

Lewis, M. (1993). The emergence of human emotions. In M. Lewis & J. Haviland (Eds.), *Handbook of emotions* (pp. 223–235). New York: Guilford Press.

Lewis, M. (1997). *Altering fate*. New York: Guilford Press.

Lewis, M. (2014). *The rise of consciousness and the development of emotional* life. New York: Guilford Press.

Lewis, M. (2015). Emotional development and consciousness. In W. F. Overton & P. C. Molenaar (Eds.), *Handbook of child psychology and developmental science: Vol. 1 Theory and method* (7th ed., pp. 407–451). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Lewis, M., & Feiring, C. (1978). A child's social world. In R. M. Lerner & G. B. Spanier (Eds.), *Child influences on marital and family interaction* (pp. 47–66). New York: Academic Press.

Lewis, M., & Lee-Painter, S. (1974). An interactional approach to the mother–infant dyad. In M. Lewis & L. A. Rosenblum (Eds.), *The effect of the infant on its caregivers* (pp. 21–48). New York: Wiley.

Lewis, M., & Rosenblum, L. A. (Eds.). (1974). *The effect of the infant on its caregivers*. New York: Wiley.

Lewis, M. D. (2000). The promise of dynamic systems approaches for an integrated account of human development. *Child Development, 71*, 36–43.

Lewontin, R. C. (1973). The apportionment of human diversity. *Evolutionary Biology, 6*, 381–398.

Lewontin, R. C. (1976). The fallacy of biological determinism. *The Sciences, 16*, 6–10.

Lewontin, R. C. (1981). On constraints and adaptation. *The Behavioral and Brain Sciences, 4*, 244–245.

Lewontin, R. C. (1992). Foreword. In R. M. Lerner (Ed.), *Final solutions: Biology, prejudice, and genocide* (pp. vii–viii). University Park, PA: Penn State Press.

Lewontin, R. C. (2000). *The triple helix*. Cambridge, MA: Harvard University Press.

Lewontin, R. C. (2011). It's even less in your genes. *The New York Review of Books, 58*(9).

Lewontin, R. C., & Levins, R. (1978). Evolution. *Encyclopedia Einaudi* (Vol. 5). Turin: Einaudi.

Lewontin, R. C., Rose, S., & Kamin, L. J. (1984). *Not in our genes: Biology, ideology, and human nature*. New York: Pantheon Press.

Liben, L. S., & Coyle, E. F. (2017). Gender development: A relational approach. In A. S. Dick & U. Müller (Eds.), *Advancing developmental science: Philosophy, theory, and method* (pp. 170–184). New York: Routledge.

Lickliter, R. (2016). Developmental evolution. *WIREs Cogn Sci* 2016. doi: 10.1002/wcs.1422.

Lickliter, R., & Honeycutt, H. (2003). Developmental dynamics: Toward a biologically plausible evolutionary psychology. *Psychological Bulletin, 129*, 819–835.

Lickliter, R., & Honeycutt, H. (2015). Biology, development, and human systems. In W. F. Overton & P. C. M. Molenaar (Eds.), *Handbook of child psychology and developmental science: Vol. 1 Theory and method* (7th ed., pp. 162–207). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Lifton, R. J. (1986). *The Nazi doctors: Medical killing and the psychology of genocide*. New York: Basic Books.

Lips, H. M. (2001). *Sex and gender: An introduction* (4th ed., pp. 60–64). Mountain View, CA: Mayfield.

Little, T. D. (2013). *Longitudinal structural equation modeling*. New York: Guilford.

Little, T. D., Card, N. A., Preacher, K. J., & McConnell, E. (2009). Modeling longitudinal data from research on adolescence. In R. M. Lerner & L. Steinberg (Eds.), *Handbook of adolescent psychology: Vol. 1 Individual bases of adolescent development* (3rd ed., pp. 15–54). Hoboken, NJ: Wiley.

Loevinger, J. (1976). *Ego development*. San Francisco: Jossey-Bass.

Lonigan, C. L. (2015). Literacy development. In L. S. Liben & U. Mueller (Eds.), *Handbook of child psychology and developmental science: Vol. 2 Cognitive processes* (7th ed., pp. 763–805). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Looft, W. R. (1972). The evolution of developmental psychology. *Human Development, 15*, 187–201.

Looft, W. R. (1973). Socialization and personality throughout the life-span: An examination of contemporary psychological approaches. In P. B. Baltes & K. W. Schaie (Eds.), *Life-span developmental psychology: Personality and socialization* (pp. 25–52). New York: Academic Press.

Lorenz, K. (1932/1970). A consideration of methods of identification of species-specific instinctive patterns in birds. In R. Martin (Ed. and Trans.), *Studies in animal and human behavior* (Vol. 1, pp. 57–100). Cambridge, MA: Harvard University Press.

Lorenz, K. (1935). Der Kumpan in der Umwelt des Vogels. *Journal für Ornithologie, 83*, 137–215, 289–413.

Lorenz, K. (1937a). The companion in the bird's world. *Auk, 54*, 245–273.

Lorenz, K. (1937b). Über den Begriff der Instinkthandlung, *Folia Biotheoretica, 2*, 17–50.

Lorenz, K. (1937c). Über die Bildung des Instinktbegriffes. *Die Naturwissenchaften, 25*, 189–300, 307–308, 324–333.

Lorenz, K. (1939). Über Ausfallerserscheinungen im Instinctverhalten von Haustieren und ihre socialpsychologische Bedeutung. In O. Klemm (Ed.), *Charackter und Erziehung: 16. Kongress der Deutschen Gesellschaft für Psychologie in Bayreuth* (pp. 139–147). Leipzig: J. A. Barth.

Lorenz, K. (1940a). Durch Domestikation verursachte Störungen arteigenen Verhaltens. *Zeitschrift für angewandte Psychologie und Charakterkunde, 59*, 2–81.

Lorenz, K. (1940b). Systematik und Entwicklungsgedanke im Unterricht, *Der Biologe, 9*, 24–36.

Lorenz, K. (1941). Kants Lehre vom Apriorischen im Lichte gegenwärtiger Biologie. *Blätter für deutsche Philosophie, 15*, 94–125.

Lorenz, K. (1943a). Die angeborenen Formen möglicher Erfahrung. *Zeitschrift für Tierpsychologie, 5*, 235–409.

Lorenz, K. (1943b). Psychologie und Stammesgeschichte. In G. Heberer (Ed.), *Die Evolution der Organismen* (pp. 105–127). Jena: G. Fischer.

Lorenz, K. (1950). Ganzheit und Teil in der tierischen und menschilchen *Gemeinschaft. Studium Generale, 9*, 455–498.

Lorenz, K. (1954). Psychologie und Stammesgeschichte. In G. Heberer (Ed.), *Die Evolution der Organismen* (2nd ed., pp. 105–127). Jena: G. Fisher.

Lorenz, K. (1965). *Evolution and modification of behavior*. Chicago: University of Chicago Press.

Lorenz, K. (1966). *On aggression*. New York: Harcourt, Brace & World.

Lorenz, K. (1974a). *Civilized man's eight deadly sins*. New York: Harcourt Brace Jovanovich.

Lorenz, K. (1974b). Interview reported in V. Cox, "A prize for the Goose Father." *Human Behavior, 2*, 17–22.

Lorenz, K. (1974c). Letter: Lorenz clarifies ideas. *Human Behavior*, September, 6.

Lorenz, K. (1975). Konrad Lorenz responds to Donald Campbell. In R. I. Evans (Ed.), *Konrad Lorenz: The man and his ideas* (pp. 119–128). New York: Harcourt Brace Jovanovich.

Lovejoy, C. O. (1981). The origin of man. *Science, 211*, 341–350.

Lowenthal, M. F., Thurnher, M., & Chiriboga, D. (1975). *Four stages of life: A comparative study of women and men facing transitions*. San Francisco: Jossey-Bass.

Lumsden, C. J., & Wilson, E. O. (1981). *Genes, mind, and culture*. Cambridge, MA: Harvard University Press.

Lumsden, C. J., & Wilson, E. O. (2006). *Genes, mind, and culture: The coevolutionary process*. Hackensack, NJ: World Scientific Publishing.

Lush, J. L. (1937). *Animal breeding plans*. Ames, IA: State College Press.

Lush, J. L. (1940). Intra-sire correlations or regressions of offspring on dam as a method of estimating heritability of characteristics. *Thirty-Third Annual Proceedings of the American Society of Animal Production, 56*, 622–629.

Luthar, S. S., Crossman, E. J., & Small, P. J. (2015). Resilence and adversity. In M. E. Lamb (Ed.), *Handbook of child psychology and developmental science: Vol. 3 Socioemotional processes* (7th ed., pp. 247–286). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Lykken, D. T. (1982). Research with twins: The concept of emergenesis. *Psycho-physiology, 19*, 361–373.

Lynch, A. D., Ferris, K. A., Burkhard, B., Wang, J., Hershberg, R. M., & Lerner, R. M. (2016). Character development within youth development programs: Exploring multiple dimensions of activity involvement. *American Journal of Community Psychology, 57*(1–2), 73–86. doi: 10.1002/ajcp.12035.

Lynn, R. (1993). Further evidence for the existence of race and sex differences in cranial capacity. *Social Behavior and Personality, 21*, 89–92.

Mabry, P. L., & Kaplan, R. M. (2013). Systems science: A good investment for the public's health. *Health Education & Behavior, 40*(1 Suppl.), 9S–12S.

MacDonald, K. (1985). Early experience, relative plasticity, and social development. *Developmental Review, 5*, 99–121.

MacDonald, K. (Ed.). (1988). *Sociobiological perspectives on human development*. New York: Springer-Verlag.

MacDonald, K. (1994). *A people that shall dwell alone: Judaism as an evolutionary group strategy*. Westport, CT: Greenwood.

MacWhinney, B. (2015) Language development. In L. S. Liben & U. Mueller (Eds.), *Handbook of child psychology and developmental science: Vol. 2 Cognitive processes* (7th ed., pp. 296–338). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Magnusson, D. (1995). Individual development: A holistic integrated model. In P. Moen, G. H. Elder, Jr., & K. Lusher (Eds.), *Linking lives and contexts: Perspectives on the ecology of human development* (pp. 19–60). Washington, DC: APA Books.

Magnusson, D. (1996). *The life-span development of individuals: Behavioral, neurobiological, and psychosocial perspectives. A synthesis*. Cambridge, UK: Cambridge University Press.

Magnusson, D. (1999a). Holistic interactionism: A perspective for research on personality development. In L. A. Pervin & O. P. John (Eds.), *Handbook of personality: Theory and research* (2nd ed., pp. 219–247). New York: Guilford Press.

Magnusson, D. (1999b). On the individual: A person-oriented approach to developmental research. *European Psychologist, 4*, 205–218.

Magnusson, D. (2000). Developmental science. In A. E. Kazdin (Ed.), *Encyclopedia of psychology* (Vol. 3, pp. 24–26). New York: American Psychological Association and Oxford University Press.

Magnusson, D., & Cairns, R. B. (1996). Developmental science: Principles and illustrations. In R. B. Cairns, G. H. Elder, Jr., & E. J. Costello (Eds.), *Developmental science* (pp. 7–30). New York: Cambridge University Press.

Magnusson, D., & Endler, N. S. (1977). Interactional psychology: Present status and future prospects. In D. Magnusson & N. S. Endler (Eds.), *Personality at the crossroads: Current issues in interactional psychology* (pp. 3–31). Hillsdale, NJ: Erlbaum.

Magnusson, D., & Stattin, H. (1998). Person–context interaction theories. In W. Damon (Series Ed.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (5th ed., pp. 685–759). New York: Wiley.

Magnusson, D., & Stattin, H. (2006). The person in context: A holistic-interactionist approach. In R. M. Lerner (Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (6th ed., pp. 400–464). Editors-in-chief: W. Damon & R. M. Lerner. Hoboken, NJ: Wiley.

Maier, N. R. F., & Schneirla, T. C. (1935). *Principles of animal behavior*. New York: McGraw-Hill.

Mariano, J. M., & Damon, W. (2008). The role of spirituality and religious faith in supporting purpose in adolescence. In R. M. Lerner, R. Roeser, and E. Phelps (Eds.), *Positive youth development and spirituality: From theory to research* (pp. 210–230). West Conshohocken, PA: Templeton Foundation Press.

Mariano, J. M., & Going, J. (2011). Youth purpose and positive youth development. In R. M. Lerner, J. V. Lerner, & J. B. Benson (Eds.), *Advances in child development and behavior: Positive youth development: Research and applications for promoting thriving in adolescence* (pp. 39–68). Amsterdam: Elsevier Publishing.

Marks, A. K., Ejesi, K., McCullough, M. B., & Coll, C. G. (2015). Developmental implications of discrimination. In M. E. Lamb (Vol. Ed.), *Handbook of child psychology and developmental science: Vol. 3 Socioemotional processes* (7th ed., pp. 324–365). Editor-in-Chief: R. M. Lerner. Hoboken, NJ: Wiley.

Marshall, P. J. (2015). Neuroscience, embodiment, and development. In W. F. Overton & P. C. Molenaar (Eds.), *Handbook of child psychology and developmental science: Vol. 1 Theory and method* (7th ed., pp. 244–283). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Marsiske, M., Lang, F. B., Baltes, P. B., & Baltes, M. M. (1995). Selective optimization with compensation: Life-span perspectives on successful human development. In R. A. Dixon & L. Baeckman (Eds.), *Compensating for psychological deficits and declines: Managing losses and promoting gains* (pp. 35–79). Mahwah, NJ: Lawrence Erlbaum Associates.

Martens, E. P., Pestman, W. R., de Boer, A., Belitser, S. V., & Klungel, O. H. (2006). Instrumental variables: Application and limitations. *Epidemiology*, *17*(3), 260–267.

Mascolo, M. (2013). Developing through relationships: An embodied coactive systems framework. In R. M. Lerner & J. B. Benson, (Eds.), *Advances in child development and behavior: Embodiment and epigenesis: Theoretical and methodological issues in understanding the role of biology within the relational developmental system. Part B. Ontogenetic dimensions* (pp. 185–225). London: Elsevier.

Mascolo, M. F., & Fischer, K. W. (2010). The dynamic development of thinking, feeling, and acting over the life span. In R. M. Lerner & W. F. Overton (Eds.), *Handbook of life-span development: Vol. 1 Cognition, biology, and meth*ods (pp. 149–194). Hoboken, NJ: Wiley.

Mascolo, M. F. & Fischer, K. W. (2015) Dynamic development of thinking, feeling, and acting. In W. F. Overton & P. C. Molenaar (Eds.), *Handbook of child psychology and developmental science: Vol. 1 Theory and method* (7th ed., pp. 113–161). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Mason, W. A., & Kenney, M. (1974). Redirection of filial attachments in rhesus monkeys: Dogs as surrogate mothers. *Science, 183*, 1209–1211.

Masten, A. S. (2001). Ordinary magic: Resilience processes in development. *American Psychologist*, *56*, 227–238.

Masten, A. S. (2014). *Ordinary magic: Resilience in development*. New York: Guilford Press.

Masten, A. S., Narayan, A. J., Silverman, W. K., & Osofsky, J. D. (2015). Children in war and disaster. In M. H. Bornstein and T. Leventhal (Eds.), *Handbook of child psychology and developmental science: Vol. 4 Ecological settings and processes* (7th ed., pp. 704–745). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Masters, R. D. (1978). Jean-Jacques is alive and well: Rousseau and contemporary sociobiology. *Daedalus, 107*, 93–105.

Mayr, E., & Provine, W. (1980). *The evolutionary synthesis*. Cambridge, MA: Harvard University Press.

McAdoo, H. P. (1977). A review of the literature related to family therapy in the Black community. *Journal of Contemporary Psychotherapy, 9*, 15–19.

McAdoo, H. (1991). Family values and outcomes for children. *Journal of Negro Education, 60*, 361–365.

McAdoo, H. (1993a). Family equality and ethnic diversity. In K. Altergott (Ed.), *One world, many families* (pp. 52–55). Minneapolis, MN: National Council on Family Relations.

McAdoo, H. (1993b). The social cultural contexts of ecological developmental family models. In P. Boss, W. Doherty, & W. Schyumm (Eds.), *Sourcebook of family theories and methods: A contextual approach* (pp. 298–301). New York: Plenum.

McAdoo, H. P. (1995). Stress levels, family help patterns, and religiosity in middle- and working-class African American single mothers. *Journal of Black Psychology, 21*, 424–449.

McAdoo, H. (1998). African American families: Strength and realities. In H. I. McCubbin, E. A. Thompson, A. I. Thompson, & J. E. Fromer (Eds.), *Resiliency in ethnic minority families: African American families* (pp. 17–30). Thousand Oaks, CA: Sage.

McAdoo, H. P. (1999). Diverse children of color. In H. E. Fitzgerald, B. M. Lester, & B. S. Zuckerman (Eds.), *Children of color: Research, health, and policy issues* (pp. 205–218). New York: Garland Publishing.

McCall, R. B. (1981). Nature–nurture and the two realms of development: A proposed integration with respect to mental development. *Child Development, 52*, 1–12.

McCall, R. B., & Green, B. L. (2004). Beyond the methodological gold standard of behavioral research: Considerations for practice and policy. *SRCD Social Policy Reports*, *18*, 1–19.

McCandless, B. R. (1967). *Children*. New York: Holt, Rinehart & Winston.

McCandless, B. R. (1970). *Adolescents*. Hinsdale, IL: Dryden Press.

McClearn, G. E. (1970). Genetic influences on behavior and development. In P. H. Mussen (Ed.), *Carmichael's manual of child psychology* (Vol. 1, pp. 39–76). New York: Wiley.

McClearn, G. E. (1981). Evolution and genetic variability. In E. S. Gollin (Ed.), *Developmental plasticity: Behavioral and biological aspects of variations in development* (pp. 3–31). New York: Academic Press.

McClearn, G. E., & DeFries, J. C. (1973). *Introduction to behavioral genetics*. San Francisco: W. H. Freeman.

McClelland, M. M., Geldhof, G. J., Cameron, C. E., & Wanless, S. B. (2015). Development and self-regulation. In W. F. Overton & P. C. Molenaar (Eds.), *Handbook of child psychology and developmental science: Vol. 1 Theory and method* (7th ed., pp. 523–565). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

McCluskey, K. A., & Reese, H. W. (Eds.). (1984). *Life-span developmental psychology: Historical and generational effects*. Orlando, FL: Academic Press.

McCrae, R. R., Costa, P. T., Jr., Ostendorf, F., Angleitner, A., Hrebícková, M., Avia, M. D., Sanz, J., Sánchez-Bernardos, M. L., Kusdil, M. E., Woodfield, R., Saunders, P. R., & Smith, P. B. (2000). Nature over nurture: Temperament, personality, and life span development. *Journal of Personality and Social Psychology*, *78*, 173–186.

McCubbin, H. I, Thompson, E. A., Thompson, A. I., & Futrell, J. A. (Eds.). (1998). *Resiliency in ethnic minority families: African-American families*. Thousand Oaks, CA: Sage.

McEwen, B. S. (1997). Possible mechanisms for atrophy of the human hippocampus. *Molecular Psychiatry, 2*, 255–262.

McEwen, B. S. (1998). Protective and damaging effects of stress mediators. *New England Journal of Medicine, 338*, 171–179.

McEwen, B. S. (1999). Stress and hippocampal plasticity. *Annual Review of Neuroscience, 22*, 105–122.

McGrath, J. E., & Tschan, F. (2004). *Temporal matters in social psychology: Examining the role of time in the lives of groups and individuals*. Washington: American Psychological Association.

McGraw, M. B. (1943). *The neuromuscular maturation of the human infant*. New York: Columbia University Press.

McGue, M. (1981). Familial studies of intelligence: A review. *Science, 212*, 1055–1059.

McHale, S. M., & Lerner, R. M. (1985). Stages of human development. In T. Husen & T. N. Postlethwaite (Eds.), *The international encyclopedia of education* (pp. 2327–2331). Oxford: Pergamon Press.

McHale, S. M., & Lerner, R. M. (Eds.). (1996). University–community collaborations on behalf of youth. *Journal of Research on Adolescence, 6*, 1–86.

McLaughlin, B. (1977). Second language learning in children. *Psychological Bulletin, 84*, 438–59.

McLoyd, V. C. (1998). Children in poverty: Development, public policy, and practice. In W. Damon (Ed.), I. E. Sigel (Vol. Ed.), and K. A. Renninger (Vol. Ed.), *Handbook of psychology: Child psychology in practice* (Vol. 4). New York: John Wiley & Sons.

McLoyd, V. C., Purtell, K. M., & Hardaway, C. R. (2015). Race, class, and ethnicity in young adulthood. In M. E. Lamb (Ed.), *Handbook of child psychology and developmental science: Vol. 3 Socioemotional processes* (7th ed., pp. 366–418). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

McNeill, D. (1966). Developmental psycholinguistics. In F. Smith & G. A. Miller (Eds.), *The genesis of language: A psycholinguistic approach*. Cambridge, MA: MIT Press.

Meaney, M. (2010). Epigenetics and the biological definition of gene x environment interactions. *Child Development, 81*, 41–79.

Meaney, M. (2014). Epigenetics offer hope for disadvantaged children. *Child & Family Blog*. https://childandfamilyblog.com/epigenetics-offer-hope-disadvantaged-children.

Meaney, M., Aitken, D., Berkel, H., Bhatnager, S., & Sapolsky, R. (1988). Effect of neonatal handling of age-related impairments associated with the hippocampus. *Science, 239*, 766–768.

Meece, J., & Eccles, J. S. (2008). *Handbook of research on schools, schooling and human development*. London: Routledge.

Meyer, J. W. (1988). The social constructs of the psychology of childhood: Some contemporary processes. In E. M. Hetherington, R. M. Lerner, & M. Perlmutter (Eds.), *Child development in life-span perspective* (pp. 47–65). Hillsdale, NJ: Erlbaum.

Michel, G. F. (2010). The roles of environment, experience, and learning in behavioral development. In K. E. Hood, C. T. Halpern, G. Greenberg, & R. M. Lerner (Eds.), *Handbook of developmental systems, behavior and genetics* (pp. 123–165). Malden, MA: Wiley Blackwell.

Michel, G. F., & Moore, C. L. (1995). *Developmental psychobiology: An interdisciplinary science*. Cambridge, MA: MIT Press.

Michel, G. F., & Tyler, A. N. (2005). Critical period: A history of the transition from questions of when, to what, to how. *Developmental Psychobiology, 46*, 156–162.

Midgley, C., Feldlaufer, H., & Eccles, J. S. (1989a). Changes in teacher efficacy and student self- and task-related beliefs in mathematics during the transition to junior high school. *Journal of Educational Psychology, 81*, 247–258.

Midgley, C., Feldlaufer, H., & Eccles, J. S. (1989b). Student/teacher relations and attitudes toward mathematics before and after the transition to junior high school. *Child Development, 60*, 981–992.

Miller, N. E., & Dollard, J. (1941). *Social learning and imitation*. New Haven: Yale University Press.

Miller J. R., &. Lerner, R. M. (1994). Integrating research and outreach: Developmental contextualism and the human ecological perspective. *Home Economics Forum, 7*, 21–28.

Millett, R. A. (1996). Empowerment evaluation and the W. K. Kellogg Foundation. In D. M. Fetterman, S. J. Kaftarian, & A. Wandersman (Eds.), *Empowerment evaluation: Knowledge and tools for self-assessment and accountability* (pp. 65–76). Thousand Oaks, CA: Sage.

Mischel, W. (1968). *Personality and assessment*. New York: Wiley.

Mischel, W. (1973). Toward a cognitive social learning reconceptualization of personality. *Psychological Review, 80*, 252–283.

Mischel, W. (1977). On the future of personality measurement. *American Psychologist, 32*, 246–254.

Mischel, W. (2007). Toward a science of the individual. In Y. Shoda, D. Cervone, & G. Downey (Eds.), *Persons in context: Building a science of the individual* (pp. 263–277). New York: Guilford.

Mischel, W., & Mischel, H. N. (1976). A cognitive social-learning approach to morality and self-regulation. In T. Lickona (Ed.), *Moral development and behavior*. New York: Holt, Rinehart, & Winston.

Misiak, H., & Sexton, V. S. (1966). *History of psychology in overview*. New York: Grune & Stratton.

Misiak, H., & Staudt, V. S. (1954). *Catholics in psychology: A historical survey*. New York: McGraw-Hill.

Misteli, T. (2013). The cell biology of genomes: Bringing the double helix to life. *Cell, 152*, 1209–1212.

Mistry, J., & Dutta, R. (2015). Human development and culture: Conceptual and methodological issues. In W. F. Overton & P. C. Molenaar (Eds.), *Handbook of child psychology and developmental science: Vol. 1 Theory and method* (7th ed., pp. 369–406). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Mistry, J., & Wu, J. (2010). Navigating cultural worlds and negotiating identities: A conceptual model. *Human Development, 53*, 5–25.

Mitchell, D. E. (1978). Recovery of vision in monocularly and binocularly deprived kittens. Paper presented at the Eleventh Symposium of the Center of the Visual Science, June, Rochester, NY.

Mitchell, D. E., Cynader, M., & Movshon, J. A. (1977). Recovery from the effects of monocular deprivation in kittens. *Journal of Comparative Neurology, 176*, 53–64.

Molenaar, P. C. M. (2007). On the implications of the classical ergodic theorems: Analysis of developmental processes has to focus on intra-individual variation. *Developmental Psychobiology, 50*, 60–69.

Molenaar, P. C. M. (2010). On the limits of standard quantitative genetic modeling of inter-individual variation: Extensions, ergodic conditions and a new genetic factor model of intra-individual variation. In K. E. Hood, C. T. Halpern, G. Greenberg, & R. M. Lerner (Eds.), *Handbook of developmental science, behavior, and genetics* (pp. 626–648). Malden, MA: Blackwell.

Molenaar, P. C. M. (2014). Dynamic models of biological pattern formation have surprising implications for understanding the epigenetics of development. *Research in Human Development, 11*, 50–62.

Molenaar, P. C. M., Boomsma, D. I., Neeleman, D., & Dolan, C. V. (1990). Using factor scores to detect G x E interactive origin of "pure" genetic or environmental factors obtained in genetic covariance structure analysis. *Genetic Epidemiology, 7*, 93–100.

Molenaar, P. C. M., Lerner, R. M., & Newell, K. (Eds.). (2014). *Handbook of developmental systems theory and methodology*. New York: Guilford.

Molenaar, P. C. M., & Nesselroade, J. R. (2012). Merging the idiographic filter with dynamic factor analysis to model process. *Applied Developmental Science, 16*, 210–219.

Molenaar, P. C. M., & Nesselroade, J. R. (2014). New trends in the inductive use of relation developmental systems theory: Ergodicity, nonstationarity, and heterogeneity. In P. C. Molenaar, R. M. Lerner, & K. M. Newell (Eds.), *Handbook of developmental systems and methodology* (pp. 442–462). New York: Guilford.

Molenaar, P. C. M., & Nesselroade, J. R. (2015). Systems methods for developmental research. In W. F. Overton & P. C. M. Molenaar (Eds.), *Handbook of child psychology and developmental science: Vol. 1 Theory and method* (7th ed., pp. 652–682). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Molenaar, P. C. M, & Newell, K. M. (Eds.). (2010). *Individual pathways of change: Statistical models for analyzing learning and development*. Washington, DC: American Psychological Association.

Molenaar, P. C. M., Smit, D. J. A., Boomsma, D. I., & Nesselroade, J. R. (2012). Estimation of specific heritabilities from intra-individual variation: iFace. *Twin Research an Human Genetics, 15*(3), 393–400.

Moltz, H. (1965). Contemporary instinct theory and the fixed action pattern. *Psychological Review, 72*, 27–47.

Moltz, H. (1973). Some implications of the critical period hypothesis. *Annals of the New York Academy of Sciences, 223*, 144–146.

Moltz, H., & Stettner, L. J. (1961). The influence of patterned-light deprivation on the critical period for imprinting. *Journal of Comparative and Physiological Psychology, 54*, 279–283.

Moore, D. S. (2002). *The dependent gene: The fallacy of nature vs. nurture*. New York: W. H. Freeman.

Moore, D. S. (2006). A very little bit of knowledge: Re-evaluating the meaning of the heritability of IQ. *Human Development, 49*, 347–353.

Moore, D. S. (2013). Behavioral genetics, genetics, and epigenetics. In P. D. Zelazo (Ed.), *Oxford handbook of developmental psychology* (pp. 91–128). New York: Oxford University Press.

Moore, D. S. (2015a). *The developing genome: An introduction to behavioral epigenetics*. New York: Oxford University Press.

Moore, D. S. (2015b). The asymmetrical bridge. Book review of James Tabery's *Beyond versus: The struggle to understand the interaction of nature and nurture*. *Acta Biotheoretica, 63*(4), 413–427.

Moore, D. S. (2016). Behavioral epigenetics. *WIREs Cogn Sci* 2016. doi: 10.1002/wcs.1333.

Moore, D. S., & Shenk, D. (2016). The heritability fallacy. *WIREs Cogn Sci* 2016. doi: 10.1002/wcs.1400.

Moores, E., Nicolson, R. I., & Fawcett, A. J. (2003). Attention deficits in dyslexia: Evidence for an automatisation deficit? *European Journal of Cognitive Psychology*, *15*, 321–348.

Müller, U. & Kerns, K. (2015). The development of executive function. In L. S. Liben & U. Mueller (Eds.), *Handbook of child psychology and developmental science: Vol. 2 Cognitive processes* (7th ed., pp. 571–623). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Müller-Brettel, M., & Dixon, R. A. (1990). Johann Nicolas Tetens: A forgotten father of developmental psychology? *International Journal of Behavioral Development, 13*, 215–230.

Müller-Hill, B. (1988). *Murderous science: Elimination by scientific selection of Jews, Gypsies, and others. Germany 1933–1945* (G. R. Fraser, Trans.). New York: Oxford University Press.

Murchison, C. (Ed.). (1931). *Handbook of child psychology*. Worcester, MA: Clark University Press.

Murray, H. A. (1938). *Explorations in personality*. New York: Oxford University Press.

Murray, H. A. (1959). Preparations for the scaffold of a comprehensive system. In S. Koch (Ed.), *Psychology: A study of a science* (Vol. 3). New York: McGraw-Hill.

Murry, V. M., Hill, N. E., Witherspoon, D., Berkel, C., & Bartz, D. (2015). Children in diverse social contexts. In M. H. Bornstein and T. Leventhal (Eds.), *Handbook of child psychology and developmental science: Vol. 4 Ecological settings and processes* (7th ed., pp. 416–454). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Mussen, P. H. (Ed.). (1970). *Carmichael's manual of child psychology* (3rd ed.). New York: Wiley.

Mussen, P. H. (Ed.). (1983). *Handbook of child psychology* (4th ed.). New York: Wiley.

Muthén, B., & Muthén, L.K. (2000). Integrating person-centered and variable-centered analyses: Growth mixture modeling with latent trajectory classes. *Alcoholism: Clinical and Experimental Research, 24*, 882–891.

Muuss, R. E. (Ed.). (1975). *Adolescent behavior and society: A book of readings* (2nd ed.). New York: Random House.

Muuss, R. E. (1996). *Theories of adolescence* (6th ed.). New York: McGraw-Hill.

Nagel, E. (1957). Determinism in development. In D. B. Harris (Ed.), *The concept of development* (pp. 15–24). Minneapolis: University of Minnesota Press.

Nagin, D. S. (2005). *Group-based modeling of development.* Cambridge, MA: Harvard University Press.

Narvaez, D. (2008). Human flourishing and moral development: Cognitive and neurobiological perspectives of virtue development. In L. Nucci and D. Narvaez (Eds.), *Handbook of moral and character education* (pp. 310–327). Abingdon: Routledge.

Nash, J. (1978). *Developmental psychology: A psychobiological approach*. Englewood Cliffs, NJ: Prentice-Hall.

National Human Genome Research Institute. (2001). International Human Genome Sequencing Consortium publishes sequence and analysis of the human genome. www.genome.gov/10002192/2001-release-first-analysis-of-human-genome.

Neimark, E. D. (1978). Improper questions cannot be properly answered. *The Behavioral and Brain Sciences, 2*, 195–196.

Nelson, C. A. (1999). How important are the first 3 years of life? *Applied Developmental Science, 3*(4), 235–238.

Nesselroade, J. R. (1970). Application of multivariate strategies to problems of measuring and structuring long-term change. In L. R. Goulet & P. B. Baltes (Eds.), *Life-span developmental psychology: Theory and research* (pp. 193–207). New York: Academic Press.

Nesselroade, J. R. (1977). Issues in studying developmental change in adults from a multivariate perspective. In J. E. Birren & K. W. Schaie (Eds.), *The handbook of the psychology of aging* (pp. 59–69). New York: Van Nostrand Reinhold.

Nesselroade, J. R. (1983). Implications of the trait-state distinction for the study of aging: Still labile after all these years. Presidential address to Division 20. Ninety-First Annual Convention of the American Psychological Association, August, Anaheim, CA.

Nesselroade, J. R. (1988). Some implications of the trait-state distinction for the study of development over the life-span: The case of personality. In P. B. Baltes, D. L. Featherman, & R. M. Lerner (Eds.), *Life-span development and behavior* (Vol. 8, pp. 163–189). Hillsdale, NJ: Erlbaum.

Nesselroade, J. R., & Baltes, P. B. (1974). Adolescent personality development and historical change: 1970–1972. *Monographs of the Society for Research in Child Development, 39*, 1–80.

Nesselroade, J. R., & Baltes, P. B. (Eds.). (1979). *Longitudinal research in the study of behavior and development*. New York: Academic Press.

Nesselroade, J. R., & Ford, D. H. (1985). P-technique comes of age: Multivariate, replicated, single subject designs for research on older adults. *Research on Aging, 7*, 46–80.

Nesselroade, J. R., & Molenaar, P. C. M. (2010). Emphasizing intraindividual variability in the study of development over the life span. In W. F. Overton (Ed.), *Handbook of life-span development: Vol. 1 Cognition, biology, methods* (pp. 30–54). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Nesselroade, J. R., & Ram, N. (2004). Studying intraindividual variability: What we have learned that will help us understand lives in context. *Research in Human Development, 1*, 9–29.

Nesselroade, J. R., & Reese, H. W. (Eds.). (1973). *Life-span developmental psychology: Methodological issues*. New York: Academic Press.

Neugarten, B. L. (1964). *Personality in middle and late life*. New York: Atherton Press.

Neugarten, B. L. (Ed.). (1968). *Middle age and aging*. Chicago: University of Chicago Press.

Neugarten, B. L., & Datan, N. (1973). Sociological perspective on the life cycle. In P. B. Baltes & K. W. Schaie (Eds.), *Life-span developmental psychology: Personality and socialization* (pp. 53–69). New York: Academic Press.

Neugarten, B. L., & Gutmann, D. L. (1958). Age-sex roles and personality in middle age: A thematic apperception study. *Psychological Monographs, 72* (Whole No. 470).

Neugarten, B. L., & Gutmann, D. L. (1968). Age-sex roles and personality in middle age: A thematic apperception study. In B. L. Neugarten (Ed.), *Middle age and aging* (pp. 58–74). Chicago: University of Chicago Press.

Neugarten, B. L., Havighurst, R. J., & Tobin, S. S. (1968). Personality and patterns of aging. In B. L. Neugarten (Ed.), *Middle age and aging* (pp. 173–177). Chicago: University of Chicago Press.

Newell, K. M., & Molenaar, P. C. M. (Eds.). (1998). *Applications of nonlinear dynamics to developmental process modeling.* Mahwah, NJ: Erlbaum.

Newman, H. H., Freeman, F. N., & Holzinger, K. J. (1937). *Twins: A study of heredity and environment.* Chicago: University of Chicago Press.

Nisbett, R. A. (1977). *Konrad Lorenz: A biography*. New York: Harcourt Brace Jovanovich.

Nisbet, R. A. (1980). *History of the idea of progress*. New York: Basic Books.

Noble, D. (2012). A theory of biological relativity: No privileged level of causation. *Interface Focus, 2*, 55–64.

Noble, D. (2015). Evolution beyond neo-Darwinism: A new conceptual framework. *The Journal of Experimental Biology*, *218*, 7–13.

Novak, M. A., & Harlow, H. F. (1975). Social recovery of monkeys isolated for the first year of life: I. Rehabilitation and therapy. *Developmental Psychology, 11*, 453–465.

Novikoff, A. B. (1945a). The concept of integrative levels and biology. *Science, 101*, 209–215.

Novikoff, A. B. (1945b). Continuity and discontinuity in evolution. *Science, 101*, 405–406.

Nucci, L. (1996). Morality and the personal sphere of actions. In E. Reed, E. Turiel, & T. Brown (Eds.), *Knowledge and values* (pp. 41–60). Hillsdale, NJ: Erlbaum.

Nucci, L. P. (2001). *Education in the moral domain*. Cambridge, UK: Cambridge University Press.

Nucci, L. (2017). Character: A multi-faceted developmental system. *Journal of Character Education*, *13*(1), 1–16.

Offer, D. (1969). *The psychological world of the teenager*. New York: Basic Books.

Olson, D. R. (1978). A structuralist view of explanations: A critique of Brainerd. *The Behavioral and Brain Science, 2*, 197–199.

Oppenheimer, L. (1991a). The concept of action: A historical perspective. In L. Oppenheimer and J. Valsiner (Eds.), *The origins of action: Interdisciplinary and international perspectives* (pp. 1–35). New York: Springer-Verlag.

Oppenheimer, L. (1991b). Determinants of action: An organismic and holistic approach. In L. Oppenheimer & J. Valsiner (Eds.), *The origins of action: Interdisciplinary and international perspectives* (pp. 37–63). New York: Springer-Verlag.

Orgel, L. E., & Crick, F. H. C. (1980). Selfish DNA: The ultimate parasite. *Nature, 284*, 604–607.

Oser, F. K., Scarlett, W. G., & Bucher, A. (2006). Religious and spiritual development throughout the life span. In R. M. Lerner (Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (6th ed., pp. 942–998). Editors-in-chief: W. Damon & R. M. Lerner. Hoboken, NJ: Wiley.

Ostrom, C. W., Lerner, R. M., & Freel, M. A. (1995). Building the capacity of youth and families through university–community collaborations: The development-in-context evaluation (DICE) model. *Journal of Adolescent Research, 10*, 427–448.

Overton, W. F. (1973). On the assumptive base of the nature–nurture controversy: Additive versus interactive conceptions. *Human Development, 16*, 74–89.

Overton, W. F. (1978). Klaus Riegel: Theoretical contribution to concepts of stability and change. *Human Development, 21*, 360–363.

Overton, W. F. (1984). World views and their influence on psychological theory and research: Kuhn—Lakatos—Lauden. In H. W. Reese (Ed.), *Advances in child development and behavior* (Vol. 18, pp. 194–226). New York: Academic Press.

Overton, W. F. (1991a). Historical and contemporary perspectives on developmental theory and research strategies. In R. Downs, L. Liben, & D. Palermo (Eds.), *Visions of aesthetics, the environment, and development: The legacy of Joachim Wohlwill* (pp. 263–311). Hillsdale, NJ: Erlbaum.

Overton, W. F. (1991b). The structure of developmental theory. In H. W. Reese (Ed.), *Advances in child development and behavior* (Vol. 23, pp. 1–37). New York: Academic Press.

Overton, W. F. (1991c). Metaphor, recursive systems, and paradox in science and developmental theory. In H. W. Reese (Ed.), *Advances in child development and behavior* (Vol. 23, pp. 59–71). New York: Academic Press.

Overton, W. F. (1991d). Competence, procedures and hardware: Conceptual and empirical considerations. In M. Chandler & M. Chapman (Eds.), *Criteria for competence: Controversies in the assessment of children's ability* (pp. 19–42). Hillsdale, NJ: Erlbaum.

Overton, W. F. (1994a). The arrow of time and cycles of time: Concepts of change, cognition, and embodiment. *Psychological Inquiry, 5*, 215–237.

Overton, W. F. (1994b). Interpretationism, pragmatism, realism, and other ideologies. *Psychological Inquiry, 5*, 260–271.

Overton, W. F. (1994c). Contexts of meaning: The computational and the embodied mind. In W. F. Overton & D. S. Palermo (Eds.), *The nature and ontogenesis of meaning* (pp. 1–18). Hillsdale, NJ: Erlbaum.

Overton, W. F. (1998). Developmental psychology: Philosophy, concepts, and methodology. In W. Damon (Series Ed.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (5th ed., pp. 107–187). New York: Wiley.

Overton, W. F. (2003). Development across the life span. In R. M. Lerner, M. A. Easterbrooks, & J. Mistry (Eds.), *Handbook of psychology: Vol. 6 Developmental psychology* (pp. 13–42). Editor-in-chief: I. B. Weiner. Hoboken, NJ: Wiley.

Overton, W. F. (2006). Developmental psychology: Philosophy, concepts, methodology. In W. Damon & R. M. Lerner (Series Eds.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (6th ed., pp. 18–88). Hoboken, NJ: Wiley.

Overton, W. F. (2010). Life-span development: Concepts and issues. In W. F. Overton (Vol. Ed.), *Handbook of life-span development: Vol. 1 Cognition, biology, and methods* (pp. 1–29). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Overton, W. F. (2011). Relational developmental systems and quantitative behavior genetics: Alternative of parallel methodologies. *Research in Human Development*, *8*(3–4), 258–263.

Overton, W. F. (2013). Relationism and relational developmental systems: A paradigm for developmental science in the post-Cartesian era. In R. M. Lerner & J. B. Benson (Eds.), *Advances in child development and behavior: Vol. 44 Embodiment and epigenesis: Theoretical and methodological issues in understanding the role of biology within the relational developmental system. Part A: Philosophical, theoretical, and biological dimensions* (pp. 21–64). Waltham, MA: Elsevier.

Overton, W. F. (2014). Relational developmental systems and developmental science: A focus on methodology. In P. C. M. Molenaar, R. M. Lerner, & K. Newell (Eds.), *Handbook of developmental systems theory and methodolog*y (pp. 19–65). New York: Guilford.

Overton, W. F. (2015a). Process and relational developmental systems. In W. F. Overton & P. C. M. Molenaar (Eds.), *Handbook of child psychology and developmental science: Vol. 1 Theory and method* (7th ed., pp. 9–62). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Overton, W. F. (2015b). Taking conceptual analyses seriously. *Research in Human Development, 12*, 163–171.

Overton, W. F., & Lerner, R. M. (2012). Relational developmental systems: Paradigm for developmental science in the post-genomic era. *Behavioral and Brain Sciences, 35*(5), 375–376.

Overton, W. F., & Lerner, R. M. (2014). Fundamental concepts and methods in developmental science: A relational perspective. *Research in Human Development, 11*(1), 63–73.

Overton, W. F., & Molenaar, P. C. M. (Eds.). (2015). *Handbook of child psychology and developmental science: Vol. 1 Theory and method* (7th ed.). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Overton, W. F., & Müller, U. (2013). Metatheories, theories, and concepts in the study of development. In R. M. Lerner, M. A. Easterbrooks, & J. Mistry (Eds.), *Handbook of psychology: Vol. 6 Developmental psychology* (2nd ed., pp. 19–58). Editor-in-chief: I. B. Weiner. Hoboken, NJ: Wiley.

Overton, W. F., & Reese, H. W. (1973). Models of development: Methodological implications. In J. R. Nesselroade and H. W. Reese (Eds.), *Life-span developmental psychology: Methodological issues* (pp. 65–86). New York: Academic Press.

Overton, W., & Reese, H. (1981). Conceptual prerequisites for an understanding of stability–change and continuity–discontinuity. *International Journal of Behavioral Development, 4*, 99–123.

Ozer, E. M., & Irwin, C. E., Jr. (2009). Adolescent and young adult health: From basic health status to clinical interventions. In R. M. Lerner & L. Steinberg (Eds.), *Handbook of adolescent psychology: Vol. 1 Individual bases of adolescent development* (3rd ed., pp. 618–641). Hoboken, NJ: Wiley.

Padin, M. A., Lerner, R. M., & Spiro, A. III. (1981). The role of physical education interventions in the stability of body attitudes and self-esteem in late adolescents. *Adolescence, 16*, 371–384.

Panofsky, A. (2014). *Misbehaving science: Controversy and the development of behavior genetics*. Chicago: University of Chicago Press.

Park, B., & Rothbart, M. (1982). Perception of out-group homogeneity and levels of social categorization: Memory for the subordinate attributes of in-group and out-group members. *Journal of Personality and Social Psychology, 42*(6), 1051–1068.

Penfield, W., & Roberts, L. (1959). *Speech and brain mechanisms*. Princeton, NJ: Princeton University Press.

Pepper, S. C. (1942). *World hypotheses: A study in evidence*. Berkeley: University of California Press.

Perkins, D. F., & Lerner, R. M. (1995). Single and multiple indicators of physical attractiveness and psychosocial behaviors among young adolescents. *Journal of Early Adolescence, 15*, 269–298.

Pervin, L. A. (1968). Performance and satisfaction as a function of individual–environment fit. *Psychological Bulletin, 69*, 56–68.

Pervin, L. A., & Lewis, M. (Eds.). (1978). *Perspectives in interactional psychology*. New York: Plenum Publishing.

Peters, M. (1995a). Does brain size matter? A reply to Rushton and Ankney. *Canadian Journal of Experimental Psychology, 47*, 751–756.

Peters, M. (1995b). Race differences in brain size: Things are not as clear as they seem to be. *American Psychologist, 49*, 570–576.

Petersen, A. C. (1983). Menarche: Meaning of measures and measuring meaning. In S. Golub (Ed.), *Menarche: The transition from girl to woman* (pp. 63–76). Lexington: D.C. Heath.

Petersen, A. C. (1987). The nature of biological psychosocial interactions: The sample case of early adolescence. In R. M. Lerner & T. T. Fochs (Eds.), *Biological-psychosocial interactions in early adolescence: A life-span perspective* (pp. 35–61). Hillsdale, NJ: Erlbaum.

Petersen, A. C. (1988). Adolescent development. In M. R. Rosenzweig (Ed.), *Annual review of psychology* (Vol. 39, pp. 583–607). Palo Alto, CA: Annual Reviews.

Petersen, A. C., Tobin-Richards, M. H., & Boxer, A. M. (1983). Puberty: Its measurement and its meaning. *Journal of Early Adolescence, 3*, 47–62.

Petersen, A. C., Verma, S., Koller, S. H., & Motti-Stefanidi, F. (2017). Global positive youth development: Framing the issues. In A. C. Petersen, S. H. Koller, F. Motti-Stefanidi, & S. Verma (Eds.), *Positive youth development in global contexts of social and economic change* (pp. 1–14). New York: Routledge.

Peterson, C., & Seligman, M. E. (2004). *Character strengths and virtues: A handbook and classification*. Washington, DC: American Psychological Association.

Petrinovich, L. (1979). Probabilistic functionalism: A conception of research method. *American Psychologist, 34*, 373–390.

Phelps, E., Balsano, A., Fay, K., Peltz, J., Zimmerman, S., & Lerner, R. M. (2007). Nuances in early adolescent development trajectories of positive and problematic/risk behaviors: Findings from the 4-H Study of Positive Youth Development. *Child and Adolescent Psychiatric Clinics of North America, 16*(2), 473–496.

Phelps, J. A., Davis, J. O., & Schartz, K. M. (1997). Nature, nurture, and twin research strategies. *Current Directions in Psychological Science, 6*, 117–121.

Piaget, J. (1923). La pensée symbolique et la pensée de l'enfant. *Archives of Psychology, Geneva, 18*, 273–304.

Piaget, J. (1932). *The moral judgment of the child* (Marjorie Worden Gabian, Trans.). New York: Harcourt, Brace, & World.

Piaget, J. (1950). *The psychology of intelligence*. New York: Harcourt Brace.

Piaget, J. (1952). *The origins of intelligence in children*. New York: International Universities Press.

Piaget, J. (1954). *The construction of reality in the child* (M. Cook, Trans.). New York: Basic Books.

Piaget, J. (1955). Les stades du development intellectuel de l'enfant et de l'adolescent. In *Le probleme des stades en psychologie de l'enfant* (Ed. P. Osterrieth et al.). Paris: Presses Universitaires de France.

Piaget, J. (1960). *The child's conception of the world*. Paterson, NJ: Littlefield, Adams.

Piaget, J. (1961). *Les mecanismes perceptifs*. Paris: Presses Universitaires de France.

Piaget, J. (1965). *The moral judgement of the child*. New York: Free Press.

Piaget, J. (1968). *Six psychological studies*. New York: Random House.

Piaget, J. (1970). Piaget's theory. In P. H. Mussen (Ed.), *Carmichael's manual of child psychology* (3rd ed., Vol. 1, pp. 703–723). New York: Wiley.

Piaget, J. (1972). Intellectual evolution from adolescence to adulthood. *Human Development, 15*, 1–12.

Piaget, J. (1978). *Behavior and evolution*. New York: Random House.

Piaget, J. (1979). Relations between psychology and other sciences. *Annual Review of Psychology, 30*, 18.

Piaget, J., & Inhelder, B. (1956). *The child's conception of space*. London: Routledge & Kegan Paul.

Pigliucci, M., & Müller, G. B. (2010). Elements of an extended evolutionary synthesis. In M. Pigliucci & G. B. Müller (Eds.), *Evolution – The extended synthesis* (pp. 3–17). Cambridge, MA: MIT Press.

Pinard, A., & Laurendeau, M. (1969). 'Stage' in Piaget's cognitive-developmental theory: Exegesis of concept. In D. Elkind, & J. Flavell (Eds.), *Studies in cognitive development: Essays in honor of Jean Piaget*. New York: Oxford University Press.

Ploetz, A. (1895). *The excellence of our race and the protection of the weak*. Berlin: Fischer.

Plomin, R. (1986). *Development, genetics, and psychology*. Hillsdale, NJ: Erlbaum.

Plomin, R. (2000). Behavioural genetics in the 21st century. *International Journal of Behavioral Development, 24*, 30–34.

Plomin, R., Corley, R., DeFries, J. C., & Faulker, D. W. (1990). Individual differences in television viewing in early childhood: Nature as well as nurture. *Psychological Science, 1*, 371–377.

Plomin, R., & Daniels, D. (1987). Why are children in the same family so different from each other? *Behavioral and Brain Sciences, 10*, 1–16.

Plomin, R., & DeFries, J. C. (1980). Genetics and intelligence: Recent data. *Intelligence, 4*, 15–24.

Plomin, R., DeFries, J. C., Knopik, V. S., & Neiderhiser, J. S. (2013). *Behavioral genetics* (6th ed.). New York: Worth Publishers.

Plomin, R., DeFries, J. C., Knopik, V. S., & Neiderhiser, J. M. (2016). Top 10 replicated findings from behavioral genetics. *Perspectives on Psychological Science, 11*(1), 3–23.

Plomin, R., DeFries, J. C., & McClearn, G. E. (1980). *Behavioral genetics: A primer*. San Francisco: Freeman.

Plomin, R., DeFries, J. C., McClearn, G. E., & McGuffin, P. (2008). *Behavioral genetics*. New York: Worth Publishers.

Popper, K. (1959). *The logic of scientific discovery*. London: Hutchinson.

Pressey, S. L., Janney, J. E., & Kuhlen, R. G. (1939). *Life: A psychological survey*. New York: Harper.

Preyer, W. (1882/1888–1889). *The mind of the child* (Vols. 1–2). New York: Appleton.

Prigogine, I. (1978). Time, structure, and fluctuation. *Science, 201*, 777–785.

Prigogine, I. (1980). *From being to becoming*. San Francisco: W. H. Freeman.

Proctor, R. N. (1988). *Racial hygiene: Medicine under the Nazis*. Cambridge, MA: Harvard University Press.

Pruden, S. M., & Odean, R. (2017). Language development: Motion verb and spatial-relational term acquisition from a developmental systems perspective. In A. S. Dick & U. Müller (Eds.), *Advancing developmental science: Philosophy, theory, and method* (pp. 133–144). New York: Routledge.

Putnam, H. (1987). *The many faces of realism*. Cambridge, UK: Cambridge University Press.

Raeff, C. (2011). Distinguishing between development and change: Reviving organismic-developmental theory. *Human Development*, *54*, 4–33.

Raeff, C. (2016). *Exploring the dynamics of human development: An integrative approach*. New York: Oxford University Press.

Ralston, P., Lerner, R. M., Mullis, A. K., Simerly, C., & Murray, J. (Eds.). (1999). *Social change, public policy, and community collaboration: Training human development professionals for the twenty-first century*. Norwell, MA: Kluwer.

Ram, N., Chow, S. M., Bowles, R. P., Wang, L., Grimm, K., Fujita, F., & Nesselroade, J. R. (2005). Examining interindividual differences in cyclicity of pleasant and unpleasant affect using spectral analysis and item response modeling. *Psychometrika, 70*, 773–790.

Ram, N., & Grimm, K. J. (2009). Growth mixture modeling. A method for identifying differences in longitudinal change among unobserved groups. *International Journal of Behavioral Development*, *33*, 565–576.

Ram, N., & Grimm, K. J. (2015). Growth curve modeling and longitudinal factor analysis. In W. F. Overton & P. C. M. Molenaar (Eds.), *Handbook of child psychology and developmental science: Vol. 1 Theory and method* (7th ed., pp. 758–788). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Rao, D. C., & Morton, N. E. (1978). IQ as a paradigm in genetic epidemiology. In N. E. Morton & C. S. Chung (Eds.), *Genetic epidemiology* (pp. 145–181). New York: Academic Press.

Rao, D. C., Morton, N. E., & Yee, S. (1976). Resolution of cultural and biological inheritance by path analysis. *American Journal of Human Genetics, 28*, 228–242.

Rapaport, D. (1959). A historical survey of psychoanalytic ego psychology. *Psychological Issues, 1*(1), 5–17.

Rathunde, K., & Csikszentmihalyi, M. (2006). The developing person: An experiential perspective. In R. M. Lerner (Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (6th ed., pp. 465–515). Editors-in-chief: W. Damon & R. M. Lerner. Hoboken, NJ: Wiley.

Ratner, A. M., & Hoffman, H. S. (1974). Evidence for a critical period for imprinting in khaki campbell ducklings (*anas platyrhynchos domesticus*). *Animal Behavior, 22*, 249–255.

Reese, H. W. (1982). Behavior analysis and developmental psychology: Discussant comments. *Human Development, 35*, 352–357.

Reese, H. W. (1993). Contextualism and dialectical materialism. In S. C. Hayes, L. J. Hayes, H. W. Reese, & T. R. Sarbin (Eds.), *Varieties of scientific contextualism* (pp. 71–105). Reno, NV: Context Press.

Reese, H. W. (1995). Soviet psychology and behavior analysis: Philosophical similarities and substantive differences. *Behavioral Development, 5*, 2–4.

Reese, H. W., & Lipsitt, L. P. (Eds.). (1970). *Experimental child psychology*. New York: Academic Press.

Reese, H. W., & Overton, W. F. (1970). Models of development and theories of development. In L. R. Goulet & P. B. Baltes (Eds.), *Lifespan developmental psychology: Research and theory* (pp. 115–145). New York: Academic Press.

Rhodes, J. E., & Lowe, S. R. (2009). Mentoring in adolescence. In R. M. Lerner & L. Steinberg (Eds.), *Handbook of adolescent psychology: Vol. 2 Contextual influences on adolescent development* (3rd ed., pp. 152–190). Hoboken, NJ: Wiley.

Ricco, R. B. (2015) The development of reasoning. In L. S. Liben & U. Mueller (Eds.), *Handbook of child psychology and developmental science: Vol. 2 Cognitive processes* (7th ed., pp. 519–570). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Ricco, R. B. (2017). Development and deductive reasoning. In A. S. Dick & U. Müller (Eds.), *Advancing developmental science: Philosophy, theory, and method* (pp. 157–169). New York: Routledge.

Richards, R. (1987). *Darwin and the emergence of evolutionary theories of mind and behavior*. Chicago: University of Chicago Press.

Richardson, K. (2010). *The evolution of intelligent systems: How molecules became mind*. Basingstoke: Palgrave Macmillan.

Richardson, K. (2013). The evolution of intelligent systems. In R. M. Lerner & J. B. Benson (Eds.), *Advances in child development and behavior: Embodiment and epigenesis: Theoretical and methodological issues in understanding the role of biology within the relational developmental system. Part A. Philosophical, theoretical, and biological dimensions* (pp. 127–159). London: Elsevier.

Richardson, K. (2017). *Genes, brains, and human potential: The science and ideology of human intelligence*. New York: Columbia University Press.

Richardson, K., & Norgate, S. (2005). The equal environments assumption of classical twin studies may not hold. *British Journal of Educational Psychology*, *75*, 339–350.

Richardson, K., & Norgate, S. (2006). A critical analysis of IQ studies of adopted children. *Human Development, 49*, 319–335.

Riegel, K. F. (1973). Dialectical operations: The final period of cognitive development. *Human Development, 16*, 346–370.

Riegel, K. F. (1975). Toward a dialectical theory of human development. *Human Development, 18*, 50–64.

Riegel, K. F. (1976a). The dialectics of human development. *American Psychologist, 31*, 689–700.

Riegel, K. F. (1976b). From traits and equilibrium toward developmental dialectics. In W. J. Arnold & J. K. Cole (Eds.), *Nebraska symposium on motivation* (pp. 348–408). Lincoln: University of Nebraska.

Riegel, K. F. (1977a). The dialectics of time. In N. Datan & H. W. Reese (Eds.), *Life-span developmental psychology: Dialectical perspectives on experimental research* (1–45). New York: Academic Press.

Riegel, K. F. (1977b). History, concepts, and theory in the psychology of aging. In J. E. Birren & K. W. Schroots (Eds.), *Handbook of the psychology of aging* (pp. 3–23). New York: Van Nostrand Reinhold.

Riegel, K. F. (1978). *Psychology mon amour: A countertext*. Boston: Houghton Mifflin.

Riley, M. W. (1976). Age strata in social systems. In R. H. Binstock & E. Shanas (Eds.), *Handbook of aging and the social sciences* (pp. 189–217). New York: Van Nostrand Reinhold.

Riley, M. W. (Ed.). (1979). *Aging from birth to death*. Washington, DC: American Association for the Advancement of Science.

Riley, M. W., Johnson, M. E., & Foner, A. (1972). *Aging and society: A sociology of age stratification*. New York: Russell Sage Foundation.

Rimfeld, K., Ayorech, Z., Dale, P. S., Kovas, Y., & Plomin, R. (2016). Genetics affects choice of academic subjects as well as achievement. *Nature/Scientific Reports, 6*(26373). doi: 10.1038/srep26373.

Roberts, B. W., Walton, K. E., & Viechtbauer, W. (2006). Patterns of mean-level change in personality traits across the life course: A meta-analysis of longitudinal studies. *Psychological Bulletin, 132*, 1–25. doi: 10.1037/0033-2909.132.1.1.

Rogoff, B. (1998). Cognition as a collaborative process. In W. Damon (Series Ed.), D. Kuhn & R. S. Siegler (Vol.

Eds.), *Handbook of child psychology: Vol. 2 Cognition, perception, and language* (5th ed., pp. 679–744). New York: Wiley.

Rogoff, B. (2003). *The cultural nature of human development*. New York: Oxford University Press.

Rogoff, B. (2011). *Developing destinies*. New York: Oxford University Press.

Rogoff, B., & Chavajay, P. (1995). What's become of research on the cultural basis of cognitive development? *American Psychologist, 50*, 859–877.

Rose, H., & Rose, S. (2000), Introduction. In. H. Rose & S. Rose (Eds.), *Alas poor Darwin: Arguments against evolutionary psychology* (pp. 1–13). London: Vintage.

Rose, S. (1995). The rise of neurogenetic determinism. *Nature, 373*, 380–382.

Rose, T. (2016). *The end of average: How we succeed in a world that values sameness*. New York: HarperCollins.

Rose, L. T., Rouhani, P., & Fischer, K. W. (2013). The science of the individual. *Mind, Brain, and Education, 7*, 152–158.

Rosenbaum, P., & Rubin, D. B. (1983). The central role of the propensity score in observational studies for causal effects. *Biometrika*, *70*(1), 41–55.

Rosenblatt, J. S. (2010) Behavioral development during the mother–young interactions in placental mammals: The development of behavior in the relationship with the mother. In K. E. Hood, C. T. Halpern, G. Greenberg, & R. M. Lerner (Eds.), *Handbook of developmental systems, behavior and genetics* (pp. 205–233). Malden, MA: Wiley Blackwell.

Rosenblatt, J. S., Turkewitz, G., & Schneirla, T. C. (1961). Early socialization in the domestic cat as based on feeding and other relationships, between female and the young. In B. M. Foss (Ed.), *Determinants of infant behaviour* (pp. 51–74). London: Methuen.

Rosnow, R. L. & Georgoudi, M. (1986). *Contextualism and understanding in behavioral science: Implications for research and theory*. New York: Praeger.

Roth, J. L., & Brooks-Gunn, J. (2003). What exactly is a youth development program? Answers from research and practice. *Applied Developmental Science, 7*, 94–111.

Roth, T. L. (2012). Epigenetics of neurobiology and behavior during development and adulthood. *Developmental Psychobiology, 54*, 590–597.

Roth, T. L., Lubin, F. D., Funk, A. J., & Sweatt, J. D. (2009). Lasting epigenetic influences of early-life adversity on the BDNF gene. *Biological Psychiatry, 65*, 760–769.

Roubertoux, P., & Carlier, M. (1978). Intelligence: Individual differences, genetic factors, environmental factors and interaction between genotype and environment. *Annales de Biologie Clinique, 36*, 101.

Rowe, D. C. (1994). *The limits of family influence: Genes, experience, and behavior*. New York: Guilford Press.

Runyan, W. M. (1978). The life course as a theoretical orientation: Sequences of person–situation interaction. *Journal of Personality, 46*, 569–593.

Runyan, W. M. (1980). A stage-state analysis of the life course. *Journal of Personality and Social Psychology, 38*, 951–962.

Rushton, J. P. (1987). An evolutionary theory of health, longevity, and personality: Sociobiology, and r/K reproductive strategies. *Psychological Reports, 60*, 539–549.

Rushton, J. P. (1988a). Do r/K reproductive strategies apply to human differences? *Social Biology, 35*, 337–340.

Rushton, J. P. (1988b). Race differences in behavior: A review and evolutionary analysis. *Personality and Individual Differences, 9*, 1009–1024.

Rushton, J. P. (1990). Sex, ethnicity, and hormones. *Behavioral and Brain Sciences, 13*, 194, 197–198.

Rushton, J. P. (1991a). Do r-K strategies underlie human race differences? A reply to Weizmann *et al*. *Canadian Psychology, 32*, 29–42.

Rushton, J. P. (1991b). Race, brain size, and intelligence: Another reply to Cernovsky. *Psychological Reports, 66*, 659–666.

Rushton, J. P. (1992). Cranial capacity related to sex, rank, and race in a stratified random sample of 6,325 military personnel. *Intelligence, 16*, 401–413.

Rushton, J. P. (1995). *Race, evolution, and behavior*. New Brunswick, NJ: Transaction Publishers.

Rushton, J. P. (1996). Political correctness and the study of racial differences. *Journal of Social Distress and the Homeless, 5*, 213–229.

Rushton, J. P. (1997). Cranial size and IQ in Asian Americans from birth to age seven. *Intelligence, 25*, 7–20.

Rushton, J. P. (1999). *Race, evolution, and behavior* (special abridged edition). New Brunswick, NJ: Transaction Publishers.

Rushton, J. P. (2000). *Race, evolution, and behavior* (2nd special abridged edition). New Brunswick, NJ: Transaction Publishers.

Rushton, J. P., Brainerd, C. J., & Pressley, M. (1983). Behavioral development and construct validity: The principle of aggregation. *Psychological Bulletin, 94*, 18–38.

Rushton, J. P., & Jensen, A. R. (2005). Thirty years of research on race differences in cognitive ability. *Psychology, Public Policy, and Law, 11*(2), 235–294.

Sachidanandam, R., Weissman, D., Schmidt, S. C., Kakol, J. M., Stein, L. D., & Marth, G., et al. (2001). A map of human genome sequence variation containing 1.42 million single nucleotide polymorphisms, *Nature*, *409*, 928–934.

Sahlins, M. D. (1978). The use and abuse of biology. In A. L. Caplan (Ed.), *The sociobiology debate*. New York: Harper & Row.

Salzen, E., & Meyer, C. C. (1968). Reversibility of imprinting. *Journal of Comparative and Physiological Psychology, 66*, 269–275.

Sameroff, A. (1975). Transactional models in early social relations. *Human Development, 18*, 65–79.

Sameroff, A. J. (1983). Developmental systems: Contexts and evolution. In W. Kessen (Ed.), *Handbook of child psychology: Vol. 1 History, theory, and methods* (pp. 237–294). New York: Wiley.

Sameroff, A. (Ed.) (2009). *The transactional model of development: How children and contexts shape each other*. Washington, DC: American Psychological Association.

Sameroff, A. (2010). A unified theory of development: A dialectic integration of nature and nurture. *Child Development, 81*(1), 6–22.

Sampson, E. E. (1977). Psychology and the American ideal. *Journal of Personality and Social Psychology, 35*, 767–782.

Sampson, R. J. (2016). The characterological imperative: On Heckman, Humphries, and Kautz's *The myth of achievement tests: The GED and the role of character in American life*. *Journal of Economic Literature, 54*(2), 493–513.

Sandhu, S. S. (2010). *Recombinant DNA technology*. New Delhi: International Publishing House.

Sanford, E. C. (1902). Mental growth and decay. *American Journal of Psychology, 13*, 426–449.

Sarason, S. (1973). Jewishness, Blackishness, and the nature–nurture controversy. *American Psychologist*, November, 961–971.

Sarbin, T. R. (1977). Contextualism: A world view for modern psychology. In J. K. Cole (Ed.), *Nebraska symposium on motivation* (pp. 1–41). Lincoln: University of Nebraska Press.

Saunders, P. T. (2010). *Social mobility myths*. London: Civitas Institute for the Study of Civil Society.

Saunders, P. T. (2013). Evolutionary psychology: A house built on sand. In R. M. Lerner & J. B. Benson (Eds.), *Advances in child development and behavior: Vol. 44 Embodiment and epigenesis: Theoretical and methodological issues in understanding the role of biology within the relational developmental system. Part A: Philosophical, theoretical, and biological dimensions* (pp. 257–284). Waltham, MA: Elsevier.

Saunders, P. T. (2014). Dynamical systems, the epigenetic landscape, and punctuated equilibria. In P. C. M. Molenaar, R. M. Lerner, & K. Newell (Eds.), *Handbook of developmental systems theory and methodology* (pp. 154–167). New York: Guilford.

Scarr, S. (1982). Development is internally guided, not determined. *Contemporary Psychology, 27*, 852–853.

Scarr, S. (1987). Three cheers for behavior genetics: Winning the war and losing our identity. *Behavior Genetics*, *17*, 219–228.

Scarr, S. (1992). Developmental theories for the 1990s: Development and individual differences. *Child Development, 63*, 1–19.

Scarr, S., & McCartney, K. (1983). How people make their own environments: A theory of genotype-environment effects. *Child Development, 54*, 424–435.

Scarr-Salapatek, S. (1971a). Unknowns in the IQ equation. *Science, 174*, 1223–1228.

Scarr-Salapatek, S. (1971b). Race, social class, and IQ. *Science, 174*, 1285–1295.

Schaie, K. W. (1965). A general model for the study of developmental problems. *Psychological Bulletin, 64*, 92–107.

Schaie, K. W. (1970). A reinterpretation of age-related changes in cognitive structure and functioning. In L. R. Goulet & P. B. Baltes (Eds.), *Life-span developmental psychology: Research and theory* (pp. 485–507). New York: Academic Press.

Schaie, K. W. (1979). The primary mental abilities in adulthood: An exploration in the development of psychometric intelligence. In P. B. Baltes & J. O. G. Brim (Eds.), *Life-span development and behavior* (Vol. 2, pp. 26–115). New York: Academic Press.

Schaie, K. W. (1984). Midlife influences upon intellectual functioning in old age. *International Journal of Behavioral Development, 7*, 463–468.

Schaie, K. W., Anderson, V. E., McClearn, G. E., & Money, J. (Eds.). (1975). *Developmental human behavior genetics*. Lexington, MA: Heath.

Schaie, K. W., & Geiwitz, J. (1982). *Adult development and aging*. Boston: Little, Brown.

Schaie, K. W., Labouvie-Vief, G., & Buech, B. V. (1973). Generational and cohort-specific differences in adult cognitive functioning: A fourteen-year study of independent samples. *Developmental Psychology, 9*, 151–166.

Schaie, K. W., & Strother, C. R. (1968). A cross-sequential study of age changes in cognitive behavior. *Psychological Bulletin, 70*, 671–680.

Schmid, K. L., & Lopez, S. (2011). Positive pathways to adulthood: The role of hope in adolescents' constructions of their futures. In R. M. Lerner, J. V. Lerner, & J. B. Benson (Eds.), *Advances in child development and behavior: Positive youth development* (Vol. 41, pp. 72–89). London: Academic Press.

Schmid, K., Phelps, E., & Lerner, R. M. (2011a). Constructing positive futures: Modeling the relationship between adolescents' hopeful future expectations and intentional self regulation in predicting Positive Youth Development. *Journal of Adolescence, 34*(6), 1127–1136.

Schmid, K. L., Phelps, E., Kiely, M. K., Napolitano, C. M., Boyd, M. J., & Lerner, R. M. (2011b). The role of adolescents' hopeful futures in predicting positive and negative developmental trajectories: Findings from the 4-H Study of Positive Youth Development. *The Journal of Positive Psychology, 6*(1), 45–56.

Schneirla, T. C. (1956). Interrelationships of the innate and the acquired in instinctive behavior. In P. P. Grassé (Ed.), *L'instinct dans le comportement des animaux et de l'homme* (pp. 387–452). Paris: Mason et Cie.

Schneirla, T. C. (1957). The concept of development in comparative psychology. In D. B. Harris (Ed.), *The concept of development: An issue in the study of human behavior* (pp. 78–108). Minneapolis: University of Minnesota Press.

Schneirla, T. C. (1959). An evolutionary and developmental theory of biphasic processes underlying approach and withdrawal. In M. R. Jones (Ed.), *Nebraska symposium on motivation* (1–42). Lincoln: University of Nebraska Press.

Schneirla, T. C. (1965). Aspects of stimulation and organization in approach/withdrawal processes underlying vertebrate behavioral development. *Advances in the Study of Behavior, 1*, 1–74. Reprinted in L. R. Aronson, E. Tobach, D. D. Lehrman, & J. S. Rosenblatt (Eds.) (1971), *Selected writings of T. C. Schneirla* (pp. 344–412). San Francisco: W. H. Freeman.

Schneirla, T. C. (1966). Instinct and aggression: Reviews of Konrad Lorenz, *Evolution and modification of behavior* (Chicago: The University of Chicago Press, 1965), and *On aggression* (New York: Harcourt, Brace & World, 1966). *Natural History, 75*, 16.

Schneirla, T. C., & Rosenblatt, J. S. (1961). Behavioral organization and genesis of the social bond in insects and mammals. *American Journal of Orthopsychiatry, 3*, 223–253.

Schneirla, T. C., & Rosenblatt, J. S. (1963). Critical periods in behavioral development. *Science, 139*, 1110–1114.

Schoon, I. (2017). Making it against the odds: A developmental-contextual approach to the study of resilience. In A. C. Petersen, S. H. Koller, F. Motti-Stefanidi, & S. Verma (Eds.), *Positive youth development in global contexts of social and economic change* (pp. 62–81). New York: Taylor & Francis.

Schoon, I., & Bynner, J. (Eds.). (2017). *Young people's development and the Great Recession: Uncertain transitions and precarious futures*. New York: Cambridge University Press.

Schulz, R., & Heckhausen, J. (1996). A life-span model of successful aging. *American Psychologist, 51*, 702–714.

Schwartz, S. J. (2005). A new identity for identity research: Recommendations for expanding and refocusing the identity literature. *Journal of Adolescent Research, 20*, 293–308.

Scott, J. P. (1962). Critical periods in behavioral development. *Science, 138*, 949–958.

Scriver, C. R., & Clow, C. L. (1980a). Phenylketonuria: Epitome of human biochemical genetics (first of two parts). *New England Journal of Medicine, 303*, 1336–1342.

Scriver, C. R., & Clow, C. L. (1980b). Phenylketonuria: Epitome of human biochemical genetics (second of two parts). *New England Journal of Medicine, 303*, 1394–1400.

Sears, R. R. (1975). Your ancients revisited: A history of child development. In E. M. Hetherington (Ed.), *Review of child development research* (Vol. 6, pp. 1–73). Chicago: University of Chicago Press.

Secord, P. F., & Backman, C. W. (1964). *Social psychology*. New York: McGraw-Hill.

Segal, N. L. (2012). *Born together – reared apart: The landmark Minnesota Twin Study*. Cambridge, MA: Harvard University Press.

Seider, S. (2012). *Character compass: How powerful school culture can point students toward success.* Cambridge, MA: Harvard Education Press.

Selman, R. L. (1976). Social cognitive understanding: A guide to educational and clinical practice. In T. Lickona (Ed.), *Moral development and behavior*. New York: Holt, Rinehart & Winston.

Selzam, S., Krapohl, E., von Stumm, S., O'Reilly, P. F., Rimfeld, K., Kovas, Y., Dale, P. S., Lee, J. J., & Plomin, R. (2017). Predicting educational achievement from DNA. *Molecular Psychiatry, 22*, 267–272.

Shah, J. Y., Higgins, E. T., & Friedman, R. (1998). Performance incentives and means: How regulatory focus influences goal attainment. *Journal of Personality and Social Psychology, 74*, 285–293.

Sheldon, W. H. (1940). *The varieties of human physique*. New York: Harper & Row.

Sheldon, W. H. (1942). *The varieties of temperament*. New York: Harper & Row.

Sherrington, C. S. (1951). *Man on his nature*. Cambridge, UK: Cambridge University Press.

Sherrod, L. R., Haggerty, R. J., & Featherman, D. L. (1993). Introduction: Late adolescence and the transition to adulthood. *Journal of Research on Adolescence, 3*, 217–226.

Shields, J. (1962). *Monozygotic twins brought up apart and brought up together.* London: Oxford University Press.

Shirley, M. M. (1933). *The first two years*. Minneapolis: University of Minnesota Press.

Shoda, Y., Cervone, D., & Downey, G. (Eds.). (2007). *Persons in context: Building a science of the individual*. New York: Guilford.

Shweder, R. A., Goodnow, J., Hatano, G., Kessel, F., LeVine, R. A., Markus, H., Miller, P., & Worthman, C. (1998). The cultural psychology of development. In R. M. Lerner (Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (5th ed., pp. 867–937). Editor-in-chief: W. Damon. New York: Wiley.

Shweder, R. A., Goodnow, J. J., Hatano, G., LeVine, R. A., Markus, H. R., & Miller, P. J. (2006). The cultural psychology of development: One mind, many mentalities. In R. M. Lerner (Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (6th ed., pp. 716–792). Editors-in-chief: W. Damon & R. M. Lerner. Hoboken, NJ: Wiley.

Siegel, A. W., Bisanz, J., & Bisanz, G. L. (1983). Developmental analysis: A strategy for the study of psychological change. *Contributions to Human Development, 8*, 53–80.

Siegler, R. S. (1978). *The origins of scientific reasoning. Children's thinking: What develops?* Hillsdale, NJ: Erlbaum.

Siegler, R. S. (1981). Developmental sequences within and between concepts. *Monographs of the Society for Research in Child Development, 46*, 189.

Siegler, R. S. (2003). Thinking and intelligence. In M. Bornstein (Ed.), *Well-being: Positive development across the lifespan* (pp. 311–320). Mahwah, NJ: Erlbaum.

Siegler, R. S. (2009). Improving the numerical understanding of children from low-income families. *Child Development Perspectives, 3*, 118–124.

Siegler, I. C., & Botwinick, J. (1979). A long-term longitudinal study of intellectual ability of older adults: The matter of selective subject attrition. *Journal of Gerontology*, *34*(2), 242–245.

Sigman, M. (1982). Plasticity in development: Implications for intervention. In L. A. Bond & J. M. Joffe (Eds.), *Facilitating infant and early childhood development* (pp. 98–116). Hanover, NH: University Press of New England.

Sim, A. T. R., & Peters, M. (2014). Compassion International: Holistic child development through sponsorship and church partnership. In B. Watson & M.

Clarke (Eds.), *Child sponsorship: Exploring pathways to a brighter future* (pp. 163–190). New York: Palgrave Macmillan.

Simmons, R. G., & Blyth, D. A. (1987). *Moving into adolescence: The impact of pubertal change and school context*. Hawthorne, NJ: Aldine.

Simmons, R. G., Carlton-Ford, S. L., & Blyth, D. A. (1987). Predicting how a child will cope with the transition to junior high school. In R. M. Lerner & T. T. Foch (Eds.), *Biological-psychosocial interactions in early adolescence* (pp. 325–375). Hillsdale, NJ: Erlbaum.

Simmons, R., Rosenberg, F., & Rosenberg, M. (1973). Disturbances in the self-image in adolescence. *American Sociological Review, 38*, 553–568.

Skinner, B. F. (1938). *The behavior of organisms*. New York: Appleton.

Skinner, B. F. (1950). Are theories of learning necessary? *Psychological Review, 57*, 211–220.

Skinner, B. F. (1953). *Science and human behavior*. New York: Macmillan.

Skinner, B. F. (1956). A case history in scientific method. *American Psychologist*, *11*.

Skinner, B. F. (1966). The phylogeny and ontogeny of behavior. *Science, 153*, 1205–1213.

Skinner, B. F. (1971). *Beyond freedom and dignity*. New York: Knopf.

Slavich, G. M., & Cole, S. W. (2013). The emerging field of human social genomics. *Clinical Psychological Science, 1*, 331–348.

Smetana, J. G., Jambon, M., & Ball, C. (2014). The social domain approach to children's moral and social judgments. In M. Killen & J. G. Smetana (Eds.), *Handbook of moral development* (2nd ed., pp. 23–45). New York: Taylor & Francis.

Smith, E. P., Petersen, A. C., & Leman, P. (Eds.). (2017). Positive youth development in diverse and global contexts. *Child Development, 88*(4), 1035–1397.

Smith, L. B., & Thelen, E. (Eds.). (1993). *A dynamic systems approach to development: Applications*. Cambridge, MA: MIT Press.

Snyder, M. (1981). On the influence of individuals on situations: Implications for understanding the links between personality and social behavior. *Journal of Personality, 51*(3), 497–516.

Sokol, B. W., Hammond, S. I., & Berkowitz, M. W. (2010). The developmental contours of character. In T. Lovat, R. Toomey, & N. Clement (Eds.), *International research handbook on values education and student wellbeing* (pp. 579–603). Dordrecht, Netherlands: Springer.

Sokol, B. W., Hammond, S., Kuebli, J., & Sweetman, L. (2015) The development of agency. In W. F. Overton & P. C. M. Molenaar (Eds.), *Handbook of child psychology and developmental science: Vol. 1 Theory and method* (7th ed., pp. 284–322). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Solé, R., & Goodwin, B. (2000). *Signs of life: How complexity pervades biology*. New York: Basic Books.

Solomon, R. L., & Lessac, M. S. (1968). A control group design for experimental studies of developmental processes. *Psychological Bulletin, 70*, 1545–1550.

Sorell, G. T., & Nowak, C. A. (1981). The role of physical attractiveness as a contributor to individual development. In R. M. Lerner & N. A. Busch-Rossnagel (Eds.), *Individuals as producers of their development: A life-span perspective* (pp. 389–446). New York: Academic Press.

Sorensen, B., Weinert, E., & Sherrod, L. R. (Eds.). (1986). *Human development and the life course: Multidisciplinary perspectives*. Hillsdale, NJ: Erlbaum.

Spelke, E. S., & Newport, E. L. (1998). Nativism, empiricism, and the development of knowledge. In W. Damon (Series Ed.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (5th ed., pp. 275–340). New York: Wiley.

Spencer, M. B. (1990). Development of minority children: An introduction. *Child Development, 61*, 267–269.

Spencer, M. B. (1995). Old issues and new theorizing about African American youth: A phenomenological variant of ecological systems theory. In R. L. Taylor (Ed.), *African-American youth: Their social and economic status in the United States* (pp. 37–69). Westport, CT: Praeger.

Spencer, M. B. (1999). Social and cultural influences on school adjustment: The application of an identity-focused cultural ecological perspective. *Educational Psychologist, 34*, 43–57.

Spencer, M. B. (2006). Phenomenological variant of ecological systems theory (PVEST): A human development synthesis applicable to diverse individuals and groups. In W. Damon & R. M. Lerner (Eds.) & R. M.

Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 6 Theoretical models of human development* (6th ed., pp. 829–894). Hoboken, NJ: Wiley.

Spencer, M. B., Noll, E., Stoltzfus, J., & Harpalani, V. (2001). Identity and school adjustment: Revisiting the "acting White" assumption. *Educational Psychologist, 36*(1), 21–30.

Spencer, M. B., & Spencer, T. R. (2014). Exploring the promises, intricacies, and challenges to Positive Youth Development. *Journal of Youth and Adolescence*, *43*, 1027–1035.

Spencer, M. B., Swanson, D. P., & Cunningham, M. (1991). Ethnicity, ethnic identity and competence formation: Adolescent transition and identity transformation. *Journal of Negro Education, 60*(3), 366–387.

Spencer, M. B., Swanson, D. P., & Harpalani, V. (2015). Development of the self. In M. E. Lamb (Vol. Ed.), *Handbook of child psychology and developmental science: Vol. 3 Socioemotional processes* (7th ed., pp. 750–793). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Spiker, C. G., & McCandless, B. R. (1954). The concept of intelligence and the philosophy of science. *Psychological Review, 61*, 255–266.

Sroufe, L. A. (1979). The coherence of individual development. *American Psychologist, 34*, 834–841.

Sroufe, L. A., & Waters, E. (1977). Attachment as an organizational construct. *Child Development, 48*, 1184–1199.

Stafford-Brizard,, K. B., Cantor, P., & Rose, T. (in press). Building the bridge between science and practice: Essential characteristics of a translational framework. *Mind, Brain and Education*.

Staiger, D., & Stock, J. H. (1997). Instrumental variables regression with weak instruments. *Econometrica, 65*(3), 557–586.

Staudinger, U. M., & Baltes, P. B. (1996). Weisheit als Gegenstand psychologischer Forschung. *Psychologische Rundschau, 47*, 57–77.

Staudinger, U. M., Marsiske, M., & Baltes, P. B. (1995). Resilience and reserve capacity in later adulthood: Potentials and limits of development across the life span. In D. Chicchetti & D. Cohen (Eds.), *Developmental psychopathology: Vol. 2 Risk, disorder, and adaptation* (pp. 801–847). New York: Wiley.

Stein, G. J. (1987). The biological bases of ethnocentrism, racism, and nationalism in national socialism. In V. Reynolds, V. Falger, & I. Vine (Eds.), *The sociobiology of ethnocentrism* (pp. 251–267). London: Croom Helm.

Stern, C. (1973). *Principles of human genetics* (3rd ed.). San Francisco: Freeman.

Stern, W. (1914). *Psychologie der frühen Kindheit bis zum sechsten Lebensiahr*. Leipzig: Quelle & Meyer.

Stevens, S. S. (1946). On the theory of scales of measurement. *Science, 103*, 677–680.

Stevenson, H. W. (Ed.). (1963). *Child psychology: 62nd yearbook of the National Society for the Study of Education*. Chicago: University of Chicago Press.

Strauss, S. (1982). *U-shaped behavioral growth*. New York: Academic Press.

Streissguth, A. P., Landesman-Dwyer, S., Martin, J. C., & Smith, D. W. (1980). Teratogenic effects of alcohol in humans and laboratory animals. *Science, 209*, 353–361.

Strohman, R. C. (1993a). Organism and experience. [Review of Lerner, R. M., *Final solutions*. University Park, PA: Penn State Press, 1992.] *Journal of Applied Developmental Psychology, 14*, 147–151.

Strohman, R. C. (1993b). Book reviews: *Final solutions*, Richard M. Lerner, Penn State Press, 1992, and *Individual development and evolution: The genesis of novel behavior*, Gilbert Gottlieb, Oxford University Press, 1992. *Integrative Physiological and Behavioral Science, 28*, 99–104.

Sunday, S. R., & Tobach, E. (Ed.). (1985). *Violence against women: A critique of the sociobiology of rape*. New York: Gordian.

Suomi, S. J. (2004a). How gene–environment interactions can influence emotional development in rhesus monkeys. In C. Garcia-Coll, E. L. Bearer, & R. M. Lerner (Eds.), *Nature and nurture: The complex interplay of genetic and environmental influences on human development* (pp. 35–51). Mahwah, NJ: Lawrence Erlbaum.

Suomi, S. J. (2004b). How gene–environment interactions shape biobehavioral development: Lessons from studies with rhesus monkeys. *Research in Human Development, 1*, 205–222.

Suomi, S. J. (2006). Risk, resilience, and gene–environment interactions in rhesus monkeys. Paper presented at the *Resilience in Children* Conference, New York Academy of Sciences and Brown Medical School, Arlington, VA.

Suomi, S. J., & Harlow, H. F. (1972). Social rehabilitation of isolate-reared monkeys. *Developmental Psychology, 6*, 487–496.

Super, C. M., & Harkness, S. (1981). Figure, ground, and gestalt: The cultural context of the active individual. In R. M. Lerner & N. A. Busch-Rossnagel (Eds.), *Individuals as producers of their own development: A life-span perspective* (pp. 69–86). New York: Academic Press.

Suppes, P., & Ginsberg, R. (1963). A fundamental property of all-or-none models, binomial distribution of responses prior to conditioning, with application to concept formation in children. *Psychological Review, 70*(2), 139–161.

Susman, E. J., & Dorn, L. D. (2009). Puberty: Its role in development. In R. M. Lerner & L. Steinberg (Eds.), *Handbook of adolescent psychology: Vol. 1 Individual bases of adolescent development* (3rd ed., pp. 116–151). Hoboken, NJ: Wiley.

Swanson, D. P., Spencer, M. B., & Petersen, A. (1998). Identity formation in adolescence. In K. Borman & B. Schneider (Eds.), *The adolescent years: Social influences and educational challenges: Ninety-seventh yearbook of the National Society for the Study of Education, part I* (pp. 18–41). Chicago: National Society for the Study of Education.

Swartz, D. (1982). Is it sex? *The Sciences, 22,* 2.

Tabery, J. (2014). *Beyond versus: The struggle to understand the interaction of nature and nurture*. Cambridge, MA: MIT Press.

Tabery, J., & Griffiths, P. E. (2010). Historical and philosophical perspectives on behavioral genetics and developmental science. In K. E. Hood, C. T. Halpern, G. Greenberg, & R. M. Lerner (Eds.), *Handbook of developmental systems, behavior and genetics* (pp. 41–60). Malden, MA: Wiley Blackwell.

Tajfel, H., and Turner, J. C. (1986) The social identity theory of intergroup behavior. *Psychology of Intergroup Relations, 5*, 7–24.

Tanner, J. M. (1973). Growing up. *Scientific American, 229*, 34–43.

Tanner, J. (1991). Menarche, secular trend in age of. In R. M. Lerner, A. C. Petersen, & J. Brooks-Gunn (Eds.), *Encyclopedia of adolescence* (Vol. 1, pp. 637–641). New York: Garland.

Taylor, H. F. (1980). *The IQ game: A methodological inquiry into the heredity-environment controversy*. New Brunswick, NJ: Rutgers University Press.

Terman, L. M. (1916). *The measurement of intelligence*. Boston: Houghton.

Terman, L. M. (Ed.). (1925). *Genetic studies of genius, I: Mental and physical traits of a thousand gifted children*. Stanford, CA: Stanford University Press.

Terman, L. M., & Oden, M. H. (1959). *Genetic studies of genius, V: The gifted group at mid-life*. Palo Alto, CA: Stanford University Press.

Terman, L. M., & Tyler, L. E. (1954). Psychological sex differences. In L. Carmichael (Ed.), *Manual of child psychology* (pp. 1064–1114). New York: Wiley.

Teti, D. M. (Ed.). (2005). *Handbook of research methods in developmental science*. Malden, MA: Blackwell Publishing.

Thelen, E., & Smith, L. B. (1994). *A dynamic systems approach to the development of cognition and action*. Cambridge, MA: MIT Press.

Thelen, E., & Smith, L. B. (1998). Dynamic systems theories. In W. Damon (Series Editor) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. I Theoretical models of human development* (5th ed., pp. 563–633). New York: Wiley.

Thelen, E., & Smith, L. B. (2006). Dynamic systems theories. In R. M. Lerner & W. Damon (Eds.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (6th ed., pp. 258–312). Hoboken, NJ: Wiley.

Thelen, E., & Ulrich, B. D. (1991). Hidden skills: A dynamical systems analysis of treadmill stepping during the first year. *Monographs of the Society for Research in Child Development, 56*.

Theokas, C., & Lerner, R. M. (2006). Observed ecological assets in families, schools, and neighborhoods: Conceptualization, measurement, and relations with positive and negative developmental outcomes. *Applied Developmental Science*, *10*(2), 61–74.

Thomas, A., & Chess, S. (1977). *Temperament and development*. New York: Brunner/Mazel.

Thomas, A., & Chess, S. (1980). *The dynamics of psychological development*. New York: Brunner/Mazel.

Thomas, A., & Chess, S. (1981). The role of temperament in the contributions of individuals to their development.

In R. M. Lerner & N. A. Busch-Rossnagel (Eds.), *Individuals as producers of their development: A life-span perspective* (pp. 231–255). New York: Academic Press.

Thomas, A., Chess, S., & Birch, H. (1968). *Temperament and behavioral disorders in childhood*. New York: New York University Press.

Thomas, A., Chess, S., Birch, H. G., Hertzig, M. E., & Korn, S. (1963). *Behavioral individuality in early childhood*. New York: New York University Press.

Thompson, R. A., & Lamb, M. E. (1986). Infant–mother attachment: New directions for theory and research. In P. B. Baltes, D. L. Featherman, & R. M. Lerner (Eds.), *Life-span development and behavior* (Vol. 7, pp. 1–41). Hillsdale, NJ: Erlbaum.

Thorndike, E. L. (1904). The newest psychology. *Educational Review, 28*, 217–227.

Thorndike, E. L. (1905). *The elements of psychology*. New York: Seiler.

Thorpe, W. (1961). Sensitive periods in the learning of animals and man: A study of imprinting. In W. Thorpe & C. Zangwill (Eds.), *Current problems in animal behavior* (pp. 194–224). Cambridge, UK: Cambridge University Press.

Timney, B., Mitchell, D. E., & Griffin, F. (1978). The development of vision in cats after extended periods of dark-rearing. *Experimental Brain Research, 31*, 547–560.

Tobach, E. (1971). Some evolutionary aspects of human gender. *Journal of Orthopsychiatry, 41*, 710–715.

Tobach, E. (1978). The methodology of sociobiology from the viewpoint of a comparative psychologist. In A. L. Caplan (Ed.), *The sociobiology debate* (pp. 411–423). New York: Harper & Row.

Tobach, E. (1981). Evolutionary aspects of the activity of the organism and its development. In R. M. Lerner & N. A. Busch-Rossnagel (Eds.), *Individuals as producers of their development: A life-span perspective* (pp. 37–68). New York: Academic Press.

Tobach, E. (1994). . . . Personal is political is personal is political . . . *Journal of Social Issues, 50*, 221–224.

Tobach, E., Gianutsos, J., Topoff, H. R., & Gross, C. G. (1974). *The four horses: Racism, sexism, militarism, and social Darwinism*. New York: Behavioral Publications.

Tobach, E., & Greenberg, G. (1984). The significance of T. C. Schneirla's contribution to the concept of levels of integration. In G. Greenberg & E. Tobach (Eds.), *Behavioral evolution and integrative levels.* Hillsdale, NJ: Erlbaum.

Tobach, E., & Schneirla, T. C. (1968). The biopsychology of social behavior of animals. In R. E. Cooke & S. Levin (Eds.), *Biologic basis of pediatric practice* (pp. 68–82). New York: McGraw-Hill.

Tolan, P., Chertok, F., Keys, C., & Jason, L. (1990). Conversing about theories, methods, and community research. In P. H. Tolan, C. Keys, F. Chertok, & L. Jason (Eds.), *Researching community psychology: Issues of theory and methods* (pp. 3–8). Washington, DC: American Psychological Association. http://dx.doi.org/10.1037/10073-016.

Tolan, P. H., & Deutsch, N. L. (2015). Mixed methods in developmental science. In W. F. Overton & P. C. Molenaar (Eds.), *Handbook of child psychology and developmental science: Vol. 1 Theory and method* (7th ed., pp. 713–757). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Toulmin, S. (1981). Epistemology and developmental psychology. In E. S. Gollin (Ed.), *Developmental plasticity: Behavioral and biological aspects of variations in development* (pp. 253–268). New York: Academic Press.

Trivers, R. L. (1971). The evolution of reciprocal altruism. *The Quarterly Review of Biology, 46*, 35–39, 45–47.

Trivers, R. (1985). *Social evolution*. Menlo Park, CA: Benjamin/Cummins Publishing Co.

Trochim, W. (2005). *Research methods: The concise knowledge base*. Mason, OH: Cengage Learning.

Trommsdorff, G. (2012). Cultural perspectives on values and religion in adolescent development: A conceptual overview and synthesis. In G. Trommsdorff & X. Chen (Eds.), *Values, religion, and culture in adolescent development* (pp. 3–45). New York: Cambridge University Press.

Turiel, E. (1969). Developmental processes in the child's moral thinking. In P. H. Mussen, J. Langer, & M. Covington (Eds.), *Trends and issues in developmental psychology*. New York: Holt, Rinehart & Winston.

Turiel, E. (1974). Conflict and transition in adolescent moral development. *Child Development, 45*, 14–29.

Turiel, E. (1978). The development of concepts of social structure: Social convention. In J. Glick & A. Clarke-Stewart, (Eds.), *The development of social understanding*. New York: Gardner Press.

Turiel, E. (1983). *The development of social knowledge: Morality and convention*. Cambridge: Cambridge University Press.

Turiel, E. (2015). Moral development. In W.F. Overton & P.C. Molenaar (Eds.), *Handbook of Child Psychology and Developmental Science. Vol. 1*: *Theory and Method* (7th ed., pp. 484–522). Editor-in-chief: R.M. Lerner. Hoboken, NJ: Wiley.

Turiel, E., & Nucci, L. (2018) Moral development in context. In A.S. Dick & U. Müller (Eds.), *Advancing developmental science: Philosophy, theory, and method* (pp. 95–109). New York: Routledge.

Turkheimer E. (2011). Still missing. *Research in Human Development*, *8*, 227–241.

Turkheimer, E. (2015). Arsonists at the Cathedral. *PsycCRITIQUES, 60*(40), 1–4. DOI: http://dx.doi.org/10.1037/a0039763.

Uphouse, L. L., & Bonner, J. (1975). Preliminary evidence for the effects of environmental complexity on hybridization of rat brain RNA to rat unique DNA. *Developmental Psychobiology, 8*, 171–78.

Urban, J. B., Hargraves, M., & Trochim, W. (2014). Evolutionary evaluation: Implications for evaluators, researchers, practitioners, funders and the evidence-based practice mandate. *Evaluation and Program Planning*, *45*, 127–139.

Urban, J. B., Lewin-Bizan, S., & Lerner, R. M. (2010). The role of intentional self regulation, lower neighborhood ecological assets, and activity involvement in youth developmental outcomes. *Journal of Youth and Adolescence*, *39*(7), 783–800.

Urban, J. B., Osgood, N., & Mabry, P. (2011). Developmental systems science: Exploring the application of non-linear methods to developmental science questions. *Research in Human Development*, *8*, 1–25.

USAID (2013). *State of the field report: Holistic, cross-sectoral youth development: USAID Youth research, evaluation and learning project*. Washington, DC: United States Agency for International Development.

Valsiner, J. (1998). The development of the concept of development: Historical and epistemological perspectives. In W. Damon (Series Ed.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human* (5th ed., pp. 189–232). New York: Wiley.

Valsiner, J. (2006). Developmental epistemology and implications for methodology. In R. M. Lerner (Ed.). *Theoretical models of human development. Vol. 1 of Handbook of Child Psychology* (6th ed., pp. 166–209). Editors-in-chief: W. Damon & R. M. Lerner. Hoboken, NJ: Wiley.

van den Berghe, P. L., & Barash, D. P. (1977). Inclusive fitness and human family structure. *American Anthropologist, 79*, 809–823.

van den Daele, L. D. (1969). Qualitative models in developmental analysis. *Developmental Psychology, 1,* (303–310).

van den Daele, L. D. (1974). Infrastructure and transition in developmental analysis. *Human development, 17*, (1–23).

van den Daele, L. D. (1975). Ego development and preferential judgement in lifespan perspective. In N. Datan, & L. H. Ginsburg, (Eds.), *Lifespan developmental psychology: Normative life crises*. New York: Academic Press.

van der Dennen, J. M. G. (1987). Ethnocentrism and in-group/out-group differentiation: A review and interpretation of the literature. In V. Reynolds, V. Falger, & I. Vine. (Eds.), *The sociobiology of ethnocentrism* (pp. 1–47). London: Croom Helm.

van Geert, P. & Fischer, K. W. (2009) Dynamic systems and the quest for individual-based models of change and development. In J. P. Spencer, M. S. C. Thomas, & J. L. McClelland (Eds.), *Toward a new grand theory of development: Connectionism and dynamic systems theory re-considered*. (pp. 313–336). New York: Oxford University Press.

Van Speybroeck, L., Van de Vijver, G., & De Waele, D. (2002). From epigenesis to epigenetics: The genome in context. *Annals of the New York Academy of Sciences*, 981.

Vandell, D. L., Larson, R. W., Mahoney, J. L., & Watts, T. W. (2015). Children's organized activities. In M. H. Bornstein & T. Leventhal (Vol. Eds.), *Handbook of child psychology and developmental science, Vol. 4: Ecological settings and processes* (7th ed., pp. 305–344). Editor-in-Chief: R. M. Lerner. Hoboken, NJ: Wiley.

Velicer, W. F., Babbin, S. F., & Palumbo, R. (2014). Idiographic applications: Issues of ergodicity and generalizability. In P. C. M. Molenaar, R. M. Lerner, and

K. M. Newell (Eds.), *Handbook of Developmental Systems and Methodology*. (pp. 425–441). New York: Guilford Press.

Venter, J. C., Adams, M. D., Myers, E. W., Li, P. W., Mural, R. J., [plus 270 others]. (2001). The sequence of the human genome. *Science, 291*, 1304–1351.

Verma, S., & Petersen, A. C. (2015). Global concerns with adolescent health with a case study of India. In L.A. Jensen (Ed.). *Oxford Handbook of Human Development and Culture: An Interdisciplinary Perspective.* New York: Oxford University Press.

Vincent, S. B. (1912). The function of the vibrissae in the behavior of the white rat. *Behavior Monographs, 1* (No. 5).

von Bertalanffy, L. (1933). *Modern theories of development*. London: Oxford University Press.

von Bertalanffy, L. (1962). *Modern theories of development: An introduction to theoretical biology.* New York: Harper.

von Bertalanffy, L. (1968). *General systems theory.* New York: Braziller.

von Bertalanffy, L. (1969). Evolution: Chance or law. In A. Koestler & J. R. Smithies (Eds.), *Beyond reductionism* (pp. 59–84). London: Hutchinson.

von Eye, A. (1990a). *Statistical methods in longitudinal research: Principles and structuring change*. New York: Academic Press.

von Eye, A. (1990b). *Introduction to Configural Frequency Analysis: The search for types and anitypes in cross-classifications*. Cambridge: Cambridge University Press.

von Eye, A., Bergman, L. R., & Hsieh, C. (2015) Person-oriented methodological approaches. In W. F. Overton & P. C. M. Molenaar (Eds.), *Handbook of Child Psychology and Developmental Science*, Vol. 1: *Theory and Method* (7th ed., pp. 789–842). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

von Noorden, G. K., Dowling, J. E., & Ferguson, D. C. (1970). Experimental amblyopia in monkeys: I. Behavioral studies of stimulus deprivation in amblyopia. *Archives of Ophthalmology, 84*, 206–14.

Vondracek, F. W., Lerner, R. M., & Schulenberg, J. E. (1986). *Career development: A life-span developmental approach.* Hillsdale, NJ: Erlbaum.

Vygotsky, L. S. (1927/1982). Istorichesskii smysl krizisa v psikhologii. In *Sobranie sochinenii* (Vol. 1). Moscow: Pedagogika.

Vygotsky, L. S. (1933/1966). Play and its role in the mental development of the child. *Vaprosy psikhologii, 12,* 62–76. [English translation: In J. Brunner, A. Jolly, & K. Sylva (Eds.), *Play* (pp. 537–554). Harmondsworth: Penguin.]

Vygotsky, L. (1978). *Mind in society: The development of higher psychological processes* (M. Cole, V. John-Steiner, S. Scribner, & E. Souberman, Trans.). Cambridge, MA: Harvard University Press.

Waddington, C. H. (1942). Canalization of development and the inheritance of acquired characters. *Nature, 150*, 563–564.

Waddington, C. H. (1953). Genetic assimilation of an acquired character. *Evolution, 7*(2), 118–126.

Waddington, C. H. (1957). *The strategy of genes*. London: George Allen and Unwin.

Waddington, C. H. (Ed.). (1968). *Towards a theoretical biology: Prolegomena v. 1.* Edinburgh: Edinburgh University Press.

Waddington, C. H. (1971). *Principles of development and differentiation*. New York: Macmillan.

Wade, N. (2014). *A troublesome inheritance: Genes, race and human history*. New York: Penguin Books.

Wahler, R. G. (1980). Parent insularity as a determinant of generalization success in family treatment. In S. Salzinger, J. Antrobus, & J. Glich (Eds.), *The ecosystem of the "sick" child, implications for classification and intervention for disturbed and mentally retarded children* (pp.187–199). New York: Academic Press.

Wahlsten, D. (1981). Review of The IQ Game by Howard F. Taylor. *Behaviorists for Social Action Journal, 3*, 33–34.

Wahlsten, D. (2003). Airbrushing heritability. *Genes, Brain, and Behavior, 2*, 327–329.

Wahlsten, D. (2010). Probabilistic epigenesis in modern behavioral and neural genetics. In K. E. Hood, C. T. Halpern, G. Greenberg, & R. M. Lerner (Eds.). *Handbook of developmental systems, behavior and genetics* (pp. 110–122). Malden, MA: Wiley Blackwell.

Wahlsten, D. (2012). The hunt for gene effects pertinent to behavioral traits and psychiatric disorders: From mouse to human. *Developmental Psychobiology*, *54*, 475–492.

Wahlsten, D. (2013). A contemporary view of genes and behavior: Complex systems and interactions.

In R. M. Lerner & J. B. Benson, (Eds.), *Advances in Child Development and Behavior: Embodiment and epigenesis: Theoretical and methodological issues in understanding the role of biology within the relational developmental system. Part A. Philosophical, theoretical, and biological dimensions*. (pp. 285–306). London: Elsevier.

Wahlsten, D. (2015). Dynamic heredity. Review of Moore, D. S. (2015). *The developing genome: An introduction to behavioral epigenetics*. New York: Oxford University Press, ISBN 978-0-19-992234-5. Developmental Psychobiology. DOI 10.1022/dev.21361.

Wahlsten, D. (2016). Dynamic heredity. *Developmental Psychobiology*, *58*, 419–421.

Wang, G., Bartell, T. R., & Wang, X. (2018). Preconception and prenatal factors and metabolic risk. In N. Halfon, C. Forrest, R. M. Lerner, & E. Faustman (Eds.), *Handbook of Life Course Health Development Science* (pp. 47–59). New York: Springer.

Wang, J., Batanova, M., Ferris, K. A., & Lerner, R. M. (2016). Character development: Tests of relational developmental systems models. *Research in Human Development, 13(2)*.

Wang, J., Ferris, K. A., Hershberg, R. M., & Lerner, R. (2015). Developmental Trajectories of Youth Character: A Five-Wave Longitudinal Study of Cub Scouts and Non-Scout Boys. *Journal of Youth and Adolescence, 44*(12), 2359–2373.

Wapner, S. (1969). Organismic-developmental theory: Some applications to cognition. In J. Langer, P. Mussen, & N. Covington (Eds.), *Trends and issues in developmental theory* (pp. 35–67). New York: Holt, Rinehart & Winston.

Wapner, S. (1977). Environmental transition: A research paradigm deriving from the organismic-developmental systems approach. In L. van Ryzin (Ed.), *Wisconsin Conference on Research Methods in Behavior Environment Studies Proceedings* (pp. 1–9). Madison: University of Wisconsin.

Wapner, S. (1981). Transactions of persons-in-environments: Some critical transitions. *Journal of Environmental Psychology, 1*, 223–239.

Wapner, S. (1986). An organismic-developmental systems approach to the analysis of experience and action. Paper presented at the conference on Holistic Approaches to the Analysis of Experience and Action, University of Catania, Sicily, Italy.

Wapner, S. (1987). A holistic, developmental, systems-oriented environmental psychology: Some beginnings. In D. Stokols & I. Altman (Eds.), *Handbook of environmental psychology* (pp. 1433–1465). New York: Wiley.

Wapner, S. (1995). Toward integration: Environmental psychology in relation to other sub-fields of psychology. *Environment and Behavior, 27*, 9–32.

Wapner, S., & Demick, J. (1990). Development of experience and action: Levels of integration in human functioning. In G. Greenberg & E. Tobach (Eds.), *Theories of evolution of knowing. The T.C. Schneirla conference series* (Vol. 4, pp. 47–68). Hillsdale, NJ: Erlbaum.

Wapner, S., & Demick, J. (1991). Some relations between developmental and environmental psychology: An organismic-developmental systems perspective. In R. M. Downs, L. S. Liben, & D. S. Palermo (Eds.), *Visions of aesthetics, the environment and development: The legacy of Joachim F. Wohlwill* (pp. 181–211). Hillsdale, NJ: Lawrence Erlbaum.

Wapner, S., & Demick, J. (1992). The organismic-developmental, systems approach to the study of critical person-in-environment transitions through the life span. In T. Yamamoto & S. Wapner (Eds.), *Developmental psychology of life transitions* (pp. 25–49). Kyoto, Japan: Kitaohji.

Wapner, S., & Demick, J. (1998). Developmental analysis: A holistic, developmental, systems-oriented perspective. In W. Damon (Series Ed.) and R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (5th ed., pp. 761–805). New York: Wiley.

Washburn, S. L. (1961). *Social life of early man.* New York: Wenner-Gren Foundation for Anthropological Research.

Washburn, S. L. (1982). Is it sex? *The Sciences, 22*, 2.

Watson, J. B. (1913). Psychology as the behaviorist views it. *Psychological Review, 20*, 158–177.

Watson, J. B. (1914). *Behavior: An introduction to comparative psychology*. New York: Holt.

Watson, J. B. (1918a). *Psychological care of infant and child*. New York: Norton.

Watson, J. B. (1918b). *Psychology from the standpoint of a behaviorist*. Philadelphia: Lippincott.

Watson, J. B. (1924). *Behaviorism*. New York: Norton.

Watson, J. B. (1928). *Psychological care of infant and child*. New York: Norton.

Watson, J. B., & Raynor, R. (1920). Conditional emotional reactions. *Journal of Experimental Psychology, 3*, 1–14.

Weiss, H. B., & Greene, J. C. (1992). An empowerment partnership for family support and education programs and evaluations. *Family Science Review, 5*, 131–148.

Werner, H. (1948). *Comparative psychology of mental development*. New York: International Universities Press.

Werner, H. (1957). The concept of development from a comparative and organismic point of view. In D. B. Harris (Ed.), *The concept of development* (pp. 125–148). Minneapolis: University of Minnesota Press.

Werner, H., & Kaplan, B. (1956). The developmental approach to cognition: Its relevance to the psychological interpretation of anthropological and ethnolinguistic data. *American Anthropologist, 58*, 866–880.

Werner, H., & Kaplan, B. (1963). *Symbol formation: An organismic-developmental approach to language and the expression of thought*. New York: Wiley.

Werner, H., & Wapner, S. (1949). Sensory-tonic field theory of perception. *Journal of Personality, 18*, 88–107.

Werner, H., & Wapner, S. (1952). Toward a general theory of perception. *Psychological Review, 59*, 324–338.

Wertsch, J. V. (1981). The concept of activity in Soviet psychology: An introduction. In J. V. Wertsch (Ed.), *The concept of activity in Soviet psychology* (pp. 3–36). Armonk, NY: Sharpe.

Wertsch, J. V. (1985). *Vygotsky and the social formation of mind*. Cambridge, MA: Harvard University Press.

West-Eberhard, M. J. (2003). *Developmental plasticity and evolution*. New York: Oxford University Press.

White, S. H. (1968). The learning-maturation controversy: Hall to Hull. *Merrill-Palmer Quarterly, 14*, 187–196.

White, S. H. (1970). The learning theory approach. In P. H. Mussen (Ed.), *Carmichael's manual of child psychology* (3rd ed., Vol. 1, pp. 657–702). New York: Wiley.

Whitehead, A. N. (1929/1978). *Process and reality: Corrected edition*. New York: Free Press.

Whitehead, A. N. (1936). Harvard: The future. *Atlantic Monthly, 158*, 267.

Wiesel, T. N., & Hubel, D. H. (1965). Extent of the recovery from the effects of visual deprivation in kittens. *Journal of Neurophysiology, 28*, 1060–1072.

Wigfield, A., Eccles, J. S., Fredricks, J., Simpkins, S., Roeser, R., & Schiefele, U. (2015). Development of achievement motivation and engagement. In M. Lamb (Ed.), *Handbook of child psychology and developmental science: Vol. 3 Socioemotional processes* (7th ed., pp. 657–700). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Willems, E. P. (1973). Behavioral ecology and experimental analysis: Courtship is not enough. In J. R. Nesselroade & H. W. Reese (Eds.), *Life-span developmental psychology: Methodological issues* (pp. 195–217). New York: Academic Press.

Williams, G. C. (1966). *Adaptation and natural selection*. Princeton, NJ: Princeton University.

Willis, S. L. (1982). Concepts from life span developmental psychology: Implications for programming. In M. Okun (Ed.), *New directions for continuing education: Programs for older adults* (No. 14, pp. 3–10). San Francisco: Jossey-Bass.

Willis, S. L., & Baltes, P. B. (1980). Intelligence in adulthood and aging: Contemporary issues. In L. Poon (Ed.), *Aging in the 1980's: Psychological issues* (pp. 260–272). Washington, DC: American Psychological Association.

Wilson, E. O. (1975a). *Sociobiology: The new synthesis*. Cambridge, MA: Harvard University Press.

Wilson, E. O. (1975b, December 11). For sociobiology. *New York Review of Books*.

Wilson, E. O. (1975c, October 12). Human decency is animal. *New York Times Magazine*.

Wilson, E. O. (1978). *On human nature*. Cambridge, MA: Harvard University Press.

Wilson, E. O. (1980). A consideration of the genetic foundation of human social behavior. In G. W. Barlow & J. Silverberg (Eds.), *Sociobiology: Beyond nature/nurture* (pp. 295–305). Boulder, CO: Westview.

Winston, A. S. (1996). The context of correctness: A comment on Rushton. *Journal of Social Distress and the Homeless, 5*, 231–250.

Winston, A. S. (1997a). Genocide as a scientific project. *American Psychologist, 52*, 182–183.

Winston, A. S. (1997b). Rushton and racial differences: Further reasons for caution. *Journal of Social Distress and the Homeless, 6*, 199–202.

Witherington, D. C. (2011). Taking emergence seriously: The centrality of circular causality for dynamic systems approaches to development. *Human Development, 54*, 66–92.

Witherington, D. C. (2014). Self-organization and explanatory pluralism: Avoiding the snares of reductionism in developmental science. *Research in Human Development*, *11*(1), 22–36.

Witherington, D. C. (2015). Dynamic systems in developmental science. In W. F. Overton & P. C. Molenaar (Eds.), *Handbook of child psychology and developmental science: Vol. 1 Theory and method* (7th ed., pp. 63–112). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Witherington, D. C., & Heying, S. (2013). Embodiment and agency: Toward a holistic synthesis for developmental science. In R. M. Lerner & J. B. Benson (Eds.), *Advances in child development and behavior: Embodiment and epigenesis: Theoretical and methodological issues in understanding the role of biology within the relational developmental system. Part A. Philosophical, theoretical, and biological dimensions* (pp. 161–192). London: Elsevier.

Witherington, D. C., & Lickliter, R. (2016). Integrating development and evolution in psychological science: Evolutionary developmental psychology, developmental systems, and explanatory pluralism. *Human Development, 59*, 200–234.

Witherington, D. C., & Lickliter, R. (2017, February 17). Clarifying our framing of development and evolution in developmental systems terms: A response to the commentary by Bjorklund [Letter to the Editor]. *Human Development, 59*. Retrieved from www.karger.com/WebMaterial/Showfile/536848.

Woese, C. R. (2004). A new biology for a new century. *Microbiology and Molecular Biology Reviews, 68*(2), 173–186.

Wohlwill, J. F. (1963). Piaget's system as a source of empirical research. *Merrill-Palmer Quarterly, 9*, 253–262.

Wohlwill, J. F. (1970). The age variable in psychological research. *Psychological Review, 77*, 49–64.

Wohlwill, J. F. (1973). *The study of behavioral development*. New York: Academic Press.

Wohlwill, J. (1980). Cognitive development in childhood. In O. G. Brim, Jr. & J. Kagan (Eds.), *Constancy and change in human development* (pp. 359–444). Cambridge, MA: Harvard University Press.

Wolff, P. H. (1960). The developmental psychologies of Jean Piaget and psychoanalysis. *Psychological Issues, 2* (Whole No. 5).

Woolard, J. L., & Scott, E. (2009). The legal regulation of adolescence. In R. M. Lerner & L. Steinberg (Eds.), *Handbook of adolescent psychology: Vol. 2 Contextual influences on adolescent development* (3rd ed., pp. 345–371). Hoboken, NJ: Wiley.

Yarczower, M., & Hazlett, L. (1977). Evolutionary scales and anagenesis. *Psychological Bulletin, 84*, 1088–1097.

Yarczower, M., & Yarczower, B. S. (1979). In defense of anagenesis, grades, and evolutionary scales. *Psychological Bulletin, 86*, 880–884.

Yates, F. E., & Iberall, A. S. (1973). Temporal and hierarchical organization in biosystems. In J. Urquhart & F. E. Yates (Eds.), *Temporal aspects of therapeutics* (pp. 17–34). New York: Plenum.

Yong, E. (2017, February) How brain scientists forgot that brains have owners. *The Atlantic.* Retrieved from: www.theatlantic.com/science/archive/2017/02/how-brain-scientists-forgot-that-brains-have-owners/517599.

Yoshikawa, H., Weisner, T. M., Kalil, A., & Way, N. (2008). Mixing qualitative and quantitative research in developmental science: Uses and methodological choices. *Developmental Psychology, 44*(2), 344–354.

Zaff, J. F., Aasland, K., McDermott, E., Carvalho, A., Joseph, P., & Jones, E. P. (2016). Exploring positive youth development among young people who leave school without graduating high school: A focus on social and emotional competencies. *Qualitative Psychology, 3*, 26–45.

Zaff, J. F., Boyd, M., Li, Y., Lerner, J. V., & Lerner, R. M. (2010). Active and engaged citizenship: Multigroup and longitudinal factorial analysis of an integrated construct of civic engagement. *Journal of Youth and Adolescence*, *39*(7), 736–750.

Zaff, J. F., Kawashima-Ginsberg, K., Lin, E. S., Lamb, M., Balsano, A., & Lerner, R. M. (2011). Developmental trajectories of civic engagement across adolescence: Disaggregation of an integrated construct. *Journal of Adolescence*, *34*(6), 1207–1220.

Ziegler, H. (1893). *Die Naturwissenschaft und die Socialdmokratische Theorie*. Stuttgart, Germany: Enke.

Zigler, E. (1998). A place of value for applied and policy studies. *Child Development, 69*, 532–542.

Zuckerman, B., & Keder, R. D. (2015). Children in medical settings. In M. H. Bornstein & T. Leventhal (Eds.), *Handbook of child psychology and developmental science: Vol. 4 Ecological settings and processes* (7th ed., pp. 574–615). Editor-in-chief: R. M. Lerner. Hoboken, NJ: Wiley.

Zukav, G. (1979). *The dancing Wu Li masters*. New York: Bantam.

Photo Credits

Page 6 Lee Lockwood/Getty Images. **13** Courtesy of Devon Cattell. **59** The Granger Collection, New York. **61** Archives of the History of American Psychology–The University of Akron. **62** Archives of the History of American Psychology–The University of Akron. **65** Archives of the History of American Psychology–The University of Akron. **66** Courtesy of Beeldbank Nationaal Archief (CC BY-SA 3.0). **67** Courtesy of the University of Chicago. **67** Courtesy of the University of Chicago. **74** Courtesy of the University of Michigan, photo by Bob Kalmbach. **77(R)** Courtesy of Harvard News Office. **81** Jerome Kagan (3, p. 123) Courtesy of Harvard News Office. **93** Photo by Bill Pierce/The LIFE Images Collection/Getty Images. **103** Culver Pictures. **139** Courtesy of Educational Testing Service. **171** Courtesy of Elijah Wald (CC BY-SA 4.0). **174** Courtesy of Fordham University, photo by Tommy Weber. **197** Courtesy of Brachyrhynchos5 (CC BY-SA 3.0). **203** Alex B. Novikoff Courtesy of University of Vermont Archives. **207** McGill University Archives. **226(L)** Daniel Lehrman Reprinted with permission from The National Academies Press. **278** Photo by Steve Hansen/The LIFE Images Collection/Getty Images. **335** Photo by Dewey Neild. **375** Max Planck Gesellschaft/Archiv (CC BY-SA 3.0). **379** John Patriquin/Portland Press Herald via Getty Images. **413** Sirswindon at English Wikipedia (CC BY-SA 3.0). **423** Rich Rygh, The Capital Times. **432** Courtesy of Psychology Department at University of Illinois-Urbana Champaign. **437** Photo by Don Hamerman.

Name Index

Page numbers in italic type refer to figures and photographs; those in bold type refer to tables.

Aboud, F. E. 185
Abravanel, E. 211
Addis, K. M. 504
Adelson, J. 298
Agans, J. P. 11, 18, 374, 490
Alcock, J. 172
Alexander, C. N. 74
Alfeld, C. 245
Allen, K. R. 471
Allister, L. 181
Allport, G. W. 163, 185
Altman, I. 352
Anastasi, A. 15, 50, 170, 173–177, *174,* 183, 184, 188, 189–190, 192, 193, 198, 223, 228, 286, 294, 351, 356, 421, 426, 450
Anderson, P. W. 95, 96, 98, 105, 128
Angrist, J. D. 506
Angus, R. M. 510
Anthony, E. J. 298
Antonucci, T. C. 296
Arbeit, M. 374, 508
Ardelt, M. 27
Aristotle 36, 52–53, 54, 56
Aronson, L. R. 201, 216, 476
Atz, J. W. 374, 460, 461
Ayorech, Z. 273, 352, 452

Babbin, S. F. 42, 166
Bachman, J. G. 496
Backman, C. W. 186
Baer, D. M. 3, 19, 55, 72, 82, 83, 99, 100, 101, 102, *102,* 103, 104, 114, 116, 118, 121, 127, 139, 153, 163, 199, 200, 263, 370
Bain, A. 55
Baldwin, J. M. 91, 211, 289
Ball, C. 291
Ballard, P. J. 510
Baltes, M. M. 22, 122, 254, 317, *317,* 321, 323, 327, 339, 364
Baltes, P. B. 8, 13, 17, 18, 19, 20, 22, 24, 30, 42, 49, 66, 72, 73, 75, 76, 77, 81, 83, 85, 122, 132, 137, 141, 160, 201, 235, 250, 251, 254, 255, 256, 259, 260, 264, 265, 286, 287, 294, 295, 297, 310–311, *313,* 313–314, **315,** 316–321, **319,** *320,* 323, 326, 327, 330, 331, 339, 344, 364, 368, 377, 489, 491, 493, 494, 497, 499, 510
Bandura, A. 28, 80, *80,* 146, 298
Barash, D. P. 453, 456, 457, 458, 460, 461
Barber, B. 296
Barlow, G. W. 399, 452
Barr, R. G. 227
Bartell, T. R. 180
Bartz, D. 85, 519
Basser, L. S. 220
Bateson, P. 18, 31, 32, 36, *36,* 37, 38, 39, 41, 84, 219, 222, 254, 266, 273, 373, 424, 467, 478, 479
Bauer, E. 388, 397
Baumrind, D. 76
Baxter, B. 221
Beach, F. A. 63
Beals, K. L. 472
Bearer, E. 87, 136, 172
Bechtold, J. 519
Belitser, S. V. 506
Bell, R. Q. 75, 121, 293
Belsky, J. 38, 50, 86, 87, 89, 170, 187, 201, 250, 273, 274, 280, 373, 391, 434, 471, 477, 483–484
Benner, A. D. 85
Benson, J. B. 216, 269, 297, 299, 307, 310, 360, 369, 510
Benson, P. L. 26, 35, 36, 83
Bentley, A. F. 123
Berg, P. 132
Berger, P. L. 322
Bergman, L. R. 22, 489, 505
Berkel, C. 85, 288–289, 519
Berkowitz, M. W. 29, 30, 491
Berlyne, D. 68
Berman, N. 221
Berndt, T. J. 153
Berra, Y. 18
Bidell. T. R. 11, 83, 85, 136, 141, 312, 331, 355, 356–358, 359–360, 361–362, 365, 367, 368, 380, 417, 489
Bigler, R. S. 185, 186, 188
Bijou, S. W. 3, 19, 55, 72, 82, 83, 99, 100, 101, 102, *102,* 104, 114, 116, 117, 118, 121, 139, 153, 163, 199, 200, 262, 263, 370
Binding, K. 50, 201, 388, 389, 392, 484
Binet, A. 19, 61
Birch, H. 181, 201, 205, 209, *210,* 210–211, 225, 226
Birkel, R. 511
Bisanz, G. L. 125
Bisanz, J. 125
Bitterman, M. E. 202, 246, 247–249, *248,* **249,** 460, 461
Bjorklund, D. F. 31, 38, 87, 88, 89, 105, 117, 121, 139, 172, 182, 217, 224, 250, 273, 277, 280, 341, 349, 352, 378, 474, 479, 481, 482, 483
Blakemore, C. 220
Blaze, J. 119
Block, J. 27, 85, 159, 163, 164, 165, 169, *253,* 254, 294, 298
Block, J. H. 254
Bloom, B. S. 193, 345
Bloom, L. 48
Blos, P. 64
Blum, R. W. 18
Blumberg, M. S. 36, 37, 222, 241, 477, 481, *482*
Blyth, D. A. 82, 493, 495
Bock, W. J. 464
Bodmer, W. F. 195
Boelsche, W. 390
Boiter, C. 301
Bolles, R. C. 97
Bonnen, J. T. 515
Bonner, J. 119, 173, 270, 515
Bonner, S. B. 84
Boomsma, D. I. 419, 448
Boring, E. G. 18, 48, 514

Bornstein, M. H. 9, 23, 29, 42, 43, *43,* 46, 48, 79, 84, 85, 86, 120, 137, 138, 181, 201, 272, 288, 290, 295–296, 305, 361, 487, 488, 489–490, 491, 496
Botwinick, J. 506
Bouchard, T. J., Jr. 401, 404, 412, 414, **415,** 416, 417, 418, 419, 420, 421, 424, 426, 429, 435, 437, 440
Bowers, E. P. 11, 17, 85, 101, 198, 306, 309, 481, 496–497
Bowers, K. S. 101, 481, 482
Bowlby, J. 116
Bowles, R. P. 502
Boyatzis, C. J. 84, 301, 302
Boyce, W. T. 181
Boyer, E. L. 514
Bradley, R. H. 519
Brainerd, C. J. 117, 153, 502
Brandtstädter, J. 8, 18, 22, 24, 28, 29, 51, 83, 85, 111, 127, 201, 208, 211, 227, 255, 256, 310, 313, 318, 321, 322–323, *323,* 323–326, 327, 331, 332, 339, 354, 356, 363, 364, 467, 510
Brazelton, T. B. 225, 295
Breland, K. 248
Breland, M. 248
Brent, S. B. 128, 284, 343
Brim, O. G., Jr. 8, 18, 72, 76, *77,* 78, 80, 122–123, 249–250, 252, 253, 254, 275, 294
Broca, P. 252
Bronfenbrenner, U. 21, 24, 29, 45, 67–68, 73, 74, 75–76, 79, 80, 82, 83, 85, 86, 132, 133, 137, 138, 180, 182, 186, 270, 276, 277, 293, 294, 296, 297, 302, 307, 312, 313, *335,* 335–339, 351, 363, 377, 439, 440, 451, 479, 489, 498, 511, 512
Bronk, K. C. 11, 300
Brooks-Gunn, J. 120, 298, 299, 510
Brown, D. A. 288
Brown, D. D. 132
Brown, J. 373
Brown, J. L. 12, 17, 519
Brown, J. R. 510
Brown, R. T. 220
Bruer, J. T. 251, 252, 254
Bruner, J. 105, 141
Brunswik, E. 75, 79, 507, 518
Buchanan, C. M. 296
Bucher, A. 83
Buck, C. 372–373, 375
Buckingham, M. H. 26, 293
Buech, B. V. 122, 497
Bühler, C. 67, 78, 211
Bundick, M. 300
Burt, C. 15, 405–411, *406,* 414, 417, 428, 440, 447
Burton, L. M. 44, 507
Busch-Rossnagel, N. A. 8, 75, 85, 111, 115, 121, 126, 127, 180, 200, 227, 252, 254, 255, 276, 287, 293, 294, 295, 322, 339, 363, 467
Busch, N. A. 12, 17, 519
Buss, A. R. 153, 440
Buss, C. 536
Butcher, L. M. 418
Butler, E. A. 38, 50, 373, 471, 477
Bynner, J. 298

Cairns, B. D. 2, 19, 21, 48, 64, 65, 83–84, 85, 86, 87, 91, 348
Cairns, R. B. 2, 19, 21, 48, 64, 65, 83–84, 85, 86, 87, 91, 255–256, 343, 348
Callina, K. S. 8, 24, 29, 30, 39, 44, 75, 80, 91, 189, 288, 308, 487, 510, 513, 518
Calvin, J. 54
Cameron, C. E. 28, 38, 84
Campbell, C. B. G. 53
Campbell, D. 478
Campbell, D. T. 504–505, 510
Campbell, J. J. 71
Campbell, R. L. 142
Cantor, P. 490
Capaldi, E. J. 75
Capitanio, J. P. 204
Caplan, A. L. 172, 452
Card, N. A. 486, 488
Carlier, M. 414
Carmichael, L. 19
Carmichael, S. L. 181
Carpendale, J. I. M. 84
Carr, D. 301
Carr, S. 181
Carr, T. H. 105
Carstensen, L. L. 317, 318, 323
Carter, C.O. 427
Case, R. 141
Catalano, R. P. 299
Cattell, R. B. *13,* 13–14, *14,* 20, 159, 499
Cauffman, E. 519
Cavalli-Sforza, F. 195, 422, 423
Cavalli-Sforza, L. L. 422, 423
Ceci, S. J. 336, 439, 440, 451
Cernovsky, Z. Z. 472, 473, *473*
Cervone, D. 291, 359
Chabris, C. F. 419
Chandler, M. J. 108
Chapman, M. 74
Charney, E. 170, 191, 418, 438, 440, 441–442, *442,* 445, 451
Charnov, E. L. 70
Chavajay, P. 365
Chen, E. 518
Chertok, F. 508
Chess, S. 79, 127, 181, 201, 225, *226,* 226–227, 277, 287, 295, 296
Chibucos, T. 514
Chiriboga, D. 141
Chomsky, N. A. 68, 114
Chorover, S. L. 380, 381, 383, 384
Chow, K. L. 221
Chow, S. M. 502
Clark, G. H. 51
Clarke, A. D. B. 253
Clarke, A. M. 253
Clausen, J. A. 141
Clow, C. L. 184, 451
Cohen, J. L. 452
Cohen, S. 465
Colby, A. 507, 509, *509,* 510
Cole, M. 28, 189, 363, 364, 365
Cole, S. W. 33, 34, 35, 36, 41, 119, 172, 173, 177, 182, 191, 195, 269, 270, 272, 275, 373, 440, 449
Coleman, M. 85, 519
Collins, L. M. 486, 488, 491, *491,* 492, 493, 494, 499, 501, 504, 510
Collins, W. A. 9, 48, 87, 90, 440, 450, 451, 462
Colombo, J. 29, 219, 220, 221, 233
Connell, J. P. 513
Connolly, K. 219
Conradt, E. 18, 449
Conway, J. 405, 406, 408, 409–410, 411, 414
Cooper, C. R. 365
Copple, C. 18
Corley, R. 440
Corneal, S. E. 492, 499
Cornelius, S. W. 122, 260
Costa, P. T., Jr. 24, 26, 27, 38, 41, 291, 374, 430, 487
Cox, V. 383, 385, 392
Coyle, E. F. 288
Craig, I. W. 419
Craighead, W. E. 256
Crawford, M. L. 221, 532
Crick, F. H. C. 464, 465
Cronbach, L. J. 79, 508, 510
Crosnoe, R. 85
Crossman, E. J. 84, 289
Crouter, A. C. 75
Cryns, A. G. 472
Csikzentmihalyi, M. 83, 85
Cumming, E. 122
Cummings, E. M. 84
Cunha, F. 18
Cunningham, M. 302, 303
Curtiss, S. 220
Cynader, M. 221

D'Andrade, R. G. 325
Dale, P. S. 273, 352, 452
Damasio, A. R. 348
Damasio, H. 348
Damon, W. 11, 27, 29, 48, 68, 71, 83, 84, 86, 108, 110, 136, 299, *300,* 300–301, 302, 347, 365, 507, 508, 509, 510
Daniels, D. 440
Dannefer, D. 262
Darwin, C. 28, 31, 52, 53, 56–58, *57,* 58, 59, 61, 62, 246, 464, 476
Datan, N. 78, 141
Davis, A. 121, 235
Davis, J. O. 180
Davis, O. S. 419
Davison, M. L. 141, 146
Dawidowicz, L. S. 395
Dawkins, R. 100, 117, 374, 399, 453–454, *454*, 455, 457, 458
Day, D. 263
de Beer, G. R. 269, 282, 467–468

de Boer, A. 506
De Mol, J. 84
de Santillana, G. 271
De Stefanis, I. 29, 317
De Waele, D. 108
DeFries, J. C. 273, 352, 383, 414, 421–422, 437, 440–441
Del Giudice, M. 31, 50, 87, 224, 474, 479, 480, 481, 482, 483
Demick, J. 125, 229, 285, 312, 351, 352, 353, *353,* 354, 355, 360, 368
Demo, D. H. 471
Descartes, R. 15, 19, 49, 50, 54–55
DeSouza, L. M. 11, 18, 374, 490, 508
Deutsch, N. L. 18, 22, 31, 44, 489, 505, 507, 508, 510
Dewey, J. 91, 123
Diamond, L. M. 84
Dick, A. S. 287, 289
Dickens, W. T. 452
Dickenson, J. 84
Dittmann-Kohli, F. 254, 317
Dixon, R. A. 51, 62, 74, 75, 76, 78, 94, 105, 115, 122, 200, 254, 276, 317, 326
Dobzhansky, T. 31, 420
Dodd, S. M. 472
Dodge, K. A. 519
Doerr, A. 373
Doherty, T. S. 119
Dolan, C. V. 448
Dollard, J. 63, 121, 246
Doob, L. W. 63
Doolittle, W. F. 465
Dorfman, D. D. 410
Douvan, J. D. 298
Dowling, J. E. 221
Downey, G. 291
Draper, P. 38, 41, 50, 280, 373, 471, 477
Dunbar, R. I. M. 452–453, 461, 463
Duncan, C. P. 435
Duncan, G. J. 519
Dunn, L. C. 192
Dupéré, V. 1, 85, 519
Dupré, J. 478
Dutta, R. 23, 24, 28, 38, 84, 189, 288, 363, 364, 365, 367, *367,* 474, 517
Dweck, C. S. 252
Dylan, B. 1

Eacker, J. N. 107
Easterbrooks, M. A. 18
Eaton, S. C. 44, 507
Eaves, L. J. 429
Ebbinghaus, H. 18, 514
Eccles, J. S. 84, 134, 245, 296, 299
Eckensberger, L. H. 51
Edelman, G. M. 87, 108, 270, 271
Edey, M. A. 28, 61, 211, 280, *281,* 281–282, 286, 471
Eisenberg, L. 224, 380, 381, 384
Ejesi, K. 84
Elder, G. H., Jr. 9, 18, 20, 21, 22, 23, 24, 26, 27, 29, 30, 33, 38, 45, 48, 80, 81, 83, 85, 86, 122, 132, 133, 141, 250, 263, 270, 276, 277, 287, 294, 297, 312, 313, 316, 328–335, *329,* 333, 338, 377, 487, 489, 498, 499, 507
Ellis, B. J. 31, 38, 50, 87, 117, 121, 139, 172, 182, 217, 224, 250, 273, 277, 280, 341, 352, 373, 374, 378, 471, 474, 477, 479, 480, 481, 482, 483
Emler, N. P. 277, 280
Emmerich, W. 87, *139,* 139–140, 148, 150, 151, 153, 154, 155, 158, 161, 163, 166, 259
Endler, N. S. 349
Entringer, S. 214
Erikson, E. H. 3, 4, 19, 57, 64, 67, 72, 111, 118, 121, 141, 143, 144, 154, 159, *159,* 162, 199, 217, 233, 262, 263, 458, 496, 508
Erlenmeyer-Kimling, L. 414, 430
Escalante, J. 277
Escher, M. C. 192
Esposito, N. J. 130
Ettekal, A. V. 514
Evans, R. I. 383
Eysenck, H. J. 250, 405, 409, 412–414, *413,* 421, 424, 428, 429, 435

Falconer, D. S. 462
Farber, S. L. 417, 420, 435
Faulkner, D. W. 440
Faustman, E. 181, 516
Fawcett, A. J. 105
Featherman, D. L. 78, 189, 277, 294
Feiring, C. 69
Feldlaufer, H. 296
Feldman, D. H. 105, 141, 142, 148–150, *149,* 151, 153, 154, 312, 365, 368
Feldman, M. 37, 87, 170, 274, 294, 424, 425, 426, 431, 432, *433,* 436, 440, 448, 474
Ferguson, D. C. 221
Fernandez, P. 295
Fetterman, D. M. 513, 517
Fine, M. A. 471
Fischer, E. 380, 388, 489
Fischer, K. W. 8, 11, 22, 48, 83, 84, 85, 105, 136, 141, 200, 204, 240, 312, 331, *355,* 355–363, 365, 366, 367, 368, 417, 450, 479, 494
Fisher, C. B. 12, 17, 18, 23, 41, 45, 90, 91, 134, 138, 270, 273, 311, 514–515, *515,* 515–516, 519–520
Fisher, H. E. 280–281
Fiske, D. W. 505, 510
Flanagan, C. 296
Flavell, J. H. 48, 66, 68, 117, 142, 143–*143,* 144–145, 147–148, 150, 151, 164, 228, 508
Flay, B. R. 299
Flohr, H. 455
Floyd, D. 298, *299*
Foner, A. 141
Ford, D. H. 9, 10, 23, 24, 82, 83, 94, 112, 121, 149, 153, 176, 200, 201, 267, 270, 276, 283–287, *284, 285,* 288, 289, 293, 296, 297, 311, 312, 322, 338, 342, 357, 360, 421, 440, 466, 467, 492
Forrest, C. B. 180, 181, 191, 516
Fox, C. V. 508
Fox, M. 508
Fraiberg, S. 251
Frankel, J. 424
Franklin, S. 245
Fredricks, J. 84, 245
Freedman, D. G. 41, 117, 374, 391, 399, 453, 456, 457, 458, 460, 461
Freel, M. A. 513, 517
Freeman, F. N. 406, 411, 417, 435
Frenkel-Brunswik, E. 67
Freud, A. 4, 64, 67, 81, 83, 241, 298, 495
Freud, S. 3, 4, 52, 57, 64, 67, 72, 111, 121, 131, 140, 143, 153, 154, 155, 199, 458, 508
Freund, A. M 22, 29, 255, 256, 317, *318,* 320, 327, 364, 507
Friedman, R. 319
Fromkin, V. 220
Fujita, F. 502
Fuligni, A. J. 296
Fulker, D. W. 428
Funk, A. J. 119
Furrow, J. L. 302
Futch Ehrlich, V. A. 508

Gagné, R. M. 235
Gallatin, J. E. 60, 61
Galton, F. 274
Gandhi, M. 30
Ganong, L. 85, 519
Garbarino, J. 297
Garcia-Coll, C. 84, 136, 172, 305
Gardner, H. 141
Gardner, W. P. 70
Garrett-Peters, R. 44, 507
Garstang, W. 269, 467–468
Gartner, L. M. 133
Gasman, D. 381, 387
Gauvain, M. 84, 288, 363
Geiwitz, J. 263
Geldhof, G. J. 11, 17, 27, 28, 29, 38, 84, 85, 198, 309, 496–497, 513
Gengerelli, J. A. 64
Georgoudi, M. 73
Gergen, K. J. 79
Gesell, A. L. 19, 58, 61, *62,* 62–63, 262
Gestsdóttir, S. 28, 29, 255, 309, 317
Gewirtz, J. L. 55, 99, 100, 370
Gianutsos, J. 87, 216, 396
Gibb, S. 20
Gibbons, C. M. 510
Gibson, E. J. 68, 145, 211
Gillie, O. 409–410, 411
Ginsberg, L. H. 78
Ginsberg, R. 504
Gissis, S. B. 18, 32, 34, 37, 39, 40, 41, 477
Glaserfeld, E. von 141, 142
Gleick, J. 343, 348
Gluckman, P. 32, 36, 37, 39

Goddard, H. H. 61, 252
Going, J. 301
Goldberg, L. R. 291, 422–423
Goldberger, A. S. 402, 410, 411, 412, 416, *423,* 424, **427,** 427–428, **428,** 429, 436, 440
Goldschmidt, R. 269, 467–468
Gollin, E. S. 50, 83, 122, 125, 126, 132, 200, 213, 253–254, 266
Goodnow, J. J. 85, 288, 363, 364
Goodwin, B. 476
Gootman, J. A. 299
Gore, A. 519
Gore, T. 519
Gorey, K. M. 472
Gottesman, I. L. 448
Gottlieb, G. 18, 23, 26, 35, 38, 39, 50, 56, 57, 61, 83, 87, 91, 108, 112, 114, 115, 118, 119, 121, 122, 124–125, 132, 139, 144, 170, 172, 173, 181, 182, 183, 191, 198, 200, 201, 213, 216, 217, 223, 227, 266, *267,* 267–273, 274, 275, 276, 277, 283, 286, 287, 293, 297, 312, 340, 341, 342, *342,* 347, 348, 349, 352, 354, 355, 358, 359, 360, 372, 417, 421, 424, 438, 439, 440, 448, 449, 466, 467, 468, 474, 476, 489
Gould, S. J. 18, 57, 60, 86, 87, 171, 172, 173, 188, 204, 211, 250, 252, 277–279, *278,* 280, 282–283, 286, 322, 373, 401, 405–406, 409–410, 411, 412, 413–414, 424, 425–426, 440, 452, 460, 462–463, 464–466, 468, 476, 477
Goulet, L. R. 20
Graham, J. W. 87
Graham, S. 63, 188
Green, B. L. 44, 506
Green, E. J. 254
Greenberg, G. 8, 87, 105, 110, 125, 173, 175, 198, 203, 204, 216, 217, 266, 267, 268, 273, 283, 297, 349, 440, 452, 476, *476,* 477–478
Greenberg, M. T.
Greene, B. 319
Greene, J. C. 517
Greenfield, P. M. 363
Greenough, W. T. 254
Griffin, F. 221
Griffith, A. N. 508
Griffiths, P. E. 172, 173
Grimm, K. 22, 42, 43, 155, 160, 164, 489, 491, 500, 501, 502, 505
Gross, C. G. 87, 216, 396
Grouse, L. D. 119, 173, 270
Gruber, H. E. 417, 421
Grzimek, B. 460
Gump, P. V. 181
Guo, S. W. 434
Gutmann, D. L. 66
Guttmann, D. L. 263

Haan, N. 263
Habermas, T. 29, 317
Hadler, N. M. 422
Haeckel, E. 53, 58–60, *59,* 61, 262, 278, 381, 382, 387, 389, 390, 392, 397
Hagen, J. W. 20
Haggerty, R. J. 189
Hailman, J. P. 481
Haith, M. 68
Halfon, N. 180, 181, 189, 191, 516
Hall, G. S. 10, 19, 53, 57, 58, *58,* 59, 60–61, 67, 78, 235, 262, 278, 298
Hallett, D. 108
Halpern, C. T. 173, 216, 268
Hamburger, V. 19, 112, 114, 118
Hamilton, M. A. 189
Hamilton, S. F. 83, 189
Hammond, S. I. 25, 43, 491
Hanson, K. A. 317
Haraway, M. H. 87
Haraway, M. M. 198, 204
Hardaway, C. R. 84, 288
Hargraves, M. 517–518
Harkness, S. 201
Harley, D. 280
Harlow, H. F. 220
Harold, R. D. 296
Harpalani, V. 84, 302, 303, 304, 509
Harpending, H. 38, 41, 50, 477
Harper, L. V. 33, 45, 75, 121, 269
Harrington, G. M. 403
Harris, D. B. 9, 10, 19, 68, 99
Harris, E. M. 374
Harris, J. R. 440
Harris, P. L. 105
Hart, D. 108
Hartigan, J. A. 422
Hartley, D. 55
Haskins, R. 519
Hatano, G. 364
Havender, W. R. 424
Havighurst, R. J. 66, 67, *67,* 78
Hayes, L. J. 73, 94
Hayes, S. C. 73, 94
Hazlett, L. 204
Hearnshaw, L. S. 410, 411, 440
Hebb, D. O. 174, 188, 193, 194, 206–208, *207,* 211, 219, 228, 286, 325, 377, 444, 445
Heberer, G. 385
Heck, K. E. 305
Heckhausen, J. 22, 211, 255, 256, 313, 322, *326,* 326–328, 332, 339, 364
Heckman, J. 18, 44, 187, 505, 506, *506*
Hegel, G. W. F. 212, 245
Hein, A. 221
Hein, A. V. 211
Heinroth, O. 376
Held, R. 211
Hempel, C. G. 4–5, 9, 13
Henle, M. 64
Henry, W. E. 122
Hernandez, D. J. 297
Herrnstein, R. J. 63, 87, 188, 250, 252, 374, 401, 402, 414, 422, 423, 447, 449
Hershberg, R. M. 11, 18, 307, 490, 508
Hershkowitz, I. 71, 84
Hertzman, C. 181
Herzig, M. E. 225
Hess, E. H. 219
Hess, R. 386
Hetherington, E. M. 48, 294, 295, 316
Hewitt, J. K. 441–442
Heying, S. 343, 344, 440
Hickey, T. 75, 507
Hiernaux, J. 470
Higgins-D'Alessandro, A. 517
Higgins, E. T. 319
Hilgard, E. R. 245, 502
Hill, N. E. 85, 288–289, 519
Hilliard, L. J. 185, 186, 188, 512
Hinde, R. A. 219
Hines, M. 84, 186, 188
Hirsch, J. 87, 163, 170, 171, 173, 181, 182, 191, 192, 193, 194, 195, 196, 294, 402, 409, 411, 422, 423, 424, 425, 426, 431–432, *432,* 436, 440, 445, 447, 448, 452, 461, 462
Hitler, A. 375, 380, 382, 387, 388–389, 390, 395
Ho, M. W. 18, 31, 39, 41, 270, 273, 412, 418, 419, *419,* 424, 425, 437, 440, 467, 475, 477
Hoagwood, K. 137
Hobbes, T. 55
Hoche, A. 50, 201, 388, 389, 392, 484
Hodos, W. 53
Hoffman, H. S. 220
Hoffman, L. W. 437, 451
Hoffman, M. 68
Hoffman, R. F. 277
Hogan, M. J. 280
Hogan, R. 277
Hollingworth, H. L. 67, 78
Holzinger, K. J. 406, 411, 417, 435
Homans, G. C. 63, 97, 262
Honeycutt, H. 37, 38, 39, 41, 84, 87, 115, 118, 121, 124, 132, 168, 191, 195, 240, 267, 269, 341, 474, 476, 477, 478, 479
Hood, K. E. 173, 216, 255–256, 268, 269, 276, 343, 369
Horn, J. L. 494
Horowitz, F. D. 48, 87, 89, 421, 434, 436–437, *437,* 439, 440, 448, 452, 474, 484–485, 541
Howard, M. 408, 409–410, 411
Hsieh, C. 22, 489, 505
Huanh-Pollock, C. L. 105
Hubbard, R. 171, *171,* 172, 201
Hubel, D. H. 220, 221
Hull, C. L. 63
Hultsch, D. F. 74, 75, 76, 94, 115, 122, 200, 276, 507
Hume, D. 55
Humphries, J. E. 187
Hunt, J. McV. 253
Hunziker, U. A. 227
Huxley, J. 31
Hwang, C. P. 71
Hyson, M. 18

Iberall, A. S. 285
Ichimura, H. 18, 505
Imbens, G. W. 506
Inhelder, B. 211
International Human Genome Sequencing Consortium 170
Irwin, C. E., Jr. 18
Isaac, G. L. 280
Isaacowitz, D. M. 507

Jablonka, E. 18, 29, 31, 32–33, 34, 35, 37, 38, 39, 40, 41, 191, 266, 373, 440, 468, 474, 477
Jackson, J. S. 296
Jacob, F. 195
Jacobs, F. 513, 517
Jacquard, A. 433–434
Jambon, M. 291
James, J. B. 67, 134
James, W. 58
Janney, J. E. 67, 78
Jarvik, L. F. 414, 430
Jason, L. 508
Jelicic, H. 85, 180, 277
Jencks, C. 410
Jenkins, J. J. 78–79, 80
Jennings, J. A. 9
Jensen, A. R. 15, 172, 188, 374, 400, *400,* 401–402, 403, 407, 409, 410, 411, 412, 413–414, 422, 423, 424, 425, 426, 428, 430, 440, 445, 447, 519
Jensen, P. 137, 519
Jerison, H. J. 204
Jinks, J. L. 428
Johanson, D. C. 28, 61, 211, 280, *281,* 281–282, 286, 471
Johnson, C. 373
Johnson, H. E. 508
Johnson, J. A. 277, 280
Johnson, M. E. 141
Johnson, S. K. 26, 293
Johnston, L. D. 496
Jones, J. 18
Jopp, D. S. 12, 17, 519
Joseph, J. 37, 39, 101, 168, 188, 191, 273, 383, 404, 405, 412, 416, 417, 418, *418,* 419, 420, 423, 440, 442, 443, 444
Juel-Nielsen, N. 406, 411, 417, 418, 435
Jung, T. 155

Kaftarian, S. J. 513, 517
Kagan, J. 3, 8, 15, 72, 80, *81,* 92, 103, 122–123, 249–250, 252, 253, 254, 262, 275, 294
Kahana, M. J. 504
Kalikow, T. J. 224, 379, *379,* 380, 381, 382, 383, 385, 386–387, 390–391, 392
Kalil, A. 18, 44
Kamin, L. 171, 172, 250, 273, 274, 380, 381, 405, 406, 407, **407, 408,** 408–409, 411–412, 417, 418, 421, 423, 424, 435
Kantor, J. R. 3
Kaplan, B. 9, 10–11, 113, 115, 153, 229, 254, 351, 478
Kaplan, R. M. 43, 505
Kapuscik, J. 513
Katsiaficas, D. 508
Kauffman, M. B. 71, 75, 80, 82, 94, 114, 347
Kautz, T. 187
Keder, R. D. 18, 180, 181, 189
Keil, F. 64, 105, 355, 356, 364
Keller, E. F. 18, 36–37, *37,* 39, 373, 448
Keller, H. 184–185
Kelley, M. F. 141, 142
Kellner, H. 322
Kellogg Commission on the Future of State and Land-Grant Colleges 515, 519
Kellogg, L. A. 208
Kellogg, W. N. 208
Kempthorne, O. 433
Kendall, P. C. 256
Kendler, H. H. 128–130, *129,* 504
Kendler, T. S. 75, 128–130, *129*
Kennell, J. 250–251
Kenney, M. 220
Kerns, K. 84
Kessen, W. 62, 68, 117, 142
Keys, C. 508
Killen, M. 84, 289
Kimble, G. A. 245, 502–503, *503,* 504
King, M. L., Jr. 30
King, P. E. 84, 300, *301,* 301–302, 509
Kinsey, A. C. 457
Kipling, R. 462–463
Kitcher, P. 452
Klaus, M. 250–251
Kleckner, J. H. 465
Kluckhohn, C. 86, 140
Klungel, O. H. 506
Knopik, V. S. 273, 352, 437, 440–441
Koffka, K. 64
Kohlberg, L. 6, *6,* 141, 143, 154, 155
Kohler, W. 64, 298
Kollar, E. J. 270
Koller, S. H. 298, 305
Konner, M. 399, 452, 453, 454, 455, 458, 466
Korn, S. 186, 225, 247, 295
Kornhaber, M. 141
Koslowski, B. 225, 295
Kovas, Y. 273, 352, 452
Krashen, S. 219, 220
Kreppner, K. 91
Kreuger, J. 326
Krimsky, S. 270, 440
Kubisch, A. C. 513
Kuczynski, L. 84
Kuebli, J. 25, 43
Kuhlen, R. G. 67, 78
Kuhn, D. 245, 246, *368,* 368–369
Kuhn, T. S. 3, 7, 11, 15, 49, 73, 92, *93,* 136, 228
Kuo, Z.-Y. 91, 114, 125, 198, 200, 267, 269, 271, 273, 341, 347, 440, 468, 476

La Rooy, D. 84
Labouvie-Vief, G. 122, 318, 497
Lachman, M. E. 67, 134
Laland, K. N. 87
Lalonde, C. E. 108
Lamarck, J.-B. 34, 37, 40
Lamb, M. E. 18, 29, 31, 32–33, 35, 38, 46, *47,* 70–71, 84, 85, 191, 266, 288, 373, 440, 468, 474
Lambdin, C. 105
Landsell, H. 220
Lang, F. B. 317
Langer, E. J. 74, 100, 229, 235, 243, 245
Langer, J. 68
Larson, R. W. 85, 299, 510
Latour, B. 27
Laurendeau, M. 145, 151
Laursen, B. 488, 492
Lawrence, J. A. 85, 288, 363
Layzer, D. 188, 194, 412, 424, 430–431, *431,* 436, *446,* 446–447
Leaper, C. 84, 186
Lee-Painter, S. 69–70
Lee, C. D. 304
Lefford, A. 205, 209, *210,* 210–211
Leger, D. W. 204
Lehrman, D. S. 72, 173, 174, 198, 201, 216, 222, 223, 224, *224,* 228, 247, 250, 266, 267, 269, 271, 273, 341, 374, 376, 377, 440, 482
Leman, P. 298
Lenneberg, E. 220
Lenz, F. 388, 392, 397
Leont'ev, A. N. 365
Lerner, J. V. 11, 17, 44, 85, 127, 134, 179, 198, 201, 227, 277, 287, 296, 299–300, 304–310, *305,* 320, 321, 486, 496–497, 510, 511
Lerner, R. M. 8, 9, 11, 12, 17, 18, 19, 21, 22, 23, 24, 25, 26, 27, 28, 29, 30, 31, 35, 36, 38, 39, 44, 45, 46, 48, 49, 50, 51, 58, 62, 71, 72, 73, 74, 75, 76, 78, 80, 82, 83, 84, 85, 86, 87, 88, 90, 91, 94, 111, 112, 113, 114, 115, 117, 120, 121, 122, 123, 125, 126, 127, 132, 133, 134, 135, 136, 137, 138, 139, 149, 153, 170, 171, 172, 173, 176, 180, 181, 186, 187, 189, 190, 195, 196, 198, 199, 200, 201, 204, 205, 211, 216, 224, 227, 228, 235, 245, 252, 253, 254, 255, 256, 265, 266, 267, 268, 269, 270, 274, 275, 276, 277, 283–287, *285,* 288, 293, 294, 295, 296, 297, 298, 299, 304–311, 312, 314, 317, 318, 320, 321, 322, 325, 331, 338, 339, 342, 344, 345, 347, 349, 352, 357, 360, 361, 363, 365, 369, **369–370,** 370, 374, 381, 421, 434, 440, 448, 451, 452, 466, 467, 474, 481, 484, 486, 487, 488, 490, 492, 493, 494, 495, 496–497, 498, 501, 504, 507, 508, 510, 511, 512, 513, 514, 515, 516, 517, 518, 519
Lessac, M. S. 492, 507
Lester, B. M. 18, 33, 34, 38, 41, 170, 176, 181, 191, 195, 269, 275, 373, 449, 480
Letendre, C. H. 270
Leventhal, T. 1, 46, *47,* 84–85, 519

LeVine, R. A. 68, 364
Levins, R. 211–212, 280, 466–467
Levinson, D. J. 141, 263
Lewin-Bizan, S. 91, 308
Lewin, K. 16, 47, 64
Lewis, C. 84
Lewis, M. *69,* 69–70, 71, 75, 84, 108, 121, 201, 293, 295, 296, 312, 439, 440, 479
Lewis, M. D. 240
Lewontin, R. C. 18, 87, 89, 171, 172, 173, 211–212, 273, 280, 294, 373, 380, 381, 423, 424, 425, 426, 431, 432–433, 436, 440, 448, 466–467, 474
Li, K. Z. H. 22, 255, 320
Liben, L. S. 46, *46,* 84, 185, 186, 188, 288
Lickliter, R. 8, 31–32, *32,* 33, 35, 37, 38, 39, 41, 48, 57, 58, 83, 84, 87, 108, 112, 114, 115, 118, 121, 124, 132, 136, 139, 168, 172, 182, 183, 191, 195, 200, 223, 240, 266, 267, 269, 272, 273, 276, 277, 341, 349, 357, 417, 438, 450, 474, 477, 478, 479–480, 483
Lifton, R. J. 380, 381, 382, 386, 389
Lindenberger, U. 22, 24, 30, 73, 201, 235, **315,** *319*
Lips, H. M. 172
Lipsitt, L. P. 18, 19, 67, 77, 122, 200, 313, 506, 507
Litman, L. C. 472
Little, R. 298, *298,* 304, 311
Little, T. D. 29, 486, 488, 491, 493, *493,* 494, 501, 504, 510
Liu, J. 181
Locke, J. 55–56, 63
Loevinger, J. 141
Lonigan, C. L. 215
Looft, W. R. 2, 68, 72, 74, 123
Lopez, S. 308
Lord, S. 296
Lorenz, K. 15, 84, 101, 117–118, 172, 216, 217, 220, 221, 222–224, 225, 233, 250, 252, 341, 374, *375,* 375–400, 402, 454, 455, 458, 481–492, 484
Lovejoy, C. O. 280–281, *281*
Lowenthal, M. F. 141
Lubin, F. D. 119
Lumsden, C. J. 172, 452, 453
Lush, J. L. 421
Luthar, S. S. 84, 289
Luther, M. 53
Lykken, D. T. 412, 435
Lynn, R. 472

Mabry, P. l. 22, 43, 46, 505
Maccoby, E. E. 48
MacDonald, K. 219, 254–255, 344, 452, 453, 458, 469
MacIver, D. 296
MacWhinney, B. 215
Magnuson, K. 519
Magnusson, D. 20, 21, 83, 85, 86, 294, 312, *346,* 346–351, 354, 360, 368, 417, 421, 437, 439, 440, 466, 467
Mahoney, J. L. 85
Maier, N. R. F. 49, 201, 368, 450
Main, M. 225, 295
Malin, H. 510
Malloy, L. C. 71, 84
Manheim, R. 387
Manski, C. F. 402, 411, 412, 422–423
Mariano, J. M. 300–301
Marks, A. K. 84
Markus, H. 364
Marquis, D. G. 502
Marshall, P. J. 84, 287–288
Marsiske, M. 317, 318
Marsit, C. 18, 449
Martens, E. P. 506
Mascolo, M. F. 8, 11, 48, 84, 85, 105, 200, 204, 240, 312, 331, 355–356, 357, 358–359, *359,* 360–361, 363, 365, 366, 367, 380, 417, 450, 479, 489, 494
Mason, W. A. 220
Masten, A. S. 85, 289, 299, 519
Masters, R. D. 277, 280
Mayr, E. 31
McAdoo, H. 471
McCall, R. B. 44, 247, 506
McCandless, B. R. 19, 68, 121, 486
McCartney, K. 115, 122, 201
McClearn, G. E. 195, 254, 383, 437, 459
McClelland, M. M. 28, 38, 84, 317
McCluskey, K. A. 78
McConnell, E. 486
McCrae, R. R. 24, 26–27, 38, 41, 291, 374, 430, 487
McCubbin, H. I. 472
McCubbin, L. 133
McCullough, M. B. 84
McEwen, B. S. 438
McGraw, M. B. 219
McGue, M. 401, 412, 414, **415,** 416, 417, 420, 421, 424, 426, 429, 435
McGuffin, P. 383
McHale, S. M. 117, 517
McLaughlin, B. 220
McLoyd, V. C. 84, 87, 288
McNeill, D. 68, 114
Meaney, M. 18, 33, 34, 35, 36, 39, 41, 119, 170, 176, 177, 191, 195, 266, 269, 275, 373, 438, 449, 477, 480
Meehl, P. E. 510
Mendel, G. 31
Menon, J. L. 11, 300
Meyer, C. C. 220
Meyer, J. W. 262
Michel, G. F. 87, 221, 267, 270
Midgley, C. 134, 296
Miech, R. A. 496
Mill, J. 55
Mill, J. S. 55
Miller, N. E. 63, 121, 246, 515, 517, 519
Miller, P. 364
Millett, R. A. 513
Mischel, H. N. 146
Mischel, W. 79, 146, 256, *291,* 291–293, 359, 361
Misiak, H. 53, 55, 64
Misteli, T. 26, 34, 35–36, 41, 269, 373
Mistry, J. 18, 23, 24, 28, 38, 84, 189, 288, 363, 364, 365, 367, 474, 517
Mitchell, D. E. 221
Modell, J. 85
Molenaar, P. C. M. 8, 9, 18, 20, 21, 22, 27, 28, 38, 41, 42, *42,* 43, 44, 45, 46, 48, 62, 84, 85, 86, 122, 135, 136, 140, 152, 162, 163, 164, 165, 166, 169, 195, 198, 201, 213, 262, 266, 286, 292, 293, 312, 347, 349, 350, 355, 358, 360, 361, 364, 366–367, 369, 374, 419, 424, 448, 487, 489, 490, 491, 494, 496, 497, 499, 500, 501, 502, 505, 510
Moltz, H. 201, *218,* 218–219, 220, 221, 266, 267
Monod, J. 195
Moore, C. L. 87
Moore, D. S. 18, 33, 34, 35, *35,* 36, 37, 38, 39, 41, 87, 119, 170, 172, 173, 175, 176, 177, 182, 184, 191, 192, 195, 223, 266, 269, 272, 273, 275, 373, 403–404, 416, 421, 422, 424, 430, 434, 436, 438, 440, 443, 445, 446, 447, 449, 451, 468, 473–474, 477, 480
Moores, E. 105
Morris, P. A. 24, 29, 73, 75, 76, 82, 83, 85, 86, 132, 133, 180, 186, 270, 276, 293, 296, 297, 307, 312, 313, 336, 337–338, 339, 351, 363, 377, 439, 451, 479, 489, 498, 511
Morton, N. E. 429
Motti-Stefanidi, F. 298, 305
Movshon, J. A. 221
Mowrer, O. H. 63
Mueller, M. K. 510
Müller-Brettel, M. 78
Müller-Hill, B. 87, 273, 380, 382, 384, 386, 388, 391, 438
Müller, G. B. 37, 40, 373
Müller, U. 18, 23, 27, 46, *46,* 84, 85, 287, 289
Murchison, C. 68
Murray, C. 87, 188, 250, 252, 374, 401, 402, 414, 422, 423, 449
Murray, E. D. 9
Murray, H. A. 86, 140, 141
Murry, V. M. 85, 288–289, 519
Mussen, P. H. 19–20, *20,* 68, 71, 82, 508
Muthén, B. 501
Muthén, L. K. 501
Muuss, R. E. 52, 60, 140

Nagel, E. 96, 111
Nagin, D. S. 501
Napolitano, C. M. 510
Narayan, A. J. 85, 289, 519
Narvaez, D. 29
Nash, J. 219
Naumann, L. P. 24
Neeleman, D. 448
Neiderhiser, J. S. 273, 352, 437, 440–441
Neimark, E. D. 153
Nelson-Mmari, K. 18

Nelson, C. A. 119, 251–252, 254
Nelson, P. G. 270
Nesselroade, J. R. 8, 9, 13, 17, 18, 20, 21, 22, 24, 27, 28, 38, 42, 43, *43,* 44, 48, 62, 72, 73, 76, 78, 81, 84, 85, 86, 122, 135, 140, 152, 160, 162, 163, 164, 165, 166, 169, 195, 235, 250, 259, 260, 262, 286, 292, 293, 294, 313, 349, 350, 355, 358, 360, 366–367, 374, 419, 487, 488, 489, 490, 491, 492, 493, 494, 496, 497, 499, 501, 502, 505, 510
Neugarten, B. L. 66, 67, *67,* 78, 141, 263
Newell, K. 22, 28, 48, 361, 489, 500
Newman, H. H. 406, 411, 417, 418, 435
Newport, E. L. 105, 355, 356, 364
Nichols, I. 220
Nicolson, R. I. 105
Nigg, J. T. 105
Nisbett, R. A. 10, 381, 383
Noble, D. 8, 30, 31, 32, 34, 37, 38, 39, 85, 454, 474, 475, *475,* 479
Noll, E. 303
Norgate, S. 172, 420, 424
Novak, M. A. 220
Novikoff, A. B. 49, 91, 108, 203, *203,* 369, 450
Nucci, L. P. 11, 29, 105, 289–291, *290*

O'Malley, P. M. 496
Oden, M. H. 61, 62
Offer, D. 298
Olson, D. R. 153
Oppenheimer, L. 51
Orgel, L. E. 465
Oser, F. K. 83
Osgood, N. 22, 46, 505
Osofsky, J. D. 85, 289, 519
Ostrom, C. W. 513, 517
Overton, W. F. 2, 3, 7, 8, 9, 11, 12, 13, 14, 15, 17, 18, 19, 21, 22, 23, *24,* 24–25, 27, 28, 29, 30, 31, 34, 37, 39, 41, 42, 43, 44, 45, 46, 48, 49, 50, 51, 54, 65, 71, 72, 73, 74, 78, 80, 83, 84, 85, 86, 89, 90, 91, 92, 93, 94, 95, 99, 100, 105, 107–108, 110, 111, 112, 113, 114, 115, 116, 117, 121, 123, 124, 125, 128, 131, 134, 135, 136, 138, 139, 142, 143, 149, 153, 155, 170, 174, 175, 176, 177, 182, 188, 190, 192, 198, 199, 200, 201, 213, 216, 223, 228, 229, 235, 240, 262, 263, 266, 267, 268, 270, 275, 283, 286, 287, 288, 289, 299, 310, 312, 334, 341, 342, 347, 349, 352, 357, 360, 361, 364, 368, 369, **369–370,** 438, 440, 444, 450, 479, 487, 489, 490, 491, 492, 498, 505, 507, 508, 509, 511, 519
Ozer, E. M. 18

Palumbo, R. 42, 166
Panofsky, A. 37, 274, 440, 451
Park, B. 185
Parke, R. D. 85
Partridge, T. 198, 204, 267, 476
Paul, B. 20
Pearce, N. 510
Penfield, W. 220
Pepper, S. C. 3, 7, 10, 15, 74, 75, 80, 82, 92, 94, 95, 111, 112, 113, 122, 123, 124, 262, 352, 358
Perez, S. 84, 288, 363
Perlmutter, M. 78, 294
Pervin, L. A. 69, 123
Pestman, W. R. 506
Petersen, A. C. 120, 298, 305, 309, 311
Peterson, C. 292
Petrinovich, L. 79–80
Phelps, E. 46, 133, 308, 345, 498
Phelps, J. A. 180, 195
Piaget, J. 2, 6, 10, 19, 48, 57, 64, 66, *66,* 67, 68, 71, 72, 74, 83, 105, 109, 111, 112, 115, 121, 131, 141, 143, 147, 149, 151, 154, 155, 211, 213, 228, 229, 233, 245, 263, 289, 324, 365, 454, 461, 494, 508
Pick, A. 68
Pick, H. 68
Pigliucci, M. 37, 40, 373
Pinard, A. 145, 151
Plato 49, 51–52, 53, 54
Ploetz, A. 382, 392, 397
Plomin, R. 38, 41, 72, 84, 87, 88, 89, 99, 100, 117, 121, 139, 172, 175, 217, 273, 274, 341, 352, 383, 414, 417, 419, 421, 434, 435, 437, 438, 440–441, 443, 448, 449, 450–451, 452, 483
Popper, K. 13
Porter, T. 510
Preacher, K. J. 486
Pressey, S. L. 67, 78
Pressley, M. 502
Preyer, W. 19
Prigogine, I. 284, 342, 343
Proctor, R. N. 87, 172, 380, 381, 382, 386, 388, 392
Proctor, R. W. 75
Provine, W. 31
Purtell, K. M. 84, 288
Putnam, H. 50, 93, 112, 176

Raeff, C. 2, 8, 9, 10, 11, 15, 31, 38, 46, 64, 108, 110, 111, 114, 115, 116, 121, 122, 125, 153, 166, 167, 189, 204, 213, 228, 229, 235, 240, 243, 245, *245,* 277, 284, 305, 312, 314, 340, 342, 343, 351, 354, 364, 440, 450, 474, 479, 483
Ralston, P. 514, 515
Ram, N. 22, 42, 43, 48, 155, 160, 164, 489, 491, 500, 501, 502, 505
Rao, D. C. 429
Rapaport, D. 4
Rathunde, K. 83, 85
Ratner, A. M. 220
Raynor, R. 63
Reese, H. W. 3, 7, 8, 9, 13, 17, 18, 19, 20, 24, 48, 67, 71, *71,* 73, 76, 77, 78, 92, 93, 94, 95, 99, 100, 103, 104, 107–108, 111, 112, 113, 114, 115, 116, 117, 121, 122, 135, 153, 199, 200, 262, 313, 489, 506, 507
Reinert, G. 122
Reuman, D. 296
Ricco, R. B. 84, 288
Richards, R. 59, 101, 375, 376, 377, 379, 386–387, 389–390, 391, 440, 482
Richardson, K. 37, 39, 41, 61, 170, 172, 188, 192, 195, 403, 405, 406, 412, 417, 418, 419, 420, 422, 423, 424–425, *425,* 430, 436, 442, 443
Richie, D. M. 142
Rickman, A. N. 510
Riegel, K. F. 21, 24, 71, 72, 73–75, *74,* 79, 94, 127, 141, 180
Riley, M. W. 18, 141, 235, 294
Rimfeld, K. 38, 273, 352, 434, 452
Roberts, B. W. 24, 27, 220
Roeser, R. 84
Rogers, L. J. 478
Rogoff, B. 11, 28, 29, 189, 288, 312, 331, 352, 363–368, *364,* 380, 489, 517
Rose, H. 38, 40–41, 474, 477
Rose, S. 38, 40–41, 171, 172, 273, 348, 381, 474, 477
Rose, S. P. 355
Rose, T. 9, 18, 21, *21,* 22, 23, 27, 42, 62, 85, 86, 120, 122, 152, 162, 166, 169, 195, 216, 225, 238, 286, 292, 295, 349, 350, 355, 374, 487, 489, 490, 494, 496, 499, 503, 505
Rosenbaum, P. 506
Rosenberg, F. 81–82
Rosenberg, M. 81–82
Rosenberger, E. 220
Rosenblatt, J. S. 154, 198, 201, 213, 216, 217, 218, 267
Rosenblum, L. A. 69, 75, 121, 201, 293, 295, 296
Rosnow, R. L. 73
Roth, J. L. 298, 299, 510
Roth, T. L. 34, 119, 269
Rothbart, M. 185
Rothstein-Fisch, C. 363
Roubertoux, P. 414
Rouhani, P. 22
Rousseau, J.-J. 55
Rowe, D. C. 50, 72, 84, 87, 88, 99, 100, 117, 121, 170, 176, 250, 263, 274, 438, 439, 440, 448, 450
Rubin, D. B. 506
Rubin, R. O. 508
Runyan, W. M. 141
Rushton, J. P. 41, 84, 87, 88, 117, 121, 170, 172, 373, 374, 391, 400, 401, 414, 421, 422, 423, 436, 447–448, 449, 452, 458, 460, *469,* 469–473, 502
Russell, L. T. 85, 519
Ryff, C. D. 122, 250

Sahlins, M. D. 277
Salapatek, P. 68
Salzen, E. 220
Sameroff, A. J. 72, 74, 83, 96, *96,* 112, 115, 123, 136, 312, 357, 360, 368, 440
Sampson, E. E. 205

Sampson, R. J. 187, 193, 250, 274, 519
Sandhu, S. S. 184
Sanford, E. C. 66–67, 78
Santayana, G. 228, 375
Sapienza, C. 465
Sarason, S. 484, 519
Sarbin, T. R. 73, 79, 94
Saunders, P. R. 39, 273, 475, *475,* 477
Saunders, P. T. 18, 31, 38, 41
Scales, P. C. 83
Scarlett, W. G. 83
Scarr-Salapatek, S. 400, 401, 403
Scarr, S. 0, 115, 122, 201, 440, 449
Schaie, K. W. 20, 73, 76, *77,* 78, 80, 122, 141, 195, 250, 263, 294, 313, 492, 497, 499, 506
Schennach, S. M. 18
Schiefele, U. 84
Schlomer, G. L. 38, 50, 373, 471, 477
Schmid, K. L. 308
Schneirla, T. C. 10, 15, 28, 29, 36, 49, 50, 75, 91, 108, 115, 117, 119, 120, 122, 124, 125, 128, 139, 154, 165, 168, 169, 173, 174, 193, 196, *197,* 197–198, 201–206, *205, 206,* 207–208, 209, 211, 213–218, 221, 223, 225, 226, 227, 228, 229, 247, 252, 266, 267, 269, 271, 273, 277, 280, 283, 286, 287, 293, 294, 295, 297, 312, 318, 322, 325, 331, 339, 341, 347, 349, 352, 368, 369, 374, 377, 395, 440, 450, 461, 467, 474, 476
Schoon, I. 298, 305
Schorr, L. B. 513
Schrier, B. K. 119, 270
Schulenberg, J. E. 295, 496
Schulz, R. 22, 326
Schwartz, S. J. 46, 133, 180, 345, 496, 498
Scott, J. P. 154, 198, 217, 218, 219
Scriver, C. R. 184, 451
Sears, R. R. 61, 63, 64
Secord, P. F. 186
Segal, N. L. 412, 418
Seligman, M. E. 292
Selman, R. L. 141
Selzam, S. 440, 450
Sesma, A., Jr. 83
Sexton, V. S. 53, 55, 64
Shackelford, T. K. 280
Shah, J. Y. 319
Shanahan, M. J. 9, 18, 22, 23, 27, 83, 85, 86, 122, 250, 328, 330, 331, 333, 334, 336, 338, 489, 498
Shaw, G. M. 181
Sheldon, W. H. 117, 197
Shenk, D. 38, 192, 403–404, 416, 421, 422, 424, 430, 434, 436, 438, 440, 443, 445, 446, 447
Sherrington, C. S. 205, 209–210
Sherrod, L. R. 189, 294
Shields, J. 406, 411, 417, 418, 435, 448
Shirley, M. M. 142
Shoda, Y. 291
Shuey, E. 1, 85, 519
Shulman, E. 519
Shweder, R. A. 48, 83, 86, 87, 122, 364
Siegel, A. W. 104, 125
Siegler, I. C. 24, 141, 506
Sigman, M. 252
Silverberg, J. 452
Silverman, W. K. 85, 289, 519
Simkins, S. 84
Simmons, R. G. 81–82, 493, 495
Simon, L. A. K. 19, 61, 515, 517, 519
Skinner, B. F. 3, 11, 55, 57, 63, 72, 99, 100, *100,* 101, 102, 107, 139, 141, 197, 199, 246–247, *247,* 248, 480–481, 482, 483, 502
Skinner, E. A. 75, 80, 127, 498
Slavich, G. M. 33, 34, 36, 41, 119, 172, 173, 177, 182, 191, 195, 269, 270, 272, 275, 373, 440, 449
Small, P. J. 84, 289
Smetana, J. G. 84, 289, 291
Smit, D. J. A. 419
Smith, C. 496
Smith, C. L. 472
Smith, E. P. 298, 305, 311
Smith, L. B. 48, 83, 85, 112, 149, 200, 276, 283, 312, 339–346, *340,* 347, 348, 349, 354, 357, 360, 365, 368, 417, 421, 438, 439, 440, 466, 467
Smyer, M. A. 511
Snyder, E. *178,* 179
Snyder, M. 256
Socrates 52
Sokol, B. W. 25, 26, 43, 84, 108, 491
Solé, R. 476
Solomon, R. L. 492, 507
Sorell, G. T. 75, 80, 127, 498
Sorensen, B. 294
Spanier, G. B. 115, 200, 293, 295
Sparks, E. S. 133
Spelke, E. S. 105, 355, 356, 364
Spencer, H. 56–57
Spencer, M. B. 83, 84, 86, 87, 187, 188, 302–304, *303,* 309, 310, 509
Spencer, T. R. 309
Spiker, C. G. 68
Sroufe, L. A. 254
St. George, R. C. C. 171
Stafford-Brizard, K. B. 490, 511
Staiger, D. 506
Stattin, H. 83, 85, 86, 294, 312, 346, 347, 348, 349, 350, 351, 354, 360, 368, 440
Staudinger, U. M. 22, 24, 30, 73, 201, 235, **315,** 318, *319,* 365
Stein, G. J. 59, 381
Steinberg, L. 38, 48, 50, 280, 373, 471, 477, 519
Stephenson, W. 411
Stern, C. 195
Stern, W. 61, 91
Stettner, L. J. 220, 221
Stevens, S. S. 430
Stevenson, H. W. 19
Stewart, D.L. 221
Stingle, K. G. 55, 99, 100, 370
Stock, J. H. 506
Stoltzfus, J. 303
Strauss, S. 350
Streicher, J. 385, *386*
Strohman, R. C. 270, 273, 372, 437
Strother, C. R. 78, 122, 492, 506
Subramaniam, A. 305
Sunday, S. R. 461
Suomi, S. J. 48, 220
Super, C. M. 201
Suzuki, L. K. 363
Swanson, D. P. 84, 302, 303, 509
Swartz, D. 280
Sweatt, J. D. 119
Sweetman, L. 43

Tabery, J. 172, 173, 175, 443, *443*, 451, 462, 473
Tajfel, H. 185
Tamis-LeMonda, C. S. 296
Tanner, J. 470, 472
Taylor, H. F. 417, 434–435
Tellegen, A. 412
Terman, L. M. 19, 58, *61,* 61–62, 252
Teti, D. M. 492
Thales of Miletus 51
Thelen, E. 48, 83, 85, 108, 112, 149, 200, 276, 283, 312, 339–346, *340,* 347, 348, 349, 354, 357, 360, 365, 368, 417, 421, 438, 439, 440, 466, 467
Theokas, C. 85, 137, 180, 277, 308
Thomas Aquinas, Saint 53, 127
Thomas, A. 79, 181, 201, 225, *226,* 226–227, 234, 277, 287, 293, 294, 295, 296
Thompson, R. A. 70
Thorndike, E. L. 61, 103, 121
Thorpe, W. 219
Thurnher, M. 141
Tilley, E. H. 38, 50, 373, 471, 477
Timney, B. 221
Tinbergen, N. 375
Tobach, E. 8, 28, 29, 50, 59, 87, 108, 114, 121, 122, 124, 125, *126,* 126–127, 175, 197, 198, 201, 203, 205, 213, 216, 217, 266, 267, 273, 277, 280, 283, 286, 287, 297, 312, 341, 349, 396, 440, 461, 463, 467, 474
Tobin, S. S. 66
Todd, P. E. 18, 505
Tolan, P. H. 18, 22, 31, 44, 489, 505, 507, 508, *508,* 509, 510
Topoff, H. R. 87, 216, 396
Toulmin, S. 133
Trickett, E. 137
Trivers, R. 172
Trochim, W. 506, 517–518
Trommsdorff, G. 28
Turiel, E. 11, 84, 105, 141, 146, 148, 150, 289–291, *290*
Turkewitz, G. 217
Turkheimer, E. 424, 425, 440, 442
Turner, J. C. 185
Twain, M. 444

Tyler, A. N. 221
Tyler, L. E. 58

Ulrich, B. D. 108
Uphouse, L. L. 119, 173, 270
Urban, J. B. 11, 22, 44, 46, 308, 486, 505, 517–518

Valentino, K. 84
Valsiner, J. 64, 65, 83, 85, 86, 93, 365
Van de Vijver, G. 108
van den Berghe, P. L. 456, 457
van den Daele, L. D. 141, 148, 150
van der Dennen, J. M. G. 394
van Geert, P. 240, 479
Van Sluyters, R. C. 220
Van Speybroek, L. 108
Vandell, D. L. 85, 306
Varga, S. M. 508
Velicer, W. F. 42, 166, 169
Venter, J. C. 87, 170, 438
Verma, S. 298, 305
Viechtbauer, W. 24
von Bertalanffy, L. 1, 9, 12, 24, 49, 91, 92, 96, 105, 108, 110, *110,* 125, 263, 275, 348, 463, 486
von Dechend, H. 271
von Eye, A. 22, 38, 42, 44, 172, 173, 445, 448, 452, 489, 491, 493, 494, *494,* 505, 510
von Frisch, K. 375
von Noorden, G. K. 221
Vondracek, F. W. 295
Votruba-Drzal, E. 519
Vrba, E. 57, 464–466, 467
Vygotsky, L. 64–65, *65,* 67, 363, 365

Waddington, C. H. 272
Wade, N. 274, 373, 374, 434, 474
Wadhwa, P. D. 214
Wahler, R. G. 449, *449*
Wahlsten, D. 83, 112, 170, 172, 191, 267, 270, 273, 423, 440, 452
Wake, W. 141
Wald, E. 171, 201
Walk, R. D. 211
Walker, K. 510
Walls, T. 8, 111, 126, 127, 180, 200, 252, 254, 276, 294, 318, 322, 363, 466, 467
Walton, K. E. 24
Wandersman, A. 513, 517
Wang, G. 180, 190
Wang, J. 305, 309, 497
Wang, L. 502, 513
Wang, X. 180
Wanless, S. B. 28, 38, 84
Wapner, S. 125, 229, 285, 312, 351–355, *352, 353,* 360, 368
Washburn, S, L. 277, 280
Waters, E. 254
Watson, J. B. 63, 91, 92, 103, *103,* 121, 197, 246
Watson, J. D. 464
Watts, T. W. 85
Way, N. 18, 44
Weinart, E. 294
Weinberg, R. A. 91
Weisner, T. M. 18, 44
Weiss, C. H. 204, 513
Weiss, E. 198, 204
Weiss, H. B. 517
Wentura, D. 318
Werner, H. 2, 9, 10–11, 15, 35, 46, 64, 65, 68, 108, 109, 113, 115, 121, 125, 154, 155, 166, 173, 204, 228, 229, 235, *239,* 239–240, 241–242, 243, 245, 253, 254, 265, 284, 286, 289, 297, 340, 343, 351, 495
Wertheimer, M. 64
Wertsch, J. V. 365
West-Eberhard, M. J. 32, 37, 40, 373
Wheeler, S. 18
White, K. 302
White, S. H. 57, 58, 63, 82, 507
Whitehead, A. N. 511
Wiatrowski, M. 253
Wickrama, K. A. S. 155
Wiesel, T. N. 220
Wigdor, A. K. 422
Wigfield, A. 84, 296
Willems, E. P. 134, 181
Williams, G. C. 464
Willis, S. L. 254
Wilson, E. O. 11, 97, 172, 312, 399, 440, 452–453, *453,* 454, 455, 458–459, 460, 461–462
Winston, A. S. 470
Witherington, D. C. 8, 29, 31, *32,* 35, 36, 37, 38, 42, 48, 58, 84, 108, 114, 121, 128, 136, 182, 240, 267, 269, 276, 277, 341, 343, 344, 349, 357, 417, 440, 467, 479–480, 483, 490, 519
Witherspoon, D. 85, 288–289
Witmer, L. 91
Woese, C. R. 32, 37, 299
Wohlwill, J. F. 117, *142,* 142–143, 144, 150, 151–152, *152,* 256, 265, 496, 500, 501
Wolff, P. H. 112
Wolters, C. 20
Woodward, S. *178*
Wrosch, C. 22, 326, 327
Wu, J. 24

Yarczower, B. S. 204, 205
Yarczower, M. 204
Yates, F. E. 285
Yeager, D. 300
Yong, E. 105
Yoshikawa, H. 18, 44, 507

Zaff, J. F. 11, 44, 308, 486, 508
Ziegler, H. 389, 390
Zigler, E. 91
Zuckerman, B. 18, 180, 181, 189
Zukav, G. 102–103, 121, 284

Subject Index

Page numbers in italic type refer to figures and photographs; those in bold type refer to tables.

A/S ratio (association cortex/sensory cortex ratio) 206–208, 210, 211, 322
abruptness 144, *145,* 145–150, 495
accelerated designs, in longitudinal studies 492, *492*
accommodation, in Piaget's theory 324
acetylation 33, 34
action:
  definition 324
  sources of in human development 8–11
action-phase model of developmental regulation 327–328
action theories of human development 313, 320, 321–322
  Brandtstädter's contributions 323–326
  contextual and developmental constraints 324–325
  regulation and plasticity 322–323
adaptation 462–463
"adaptationist program" 466, 467
adaptive characteristics 57, 58
adaptive developmental regulations 29–30
adolescence 60, 82, 120, 496
  deficit model of 298
  and Hall 58
  storm and stress in 60, 262, 298
  see also Positive Youth Development; puberty
ADS (applied developmental science) 510–514
  civil society and social justice 518–520
  emergence and definition of 514–517
adult development 66–67
AEC (active and engaged citizenship) 308
African Americans:
  and intelligence testing 400–403
  misuse of heritability concept in discussion of race differences 421–424
  PVEST ("phenomenological variant of ecological systems theory") model 302–304
  racism, and the nature-nurture controversy 274
  and social stereotypes 188
  see also race
agency, human 28–30
agent-in-habitat system 354
aggregation 501–504, *503*
aggression:
  child development 235–236, *236*
  Lorenz 393–395
  sociobiology 454–455
aging 58, 66–67, 132, 235
anagenesis 204
anal stage of psychosexual development 140, 141
Anastasi, A. 15, 50, 170, 173–177, *174,* 183, 184, 188, 189–190, 192, 193, 198, 223, 228, 286, 294, 351, 356, 421, 426, 450
antecedent-consequent relations, *versus* structure-function relations 114–115, 121
anti-Semitism, Nazi era 382, 385, *386,* 395
applied developmental science *see* ADS
Aristotle 36, 52–53, 54, 56
arrhythmic infants 225–227, 294
assimilation, in Piaget's theory 66, 324
association cortex *see* A/S ratio (association cortex/sensory cortex ratio)
atomism 95
attribute interrelation in ipsative analysis 164–165, 166, 167, 168
attribute repertoire in ipsative analysis 163–165, 166–167, 168, 169
autopoiesis 8, 42, 285
Baer, D. M. 3, 19, 55, 72, 82, 83, 99, 100, 101, 102, *102,* 103, 104, 114, 116, 118, 121, 127, 139, 153, 163, 199, 200, 263, 370
Baltes, M. M. 22, 122, 254, 317, *317,* 321, 323, 327, 339, 364
Baltes, P. B. 8, 13, 17, 18, 19, 20, 22, 24, 30, 42, 49, 66, 72, 73, 75, 76, 77, 81, 83, 85, 122, 132, 137, 141, 160, 201, 235, 250, 251, 254, 255, 256, 259, 260, 264, 265, 286, 287, 294, 295, 297, 311, *313,* 313–314, **315,** 316–321, **319,** *320,* 323, 326, 327, 330, 331, 339, 344, 364, 368, 377, 489, 491, 493, 494, 497, 499, 510
Bandura, A. 28, 80, *80,* 146, 298
Bateson, P. 18, 31, 32, 36, *36,* 37, 38, 39, 41, 84, 219, 222, 254, 266, 273, 373, 424, 467, 478, 479
behavior genetics 436–438
  as "emperor's new clothes" 440–452
  flaws in gene function model 438–440
behavioral attributes 156–158, *157,* 160, 162
behavioral change, *versus* structural change 115–116
behavioral neophenotypes 467–468
behavioral stereotype *versus* behavioral plasticity 204–206, *205*
behaviorism 11, 103, 121
  and learning theory 63–64
bidirectional relations 2, 22, 25–26, 28, 29, 33, 34, 38, 39, 43, 45, 216, 227, 342, 348, 350, 505
  knowledge generation and knowledge application 516
  organism-context relations in evolution 73
  parent-child 294, 296, 297
  structure-function 124–125
  young people 301, 303, 509
"Big Five" personality traits 24, 26, 27, 38, 291

Bijou, S. W. 3, 19, 55, 72, 82, 83, 99, 100, 101, 102, *102,* 104, 114, 116, 117, 118, 121, 139, 153, 163, 199, 200, 262, 263, 370
binocular vision, development of 220–221
bioecological model of human development 73, 75–76, 313, 335–339
Birch, H. 181, 201, 205, 209, *210,* 210–211, 225, 226
birth cohorts 80–81, 499
Block, J. 27, 85, 159, 163, 164, 165, 169, *253,* 254, 294, 298
Blumberg, M. S. 36, 37, 222, 241, 477, 481, *482*
Bornstein, M. H. 9, 23, 29, 42, 43, *43,* 46, 48, 79, 84, 85, 86, 120, 137, 138, 181, 201, 272, 288, 290, 295–296, 305, 361, 487, 488, 489–490, 491, 496
Brandtstädter, J. 8, 18, 22, 24, 28, 29, 51, 83, 85, 111, 127, 201, 208, 211, 227, 255, 256, 310, 313, 318, 321, 322–323, *323,* 323–326, 327, 331, 332, 339, 354, 356, 363, 364, 467, 510
breast cancer genes 171
Brim, O. G., Jr. 8, 18, 72, 76, *77,* 78, 80, 122–123, 249–250, 252, 253, 254, 275, 294
broad heritability 443
  see also heritability
Bronfenbrenner, U. 21, 24, 29, 45, 67–68, 73, 74, 75–76, 79, 80, 82, 83, 85, 86, 132, 133, 137, 138, 180, 182, 186, 270, 276, 277, 293, 294, 296, 297, 302, 307, 312, 313, *335,* 335–339, 351, 363, 377, 439, 440, 451, 479, 489, 498, 511, 512
  bioecological model of human development 73, 75–76, 313, 335–339
Buck, C. 372–373, 375
Burt, C. 15, 405–411, *406,* 414, 417, 428, 440, 447

canalization 271–272
CAPS (cognitive-affective-personality) system 292–293
categorization, and social stereotypes 186–188
Catholic Church 53
Cattell, R. B. *13,* 13–14, *14,* 20, 159, 499
causality:
  in holistic relations 347–348, 350
  and sequential change 488–489
Cernovsky, Z. Z. 472, 473, *473*
Charney, E. 170, 191, 418, 438, 440, 441–442, *442,* 445, 451
Chess, S. 79, 127, 181, 201, 225, *226,* 226–227, 277, 287, 295, 296
child development 19, 20
  aggression 235–236, *236*
  interactions 200
  intersensory *210,* 210–211
  learning 236–239, *237, 238, 239*
  myth of the first three years 251
  see also children; nature-nurture controversy
child effects 294
child psychology 19, 58, 200
child↔parent relations:
  and developmental contextualism 293–297
  reciprocity in 349
children:
  cultural contexts 288
  peer interaction 366
  stepping response 344, 345
  temperament of 225–227, 294
  and trauma 289
  see also child development; child psychology; child↔parent relations
Christianity 53, 54
circular functions in development 168, 225–227
  parent-child relations 293, 294, 295, 297
civil society, and ADS (applied developmental science) 518–520
classical approach *see* stage theory approach to development
classical conditioning 3, 63, 72, 100, 104, 114
  see also conditioning
coactions, horizontal and vertical 269
cognitive development:
  and levels of integration 368–369
  stage-related properties of 143
  see also dynamic coactive systems theory (Fischer)
cognitive revolution 105
Colby, A. 507, 509, *509,* 510
Collins, L. M. 486, 488, 491, *491,* 492, 493, 494, 499, 501, 504, 510
compensation *see* SOC (selective optimization with compensation) model
"compromise" conception, probabilistic epigenesis as 128–132, *129, 130, 131*
"concurrence" 145, 151–152, **152**
conditioning 3, 100–101, 104, 114, 116, 393, 394, 502
constitutive rules 325
contextual organicism 125
contextualism/contextual models 75–80, 94, 123–135, *129, 130, 131*
ontogenetic and historical, as a paradigm 316–317
  and probabilistic epigenesis 124–125
  see also developmental contextualism; individual↔context relations
continuity-discontinuity issue 15, 97–98, 116, 229–230, 249–256, 264–265, 276
  defining the issue *230,* 230–234, *231, 232, 233, 234*
differential approach to development 160, *161*
  metatheoretical and theoretical proscriptions and prescriptions 262–264
  ontogenetic implications 249–256
  and the orthogenetic principle 243, 245
  and phylogeny of learning 245–249, *247, 248, 249*
  quantitative and qualitative change 239–242, *240*
  and the stability-instability issue 259–261, *260, 261*
  as a theoretical issue 234–239, *236, 237, 238, 239*
  Werner's scholarship 229, 239–240, 241–242
continuum of breadth *188,* 188–191
continuum of indirectness *183,* 183–188
cortical potentials, evoked 412, 414
coupling-uncoupling phenomena 212
critical-periods hypothesis 198, 217–221
cross-sectional designs 20, 486–487
crystallized intelligence 412
culture:
  cultural contexts 288
  sociohistoric-cultural theory (Rogoff) 363–368
cytosine 33, 34

Damon, W. 11, 27, 29, 48, 68, 71, 83, 84, 86, 108, 110, 136, 299, *300,* 300–301, 302, 310, 347, 365, 507, 508, 509, 510
Darwin, C. 28, 31, 52, 53, 56–58, *57,* 58, 59, 61, 62, 246, 464, 476
data analysis issues in developmental science 499–501, **500,** *500,* 510–511
  aggregation 501–504, *503*
  decline of RCT "gold standard" 506–507
  qualitative and mixed-methods research 504–505, 507–510
  quantitative innovations 505–506
  see also methodology
Dawkins, R. 100, 117, 374, 399, 453–454, *454,* 455, 457, 458
décalage 151
deductive reasoning 5, 13, 14, 288
Descartes, R. 15, 19, 49, 50, 54–55
descriptive continuity/discontinuity *230,* 230–231
determinism, genetic *see* genetic determinism
development 1–16
  and biology 10
  changes characteristic of 261–262
  circular functions and self-stimulation in 225–227
  concepts representing 213–217

definitions of 9–11, 213–214
differential approach 139–140, 155–162, *156, 157, 158,* **159,** *161,* 169
  idiographic (ipsative) approach 139–140, 162–165, *165,* 169
  as a living system 283–286, *285*
  metatheoretical models of 92–138
  philosophical models 71–73, 92–135
  philosophical roots 51–56
  and RDS (relational developmental systems) metatheory 7–8, 17, 21–28, *25,* 44–45, 85–86
  Schneirla's model of 213, 214–218
  scientific roots 56–91, **88, 89**
  sources of 117–121
  sources of action in 8–11
  stage theory approach 139–155, *145,* **152,** 169
  stages of 117
  theory and philosophy 11–14, *12, 14*
  "third source" of 225–227
developmental approach *see* stage theory approach to development
developmental complexity 479–482
developmental contextualism 125, 266–267, 293–297
developmental deadlines 327
developmental psychology 58
developmental regulation 28–30
  action-phase model 327–328
  action theories 321–323
  SOC model 317–318
developmental science:
  applied (ADS) 510–520
  concepts and theories overview 17–46
  contemporary foci 17–18
  data analysis issues 499–511, **500,** *500, 503*
  developmental methodology and collaboration 275–276
  and diversity 86–87
  diversity in 86–87
  econometric methods 44
  and epigenetics 33–41
  and evolutionary biology 31–33
  future of 46, 520
  idiographic and systems science methodology 42–44
  implications of philosophical models of development for 135–138
  metatheoretical models 92–139
  methodological issues 486–520
  past of 19–21
  philosophical and scientific roots 48–91
  see also RDS (relational developmental systems) theories and concepts
developmental stage theory *see* stage theory approach to development
developmental systems:
  Ford & Lerner's design criteria 283–287, *285*
  Gottlieb's theory of 267–273, 283
dialectical model of development 73–75, 94
differential approach to development 155–158, *156, 157,* 162, 169
  continuity-discontinuity 160, *161*
  individual differences in 158, *158*
  relation of concepts of development 162
  stability-instability 160–162
  study of development in 158–160, **159**
discontinuity:
  continuity-discontinuity issue 15, 97–98, 116
disease, inherent susceptibility to 195
diversity:
  in developmental science 86–87, 191
  DNA (deoxyribonucleic acid) 33–34, 36, 39, 119, 132, 464
  see also epigenetics
Down syndrome 177–179, *178,* 184, 190
dramaturgical model 79
Dutta, R. 23, 24, 28, 38, 84, 189, 288, 363, 364, 365, 367, *367,* 474, 517
dynamic coactive systems theory (Fischer) 355–356
  as alternative to neo-nativism 356–360
  conclusions about 363
dynamic skill concept 360–363
dynamic factor analysis 43, 166, 497, 505
dynamic interaction 347
dynamic skill 360–363
dynamic systems theory 339–340, 360–361
  conclusions about 345–346
  developmental system dynamics *342,* 342–343
  novel forms development 340–341
  stability and change in 343–344
  system transitions 344–345
  time scales 345

EBPs (evidence-based practices) 517
ecological contexts 70, 481
ecological validity 44, 79–80, 138, 174, 516, 518
econometric methods 18, 31, 44, 505–506
EDP (evolutionary developmental psychology) 31, 32, 38, 87, 374, 474–479
  conclusions about 482–483
  and developmental complexity 479–482
EE (evolutionary evaluation) 517–518
ego 3, 4, 159
ego resiliency 254
Elder, G. H., Jr. 9, 18, 20, 21, 22, 23, 24, 26, 27, 29, 30, 33, 38, 45, 48, 80, 81, 83, 85, 86, 122, 132, 133, 141, 250, 263, 270, 276, 277, 287, 294, 297, 312, 313, 316, 328–335, *329,* 333, 338, 377, 487, 489, 498, 499, 507
elementarism, *versus* holism 113–114
embodiment 25, 30, 31, 39, 40, 41, 288
emergence 107
Emmerich, W. 87, *139,* 139–140, 148, 150, 151, 153, 154, 155, 158, 161, 163, 166, 259
empowerment evaluations 513
endogeneity 44, 505–506
entelechy 52
environment:
  levels of 180–182
  organic effects of 189
  stimulative effects of 189–191, *190*
  see also heredity-environment relations
environmental stimulation 119–120
EP (evolutionary psychology) 40–41
EPBM (evolved probabilistic behavioral mechanism) 482
EPCMs (evolved probabilistic cognitive mechanisms) 479, 480, 481, 483
epigenesis 35–36, 108–109, 118, 476
  environmental stimulation 119–120
  Gottlieb's theory 268–276
  probabilistic 38, 39
epigenetics 17, 18, 41, 52, 108
  behavior genetics 449
  genetic reductionism 37–38, 39
  overview 33–37
  RDS (relational developmental systems) metatheory 38–41
epistemology 17
equilibration model (Piaget) 148–149, 154
ergodic theorems 20–21, 42
Erikson, E. H. 3, 4, 19, 57, 64, 67, 72, 111, 118, 121, 141, 143, 144, 154, 159, *159,* 162, 199, 217, 233, 262, 263, 458, 496, 508
essentialism 11, 95
ethical degeneration 396–399
ethology 375
eugenics 38, 50, 87, 170, 484
  and Lorenz 380, 383, 392
European Americans:
  and intelligence testing 400–403
  see also race
evaluation, of programs, policies and interventions 513–514
EE (evolutionary evaluation) 517–518
evoked cortical potentials 412, 414
evolutionary biology 18, 31–33, 41
exaptation 464–466
exosystem 76, 335
experience, role in development 214, 218, 228
  maturation↔experience relations 214–215, 216
  modes of experiential contribution 271–273
experimental child psychology approach 200
explanatory continuity/discontinuity 231–323, *232, 233,* 234, *234,* 263, 264

  see also continuity-discontinuity issue
Eysenck, H. J. 250, 405, 409, 412–414, *413,* 421, 424, 428, 429, 435

“families” of theories 94, 121
FAP (fixed action pattern) 481, 482
FAS (fetal alcohol syndrome) 134
father-effects 477
faulty logic in developmental research 224
Feldman, D. H. 105, 141, 142, 148–150, *149,* 151, 153, 154, 312, 365, 368
Feldman, M. 37, 87, 170, 274, 294, 424, 425, 426, *433,* 436, 440, 448, 474
females:
  sex differences in gametic potential 455–456
  social and sexual development and gametic potential 456–458
FFT (Five Factor Theory) 24, 26, 27, 38, 291, 292, 374
fidelity, and spirituality 302
first three years, myth of 251
Fischer, K. W. 8, 11, 22, 48, 83, 84, 85, 105, 136, 141, 200, 204, 240, 312, 331, *355,* 355–363, 365, 366, 367, 368, 417, 450, 479, 494
Fisher, C. B. 12, 17, 18, 23, 41, 45, 90, 91, 134, 138, 270, 273, 311, 514–515, *515,* 515–516, 519–520
Flavell, J. H. 48, 66, 68, 117, 142, 143–*143,* 144–145, 147–148, 150, 151, 164, 228, 508
Floyd, D. 298, *299*
follow-up/follow-back research designs 488
Ford, D. H. 9, 10, 23, 24, 82, 83, 94, 112, 121, 149, 153, 176, 200, 201, 267, 270, 276, 283–287, *284, 285,* 288, 289, 293, 296, 297, 311, 312, 322, 338, 342, 357, 360, 421, 440, 466, 467, 492
Ford & Lerner’s design criteria for developmental systems theories 283–287, *285*
formal operational stage 72, 74, 111, 116
formative evaluations 513
foundationalism 95
Freud, A. 4, 64, 67, 81, 83, 241, 298, 495
Freud, S. 3, 4, 52, 57, 64, 67, 72, 111, 121, 131, *140,* 140, 143, 153, 154, 155, 199, 458, 508
Freund, A. M. 22, 29, 255, 256, 317, *318,* 320, 327, 364, 507
functional characteristics 57
functionalism, probabilistic 79–80
fusion 198, 213, 218

gametic potential:
  sex differences in 455–456
  social and sexual development 456–458
Gandhi, M. 30
general-and-specific-laws compromise 130–132, *131*
generalizability, limits of 309–310, 501
generativity, sense of 233, 262
genes:
  gene expression 34–37, 39, 41
  gene function, flaws in behavior genetics model of 438–440
  role in human development 170–172
  see also epigenetics; nature-nurture controversy
genetic determinism 35, 253, 478
  and sociobiology 453–454, 458
genetic-difference hypothesis of intelligence 401–403
genetic reductionism 15, 28, 31, 87–90, **88, 89,** 372–373, 433–435, 436
  behavioral genetics 436–452
  Burt’s and Jensen’s scholarship 400–412, **407, 408**
  conceptual and empirical failures 373–375
  conclusions about 483–485
  EDP (evolutionary developmental psychology) 474–483
  and epigenetics 37–38, 39
  Lorenz’s scholarship 374, 375–400
  models and mistakes in heritability studies 424–433, **427, 428**
  other 20th century hereditarian studies 412–414, **415,** 416–424
  RDS (relational developmental systems) metatheory as frame for discussing 369–371
  sociobiology 452–474
genital stage of psychosexual development 140
genotypes:
  genotype-environment correlation *446,* 446–447
  genotype invariance 179
  and the norm-of-reaction concept 192–196
Gesell, A. L. 19, 58, 61, *62,* 62–63, 262
Gestalt psychology 64, 109
GMM (growth mixture modeling) 159–160
Goldberger, A. S. 402, 410, 411, 412, 416, *423,* 424, **427,** 427–428, **428,** 429, 436, 440
“good enough” approximations 487
goodness-of-fit model 134, 277, 287, 296
Gottlieb, G. 18, 23, 26, 35, 38, 39, 50, 56, 57, 61, 83, 87, 91, 108, 112, 114, 115, 118, 119, 121, 122, 124–125, 132, 139, 144, 170, 172, 173, 181, 182, 183, 191, 198, 200, 201, 213, 216, 217, 223, 227, 266, *267,* 267–273, 274, 275, 276, 277, 283, 286, 287, 293, 312, 340, 341, 342, *342,* 347, 348, 349, 352, 354, 355, 358, 359, 360, 372, 417, 421, 424, 438, 439, 440, 448, 449, 466, 467, 468, 474, 476, 489
Gould, S. J. 18, 57, 60, 86, 87, 171, 172, 173, 188, 204, 211, 250, 252, 277–279, *278,* 280, 282–283, 286, 322, 373, 401, 405–406, 409–410, 411, 412, 413–414, 424, 425–426, 440, 452, 460, 462–463, 464–466, 468, 476, 477
Greenberg, G. 8, 87, 105, 110, 125, 173, 175, 198, 203, 204, 216, 217, 266, 267, 268, 273, 283, 297, 349, 440, 452, 476, *476,* 477–478
group differential approach 15

Haeckel, E. 53, 58–60, *59,* 61, 262, 278, 381, 382, 387, 389, 390, 392, 397
Hall, G. S. 10, 19, 53, 57, 58, *58,* 59, 60–61, 67, 78, 235, 262, 278, 298
Handbook of Child Psychology 19, 46, 86, 508
Handbook of Child Psychology and Developmental Science 46, 84–85, 201, 287, 289, 311
Havighurst, R. J. 66, 67, *67,* 78
Hebb, D. O. 174, 188, 193, 194, 206–208, *207,* 211, 219, 228, 286, 325, 377, 444, 445
Heckhausen, J. 22, 211, 255, 256, 313, 322, *326,* 326–328, 332, 339, 364
Heckman, J. 18, 44, 187, 505, 506, *506*
heredity-environment relations 177, 228
  Anastasi’s scholarship on 173–177
  continuum of indirectness *183,* 183–188
  indirect nature effects 177–180, *178*
  levels of environment 180–182
  norm-of-reaction concept 192–196
  nurture effects *188,* 188–191
  see also nature-nurture controversy
heritability:
  behavioral genetics 436–452
  Burt 405–411
  calculation issues 424–433, **427, 428,** 441–442, 442–447, *446*
  genotype-environment correlation *446,* 446–447
  and inherited 447–449
  and intelligence 401–434, **407, 408, 415, 427, 428**
  IQ amplitude and latency of evoked potentials 412–414
  misuse of in discussion of race differences 421–424
  molecular genetics research 418–420
  post-Burt reviews of family studies 414, **415,** 416–418
  sociobiology 452–474
heterochrony 278–279
Hirsch, J. 87, 163, 170, 171, 173, 181, 182, 191, 192, 193, 194, 195, 196, 294, 402, 409, 411, 422, 423, 424, 425, 426, 431–432, *432,* 436, 440, 445, 447, 448, 452, 461, 462
historical contextualism 316–317

- historical events, and life-course theory 333, *333*
- Hitler, A. 375, 380, 382, 387, 388–389, 390, 395
- Ho, M. W. 18, 31, 39, 41, 270, 273, 412, 418, 419, *419,* 424, 425, 437, 440, 467, 475, 477
- holism, *versus* elementarism 113–114
- holistic, developmental, systems-oriented approach (Wapner) 351–355, *353*
- holistic interactionism 346–348
- holistic person↔context interaction theory (Magnusson) 346–347
  - causality in 347–348
  - conclusions about 351
  - features of person↔environment system 348–351
- homology 460–461
- homunculus 54, 480
- horizontal coactions 269
- horizontal décalage 151
- Horowitz, F. D. 48, 87, 89, 421, 434, 436–437, *437,* 439, 440, 448, 452, 474, 484–485, 541
- Hubbard, R. 171, *171,* 172, 201
- human aggression *see* aggression
- human evolution:
  - and probabilistic epigenesis 277–278
  - *see also* Darwin, C.
- hylomorphic doctrine 52
- hypotheses 5, 13–14

- IARYD (Institute for Applied Research in Youth Development) 307
- id 3, 52
- identity of opposites 26–27, 28, 487
- idiographic (ipsative) approach to development 162–165, *165,* 168–169
  - developmental changes within 166–167
  - individual differences in 165–166
  - relation of concepts of development to 167–168
- Idiographic Filter 166, 366, 367, 497, 505
- idiographic methodology 42–43, 86
- idiographic processes 152
- imprinting 375–376
- inclusive fitness 100, 454, 455, 456, 462
- independent, additive-action assumption 175
- indirectness:
  - continuum of *183,* 183–188
  - of nature effects 177–180, *178*
- individual differences:
  - differential approach to development 158, *158*
  - idiographic (ipsative) approach to development 165–166
  - stage theory approach to development 153
- individual-psychological level of environment 181
- individual↔context relations 18, 21, 22, 23, 30, 41, 80–82, 126, 133–134
  - evolutionary bases of 277–283
  - paleoanthropological perspectives on 280–282, *281*
  - *see also* contextualism
- individual↔culture relations *see* sociohistoric-cultural theory (Rogoff)
- inductive-hypothetical-deductive spiral 14, *14*
- inductive reasoning 6, 13
- infant attachment 70, 251
- infant development:
  - theory and research 69–71
  - *see also* child development
- information processing theory 104–105
- inherited, and heritability 447–449
- inner-biological level of environment 180
- instinct (innate behavior) 217, 221–225
  - and learning 377, 378
  - Lorenz's conception of 375, 376–380, 391, 393, 395, 481–482
- integrated coaction approach 176
- integration:
  - interdisciplinary 453–454
  - intersensory 208–211, *210*
  - levels of, and cognitive development 368–369
- integration of opposites 192, 229
- intelligence:
  - and genes 174, 175
  - the genetic-difference hypothesis 401–403
  - and heritability 401–434, **407, 408, 415, 427, 428**
- intelligence testing 61–62, 192–194, 258
  - and Burt 405–411
  - differences between African American and European American children 400–403
  - IQ amplitude and latency of evoked potentials 412–414
  - misuse of heritability concept in discussion of race differences 421–424
  - modeling and calculation issues 424–433, **427, 428**
  - molecular genetics research 418–420
  - post-Burt reviews of family studies 414, **415,** 416–418
- intensive designs, in longitudinal studies 492, *492*
- interactionism 54, 346, 348
- interactions 114–115, 198–199
  - holistic person↔context interaction theory (Magnusson) 346–351
  - moderate 100
  - moving beyond concept of 198–201
  - strong 200–201
  - structure-function relations 202–203
  - terminology 347
  - weak 199–200
- interdisciplinary integration 453–454
- intersensory integration 208–211, *210*
- interventions 512
- intervention research 137
  - issues for 134–135
- intraindividual change:
  - change-sensitive methods 487
  - descriptions of *230,* 230–231
  - descriptive and explanatory combinations 231–233, *232, 233*
  - explanations of 231, *231*
  - quantitative *versus* qualitative 233–234, *234*
- ipsative approach to development *see* idiographic (ipsative) approach to development
- IQ (intelligence quotient) *see* intelligence testing
- IRM (innate releasing mechanism) 376, 377–378, 481, 483
- IV (instrumental variable) analyses 44, 506

- Jensen, A. R. 15, 172, 188, 374, 400, *400,* 401–402, 403, 407, 409, 410, 411, 412, 413–414, 422, 423, 424, 425, 426, 428, 430, 440, 445, 447, 519
- Jews, Nazi-era anti-Semitism 382, 385, *386,* 395
- Johanson, D. C. 28, 61, 211, 280, *281,* 281–282, 286, 471
- Joseph, J. 37, 39, 101, 168, 188, 191, 273, 383, 404, 405, 412, 416, 417, 418, *418,* 419, 420, 423, 440, 442, 443, 444

- Kagan, J. 3, 8, 15, 72, 80, *81,* 92, 103, 122–123, 249–250, 252, 253, 254, 262, 275, 294
- Keller, E. F. 18, 36–37, *37,* 39, 373, 448
- King, M. L., Jr. 30
- King, P. E. 84, 300, *301,* 301–302, 509
- knowledge generation and knowledge application, bidirectional relation between 516
- Kuhn, D. 245, 246, *368,* 368–369
- Kuhn, T. S. 3, 7, 11, 15, 49, 73, 92, *93,* 136, 228

- Lamb, M. E. 18, 29, 31, 32–33, 35, 38, 46, *47,* 70–71, 84, 85, 191, 266, 288, 373, 440, 468, 474
- language acquisition 220
- latency stage of psychosexual development 140
- Layzer, D. 188, 194, 412, 424, 430–431, *431,* 436, *446,* 446–447
- learning 502
  - and behaviorism 63–64
  - child development 236–239, *237, 238, 239*
  - continuity-discontinuity issue 245–249, *247, 248, 249*
  - and instinct 377, 378

  phylogeny of 245–249, *247, 248,* **249**
learning curves 246–247, *247*
Lerner, J. V. 11, 17, 44, 85, 127, 134, 179, 198, 201, 227, 277, 287, 296, 299–300, 304–310, *305,* 320, 321, 486, 496–497, 510, 511
Lerner, R. M. 8, 9, 11, 12, 17, 18, 19, 21, 22, 23, 24, 25, 26, 27, 28, 29, 30, 31, 35, 36, 38, 39, 44, 45, 46, 48, 49, 50, 51, 58, 62, 71, 72, 73, 74, 75, 76, 78, 80, 82, 83, 84, 85, 86, 87, 88, 90, 91, 94, 111, 112, 113, 114, 115, 117, 120, 121, 122, 123, 125, 126, 127, 132, 133, 134, 135, 136, 137, 138, 139, 149, 153, 170, 171, 172, 173, 176, 180, 181, 186, 187, 189, 190, 195, 196, 198, 199, 200, 201, 204, 205, 211, 216, 224, 227, 228, 235, 245, 252, 253, 254, 255, 256, 265, 266, 267, 268, 269, 270, 274, 275, 276, 277, 283–287, *285,* 288, 293, 294, 295, 296, 297, 298, 299, 304–311, 312, 314, 317, 318, 320, 321, 322, 325, 331, 338, 339, 342, 344, 345, 347, 349, 352, 357, 360, 361, 363, 365, 369, **369–370,** 370, 374, 381, 421, 434, 440, 448, 451, 452, 466, 467, 474, 481, 484, 486, 487, 488, 490, 492, 493, 494, 495, 496–497, 498, 501, 504, 507, 508, 510, 511, 512, 513, 514, 515, 516, 517, 518, 519
Lerner and Lerner model of PYD 304–311
levels 96–97
  and the nonequivalent temporal metric 497–499
levels-of-organization hypothesis 128–130, *129, 130,* 204
Leventhal, T. 1, 46, *47,* 84–85, 519
Lewis, M. *69,* 69–70, 71, 75, 84, 108, 121, 201, 293, 295, 296, 312, 439, 440, 479
Liben, L. S. 46, *46,* 84, 185, 186, 188, 288
libido 131, 143, 199
Lickliter, R. 8, 31–32, *32,* 33, 35, 37, 38, 39, 41, 48, 57, 58, 83, 84, 87, 108, 112, 114, 115, 118, 121, 124, 132, 136, 139, 168, 172, 182, 183, 191, 195, 200, 223, 240, 266, 267, 269, 272, 273, 276, 277, 341, 349, 357, 417, 438, 450, 474, 477, 478, 479–480, 483
life-course theory (Elder) 313, 328–331, *329*
  conclusions about 334–335
life course construction 332–334, *333*
life-span developmental theory 66–67, 73, 77–78, 313–314
  conclusion regarding 321
  framing concepts 314, **315,** 316
  ontogenetic and historical contextualism paradigm 316–317
  SOC (selective optimization with compensation) model 317–321, **319,** *320,* 321, 327, 328
life-span theory of control (Heckhausen) 326–328
limits, presupposition of 250–253
Little, R. 298, *298,* 304, 311
Little, T. D. 29, 486, 488, 491, 493, *493,* 494, 501, 504, 510
living system, human development as 283–285, *285*
Locke, J. 55–56, 63
logic, faulty, in developmental research 224
longitudinal studies 505–506
  innovations in 496
  nonequivalent temporal metric 497–499
  ontogenetic observational point selection 496–497
  temporal spacing of observations 494–496
  three bases of methodological decisions 493–494
  time and timing issues 491–493, **492**
Lorenz, K. 15, 84, 101, 117–118, 172, 216, 217, 220, 221, 222–224, 225, 233, 250, 252, 341, 374, *375,* 375–400, 402, 454, 455, 458, 481–492, 484
Lovejoy, C. O. 280–281, *281*

macrosystem 76, 335, 338
Magnusson, D. 20, 21, 83, 85, 86, 294, 312, *346,* 346–351, 354, 360, 368, 417, 421, 437, 439, 440, 466, 467
males:
  sex differences in gametic potential 455–456
  social and sexual development and gametic potential 456–458
manipulative studies 63
Mascolo, M. F. 8, 11, 48, 84, 85, 105, 200, 204, 240, 312, 331, 355–356, 357, 358–359, *359,* 360–361, 363, 365, 366, 367, 380, 417, 450, 479, 489, 494
maturation 118, 154, 213–214, 218, 228
maturation↔experience relations 214–215, 216
maturational readiness 62
mechanism elucidation 443
mechanist models 72–73, 78, 94, 95–99, *98,* 121, 199
  continuity-discontinuity issue 263, 264
  issues of development 112–123
  nurture, mechanist theory 100–105, 136, 153, 154, 199
  problems of 106–107
  translation into theory of development 99–100
Medieval Christianity 54
mesoderm 213
mesosystem 76, 335, 338
metatheory 7, 11, 15, 18
  models of development 92–139
methodology 17–18, 31
  change, problematics in study of 488–489
  cross-sectional designs 20, 486–487
  data analysis issues 499–511, **500,** *500, 503*
  implications of Gottlieb's ideas for 275–276
  implications of RDS-based theories for 486–520
  person-centred, idiographic 42–43
  systems science 43–44
  variable *versus* person-centred foci 489–491
  see also longitudinal studies; mixed-methods research; qualitative research methods; quantitative research methods; RCTs (randomized control trials)
methylation 33, 34
microsystem 76, 335, 338
militant enthusiasm 377, 393–396, 454
Mischel, W. 79, 146, 256, *291,* 291–293, 359, 361
Mistry, J. 18, 23, 24, 28, 38, 84, 189, 288, 363, 364, 365, *366*, 367, 474, 517
mixed-methods research 504–505, 510
moderate interactions 200
modern synthesis 31, 32
Molenaar, P. C. M. 8, 9, 18, 20, 21, 22, 27, 28, 38, 41, 42, *42,* 43, 44, 45, 46, 48, 62, 84, 85, 86, 122, 135, 136, 140, 152, 162, 163, 164, 165, 166, 169, 195, 198, 201, 213, 262, 266, 286, 292, 293, 312, 347, 349, 350, 355, 358, 360, 361, 364, 366–367, 369, 374, 419, 424, 448, 487, 489, 490, 491, 494, 496, 497, 499, 500, 501, 502, 505, 510
Moltz, H. 201, *218,* 218–219, 220, 221, 266, 267
Monist League 59, 381, 387, 390
Moore, D. S. 18, 33, 34, 35, *35,* 36, 37, 38, 39, 41, 87, 119, 170, 172, 173, 175, 176, 177, 182, 184, 191, 192, 195, 223, 266, 269, 272, 273, 275, 373, 403–404, 416, 421, 422, 424, 430, 434, 436, 438, 440, 443, 445, 446, 447, 449, 451, 468, 473–474, 477, 480
moral development 11, 84, 289–291, 509
  Kohlberg's theory of 6, **6**
  see also PYD (Positive Youth Development)
mRNA (messenger ribonucleic acid) 33, 34, 36
  see also epigenetics
Müller, U. 18, 23, 27, 46, *46,* 84, 85, 287, 289
multilevel context of probabilistic epigenesis 276–278

Mussen, P. H. 19–20, *20,* 68, 71, 82, 508
myth of the first three years 251

narrow heritability 443
  see also heritability
National Task Force on Applied Developmental Science 514–516
native competence 357
nativism 473–474
  see also neo-nativism
natural selection 32, 33, 56–57, 59
  and genetic reductionism 378, 379, 382, 383, 384, 386, 392–393, 396, 397, 399, 432, 454, 455, 456, 457, 459, 463, 464, 466, 468, 477, 478
nature effects:
  continuum of indirectness *183,* 183–188
nature-nurture controversy 15, 93, 154, 170–173, 275
  Anastasi's scholarship on 173–177, 183–192
  "Big Five" personality traits 24
  and epigenetics 34
  factors in maintenance of 273–274
  heredity-environment relations 177–192, *178, 183, 188*
  historical role of **49,** 49–51
  idiographic (ipsative) approach 168
  norm-of-reaction concept 192–196
  RDS metatheory 312
  Rushton's scholarship on 469–473
  Schneirla's scholarship on 197–198, 201–206, *205, 206,* 209, 211, 213–218, 219, 221, 223, 225–227
  terminology 49, **49**
  see also child development
Nazism:
  anti-Semitism 382, 385, *386,* 395
  Lorenz's Nazi-era work 380–387, *386*
negentropy 343
neo-eugenics 38, 50
neo-nativism 356–360
  see also nativism
neoteny 278, 279
Nesselroade, J. R. 8, 9, 13, 17, 18, 20, 21, 22, 24, 27, 28, 38, 42, 43, *43,* 44, 48, 62, 72, 73, 76, 78, 81, 84, 85, 86, 122, 135, 140, 152, 160, 162, 163, 164, 165, 166, 169, 195, 235, 250, 259, 260, 262, 286, 292, 293, 294, 313, 344, 350, 355, 358, 360, 366–367, 374, 419, 487, 488, 489, 490, 491, 492, 493, 494, 496, 497, 499, 501, 502, 505, 510
Neugarten, B. L. 66, 67, *67,* 78, 141, 263
neuroscience 105
newborn stepping response 344, 345
niche picking 274
Noble, D. 8, 30, 31, 32, 34, 37, 38, 39, 85, 454, 474, 475, *475,* 479
nomothetic processes 152, 163, 165, 166
  see also stage theory approach to development
non-ergodic methodology 505
non-normative, life-event influences on development 316, 317
nonequivalent temporal metric 497–499
nonlinearity, in the person↔environment system 349–350
nonreversal shift 129, *129,* 130
norm-of-reaction concept 192–194
  limitations of 194–196
normative, age-graded influences on development 316
normative, history-graded influences on development 316–317
norms 62
novel forms development across life 340–341
Novikoff, A. B. 49, 91, 108, 203, *203,* 369, 450
Nucci, L. P. 11, 29, 105, 289–291, *290*
nurture *see* nature-nurture controversy
nurture, mechanist theory of development 100–105, 136, 153, 154, 199
  see also mechanist models
nurture effects:
  continuum of breadth *188,* 188–191

object permanence 167
ontogenetic contextualism 316–317
ontogeny 31, 32, 35, 41, 53, 57, 269, 270, 277–278
  Gould's views on 277–283
ontology 17
operant conditioning 3, 63, 72, 97, 100, 101, 104, 114, 502
  see also conditioning
opposites of identity 26, 27, 37, 177, 487
opposites of integration 188
optimization *see* SOC (selective optimization with compensation) model
oral stage of psychosexual development 131, 140, 141
organic effects of nurture 189
organism-in-environment system 352–355
organismic models 72–73, 74, 94, 95, 107–111, *109,* 121–122, 136, 153, 154, 199, 229, 241
  continuity-discontinuity issue 263, 264
  issues of development 113–123
  translation into theory of development 111–112
original sin 54
orthogenetic principle 11, 15, 64, 166–168, 229–230, *242,* 242–243, *244,* 254, 264, 343
  and the continuity-discontinuity issue 243, 245
outreach scholarship 137, 517
overgeneralization in developmental research 187
Overton, W. F. 2, 3, 7, 8, 9, 11, 12, 13, 14, 15, 17, 18, 19, 21, 22, 23, *24,* 24–25, 27, 28, 29, 30, 31, 34, 37, 39, 41, 42, 43, 44, 45, 46, 48, 49, 50, 51, 54, 65, 71, 72, 73, 74, 78, 80, 83, 84, 85, 86, 89, 90, 91, 92, 93, 94, 95, 99, 100, 105, 107–108, 110, 111, 112, 113, 114, 115, 116, 117, 121, 123, 124, 125, 128, 131, 134, 135, 136, 138, 139, 142, 143, 149, 153, 155, 170, 174, 175, 176, 177, 182, 188, 190, 192, 198, 199, 200, 201, 213, 216, 223, 228, 229, 235, 240, 262, 263, 266, 267, 268, 270, 275, 283, 286, 287, 288, 289, 299, 310, 312, 334, 341, 342, 347, 349, 352, 357, 360, 361, 364, 368, 369, **369–370,** 438, 440, 444, 450, 479, 487, 489, 490, 491, 492, 498, 505, 507, 508, 509, 511, 519

paedomorphosis 278, 279
palaeoanthropology 280–282, *281*
panel designs, in longitudinal studies 492, *492*
paradigms 7, 11, 15, 69
paradigm shifts 228
parenting 288
  child↔parent relations 293–297, 349
peer groups 4
people of color:
  and social stereotypes 186–188
  see also African Americans; race
perception, capacity for 209
person-centred, idiographic methodology 42–43
person-in-world system 354
personality traits:
  FFT (Five Factor Theory) 24, 26, 27, 38, 291, 292, 374
person↔context relations 25, 70–71
person↔environment system:
  features of 348–351
  see also holistic person↔context interaction theory (Magnusson)
phallic stage of psychosexual development 140, 141
phase shifts 344, 345
phenotypes, and the norm-of-reaction concept 192–196
philosophy:
  and developmental scientists 11–14, *12, 14*
philosophical models of development 71–73, 92–95
  implications of 135–138
  role of 71–73
philosophical roots of developmental science 51–56
  relationship with research and theory 3–7, **6**
phylogenetic development 53, 56, 58, 203, 245

- phylogeny 31, 32, 35, 53, 57, 58, 269, 277–278
  - Gould's views on 277–283
  - of learning 245–249, *247, 248,* **249**
- physical disability 214–215
- physical-environmental level of environment 181
- Piaget, J. 2, 6, 10, 19, 48, 57, 64, 66, *66,* 67, 68, 71, 72, 74, 83, 105, 109, 111, 112, 115, 121, 131, 141, 143, 147, 149, 151, 154, 155, 211, 213, 228, 229, 233, 245, 263, 289, 324, 365, 454, 461, 494, 508
- PKU *(phenylketonuria)* 179, 184
- plasticity 20, 22, 23, 30, 35, 40, 41, 45, 190, 211, 213, 256
  - action theories 322–323
  - dynamic systems theory 342–344
  - limits of 132–133
  - parameters of 254–245
  - presupposition of 250–253
  - probabilistic epigenesis 253–254
  - stereotypy 204–206, *205,* 207–208
- Plato 49, 51–52, 53, 54
- Plomin, R. 38, 41, 72, 84, 87, 88, 89, 99, 100, 117, 121, 139, 172, 175, 217, 273, 274, 341, 352, 383, 414, 417, 419, 421, 434, 435, 437, 438, 440–441, 443, 448, 449, 450–451, 452, 483
- policies 512–513
  - and human aggression 395–396
  - and the nature-nurture controversy 274
- polygenic inheritance 416
- polymorphism 418–419, 441, 442
- potentials:
  - evoked cortical 412, 414
  - gametic potential, sex differences in 455–456
  - gametic potential, social and sexual development 456–458
- PPCT (process-person-context-time) model 337–337
- predetermined epigenesis 112, 199, 271
- preempirical beliefs 250
- preoperational stage 72, 213
- presupposition of limits 250–253
- presupposition of plasticity 250–253
- primary control 327–328
- probabilistic epigenesis 38, 39, 95, 112, 118–119, 175, 203, 213, 227, 247, 267, 268–276, 283, 476
  - as a "compromise" conception 128–132, *129, 130, 131*
  - concept of development in 125–128
  - and contextual models 124–125
  - and dynamic coactive systems theory 355–356, 362–363
  - and dynamic systems theory 341, 342
  - and human evolution 277–278
  - limitations of concept 132–134
  - multilevel context 276–278
  - and plasticity 253–254
  - and strong interactions 200, 201
- probabilistic functionalism 79–80
- process evaluations 513
- process-relational paradigm 7, 11, 15, 22, 23, 24–25, 26, 30, 39, 42, 45, 228, 490
- programs 512–513
- propensity score analyses 44, 506
- Protestant Reformation 53
- psychoanalysis 64, 68, 71, 112, 389
- psychological structures, dynamic development of 355–356, 361
- psychosexual stages of development (Freud) 131, 140–141
- psychosocial development 162
- puberty 116, 241, 258, 332, 470, 496
  - *see also* adolescence
- purpose, Damon's study of 300–301, 302
- PVEST ("phenomenological variant of ecological systems theory") model 302–304
- PYD (Positive Youth Development) 298–300
  - 4-H study 306, 307–310
  - Damon, and the study of purpose 300–301, 302
  - Five Cs model 304, 305–307, 309–310
  - King's work 301–302
  - Lerner and Lerner model 304–311
  - qualitative research methods 509, 510
  - Spencer and the PVEST Model 302–304

- qualitative change 241–242
  - *versus* quantitative change 233–234, *234,* 239–240
- qualitative research methods:
  - importance of 504–505
  - innovations in 507–510
- quantitative change *240,* 240–241
  - *versus* qualitative change 233–234, *234,* 239–240
- quantitative research methods:
  - innovations in 505–506

- race:
  - "race-realist" views 469–470
  - racial hygenics, and Nazism 380, 382, 388, 390, 392, 397–398, 397–399
  - Rushton's views on 469–473
  - tripartate theory of 470
- racism:
  - and genetic reductionism 374
  - and the nature-nurture controversy 274
- Raeff, C. 2, 8, 9, 10, 11, 15, 31, 38, 46, 64, 108, 110, 111, 114, 115, 116, 121, 122, 125, 153, 166, 167, 189, 204, 213, 228, 229, 235, 240, 243, 245, *245,* 277, 284, 305, 312, 314, 340, 342, 343, 351, 354, 364, 440, 450, 474, 479, 483
- RCTs (randomized control trials) 44, 506–507, 518
- RDS (relational developmental systems) theories and concepts 11, 12, 15, 16, 17, 18, 21–22, 21–26, *25,* 49, 51, 266–267, 310–311, 312–313
  - action theories of human development 313, 320, 321–328
  - and "behavioral neophenotypes" 467–468
  - bioecological model of human development (Bronfenbrenner) 313, 335–339
  - change, problematics in study of 488–489
  - and the concept of development 7–8
  - continuity-discontinuity issue 263, 264
  - defining features of **369–370**
  - design criteria 286–287, 311
  - developmental contextualism sample case 293–297
  - developmental regulations, adaptive developmental relations and human agency 28–30
  - developmental systems, Gottlieb's theory of 267–273
  - dynamic coactive systems theory (Fischer) 355–363
  - dynamic systems theory (Thelen & Smith) 339–346
  - emergence of 82–85
  - epigenesis, Gottlieb's theory of 268–276
  - and epigenetics 38–41
  - examples of models 287–293
  - as frame for discussing genetic reductionist models 369–371
  - holistic, developmental, systems-oriented approach (Wapner) 351–355, *353*
  - holistic person↔context interaction theory (Magnusson) 346–351
  - and the idiographic (ipsative) approach 169
  - implications for developmental science 85–86
  - implications of for methodology 486–520
  - levels of integration and explanation of cognitive development 368–369
  - life-course theory (Elder) 313, 328–335, *329, 333*
  - life-span developmental theory (Baltes) 313–314, **315,** 316–321, **319,** *320*
  - life-span theory of control (Heckhausen) 326–328
  - multilevel context of probabilistic epigenesis 276–278

  ontogeny and phylogeny, Gould's views on 277–283
  philosophical models of development 136–137, 138
  PYD (positive youth development) 298–311
  and social justice 44–45
  sociohistoric-cultural theory (Rogoff) 363–368
  three moments of analysis 26–28
readiness, maturational 62
recapitulation theory 59–61
reciprocity, in child↔parent relations 349
recursive sequences 149, 150
reductionism 11, 95, 97–98, 106–107
reductionist models of development, vestiges of 87–91, **88, 89**
Reese, H. W. 3, 7, 8, 9, 13, 17, 18, 19, 20, 24, 48, 67, 71, *71,* 73, 76, 77, 78, 92, 93, 94, 95, 99, 100, 103, 104, 107–108, 111, 112, 113, 114, 115, 116, 117, 121, 122, 135, 153, 199, 200, 262, 313, 489, 506, 507
regression continuity designs 506
regression discontinuity designs 44
regulative rules 325
relational developmental systems *see* RDS (relational developmental systems) theories and concepts
religion, and Positive Youth Development 302
research:
  reasons for 4–7, **6**
  relationship with theory and philosophy 3–7, **6**
research methodology *see* methodology
respondent-in-ambience system 354
reversal shift 129, *129,* 130
rhythmic infants 225–227, 294
Richardson, K. 37, 39, 41, 61, 170, 172, 188, 192, 195, 403, 405, 406, 412, 417, 418, 419, 420, 422, 423, 424–425, *425,* 430, 436, 442, 443
Riegel, K. F. 21, 24, 71, 72, 73–75, *74,* 79, 94, 127, 141, 180
RNA (ribonucleic acid) 33, 36, 119
  see also epigenetics; mRNA (messenger ribonucleic acid)
Rogoff, B. 11, 28, 29, 189, 288, 312, 331, 352, 363–368, *364,* 380, 489, 517
Rose, T. 9, 18, 21, *21,* 22, 23, 27, 42, 62, 85, 86, 120, 122, 152, 162, 195, 216, 225, 238, 286, 292, 295, 349, 350, 355, 374, 487, 489, 490, 494, 496, 499, 503, 505
Rushton, J. P. 41, 84, 87, 88, 117, 121, 170, 172, 373, 374, 391, 400, 401, 414, 421, 422, 423, 436, 447–448, 449, 452, 458, 460, *469,* 469–473, 502

Sameroff, A. J. 72, 74, 83, 96, *96,* 112, 115, 123, 136, 312, 357, 360, 368, 440
Saunders, P. R. 39, 273, 475, *475,* 477
Schaie, K. W. 20, 73, 76, *77,* 78, 80, 122, 141, 195, 250, 263, 294, 313, 492, 497, 499, 506
Schneirla, T. C. 10, 15, 28, 29, 36, 49, 50, 75, 91, 108, 115, 117, 119, 120, 122, 124, 125, 128, 139, 154, 165, 168, 169, 173, 174, 193, 196, *197,* 197–198, 201–206, *205, 206,* 207–208, 209, 211, 213–218, 221, 223, 225, 226, 227, 228, 229, 247, 252, 266, 267, 269, 271, 273, 277, 280, 283, 286, 287, 293, 294, 295, 297, 312, 318, 322, 325, 331, 339, 341, 347, 349, 352, 368, 369, 374, 377, 395, 440, 450, 461, 467, 474, 476
school environment 81–82
science, and politics 380
scientific revolutions 228
scientific roots of developmental science 56–91, **88, 89**
  behaviorism and learning theory 63–64
  Darwin 56–58
  Haeckel 59–60
  Hall 58, 60–61
  1970s, 1980s and 1990s period 69–82
  1950s and 1960s period 66–68
  twenty-first century 82–91, **88, 89**
  World War II period 64–66
secondary control 327–328
selection:
  and ethical degeneration 396–399
  see also SOC (selective optimization with compensation) model
self-fulfilling prophecies 187, 188
self-regulation 28–29
  see also developmental regulation
self-stimulation in development 225–227
sensation, capacity for 209
sensitive period 219, 251, 254–255
sensorimotor stage 72, 213
sensory cortex *see* A/S ratio (association cortex/sensory cortex ratio)
sequential designs, in longitudinal studies 492, *492*
sex differences in gametic potential 455–456
sexual behavior, and homology 460–461
sexual development, and gametic potential 456–458
single-cohort designs, in longitudinal studies 492, *492*
skill, dynamic 360–363
skin color, and social stereotypes 186–188
Skinner, B. F. 3, 11, 55, 57, 63, 72, 99, 100, *100,* 101, 102, 107, 139, 141, 197, 199, 246–247, *247,* 248, 480–481, 482, 483, 502
Smith, L. B. 48, 83, 85, 112, 149, 200, 276, 283, 312, 339–346, *340,* 347, 348, 349, 354, 357, 360, 365, 368, 417, 421, 438, 439, 440, 466, 467
SOC (selective optimization with compensation) model 317–321, **319,** *320,* 321, 327, 328
social class, and the nature-nurture controversy 274
Social Darwinism 59, 380, 387, 390, 393, 396
social development, and gametic potential 456–458
social isolation 220
social justice 17, 18, 23, 30, 44–45, 90, 310
  and ADS (applied developmental science) 518–520
social learning theory 63–64, 80
social policies *see* policies
social programs *see* programs
social stereotypes 185–188
socialization 50
sociobiology 11, 38, 87, 97, 172–173, 452
  adaptations 462–463
  conclusions about 468–469, 473–474
  evaluation of 458–459
  exaptation 464–466
  gametic potential, and social and sexual development 456–459
  and genetic determinism 453–454, 458
  genetic determinism and human development 458
  genetic determinism and interdisciplinary integration 453–454
  heritability analyses 461–462
  homology 460–461
  human aggression 454–455
  Lorenz 399–400
  nativism 473–474
  RDS, and role of behavioral neophenotypes 467–468
  role of organism in own evolution 466–467
  Rushton's work 469–473
  scientific goals of 452–453
  sex differences in gametic potential 455–456
sociocultural-historical level of environment 181–182
sociohistoric-cultural theory (Rogoff) 363–368
soft assembly 344
sources of development 117–121
specificity principle 23, 42, 43, 138, 304, 361, 491
Spencer, M. B. 83, 84, 86, 87, 187, 188, 302–304, *303,* 309, 310, 509
spirituality, and Positive Youth Development 302
SRCD (Society for Research in Child Development) 19
stability-instability issue 256–262, *257, 259, 260, 261*
  differential approach to development 160–162
  dynamic systems theory 342–344

stage mixture 150
stage theory approach to development 139, 140–141, 155, 169
  abruptness, and stage transition 144, *145,* 145–150
  concurrence 145, 151–152, **152**
  developmental stage definition 141–145
  individual differences in 153
  relation of concepts of development 153–155
stage transition 144, *145,* 145–150
stages of development 117
status attributes 156, *156,* 162
stepping response 344, 345
sterilization, forced 372–373
stereotypes, social 185–188
stereotypy 213
  and plasticity 204–206, *205,* 207–208
stimulative effects of nurture 189–191, *190*
stimulus–response 100–101, 102, 104, 116
storm and stress in adolescence 60, 262, 298
strong interactions 200–201
structural change, *versus* change 115–116
structure, properties of 144
structure-function relations 202–203
  versus antecedent-consequent relations 114–115
summative evaluations 513
superego 3, 81
survival of the fittest 56–57
synchrony, within stage 145, 151–152, **152**
synthesis of wholes 26, 27–28, 190, 487
systems science methodology 43–44, 505

Tabery, J. 172, 173, 175, 443, *443,* 451, 462, 473
tabula rasa 55
teleological-directedness 111–112, 116, 121, 354
Terman, L. M. 19, 58, *61,* 61–62, 252
Thales of Miletus 51
Thelen, E. 48, 83, 85, 108, 112, 149, 200, 276, 283, 312, 339–346, *340,* 347, 348, 349, 354, 357, 360, 365, 368, 417, 421, 438, 439, 440, 466, 467
theoretical reductionism 275
theory:
  and developmental scientists 11–14, *12, 14*
  life-cycles of 48–49
  relationship with research and philosophy 3–7, **6**
"third source" of development 225–227
Thomas, A. 79, 181, 201, 225, *226,* 226–227, 234, 277, 287, 293, 294, 295, 296
Thomas Aquinas, Saint 53, 127
time:
  in dynamic systems 345
  in longitudinal studies 491–493, **492,** 494–496
  nonequivalent temporal metric 497–499
Tobach, E. 8, 28, 29, 50, 59, 87, 108, 114, 121, 122, 124, 125, *126,* 126–127, 175, 197, 198, 201, 203, 205, 213, 216, 217, 266, 267, 273, 277, 280, 283, 286, 287, 297, 312, 341, 349, 396, 440, 461, 463, 467, 474
Tolan, P. 18, 22, 31, 44, 489, 505, 507, 508, *508,* 509, 510
topic-specific theories 11
transitions, in systems 344–345
trauma, and children 289
tripartate theory of race (Rushton) 470
Turiel, E. 11, 84, 105, 141, 146, 148, 150, 289–291, *290*
twins 179–180, 190, 191, 294
  behavior genetics 450
  intelligence and heritability studies 404, 406–409, **407, 408,** 410–412, 414, **415,** 416–418, 418–421

University of West Virginia conferences 20, 21, 78

value attributes 163–165
variable-specific theories 11
variance-partitioning 443
vertical coactions 269
vertical décalage 151
VIA (Values in Action) 292
Vincentized curves *503,* 503–504
vision, development of binocular 220–221
vitalism 52, 203
von Bertalanffy, L. 1, 9, 12, 24, 49, 91, 92, 96, 105, 108, 110, *110,* 125, 263, 275, 348, 463, 486
von Eye, A. 22, 38, 42, 44, 172, 173, 445, 448, 452, 489, 491, 493, 494, *494,* 505, 510
Vygotsky, L. 64–65, *65,* 67, 363, 365

Wahlsten, D. 83, 112, 170, 172, 191, 267, 270, 273, 423, 440, 452
Wapner, S. 125, 229, 285, 312, 351–355, *352, 353,* 360, 368
Watson, J. B. 63, 91, 92, 103, *103,* 121, 197, 246
weak interactions 199–200, 347
Wilson, E. O. 11, 97, 172, 312, 399, 440, 452–453, *453,* 454, 455, 458–459, 460, 461–462
Witherington, D. C. 8, 29, 31, *32,* 35, 36, 37, 38, 42, 48, 58, 84, 108, 114, 121, 128, 136, 182, 240, 267, 269, 276, 277, 341, 343, 344, 349, 357, 417, 440, 467, 479–480, 483, 490, 519
Wohlwill, J. F. 117, *142,* 142–143, 144, 150, 151–152, *152,* 256, 265, 496, 500, 501
World War II period:
  Lorenz's work in 380–391
  scientific roots of developmental science 64–66

youth programs *see* PYD (Positive Youth Development)

zone of proximal development 65