CHILD PSYCHOLOGY IN RETROSPECT AND PROSPECT

In Celebration of the 75th Anniversary of the Institute of Child Development

The Minnesota Symposia on Child Psychology

Volume 32

Edited by

WILLARD W. HARTUP

RICHARD A. WEINBERG

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LEA
2002
LAWRENCE ERLBAUM ASSOCIATES, PUBLISHERS
Mahwah, New Jersey London

Lawrence Erlbaum Associates, Inc., Publishers
10 Industrial Avenue
Mahwah, NJ 07430

Cover design by Kathryn Houghtaling Lacey

Library of Congress Cataloging-in-Publication Data

Child psychology in retrospect and prospect: in celebration of the 75th anniversary of the Institute of Child Development / edited by Willard W. Hartup, Richard A. Weinberg.

p. cm.—(The Minnesota symposia on child psychology; v. 32) Includes bibliographical references and indexes. ISBN 0-8058-3971-2 (alk. paper)

1. Child psychology—Congresses. 2. Child development—Congresses. I. Hartup, Willard W. II. Weinberg, Richard A. III. University of Minnesota. Institute of Child Development. IV. Minnesota symposia on child psychology (Series); v. 32.

BF721 .C5164 2001
155.4—dc21 2001040929

Books published by Lawrence Erlbaum Associates are printed on acid-free paper, and their bindings are chosen for strength and durability.

Printed in the United States of America
1 0 9 8 7 6 5 4 3 2 1

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Preface

Anniversaries suggest stock taking as well as celebration. Accordingly, when the Institute of Child Development, University of Minnesota, neared its 75th anniversary, the faculty wished to observe the event in two ways—by examining current research in developmental psychology in historical context, and by studying the history of the Institute itself. Included in this volume are eight essays based on presentations made at the 32nd Minnesota Symposium on Child Psychology held at the university on October 19–21, 2000, in observance of this Diamond Jubilee.

All of the essays in the book are devoted to developmental science, its history and current status. The editors deliberately sought diversity in these manuscripts. For example, one contains an assessment of new ideas concerning the manner in which development affects evolution rather than the reverse. In another manuscript, current work on the linkages between brain and behavior is assessed within an extended historical context. And so, too, the volume includes essays on the development of symbolic thought, the understanding of spoken language, and personality—each describing contemporary work within an analysis of the history of ideas leading to it.

The volume also contains two essays in which "history" figures some-what differently: One deals with the manner in which the child's development contributes to social historical change as well as the reverse. In this essay, readers are reminded clearly that the dialectic between ontogeny and the social-historical context must be a constant consideration in developmental science. Still another essay contains a penetrating analysis of the philosophical and ideological foundations of the contemporary field of child psychology as a whole. "History" thus comes alive in this volume in several guises: as the context in which changing ideologies shape scientific endeavor; as an accounting, through time, of ideas undergirding contemporary research; and as processes through which ontogeny contributes to both the origin of species and the origins of social change.

The remaining essay deals with institutional history, specifically the history of the Institute of Child Development. No such account has been published previously and this one was written to complement the other essays in the volume. As it happens, the Institute came into being as a result of major changes that occurred in the United States during the early

20th century in ideas about childhood, in the uses and responsibilities of science with respect to children, and in the utilization of great philanthropic and educational institutions to shape public policy concerning children and families. In this chapter, the forces are described that brought the Institute of Child Development into being as well as the forces that affected its subsequent history.

Taken together, the manuscripts included in this book show how the history of science connects past and future, how it gives the individual investigator an identity and sense of purpose that he or she would not have without it, how contemporary studies occur within larger traditions, and how institutions like the Institute of Child Development constitute cultural traditions of their own. Collectively, these essays show that the past explains a great deal—whether we want to know about the processes through which the child acquires symbolic thought or whether we want to know how and why, during the last century, a few enduring centers were established for the scientific study of children and adolescents. Reading these essays, one obtains a sense of how the past becomes evidence, how it forms models for the way we think, and how intellectual challenges arise.

As editors, we are grateful to the authors for undertaking the retrospective/prospective task we laid out; most of them are not trained historians and the task was challenging. The historians among our authors also deserve great credit for the originality of their syntheses. The anniversary celebration held in October 2000 brought together current and former students, current and former faculty, and current and former staff; the presence of these individuals was crucial to the success of the symposium that had been planned. In fact, the celebration was gala: There was discussion, thesis and antithesis, feasting, music, and dancing.

Special thanks are also due to several other individuals: to Ann Masten, Director of the Institute, for her many efforts on behalf of this event including the scholarly proceedings, the gala celebration, and this volume; to Lu-Jean Huffman-Nordberg, who headed the support staff for a number of previous symposia as well as this one (just before she retired); and to Claudia Johnston and Wendy McCormick for services too numerous to mention. Sponsors of the celebration included the Institute of Child Development, The College of Education and Human Development, the Leon and Marian Yarrow Endowment for Research in Social Relationships, and the Parents' Institute Fund. As organizers of this event, we are grateful to these entities and to the individuals responsible for them.

Finally, we wish to express our gratitude to the thousands of children, adolescents, women, and men who have been active in the Institute of Child Development over the last three quarters of a century. These include babies and preschool children, school children and adolescents,

professors, teachers, undergraduate and graduate students, cooks, librarians, accountants, secretaries, statisticians, laboratory technicians, parents, grandparents, University alumni, deans, presidents, site visitors, individuals on sabbatical leave, Minnesota Symposia guests, maintenance personnel, donors, colloquium speakers, and many others. Our research, our nursery school, our classes and seminars, our research and writing, and the many other activities that constitute the Institute would not have been possible without them. We believe these contributions merit celebration and, with these words, we invite readers to celebrate along with us.

—Willard W. Hartup
—Richard A. Weinberg

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1

Origin of Species: The Potential Significance of Early Experience for Evolution

Gilbert Gottlieb
University of North Carolina at Chapel Hill

The traditional approach to evolutionary psychology relies entirely on natural selection as the cause of the evolution of adaptations. Exclusive reliance on natural selection overlooks the fact that changes in development are a necessary prerequisite for evolutionary change. These developmental changes provide the material for natural selection to work on. In the neo-Darwinian scenario, the mechanisms of evolution are mutation or genetic recombination, selection, migration, and eventual reproductive isolation. In the spirit of evolutionary pluralism, I describe a different three-stage scenario in which migration (the invasion of new niches or habitats) may occur without mutation or genetic recombination and selection first initiating a change in genes or gene frequencies:

1. Known changes in developmental rearing conditions foster behavioral novelties (behavioral neophenogenesis) that heighten exploratory behavior, resistance to stress, and problem-solving ability, all of which favor the occupation of new niches (or survival in a greatly changed old niche).
2. Survival in an altered niche or occupation of a new niche brings out latent anatomical and physiological changes through the activation of previously quiescent genes.
3. The genetic composition of the population may eventually change through reproductive isolation, but in this model, that occurs only after phenotypic evolution has already occurred at the anatomical, physiological, and behavioral levels.

The modes of behavioral neophenogenesis—the developmental induction of novel behavior—are described and discussed, closing with an example of incipient speciation mediated by a developmental-behavioral change in the apple maggot fly *(Rhagoletis pomonella).*

CONVENTIONAL THINKING IN EVOLUTIONARY BIOLOGY

In biological circles, it is an open secret that Charles Darwin (1859), in his magnificent book *On the Origin of Species by Means of Natural Selection,* did not actually describe the origin of any species. Although Darwin did claim to have marshaled facts that supported his thesis that natural selection was the means for the origin of species, actually natural selection refers to what is happening in the subtitle rather than in the title of his book: *Or the Preservation of Favoured Races in the Struggle for Life.* Natural selection, acting as a filter, preserves (or destroys) that which is put to it; the question remains as to the origin of what is put to natural selection. When these two aspects of the problem—origin versus preservation—are not distinguished, a main problem in evolution, the origin of the variants thrown up to selection, is finessed, and one gets a distorted version of natural selection *causing* the adaptations, rather than preserving them, as in the following quotation: "Adaptation and natural selection are central concepts in the emerging science of evolutionary psychology. Natural selection is the only known causal process capable of producing complex functional organic ... adaptations" (Buss, Haselton, Shackelford, Bleski, & Wakefield, 1998, p. 533).

In contrast to the aforementioned, as first pointed out by St. George Jackson Mivart (1871), adaptations arise *before* they are selected for and are, therefore, not a consequence of natural selection. Adaptations are a consequence of individual development; changes in individual development produce new behavioral variations and different adaptations. Thus, it is changes in individual development that produce evolution through natural selection. As noted by Endler (1986), "Natural selection cannot explain the origin of new variants and adaptations, only their spread" (p. 51).

This essay is about the role that behavior plays in instigating evolution; more specifically, the fundamental importance of changes in behavior brought about by changes in prenatal and postnatal rearing environments. Behavior is the leading edge of evolution, as has been intermittently recognized since the time of Lamarck (Leonovicova & Novak, 1987). Conventional thinking in evolutionary biology would hold that genetic mutations, genetic recombination, or genetic drift are the leading edge of evolution. In the spirit of evolutionary pluralism, I am proposing a different pathway in which behavioral change leads the way to genetic change. Although

these different pathways are not mutually exclusive, what the molecular biologist Francois Jacob (1982) wrote in discussing the evolutionary path-way pertains to the present model: "It was not, however, biochemical innovation that generated the diversification of organisms. In all likelihood, things worked the other way around. It was the selective pressure resulting from changes in behavior or in ecological niches that led to biochemical adjustments and changes in molecular types"[1] (p. 41). Given that behavior can change before the population genotype changes, it is important to understand how such behavioral changes can be maintained across generations in the early stages of evolution.

RAPID "EVOLUTION" OF BEHAVIORAL DIFFERENCES BY SELECTIVE BREEDING AND THEIR MAINTENANCE ACROSS GENERATIONS BY STABILITY IN THE REARING ENVIRONMENT

Because genes are a part of the developmental system, of which behavior is also a necessary component, it is possible to selectively breed for any behavioral trait *once that trait has surfaced.* The evidence for that conclusion is presented in Table 1.1.

As can be seen in Table 1.1, it is possible to selectively breed animals for all kinds of existing behavioral differences: learning (maze dull vs. maze bright), spontaneous activity (high, low), audiogenic seizures (susceptible, nonsusceptible), alcohol preference (high, no preference), aggressiveness (high, low), mating speed (fast, slow), and so on. The differences in many of these strains (or lines) of animals developed rather quickly in the course of four or five generations of selectively breeding like to like. The trick is not in bringing about these line (strain) differences (selective breeding does that), but in maintaining the differences across generations. To maintain the differences one needs not only to selectively breed the animals in each generation but to make certain the prenatal and postnatal rearing conditions remain the same. If the genes alone were bringing about the behavioral changes, then the rearing environment would be irrelevant. But because the genes do not make behavior, it is the genes-in-the-recurring-developmental-system that make for the stability of the behavioral changes across generations. The only reason it is possible to perpetuate virtually any trait by selective breeding in the laboratory is that great pains are taken to ensure that environmental (prenatal and postnatal rear-

[1]I appreciate Professor Gerald Turkewitz, of Hunter College, calling this quotation to my attention.

TABLE 1.1
Outcomes of Selective Breeding for Behavioral Differences

Species	*Behavior*	*Source*
Rats	Maze learning	Tolman, 1924
Mice	Speed in traversing runway	Dawson, 1932
Rats	Spontaneous activity (wheel running)	Rundquist, 1933
Rats	Maze learning	Heron, 1935
Rats	"Emotionality" in open field	Broadhurst, 1965; Hall, 1938
Mice	Sound-induced (audiogenic) seizure	Frings & Frings, 1953
Fruitflies	Orientation toward or away from light source (positive and negative phototaxis	Hirsch & Boudreau, 1958
Rats	Saccharin preference	Nachman, 1959
Chickens	Aggressiveness	Guhl, Craig, & Mueller, 1960
Fruitflies	Components of courtship movements	Ewing, 1961
Fruitflies	Upward or downward orientation with respect to gravity (negative and positive geotaxis)	Hirsch & Erlenmeyer-Kimmling, 1961
Fruitflies	Mating speed	Manning, 1961
Mice	Alcohol preference	Rodgers & McClearn, 1962
Fruitflies	General activity	Manning, 1963
Honeybees	Nest cleaning, stinging	Rothenbuhler, 1967

ing) conditions remain as constant as possible over the course of the generations of selective breeding. (It is important to notice that not only adaptive traits can be selectively bred; pathological or disadvantageous traits are also responsive to selective breeding.)

The importance of controlling the rearing environment across generations is shown in a study by Kathryn Hood and Robert Cairns (1989), in which male mice were selectively bred for high or low aggression in dyadic tests in which two male mice were introduced to each other for the first time. During the course of selectively breeding for high and low aggression, the animals were reared in social isolation from the time of weaning until the dyadic test some 3 to 4 weeks after weaning. The investigators wanted to find out how important the social isolation period was in maintaining the line difference in aggression, so, in the present experiment, they reared half of the males from each line in social groups from weaning until testing. As shown in Fig. 1.1, there is a large difference in the two lines in attack frequency when they are reared as usual in social isolation (left side of Fig. 1.1), but this difference disappears entirely when the high-line mice are reared in social groups. The findings hold for testing both in the home cage as well as in a neutral cage under both rearing conditions.

So, once a new behavior has arisen or manifested itself, it can be perpetuated by selective breeding and a recurring developmental medium. But how does the new behavior arise in the first place?

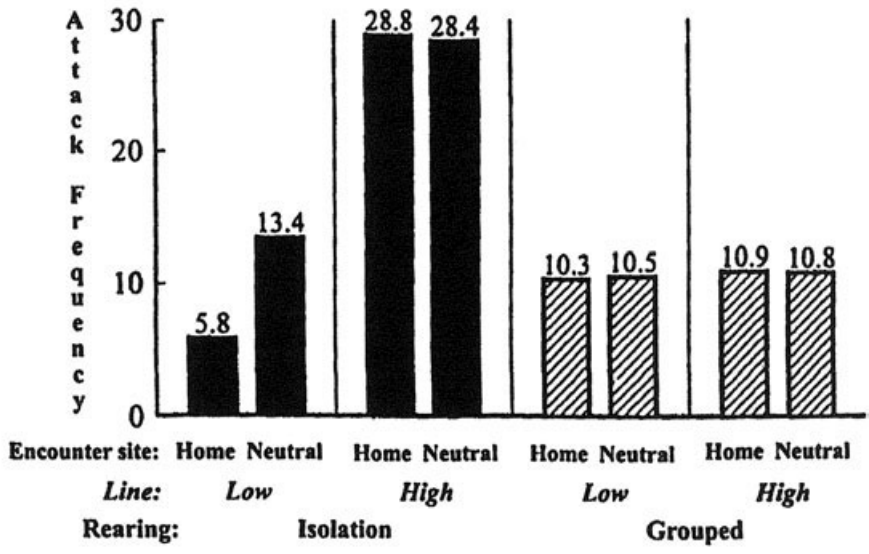

FIG. 1.1. Failure to maintain selectively bred line difference in aggression when rearing environment changed from social isolation (usual rearing environment) to social grouping. Data from Hood and Cairns, 1989. Figure from "The Significance of Biology for Human Development: A Developmental Psychobiological Systems Perspective," by G. Gottlieb, D. Wahlsten, and R. Lickliter, 1998, in *Handbook of Child Psychology: Theoretical Models of Human Development* (Vol. 1, 5th ed.) Copyright © 1998 by John Wiley & Sons, Inc. Reprinted by permission of John Wiley & Sons, Inc.

THE DEVELOPMENTAL INDUCTION OF NOVEL BEHAVIORAL PHENOTYPES

Since the 1950s developmental animal psychologists have demonstrated repeatedly that varying the early experiences of young animals alters their behavioral phenotype, even into adulthood if the rearing environment supports such persistence. The earliest of these studies concerned "handling" of rodents in the preweaning period (0–3 or 4 weeks of postnatal age). The so-called handling procedure involves separating the pups from their mothers for a few minutes each day during the preweaning stage of development. Although there are species and strain differences, by and large handled animals benefited from this intrusion, showing heightened exploration, increased resistance to stress, and improvements in learning ability in adulthood (Denenberg, 1969; Levine, 1956). It turned out that the most important mediator of the handling effect was the pronounced "extra" mothering (grooming, etc.) that the handled pup received upon return to the nest. The handled pups emit ultrasonic vocalizations upon

being returned to the nest and females become particularly solicitous upon hearing these vocalizations (Noirot, 1964). In studies that did not involve human intervention, Ressler (1963, 1966) found strain differences in mice in the amount of maternal handling of offspring and his findings on later effects in adulthood coordinate well with the findings from the human intervention studies: Maternally "handled" offspring (BALB strain) outperform offspring in the less maternally handled strain (C57BL) in operant bar pressing and visual exploration. Of special significance in the present context is Ressler's findings of a transgenerational effect: The grandchildren of both strains did well when their grandparents were of the BALB strain (foster BALB grandparents in the case of the C57BL mice).

In brief, then, to summarize the very extensive handling literature, this experience results in relatively stress-resistant animals, ones who would be capable of exploration (instead of freezing) and adaptive learning when faced with a completely strange and unfamiliar environment in adulthood. The research showed that the axis between the hypothalamus and the adrenal and pituitary glands was enhanced by the handling experience and that this anatomical-physiological change was correlated with the handled animal being able to tolerate greater stress in adulthood. Denenberg and colleagues showed that the handling experience had to occur early in development if it was to be effective. Animals subjected to the same experience at older ages did not benefit from the experience, as indicated by later tests of resistance to stress and of exploratory behavior.

Another experimental intervention that creates novel behavioral phenotypes through changes in development is the so-called "free" or environmental enrichment paradigm, in which, once again beginning in the 1950s, developmental animal psychologists began exposing rodents to what were called enriched early-rearing environments and examining the effect of the experiential manipulations on changes in brain size and brain biochemistry, and on problem solving ability, in adulthood (review in Renner & Rosenzweig, 1987). In 1952, Bernard Hymovitch showed in a definitive manner how enriched early experiences are crucial to later problem solving in adulthood. In this pioneering study, Hymovitch reared young rats under four conditions and then later tested them in the difficult Hebb-Williams maze. (He might not have gotten any effects of early experience if he had tested their learning ability in much simpler learning tasks, such as a Y-maze or a T-maze.) The animals were housed individually in (a) a stovepipe cage (which permitted little motor or visual experience, (b) an enclosed running or activity wheel (which provided a lot of motor experience but little variation in visual experience), or (c) a mesh cage that restricted motor activity but allowed considerable variation in visual experience as it was moved daily to different locations in the laboratory. A fourth group was made up of 20 animals that were reared socially in an

enriched environment that was very large (6' X 4') compared with the other conditions, and was fitted with a number of blind alleys, inclined runways, small enclosed areas, apertures, and so on, that offered the rats a wide variety of opportunities for motor and visual exploration and learning in a complex physical and social environment (labeled Free Environment in Tables 1.2 and 1.3). The animals lived in these four environments from about 27 days of age to 100 days of age, at which time testing in the Hebb-Williams maze was completed. The results of testing are shown in Table 1.2.

As can be seen in Table 1.2, rearing in the stovepipe and the enclosed running wheel led to the same level of poor performance, whereas rearing in the mesh cage and the free environment led to the same level of good performance over 21 days of testing in the Hebb-Williams maze. All the groups also showed the same level of improvement over the 3 weeks of testing, so the animals reared in the mesh cages and free environment began functioning at a superior level early in the testing.

Next, in order to determine whether it was the *early* experience in each environment that made for the differences among the groups, Hymovitch repeated the experiment with four groups of animals that differed in *when* they had the free (enriched) environment or stovepipe experience: One group had the free-environment experience from 30 to 75 days of age and then were placed in the stovepipe for 45 days; a second group had the stovepipe experience from 30 to 75 days and then had the free-environment experience for 45 days; a third group remained in the free environment throughout the experiment; and a fourth group remained in their normal laboratory cages throughout the experiment. This last group would be the most thoroughly or consistently deprived from the standpoint of motor and visual experience.

As can be seen in Table 1.3, the animals that experienced the free environment early and the stovepipe later in life performed just as well as the animals that remained in the free environment throughout the experi-

TABLE 1.2
Errors in Hebb–Williams Maze of Rats Reared Under Four Different Conditions

Stovepipe	*Running Wheel*	*Mesh Cage*	*Free Environment*
233	235	140	137

Note. The Stovepipe and Running Wheel groups made significantly more errors than the other two groups. From "The Effects of Experimental Variations on Problem Solving in the Rat," by B. Hymovitch, 1952,*Journal of Comparative and Physiological Psychology, 45,* pp. 313–321. Copyright 1952 by the American Psychological Association. Adapted by permission.

TABLE 1.3
Errors in Hebb–Williams Maze of Rats with Different Early and Late Environmental Experiences

Free Environment/ Stovepipe	*Stovepipe/ Free Environment*	*Free Environment/ Free Environment*	*Normal Cage/ Normal Cage*
161	248	152	221

Note. The Stovepipe/Free Environment and Normal Cage groups made significantly more errors than the other two groups. From "The Effects of Experimental Variations on Problem Solving in the Rat," by B. Hymovitch, 1952, *Journal of Comparative and Physiological Psychology, 45,* pp. 313–321. Copyright 1952 by The American Psychological Association. Adapted by permission.

ment. The crucial finding is that the animals which experienced the stovepipe environment early and the free environment later in life performed as poorly as the animals that remained in their normal cages throughout the experiment (the most deprived group). It is important to note that these differences in problem-solving ability were not in evidence when Hymovitch challenged the rats with a simpler alley maze, more like the ones that were in wide use in most animal-learning laboratories at the time. It is only when they were challenged by the much more difficult Hebb–Williams series of problems that the differences in problem-solving ability were in evidence.

It was not long before these early-experience studies were extended to other animal species, including nonhuman primates, where social isolation and otherwise highly restricted, deprived rearing conditions were employed. Indeed, even in primates with relatively large brains, the normal or usual variety of experiences early in life was critical for the appearance of normal exploratory and learning abilities later in life. Deprived infants showed severe deficiencies in their later behavior (Harlow, Dodsworth, & Harlow, 1965). Just having a large brain is insufficient for the development and manifestation of the superior problem-solving skills characteristic of primates (Mason, 1968; Sackett, 1968).

Thus, behavioral plasticity that is essential to the genesis of new behavioral phenotypes is dependent on variations in early experience (as well as possessing a large brain, as reviewed earlier in Gottlieb, 1992, 1997). The conditions that favor the appearance of a behavioral neophenotype are severe or species-atypical alterations in environmental contingencies early in life (Kuo, 1976). These changed contingencies can arise in two ways in animals living in nature: (a) some sort of physical or geographical change happens to (is forced on) the animal (a disruption of habitat, climactic change, etc.) and, probably more frequently, (b) the animal migrates into a somewhat different habitat based on normal exploratory be-

havior. The animals that are more likely to withstand (a) and commit (b) are ones that have had not only traditional but nontraditional variations in early experience. To put it the other way around, exposure to conservative or narrow social and physical environmental contingencies early in life will make animals less likely to withstand (a) and unlikely to perpetrate (b). These predictions on evolutionary readiness, as it were, follow from the results of the early-experience studies reviewed previously.

THE REPETITION IN ADULTHOOD OF FAMILIARITY-INDUCING EARLY EXPERIENCE

As first recognized by psychoanalysts and called a "repetition compulsion" (review in Levy, 2000), there is a developmental dynamic that causes humans (and other animals) to prefer the familiar and thus to strive to reinstate earlier life situations or repeat versions of their early life experiences in adulthood, a psychological process akin to imprinting. Consequently, it could be that animals that had had considerable variation in their early social and physical experience will tend to seek out such variation in adulthood—just what is needed to heighten exploratory behavior and encourage novelty seeking! Although actual developmental experiments have not yet been done to show that animals (including humans) that had had considerable variation in their early experience will tend to seek out novel experiences as adults, there are two studies of adult mammals and birds that show that novelty is a psychological dimension of experience that can be abstracted such that animals so trained will consistently prefer to interact with novel rather than familiar objects or situations when given a choice (Honey, 1990; Macphail & Reilly, 1989). From the present theoretical perspective, it would be most valuable to validate the developmental induction of novelty-seeking behavior in later life through the experience of considerable variation early in life.[2]

In anticipation of the material reviewed in the next section, I wish to elaborate on the familiarity-inducing experiential mechanism described previously. It has long been recognized that there is a form of learning that takes place through mere exposure to physical and social objects; the exposure leads to subsequent attachment, preference, or acceptance of these objects (i.e., animals and humans not exposed to these objects do not show attachment, preference, or acceptance of them). This familiarity mechanism, termed *exposure learning* (Sluckin, 1965), is pervasive; it is found in many invertebrates as well as vertebrate species (Szentesi & Jermy, 1989),

[2]I have benefited from discussions of this idea with Professor Gerald Turkewitz.

and it is often referred to as imprinting, although, technically speaking, imprinting involves a sensitive period early in life and is usually defined as inducing an enduring preference, not merely acceptance (nonavoidance) of an object (or an experience). Though particularly striking in young animals (Sluckin, 1965), exposure learning operates throughout life in some species such as our own (Hebb, 1946; Rheingold, 1985; Zajonc, 1971). Psychologically speaking, exposure learning (the acquisition of familiarity) is above the level of the lowest forms of learning—habituation and sensitization—in that it requires a higher degree of perceptual differentiation and longer term memory. It would almost always be adaptive to form attachments, preferences, and acceptance of familiar physical and social objects.

AN EXAMPLE OF THE DEVELOPMENTAL-BEHAVIORAL BASIS OF EVOLUTION: INCIPIENT SPECIATION IN TWO VARIANTS OF APPLE MAGGOT FLY

Mayr (1963) wrote: "A shift into a new niche or adaptive zone is, almost without exception, initiated by a change in behavior. The other adaptations to the new niche, particularly the structural ones, are acquired secondarily.... This is not the place to discuss how the behavior changes themselves originate, a problem still poorly understood" (pp. 604–605).

In the preceding sections I have sketched a theory of how such behavioral changes could originate. An empirical example of incipient speciation mediated by a transgenerational, developmental change in behavior is provided by the apple maggot fly, *Rhagoletis pomonella* (Figs. 1.2 and 1.3). The original native (U.S.) host for the female apple maggot fly's egg laying was the hawthorn, a spring-flowering tree or shrub. Domestic apple trees were introduced into the United States in the 17th century. Haws and apple trees occur in the same locale. The first known infestation of apple trees by apple maggot flies was in the 1860s (Walsh, 1867). There are now two variants of *R. pomonella,* one that mates and lays its eggs on apples and the other that mates and lays its eggs on haws.

As shown in Table 1.4, the life cycles of the two variants are now desynchronized because of the fact that apples mature earlier than haws. Incipient speciation began, and likely has been maintained, by a trans-generational behavior: something akin to, but not as straightforward as, an imprinting-like olfactory preference (i.e., a familiarity-inducing early-rearing experience) for courting, mating, and ovipositing on the host in which the fly developed (reviews in Bush & Smith, 1998; Prokopy & Bush, 1993).

FIG. 1.2. Female apple maggot fly on apple. Courtesy of Ronald J. Prokopy. Reprinted with permission.

The cause of the original shift from hawthorns to apples as the host species for egg laying can only be speculated upon. Perhaps the hawthorn hosts became overburdened with infestations or, for other reasons, died out in a part of their range, bringing about a shift to apples on the part of a small segment of the ancestral hawthorn population that did not have such a well-developed olfactory sensitivity that would have led to an olfactory aversion to apples (see later discussion). This latter supposition is supported behaviorally as well as physiologically. In behavioral tests, the apple variant accepts both apples and haws as hosts, whereas in the haw variant only a small percentage will accept apples and most show a strong preference for haws (Luna & Prokopy, 1995; Prokopy, Diehl, & Cooley, 1988). To substantiate the olfactory basis of host selection, early studies showed that the flies are strongly attracted to specific odors emanating from the host fruits (Prokopy, Moericke, & Bush, 1973):

FIG. 1.3. Male apple maggot fly. Courtesy of Guy L. Bush. Reprinted with permission.

> Volatile esters isolated from whole fruit extracts have been shown by electroantennogram (EAG) assays and behavioral observations to be important in eliciting the response to fruits.... It is therefore clear that *R. pomonella* flies use chemical cues to find host fruits and that olfactory information transmitted from antennae play a significant role in this process. (Frey, Feder, Palma, & Bush, 1998, p. 176)

The original shift from haws to apples, presumably by a small number of less sensitive individuals, fits nicely with the current finding that the apple variant is less sensitive to odor differences than is the haw variant (Frey et al., 1998). Further:

> Within the volatile region found to be attractive to *R. pomonella* by Fein et al. (1982), the chemical profile of apples and hawthorns are fairly similar, which could have facilitated the original shift of the fly to apple (Carlé et al., 1987). Nevertheless, certain chemical differences do exist between the two fruits (Carlé et al., 1987), affording the possibility that the fly races are exploiting these differences when deciding between fruits. (Frey et al., 1998, p. 176)

Given the "repetition-compulsion" or exposure learning mechanism described earlier, in which adults reinstate familiar sensory stimulative,

TABLE 1.4
An Example of the Developmental-Behavioral Basis of Evolution: Incipient Speciation in Two Variants of Apple Maggot (Fruit) Fly *(Rhagoletis pomonella)*

Time	*Apple Host*	*Hawthorn Host*
Year 1	Eggs Laid	Eggs Laid
	↓ Fruit matures earlier than haw	↓ Fruit matures later than apple
Year 2	Hatch Late Summer	Hatch Early Fall
	↓ 5-12 days	↓ 5-12 days
	OFFSPRING court and mate on or near host, and female lays eggs on same host	OFFSPRING court and mate on or near host, and female lays egg on same host
	↓	↓
Year 3	Cycle Repeats	Cycle Repeats

Note. Abstracted from Bush and Smith, 1998; Prokopy and Bush, 1993.

perceptual, and/or cognitive features of their early experience—a familiarity reinstatement process—in the first generation, some portion of the male and female offspring of the original apple flies, if not all, would accept apples for courtship, mating, and egg laying, and thus, through this developmental-behavioral means perpetuate the initiation of the potential evolution of a new species of *R. pomonella.* Admittedly, we can not know in this case the factors that contributed to the original selection of the apple host in generation 0; otherwise, the present information conforms to the developmental-behavioral evolutionary hypothesis advocated in this essay.

The reason for saying that the imprinting-like olfactory preference is not straightforward is that the behavioral experiments done so far indicate that rearing on the apple host does not actually create a preference for the apple host, but, rather, the exposure learning renders the apple host more acceptable (less aversive) to the apple variants, so they will accept both apples and haws as hosts when the opportunity presents itself in laboratory and field experiments that manipulate the environment in such a way as to bring about a "choice" test (Luna & Prokopy, 1995; Prokopy et al., 1988). So, it is the familiarity-inducing (exposure-learning) rearing experience that sustains the choice of the apple as host in the apple variant. A similar hypothesis was put forth by Frey et al. (1998), although they believed the rearing experience induces a preference whereas I think the existing data support an acceptance of, rather than a preference for, the apple host.

In a seminaturalistic experiment, Luna and Prokopy (1995) created four separate patches of trees with either apple or hawthorn fruits and released either hawthorn-reared or apple-reared flies into one of the patches. This is an acceptance test rather than a preference test, because only one fruit was available in each patch, as would be the case in nature. The apple-reared variants did not differ from the hawthorn-reared variants in the number of eggs layed in the hawthorn patches. However, in the apple patches, the hawthorn-reared variants layed only 20% as many eggs as the apple-reared variants. This result is significant in that it shows not only that the apple-reared variants have a greater acceptance of apples but that a small percentage of the hawthorn-reared flies will accept an apple host as must have been the case when apples were noticeably infested for the first time in the mid-1800s.

Most of the behavioral experiments with the apple maggot fly have been done with females. In a behavioral experiment with males, Prokopy et al. (1988) found that, in an acceptance test, young apple-reared males remained significantly longer on apples than did young hawthorn-reared males, thus supporting the field observations that apple-reared males stay around apple trees and are therefore likely to court and mate with apple-reared females (Prokopy & Bush, 1993).

Further evidence that the rearing experience of the apple variants makes them more accepting of the apple host—but does not actually instill a preference for apple over hawthorn—comes from experiments in which apple-reared and hawthorn-reared females were given acceptance tests (one host present) and true preference tests (both hosts present), and their egg-laying behavior was tallied (Prokopy et al., 1988). In both kinds of tests, a greater percentage of the flies displayed egg-laying behavior (i.e., boring their ovipositor into the fruit) in the hawthorn host, irrespective of their rearing experience. So, clearly, the apple-reared flies do not have a preference for the apple over the hawthorn host, as would be the case if they were truly imprinted by their rearing experience. Rather, as indicated in the single-host acceptance tests, the apple-reared flies show a greater percentage of egg-laying behavior on the apple host than do the hawthorn-reared flies. Thus, the familiarity-inducing rearing experience (exposure learning) makes the apple-reared flies more accepting of the apple host, although they still maintain a preference for the hawthorn host. Their actual preference for the hawthorn host is shown in the tests with both hosts present: They show as high a percentage of egg-laying behavior on the hawthorn host as do the hawthorn-reared flies, and the same lower percentage of egg-laying behavior on the apple host as do the hawthorn-reared flies, regardless of whether they visited only one or both hosts. The fact that the rearing experience of the apple flies does not truly imprint an olfactory preference but, rather, extends the range of accept-

able odors was also found in experiments with fruit flies *(Drosophila* species). In that study, once again, rather than the early experience of a novel odor creating a preference for that odor, the early experience merely reduced the level of later behavioral avoidance of the odor (i.e., made it more acceptable; Manning, 1967).

So, for terminological correctness, the acceptance of a wider range of odors of potential hosts induced by the apple-rearing experience should not be considered an instance of olfactory imprinting but, rather, the induction of olfactory familiarity through exposure learning that increases the likelihood of acceptance of the apple host in adulthood. Given the ecological circumstances, the increased likelihood of acceptance of the apple host, even in the face of a preference for hawthorn, would work just fine to perpetuate the transgenerational courting, mating, and laying of eggs in apple orchards. As described by Prokopy and Bush (1993), apple maggot flies hatch out at the base of the tree in which their mother had layed their egg the previous summer. While becoming sexually mature in the next 2 weeks, they wander around the vicinity of the apple orchard to for-age for food. When they become sexually mature, even though they have wandered tens or hundreds of yards, they are still in the vicinity of the apple orchard, if not still in the orchard. The scent of the apples attracts them, and the early-rearing experience having rendered the apple scent acceptable, the cycle renews itself, because of the high probability that the early-maturing apple maggot fly will encounter the odor of apples rather than hawthorns (see Table 1.4 for the desynchronized life cycles of the apple and hawthorn variants).[3] The two variants are now genetically somewhat distinct, and do not interbreed freely in nature although they are morphologically the same and remain interfertile.[4]

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[3]Another reason for emphasizing that the early experience of the apple maggot fly merely influences the olfactory acceptability of hosts for oviposition rather than imprinting a preference is that the adult fly continues to be open to experience. Namely, the egg-laying experience of adult apple maggot flies influences their preference for familiar perceptual features (e.g., visual) of the host on which they have already laid some eggs (reviewed by Prokopy & Papaj, 2000, pp. 239–240). (This species lays one egg at a time.) The mature fly's openness to perceptual experience beyond olfaction would thus reenforce (extend) the olfactory influence stemming from the larval feeding experience by building other perceptual features on it in adulthood. Thus, in this species, the exposure-learning, familiarity-inducing perceptual mechanism continues to operate in adulthood, embracing other senses besides olfaction. The early olfactory influence orients the mature fly to prospective egg-laying sites and the visual experience consolidates the egg-laying preference.

[4]In order not to digress from the main point of this essay, I have not discussed the controversial subject of allopatric (geographical) versus sympatric (nongeographical) speciation. *R. pomonella* is an incipient example of sympatric speciation (Bush & Smith, 1998). An insightful review of the allopatric-sympatric controversy, and especially the different roles ascribed to behavior in the two views, was provided by Kremencov and Georgievskij (1987).

In contrast to the transgenerational behavioral scenario being put forward here, as mentioned in the introduction, conventional evolutionary-biological thinking would hold that "most likely some mutations in genes coding for larval/pupal development and adult emergence" brought about the original divergence and maintains the difference in the two populations (Ronald Prokopy, personal communication, August 2000). Although it is not something anyone can know with certainty in this case, present evidence (discussed later) would suggest that a genetic mutation was not necessary. This is not a behavior versus genes argument because the trans-generational behavioral initiation requires genetic compatibility, otherwise it would not work. The question is whether the original initiation (switch to the apple host) required a genetic mutation or not. The developmental timing change in the life histories of the two *R. pomonella* forms (described in Table 1.4) has resulted in correlated genetic changes in the two populations (allele frequency differences: Feder, Roethele, Wlazlo, & Berlocher, 1997). That finding is consonant with the evolutionary model presented here (i.e., gene frequencies change sometime *after* the behavioral switch). From the present point of view, another significant feature of the findings of Feder et al. is that, when immature hawthorn flies (pupae) are subjected to the prewintering environment of the apple flies (pupae), those that survive have a genetic makeup that is similar to the apple flies. Most important, this result shows that there is still sufficient individual developmental-genetic variability in the hawthorn population, even at this late date, to support a sheerly transgenerational behavioral initiation of the switch from hawthorns to apples without the necessity of a genetic mutation.

To summarize, a behavioral change involving the apple maggot fly's choice of oviposition site gets it into a situation where it must be able to withstand certain prewintering low temperatures for given periods of time (that differ between the apple and hawthorn forms [Table 1.4]). This situation sets up the natural selection scenario that brings about changes in gene frequencies that are correlated with the prewintering temperature regimen, as demonstrated in the Feder et al. (1997) experiments. So it is the change in egg-laying behavior that leads the way to genetic change, the genetic change thus being a consequence of the change in behavior, as advocated in the present theory.

CONCLUSION

Thus, in conclusion, evidence has been presented to show that the first stage in the pathway to evolution is a change in ontogenetic development that results in a novel behavioral shift (a new behavioral phenotype) that recurs across generations, encouraging new environmental relationships.

In our hypothesized second stage in the evolutionary pathway, the new environmental relationships can bring out latent possibilities for anatomical/physiological change. Somatic mutations or changes in genetic regulation may also occur in this stage, but a change in structural genes need not occur at this stage. A change in genes may occur in the third stage of the evolutionary pathway, resulting from long-term geographic or behavioral isolation (separate breeding populations). It is important to observe that, in this theory, evolution has already occurred phenotypically at the behavioral, anatomical, and physiological levels before the third stage is reached.

Thus, new variations and adaptations arise before they are selected for and are, therefore, not a consequence of natural selection as Buss et al. (1998) and other authors have claimed. New variations and adaptations are a consequence of changes in individual development mediated by transgenerationally persistent changes. In this view, natural selection is not the cause of the new adaptations but acts only as a filter through which the new adaptations must pass. Changes in behavior create the new variants on which natural selection works. As noted earlier, "Natural selection cannot explain the origin of new variants and adaptations, only their spread" (Endler, 1986, p. 51). And, as Ernst Mayr remarked in 1963, novel behavioral shifts antedate the anatomical changes, which arise secondarily, in the evolution of new species. The contribution of the present theory is to offer a developmental scenario to bring about the novel changes in behavior and an explanation of how such changes can be maintained across generations without any initial change in genotype. As noted in the introduction, this theory is offered in the spirit of evolutionary pluralism: There is not only one evolutionary pathway.

PAST, PRESENT, AND FUTURE PROSPECTS FOR THE ROLE OF BEHAVIORAL DEVELOPMENT IN EVOLUTION

The usual genocentric natural selection view of evolution ignores the supragenetic origin of new variants and adaptations. Though sometimes recognizing that individual development needs to be taken into account in the evolutionary scenario, there is resistance to accepting evolutionary pluralism, in the sense of alternative evolutionary pathways that do not involve genetic origination, the mainstay of the "modern synthesis" (Futuyama, 1988; Løvtrup, 1987). Even Darwin himself was an evolutionary pluralist: "I am convinced that Natural Selection has been the main, but not exclusive, means of modification" (Darwin, 1859, p. 6). In the spirit of evolutionary pluralism, in recent years in the field of biology there have appeared a small number of individuals who have been calling for a more developmentally based view of evolution (e.g., individual essays in Ho & Saunders', 1984, *Beyond Neo-Darwinism).* Even more recently, within the

Society for Integrative and Comparative Biology, formerly the American Society of Zoologists, a group has emerged with the label "evo-devo" (Division of Evolutionary and Developmental Biology) to distinguish themselves from the more popular neo-Darwinian perspective ("evo-populo"). In 1999, they even started two new journals, *Evolution and Development* and *Molecular and Developmental Evolution.* So there is a developmental-evolutionary movement of sorts in biology proper, although still a minority voice, to be sure. The reason I say "of sorts" is that there is an important if subtle difference between advocating that modes of development evolve versus seeing changes in development as the cause or basis of evolution itself. Many of the evo-devo advocates are of the former persuasion.

With regard to the acceptance of the role of behavior in evolution (in the sense of seeing behavior as the leading edge of evolution), that perspective has a following beyond the more than 30 authors who contributed to the volume edited by Leonovičová and Novák (1987), *Behavior as One of the Main Factors of Evolution.* Along with Ernst Mayr (1963), a number of biologists and a few psychologists have recognized that novel shifts in behavior are involved in speciation (e.g., Bonner, 1983; Hardy, 1965; Larson, Prager, & Wilson, 1984; Parsons, 1981; Piaget, 1978; Plotkin, 1988; Reid, 1985; Sewertzoff, 1929; Wyles, Kunkel, & Wilson, 1983). But what has not been recognized (beyond genetic mutation, recombination, or drift) is the supragenetic developmental conditions that allow, prepare, or dispose animals to make novel shifts in behavior. The "mechanisms" I described earlier (handling, enrichment, exposure learning) are the means that have already been experimentally delineated; obviously, there are likely other similar developmental mechanisms that have not yet been recognized. The developmental causes of novel behavior have been too little explored—perhaps it is not too much to hope that will change in the future, now that there is a theoretical rationale.

ACKNOWLEDGMENTS

Research and scholarly activities connected with this article were supported in part by National Institutes of Mental Health Grant MH-52429. I am indebted to Daniel Bauer for directing my attention to research on *Rhagoletis pomonella* and its fit with my developmental-behavioral theory of evolutionary initiation. I appreciate the comments of Jaan Valsiner, Tom Cadwallader, and Derrick Nehrenberg on the first version of the manuscript, and Ronald J. Prokopy on several later versions. A portion of the author's article, "Developmental-Behavioral Initiation of Evolutionary Change," *Psychological Review* (in press), is reprinted here with the permission of the American Psychological Association.

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2

How a Child Builds a Brain: Insights From Normality and Psychopathology

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Throughout the past several decades, scientists concerned with elucidating the pathways to normal and atypical development have focused increasing attention on the processes and mechanisms contributing to the genesis and epigenesis of brain structure, function, and organization (see, e.g., Charney, Nestler, & Bunney, 1999; Ciaranello et al., 1995; Cicchetti & Cannon, 1999; Cicchetti & Tucker, 1994b; Dawson & Fischer, 1994; Greenough, Black, & Wallace, 1987; Keshavan & Murray, 1997; Mednick, Cannon, Barr, & Lyon, 1991; Sapolsky, 1992; Tager-Flusberg, 1999). Drawing on historical and contemporary conceptualizations, in this chapter I strive to provide a multilevel, integrative perspective on brain development. Toward this end, the influences of genetic, experiential, and neural and psychological self-organization are highlighted. In accord with a developmental psychopathology approach, exemplars from both normal and atypical populations are provided.

Given the important occasion of the 75th anniversary of the University of Minnesota's Institute of Child Development, I begin by providing a brief historical perspective on the ways in which brain-behavior relations have been viewed in the field of developmental psychology. I then examine some of the changing theoretical perspectives, empirical discoveries, and technological advances that have played a major role in rejuvenating scientific interest in investigating the relations between neurobiological and behavioral development in normal persons and in individuals who are at "high risk" for, or who have, mental disorders. Next I provide a his-

torical and present-day perspective on normal brain development in humans. In the penultimate section of the chapter, schizophrenic disorders are used to elucidate insights that can be gained on normal brain development from psychopathology, as well as to illustrate how knowledge of normal neural development can provide important insights into the genesis and epigenesis of this serious mental disorder. Finally, I conclude by proffering future interdisciplinary research directions that I believe are important to ensure that the exciting collaborations among neuroscientists, developmental psychologists, and developmental psychopathologists will continue to flourish.

HISTORICAL PERSPECTIVES ON BRAIN-BEHAVIOR RELATIONS IN DEVELOPMENTAL PSYCHOLOGY: SOME REFLECTIONS

Although influential developmentalists such as Wilhelm Preyer (1888–1889) and Leonard Carmichael (1933, 1946, 1970) published major works detailing the unsolved problems in comprehending the relations between biological and behavioral development, and early editions of the *Handbook of Child Psychology* each provided scholarly reviews of the relation between biological (including the brain) and behavioral development (see, e.g., Carmichael, 1933, 1946, 1970; Gesell, 1933; McGraw, 1946; Tanner, 1970), the epigenesis of neurobiological development was accorded little attention in the prominent developmental theories in existence through much of the 20th century (Crnic & Pennington, 1987; Fishbein, 1976; Goldman-Rakic, 1987; Johnson, 1998; Segalowitz, 1994). Undoubtedly, the relative neglect of developmental neurobiology as relevant to developmental theorizing on the unfolding of behavioral epigenesis was due, in part, to the paucity of information that existed about the structural and functional organization of the brain. Writing in the late 19th century, both James Mark Baldwin (1895) and Sigmund Freud (1895/1966) conveyed a great interest in the phylogenesis of the brain, but expressed surprisingly little concern with its ontogenesis (Segalowitz, 1994).

In the introduction to his book, *Mental Development in the Child and the Race,* Baldwin (1895) articulated that the major goal of this work was to outline a system of genetic (i.e., developmental) psychology that would achieve a "synthesis of the current biological theory of organic adaptation with the doctrine of the infant's development" (p. vii). However, Baldwin asserted that much more knowledge about the process of brain development needed to be discovered before the study of child development and neurobiological growth could be integrated in a manner commensurate with the complexity of the developing child and nervous system.

Similar to Baldwin's belief on brain-behavior relations, in 1894 Freud stated that biological knowledge had not progressed far enough to prove

helpful to psychoanalysis (Kandel, 1999). However, in his developmental theory of psychoanalysis, Freud was deeply aware that advances in biology would likely make critical contributions to the comprehension of mental processes. For example, in *On Narcissism,* Freud (1914) expressed the viewpoint that the entirety of our provisional ideas in psychology would one day be based on an organic substructure. Relatedly, in *Beyond the Pleasure Principle,* Freud (1920) stated that the deficiencies in our understanding would probably disappear if we were currently in a position to replace psychological terms with physiological or chemical ones.

During the 1920s and 1930s, an integration of developmental psychology with the study of brain maturation appeared to be on the verge of flourishing. Leonard Carmichael (1926, 1933) and Arnold Gesell (1929, 1933) conjectured that the physiological maturation of the brain was responsible for the development of some of the skills that emerge during childhood. Jean Piaget, too, was intrigued with the theoretical implications that brain phylogenesis might hold for child development (Segalowitz, 1994); however, like Baldwin and Freud before him, Piaget believed that a neuropsychological theory of development was not possible at the time because the field did not possess enough knowledge about brain development and brain function to articulate their role in the genesis and epigenesis of mental processes (Piaget, 1947/1966; Piaget & Inhelder, 1966/1969).

Since the time of Myrtle McGraw (1932), there has been a tradition in the field of human development to seek explanation at the neural level—that is, to discover an observed change in behavior and attribute its emergence as being the result of a preceding change in the brain. Historically, the appearance of isolated anatomic findings, such as the presence of myelin and neuronal distribution in various areas of the brain, has been used to explain the emergence of behavior (Conel, 1939–1967; Parmalee & Sigman, 1983; Yakovlev & LeCours, 1967). This point of view exists in some contemporary circles. For example, although their hypothesis is now discounted, as recently as a decade ago Diamond and Goldman-Rakic (1989) suggested that the massive reorganization of synaptic connections in the prefrontal cortex caused the improvements in spatial cognition, the onset of language, and the development of inhibition of prepotent response tendencies displayed by 8- to 12-month-old infants. Likewise, Thatcher (1991, 1992) has contended that Piagetian cognitive stages develop as a result of stagelike changes in brain activity.

McGraw (1932, 1946), a contemporary of Carmichael and Gesell, urged the field to synthesize the extant body of knowledge of brain growth and psychological experience into a comprehensive theory of development. Segalowitz (1994) noted that McGraw was especially intrigued with the work of Max de Crinis, published in 1932, that mapped out histologically the cortical areas in humans where the growth of dendrites matured ear-

lier (e.g., sensory and motor areas) versus later (e.g., higher order functions). De Crinis believed that his findings on the histology of dendrites across cortical areas reflected functional brain maturation and not merely physical growth, a speculation that was congruent with an earlier contention in the field that myelination occurred first in sensory and motor areas and last in multimodal areas of integrative functions (Fleschig, 1876; Langworthy, 1929, 1933; see also Sidman & Rakic, 1982). However, in a manner reminiscent of Santiago Ramón y Cajal's notions of an "histology of the mind" put forth in the late 19th century, de Crinis proffered a reductionistic interpretation of his findings on dendritic maturation in various cortical areas (Segalowitz, 1994). Experience was not considered to be capable of exerting an impact on brain development. Because the major developmental theories in the United States were predominantly reductionistic in nature at the time of McGraw, the efforts of the systematizers who wanted to integrate brain maturation into our knowledge base of children's functioning over time were stymied for nearly a quarter of a century.

Several important works during the 1960s exerted a catalytic role in rejuvenating interest in brain maturation for developmental psychologists. These include, perhaps most notably, Eric Lenneberg's (1967) book, *The Biological Foundations of Language*. Lenneberg sought to answer questions about sensitive periods and brain plasticity that, although focused on the domain of language (e.g., Can the left hemisphere acquire language for the first time as late as adulthood?), exerted a major impact on developmentalists interested in investigating similar issues of sensitive periods and brain plasticity in other domains.

Despite the fact that our understanding of the nature of the relations between neurobiological and behavioral development across the lifespan in normal and atypical populations remains in a relatively primitive state, our understanding of brain development and function have advanced greatly in recent decades. Researchers in the fields of developmental neuroscience, developmental psychology, and developmental psychopathology are in a position to usher in an exciting new era of discovery that will greatly enhance our knowledge of brain–behavior relations (cf. Cicchetti & Tucker, 1994a; Nelson & Bloom, 1997).

THEORETICAL INFLUENCES, EMPIRICAL CONTRIBUTIONS, AND TECHNOLOGICAL ADVANCES IN THE STUDY OF BRAIN–BEHAVIOR RELATIONS

In present-day neuroscience, information in the brain is depicted as being represented and processed by distributed groups of neurons that maintain a functional interconnection based on experiential demands rather than

by a strictly predetermined scheme (Black & Greenough, 1992; Changeaux, 1985; Ciaranello et al., 1995; Courchesne, Chisum, & Townsend, 1994; Johnson, 1998). Because levels of organization and processes are reciprocally interactive, it is difficult, if not impossible, to impute ultimate causation to one level of organization over another.

In recent decades, it has become increasingly recognized that the investigation of developmental processes, normal and abnormal, is an inherently interdisciplinary enterprise (Cicchetti, 1984, 1990, 1993; Gottlieb, Wahlsten, & Lickliter, 1998; Thelen & Smith, 1998). Different levels and methods of analysis must be employed (e.g., molecular, cellular, behavioral, and macro-system levels) depending on the question being addressed and the developmental period being inspected (Gottlieb, 1992; Johnson, 1998; Nelson & Bloom, 1997). Neuroscientists increasingly have changed their emphasis from investigating molecules, membranes, and single neurons and tracts to examining complex neural systems (Crick, 1994; Damasio, 1994; Edelman, 1987; Thelen & Smith, 1998). In these more contemporary theoretical conceptualizations of brain–behavior relations, the brain is viewed as operating in a plastic and dynamic, self-organizing fashion, and as being less constrained by predetermined "localized" boundaries than previously thought (Cicchetti & Tucker, 1994a; Finger, 1994).

The viewpoint that the nervous system is dynamic is not exclusively a product of modern neuroscientific principles. For example, in *The Brain of the Tiger Salamander,* C. J. Herrick (1948) restated the point that he had made in several prior publications spanning from 1908 through 1933—namely, that in all phylogenetic investigations of morphogenesis it is important to keep in mind the conservative factor of stable genetic organization and the more labile influence of the functional requirements. Herrick contended that any morphology that overlooked the dynamic factors of tissue differentiation in terms of physiological adaptiveness lacks something and is sterile.

Systems-theory viewpoints have historical roots in the investigations of a number of eminent developmental psychobiologists whose work took place in the 1930s through the 1960s (see, e.g., Kuo, 1967; Lehrman, 1953; Schneirla, 1957). In contemporary developmental psychobiology, the system viewpoint is represented most elegantly in the writings of Gilbert Gottlieb (1983, 1992; Gottlieb et al., 1998).

Gottlieb (1991, 1992) has enunciated the viewpoint that the activity of the genes themselves can be affected through the cytoplasm of the cell by events at all system levels (i.e., genetic, neural, and behavioral *and* internal and external physical, social, and cultural environmental components). Gottlieb articulated the view that a genome is a part of an organism's general developmental-physiological adaptation to environmental stresses and signals. This viewpoint differs from that of Ernst Mayr's (1982), who

contended that "the DNA of the genotype does not itself enter into the developmental pathway but simply serves as a set of instructions" (p. 824).

In addition to the historical and contemporary contributions made to systems theorizing by developmental psychobiologists, several major scientific figures have advanced the major principles of the systems viewpoint. From his earliest writings in the 1930s until the publication of his classic work, *General System Theory,* in 1968, Ludwig von Bertalanffy played a dominant role in spearheading an antireductionist view of biological systems. Instead of isolating systems into increasingly small elements, von Bertalanffy urged scientists to examine the interrelations among these separate parts. Unless this shift in emphasis occurred, von Bertalanffy believed that it would never be possible to truly understand the complexity of the whole organism.

Von Bertalanffy (1968) developed a set of principles that he thought characterized all living systems. Included among von Bertalanffy's principles were:

1. Organisms are active, open systems.
2. Organisms exist in a state of disequilibrium (i.e., dynamic stability) and actively participate in and seek out stimulation.
3. Organisms are systems of wholeness and order, whereby a whole adds the property of relations to the existing parts.
4. Organisms are characterized by equifinality and self-stabilization, a self-regulatory process that allows the system to resist perturbations.
5. Organisms have a hierarchical integration and organization whereby systems at each level of a hierarchy do not exert unidirectional control over systems at lower levels. Rather, there is dual control between higher and lower systems.
6. Organisms are capable of self-organization, a reorganization that alters a system in an adaptive fashion when it is subject to new constraints.

These principles have exerted a major impact on contemporary thinking in developmental psychology (see, e.g., Ford & Lerner, 1992; Lewis & Granic, 2000; Sameroff, 1983; Wapner & Demick, 1998) and in developmental psychopathology (see, e.g., Cicchetti & Rogosch, 1996; Cicchetti & Tucker, 1994a; Sameroff, 1995). For example, many developmental theories contend that disequilibrium enables individuals to exhibit change and flexibility throughout epigenesis (Piaget, 1971; Werner, 1957).

As Ilya Prigogine (1978), a preeminent contributor to systems theory, noted, nonequilibrated systems assume a number of special properties, including the ability to self-organize into patterns and nonlinearity or sen-

sitivity to initial conditions. Cicchetti and Tucker (1994a) suggested that the concept of self-organization may serve as one of the processes whereby individuals function competently despite experiencing great adversity (i.e., resilience—see Luthar, Cicchetti, & Becker, 2000) and act as one of the mediators of brain plasticity across the life course. Arnold Sameroff (1983, 1995), a leading contemporary proponent of systems approaches to normal and atypical development, has asserted that context is always crucial to consider because, inherent to the concept of an open system, individuals are constantly transacting with the environment. Thus, biological vulnerability or risk do not exist in a vacuum, but are in transaction with aspects of the external environment (Sameroff & Chandler, 1975; see also Boyce et al., 1998; Cicchetti & Aber, 1998).

One outgrowth of systems theorizing has been a growing acceptance of the viewpoint that neurobiological development and experience are mutually influencing (Cicchetti & Tucker, 1994a; Eisenberg, 1995; Kandel, 1998; Nelson & Bloom, 1997). For example, it has been demonstrated that, just as gene expression alters social behavior (Young, Nilson, Waymore, MacGregor, & Insel, 1999), social experience exerts actions on the brain by feeding back upon it to modify gene expression and brain structure, function, and organization (Francis, DiOrio, Liu, & Meaney, 1999; Kandel, 1998). The idea that experience can modify brain structure can be traced back at least to the writings of Ramon y Cajal (see Ramon y Cajal & May, 1959) who, despite his belief that the connections that occurred between neurons unfolded according to a definite plan (i.e., the principle of connection specificity), asserted that the strength and effectiveness of these neuronal connections were not predetermined and that they could be altered by experience (Kandel & Squire, 2000). Likewise, D. O. Hebb (1949) believed that experience could alter brain structure and function and made this viewpoint a central feature of his neuropsychological theory.

In contemporary perspectives, experience is broadly construed to include not only external social and psychological events, but also, for example, internal events such as the effects of psychopathology, trauma, abuse, or injury, the actions of hormones, and the consequences of development and aging. Furthermore, it has been shown that alterations in gene expression induced by learning and by social and psychological experiences produce changes in patterns of neuronal and synaptic connections and thus in the function of nerve cells (Kandel, 1998, 1999; Post, Weiss, & Leverich, 1994). These modifications not only contribute to the biological basis of individuality, but also exert a prominent role in initiating and maintaining the behavioral anomalies that are provoked by social and psychological experiences (Kandel, 1998).

In addition to the changing theoretical perspectives that have occurred, recent decades have been characterized by increased interaction among

scientists from multiple disciplines. Two fields that epitomize the movement toward interdisciplinary approaches to the study of brain–behavior relations are neuroscience and developmental psychopathology.

Cowan, Harter, and Kandel (2000) and Kandel and Squire (2000) have noted the unprecedented growth and achievements in the fields of neuroanatomy, neurochemistry, and neurophysiology that have taken place over the past several decades. Despite the successes of research in these isolated areas, the current excitement engendered by neurobiological research stems from the integration into one intellectual framework of several previously independent disciplines into the interdisciplinary framework known as neuroscience (Cowan et al., 2000; Kandel & Squire, 2000).

Cowan and colleagues (2000) described the historical roots, as well as three 20th-century phases of growth, for neuroscience. In the latter part of the 19th and the first decades of the 20th centuries, a number of landmark discoveries occurred, each of which made a significant contribution to one or another of the long established disciplines of neurophysiology or neuroanatomy. However, Cowan et al. noted that none of these discoveries transcended traditional disciplinary boundaries, the defining feature of the current-day field of neuroscience.

Kandel and Squire (2000) concluded that the modern cellular science of the nervous system was based on two fundamental discoveries: the neuron doctrine and the ionic hypothesis. Wilhelm His' description of the axon as an outgrowth from the immature nerve cell was an important step toward the formulation of the neuron doctrine. Evidence revealing that there was a discontinuity from neuron to neuron emanated from four scientific areas—embryology, histology, physiology, and pathological anatomy. The demonstration by the Spanish neuroscientist Ramon y Cajal that nerve fibers have terminal structures that contact with other nerve cells but do not fuse with them—that they are contiguous rather than continuous—provided critical support for neuronal development. Ramon y Cajal established the neuron doctrine after demonstrating that the brain was composed of discrete cells called neurons that were thought to serve as elementary signaling units. In Ramon y Cajal's time, investigations on neurogenesis were conducted in the field of histology. In contemporary neuroscience, the focus is on the molecular and cellular mechanisms involved in neuronal development. The ionic hypothesis, proffered by Alan Hodgkin, Andrew Huxley, and Bernard Katz in the late 1940s, explained the resting and action potentials of nerve cells in terms of movement of specific ions, thereby enabling the nervous system to be comprehended in terms of physiochemical principles common to all of cell biology (Kandel & Squire, 2000).

The 1950s and 1960s witnessed the integration of neuroanatomy, neuropharmacology, neurochemistry, and behavioral science into neuroscience (Cowan et al., 2000). In the early 1980s, neuroscience integrated with other

areas of biology—in particular molecular biology and molecular genetics. The emergence of molecular neuroscience enabled the field of neuroscience to surmount the intellectual barricades that had separated the study of brain processes, couched firmly in neuroanatomy and electrophysiology, from the remainder of the biological sciences, based more in biochemistry and cell and molecular biology (Alberts et al., 1994; Cowan et al., 2000; Kandel & Squire, 2000). Kandel and Squire concluded that the modern molecular era of developmental neuroscience began in 1956 when Levi-Montalceni and Cohen isolated nerve growth factor (NGF), the first peptide growth factor to be discovered in the nervous system. The third phase in the evolution of neuroscience as a discipline was marked by its incorporation of the methods of cognitive psychology, thereby bringing together the investigation of mental activity with the biology of the brain (see Kandel & Squire, 2000; Nelson & Bloom, 1997; Posner & DiGirolamo, 2000).

In a similar fashion, Cicchetti (1984, 1990) has described developmental psychopathology as a new discipline that is the product of an integration of various disciplines (e.g., genetics, embryology, neuroscience, sociology, psychiatry, psychoanalysis, and clinical, developmental, and experimental psychology), the efforts of which had previously been separate and distinct. Because one of the principal tenets of the field of developmental psychopathology is that the study of normality and pathology are mutually influencing, the discipline of developmental psychology has played a prominent role in the growth of developmental psychopathology. The integration of the study of normal and atypical ontogenetic processes within a developmental psychopathology framework has resulted in dramatic knowledge gains in the multiple domains of child and adult psychological and neurobiological development (see Cicchetti & Sroufe, 2000a, 2000b).

Moreover, national conferences on graduate training in applied developmental psychology have advocated an integration of perspectives from pertinent biological, social, and behavioral sciences (Fisher et al., 1993). Relatedly, discussions of training in developmental psychopathology similarly have called for a broad-based, multi- and interdisciplinary approach (Cicchetti & Toth, 1991). Increasingly, there are encouraging signs that these pleas for interdisciplinary integration have not gone unheard by developmental psychologists (see, e.g., Johnson, 1998; Nelson, 2000a; Sameroff, 1995).

The existing empirical contributions of molecular genetics to neurologic disease, which for the first time enable us to comprehend the genetic basis of certain diseases without requiring foreknowledge of the underlying biochemical abnormalities (Ciaranello et al., 1995; Lander & Weinberg, 2000), make developmental mechanisms more accessible than ever before and offer the hope not only of understanding normal development, but also of intervening to prevent or ameliorate genetic disorders (cf. Shep-

herd, 1994). Moreover, advances in molecular biology and molecular genetics have helped to engender renewed interest in the potential contributing role that the field of molecular genetics can make to unraveling the complex pathways underlying the development of psychopathological disorders (Brodsky & Lombroso, 1998; Hyman & Nestler, 1993; Plomin & Crabbe, 2000; Plomin & Rutter, 1998; Rutter, Silberg, O'Connor, & Simonoff, 1999a, 1999b).

There also has been a veritable explosion in our knowledge of developmental and molecular neurobiology, the areas of neuroscience that place their emphasis on factors regulating the development of neurons, neural circuitry, and complex neuronal organization systems, including the brain (Cowan, Jessell, & Zipursky, 1997; Jessell & Sanes, 2000; Kandel, Schwartz, & Jessell, 2000; Nowakowski, 1987; Rakic, 1988a, 1988b; Sidman & Rakic, 1982; Zigmond, Bloom, Landis, Roberts, & Squire, 1999). For example, early gene markers have been identified that specify which neurons emerge earliest from the proliferative zone, taking up permanent location in the deepest layers of the cortex, and that designate whether these differentiating neurons migrate to cortical or subcortical regions of the central nervous system (CNS; Ciaranello et al., 1995; Nelson & Bloom, 1997; Rakic, 1996).

Finally, there has been a rapid growth in sophisticated technology that enables the anatomical and physiological imaging of the brain (Andreasen, 1989; Ernst & Rumsey, 2000; Nelson & Bloom, 1997; Stark & Bradley, 1999; Thatcher, Hallett, Zeffiro, John, & Huerta, 1994; Thatcher, Lyon, Rumsey, & Krasnegor, 1996; Toga & Mazziotta, 1996). Diverse information about the brain is provided by these various neuroimaging techniques, including: brain metabolic processes, such as cerebral blood flow and blood volume, and glucose metabolic rate; the capacity to distinguish among cerebrospinal fluid (CSF) and white and gray matter; and the ability to detect biochemical changes within brain cells, such as alterations in neurotransmitter receptors. These new neuroimaging methods have been used by neuroscientists to enhance our understanding of normal and abnormal neurobiological development (Giedd, Snell et al., 1996; Giedd, Vaituzis et al., 1996; Jernigan & Tallal, 1990) and to augment our knowledge concerning the processes and pathways linking neurodevelopmental anomalies and later disordered outcomes (Cicchetti & Cannon, 1999; Hendren, Backer, & Pandina, 2000; Keshavan & Murray, 1997; Peterson, 1995).

However, despite these impressive technological advances, it is important to heed the cautionary words of Huttenlocher (1990) when interpreting findings from structural magnetic resonance imaging investigations: "Development is an ongoing dynamic process. The anatomical method is a static one, providing a glimpse of only one point in time for any given case" (pp. 523-524). As Luu and Tucker (1996) articulated: "To understand neuropsychological development is to confront the fact that the

brain is mutable, such that its structural organization reflects the history of the organism. Moreover, this structure reflects both what is most important to the organism and what the organism is capable of at that particular time" (p. 297). Thus, in order for neuroscientists utilizing neuro-imaging techniques to move beyond the descriptive anatomical level, their experimental hypotheses, methods (e.g., functional magnetic resonance imaging), and interpretation of their results must be framed in a fashion consistent with the dynamic nature of cortical organization. Anatomical perspectives are no longer adequate to understand structure-function relations in development. These anatomical models emphasize that structure dictates function; however, the principles of cortical plasticity and Gottlieb's (1976) notion of the inductive function of experience provide evidence that the obverse is also true—namely that function dictates structure.

THE DEVELOPING BRAIN

Although the field of neuroscience has made remarkable discoveries in recent decades, the late 19th and early 20th centuries, too, were an extremely productive period in neurobiological research. Wilhelm His was perhaps the preeminent contributor to research on the histogenesis of the central and peripheral nervous systems in the 19th century. Among His' discoveries were that changes in the shape of cells were involved in the folding of tissues such as the neural plate. In addition, His discovered the neural crest and the origin of the peripheral nervous system, and demonstrated that cranial and spinal ganglia are formed by cells that migrate from the neural crest (Jacobson, 1991). Moreover, His found that nerve cells originate by mitosis of stem cells near the ventricle of the neural tube rather than in the cerebral cortex itself (Jacobson, 1991). Furthermore, the concept of cell migration in the vertebrate central and peripheral nervous systems also was discovered by His, initially through his observations on the origins of the peripheral nervous system from the neural crest and subsequently by his discovery that neuroblasts migrate individually from the ventricular germinal zone to the overlying mantle layer of the neural tube (Jacobson, 1991).

In 1904, His published a monograph in which he summarized more than three decades of his experimental work on the development of the embryonic nervous system of humans. In this monograph, His delineated the basic principles of neurogenesis for vertebrates in general. His' ideas serve as the framework for virtually all subsequent studies of the nervous system (Sidman & Rakic, 1982). The virtual identity of the neural plate and the neural tube in all vertebrates provided suggestive evidence for

the operation of very similar mechanisms in central nervous system development throughout phylogenesis.

In subsequent years, important contributions were made by a number of investigative teams that revealed the extraordinary diversity, organizational complexity, and precision of connections between cells in the nervous system of both vertebrates and invertebrates. Among these most prominent historical contributions to the early understanding of neural development were those of Ramón y Cajal in Madrid, Hochstetter in Vienna, and scientists at the Moscow Brain Institute (1935–1965) and the Carnegie Institute in Baltimore (1942–1962; see review in Sidman & Rakic, 1982).

Cowan (1979) concluded that in the development of any part of the brain eight major stages can be identified: (a) the induction of the neural plate, (b) the localized proliferation of cells in different regions, (c) the migration of cells from the region on which they are generated to the places where they ultimately reside, (d) the aggregation of cells to form identifiable parts of the brain, (e) the differentiation (expression) of the immature neurons, (f) the formation of axonal pathways and synaptic connections with other neurons and the onset of physiological function, (g) the selective death of certain cells, and (h) the elimination of some of the connections that were initially formed and the stabilization of others. As Steingard and Coyle (1998) noted, these stages proceed in a stepwise fashion following a genetically encoded plan that is influenced by environmental events. Specifically, within each of these phases, there are parallel processes of metabolic differentiation and maturation. Genes that take their regulatory cues from the immediate neurochemical (and experiential) environment regulate the onset and offset of each stage.

Moreover, each stage of brain development is dependent on the successful completion of the preceding stages. Alterations in these processes can eventuate in aberrant neural development, connectivity, or function (Rakic, 1996; Steingard & Coyle, 1998; Volpe, 1995). In general, early disruption in the neurodevelopmental process is associated with a greater and more diffuse pathology (Nelson, 2000a; Volpe, 1995), whereas later disruptions in this process are associated with less severe pathology and more discrete neurological lesions (Steingard & Coyle, 1998). For example, if neuronal migration is disrupted, then abnormalities in cell position result. When this eventuates, the neurons are said to be ectopic or heterotopic (Nowakowski, 1987). Likewise, delaying, extending, shortening, or blocking either the *progressive* (e.g., synaptogenesis and neuronal maturation) and *regressive* (e.g., cell death and synaptic pruning) events in the neurodevelopmental process will exert varying effects on structure-function relations, on genetic regulatory processes, and on the emergence of later neurodevelopmental events that are dependent on earlier events (Nowakowski & Hayes, 1999; Steingard & Coyle, 1998).

In 1939 Conel (1939-1967) suggested that most cortical neurons in the human cerebrum were generated prenatally. More sophisticated marking of cells has enabled neuroscientists to permanently date DNA cell replication and thereby provide direct evidence and precise time of neuron data on the origin and termination of corticogenesis in primates (Rakic, 1996). Through utilizing the same marking of DNA replication technique in humans, we now know that humans develop their full complement of neurons mainly during the 4th, 5th, and 6th prenatal months (Rakic, 1981). The onset of corticogenesis in humans is approximately 6 prenatal weeks (Rakic, 1996). Through a more sophisticated autoradiographic analysis, Sidman and Rakic (1982) confirmed His' earlier discovery that all neurons destined for the neocortex were produced in the proliferative zone near the cerebral ventricle.

In the proliferative zone, precursor cells divide asynchronously; their nuclei move from the ventricular surface to synthesize DNA and then return to the surface to undergo another mitotic cycle (Rakic, 1996). There are two proliferative sites: (a) the ventricular zone, which contributes to cell proliferation and division in phylogenetically older brain structures, and (b) the subventricular zone, which contributes to cell proliferation and division in more recently evolved brain structures such as the neocortex. These two proliferative zones generate separate glial and neuron cell lines and give rise to different forms of migration, the process whereby neuronal cell bodies are displaced from their last cell division in the proliferative zone to their final destination in the mature brain (Rakic, 1988a, 1988b).

In 1874, His inferred the phenomenon of neuronal cell migration through analyzing the fixed brains of human embryos. More sophisticated autoradiographic techniques led to the discovery of the underlying cellular and molecular mechanisms of migration (Hatten, 1999; Price & Willshaw, 2000; Sidman & Rakic, 1982).

There are two types of cell migration, tangential and radial. In tangential migration, once cells are generated they are passively displaced and pushed farther away from the proliferative zone by more recently born cells in a so-called "outside-in" gradient. This tangential form of migration occurs at later developmental stages of the prenatal brain and gives rise to brain structures such as the thalamus, dentate gyrus of the hippocampus, and many regions of the brain stem.

In contrast, in radial migration, the most recently born cell actively moves beyond previously generated cells to create an "inside-out" gradient. This radial form of migration is visible during early stages of prenatal brain development and is found in the cerebral cortex and in some subcortical areas that have a laminar (layered) structure.

The area-specific features in the human adult neocortex are not evident in the immature cortical plate and emerge gradually over ontogenesis.

The cells of the neocortex are not committed to differentiate the area-specific architectural and connectional features that distinguish neocortical areas in the adult at the time the neocortical plate is assembled during embryogenesis (Nowakowski, 1987; Sidman & Rakic, 1973, 1982; Steingard & Coyle, 1998). There are two primary models of cortical area differentiation that have been put forward in the literature. Rakic (1988a, 1988b) has proposed the *protomap* hypothesis, a model whereby future cytoarchitectonic are thought to be areas genetically specified in the neuroepithelium and recapitulated in the developing cortical plate by a point-to-point migration along a radial glial scaffolding. In this "radial unit hypothesis," Rakic (1988a, 1988b) contended that the neuroepithelium is genetically programmed to generate area-specific cohorts of cortical plate neurons, and that the relative positions and sizes of areas of the cerebral cortex are prespecified. Rakic's protomap hypothesis applies the same mechanisms of development to areas throughout the cerebral cortex (Johnson, 1997; O'Leary, 1989). Although the programmed emergence of discrete, cytoarchitectonic areas requires an interaction with afferents, the capacity of developing cortical plate neurons to differentiate features normally associated with other areas is conceived as being restricted by their commitment to specific area plates.

A contrasting viewpoint of cortical area differentiation, the *protocortex* model, was proposed by O'Leary (1989; see also Johnson, 1997, 1998). The protocortex model emphasizes the role of epigenetic influences and applies only to the development of neocortical areas. According to the protocortex model, the neocortical epithelium is not genetically programmed to generate cortical plate cells that are committed to a particular areal fate. Rather, it is hypothesized that the neurons of the neocortical plate have the potential to develop the range of features associated with diverse neocortical areas. The differentiation of the neurons into area-specific connections and architecture requires inputs from thalamic (experiential) afferents to each region (O'Leary, 1989).

In sum, despite their differing emphases, both the protomap and protocortex models recognize contributions from genetic and epigenetic mechanisms in the differentiation of neocortical areas, as well as a vital role for thalamocortical afferents in this process. Original to the protocortex model is that molecular differences are thought to exist throughout the neocortex and contribute to the process of areal differentiation (O'Leary, 1989). Numerous investigations suggest that afferent inputs, especially thalamocortical afferents, play a fundamental role in regulating the differentiation of area-specific features (see Johnson, 1997). Thus, from the viewpoint of the protocortex model, normal cortical development is believed to enable a considerable amount of cortical plasticity. Cortical regions are capable of supporting a number of different types of representations depending on the nature of their input (Johnson, 1999).

Experience and Brain Development

Black, Jones, Nelson, and Greenough (1998) described brain development as a complex scaffolding of three types of neural processes: gene-driven, experience-expectant, and experience-dependent. Black et al. conceptualized *gene-driven processes* as being largely insensitive to experience. Gene-driven processes serve to guide the migration of neurons, to target many of their synaptic connections, and to determine their differentiated functions. In order to protect the development of the brain, much of the basic organization of most nervous systems is thought to be relatively impervious to experience. The recalcitrance to environmental influences during embryonic development was termed *canalization* by Waddington (1966). Black and colleagues noted that this canalization process can be both helpful (e.g., the minimization of experiential effects on embryogenesis can aid survival) or harmful (e.g., in cases of genetic diseases, prenatal brain development proceeds along a maladaptive pathway that is largely resistant to any therapeutic interventions).

Experience-expectant processes correspond roughly to critical or "sensitive" periods and take place in early age-locked sensory system development (Greenough & Black, 1992; Greenough et al., 1987). During experience-expectant periods the brain is primed to receive particular classes of information from the environment. The brain builds an overabundance of synapses that are then pruned back by experience to a selectively retained subset based on experience (Huttenlocher, 1990; Huttenlocher & Dabholkar, 1997). The pruning of synapses appears to be initiated by competitive interactions between neuronal connections such that inactive neural connections are eliminated and synapses that are most actively mediated by experience are selectively maintained (Greenough et al., 1987). In human embryology, the pruning process applies to neurons, whereas postnatally it applies predominantly to synapses (Edelman, 1987). In the absence of behavioral and neural activity, cells do not die, and circuits do not become pruned in an adaptive way that aids the organism in adapting to the demands of its environment. Experience-expectant brain plasticity is usually embedded in a developmental program and it requires appropriate timing and quality of the information stored for brain development to be normal. Abnormal experience or deprivation during experience-expectant development may exert enduring deleterious effects on brain and behavioral epigenesis (Black et al., 1998).

In later development, synaptogenesis seems to be generated in response to events that provide information to be encoded in the nervous system. This *experience-dependent* synapse formation involves the brain's adaptation to information that is unique to the individual (Greenough & Black, 1992; Greenough et al., 1987). Because all individuals encounter distinctive environments, each brain is modified in a singular fashion. Experience-

dependent synaptogenesis is localized to the brain regions involved in processing information arising from the event experienced by the individual. Unlike the case with experience-expectant processes, experience-dependent processes do not take place within a stringent temporal interval because the timing or nature of experience that the individual engages or chooses cannot be entirely and dependably envisioned. An important central mechanism for experience-dependent development is the formation of new neural connections in contrast to the overproduction and pruning back of synapses often associated with experience-expectant processes (Greenough et al., 1987). Because experience-dependent processes can occur throughout the life span, social interventions, psychotherapy, and pharmacotherapy have the capacity to repair brains that are afflicted with disorders (Black et al., 1998; Cicchetti, 1996; Cicchetti & Sroufe, 2000a; Cicchetti & Walker, 2001).

Rakic (1996) discussed that in the neocortex of the rhesus monkey, synaptogenesis and synapse elimination occur simultaneously and at an equal rate in all cortical regions (see also Goldman-Rakic, Bourgeois, & Rakic, 1997). On the other hand, because there are known regional differences in neurobiological development, including timing of maximum brain growth, dendritic arborizations, and myelination of cortical afferents and efferents (Huttenlocher & Dabholkar, 1997), it is not surprising that concurrent synaptogenesis does not occur in humans. The latest region to develop is the prefrontal cortex (Huttenlocher, 1984). Thus, competition between and correlated activity within neural networks drive the selective stabilization of some neural connections at the expense of others and lead to the normal parcellation of neural systems into specific structural and functional units (Courchesne, et al., 1994).

The normal brain develops from a network of few elements, infrequent interactions among elements, less stability, and less structural and functional differentiation, to one of additional elements, more intricate interactions among elements, greater stability, and increased structural and functional parcellation and specialization (see, e.g., Brown, 1994; Cicchetti & Tucker, 1994a; Courchesne et al., 1994). In this view of normal brain development, one can clearly see parallels with organismic, holistic developmental systems theory views of behavioral development. For example, Wapner and Demick (1998) conceptualized optimal development as a differentiated and hierarchically integrated person-in-environment system that possesses the characteristics of flexibility, self-mastery, and the ability to shift from one mode of person-in environment relationship to another as required by the contextual demands of the situation, the individual's goals, and so on. In essence, the orthogenetic principle (Werner, 1957) operationalizes development in terms of the degree of integration and organization achieved by behavioral (and biological) systems (Cicchetti & Schneider-Rosen, 1986).

Children whose gene-driven processes construct a disordered brain are likely to experience the world in a vastly different fashion than children who do not have such a genetic predisposition. Even if the subsequent experience-expectant and experience-dependent processes are unimpaired, the experience distorted by the neuropathology is not likely to be appropriately utilized. Thus, children with brains whose development deviated from the normal genetic program must have their environments adapted to their needs; otherwise these children's experiences will be further distorted and development will proceed along a maladaptive course.

Likewise, children born with relatively normal brains may encounter a number of experiences (e.g., extreme poverty, physical abuse, sexual abuse, and neglect, etc.) that can negatively impact upon developing brain structure, function, and organization and contribute to distorting these children's experiences of the world (see, e.g., Cicchetti & Rogosch, 2001; DeBellis et al., 1999; Nelson & Bosquet, 2000; Pollak, Cicchetti, & Klorman, 1998). Children may be especially vulnerable to the effects of pathological experiences during periods of rapid creation or modification of neuronal connections (Black et al., 1998). Pathological experience may become part of a vicious cycle, as the pathology induced in brain structure may distort the child's experience, with subsequent alterations in cognition or social interactions causing additional pathological experience and added brain pathology (Cicchetti & Tucker, 1994a). Because experience-expectant and experience-dependent processes may continue to operate during psychopathological states, children who incorporate pathological experience during these processes may add neuropathological connections into their developing brains instead of functional neuronal connections (Black et al., 1998).

Finally, because some children can function in a resilient fashion even though they have experienced adversity (Luthar et al., 2000; Masten, Best, & Garmezy, 1990), it is essential to keep in mind that such individuals are likely to thrive in a wide array of environments. Just as is the case with neurobiological growth, resilience is a dynamic developmental process (Cicchetti & Rogosch, 1997; Egeland, Carlson, & Sroufe, 1993). Children who function resiliently play an active role in seeking and receiving the experiences that are developmentally appropriate for them. In this manner, through utilizing experience-dependent processes, they likely modify and/or protect their brain anatomy to ensure an adaptive developmental outcome. Even though early brain pathology, be it gene-driven or induced by experience, may set a maladaptive course for development, the organism often makes efforts to compensate for it (i.e., "self-righting" tendencies—see Cicchetti & Rogosch, 1997; Sameroff & Chandler, 1975; Waddington, 1957). At one level, different parts of the brain may attempt to compensate, and at another, the organism may seek out new experience in areas where it has strength (Black et al., 1998). Because plasticity is a

central feature of the mammalian brain (Kemperman, van Praag, & Gage, 2000), early brain anomalies or aberrant experience should not be considered as determining the permanent fate of the organism (see Cicchetti & Tucker, 1994a). Biological and psychological protective and buffering mechanisms can serve as mediators or moderators of resilient adaptation in the face of serious adversity.

Neural Plasticity

In the denouement to his treatise entitled *Degeneration and Regeneration of the Nervous System,* Ramón y Cajal declared: "Once development is completed, the sources of growth and regeneration are irrevocably lost, In the adult brain, nervous pathways are fixed and immutable; everything may die, nothing may be regenerated" (Ramón y Cajal & May, 1913/1959, p. 750).

After over a decade of innovative and meticulous research in which he investigated whether the brain was capable of regeneration in animals with injuries to the spinal cord, the cerebellum, or the cerebral cortex, Ramón y Cajal proclaimed that "the vast majority of regenerative processes described in man are ephemeral, abortive, and incapable of completely and definitely repairing the damaged pathways" (Ramón y Cajal & May, 1913/1959, p. 750). Since Ramón y Cajal's early work, it has been known that there is some axonal regeneration subsequent to spinal cord injury; however, in the experimental conditions that Ramón y Cajal designed, regeneration was limited and, therefore, not believed to have any functional significance (Stein & Dawson, 1980). Ramón y Cajal's assertions served as the prevailing dogma for a large portion of the 20th century. The widespread belief put forward by neuroscientists was that because no new neuron generation was deemed possible, rewiring of existing connections, dendritic branching, and elimination of synaptic connections were the only ways whereby neural plasticity could occur.

It was not until the 1970s that regenerative capacity was demonstrated in the adult mammalian brain. Two types of growth were found to occur in response to nerve injury (Stein & Dawson, 1980). The first is regenerative sprouting, a process whereby when the axons of cells are cut and the distal portion begins to degenerate, the remaining stump, including the cell body, begins to form growth cones and regenerate new terminals. In the second, known as collateral symmetry, a number of cells innervating a given structure are destroyed and their terminals degenerate. However, the remaining intact cells begin to grow additional new terminals (sprouting) that innervate the target area evacuated by the damaged neurons. As such, the degenerated inputs are replaced by terminals arriving from intact neurons.

In 1975, the eminent neuropsychologist Hans-Lucas Teuber declared that if one is going to have brain damage, then it would be preferable to have it early rather than late in life. Teuber based his conclusion on the findings of Margaret Kennard's experiments on the long-term effects of brain damage in monkeys of different ages. Kennard (1938) discovered that damage to the adult nervous system resulted in more deleterious and less reversible effects than similar brain damage inflicted during early development. The field's acceptance of the so-called "Kennard principle" led to the belief that little reorganization of function could occur after injury to the mature mammalian nervous system and that most structure-function relations were permanently established during the early years of life. In fact, Teuber's (1975) own studies with humans revealed that improvement in traumatic head injury cases was substantially greater when the brain damage had taken place in an early developmental period. However, scientists in the field had differing interpretations of Teuber's belief that brain damage that occurred later in development would exert more disruptive effects than would damage originated earlier in life. Critics noted that the results of the experimental data of Teuber and his contemporaries in the field could be interpreted to indicate that the maturational status of the nervous system at the time of injury must be considered in any explanation of recovery or sparing of function after early brain damage (Stein & Dawson, 1980). Commentators on the aforementioned body of work reasoned that if subjects were tested at a time when the substrate for a particular function had not yet developed, then the function would appear to have been compensated when, in fact, it had never been lost (Isaacson, 1975). Depending on the location of the lesion and the precise timing of the injury, the developing brain can suffer from far more neuronal degeneration than that evidenced in the mature brain. For example, excessive amounts of excitatory amino acids such as glutamate produce much more severe lesions in the immature brain than in the adult brain. Thus, there are a number of instances in which early brain damage can be viewed as more disastrous than later brain damage because early lesions often result in the formation of anomalous circuitry and neural pathways as well as reduction in brain size (Black et al., 1998; Isaacson, 1975; Steingard & Coyle, 1998).

Kolb, Forgie, Gibb, Gorny, and Rowntree (1998), in their programmatic enrichment studies on brain plasticity and behavior in rats, found that enriched experience could have varying effects on the brain at different ages. They reported discovering compensatory changes in brain plasticity following brain injury that are similar in kind to those observed when animals learn from experience (e.g., see the work of D. O. Hebb at McGill in the 1940s; Diamond, Krech, Rosenzweig, and their colleagues at Berkeley in the 1960s, 1970s, and 1980s; Greenough and colleagues at Illinois from

the 1970s to the present; and Purves and colleagues at Duke University in the 1980s). Kolb and colleagues (1998) found that experience alters the synaptic organization of the cortex and that these changes in synaptic organization are associated with behavioral changes. The similarity between plastic changes in the brain in response to injury or experience suggests that it is conceivable that there may be basic mechanisms of synaptic change in the mammalian cortex that are used in many forms of plasticity (see also Kolb, 1995).

Accordingly, it appears that there are not any simple rules that govern whether plasticity will occur following lesions in early life. Most of the early neuropsychological models of the effects of brain lesions were based on work with patients, were predominantly focused on brain localization, and were couched within unidirectional models of causality (i.e., brain lesions were believed to affect behavior, but not vice versa). These early models did not take into account the dynamic interplay that occurs among brain regions and the bidirectional impact that brain-behavior relations exert upon each other. Modern-day research, conducted predominantly, but not exclusively with animals, provides suggestive evidence that the mammalian CNS possesses much greater potential for producing new neurons and repairing damaged areas than has heretofore been thought (Cicchetti & Tucker, 1994b; Hann, Huffman, Lederhendler, & Meinecke, 1998; Kemperman et al., 2000; Knudsen, 1999). The discovery of brain behavioral plasticity can make important contributions to the understanding of development through demonstrating that neural representations are dynamic processes. Moreover, experimental results that provide evidence of plasticity can provide critical insights into the actual processes through which development occurs.

Recovery of function following brain injury can be characterized as a maturational process in which the brain is thought to trigger new structures via specialized mechanisms that were triggered by the injury (i.e., causal epigenesis; see Johnson, 1999). This viewpoint involves the restriction of fate—that is, biological tissue that initially had many possibilities for subsequent specialization throughout development is reduced to a subset of these possibilities by the injury. In contrast, according to Gottlieb's (1992) probabilistic epigenesis framework, brain plasticity may be conceived as a fundamental and inherent property of the developing brain. From this viewpoint, plasticity is conceptualized as still having a number of options for specialization even after brain injury. This process is influenced by neural activity rather than by molecular markers. In humans, neurobiological development is a more prolonged process than in animals, extending well into postnatal life (Giedd, Vaituzis et al., 1996; Nelson & Bosquet, 2000; Spear, 2000). Critical aspects of neurobiological development occur after middle childhood (Spear, 2000). Therefore, some

degree of functional specialization in the cerebral cortex is likely to be influenced by the child's interaction with the postnatal environment. The extremely long juvenile period (neotony) in humans may have evolved, in part, for the primary purpose of shifting cortical specification from genetic to epigenetic control (Cicchetti & Tucker, 1994a).

During the past several decades, scientific research has begun to reveal that within certain limits, forms of neural plasticity may take place throughout epigenesis and are not limited to early development (Cicchetti & Tucker, 1994b; Hann et al., 1998; Kandel & Squire, 2000). The cortex appears to be capable of organizational changes throughout the life course of the organism (Cicchetti & Tucker, 1994a; Jacobs, van Praag, & Gage, 2000). For example, Thatcher's (1992, 1994, 1997) work on the development of EEG (electroencephelogram) coherence in humans suggests that cortical organization proceeds in stages that are repeated cyclically over development. Each stage of cortical organization reflects the ongoing and dynamic shaping of cortical circuitry throughout an individual's life span. These periods of EEG coherence are thought to represent rapid synaptic growth within functionally differentiated neural systems. Neural plasticity involves the genetically driven overproduction of synapses and the environmentally driven maintenance and pruning of synaptic connections.

Neuroscientists conceive plasticity as being reflective of anatomic, chemical, or metabolic changes in the brain. Nelson (2000c) stated that neuroanatomic changes illustrate the ability of existing synapses to alter their activity through sprouting new axons or regenerating old ones, or by elaborating their dendritic surfaces. Thus, for example, loss of fibers in an area of the cortex (e.g., the corpus callosum) may eventuate in a reduction of synapses in the affected area that is subsequently compensated for by an influx of thalamic synapses into the vacated space that reestablishes communication between the hemispheres. Nelson also defined neurochemical plasticity as the ability of synapses to alter their activity either through augmenting the synthesis of neurotransmitters or by enhancing the response of the postsynaptic receptor to the neurotransmitter. Additionally, Nelson delineated fluctuations in cortical and subcortical metabolic activity, for example, at the site of an injury, as another possible sign of neural plasticity.

In the developing organism, studies conducted with a variety of species have revealed that positive or negative early life experiences can modify both brain structure and function. For example, Sur and colleagues (Sur, Garraghty, & Roe, 1988; Sur, Pallas, & Roe, 1990) trained adult ferrets, who had one hemisphere rewired at birth, to discriminate between auditory and visual stimuli that were presented to the normal hemisphere. The results of these experiments provide support for the functional equipotentiality of cortical mapping. Specifically, the findings

in the Sur et al. investigations demonstrate that it is possible to rewire sensory inputs to the thalamus such that processing of auditory stimuli takes place in primary visual cortex and vice-versa; that is, the cortical field that usually mediates vision could attain a functional organization capable of processing sound, and the cortical field that usually mediates audition could attain a functional organization capable of processing sight.

Recently, Sur and colleagues (Sharma, Angelucci, & Sur, 2000) demonstrated that, in ferrets whose retinal projections were routed into their auditory pathway, visually responsive neurons in the "rewired" auditory cortex, just as is the case with neurons in the primary visual cortex, were characterized by orientation modules—groups of cells that share a preferred stimulus orientation. Although the orientation tuning of neurons within the "rewired" auditory cortex was comparable to the tuning of cells in the primary visual cortex, the orientation map was less orderly. Thus, the findings of this investigation reveal that afferent activity profoundly influences diverse components of cortical circuitry.

As discussed earlier, in most mammalian brain regions, neuronal birth and migration take place during a discrete period of prenatal development, followed several days later by cell death (Rakic, 1988a, 1988b, 1996). In contrast, the granule cells of the dentate gyrus in the hippocampus, olfactory bulb, and cerebellum are generated predominantly during the postnatal period (Gould & Cameron, 1996). Approximately 85% of dentate gyrus neurons are generated postnatally; however, their production begins during the embryonic period (Gould & Cameron, 1996). A large population of cells reaches the dentate gyrus without undergoing final division; these precursor cells remain in the dentate gyrus and become the source of granule neurons born in the postnatal period. Granule cells are excitatory neurons that utilize glutamate as their primary neurotransmitter. A growing body of investigations indicates that excitatory input plays a prominent role in the formation of many neuronal populations.

In fact, in the mature CNS, pools of progenitor cells appear to proliferate and migrate well into adulthood. For example, Kempermann, Kuhn, and Gage (1998) discovered that neurogenesis continues to occur in the dentate gyrus of senescent mice and can be stimulated when the mice are placed in an enriched environment. Kempermann et al. found that neurogenesis declined with increasing age; however, stimulation of adult and aged mice by changing from regular housing to an enriched environment that provided opportunities for social interaction, physical activity, and exploration brought about an increased number of survival cells. Furthermore, animals residing in enriched environments had more of their cells differentiate into neurons than did mice housed in standard conditions. These findings suggest that the new neurons were recruited in the hippocampal area and that neural plasticity can take place in the aging brain in mice.

Likewise, Gould, Tanapat, McEwen, Flugge, and Fuchs (1998) discovered that new neurons are produced in the dentate gyrus of adult monkeys. Moreover, these investigators found that a single exposure to a socially stressful condition (i.e., a resident intruder unfamiliar adult male conspecific) inhibits the proliferation of granule cell precursors. Cortisol and glucocorticoids control the rate of the development of these new neurons, and it is clear that the existing neurons are not merely reorganizing their connections. Furthermore, the mature CNS continues to express an array of molecules that are required for the formation of neuronal networks during embryonic development (e.g., neurotrophic growth factors, embryonic forms of cell adhesion molecules, axon-guidance molecules, etc.). The presence of these molecules suggests that the degree of potential network remodeling in the mature CNS may be more extensive than generally thought (Lowenstein & Parent, 1999).

In addition, dormant progenitor cells persist in the CNS of adult mammals and these cells have the capacity to give rise to a variety of CNS cells. Thus, although there is currently no evidence that the fully developed CNS continues to generate new neurons and glial cells everywhere, progenitor cells with the potential to produce new cells are prevalent throughout the mature mammalian CNS (Gage, 2000; Lowenstein & Parent, 1999). It remains to be discovered what brain regions other than the dentate gyrus are capable of such plasticity and to be determined what the limits to neural plasticity may be.

Two factors, adrenal steroids and excitatory input, have been identified that regulate the proliferation, migration, and survival of granule neurons during the postnatal period through adulthood. In general, increases in adrenal steroid levels or NMDA receptor activation diminish the rate of cell proliferation, whereas decreases in adrenal steroid levels or NMDA receptor activation increase the rate of cell production. These results also suggest that decreased neurogenesis associated with increased corticosteroid levels may contribute to age-related memory deficits. Moreover, the finding that the neuronal precursor population in the dentate gyrus remains stable into old age, but that neurogenesis is slowed by high levels of adrenal steroids, suggests that these memory deficits may be reversible (Cameron & McKay, 1999). Relatedly, Gould (1999) also reported that activation of serotonergic receptors enhanced neurogenesis in the adult mammalian dentate gyrus. Because adult-generated hippocampal neurons are affected by, and conceivably involved in, learning and memory (Gould, Beylin, Tanapat, Reeves, & Shors, 1999), serotonergic agonists that stimulate granule cell production may prevent memory deficits.

Cells that divide in adulthood do not die soon thereafter. Thus, the cell death that occurs in adulthood does not simply remove cells that were generated incorrectly in adulthood. In fact, because granule neurons that

generate in adults survive for at least 1 month and form connections, it is likely that their addition to the granule cell layer has significant functional consequences. In this regard, Gould, Beylin, et al. (1999) discovered that stress does not alter the survival of recently produced neurons in the dentate gyrus. Accordingly, young (immature) adult-generated hippocampal cells may make them uniquely qualified to form synaptic connections rapidly and to participate in the transient storage of information. Furthermore, Gould, Beylin et al. found that in order for learning to further enhance the number of new hippocampal neurons, the animal must be engaged in a task for which this brain region is essential. In addition, Gould, Reeves, Graziano, and Gross (1999) discovered that in adult macaque monkeys new neurons are added to the prefrontal, posterior parietal, and inferior temporal cortex. These investigators hypothesized that the new neurons added to these regions of association cortex might play a role in such functions. Moreover, Gould, Reeves, and colleagues conjectured that immature neurons are capable of undergoing rapid structural changes and thereby serve as a substrate for learning.

Finally, the long-held assumption that neural reorganization following injury was restricted to the period of infancy, with only modest neural reorganization possible in the child and adult, has been challenged through recent research with humans. Results from a number of investigations suggest that reorganization of cortical pathways can occur in the brains of older children and adults (see, e.g., Merzenich, 1998; Merzenich et al., 1996; Tallal et al., 1996). Although the majority of these neural changes to date have been demonstrated in work on sensory or motor pathways (see, e.g., Aglioti, Bonazzi, & Cortese's, 1994, work on phantom lower limb as a perceptual marker of neural plasticity), recent work provides suggestive evidence that cognitive systems (i.e., language) can reorganize beyond infancy (see, e.g., Merzenich et al., 1996; Tallal et al., 1996; also see discussions in Johnson, 1999). Thus, it is becoming quite clear that under certain conditions at least some regions of the brain can incorporate the signature of experience into the structure, function, and organization of the brain.

Self-Organization

Writing over half a century ago, Herrick (1948) stated that in embryogenesis the pattern of differentiation of sensory and motor systems was determined chiefly by intrinsic agencies. Herrick believed that embryogenesis proceeds in a predominantly autonomous fashion up to functional capacity. Herrick contended that these structural patterns were laid down in the inherited organization, and that organization has been elaborated in the course of phylogenetic development in adaptation to the physiological needs of the species in question. Thus, morphogenesis is interrelated with

the demands of ecology. Accordingly, the intrinsic agencies that initiate and shape the course of embryonic differentiation are not strictly autonomous and even the hereditary factors arise from the beginning as responses by the protoplasmic organization to the environmental influences.

Prior to the discovery of any evidence for cortical plasticity, John Hughlings Jackson (1931) commented on the importance of self-organization and brain evolution: "We develop as we must, that is, according to what we are by inheritance; and also as we can, that is, according to external conditions. There is something more: there is what I will call Internal Evolution, a process which goes on most actively in the highest centers" (p. 71). Jackson thought that internal evolution resulted in lasting reorganizations of cortical representations, some of which may not actually correspond to any specific experience with the environment nor to any behavioral consequences. Rather, Jackson conceived these representations as the result of self-organizing processes that are relatively free of environmental influence.

In recent years, developmentalists have been advocating a self-organizing systems-theory viewpoint of brain development (Cicchetti & Tucker, 1994a; Courchesne et al., 1994; Gottlieb, 1992; Johnson, 1997; Thelen & Smith, 1994, 1998). From this perspective, although brain development is guided and controlled to some extent by genetic information, a significant portion of postnatal brain structuration and neural patterning is thought to occur through interactions of the child with the environment. Changes in the internal and external environment may lead to improvements in the ability of the individual to grapple with developmental challenges. Consequently, although historical factors canalize and constrain the adaptive process to some degree, plasticity is possible as a result of adaptive neural and psychological self-organization. Cortical development and organization should no longer be viewed as passive processes that solely depend on genetics and environmental input. Development, psychological and biological, is more than nature-nurture interaction. Thus, corticogenesis and organization should be conceived as processes of self-organization guided by self-regulatory mechanisms (Cicchetti & Tucker, 1994a).

In self-organizing brain development, some regions of the brain serve to stabilize and organize information for other areas, whereas other regions utilize experience to fine-tune their anatomy for optimal function. In this manner, individuals can use the interaction of genetic constraints and environmental information to self-organize their highly complex neural systems. Accordingly, each organism may traverse upon a potentially unique and partly self-determined developmental pathway of brain building (cf. Cicchetti & Rogosch, 1997).

Recognizing that mechanisms of plasticity are integral to the very anatomical structure of cortical tissue, and that they cause the formation

of the brain to be an extended malleable process (Giedd, Snell et al., 1996; Jernigan & Tallal, 1990; Nelson & Bosquet, 2000; Pfefferbaum et al., 1994), neuroscientists and developmental psychopathologists are presented with new avenues for understanding the vulnerability of the brain as a contributor to the genesis and epigenesis of psychopathology. Moreover, because the mechanisms of plasticity cause the brain's anatomical differentiation to be dependent on stimulation from the environment, it is now clear that the cytoarchitecture of the cerebral cortex is shaped by input from the social environment.

This basic model of plasticity might be fit within a conventional nature-nurture causal model of development. Nature provides the experience-expectant processes, whereas nurture provides the experience to select the functionally adaptive network connections; and a theory that considers the interaction of these factors may account for neuropsychological development. However, in the determination of what information is adaptive, the brain's adaptive self-regulation becomes apparent. As this operates over time, guided by the unique network patterns formed within the child's brain, the cumulative effect is one of increasing complexity and increasing uniqueness of that child's developmental organization (i.e., personality). A theory of self-organizing complexity is necessary to go beyond the nature-nurture model. Because the human cortex is only diffusely structured by the genetic plan, and because its eventual differentiation is highly reactive to the organism's active coping in a particular environment, we may expect that both normal and abnormal outcomes of the neotenic developmental process would encompass a diverse range of cortical network anatomies and individual personalities.

The role of nurture can now be reconceptualized. The traditional assumption was that the environment determines only the psychological residuals of development, such as memories and habits, whereas brain anatomy matures on its fixed ontogenetic calendar. Environmental experience is now recognized to be critical to the differentiation of the brain itself. Nature's potential can be realized only as it is enabled by nurture. In the words of Torsten Weisel (1994): "Genes controlling embryonic development shape the structure of the infant brain; the infant's experience in the world then fine tunes the pattern of neural connections underlying the brain's function. Such fine-tuning... must surely continue through adulthood" (p. 1647). Even if the basic ground plan for neurological growth is specified in the genome, the precise neuroanatomic details are specified by activity-dependent competition between presynaptic axons for common postsynaptic target neurons (cf. Courchesne et al., 1994; Eisenberg, 1995; Greenough et al., 1987).

Several mechanisms mediate the self-organization of neural plasticity: (a) primitive controls on activity level, (b) corticolimbic mechanisms guid-

ing memory formation, and (c) goal-directed regulatory functions of the frontal lobe (see Cicchetti & Tucker, 1994a, for an elaboration). Fundamental mechanisms of self-regulation are provided by brainstem neuromodulatory projection systems. With widespread projections to cortical and subcortical targets, the neuromodulator systems act as focal control points for regulating neural activity. By directing neural activity, we might expect that the neuromodular systems of the developing brain direct the activity-dependent pruning of the synaptic connectional architecture. Each neuro-modular system appears to tune neural and behavioral activity in a specific way and this specificity of effect may have direct implications for the control of neural plasticity.

INSIGHTS FROM PSYCHOPATHOLOGY: A VIEW FROM SCHIZOPHRENIA

Often, the investigation of a system in its smoothly operating normal or healthy state does not afford the opportunity to comprehend the interrelations among its component systems (see, e.g., Caviness & Rakic, 1978). Because pathological conditions enable scientists to isolate the components of the integrated system, investigation of these non-normative conditions sheds light on the normal structure of the system.

Abnormal brain development is also a dynamic, self-organizing process. Unlike the case for normal neurobiological development, the final product of abnormal brain development includes a substantial measure of misorganization. Perturbations that occur during brain development can potentiate a cascade of maturational and structural changes that eventuate in the neural system proceeding along a trajectory that deviates from that generally taken in normal neurobiological development (Cicchetti & Tucker, 1994a; Courchesne et al., 1994; Nelson & Bosquet, 2000; Nowakowski, 1987).

Accordingly abnormal perturbations at one stage of brain development hinder the creation of some new structures and functions, distort the form of later-emerging ones, make possible the construction of ones that normally never become manifest, and/or limit the elaboration and usage of structures and functions that had appeared earlier (Courchesne et al., 1994; Steingard & Coyle, 1998). Eventually, successively more complex, specialized, and stable abnormal neural network configurations and operations develop that differ greatly from antecedent ones (Courchesne et al., 1994). Abnormal competition between, and abnormal correlated activity within, undamaged as well as damaged neural networks can drive the abnormal elimination of some connections and neural elements (e.g., remote loss) and the abnormal selective stabilization of others (e.g., aberrant

connections retained or created) (Courchesne et al., 1994). Such early developmental abnormalities may lead to the development of aberrant neural circuitry and often compound themselves into relatively enduring forms of psychopathology (Cicchetti & Cannon, 1999; Nelson & Bosquet, 2000; Nowakowski & Hayes, 1999). In keeping with a holistic, developmental systems approach, it is expected that a dedifferentiation and disintegration of biological and behavioral functioning would characterize individuals with mental disorders (see Kaplan, 1967; Wapner & Demick, 1998; Werner & Kaplan, 1963).

Much of the modern-day research in the area of neurodevelopment and psychopathology owes a significant portion of its historical roots to the formulations of Emil Kraepelin (1919), who conceived of schizophrenia as a deteriorating brain disease in its natural history, albeit with an onset in early adult life. Since the 1980s, when there was a resurgence of interest in neurobiological studies of schizophrenia, Kraepelin's view has been challenged and radically changed by advances from several levels of inquiry that point to a prenatal-perinatal origin of at least some of the brain abnormalities found in persons with schizophrenia (see, e.g., Cannon, 1998; Keshavan & Hogarty, 1999; Mednick et al., 1991; Weinberger, 1987), paving the way for viewing schizophrenia as resulting, in part, from prenatal disturbances.

The retrospective observations of Laura Bender (1947) and Barbara Fish (1957), as well as the follow-back study by Norman Watt (1972), in which a pattern of abnormalities in neurological and behavioral parameters dating back to childhood were found in adults with schizophrenia, laid the seeds of the neurodevelopmental hypothesis of schizophrenia (Marenco & Weinberger, 2000). Moreover, a number of longitudinal studies demonstrated that some degree of recovery was possible in some cases of schizophrenia (Garmezy, 1970; Tsuang, Wollson, & Fleming, 1979), thereby casting doubt upon the Kraepelinian viewpoint that schizophrenia is a degenerative disease of early adulthood ("dementia praecox").

Similarly, prospective longitudinal high-risk offspring studies have revealed that behavioral antecedents of schizophrenia occurred before the disease. Fish (1977) demonstrated that a neurobiologic disorder exists in infants and children prior to the onset of more chronic forms of schizophrenia. Fish's discovery of pandevelopmental retardation was considered an early marker of the inherited neurointegrative defect (i.e., schizotaxia) postulated to exist in schizophrenia by Paul Meehl (1962). Important to note, Fish observed that the phenotypic manifestations of the neurointegrative defect change over epigenesis and that the signs of dysregulation of maturation are found in many developing systems. Specifically, Fish reported that the neurointegrative disorder present from infancy disrupted the normal timing, sequence, and overall organization of development. Moreover, in a landmark prospective longitudinal investigation, Fish,

Marcus, Hans, Auerbach, and Perdue (1992) discovered that the infant off-spring of schizophrenic mothers displayed greater lags in their motor development during infancy and that a number of these infants themselves went on to develop schizophrenia or schizotypal personality disorders.

Relatedly, Cannon, Rosso, Bearden, Sanchez, and Hadley (1999), in their epidemiological investigation of the Philadelphia cohort of the National Collaborative Perinatal Project, provided compelling evidence that adverse experiences during gestation and birth, as well as deviant cognitive, motor, and behavioral functioning during early childhood, are associated with an increased risk for schizophrenia. In particular, these investigators demonstrated that the risk for schizophrenia increases linearly with the severity of fetal oxygen deprivation. In prior neuroimaging studies of high-risk samples (Cannon, Mednick, Parnas, & Schulsinger, 1993; Cannon in press), a history of perinatal hypoxia was found to be associated with increased severity of a neuropathological indicator of schizophrenia (i.e., ventricular enlargement) among individuals with an elevated genetic risk for the disorder, but not among controls at low genetic risk. Together, this evidence suggests that a genetic factor in schizophrenia may render the fetal brain particularly susceptible to the effects of oxygen deprivation and encourages search for molecular mechanisms underlying this heightened neural vulnerability.

Cannon et al. (1999) also discovered that preschizophrenic individuals show evidence of cognitive, motor, and behavioral dysfunction during the first 7 years of life. Because there was not evidence of significant intra-individual decline during this period within any domain of functioning, the results argue against the view that a deteriorative neural process underlies these early phenotypic expressions of liability to schizophrenia. Rather, the findings suggest that an increasing number of diverse phenotypic signs emerge with age as the diverse brain systems required for their expression reach fundamental maturity. Finally, because similar functional disturbances were observed in the unaffected siblings of the preschizophrenic cases, it would appear that these cognitive, motor, and behavioral disturbances are indicators of an inherited neural diathesis to schizophrenia (cf. Walker & Diforio, 1997).

In addition to the early and more recent work with preschizophrenic infants and children that served as an impetus for modifying the Kraepelinian (1919) view of schizophrenia, modern-day findings have contributed to the belief that the neurobiological foundations of schizophrenia are established, at least in part, during the development of the brain. These include the following:

1. Several prospective longitudinal investigations have discovered an association between prenatal and perinatal complications (e.g., fetal hypoxia) and an increased risk for the later development of schizophrenia.

These results suggest that the adverse effects of obstetric complications on the developing fetal brain may play a role in the etiology of schizophrenia.

As Rakic (1988a, 1988b, 1996; Sidman & Rakic, 1982) and Nowakowski (1987; Nowakowski & Hayes, 1999) have concluded, during periods of rapid brain development in which neuronal migration is occurring and synaptic connections are formed, the fetal brain is especially vulnerable. Exogenous teratogens (such as maternal influenza), along with obstetric complications (such as perinatal hypoxia—see Mednick, Machon, Huttunen, & Bonett, 1988), in concert with the genetic predisposition to schizophrenia, may exert dramatic effects on the regions of the brain experiencing the most rapid growth. Introducing birth complications and teratogens also may place the cortical connections being established and refined at increased risk for aberrant development.

2. A number of postmortem neuropathology studies have found evidence of heterotopic displacement of neurons in various regions of the brain including the hippocampus and the frontal and temporal cortices. These findings suggest that there are disturbances of brain development in utero in many schizophrenics.

3. Disturbances in neurogenesis, neuronal migration and differentiation, synaptogenesis, neuron and synaptic pruning (perhaps resulting in reduced synaptic connectivity—cf. McGlashan & Hoffman, 2000), and myelination, occurring at the cellular and molecular levels, suggest that schizophrenia is a disorder that is instantiated in brain development (Arnold, 1999; Weinberger, 1987).

For example, the laminar distribution of cortical neurons is displaced inward in schizophrenia, indicating a defect in cortical organization, suggesting that the normal process of "inside-out" neuronal migration (cf. Rakic, 1988a, 1988b) during the second trimester of gestation is also likely to be anomalous, as should be the neuronal connectivity and circuitry.

4. A growing body of neuroimaging studies has identified gross structural neuroanatomy changes in young, untreated patients in their first psychotic episode. Further, and in contrast to a Kraepelinian (1919) neuro-degenerative viewpoint, these investigations have failed to discover evidence of deterioration in these neuropathological markers with increasing length of illness.

5. A number of the unaffected first-degree relatives of schizophrenic patients manifest the structural and functional brain abnormalities observed in schizophrenics, implying that such abnormalities may be mediated, in part, by genetic predisposition to the disorder (Cannon et al., 1993, 1994). Homeobox genes, which serve as transcription factors regulating gene expression, represent potential candidate genes in disorders in which a disruption of cortical neurogenesis has been implicated, such as in schizophrenia (Ruddle et al., 1994; Steingard & Coyle, 1998).

Important to note, the extant models linking neurodevelopment and schizophrenia point to a nonlinearity of relations. There is a long latency between the gestational events hypothesized to create a predisposition to schizophrenia and the onset of the symptoms of the disorder later in life. Longitudinal follow-up of individuals who have experienced traumatic insults to the brain at early stages of development, such as is likely the case in many instances of schizophrenia, enables investigators to chart and observe the changing expression of these early lesions as development modifies behavior in general.

Alternatively, for some individuals it also is conceivable that the lesion directly affects later developmental processes via cascade, propagation, and expansion (Cicchetti & Tucker, 1994a; Courchesne et al., 1994; Post et al., 1994; Steingard & Coyle, 1998). These options all provide an opportunity to discover how brain and behavior reorganize following the experience of insults at different points in the developmental course.

Because not all persons who experience the gestational disturbances noted in the literature go on to develop clinical schizophrenia, Gottlieb's (1992) concept of probabilistic epigenesis is evoked. Furthermore, the existing research reveals that there are a number of pathways through which the early neurodevelopmental anomalies may result in schizophrenia. The identification of these diverse pathways to schizophrenia provides insight into how specificity and differentiation into a syndrome may result from a commonality of initiating circumstances (i.e., equifinality). These multiple pathways embrace a number of possible contributors that may potentiate or mediate the links between early neurodevelopmental anomalies and schizophrenia in genetically vulnerable individuals. These include the normal developmental changes that take place during late adolescence and early adulthood such as: (a) synaptic pruning of the prefrontal cortex (Feinberg, 1982; McGlashan & Hoffman, 2000), (b) pubertal increases in gonadal hormones during adolescence (Spear, 2000), (c) developmental transformations in prefrontal cortex and limbic brain regions (Keshavan & Hogarty, 1999), (d) continued myelination of intracortical connections (Benes, 1989; Gibson, 1991; Yakovlev & LeCours, 1967), (e) alterations in the balance between mesocortical and mesolimbic dopamine systems (Benes, 1989, 1994, 1997), (f) the stress that arises during postnatal social development (Keshavan & Hogarty, 1999; Walker & Diforio, 1997), (g) the transformations that occur in cognitive and social-cognitive development (Keating, 1990; Noam, Chandler, & LaLonde, 1995; Spear, 2000), and (h) the growing importance of the peer group (Spear, 2000). Such an integrative, interdisciplinary approach is necessary to capture the full complexity of schizophrenic illness, including the multiple pathways to, and the diverse outcomes associated with, the disorder. As Cicchetti and Tucker (1994a) pointed out, such an interaction among these various changing develop-

mental domains must also take into account the individual's active strivings for selforganization that critically impact on brain development. Relatedly, such a perspective suggests that specific treatments should be developed and implemented for use at particular developmental stages, pre-, post, and during illness episodes.

As Cannon (1998) noted, to date no study has demonstrated a direct link between a molecular or cellular event during brain development to the etiology of schizophrenia. Consequently, the evidence of schizophrenia as definitively being a disorder of neurodevelopment remains circumstantial. An alternative account of the evidence may be that the neuropathological anomalies reflect a brain that was initially normal but that later encountered some pathological process, concomitant with or preceding the onset of psychosis. Nonetheless, if there is a dementia-like deterioration in the brain of individuals with schizophrenia, then it is not like those observed in known disorders of dementia. Specifically, markers of neuronal atrophy are not prominent and the rate of tissue loss is so slight as not to be detectable in the majority of investigations (Cannon, 1998). It appears likely that the processes underlying the normal development and maturation of cortical circuitry and connectivity may have gone awry in schizophrenia (Arnold, 1999; Benes, 1995; Marenco & Weinberger, 2000; Weinberger, 1987). Unraveling these misorganizations in brain development should contribute greatly to understanding the genesis and epigenesis of schizophrenic disorders.

FUTURE PROSPECTS

The process of "building a brain," whether it be a normal or an abnormal neural system, is a dynamic, self-organizing, genetic and epigenetic, multilevel phenomenon that unfolds from the prenatal period throughout the life course. Lander and Weinberg (2000) asserted that 20th-century biology triumphed because of its focus on intensive analysis of the individual components of complex biological systems. However, Lander and Weinberg believed that, in the 21st century, the discipline will need to focus increasingly on the examination of entire biological systems, through endeavoring to comprehend how component parts collaborate to create a whole. By taking this prescribed course of scientific action, Lander and Weinberg believed that, for the first time in over a century, reductionists will yield ground to investigators who are attempting to gain a holistic view of cells and tissues.

I believe that it is now time for the disciplines of developmental neuroscience, developmental psychology, and developmental psychopathology to conduct longitudinal investigations that examine the same individuals

at multiple levels of analysis over the course of epigenesis (Cicchetti & Cannon, 1999; Cicchetti & Toth, 1998; see Cacioppo, Berntson, Sheridan, & McClintock, 2000, for a similar point of view). In this regard, there is a need for ongoing knowledge of the scientific advances generated in investigations of normal brain development. Research also must be undertaken to elucidate the fundamental neural mechanisms involved in the development of various mental disorders.

Studies with humans should be conducted to ascertain whether, and if so, how, specific environmental occurrences, such as the presence of serious psychopathology in caregivers or child maltreatment, selectively impact the development of children's various neurobiological systems as a function of the timing and duration of exposure to adverse experiences (Cicchetti, 1996; Cicchetti & Toth, 1995; Dawson & Carver, 2000). Relatedly, it is essential that researchers identify the precise aspects of parenting that foster optimal brain development and function in children (Bruer, 1999; Nelson, 2000c). Furthermore, additional molecular biological investigations on the structure and function of genes and proteins involved in neural proliferation, migration, and differentiation need to be conducted (Nowakowski & Hayes, 1999). Even scientists who are focusing on similar levels of analysis should engage in interdisciplinary investigations in order to develop more thorough answers to the questions they are pursuing. For example, neuroscientists investigating schizophrenia or manic-depressive illness may conduct postmortem histo-pathological studies of the brains of individuals with these mental disorders. These researchers should also join forces with scientists who are investigating the brain chemistry of schizophrenia and manic-depressive illness; through such a collaborative process, the study of the anatomy and the neurochemistry of the same brains will occur, leading to a multilevel understanding of the correlates and/or contributors to these mental disorders.

Such efforts will necessitate not only interdisciplinary collaborations, but also a move to a more interdisciplinary training of developmental scientists (Cicchetti & Toth, 1991; Pellmar & Eisenberg, 2000; Singer & Ryff, 2000). The former will help to reduce the schisms that so often militate against the quest to discover scientific truth. The latter would help to prevent the development of disciplinary dogma and parochialism and foster an early appreciation of the complex processes—genetic, neural, psychological, social-contextual, and cultural—that transact to bring about normal and abnormal development. It is as equally heinous to advocate a "brainless" developmental psychology or psychopathology as it is to promote a "mindless" neuroscience. Concepts derived from systems-theory views can be utilized to help develop curricula that ensure in-depth training and an ongoing respect for and encouragement of the interdisciplinary, multidomain perspective essential for comprehending the genesis

and epigenesis of normal and abnormal brain development. Moreover, an interdisciplinary, theory-driven training of our future developmental scientists will contribute greatly to ensuring that research is guided by pressing theoretical and practical questions and not merely by advances in technology.

In addition, now that it is evident that experience can impact on the microstructure and biochemistry of the brain, a vital role for very early and continuing neural plasticity throughout epigenesis in contributing to the development of, and recovery from, various forms of maladaptation and psychopathology is suggested. Research has revealed that some early lesions may not be easily reversible, despite the historically prevalent belief that brain insults occurring near the beginning of development were most amenable to reorganization and repair. Conversely, contemporary neurobiological research suggests that in some domains (e.g., sensory, motor, cognitive, memory, and linguistic) and in some areas of the brain, plasticity is possible, including new neuron generation, well into adulthood (Cicchetti & Tucker, 1994a, 1994b; Nelson, 2000b). Moreover, future research must be conducted to examine the limits of plasticity in the social and emotional domains (see Davidson, 2000, and Davidson, Jackson, & Kalin, 2000, for evidence for neural plasticity in the central circuitry of emotion). It also will be essential to discover the mechanisms whereby latent progenitor cells are controlled and glial cell activation is modulated, in order to elucidate the bases of the brain's self-repair processes across various neurobiological systems (Lowenstein & Parent, 1999). If scientists can discover the mechanisms underlying the neural plasticity of the circuits of specific domains in individuals with various high-risk conditions and mental disorders, then such information should provide crucial insights for prevention and intervention efforts.

In this regard, prevention research can be conceptualized as true experiments in altering the course of development, thereby providing insight into the etiology and pathogenesis of disordered outcomes. Relatedly, the time has come increasingly to conduct interventions that not only assess behavioral changes, but also ascertain whether abnormal neurobiological structures, functions, and organizations are modifiable or are refractory to intervention (Cicchetti, 1996). There is growing evidence that successful intervention modifies not only maladaptive behavior, but also the cellular and physiological correlates of behavior (Kandel, 1979, 1998, 1999).

Unlike the belief espoused by Huttenlocher (1984) that "intervention programs, to be effective, would have to be implemented during [the] early prenatal period, and certainly prior to school age, by which time synaptic and neuronal plasticity appears to be greatly diminished, if not totally lost" (p. 495), recent demonstrations of plasticity across an array of

developmental systems suggest that interventions have promise to exert ameliorative effects long beyond the early years of life (Bruer, 1999; Nelson, 2000c). A major implication of a dynamic developmental systems approach is that the implementation of intervention following the experience of trauma or an episode of mental illness should ameliorate the intensity and severity of the response to the illness, as well as the illness course (Toth & Cicchetti, 1993, 1999). Such interventions that are closely timed to trauma and disorder onset also should decrease the probability of developing, in a use-dependent fashion, sensitized neural systems that may cascade across development (Post et al., 1998).

As Nelson (2000c) aptly articulated, "the efficacy of any given intervention will depend on the capacity of the nervous system (at the cellular, metabolic, or anatomic levels) to be modified by experience" (p. 204). Likewise, Nowakowski (1987) asserted that "in order to understand how a modification in a developmental process exerts its influences, it is essential to know where the developmental process is being modified, how the structure of the mature brain will be changed, and how the structural changes that are produced will change the ability of the brain to process the information it confronts during a complex behavioral task" (pp. 568–569). Successful psychotherapy, behavioral therapy, or pharmacotherapy should change behavior and physiology by producing alterations in gene expression (transcription) that produce new structural changes in the brain (Kandel, 1979, 1999). For example, as discussed earlier, stress has been demonstrated to suppress the birth of new neurons in adulthood (see also Sapolsky, 2000), and serotonin has been shown to enhance the rate of neurogenesis in the dentate gyrus. Extrapolating from these findings, Jacobs and colleagues (2000) hypothesized that stress-induced decreases in dentate gyrus neurogenesis play an important causal role in precipitating episodes of major depressive disorder. Reciprocally, pharmacotherapeutic interventions for depression that increase the neurotransmission of serotonin work at least partly through their role in augmenting the birth of new neurons in the dentate gyrus, thereby contributing to the recovery from episodes of clinical depression.

Finally, a substantial research literature suggests that not all individuals who have the same vulnerabilities and who have encountered the same adverse experiences develop in the same fashion (Luthar & Cicchetti, 2000; Luthar et al., 2000; Masten & Curtis, 2000). For example, although a growing body of evidence suggests that maltreatment experiences can impact negatively on brain structure, function, and organization (Cicchetti & Tucker, 1994a; DeBellis et al., 1999; Pollak, Cicchetti, Klorman, & Brumaghim, 1997), it is highly unlikely that the brains of all maltreated children are uniformly affected (Cicchetti & Rogosch, 2001). Indeed, because some mal-

treated children function extremely well, even in the presence of adversity (Cicchetti & Rogosch, 1997), it is conceivable that maltreatment exerts different effects on neurobiological structure, function, and organization in these children than it does in the typical maltreated child. The impact of life events, such as child abuse and neglect, on brain microstructure and biochemistry may be either pathological or adaptive. Future research should ascertain whether the brain structures and function of resilient maltreated children indeed differ from those of their less resilient maltreated counterparts. Similar work on individual differences underlying neurodevelopment and behavioral outcomes in other high-risk groups of individuals should be conducted (see also Davidson, 2000, in this regard). The fact that interventions are able to exert beneficial effects beyond the early years of life suggests that there is a psychobiology and a neuropsychology of hope and optimism that await discovery.

ACKNOWLEDGMENTS

My work on this chapter was supported, in part, by grants received from the Spunk Fund, Inc. I also want to thank Bill Hartup, Chuck Nelson, and Rich Weinberg for their comments on an earlier version of this chapter. I dedicate this work to my sister, Candace Cicchetti.

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3

The Symbol-Mindedness of Young Children

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> *A symbol is something that someone intends to stand for or represent something other than itself.*

There are two reasons why it is important to start this chapter on the early development of symbolic understanding and use with this definition of a symbol. The first is to make it clear exactly how I am using the term, given the wide historical variation in its usage; the second is to review what some others have had to say about symbolization and its development.

With respect to the first point, it is very important to specify both how the term *symbol* is and is not being used. Much confusion has arisen from the unfortunate fact that many different people in many different disciplines have used both *symbol* and *representation* in many different ways.

To begin with, *symbol* has been used by many psychologists to refer to purely internal, mental representations—the encoding of experience in memory (e.g., Newell & Simon, 1972). It is also used for language and some gestures; indeed, most writers on the topic of symbolization—from Peirce (1897, 1903/1955) to Langer (1942) to Deacon (1997)—have been primarily interested in language. Finally, symbol is also used to refer to a variety of external representations, artifacts created to serve as vehicles of thought (Vygotsky, 1978).

Representation is sometimes used as the superordinate category to which symbols belong. For example, Piaget (1951) distinguished between two senses of representation: "In its broader sense representation is iden-

tical with thought.... In its narrower sense, representation is restricted to the mental or memory image, i.e., the symbolic evocation of absent realities" (p. 67). Mandler (1983) also distinguished between broad and narrow senses of representation. The former refers to "knowledge and the way it is structured," whereas the latter is specifically symbolic representation—"words, artifacts, or other symbolic productions that people use to represent (to stand for, to refer to) some aspect of the world or some aspect of their knowledge of the world" (p. 420).

Dividing the representational pie somewhat differently, Bruner, Olver, and Greenfield (1966) argued: "There are two senses in which representation can be understood: in terms of the *medium* employed and in terms of its objective" (p. 6). The medium includes three pieces: *enactive* (motoric), *iconic* (e.g., pictures, images), and *symbolic* (language) representations. These three forms of representation develop in sequence, culminating in "representation in words and language. Its hallmark is that it is symbolic in nature" (Bruner, 1965, p. 11).

In stark contrast to the equation of symbol and language is Sigel's (1970) position: "My definition of representation does not include language since language is considered as a system of signs that can function to evoke representations" (p. 111). Yet another take on the relation between symbol and representation comes from Wolf and Gardner (1981). Representation is "the ability to reconstruct or recall information to guide behavior," whereas symbolization is "the ability to convert such information into observable forms that refer to, rather than simply guide, experience" (p. 295).

Noting that "Representation and its derivatives have many other uses ...," philosopher Nelson Goodman (1976) attempted to clarify his own view with respect to pictures: "Some writers use 'representation' as the general term for all varieties of what I call symbolization or reference, and use 'symbolic' for the verbal and other nonpictorial signs I call non-representational" (p. 4). I leave it to the reader to judge how much clarification was achieved.

Further confusion is introduced by the fact that different theorists distinguish in different ways among terms such as *symbol, sign, signal, index,* and *icon.* In one of the most influential efforts to chart the representational landscape, the philosopher Charles S. Peirce (1897, 1903/1955) proposed three categories of sign relations: icon, index, and symbol. An icon is linked to its object through similarity or resemblance, whereas an index is related to something via temporal or spatial association. A good example of an icon is a picture, and an example of an index is a weathervane indicating the direction of wind. Symbols, for Peirce, involve purely arbitrary, formal, conventional relations to what they stand for. The prototypical case of symbols is language.

Although Peirce, Bruner, and other theorists reserved *symbol* for arbitrary, noniconic relations between entities, others have argued strongly that iconicity per se is irrelevant to whether something serves a symbolic function (Goodman, 1976). The relation between signs and symbols also differs from one writer to another: For example, unlike Peirce, Werner and Kaplan (1963) took signs to be lower level representations than symbols.

Perhaps of most importance, however, is the simple fact that, in spite of this great variability and confusion in how we talk about symbolization, we do talk about it. We talk about it a lot, because it is so important. Indeed, more "philosophic ink has been spilt over attempts to explain the basis for symbolic reference than over any other problem" (Deacon, 1997, p. 43).

Here I spill some more metaphorical ink in an effort to reach a better understanding of the very early development of symbolization. I focus primarily on very young children's understanding and use of symbolic artifacts.

FOUR COMPONENTS OF *SYMBOL*

The definition with which I started—"a symbol is something that someone intends to stand for or represent something other than itself"—contains four elements, each of which is worth considering closely:

1. *Someone:* First and foremost is the component *someone.* Humans are "the symbolic species" (Deacon, 1997). Our remarkably creative and flexible use of a vast array of symbols is a unique and distinctively human talent. As the philosopher Suzanne Langer (1942) noted, "It is the power of using symbols ... that makes [humans] lord of the earth" (p. 26). Symbolization is the "most characteristic mental trait of mankind" (p. 72). Our symbolic capacity vastly expands our intellectual horizons, liberating us from the constraints of time and space. Unlike any other creature, we can mentally exist in the present moment, the past, or the future, in this place or some other. We collect an enormous amount of information about reality without directly experiencing it, adding "the experiences of other people to [our] own" (Langer, 1942, p. 29).

The emergence of the symbolic capacity irrevocably transformed our species in the course of evolution (Deacon, 1997). It eventually made possible the historical development of culture and the preservation and transmission of knowledge from one generation to succeeding ones (Vygotsky, 1978). So too does the emergence of symbolization transform children in the course of their individual development. The early German psychologist Wilhelm Stern identified as "the greatest discovery in the child's life"

the recognition of the fact that words have meaning (cited in Vygotsky, 1978, p. 23). Vygotsky concurred: Symbols are vital cognitive tools, and "the most significant moment in the course of intellectual development... occurs when speech and practical activity ... converge" (p. 24).

Becoming symbol-minded is thus a universal developmental task. All children growing up anywhere in the world must master the symbols and symbol systems that are crucial for full participation in their society.

2. *Something:* The very indefinite term *something* is used quite deliberately to accord with Goodman's (1976) assertion that "almost anything may stand for almost anything else" (p. 5). Virtually any entity can serve as a symbol. Spoken words, printed words, pictures, video images, numbers, graphs, a block of wood, a chair in a store window, maps—the list is infinite. Similarly, virtually anything can be symbolized, that is, can serve as the *referent* of a symbol. However, several authors have emphasized that it is anything *for which one has a concept* that can serve as a referent. Huttenlocher and Higgins' (1978) statement that "A symbol brings to mind something other than itself ..." (p. 109) presupposes the existence of a mental representation that is activated by the symbol. Potter (1979) explicitly stated that a symbol refers to or represents a "mental *concept* of an object, rather than the real-world object" (p. 56).

3. *Represents or stands for:* Symbols *represent;* they refer to, they denote, they are about something. "[Symbols are] entities which subserve a novel and unique function, the function of *representation.* The function of representation is a constitutive mark of a symbol...." (Werner & Kaplan, 1963, p. 13). "Symbolization is the representing of an object or event by something other than itself" (Potter, 1979, p. 41). "Denotation is the core of representation" (Goodman, 1976, p. 5).

It probably goes without saying that symbol and referent are always different entities; the referent is always something "other than" the symbol itself. It is probably worth saying, however, that a necessary element of symbolization is being able to distinguish between symbol and referent. Indeed, if for some reason one cannot tell two entities apart, one cannot be a symbol for the other.

A distinctive feature of symbol-referent relations is that they are inherently asymmetrical. It is typically the referent to which the symbol is designed to draw attention. This asymmetry holds even when symbol and referent resemble one another. As Goodman (1976) noted, "while a painting may represent the Duke of Wellington, the Duke doesn't represent the painting" (p. 4). A scale model of the White House is always the symbol, the real structure always the referent. An important source of this asymmetry is the fact that symbols and their referents typically have different affordances: One can shake the hand of the real Duke, but not his representation; celebrities can spend the night in the real Lincoln's Bedroom,

but not in its model.

We can reason in either direction. Driving in an unfamiliar town, we can consult a map to try to figure out where in the world we are; and we can also look around at our surroundings to try to figure out where on the map we are. However, the map–world relation is invariant. The "designative function [of symbols] involves a fixed functional relation ... regardless of the direction of activation (Huttenlocher & Higgins, 1978, p. 105). Indeed, it may be this ineluctable asymmetry that accounts for the charm of comedian Steven Wright's concept of a "life-size map of the world."

4. *Intends:* The human element comes back into the definition with respect to intention: A symbolic relation exists only if *some person intends* for that relation to exist. The *necessary and sufficient* condition is that someone has stipulated a referential relation between two entities. "In order for a symbolic relationship to be established, an *intentional act of denotative reference* is required" (Werner & Kaplan, 1963, p. 21). Further, "when a symbolic vehicle is taken to 'represent' a referent,... [it] functions to 'depict' or 'reveal,' through some sort of correspondence or analogy, the connotational structure of the referent. It should, however, be emphasized ... that correspondence or analogy is not... 'given objectively' in and by itself, but is *established through an intentional act ..."* (Werner & Kaplan, 1963, p. 15). Thus, human action is at the heart of symbolization. Nothing is inherently a symbol; only as a result of someone using it to denote or refer does anything become a symbol. "Psychologically, any item that is to have meaning must be *employed* as a sign or a symbol; that is to say, it must be a sign or a symbol to someone" (Langer, 1942, p. 53).

The centrality of intention has recently been emphasized in other domains as well. For example, Tomasello (1999) believed that crucial to early language learning is the child's appreciation of the intention of the other person. He claimed that: "joint attentional scenes are defined intentionally; that is, they gain their identity and coherence from the child's and the adult's understandings of 'what we are doing'" (p. 98). Bloom (1996) made the case that artifacts in general, including pictures, are defined by the intention of their creator: "In general, we construe the extension of the kind *picture of an X* as all and only those pictures created with the intention to represent an X" (p. 6).

The pivotal role of intention in symbolization is nicely illustrated by a photograph of a Holstein (black-and-white spotted) cow named "Miss USA" that appeared in *National Geographic.* The reason for the unusual (for a bovine) appellation is clear from the picture: Sprawling across the left side of the cow is a large white spot in the shape of the United States. The spot looks, to a truly remarkable degree, like a map of the United States. (The authors of the article were unable to restrain themselves—as am I— from referring to this as an outstanding example of "cowtography.") The

presumed reluctance of anyone to call this shape a symbol testifies to the criterial status of intention in symbolization.

THE DUALITY OF SYMBOLS

As the preceding discussion shows, symbols are unique entities. An important aspect of their uniqueness is their inherently dual or double nature, a fact upon which many have commented (Kennedy, 1974; Potter, 1979; Sigel, 1978). "The distinctive mark of the concept [symbol] ... is its inherent duality:... a symbol entails a 'vehicle' which, through its particular formal and qualitative properties, represents a 'referent,' that is, an object, a concept, or a thought ..." (Werner & Kaplan, 1963, p. 16). The paradoxical, dual nature of pictures has often been noted: "A picture is both a surface in its own right and a display of information about something else" (J. J. Gibson, 1979, p. 282). "Pictures are unique among objects; for they are seen both as themselves and as some other thing, entirely different from the paper or canvas of the picture" (Gregory, 1970, p. 32). Ittelson (1996) made the case particularly clearly:

> A picture, no matter how "realistic" or "representational," always presents two broad classes of visual information: (1) information that would be provided by viewing the pictured real-world scene ... and (2) information that is unrelated to the pictured scene but comes from the real-world surface on which the picture appears.... These two types of information can be analyzed separately by the psychologist, and they can be decoupled by the observer, but they are always encountered together. (pp. 175–176)

DUAL REPRESENTATION OF SYMBOLS

The dual nature of symbolic objects themselves requires that both aspects be represented for the object to function as a symbol. I have proposed the necessity for *dual representation:* To understand and use a symbol, one must represent both facets of its dual reality, both its concrete characteristics and its abstract relation to what it stands for. Very similar ideas were previously put forth by Sigel (1978), as well as by Potter (1979), who cited the need to "apprehend the identity of the symbolic medium at the same time as the identity of the referent" (p. 60). With respect to pictures, J. J. Gibson (1979) mentioned "two kinds of apprehension that go on at the same time" (p. 283).

The need for dual representation is the source of a great deal of the difficulty that very young children have understanding and using a variety of symbolic artifacts. According to Potter (1979), "A child's limited ability

to hold two or more things in mind simultaneously may prevent him from realizing the relation between the properties of the symbol and those of the referent. Either he focuses on the conceptual referent, or he looks at the symbol as an object...." (p. 60).

It follows from the concept of dual representation that some symbolic artifacts would be more difficult than others for young children. The more an object draws attention to itself, to its inherent physical characteristics, the more difficult it is to appreciate its abstract function as a representation of something other than itself. A good symbol, according to Potter (1979), is "deliberately self-effacing: one reads the message and ignores the medium" (p. 59). This idea was expressed in an especially compelling way by Langer (1942): "A symbol which interests us *also* as an object is distracting. It does not convey its meaning without obstruction.... the more barren and indifferent the symbol, the greater is its semantic power. Peaches are too good to act as words; we are too much interested in peaches themselves" (p. 75).

To achieve dual representation, children must avoid becoming "captured" by the symbolic object itself; they must psychologically distance themselves from it. The idea of psychological *distancing* was originally proposed by Werner and Kaplan (1963) and substantially elaborated by Sigel (1970,1990).

DUAL REPRESENTATION AND DEVELOPMENT

The challenge that dual representation poses is rarely evident in modern adults: We have all had such extensive experience with such a vast array of symbols that most seem transparent to us. But the importance of dual representation, and the challenge involved in achieving it, become much more obvious when examined in the context of development. It is revealing to look both at the *historical* development of symbol use and at the development of early symbolization in young children.

Dual Representation in Mesopotamia

According to archaeologist Denise Schmandt-Besserat (1978), the ancient Sumerians, around 8500 BC, started keeping track of agricultural trades with small tokens fashioned out of clay. Each shape stood for one of a particular category of agricultural product (sheep, sacks of grain, jars of olive oil, etc.), so three spheres might mean that three goats had changed hands. After a few thousand years, the Sumerians were trading more broadly and needed more permanent and transportable records than a pile of tokens, so they began sealing the tokens up in clay containers. They would

take a sheet of damp clay, put the tokens on top, and then fold it up to make a container for the tokens. This practice resulted in a permanent, transportable record, but there was one obvious problem—the record could be inspected only by destroying it, by breaking open the container. After a few decades, they solved this problem; they pressed each token into the sheet of clay before folding it, leaving the imprint of the tokens on the outside of the container. Now one could tell what tokens were inside the container—and hence what goods had been traded—just by inspecting the marks on the outside. The next insight took another hundred years, when the Sumerians realized they could dispense with the tokens altogether and just incise marks on the clay. Eventually, according to Schmandt-Besserat, both number representations and writing evolved from these clay tablets.

This archaeological account provides a wonderful illustration of the difficulty inherent in the dual nature of symbolic artifacts. Long after the Sumerians had taken the giant leap of creating a set of symbols, they still had trouble realizing how fully abstract their system could be. Their thought remained tied to the original symbolic objects. Thus, in our cultural history, the development of symbolic artifacts was a long and complex process characterized by difficulty coping with the dual nature of symbols. We turn now to young children's understanding and use of symbolic artifacts, where we also find that development is slow, complex, and characterized by difficulty coping with the dual nature of symbols.

Dual Representation in the Modern World

Perhaps one reason the Sumerians had so much difficulty achieving psychological distance from their original symbolic creations is that they had relatively little experience with symbolic artifacts in general, a situation extremely different from that in which modern people find themselves. In industrialized, technologically advanced societies, we are immersed in symbols all our waking hours. This point was made forcefully by Ittelson (1996) with respect to visual representations:

> As I sit here at my breakfast table, my morning newspaper has **printing** on it; it has a **graph** telling me how the national budget will be spent, a **map** trying to tell me something about the weather; a table of baseball **statistics,** an engineering **drawing** with which I can build a garden chair, **photographs** of distant places and people, a **caricature** expressing what the editor thinks of a political figure.... On the wall in front of me hangs ... a **calendar** [and above it] is a **clock.** All this and more, and I haven't even turned on the **TV** or the **computer**... (p. 171, emphasis added).

The same massive exposure to symbolic artifacts is true for young children in our society as well. Young children, and increasingly even infants, are exposed to picturebooks in frequent parent–child reading sessions, and pictures adorn the walls and other surfaces of most homes. Television and representational toys are nearly ubiquitous. Given children's enormous exposure to symbolic artifacts in their daily lives and the crucial role of symbolization in modern societies, it is important to understand how children initially respond to this unique class of objects and how they develop a mature understanding of them. The course of development turns out to be very complex—and very interesting.

The complexity of symbolic understanding stems in large part from a variety of factors that make it more or less difficult to achieve dual representation. In addition, there is a substantial amount of conceptual knowledge about symbols that must be acquired—knowledge we might characterize as the *that, what, how,* and *who* of symbols. Mature symbolic understanding with respect to any particular symbolic artifact involves realizing *that* it stands for something other than itself, that it is supposed to be interpreted with respect to its referent. Adults in modern societies immediately and automatically categorize any picture they encounter as merely a representation, so a highly realistic photograph of a charging elephant—something that in real life would be terrifying—is not for a moment frightening. One must also know *what* the content of the symbol is; one has to recognize and interpret that content. Our exemplar picture is immediately interpreted as an elephant. One must also appreciate *how* the symbol is related to its referent, the nature of the symbol-referent relation. Most adults know a great deal about the relation between color photographs and reality. They could readily infer that the photograph is of a real elephant that exists or once existed, that a person with a camera stood at some unknown distance in front of the elephant, and so on. Finally, mature understanding also includes some appreciation of *who*—recognizing that someone has some intention or purpose in using a symbol.

It might seem that the developmental course of understanding these four components would be straightforward, but it is not. Which components are understood at any given time depends not only on the age of the child, but also on the symbolic medium and how the symbol is used. This point is illustrated by research I and my colleagues have done on infants' and young children's understanding and use of two kinds of symbolic artifacts: one kind—pictures—is prominent in the everyday environment and highly familiar to most young children; the second—scale models—is relatively unfamiliar and uncommon outside the psychological laboratory. In all this research, the symbols—pictures and models—are highly iconic. The pictures are highly realistic color photographs, and the models are constructed to be as similar as possible, except for size, to the larger

spaces they represent. As a result, understanding *what*—the content of these symbols—is easy, even for infants and very young children. However, a high level of similarity between symbol and referent may simplify the child's task of detecting the relation between them, but definitely does not remove the challenge involved in achieving dual representation. The research I review helps us understand more about how infants and young children master the *that, how,* and *who* of symbolic objects. It focuses particularly on how and when the dual nature of symbols and the role of human intention is gradually understood.

GRASPING THE NATURE OF PICTURES

Imagine that as you are sitting in a lecture hall, the image of a charging elephant suddenly appears in front of you. Would you leap up and flee? That's supposedly what the residents of a remote village in Uganda did around the turn of the century when a visiting Scottish missionary showed a lantern slide of a charging elephant (Deregowski, 1989). I'm fully confident that you would remain firmly rooted in your seat for three reasons. One is *perceptual*—specifically, the absence of many of the cues for three-dimensionality, as well as movement, that would indicate the presence of a real elephant. The second is *conceptual*—your extensive knowledge about pictures; you would automatically categorize this as a *picture* of an elephant, not a real one, and you would immediately conclude that this elephant has absolutely nothing to do with the immediately present world. Third, you have extensive *pragmatic* knowledge about the reasonably small likelihood of elephants in lecture halls, as well as the high likelihood of a slide accompanying a lecture. So, because of your knowledge, and in spite of some aspects of your perception, you would not for one moment entertain the hypothesis that a huge elephant was actually bearing down on you.

Why would you, and any other adult with pictorial experience, so readily interpret appropriately the charging elephant picture? That was the subject of a long-standing debate about the role of experience in the perception and interpretation of pictures. According to J. Gibson (1971, 1979), pictures provide much of the same information available from the three-dimensional world, and an observer "picks up" stimulus information from a picture in much the same way he or she picks up information from the environment. As a result, no special picture-perceiving abilities are necessary to recognize a picture. In contrast, Goodman (1976) argued that, "Pictures in perspective, *like any others* [italics added], have to be read; and the ability to read has to be acquired" (p. 14). Art historian Ernst

Gombrich (1974) agreed that picture perception requires learning a code to interpret the somewhat arbitrary correspondence between pictures and the world. Some psychologists agree: "It requires practice to see the meanings and the spatial relations in two-dimensional representations and displays" (Stone & Church, 1957, p. 329).

We thus have a classic debate of the "It's learned," "is not," "is so" variety. The truth is, as usual, both more complex and more interesting. There is clear evidence supporting the Gibson view that picture perception is unlearned, starting with the classic study by Hochberg and Brooks (1962) of their own son. The child was prevented from seeing any pictures until 19 months of age, at which point he was presented with a series of photographs and line drawings of familiar objects. In spite of his lack of experience with pictures, he had no difficulty identifying most of the items. Further evidence comes from several studies that showed that much younger infants can perceive and recognize pictures of people (Barrera & Maurer, 1981; Dirks & E. Gibson, 1977) and objects (DeLoache, Strauss, & Maynard, 1979). Even newborns are able to recognize pictures of simple shapes (Slater, Rose, & Morison, 1984).

At the same time, other evidence supported the learning view espoused by Goodman and Gombrich. From the turn of the century on, anthropologists recorded a variety of confusions experienced by pictorially inexperienced adults on their first encounters with pictures (see Deregowski, 1989). One of the most vivid and frequently cited (although possibly apocryphal) accounts was of the reaction of residents of a remote Ugandan village to the lantern slide show presented by a visiting Scottish missionary: "When all the people were quietly seated, the first picture flashed on the sheet was that of an elephant. The wildest excitement immediately prevailed, many of the people jumping up and shouting, fearing the beast must be alive, while those nearest to the sheet sprang up and fled ..." (Deregowski, 1989, p. 58).

A resolution to the "is learned/is not" debate was proposed by Sigel (1978, 1990), who argued that previous researchers had confused the *recognition* and *comprehension* of pictures. As the infant research revealed, pictorial experience is not necessary to identify or recognize pictures of familiar entities. However, as the cross-cultural work and Sigel's own research revealed, older children and even adults do not necessarily understand the nature and use of pictures. Kennedy (1974) argued forcefully that the cross-cultural results often interpreted as a failure of picture perception were simply an investigative response to a very unfamiliar kind of object: "Photographs are clearly special objects. Would not anyone meeting a photograph for the first time be puzzled, not know quite what to say, but certainly deny that it was, physically, the represented object?" (p. 67)

Sigel (1990) later observed, "I felt very much alone in arguing that picture comprehension is an important cognitive achievement for all children and not one to be taken for granted" (p. 91). He has had a presumably gratifying amount of company in recent years, as researchers have provided further evidence of incomplete understanding by young children of the nature of pictures, particularly the relation between pictures and reality. Preschoolers sometimes say, for example, that a picture of ice cream would feel cold to the touch (Beilin & Pearlman, 1991) or that a photograph of a display of objects would change if the objects themselves were changed (Robinson, Nye, & Thomas, 1994; Zaitchik, 1990). Thus, young children have a very tenuous grasp on the ways that pictures are and are not like their referents.

At the same time, young children display fairly sophisticated understanding of one important aspect of pictures—their intentional basis. For example, Gelman and Ebeling (1998) showed that very young children think something is a picture only if it was intentionally created. They showed 2½- to 3½-year-old children simple line drawings and told them either that someone had worked very hard to make it or that it had come about as the result of an accident, such as someone spilling some paint. When the children were asked, "What's that," they named the depiction ("That's a bear") when it had been described as intentionally created. When it was described as the result of an accident, however, they usually did not attach a name to the shape, often referring to it in other ways, such as, "some paint."

Bloom and Markson (1998) provided another example of young children using intention to judge pictures. Three- and 4-year-old children were asked to draw four pictures—a balloon, a lollipop, themselves, and the experimenter. Children of this age are not very skilled at drawing, so their drawings of the balloon and lollipop were pretty much indistinguishable, as were their renditions of the two people. However, when later asked to name the drawings they had produced, the children reliably applied the correct labels. Whatever they had *intended* to be a balloon *was* a balloon, even though it looked just as much like a lollipop.

Back to Babies

This resolution to the historical debate—that learning is not required for the perception of pictures (at least relatively simple pictures) but is required to understand the nature and use of pictures—turns out to be the resolution to another mystery. For a long time, anecdotes and informal accounts appeared here and there of infants or young children confusing a picture with its referent. For example, Church (1961) described this striking sight:

> [We see] the child's first reaction to two-dimensional patterns such as the design on a playpen pad, spots of sunlight on the floor, or a stain on the woodwork: he tries, stubbornly and persistently, to pick them up. The child cannot at first recognize pictured objects. When he does, he distinguishes poorly between pictures and the objects they represent. Not only does he try to lift them off the printed page, he pets the pictured animal and tries to hear the ticking of the pictured watch. (p. 10)

Ninio and Bruner (1978) described one infant's efforts to grasp depicted objects, and Murphy (1978) reported that, in a study of mother-child picture-book interactions, 9-month-olds sometimes "hit the pictures in the book and scratched at the pages as if trying to lift the picture from the page" (p. 379). More recently, Perner (1991) recounted his 16-month-old son's efforts to fit his foot into the picture of a shoe. A colleague described her 20-month-old daughter's reaction to seeing a snapshot of her father that had been put under glass on the top of a play table in her preschool: "Chloe became distraught, pointing to her father and crying: 'Daddy out. Open. Open. Daddy out.' When she could not be soothed, the teacher removed the picture from the table and handed it to her. At first, she tried to liberate her father from the photograph with her fingers. Giving up, she hugged the picture to her face" (DeLoache, Pierroutsakos, & Troseth, 1996, p. 12).

Such anecdotes sound like they could be related to Werner and Kaplan's (1963) notion of *symbol realism*—"the tendency to treat entities which are patently symbolic ... as if they possessed the properties and the causal efficacy of the objects which they denoted" (p. 35). However, in the absence of any systematic investigation of infants' response to pictures, it was always hard to know what to make of these accounts.

Accordingly, I and my colleagues (DeLoache, Pierroutsakos, Uttal, Rosengren, & Gottlieb, 1998) began studying infants' reactions to pictures to see whether infants commonly react to depicted objects as if they were real or whether the various anecdotes and informal reports of such behavior represent unusual occurrences. We presented 9-month-olds with a book composed of highly realistic color photographs of single objects. To our surprise, every single one of the infants in our original study behaved like Perner's son, except that they used their hands instead of their feet. (See Fig. 3.1.) They felt, rubbed, patted, and struck at the pictures, and often they even grasped at the image as if trying to lift it off the page. Over the course of several studies, two infants have actually leaned over the picture of a baby bottle of milk to put their mouths on the nipple. Some infants were remarkably persistent in these behaviors, some made only tentative, fleeting efforts; but our results clearly established that infants' manual (and occasionally oral) exploration of depicted objects is a very real, very common phenomenon.

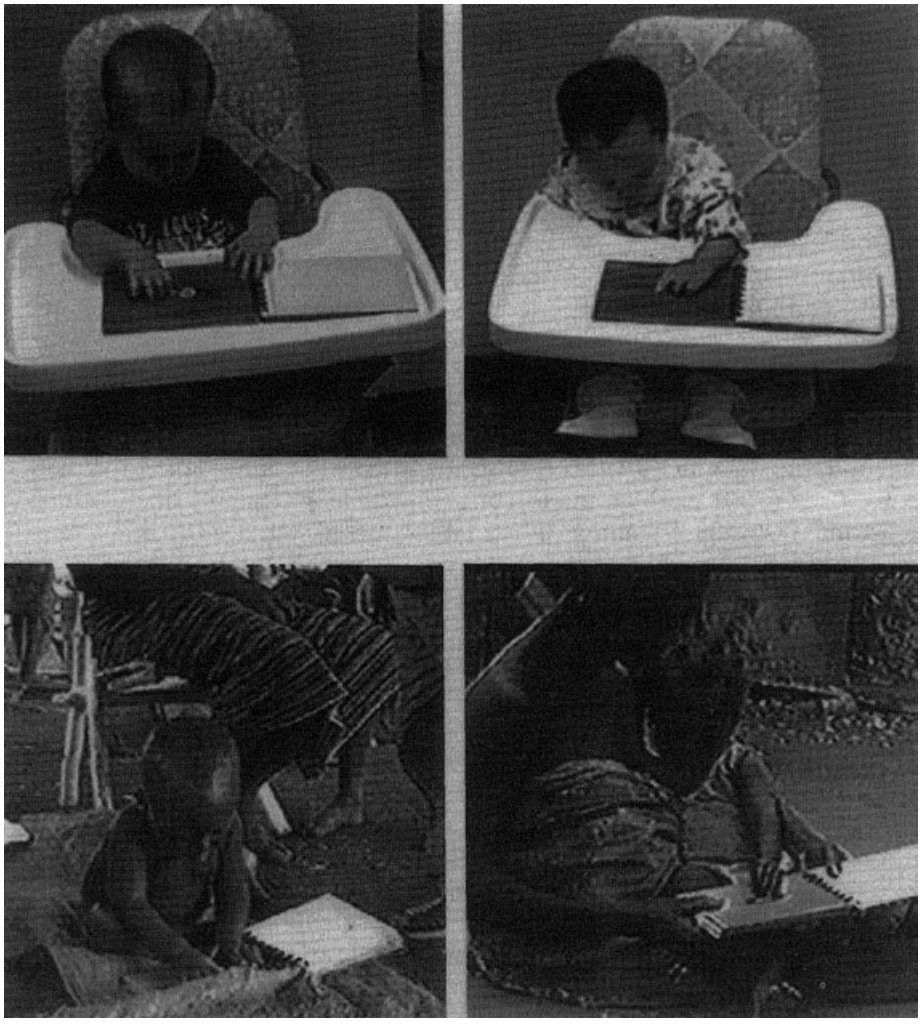

FIG. 3.1. Manual exploration of depicted objects by infants—two 9-month-olds from the United States and one 9-month-old (on the left) and a slightly older infant from Ivory Coast of West Africa.

Just how common became clear when the anthropologist in our group presented our books to some infants living in an almost picture-free society—an impoverished, rural village in Ivory Coast of West Africa. There were dramatic differences in the experimental setting from our lab in Urbana; for one thing, chickens and goats wandered through the testing site. Nevertheless, the Beng babies behaved just like the Americans. They felt,

patted, and rubbed the depicted objects and tried to pluck them off the page, even when, like the little girl in the figure, they were otherwise engaged.

Our results thus provide evidence that the behaviors casually observed long ago by Church (1961) are a common and easily elicited response of infants to highly realistic pictures. We disagree, however, with his belief that infants do not recognize pictured objects or distinguish between pictures and what they represent. Not only do the infant visual attention data argue against this view, but so do our results for manual exploration of pictures. Clearly, the infants who applied their lips to the nipple of the depicted bottle recognized it. Further, when offered a choice between a real object and a picture of that object, infants overwhelmingly choose the real thing.

We think the phenomenon of manual exploration of objects that we have documented reflects a *conceptual,* not a perceptual, deficit. Although infants have no problem perceiving pictures, and can discriminate between pictures and objects, they do not know *what kind of thing* a picture is. Not understanding the significance of two-dimensionality, infants respond to realistic pictures as if they were three-dimensional.

This idea is supported by a study (Pierroutsakos & DeLoache, 2000) showing that the extent of manual exploration by 9-month-olds is a direct function of the degree of realism in the pictures. Most exploration occurred to color photographs and least to black-and-white line drawings. Thus, the more a depicted object looks like a real object, the more infants treat it like a real object.

The infants in our research apparently do not share Goodman's (1976) view that, even when looking at highly realistic pictures, "I seldom suppose that I can literally reach into the distance, slice the tomato, or beat the drum" (p. 35). Our subjects' behavior belies Huttenlocher and Higgins' (1978) assumption that "... a person does not react similarly to icon and instance; he does not eat the picture of an apple ..." (p. 106). Some persons do, although only when they are very young.

Infants gradually figure out the nature of pictures. By 19 months, manual exploration occurs only rarely. Instead of trying to feel or pick up depicted objects, older infants point to them, sometimes naming the object (DeLoache et al., 1998). They have thus learned something about the nature of pictures; they've figured out what pictures are *not,* and something about their intended function. In the terms of Werner and Kaplan (1963), for these children pictures have undergone "a most significant transformation from things-of-action to objects-of-contemplation" (p. 18). I need to qualify the aforementioned. Infants in picture-rich societies like ours figure out the limitations of pictures during their 2nd year. It seems unlikely, however, that learning would be so rapid in societies—such as the Beng of Ivory Coast—in which infants have little or no exposure to pictures.

After American children, presumably as a function of spending hours and hours looking at picturebooks on their parents' laps, stop manually exploring pictures, they are still ignorant of one of the most basic pictorial conventions. Eighteen-month-old children simply do not care whether the picture they're looking at is right-side-up or upside-down (DeLoache, Uttal, & Pierroutsakos, 2000). Although they typically reorient an object that is handed to them upside-down, they are content to leave an upside-down picture as it is.

To return to the four components of mature knowledge of symbols, we can conclude that there is an innate ability to identify *what* is in a realistic picture. Infants gradually come to understand *that* depicted objects are not real, that pictures stand for something else. By around 3 years of age, children grasp the importance of *who;* they heavily weight intention in naming pictures. Coming to a full understanding of *how* pictures are related to their referents takes several more years.

THE MODEL BEHAVIOR OF YOUNG CHILDREN

We turn now to another line of investigation into the early development of symbolization in which children's mastery of the four components of symbolic knowledge takes a very different path. In this line of work, my colleagues and I have extensively studied very young children's use of symbolic artifacts as a source of information for solving a problem. We use a symbol—most often a scale model—to communicate the location of a toy hidden in the room. The model is highly iconic: It contains miniature versions of all the items of furniture in the room that look as much as possible like the corresponding larger ones and are in the same spatial positions.

First, we tell our young participants everything about the task. We go to great lengths to explain and demonstrate the relation between the two spaces. For example, we carry each miniature item of furniture from the model into the room, hold it up against its larger counterpart, and comment on the similarity between them. We explain that we will hide a miniature toy in the model and that a larger toy will then be hidden in the same place in the room.

After this extensive orientation, the child watches as the experimenter hides the miniature toy somewhere in the model (e.g., behind the couch, under a pillow). Next, out of the child's sight, she hides the larger toy in the corresponding place in the room. The child is then reminded that the larger toy is hidden in the same place in the room and is asked to go find it. After searching for the larger toy in the room, the child returns to search for the miniature toy in the model. Children of all ages are always highly successful at finding the toy in the model. In other words, they have ex-

cellent memory for the hiding event they observe directly. The same procedure is repeated for multiple trials, with the toy hidden in a different place each time.

We have repeatedly found that 3-year-old children readily appreciate the relation between the model and room (DeLoache, 1987; see DeLoache, 1995a, 1995b). They use their memory for where the miniature toy is hidden in the model to infer where to find the larger toy in the room. In contrast, 2½-year-old children seem almost completely unaware of the relation between the two spaces. They do remember the location of the small toy in the model, but they do not realize that it tells them where the larger one is in the room. This basic pattern of performance for these age groups has been replicated many times.

The younger children's failure in the standard task is so predictable that we learned to warn parents of 2½-year-olds that their children would probably not do well. The task looks so simple, the relation between model and room so obvious that parents are typically shocked when their child fails to find the toy. Indeed, it was even surprising to a former participant. I recently received a testimonial e-mail from 12-year-old Charlotte who had seen a PBS rebroadcast of a program in which she had appeared as a 2½-year-old failing the model task: "I can't believe I couldn't find the Snoopy. I think even my dog could of found him."

The fact that performance in the standard model task is so reliable and replicable does not mean that it is immutable. By manipulating the degree of iconicity, or similarity, between the model and room, we can easily make the standard task either easier or harder, enabling 2½-year-olds to succeed or causing 3-year-olds to fail (DeLoache, Kolstad, & Anderson, 1991). Giving children prior experience with a symbolic task that they understand produces dramatic improvement in their subsequent performance in more difficult tasks that they normally fail (Marzolf & DeLoache, 1994).

An important question to ask is, when children succeed in the model task, what is the basis for their success? It is clear that children must detect the lower order object correspondences; they have to recognize that the miniature chair is related to the larger chair, the little couch to the big one, and so forth. Mental representations of these lower order relations could, in principle, be adequate for success. However, several aspects of our results make it clear that representation of these lower order object correspondences is *necessary,* but not *sufficient,* for success in this task.

In one study that makes this point, the experimenter pointed to objects in the model and asked 2½-year-olds to "show me the one just like this" in the room. They were highly successful at doing so; when we pointed to the table in the model, they ran in and indicated the table in the room. Immediately afterward, we gave these children the standard model task. In spite of having just demonstrated their ability to detect the similarity between

all the objects in the two spaces, they failed. Although they knew that the small couch in the model was like the large couch in the room, they did not know that the fact that the miniature toy was behind the couch in the model meant that the large toy was behind the couch in the room.

Dual Representation and the Model Task

Thus, similarity does not a symbol make. Why not? I think the problem is largely due to the difficulty young children have achieving dual representation. Young, inexperienced symbol users are inclined to respond to novel entities primarily—and often exclusively—in terms of their physical reality. The more salient a symbol is as an interesting, attractive object—like a scale model—the more difficult it is for young children to achieve dual representation; that is, the more difficult it is for them to appreciate its higher order relation to what it stands for. They see the object—the model—but fail to see through it to the room it represents.

The dual representation hypothesis has received strong support from several studies. In the most dramatic one, we showed that 2½-year-old children could reason between two spaces when we eliminated the need for dual representation (DeLoache, Miller, & Rosengren, 1997). We did this by convincing 2½-year-olds that we had a machine that could shrink a room; that is, the machine could transform a room into a model. The basic idea is that if children believed us, then there would no longer be a symbolic relation between the two spaces and hence no need for dual representation. Believing the model to be the room, these young children (who would fail the standard model task) should be able to reason between the two spaces.

In the orientation to the task, each child was first shown "Terry the Troll" and "Terry's room" (a tentlike portable room used in many previous model studies). Then we told the children we had a machine that could shrink things. The troll was placed in front of the shrinking machine (which looked suspiciously like an oscilloscope). The machine was "turned on," and the child and experimenter waited in an adjoining area, listening to what was described as the "sounds the shrinking machine makes while it's working." When the child returned to the lab, a miniature troll sat in place of the original one. The child was then shown that the machine could also make the troll "get big again." We next demonstrated that the machine could also shrink and enlarge Terry's room. The machine was pointed at the room, and the child and experimenter waited in the adjoining area again listening to the sounds the machine made while shrinking the room. When they returned to the room, the small model sat in the middle of the large space previously occupied by the portable room. It was a very dramatic sight. We then demonstrated that

the machine could also enlarge both the troll and the model. The children gave many signs of believing us, and their accompanying parents judged that they did. Finally, we were ready for the actual experiment.

For the retrieval task, the child watched as the experimenter hid the larger troll somewhere in the portable room. After waiting while the machine shrank the room, the child was asked to find the hidden toy. On the next trial, the miniature toy was hidden in the model, and after it was enlarged, the child searched in the room. Thus, just as in our standard model task, the child had to use his or her knowledge of where the toy was hidden in one space to figure out where to search in the other. As predicted, performance was significantly better than that of a control group in the standard model task. Thus, when there is no symbolic relation between two spaces and hence no need for dual representation, 2½-year-old children can reason from one to the other.

Intention and the Model Task

Given the pivotal role of intention in the definition of a symbol, it is important to analyze the model task, and children's performance in it, in that context. The scale models that we use in our research are clearly designed by people to stand for a particular larger space. Although the model typically resembles the larger space it represents, it is the intentions and actions of the people using it that establish the symbolic relation between the two spaces. In other words, it is the fact that I intend that the model stand for the room that establishes that relation. Similarly, it is my intention alone that establishes the correspondence between the hiding events in the two spaces. Only because I intend that observed events in the room have significance for unseen events in the room is that the case. I make it so.

This suggests that part of a mature understanding of the model-room relation is recognition of the intention of the adult using the model as a representation of the room. Several questions follow: First, could we improve the performance of young children who typically fail the model task by emphasizing the intentional nature of the model-room relation? Second, at what age could children detect the nature of that relation on their own, that is, without the extensive explanation of the model-room relation that we typically provide? Third, to what extent are children who succeed in the task explicitly aware of the experimenter's intentions to make the events in the two spaces correspond?

Emphasizing the Intentional Nature of the Model-Room Relation. We have recently tried to improve the performance of two age groups of young children by highlighting the intentional nature of the model-room relation (Sharon & DeLoache, 2001a). To do so, we modified the standard task

in several ways: First, the children were given a "blind" trial in which they were told that Snoopy was hidden in the room and they should try to find him. The point was to make them realize they had no way of knowing where the toy was. Next a "helpful" assistant to the experimenter indicated that she could help the child know how to find the toy. She then proceeded to assemble the model of the room, commenting on the similarity between the two spaces and explaining how the model would help the child know where Snoopy was hidden. On the first hiding trial, after the experimenter entered the room to hide the larger toy, the friendly assistant assumed a conspiratorial stance, peeking through the door as the experimenter was hiding the toy. She then hid the miniature toy in the model, telling the child this would help her know where Snoopy was hiding in the room. Finally, the child searched in the room.

A group of 3-year-old children received this assistance in the context of the low-similarity task that their age group typically fails. They clearly benefited from the "helpful" experimenter's efforts to clue them in to the model-room relation. However, a group of 2½-year-olds, who were given the standard task that their age group typically fails, did not benefit from the helpful assistant's conspiratorial behavior. Thus, we find that emphasizing the intentional nature of the adults' actions in the task can assist 3-year-olds to appreciate the model-room relation, but, at least in this initial study, 2½-year-olds were less receptive to the intervention.

Children's Ability to Detect the Model-Room Relation on Their Own. We have discovered that it is not until 5 to 7 years of age that children can figure out the model-room relation on their own (DeLoache, DeMendoza, & Anderson, 1999). In this case, we simply showed them the model and the room and they watched a hiding event in the model. They were then told to find a larger toy that was hidden in the room. Unlike younger children, these children did not have to be told the purpose of the model to appreciate the symbolic relation between it and the room.

The children's comments were highly informative. Upon first seeing the model and room, some children commented on the resemblance:

"This looks the same. Everything's the same!"
"How do you make the same thing?"
"Hey, these rooms look the same."

Some seemed to catch on after the first retrieval trial:

"I think I know how it's done. I know the secret!"
"He's in the same place! Always in the same place.... That's funny!"

A few children voiced an explicit, full understanding of the situation:

"I know where you hide him, because the place you hide him is the same as the other place ... sorta the same room."
"I better remember where Little Snoopy is hiding because she might hide Big Snoopy in the same place."

One child was clearly thinking ahead; after watching the experimenter lift the miniature table to hide the toy under it, he worried: "I can't carry that when it's big." This child was clearly seeing through the model, thinking about searching in the room while observing the hiding event in the model.

This study shows that by 5 to 7 years of age, the model-room relation is relatively transparent to children, just as it is to adults. From simply seeing the two spaces and observing a hiding event in one of them, they infer the nature of the symbolic relation that the experimenter has established. They assume that the adult's actions that they observe in one space have implications for her unseen actions in a different space.

Children's Explicit Awareness of the Model-Room Relation. In another recent study, we asked whether young children's understanding of the intentional nature of the model-room relation is explicit enough to enable them to ignore irrelevant information (Sharon & DeLoache, 2001b). A group of 3½-year-olds participated in the standard model task, with two very non-standard trials interpolated among four regular ones. On the two "accidental hiding" trials, the children watched as the experimenter hid the miniature toy in the model as usual and then went into the room to hide the larger toy. At this point, a second, "clumsy" experimenter "accidentally" kicked the model, dislodging its contents. She replaced all the furniture in the correct locations. Then, picking up the miniature toy, she said, "Hmm, I don't know where this was; I'll just put it here." She proceeded to put the toy in a different place from where the experimenter had hidden it.

The question was whether the children would realize that the "accidental" hiding was irrelevant to where the larger toy was hidden in the room. In other words, would they realize that the intention of the first experimenter determined its location, not the last hiding event they had actually observed in the model.

The results were quite clear. Of the eight participants, six clearly understood that the appropriate guide to the location of the toy in the room was where the experimenter had originally hidden the miniature toy in the model, not where her clumsy sidekick had subsequently placed it. The children ignored the "accidental" hiding to the extent that their retrieval performance did not suffer at all. Thus, these children recognized the importance—and information value—of the experimenter's intentions in

hiding the two toys. We are currently examining whether younger children (3-year-olds) are equally sensitive to the intentional basis for the model-room relation.

THE DEVELOPMENTAL STORY

The research we have reviewed here reveals that progress in young children's use of symbolic artifacts as a source of information is relatively rapid. For example, 2-year-old children fail miserably at our basic picture task, but 2½-year-olds are very successful in it. Similarly, 2½-year-olds generally perform extremely badly in the basic model task, whereas 3-year-olds perform extremely well. What accounts for the fact that children who find a task impossibly difficult at one age find it trivially simple only 6 months later?

Age changes in our symbolic object-retrieval tasks are not all that must be explained. What accounts for the fact that children who find a task impossibly difficult at one age find that same task trivially simple after experience with a related one? Or how can children of a given age who find a task trivially simple find a *very* similar task impossibly difficult?

The research reviewed here makes it clear that there is substantial complexity involved in the understanding and use of simple symbol-referent relations. Therefore, it should not be surprising if the developmental story is similarly complex, involving change in numerous domains. We think this is exactly the case. It is unlikely that any one or two factors are by themselves responsible for the rapid development we have documented.

One factor that we know improves performance over time is prior symbolic experience, as shown by our transfer studies. We assume that general symbolic experience also contributes to more successful performance with age in our tasks. Most of the children who have participated in this program of research receive an enormous amount of exposure to a variety of symbolic artifacts in the first years of life: Among other things, most middle-class American toddlers enjoy numerous picturebook interactions with their parents and others, watch television, engage in pretense with representational toys, and begin drawing (although their representational intent is not usually obvious). None of these experiences involves using symbolic objects to solve a problem based in current reality, but we assume that their increasing understanding of and facility with such entities makes children more flexible when they come into our laboratory and are asked, for the first time, to use a symbolic artifact as a source of information about current reality.

Understanding intentionality is also likely to be a key contributor to developmental progress. An increasing body of research is establishing substantial understanding of intention by young children and even in-

fants, but such understanding no doubt expands dramatically in the 3rd year of life and for years to come. This probably makes it easier for children to figure out the basic nature of the symbolic object-retrieval task.

Another likely source of developmental progress is in the ease of achieving dual representation. Increasing inhibitory control may be very important. To achieve dual representation, a child has to inhibit responding to a symbolic artifact exclusively or primarily as an object. The more a young child responds to a scale model as an interesting toy, the less likely he or she is to appreciate its role as a representation of something else. General inhibitory control is known to increase during the first several years (Harnishfeger & Bjorklund, 1993). Basic brain development could be making an important contribution in this regard: Frontal lobe development is known to be important in inhibitory control, and it is also known to be proceeding rapidly throughout this period (Diamond, 1991; Welsh & Pennington, 1991).

Another important aspect of early development is a large increase in the basic amount of information that children can represent. Their steadily increasing speed of information processing and resulting growth in working memory (Kail, 1995) no doubt help young children cope with the cognitive demands of symbolic tasks. Both Case (1992) and Halford (1993) stressed the growing ability to represent multiple relations simultaneously. Any symbolic-retrieval task has, at a minimum, one more relation—the relation between symbol and referent—to represent than does any memory-based retrieval task (Marzolf & DeLoache, 1997; Troseth & DeLoache, 2001).

In addition, between the ages of 2 and 3, children undergo extensive language development. At a minimum, their increased language skills should make it easier for them to apprehend our instructions. Also, increasing linguistic capability should facilitate the achievement of psychological distance (Sigel, 1990) and hence of dual representation. The well-documented increases in analogical reasoning skills that occur in the early years of life (Chen, 1996; Gentner, Ratterman, Markman, & Kotovsky, 1995; Goswami, 1992) should be another source of developmental progress in symbolic retrieval tasks.

This list is by no means intended to be complete; rather, it serves to illustrate the assumption that the rapid developmental improvement that occurs in our symbolic object-retrieval tasks is almost certainly attributable to several converging lines of development. The account will not be simple.

PRACTICAL APPLICATIONS

I turn now to some practical implications of what we have learned about early symbolic development. Because adults assume that symbol-referent relations are more transparent than they actually are, many well-intended

efforts to assist young children miss the mark. One example is the anatomically detailed dolls that have often been used to interview young children when sexual abuse is suspected. It has simply been taken for granted that children would easily be able to use a doll to represent themselves, and the further assumption is made that the doll would help them provide a more complete and accurate memory report. Everything in this account of early symbolic development suggests that this assumption should be questioned, and indeed there is accumulating evidence from my own lab and several other investigators that children younger than 5 have substantial difficulty understanding and using a doll as a self-representation.

A second case concerns the manipulatives that are a staple of early math education. These are objects such as rods, blocks, counters, and so forth that young children are encouraged to use to help them solve problems and understand concepts. However, teachers too often assume that the relation between the manipulatives and what they are supposed to represent is transparent. As a consequence, teachers often provide little or no instruction about the relation between them. And as a consequence of that, their pupils often totally miss the point (Uttal, Liu, & DeLoache, 1999; Uttal, Scudder, & DeLoache, 1997).

This was vividly illustrated by 7-year-old Michael whose mother gave him a stack of candies to use to solve the arithmetic problem of 3 + 1 = ?. Michael obediently used the candies, but not precisely in the way his mother had in mind. He made a group of three, then constructed a plus sign out of five more, put a single candy after that, then used two more to form an equals sign, and finally made a group of four for the answer. He then proceeded to count on his fingers before giving his answer. Michael understood part, but only part, of what he was supposed to do with the candies, and it is almost certain that the arrangement he created did nothing either to help him solve this particular problem or to acquire a better understanding of addition.

We have thus come full circle, from the ancient Sumerians and their failure to fully appreciate the abstract nature of their tokens to a thoroughly modern child having a similar problem.

FUTURE DIRECTIONS

It seems likely that research into the development of symbolization will increase in the future, with respect both to issues of current interest in developmental psychology and to issues of more general concern to society. One particular area within developmental psychology that is ripe for further investigation is the role of children's understanding of intentionality in their understanding of symbolic artifacts. Intention is fundamental to

symbolization. As discussed here, I and my colleagues have recently begun examining this issue in the context of symbolic object-retrieval tasks, and others have been studying children's sensitivity to the role of intention in the interpretation of pictures. As increasing attention is paid to children's developing understanding of intentionality across a wide array of domains, it will be worthwhile to examine it further in the context of early symbolic development.

More generally, children in Western societies today are exposed to a wider variety of media and symbolic artifacts at younger ages than ever before. Some television programming is now directed specifically to toddlers, and preschool children are expected to begin learning numbers and letters. Computers are becoming increasingly common throughout society, and children are being exposed to them increasingly early. Full participation in society thus requires that children begin to develop several kinds of literacy quite early in life.

The fact that very young children receive more exposure to symbolic media than ever before means that it is more important than ever to know more about the processes involved in understanding and interpreting symbols. It would be valuable to have better information about how very young children can be helped to correctly interpret symbolically mediated information. Research designed to improve the instructional materials used in classrooms could be particularly beneficial.

ACKNOWLEDGMENTS

Preparation of this manuscript was facilitated by Grant HD-25271 from the National Institute of Child Health and Human Development.

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Understanding Understanding: Historical Origins of Current Questions About the Early Development of Receptive Language Competence

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Children learn to listen before they learn to speak. In a field prone to controversy, this claim is uncontroversial. The observation that infants first reveal knowledge of language by responding meaningfully to speech, often months before producing their first words, has been a starting point in scholarly discussions of language development for some 200 years. Yet beyond the commonplace acknowledgment that early signs of understanding are at the leading edge of language learning, much less is known about what infants can understand than about what they can say. The historical emphasis has been on speech production rather than on comprehension as the basis for inferences about developmental processes in language acquisition. An obvious practical reason for this asymmetry is that speaking is directly observable and understanding is not. Because comprehension is a mental event that must be inferred indirectly from a child's behavior in a particular context, the emergence of receptive language competence has been less accessible to scientific inquiry than the development of speech production abilities. However, research on infant cognition over the past three decades has provided valuable experimental methods for reading infants' minds. Such techniques have made it possible to begin exploring early understanding in ways that go far beyond the age-old observation that infants who cannot yet speak in fact know a whole lot more about language than they let on.

This chapter is organized in three parts. The first section describes some exemplary studies of receptive language competence from the first 150 years of research on language acquisition. These studies reveal a lively interest in the development of speech comprehension that was evident in early research but receded as the field began to focus increasingly on speech production. The second section examines a dozen research themes related to the development of comprehension that began to emerge more or less independently between 1970 and 1980, along with the new methods for their empirical exploration that developed around the same time. Although these diverse paradigms had roots in different disciplinary traditions and reflected a range of theoretical perspectives, they converged on many levels over the next two decades as studies of early comprehension began to flourish. The final section relates these converging themes and methods to four important discoveries about the early development of language understanding that are emerging from recent research.

EARLY RESEARCH ON THE DEVELOPMENT OF RECEPTIVE LANGUAGE ABILITIES IN INFANCY

Such keen observers of the human condition as Plato, Herodotus, and Augustine have found it interesting to speculate on how infants learn language, although it was not until the late 18th century that research on language acquisition began to take shape as an empirical enterprise. In 1787, the German philosopher Tiedemann published observations on the psychological development of his son from birth to 30 months of age, describing the child's gradual progress in understanding as well as in speaking. Tiedemann noted that at 6 months his son reacted to intonation positive or negative in tone and by 8 months he appeared to recognize the names of a few familiar objects, turning to search for the object when he heard the word. At 14 months the child could articulate a few words but did not yet appear to use them intentionally. Tiedemann concluded that at this age "words awakened in him their proper images and ideas, but not conversely, images of objects and desire of them, any concept of the corresponding word; primarily because children begin by learning words more for the sake of understanding the intention of others than in order to impart their own" (Tiedemann, 1787 / 1927, p. 221). This and other thoughtful comments in Tiedemann's diary study foreshadow theoretical questions that were to become central to discussions of language development in the 20th century, including issues of lexical representation, concept formation, and intentionality.

Tiedemann's work was republished in German, French, and English over the next 150 years, the first of several influential "baby biographies" that provided anecdotal data on linguistic development (see Leopold,

1952). In 1877 the diary studies of Taine and Darwin both appeared in the philosophical journal *Mind.* Although different in emphasis, each anticipated questions that have proven to be of enduring scientific interest. For instance, Taine noted that when his 11-month-old daughter was asked "Where is Mama?" she always turned toward her mother. This example echoed an observation by Tiedemann (1787/1927) 100 years earlier; when asked to "Make a bow" or "Swat the fly," his 8-month-old son also made appropriate gestures, although a different interpretation was offered. Whereas Tiedemann asserted that such responses showed how his son had "learned to comprehend" simple sentences, Taine (1877) was more cautious, suggesting that "there is nothing more in this than an association" (p. 253). By the age of 12 months, however, Taine was convinced that his daughter demonstrated true comprehension of the word *bébé.* Although it was clear from the child's responses that her understanding did not coincide with the conventional meaning, Taine claimed that *bébé* nevertheless had "a general signification" for her that went well beyond a limited association between a sound pattern and a gestural response.

Taine's (1877) article motivated Darwin to publish *A Biographical Sketch of an Infant* a few months later, based on diary observations he had made of his son Doddy 30 years before. Darwin had recently written *The Expression of Emotions in Man and Animals* (1872/1956) in which he explored the continuity of emotional signals across species, and this interest in vocal and facial affect was reflected in his recollections of Doddy's early development. Turning to his son's communicative abilities at the end of the essay, Darwin noted that "before he was a year old he understood intonations and gestures, as well as a few words" (1877, p. 294). Darwin also claimed that at 7 months the child understood a single word, his nurse's name, 5 months before he uttered his first word.

The originality of these early diary studies is easy to underestimate because they fall so far short of current standards of scientific rigor. However, originality and rigor have often traveled separate paths, in this field as in any other. The observations of Tiedemann, Taine, and Darwin reflect curiosity and wonder at their children's language development, years before this area was viewed as a legitimate object of study. Because there was no established research tradition, their commentaries ranged widely, unconstrained by received opinion about which questions were worthy of investigation. The diary studies are admirable for the imagination that inspired them and for their initial formulation of theoretically important questions that guided later research. They also contributed insights about first-language learning that were often overlooked in the more carefully controlled research that followed. For example, they balanced an interest in expressive development with an interest in advances in comprehension, whereas the emphasis in later more quantitative research shifted predominantly toward the study of speech production. And these studies

viewed the development of communication skills as continuous with affective processes in infancy, tracing the origins of understanding back to the infant's early emotional responses to speech.

In different ways, the diary studies all addressed a challenging question: When and how does the infant begin to understand spoken language? Tiedemann (1787/1927) was content to infer understanding from any appropriate behavior routinely shown by the child in response to parental speech. Taine (1877) proposed more stringent criteria, distinguishing between his child's regular and appropriate response to a particular word such as *bébé* at 12 months and earlier stereotyped responses to the situation in which *bébé* was spoken, which he dismissed as "association." This distinction was elaborated over the next 50 years by researchers who began to conduct informal experiments to explore how their infants came to associate spoken words with behavioral routines. In 1908 Meumann reported an anecdote about a father who had trained his 8-month-old son to turn toward the window in response to "Wo ist das Fenster?" When one day the father asked "Où est la fenêtre?" with the same intonation, the child made the same response. Schäfer (1922) explored this phenomenon more systematically with his own son, varying words, intonation, and gesture while keeping detailed notes of the child's response. For example, Schäfer trained his 9-month-old son to clap his hands when he heard the phrase "Mache bitte bitte," spoken in exaggerated intonation. At first the child responded only when his father performed the action while saying the words, but 2 weeks later the phrase alone was sufficient to elicit the response, as long as it was spoken with exaggerated intonation. By 10 months the boy would also clap when the phrase was spoken with normal intonation. Schäfer varied segmental content as well as prosody, reporting that a phonetically similar phrase such as "Kippe kippe" also provoked a response, although less similar sounds did not. Although his primary focus was on early speech production, Schäfer's informal experiments constituted the first parametric variations of segmental and supra-segmental dimensions of speech that influence the infant's early responses to spoken language.

Schäfer's (1922) observation that young infants pay special attention to intonation when learning to associate sound patterns with specific responses was of central importance to Morris Lewis in his 1936 monograph, *Infant Speech: A Study of the Beginnings of Language*. Lewis proposed that speech understanding originates in the infant's sensitivity to emotion expressed through intonation in the caretaker's voice. In one of the first experimental studies of infant social behavior, Bühler and Hetzer (1928) showed that infants are responsive to vocal emotion in the early months of life. Lewis made the case that infants are initially moved by prosody to respond to words on an affective level, and only gradually learn to uncouple phonetic patterns from prosodic form. Although bold and original

for its time, Lewis' work was also solidly situated in the intellectual landscape of the 1920s. He integrated the recent empirical findings of Schäfer, Charlotte Bühler, and others with ideas about learning and development that resonated with behaviorist notions as well as with principles of Gestalt psychology. In his engaging book, Lewis insisted on high standards for collecting child language data and offered a synthetic view of language development as grounded in affective communication, raising questions and ideas that are still provocative today (see L. Bloom, 1993).

Lewis' (1936) book was unusual in giving as much attention to the origins of speech comprehension as to early speech production at a time when the emerging scientific literature on language development was increasingly focused on how children learn to talk. The classic study of William and Clara Stern (1907/1928) reflected this emphasis. The Sterns provided exquisitely detailed observations of the language development of three children, documenting the growth of their vocabulary and the increasing semantic sophistication and syntactic complexity of their verbal productions. However, they paid much less attention of the growth of language understanding, reiterating the observation that some form of comprehension is evident before speech production begins at the end of the first year, and that infants may respond to verbal routines associated with movements without really understanding the words. Reflecting on Schäfer's (1922) findings, the Sterns speculated that such early responses to vocal routines might be mediated by associations between speech and gesture, but this hypothesis about the nature of early comprehension was only briefly mentioned.

It is hardly surprising that researchers in the 1920s and 1930s focused on speech production as the primary source of data on the growth of linguistic competence. The field of psychology was still emerging as a scientific discipline, with the goal of developing reliable quantitative methods and clearly operationalized measures that would generate replicable results. To meet these higher standards of evidence, psychologists interested in language development produced a stream of studies of children's speech with large numbers of subjects and predetermined criteria for counting words and categorizing parts of speech (see McCarthy, 1933). According to the behaviorist principle that the proper subject of study in psychology is not the mind but behavior, defined as the observable actions of people and other animals (see J. B. Watson, 1913), it was scientifically defensible to study the child's observable speech behaviors but not to speculate about inaccessible processes of understanding in the child's mind.

Developments in the field of linguistics in the 1940s also began to shape the agenda for research on child language. It was Leopold's comprehensive study on *Speech Development of a Bilingual Child,* published in four volumes between 1939 and 1949, that convinced linguists that children's speech productions were worthy of their attention. However, Leopold did

not view the recent outpouring of descriptive research on children's speech as a sign of progress, lamenting the "crushing bulk of data amassed in thousands of studies from many lands and many fields of scholarship" (1953, p. 1) that lacked theoretical motivation and conceptual coherence. He credited Roman Jakobson for bringing insight and direction to the field with his influential study *Kindersprache, Aphasie, und allgemeine Lautgesetze,* published in 1941 (Leopold, 1956). Jakobson (1941/1968) applied principles of structural linguistics to the analysis of phonological development in children, which he also related to language breakdown in aphasics and to the distribution of sound forms across the world's languages. Leopold made use of Jakobson's theory in his extensive analyses of patterns in children's speech production, and others followed. By the end of the 1950s, when Chomsky (1959) challenged the fundamental tenets of behaviorism in his critique of Skinner's *Verbal Behavior,* it was clear that linguistic theory was central to research on language acquisition.

At the 2nd Minnesota Symposium on Child Development in 1967, Roger Brown reported on a new research program that was completely unlike what had gone before, yet preserved the strengths of earlier approaches. Using the high-quality audio-recording methods that had only recently become available, Brown and his group collected detailed longitudinal data on the language of three children, observations that had all the richness of reports in the diary studies but that were much more extensive, reliable, and representative. These data were analyzed using quantitative measures that were carefully defined and theoretically motivated, with the goal of going beyond description to address two fundamental questions: "What does the child know of the structure of English at successive points in his development?" and "By what processes does he acquire his knowledge?" (Brown, Cazden, & Bellugi-Klima, 1968, p. 29). Although the behaviorists may have applauded the conscientious methods used in this research, the focus on "knowledge" was presumably unsettling. This new generation of language researchers made the claim that by studying speech productions, they could make inferences about the knowledge underlying children's progressive mastery of the complex structures of human language.

NEW WAYS OF UNDERSTANDING UNDERSTANDING IN RESEARCH ON LANGUAGE DEVELOPMENT IN THE 1970S

The research on language acquisition that began to flourish in the 1960s relied almost exclusively on transcripts of children's speech as the source of evidence for development in linguistic competence. Although it was clear that knowledge of phonology, morphology, and syntax was required in order to understand speech even before such knowledge could be used

to produce speech, there were obvious practical reasons why researchers chose to focus on what children can say. Convinced that children's developing ability to comprehend speech was a mystery equally worthy of attention, Bernard Friedlander (1970) published an article entitled "Receptive Language Development in Infancy: Issues and Problems," motivated by the following concerns:

> Judging by the theoretical and speculative literature as it stands today, receptive language development in infancy is a minor topic of marginal significance. Issues related to infant listening and receptive processes are virtually ignored in the literature of the new wave of language studies that assumed torrential proportions in the early 1960's. Though there is a general acknowledgment... that language input is a necessary prerequisite for the organization of speech, the topic is seldom accorded more than a few sentences or at most a few paragraphs—and some of these discussions are highly patronizing in tone. They seem to suggest that auditory perception in general and language perception in particular are topics on which thoughtful observers would hardly need to spend much time. There is little in this literature to suggest that the problem of how babies come to recognize the phonological, lexical, semantic, and grammatical systems in the language they hear represents a psychological, linguistic, and developmental problem of the greatest magnitude.... Except among audiologists concerned with problems of hearing acuity, investigators have focused their attention almost exclusively on the young child as speaker, and little attention has been paid to him as a listener. (p. 7)

This article was more an essay than a review, because there was almost no relevant research to be reviewed: "Hardly enough is known at a factual level about early listening processes and their role in language growth even to organize the phenomena in reasonably durable categories" (Friedlander, 1970, p. 8). Linguistic theory had provided the reasonably durable categories researchers needed to begin investigating the development of language competence as manifested in production, but gaining access to the unspoken processes involved in comprehension still presented daunting methodological challenges. Friedlander cited a few "fragments of provocative data" available at the time to indicate the potential significance of research on receptive language development, examples drawn mainly from the new research on auditory and visual perception in infancy that had begun to emerge in the 1960s. He also provided an overview of what he considered to be the most important processes and variables involved in early language comprehension. In a prescient analysis of these issues, Friedlander delineated several of the questions that have been of central interest in the literature on the development of understanding that began to grow over the next 10 years and that has flourished since his time.

In retrospect it is clear that almost all of the dominant themes in contemporary research on receptive language development have roots in questions and paradigms that came to life in the 1970s. To give a sense of the originality and excitement of infancy research in those days, several innovative research areas are described here that all focused on phenomena in some way relevant to early understanding. These new research programs emerged from a wide range of disciplines, including cognitive psychology, audiology, speech science, linguistics, anthropology, ethology, pediatrics, and psychiatry. Although the connections among them at first were minimal or nonexistent, studies on infants within these diverse intellectual traditions around the period from 1970 to 1980 provided a foundation for what has now become a coherent and productive research effort focusing on the origins of language understanding. Following is a brief overview of 12 research areas originating in that period that defined questions and introduced experimental methods that continue to influence the field of developmental psycholinguistics today:

1. *Fetal sensitivity to voices:* Given that it was still controversial in the first part of the past century whether newborns could hear at all, the first carefully controlled studies showing that even fetuses could hear were especially newsworthy (e.g., Grimwade, Walker, Bartlett, Gordon, & Wood, 1971). Studies of the intrauterine transmission of acoustic stimuli showed that the ambient noise level within the womb is high enough to mask higher frequencies in sounds coming from outside the uterus (Walker, Grimwade, & Wood, 1971). However, because lower frequencies are transmitted with minimal attenuation, the pitch and intonation of the voice are potentially audible to the fetus. In 1980, DeCasper and Fifer were the first to show that newborn infants recognize their own mother's voice with no postnatal experience, indicating that prenatal auditory experience can influence listening preferences after birth. This discovery led to numerous experiments by investigators interested in how prenatal exposure to a particular language influences the infant's attention to language-specific features of speech (e.g., Mehler et al., 1988).

2. *Auditory sensitivity and pattern detection abilities in infancy:* Studies of postnatal responsiveness to sound also began to make impressive progress during this period. In the 1960s developmental researchers had devised simple but powerful new techniques based on habituation or operant conditioning to investigate many different aspects of visual perception and information processing. However, research on infant audition had lagged behind research on infant vision, in part because there is no easily observable "listening behavior" associated with hearing that is comparable to the visual orienting and fixation behaviors associated with seeing. In 1975 an operantly conditioned head-turn procedure was developed by Moore,

Thompson, and Thompson for assessing auditory thresholds in young infants. This procedure, known as visual reinforcement audiometry, was first designed for clinical purposes but was soon modified for exploring infants' perception of speech sounds (Eilers, Wilson, & Moore, 1977). Operant methods also enabled researchers interested in psychoacoustics to show that infants are sensitive to rhythmic patterns in sequences of sounds (Demany, McKenzie, & Vurpillot, 1977). The methods developed at this time for assessing basic auditory competence in infants, as well as the findings emerging from these new clinical and basic research programs, provided tools and inspiration for many later studies investigating early sensitivity to patterns in music, speech, and other auditory stimuli.

3. *Infant speech perception:* One of the most innovative of the new procedures for assessing infants' auditory sensitivity was developed by Eimas, Siqueland, Jusczyk, and Vigorito (1971), who were the first to show that young infants are able to discriminate and categorize speech sounds. To fluent English speakers */ba/* and */pa/* seem clearly distinct, so it is difficult to appreciate how formidable this task could be to a linguistically inexperienced listener. In fact, the consonants in these two syllables are acoustically very similar to each other, and they also vary acoustically when combined with different vowels or in different positions in a word. For these reasons computer scientists have found it difficult to develop automatic speech recognition routines that can reliably identify a */b/* as a */b/* across different contexts. Using another newly developed operant technique called the high-amplitude-sucking procedure, Eimas et al. showed that even very young infants could discriminate */ba/* from */pa/*. Moreover, just like adults, they failed to distinguish variations of */ba/* that were members of the same phonetic category. Infants can also appreciate the fact that vowel tokens that are acoustically dissimilar may be equivalent in terms of their phonetic identity. Using the operantly conditioned head-turn procedure mentioned earlier, Kuhl (1979) showed that 5-month-old infants readily discriminated */a/* from */i/* when spoken with the same intonation by the same female speaker. However, they grouped together several different tokens of */a/* that were acoustically variable, spoken by male and female speakers using both rising and falling pitch contours. These pioneering studies on early speech-processing abilities showed that infants can attend to the acoustic variability relevant to the phonetic identity of speech sounds, while ignoring acoustic variability that is linguistically irrelevant. The operant methods developed by Eimas et al. (1971) and Eilers et al. (1977) have been used extensively in research on early sensitivity to phonetic contrasts in speech (see Jusczyk, 1997).

4. *Discrimination of phonological contrasts in the second year:* Around the same time that the first speech perception studies with infants were revealing exquisite sensitivity to a range of phonetic contrasts at 2 months

of age, linguists interested in phonological development began testing older children on their ability to discriminate words that are very similar in pronunciation. Garnica (1973) taught 20-month-old children names for novel objects, some of which differed by only one segment, and then later asked them to identify objects paired with a similarly named distracter. Replicating the results of an earlier study by Shvachkin (1973), Garnica found that very few children at this age demonstrated consistent discrimination of all the contrasts. When Barton (1978) tested 24-month-olds with pairs of toys whose names constituted minimal pairs, such as *coat /goat* and *bear/pear,* he also found poor performance. These findings on the failure of 2-year-olds to discriminate */b/* from */p/* in a task requiring word recognition and object retrieval would seem to be at odds with the findings of Eimas et al. (1971) showing that infants a few months old have no difficulty discriminating */ba/* from */pa/* in a speech perception task. Because these studies were done by researchers from different disciplines that were still quite distant from one another, there was no discussion of this apparent discrepancy in the literature at the time. However, the relation of speech sound discrimination skills in early infancy to word recognition abilities in the second year is a topic of considerable interest in research on the development of understanding today (e.g., Stager & Werker, 1997).

5. *Comprehension of single words:* The first experimental procedures for testing infants' knowledge of object words in a controlled setting were also introduced at the end of the 1970s. Benedict (1979) found that 12-month-olds would orient reliably to a familiar object when it was named even when nonverbal behaviors such as gaze and pointing by the speaker were eliminated. In a more stringently controlled procedure that used looking time as an index of word recognition, Thomas, Campos, Shucard, Ramsey, and Shucard (1981) compared the ability of 11- and 13-month-old infants to identify a familiar named object from an array of competitors matched for visual salience. The finding that some 12-month-olds responded as if they understood the meaning of a few familiar words was hardly surprising, because that much was known from parental reports and observational studies of the spontaneous behavior of 1-year-olds interacting with adults. What these new methods offered was a way to assess word recognition more objectively, providing data that could potentially be useful in clinical assessments of individual differences. Unlike the informal observations used earlier to assess early receptive vocabulary, these techniques required the experimenter to standardize stimulus presentations, to define carefully which behaviors counted as a correct response, and to eliminate gestural and other nonverbal cues that might indicate the target object or action. The Thomas et al. procedure was an early precurser to the more powerful eye movement methods used today

to monitor the time course of spoken language comprehension (e.g., Fernald, Pinto, Swingley, Weinberg, & McRoberts, 1998).

6. *Understanding words in combination:* In addition to studies investigating when infants begin to understand individual words, other language researchers interested in syntactic development began to explore when and how children begin to make sense of words in combination. For example, in order to distinguish between "John hit Andy" and "Andy hit John," English-learning children need to appreciate that word order is important in their language. Are children able to understand this distinction even before they begin to produce multiword sentences themselves? Shipley, Smith, and Gleitman (1969) found that 2-year-olds would fetch the ball in response to "Bring me the ball," but also in response to "Ball me the bring," even after they had learned to produce such verb-object sequences correctly in their own speech. These results suggested that the children were understanding each word separately and making the most plausible response given the situation (see Wetstone & Friedlander, 1973). One problem was how to tell if both the verb and the noun were understood in a sentence such as "Drink the milk," or if the child only understood the word *milk* but knew from experience that the act of drinking goes with milk. In either case the child could respond to the command with the correct action. Sachs and Truswell (1978) addressed this question by pairing familiar object names with familiar verbs, but in unfamiliar combinations such as "Kiss the ball" and "Tickle the cup." Many of these studies were motivated by an interest in how the child's understanding of phrases and sentences compared to the level of linguistic development evident in the child's productive speech. Some were also designed to monitor developmental progress in children's understanding of grammatical relations marked by particular types of words, such as prepositions indicating spatial relations (e.g., Clark, 1973).

7. *Word learning by young children:* Whereas testing children's recognition of familiar words showed what sounds they had learned to associate with what concepts, controlling the circumstances under which children learned unfamiliar words enabled researchers to explore how children formed lexical categories in the first place. In 1957, Brown asked whether preschoolers used their knowledge of grammar to infer the meanings of new words. He showed children pictures in which a pair of hands was performing a kneading motion in a bowl full of confetti. Some of the children were asked, "In this picture, can you see *sebbing?"* They responded by making the same motions with their hands. When other children were asked "In this picture can you see a *seb?"* they responded by pointing to the container. Still others were asked "Can you see any *seb?"* and they pointed to the confetti. Brown concluded that children used their knowledge of the structure of the language to guide their word learning. This

kind of paradigm was used in more formal experiments in the 1970s to explore linguistic and cognitive factors influencing word learning (e.g., Katz, Baker, & MacNamara, 1974). Carey's (1978) demonstration of "fast mapping" showed that young children can make rapid inferences about the meaning of an unfamiliar word based on limited contextual information, a study that was particularly influential in generating interest in how children figure out what new words refer to (see P. Bloom, 2000; Woodward & Markman, 1998).

8. *Vocabulary development in the first 2 years:* The first questionnaires and interview procedures for estimating the size of children's productive and receptive vocabulary were developed in the spirit and service of "quantitative" research in the 1920s to document growth in vocabulary with age (Descoeudres, 1921; Van Alstyne, 1929). Fifty years later the emphasis in studies of lexical development had shifted toward more focused and theoretically motivated questions, as in Nelson's (1973) research on individual differences in referential style. Using data from maternal diaries and tape-recorded home observations, Nelson noted that even at the one-word stage children differed in the composition of their productive vocabularies. Based on the first 50 words produced by the children in her study, Nelson categorized the children on a dimension of variation from "referential" to "expressive" style, depending on the relative proportion of object words to social-regulatory words in their vocabularies. Bates and her colleagues extended these findings in a longitudinal study of lexical variation in receptive as well as productive vocabulary, using data from maternal interviews based on a checklist of words and phrases likely to be understood and spoken by children in the second year of life (Snyder, Bates, & Bretherton, 1981). This checklist became the prototype for the MacArthur Communicative Development Inventory, a vocabulary assessment instrument now used extensively for clinical purposes as well as in basic research on language acquisition (Fenson et al., 1994).

9. *Adult speech to infants:* The first systematic observations of the special characteristics of adult speech to infants and young children came from field reports by anthropologists and linguists studying languages as diverse as Comanche and Latvian (Ferguson, 1964). In his analysis of anecdotal observations of child-directed speech in six languages, Ferguson noted that adults interacting with infants typically use shorter utterances, simplified syntax, exaggerated prosody, and special lexical items as compared to adult-directed speech. When psychologists first began to study language input in the 1970s, their interest was motivated in part by an argument Chomsky (1965) had recently put forth in support of his nativist views on language acquisition. Chomsky claimed that adult speech to children was often so ungrammatical and fragmented that children could not possibly rely on this degraded form of speech as a model for learning language. Snow (1972), Broen (1972), and Phillips (1973) provided evi-

dence to the contrary, showing that mothers' speech to English-learning children was simplified in ways that might be beneficial to the inexperienced listener. These studies led to lively debate about the potential linguistic and nonlinguistic functions of child-directed speech (see Snow & Ferguson, 1977), and to the first longitudinal studies attempting to test the hypothesis that particular modifications in mothers' speech influence the rate of acquisition of particular grammatical structures by the child (e.g., Newport, Gleitman, & Gleitman, 1977). The initial emphasis in this literature was on how child-directed speech might facilitate syntactic development as evidenced in children's productions. However, Ferguson (1977) and Brown (1977) also speculated that the modifications in mothers' speech might enhance understanding as well by making it easier for the infant to parse and process the speech stream, a topic that is currently of great interest in research on the development of receptive language skills (see Morgan & Demuth, 1997).

10. *Nonverbal communication in mother-infant interaction:* The 1970s were productive years in yet another branch of the emerging field of infant studies, as researchers in pediatrics, psychiatry, and developmental psychology began to investigate how mothers and young infants coordinate their movements and emotional responses in the context of face-to-face interaction (e.g., Brazelton, Koslowski, & Main, 1974). These studies led to insights about how the caretaker helps to regulate the infant's attention and both elicits and shares the infant's affect (D. Stern, 1977). Trevarthen (1979) proposed that through such interactions the child begins to experience "inter-subjectivity," a prerequisite for communicating with and understanding other minds. Meanwhile, experimental psychologists were also exploring infants' responsiveness to nonverbal communication from a different point of view. Scaife and Bruner (1975) showed that by 12 months of age the child could follow the gaze of the caretaker. This finding was then extended in parametric studies of infants' ability to follow the pointing finger and the gaze of an experimenter (e.g., Butterworth & Cochran, 1980). In most of these studies of infants' attentiveness to nonverbal communication, the research questions and findings were not framed in a way that had any apparent relevance to the development of language understanding. However, Bruner (1977) emphasized the connection and argued eloquently that experiences in infancy such as sharing joint attention with the mother provided a foundation for understanding linguistic reference later on. And Messer (1980) provided the first detailed analysis of how mothers coordinate speech and gesture when introducing new words to the child, a theme that became important in later experimental studies of joint attention and word learning (e.g., Tomasello & Farrar, 1986).

11. *Cross-linguistic studies of language learning:* At a time when nativist perspectives were dominating discussions of language development and the search for universals was a widely shared goal, the work of Slobin

(1970) and others on how children learn languages very different from English provided valuable evidence of diversity as well as commonalities. Although this research was based primarily on analyses of children's speech productions, Slobin's discussions of how language-specific features might influence the course of learning implicated cognitive processes involved in speech perception. He proposed a series of universal "operating principles" for segmenting speech, such as "pay attention to the ends of words," that guide children's attention to particular parts of the speech stream (Slobin, 1973). A perceptual bias of this sort might explain why children learning Hungarian begin to use locatives in their speech earlier than do children learning Serbo-Croatian. Because locative information in Hungarian is consistently encoded in a word-final morpheme, children should have easier access to this information in Hungarian than in Serbo-Croatian, where spatial information is encoded in forms that are more variable and less perceptually salient. Slobin's ideas about the perceptual challenges inherent in different languages and their potential influence on children's success in segmentation and speech processing have informed recent research on perceptual strategies that infants bring to the task of interpreting strings of speech sounds (see Fernald, 2000; Shady & Gerken, 1999).

12. *Ethological research on innately guided learning:* Studies of species-specific perceptual abilities and communication patterns in birds and mammals provided yet another source of insight for developmentalists thinking about the influence of early language experience on the emerging ability of human infants to process speech. Research on song learning by white-crowned sparrows and vocal signals in monkeys and apes led Marler and colleagues to formulate the notion of "innately guided learning" to describe inherent perceptual strategies that enable the young animal to learn quickly communication skills essential for survival (e.g., Marler, Zoloth, & Dooling, 1981). Gottlieb's (1976) studies of the role of early vocal and auditory experience in enabling the young duckling to recognize the call of conspecifics were also influential. Gottlieb's model of the potential effects of experience on perceptual development was adapted by Aslin, Pisoni, and Jusczyk (1983) to consider how hearing the speech sounds of a particular language might direct the course of perceptual learning by infants. Ethological models have been central to recent debates about possible critical period effects in the development of infants' ability to process speech and master syntax (e.g., Doupe & Kuhl, 1999; Werker & Tees, 1999).

The findings, methods, and perspectives just reviewed are indicative of the vitality and creativity of researchers in the 1970s who were beginning to explore the early development of communicative competence from

many different directions. Looking back, it is easy to see how ideas and innovations originating in this period continue to resonate in contemporary research, and how many research questions that seem new today were actually identified a quarter century ago. At the time, however, researchers in the various areas mentioned may have had little awareness of what was going on in the other areas, because the research directions were so novel and because they were scattered across a broad range of disciplines. For example, initially there was little connection between researchers studying early phonetic perception, many of whom were trained or influenced by the paradigms of speech science and psychoacoustics, and researchers studying word recognition in the second year, who typically had a background in linguistics. However, as these new perspectives and methods became established, communication across disciplinary boundaries increased, converging on a common interest in the development of spoken-language comprehension.

LEARNING TO LISTEN: RECENT DISCOVERIES ABOUT EARLY DEVELOPMENTS IN LANGUAGE UNDERSTANDING

The field of developmental psycholinguistics has become both broader and more centered as well as more exuberant over the past two decades, as investigators with diverse training and perspectives on the origins of human language have increasingly shared ideas and findings and worked together. To review the full range of new developments in this thriving area of inquiry would go beyond the goals of this chapter (see Aslin, Jusczyk, & Pisoni, 1998, and Jusczyk, 1997, for reviews). Instead the focus here is on four of several important discoveries about the early development of language understanding that are emerging from recent research. The first discovery is that even very young infants are engaged in learning about regularities in spoken language that help to prepare them for later understanding. Second, infants adapt their perceptual strategies for efficiency in processing the ambient language through early experience with the speech they hear around them. Third, over the second year of life, infants make dramatic gains in the speed and efficiency of word recognition and sentence understanding. And fourth, when talking to infants adults modify their speech in ways that serve both to elicit and communicate emotion and to facilitate speech processing and understanding. The first two of these discoveries are reviewed more briefly, whereas the last two are described in greater detail. This distribution of attention in no way reflects the extent of these research contributions or their relative importance, but merely the prerogative of the author to elaborate on those research questions closest to home.

Months before infants can understand the meanings of words, they can extract and retain information about patterns and regularities in spoken language that help them to identify linguistic units in continuous speech. The first experimental studies on early speech perception focused on infants' ability to discriminate and categorize isolated syllables (see Aslin et al., 1983, for a review). The questions initially of interest were derived from controversial issues in research on adult speech perception, with the infant representing the "initial state," or the listener innocent of experience. Studies with infants were seen as test cases relevant to current debates about which acoustic features are most critical for human speech perception (e.g., Eimas & Corbit, 1973), whether speech and nonspeech sounds are processed in fundamentally different ways (e.g., Jusczyk, Rosner, Cutting, Foard, & Smith, 1977), and whether speech sounds are represented in terms of syllables or phonetic features (Bertoncini, Bijeljac-Babic, Jusczyk, Kennedy, Mehler, 1988). Though providing valuable information about early perceptual abilities crucial for speech processing, most of these early studies were "developmental" only in the sense that they showed these capabilities were already present at birth.

A much more dynamic picture of developmental change in speech perception has emerged in recent years, as the emphasis has shifted from assessments of infants' initial capabilities in discriminating isolated speech sounds to an interest in exploring the discovery procedures infants use to identify higher order elements in spoken language. A provocative article by Lila Gleitman and colleagues was influential in stimulating this shift in focus (Gleitman, Gleitman, Landau, & Wanner, 1988). Gleitman et al. argued that infants are attentive to acoustic patterns in the speech they hear and thus could use certain prosodic features in continuous speech, such as pauses and the vowel lengthening typically preceding pauses, as cues to the boundaries of phrases and clauses. Such a perceptual discovery strategy could be very useful to the child beginning to learn syntax. This "prosodic bootstrapping hypothesis" generated considerable interest, leading to experiments showing that 10-month-old infants seemed to recognize violations of common prosodic rhythms in the ambient language (e.g., Kemler Nelson, Hirsh-Pasek, Jusczyk, & Wright-Cassidy, 1989). There were also counterarguments against the view that prosodic cues are sufficiently regular as to provide reliable cues to syntactic units in speech (e.g., Fernald & McRoberts, 1995).

The strong claim that young infants must first rely on prosodic information in order to organize segmental information in continuous speech has receded in light of new findings showing that infants are much more adept at identifying wordlike units in fluent speech than anyone had imagined. Jusczyk and Aslin (1995) investigated the ability of 7-month-old infants to detect repeated words embedded in fluent speech. When in-

fants were first familiarized with multiple repetitions of a word such as *bike* or *feet* and then tested in an auditory preference procedure with passages that either did or did not contain the familiarized word, they preferred to listen to passages containing the familiar word. This finding indicated that infants were able to segment speech into words without benefit of exaggerated prosodic cues. However, prosody at the level of word stress does play a role in facilitating such segmentation. English-learning infants are more successful in segmenting words such as *bor'der,* which have a strong-weak accent pattern, than words such as *guitar',* which have the opposite pattern, perhaps because they have already learned that the strong-weak pattern is dominant in the language they are hearing (Jusczyk, 1998).

Further evidence for early sensitivity to the fine structure of segmental organization in continuous speech comes from the work of Saffran, Aslin, and Newport (1996), who showed that 8-month-olds can segment a stream of meaningless syllables containing no acoustic or prosodic cues to word boundaries after only a few minutes of listening experience. The information infants are using to identify wordlike units in this case is distributional evidence, the regularities in the relative position and order of particular syllables over the whole sequence. For example, one string of syllables consisted of *pa bi ku go la tu da ro pi ti pu do da ro pi go la tu*.... After familiarization with this sequence, infants were tested with "words" that had occurred in the string, that is, sequences of syllables that always occurred in the same order, such as *pabiku* and *tudaro.* They were also tested with combinations of the familiar syllables but in a different order, such as *pibatu.* Although there was no acoustic information indicating word boundaries, the transitional probabilities were much higher between syllables within words than between words. Thus there was statistical information that could enable infants to identify the familiar wordlike units in the stream of speech. The finding that they are capable of performing such computations reveals the sophisticated talents young learners bring to the task of segmenting speech, months before they are able to understand meanings in the words they hear.

Through early experience with the speech around them, infants adapt their perceptual strategies for efficiency in processing the language they are learning. Although infants are clearly born with perceptual abilities and biases that equip them for organizing speech sounds into linguistically relevant categories, these perceptual grouping strategies are neither unique to humans nor unique to speech sounds. Other primates organize human speech sounds categorically, and some other kinds of acoustic stimuli are perceived in a similar fashion (see Kuhl, 2000). What *is* presumably unique to humans is the perceptual learning that occurs over the first few months

of life as a result of hearing a particular language. Adults often find it difficult or even impossible to distinguish certain speech sounds in an unfamiliar language. For example, the consonants */Ta/* and */ta/* are easily discriminated by native speakers of Hindi, but to monolingual English-speaking adults they sound like indistinguishable tokens from the English category */t/*. However, 6-month-old infants raised in English-speaking families can effortlessly discriminate the Hindi contrast */Ta/ - /ta/* (Werker & Tees, 1984). Recent research on the perception of speech sounds in native and non-native languages shows that adults have become specialists, attentive to phonetic distinctions relevant in the languages they have learned but less acute in making other distinctions. However, infants start out with the potential to make a wide range of distinctions. When does this process of perceptual specialization begin? Werker and Tees tested English-learning infants at three ages between 6 and 12 months, to investigate whether they retained their ability to discriminate non-native speech contrasts across the first year. Infants at each age listened either to the Hindi consonants */Ta/-/ta/* or to consonants from the Nthlakampz language, */k'i/-/q'i/,* which are also very difficult for English-speaking adults to discriminate. Almost all of the English-learning infants at 6-8 months could discriminate both non-English contrasts, although at 10-12 months very few were able to distinguish either pair.

Further evidence for the influence of the ambient language on infants' emerging phonetic categories comes from research by Kuhl, Williams, Lacerda, Stevens, and Lindblom (1992), who showed that 6-month-old infants hearing only Swedish or English already grouped vowels perceptually in categories appropriate to the language they were learning. Recent studies measuring brain activity in response to speech are generally consistent with the behavioral findings showing increasing specialization for familiar speech sounds over the first year. At 6 months of age, infants show an electrophysiological response to changes in both native and non-native speech contrasts, but by 12 months the response is elicited only by changes in speech sounds native to the language the child has been hearing (Cheour-Luhtanen et al., 1995). These results indicate that auditory experience over the first year results in neural commitment to a particular perceptual organization of speech sounds appropriate to the ambient language.

Infants in the 2nd year make dramatic gains in the speed and efficiency of recognizing words and understanding sentences. In addition to research focusing on developments in speech-processing abilities in infants not yet able to speak, there is considerable interest in receptive language abilities as infants begin to understand word meanings in the second year. The most widely used approach to assessing lexical development over the second

year has been to ask parents to report on which words their child understands and says, based on their experience interacting with the child at home. The vocabulary checklist originally developed by Bates and colleagues (Snyder et al., 1981), now expanded into the MacArthur Communicative Development Inventory (CDI), has been used extensively in recent years to explore the relation between growth in productive and receptive language capabilities (Fenson et al., 1994). The use of this instrument in conjunction with many other observational and experimental measures of children's language capabilities has provided norms for lexical development across large samples, as well as valuable information about the relation of vocabulary growth to other milestones in the development of language competence (e.g., Bates et al. 1994). However, although CDI measures enable investigators to track age-related changes in the estimated size of the child's lexicon, they reveal nothing about the less obvious changes in the speed and efficiency with which known words are comprehended. The word *doggy,* for example, may be recognized and spoken at 15 months, but the child's ability to understand this word when it occurs in fluent speech continues to improve in ways that are not readily observable in spontaneous behavior. The recent development of high-resolution measures of online speech comprehension has enabled us to explore how infants in the second year become much faster, more reliable, and more flexible in understanding familiar words.

Beginning with the study by Thomas et al. (1981), a variety of techniques using gaze patterns have been used to study word recognition in infancy (e.g., Golinkoff, Hirsh-Pasek, Cauley, & Gordon, 1987; Shafer & Plunkett, 1998). All of these techniques rely on an auditory-visual matching procedure in which children look at an array of object pictures while listening to speech naming one of the objects, enabling the investigator to determine whether the child indicates comprehension by looking significantly longer at the matching picture after hearing the spoken word. In our laboratory, this procedure has been further refined to allow us monitor precisely the actual time course of word recognition. In a cross-sectional study of infants at 15,18, and 24 months of age, we calculated infants' latency to shift to the correct picture in response to a familiar target word. Although the 15-month-olds demonstrated word recognition by looking reliably at the correct picture in response to naming, they typically did not initiate a shift in gaze from the distractor to the target picture until *after* the target word had been spoken. In contrast, the 24-month-olds typically began to shift to the correct picture *before* the end of the target word.

These findings suggested that the older infants were identifying the spoken word using only partial phonetic information, just as adults do. The ability to process speech incrementally is a robust characteristic of spoken-language understanding by adults. Because adults make continu-

ous use of acoustic-phonetic information, they can identify familiar words as soon as sufficient detail is available to enable them to disambiguate the word from other alternatives (e.g., Marslen-Wilson, 1987). For example, a word onset such as */el/* activates several English words including *elf, elbow, elegant, elephant,* and others consistent with the initial segments. When the adult listener hears */ele/,* most of these candidates can be rejected; a few milliseconds later when the */f/* is heard, the word *elephant* can be uniquely identified before the end of the word is spoken.

Although the Fernald et al. (1998) findings revealed that 2-year-olds often responded before the word was completely spoken, it was not clear how much phonetic information they needed to hear before recognizing the word. Two further studies addressed this question directly. In the first study, 24-month-old infants were presented with paired pictures of familiar objects whose names overlapped substantially (*doggie/doll*) or did not overlap (e.g., *doggie/tree*) at word onset (Swingley, Pinto, & Fernald, 1999). Whereas *doggie* could potentially be distinguished from *tree* right from the beginning of the word, *doggie* and *doll* were not distinguishable until the occurrence of the second consonant 300 ms into the word. If 2-year-olds can process words incrementally, their reaction times should be slower in the case of phonetic overlap. Thus the question of interest was whether infants hearing the target word *doggie* would be slower to shift to the correct picture when the dog was paired with the doll than when the dog was paired with the tree. Indeed, infants rapidly distinguished *doggie* from *tree,* but their response was delayed by about 300 ms when distinguishing *doggie* from *doll.* These results suggested that by 24 months of age, children are able to monitor speech continuously, taking advantage of word-initial information to identify a familiar word and responding as soon as disambiguating information becomes available.

The second study provided further evidence for incremental processing of speech at even younger ages. This time we presented 18- and 21-month-olds with truncated target words in which only the first part of the word was available, such as */bei/* for *baby* and */kI/* for *kitty* (Fernald, Swingley, & Pinto, 2001). We also explored the relation of our speech-processing measures to infants' productive vocabulary size, as assessed by parental report on the MacArthur CDI. The question of interest was whether infants who are faster and more accurate in word recognition are also more advanced in vocabulary development. Three main findings emerged from this study. First, 18- and 21-month-old infants were able to recognize spoken words rapidly and reliably after hearing only the first few hundred milliseconds, showing that word-initial phonetic information was sufficient for infants to identify known words. Second, speed of word recognition was associated with accuracy in this task: Infants who were faster to respond were also more accurate overall. And finally, we

found that speed of word recognition was also related to lexical development: Those infants who could identify words more rapidly also tended to have larger spoken vocabularies.

These studies all show that during their second year infants make impressive improvement in the speed and efficiency with which they can recognize familiar words in fluent speech. During the period known as the vocabulary burst, many infants begin to acquire new words more rapidly starting around the age of 18 months (e.g., L. Bloom, 1973). Research using online measures of infants' word recognition skills during this same period reveals parallel developments in receptive language competence, in the increased efficiency with which long-familiar words are recognized and understood. Future studies using eye-tracking methods, in conjunction with neuropsychological research that explores the brain correlates of lexical access in children (e.g., Mills, Coffey-Corina, & Neville, 1997), will bring us closer to understanding many dimensions of the gradual and largely hidden developments in language comprehension that are rarely evident in the child's spontaneous behavior.

When talking to infants, adults modify their speech in ways that serve both to elicit and communicate emotion and to facilitate speech processing and understanding. In his review of field observations of parental speech, Ferguson (1964) observed that caretakers in many cultures alter their speech when interacting with infants and young children. Since that time, numerous studies in diverse cultures have analyzed more extensive samples of talk to children, using acoustic measures to document differences in the linguistic and prosodic structure of infant-directed (ID) and adult-directed (AD) speech. Consistent with the early observations, these studies show that both mothers and fathers use shorter utterances, longer pauses, higher pitch, more exaggerated intonation, and more repetition when interacting with infants (e.g., Fernald et a., 1989). Noting the prevalence and consistency of modifications in ID-speech across languages, Ferguson (1977) speculated that this special speech register might serve both an "affective" and a "clarification" function. More recent research on the nature of ID speech and its effects on infant listeners provides support for both of these hypothesized functions.

The idea that speech to young infants serves an affective function appeared repeatedly in the early diary studies. For example, Tiedemann (1787/1927), Darwin (1877), and Taine (1877) all noted that their children responded first at an emotional level to the intonation in parental speech. Experimental studies using auditory preference procedures confirm that the melodic qualities of ID prosody engage attention and elicit emotion in young infants. For example, infants show a listening preference for ID speech over AD speech (e.g., Fernald, 1985), and are more emotionally re-

sponsive to ID speech as well (Werker & McLeod, 1990). Tiedemann also observed that his 6-month-old son responded differently to positive and negative intonation: "Differences of speaking tone, characteristic of different emotions, especially of disapproval and satisfaction, he had learned to comprehend, for he let himself be silenced by threats" (p. 218). The claim that infants are responsive to certain emotional "meanings" conveyed by adult speech has also been explored experimentally, in a study of infants' responsiveness to vocalizations expressing approval and disapproval in a familiar language as well as in several unfamiliar languages (Fernald, 1993). Five-month-old infants reacted with more positive emotion when listening to approvals and with more negative emotion when listening to prohibitions, even in languages completely unfamiliar to them. These findings show that infants are indeed affectively responsive to the prosody in speech long before they can understand word meanings.

Research on the "clarification" functions of ID-speech also proposed by Ferguson (1977) has taken two different directions: first, descriptive analyses of prosodic and phonetic properties of mothers' speech that might enhance intelligibility, and second, experimental studies testing hypotheses about the facilitative effects of particular acoustic modifications in ID speech on speech processing by young infants. Several studies have found that adults enunciate words more clearly when speaking to an infant than when speaking to another adult (e.g., Bernstein-Ratner, 1984; Kuhl, et al., 1997). These results suggest that words in ID speech might be perceptually more accessible to infants, because caretakers intuitively articulate speech sounds in ways that make them more prototypical exemplars of the vowels and consonants of the language. Another prominent characteristic of speech addressed to infants in English has to do with the positioning of words rather than with phonetic structure, namely that object words especially relevant to the conversation are frequently placed at the end of the utterance rather than in the middle. When speaking to infants, mothers use more exaggerated stress patterns and consistently position focused words in utterance-final position, whereas in speech to adults they use more variable strategies to mark focused words (Fernald & Mazzie, 1991). By positioning focused noun labels at the ends of utterances, speakers seem to be intuitively exploiting listening biases that make final elements in an auditory sequence easier to detect and remember, a phenomenon well established in research on auditory processing and memory (e.g., C. S. Watson, 1976). If even adults can benefit from having new auditory information presented in this format, it seems likely that infants facing the challenges of word segmentation in continuous speech would benefit even more.

The findings from these descriptive studies of caretakers' speech are all consistent with the "clarification" hypothesis, that one function of the modifications in speech to children is to enhance understanding. How-

ever, this hypothesis can be tested directly only in experiments that explore the effects of particular ID-speech modifications on infants' ability to understand words in fluent speech. Research on this question is just beginning, but one study has shown how variations in the structure of the input speech can influence the infant's success in understanding spoken words at different points in development (see Fernald, McRoberts, & Swingley, 2001). The goal of this study was to investigate the potential perceptual benefit for the infant of placing object words in sentence-final position, as English-speaking mothers tend to do in ID speech. Using the word recognition procedure described earlier, 15- and 19-month-old English-learning infants were presented with familiar target words spoken either in utterance-medial *(There's a ——— over there)* or in utterance-final position *(Over there there's a ———)*. At both ages infants were more successful in recognizing final target words than medial target words, suggesting that this common pattern of word order in ID speech could serve as a kind of support system for the inexperienced listener. By using short utterances and placing focused words at the end of the string of words, caretakers may intuitively compensate for processing limitations that make it difficult for infants to succeed in segmenting, recognizing, and understanding a familiar word when it is followed by other words in continuous speech.

CONCLUSION

The field of developmental psycholinguistics has expanded and flourished over the past 30 years. Many of the questions of central interest to researchers today were first intimated in perceptive observations chronicled in the diary studies of the 18th and 19th centuries. When does the young infant first pay attention to speech and what is it in the voice that evokes interest? When the child starts to respond systematically to parental vocalizations, what kind of meaning is being understood? How is the observable progress in speech production related to the unobservable development of understanding? However, other research questions of interest today were undreamed of a century ago, when it was inconceivable that methods would be developed capable of closely monitoring an infant's mental processes. A few of these new paradigms have been described here, techniques that now enable researchers to investigate the parsing strategies of 8-month-olds, to document the effects of exposure to a language on the formation of phonetic categories, and to track the underground development of speed and efficiency in the comprehension of familiar words. Other powerful new paradigms include computational models of language learning that give insight into the influence of speech-

processing factors (e.g., Brent & Cartwright, 1998), and neuropsychological techniques that enable investigation of the brain bases of speech comprehension (e.g., Choeur-Luhtanen et al., 1995; Mills et al., 1997). The vitality of the field today draws on two centuries of thoughtful observations of children learning to talk and to understand, on the remarkable flowering of methodological ingenuity three decades ago, and on the convergence of theoretical insight and creativity from researchers in a wide range of disciplines who share a common interest in the early development of understanding.

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5

Emotion-Related Regulation and Its Relation to Quality of Social Functioning

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Emotion-related regulation, a topic neglected by developmental psychologists for many years, has become a major area of research. Current interest in this topic is due, in part, to the renaissance in work on the topic of emotion in the past two decades, after the strong influence of the behaviorist movement subsided. Another factor contributing to current interest in emotion-related regulation is the strong concern among psychologists about developmental psychopathologies, including problems with anxiety and depression and especially with externalizing behaviors such as violence and substance abuse. Because of the role of frustration, anger, and lack of regulation in some antisocial behavior, the topic of regulation has emerged as an important issue in research on externalizing behavior problems.

A major theme of this chapter is the importance of the distinction between effortful or more voluntary aspects of regulation and less voluntary, more passive modes of responding that are reflected in individuals' emotion-related inhibition (or the lack thereof). I believe this distinction is essential if we are to understand the role of regulatory processes in adaptation and in optimal (vs. nonoptimal) development. This distinction is not really new; ideas relevant to it can be found in various writings in psychology, especially in the last three or four decades.

In this chapter, a brief history of ideas related to emotion-related regulation is presented; special emphasis is given to writings relevant to the role of volition and adaptation in the construct of emotion-related regula-

tion. We start with writings and research in which regulation typically is viewed as the outcome of socialization and/or is implicitly assumed to be reflected in children's acceptance of socially appropriate values and expectations. A salient example of this approach is research and theory on moral development. Another body of investigation pertains to the role of early social relationships in children's development of emotion regulation. Common notions in this approach are that caregivers typically help children develop emotion regulation and that the quality of the parent-child relationship can enhance or disrupt its development and the degree to which regulation is internalized and under the child's control. A third direction is one that emphasizes dispositional characteristics of the child, originating from both constitutional and environmental factors, that influence individual differences in emotion regulation. Notions related to over-control versus undercontrol and flexibility and adaptiveness of control are embedded in this work. Temperament theorists, investigators studying ego control, and some coping theorists have contributed to this approach. Finally, neurophysiological research has played a prominent role in identifying different types of inhibition and their physiological bases, and is increasingly important in psychological conceptions of regulation and control.

After the historical review, some recent directions of investigations of emotion-related regulation are described, including research from our laboratory. Findings on the relation of emotion-related control/regulation to quality of children's social functioning are emphasized, as are data relevant to the conceptual distinction between effortful and passive types of control. Issues pertaining to the measurement of different types of control/regulation also are examined briefly. Finally, possible future directions for theory and research are outlined; issues related to measurement and conceptualization of relevant constructs are highlighted. We begin, however, with a quick overview of what is subsumed under the title of "emotion-related regulation."

EMOTION-RELATED REGULATION: DEFINITIONS

To make clear what we mean by emotion-related regulation, it is useful to differentiate conceptually between two interrelated aspects of emotion-related regulation: (a) emotion regulation, which often involves effortful attentional processes, and (b) emotion-related behavioral regulation. We define *emotion regulation* as the process of initiating, avoiding, maintaining, modulating, or changing the occurrence, intensity, or duration of internal feeling states, emotion-related physiological processes, and/or emotion-related cognitions and goals, generally in the service of accomplishing

one's goals. In research on temperament, emotion regulation sometimes has been operationalized as involving attentional processes such as the abilities to shift and focus attention as needed (Derryberry & Rothbart, 1988; Windle & Lerner, 1986). Further, it includes cognitive processes discussed by coping theorists such as cognitive distraction and positive cognitive restructuring (i.e., reframing the situation to highlight positive aspects of it; Lazarus & Folkman, 1984; Sandler, Tein, & West, 1994). It also can involve the modification of goals or their relative valuation (e.g., devaluing the importance of success at work and emphasizing the value of family and close friends in old age).

While and once emotion is aroused, it often is expressed in facial or gestural reactions, or through overt behaviors. We define *emotion-related behavioral regulation* as the process of initiating, maintaining, inhibiting, modulating, or changing the occurrence, form, and duration of behavioral concomitants of emotion. Thus, emotion-related behavioral regulation (labeled *behavioral regulation* for brevity) includes voluntary control or modulation of facial and gestural reactions and other overt behaviors that stem from, or are associated with, internal emotion-related psychological or physiological states and goals. Behavioral regulation is tapped by some temperament researchers who assess inhibitory or activation control, that is, the abilities to *voluntarily* inhibit or activate behavior. In addition, clinical, developmental, and personality theorists frequently have discussed constructs such as self-regulation, constraint, or ego control, which involve the ability to modulate the behavioral expression of impulses and feelings and likely are at least partially voluntary (e.g., J. H. Block & J. Block, 1980; Tellegen, 1985). Behaviors such as problem-focused or instrumental coping—that is, attempts to manage a stress-inducing and emotion-eliciting context through processes such as planning and active problem solving—also could be considered an aspect of emotion-related behavioral regulation (Lazarus & Folkman, 1984; Sandler et al., 1994), although at times they may reflect emotion regulation (i.e., attempts to reduce internal emotional and physiological responding) or both aspects of emotion-related regulation.

Emotion-related regulation is a process that can occur either when confronted with an evocative situation or prior to the elicitation of emotion. The latter type of emotion-related regulation has been called *antecedent emotion regulation* or *proactive coping* (Aspinwall & Taylor, 1997; Gross, 1999; Thompson, 1994). Proactive coping is defined as "efforts undertaken in advance of a potentially stressful event to prevent it or to modify its form before it occurs" (Aspinwall & Taylor, 1977, p. 417). It is viewed as virtually always active and as not involving positive reappraisals or other methods of emotion soothing. In contrast, antecedent emotion regulation is defined as involving proactive coping (e.g., the selection or modifica-

tion of situations) and emotion regulation processes such as attention deployment or cognitive change (Gross, 1999). Thus, both proactive coping and antecedent emotion regulation often involve managing emotional reactions before they occur by the selection and shaping of the environment to which a person is exposed. For example, a very shy person may decide not to go to a large social gathering because he or she does not want to experience social anxiety or a person may seek information to make decisions that avoid stressful events. Or this shy person may invite only four rather than eight people to a dinner party to minimize his or her likelihood of experiencing distress. In addition, antecedent emotion regulation involves managing emotional reactions before they occur by using attention and cognitive processes to choose the situations that are focused on and how they are interpreted (Gross, 1999). Thus, attentional, cognitive, and behavioral resources, including planning, are involved in antecedent emotion regulation.

Thus, emotion-related regulation can involve the control of perceptual input through attention and the selection or modification of context that the individual encounters, modifying the meaning and significance of relations between the person and the environment, and changing behavioral responses associated with emotion or anticipated emotion (J. J. Campos, Mumme, Kermoian, & R. G. Campos, 1994). Because these processes are intertwined, emotion regulation and emotion-related behavioral regulation—especially when they occur during emotional arousal—obviously are intimately associated. Moreover, it would appear that some processes can serve as a mechanism for both emotional regulation or behavioral regulation. For example, inhibitory control—the ability to voluntarily cease behavior—is usually used to curb the expression of behavior stemming from or associated with emotion. However, it also could be used to inhibit approach toward, and exposure to, a potentially distressing situation—which likely will affect the internal experience of emotion. In addition, it is clear that many of the same processes involved in emotion-related regulation, such as effortfully focusing attention and inhibiting behavior, may be involved in regulation of mental and physical activities that do not involve substantial emotion.

AN HISTORICAL PERSPECTIVE ON EMOTION-RELATED REGULATION

In recounting some of the historical roots of emotion-related regulation, I do not review the history of self-regulation models in personality, clinical, and social psychology, which focus heavily on constructs such as goal setting, discrepancy detection and self-monitoring, and feedback loops

(e.g., Carver & Scheier, 1998; Karoly, 1993). Such models have more to do with the steps in behavioral regulation and generally have not been used to study emotion or outcomes of interest to developmentalists.

The topic of emotion-related regulation, as represented in the developmental literature, has more direct intellectual roots in several lines of inquiry. The first is the broader literature pertaining to the development of regulation, as manifest in behaviors such as compliance, internalization of parental or societal values, and other aspects of "successful" socialization. Although this work generally has not been integrated with theory and research on emotion-related regulation (see Eisenberg, 2000a), it is of historical and conceptual relevance.

A second body of relevant study is research on the quality of early parent-child relationships and interactions. For example, attachment theory has implications for children's expression and regulation of emotion early in life, as does work on early dyadic social interactions.

A third area of direct historical relevance to current perspectives on emotion-related regulation is early research on temperament, especially if temperament was defined or operationalized as including aspects of regulation. Ideas from this body of work play an important role in thinking about emotion-related regulation. Conceptually related ideas are also to be found in the bodies of theory and research on coping and ego control, which usually have been treated as literatures separate from one another and from research or theory on temperament. Finally, study of inhibition in the nervous systems, including attentional inhibition, are of growing importance to developmental research on emotion regulation. Early investigations on this topic generally were conducted by physiologists or biologists; more recent research on this issue has been the domain of neuropsychologists.

Regulation and Socialization

The topic of the socialization of children has been of interest to scholars for millennia. Often this concern has been linked to the issue of self-regulation and how to instill it in children. The focus in early writings generally was on behavioral regulation—how to get children to pay attention, behave in socially expected ways, and regulate their own behavior based on internalized values or a conscience. However, the notion of emotion regulation sometimes was an implicit concern because the loss of emotional control and unbridled desires were linked to lack of regulation.

For example, according to Borstelmann (1983), in his review of ideas about children in the past, in ancient Greek society, "there was great concern with discipline and control, implying that the child was deemed unwilling or unable to sit still and pay attention" (p. 5). Plato, in the *Republic*

(Bk. 9, 591, cited by Borstelmann, 1983, p. 6), asserted that "[Self-control] is the aim of our control of children, our not leaving them free before we have established, so to speak, a constitutional government within them and, by fostering the best element in them with the aid of the like in ourselves, have set up in its place a similar guardian and ruler of the child, and then, and then only, we leave it free." We know Plato was concerned with the regulation of emotion because, in his discussion of socialization he said, "While spoiling of children makes their tempers fretful, peevish, and easily upset by mere trifles, the contrary treatment, the severe and unqualified tyranny which makes its victims spiritless, servile, and sullen, renders them unfit for the intercourse of domestic and civic life (*Laws,* Bk. 7, 791; cited by Borstelmann, 1983, p. 6). Apparently, the Romans were even more likely than the Greeks to refer to the unruly, angry, deceitful, and jealous character of children, and, like the Greeks, were concerned with instilling good character in children (Borstelmann, 1983).

The writings of John Locke contribute more directly to psychological conceptions about self-control in children. Locke emphasized the crucial importance of the development of internalized self-control: "He that has not a mastery over his inclinations, he that knows not how to resist the importunity of present pleasure or pain for the sake of what reason tells him is fit to be done, wants the true principle of virtue and industry and is [in] danger never to be good for anything" (Locke, 1964; cited in Borstelmann, 1983, p. 20). Interestingly, Locke believed children develop selfcontrol through early exposure to models of reasoned external control, very similar to Baumrind's (1971) authoritative parent.

Of course, Sigmund Freud had a deep and abiding interest in the control of noncognitive impulses and desires and the transformation of the child from an ungoverned, impulse-driven being to one controlled by the ego and superego. The reality principle, by which the ego is operated, "does not abandon the intention of ultimately obtaining pleasure, but it nevertheless demands and carries into effect the postponement of satisfaction, the abandonment of a number of possibilities of gaining satisfaction and the temporary toleration of unpleasure as a step on the long indirect road to pleasure" (p. 239, from Freud, 1961/1997). In brief, the ego involves the use of self-regulation to manage the impulses and drives, often involving emotion, housed in the id, for the purpose of obtaining more pleasure in the long run. Moreover, the emergence of the superego in the phallic stage of development adds a new type of control, based on the conscience and the internalization of values and standards of the same-sex parent.

Freud's notion of internalization strongly influenced Erikson's writings and thinking about moral internalization in the 1950s and 1960s. Similar to Freud, Erikson argued that during the stage of initiative versus guilt, the

child takes on from his or her parents the capacities for self-observation, self-guidance, and self-punishment. These capabilities are used to regulate the self, but if the superego is too uncompromising, can result in children overcontrolling themselves and becoming overconstricted. This notion of overcontrol is a topic of considerable contemporary attention.

Although often disassociated from the issue of infantile sexuality that was so pervasive in Freud's theory, the Freudian notion of internalization of parental standards was adapted by social learning theorists to explain the emergence of resistance to deviation, guilt and reparation, as well as aggression. Well-known examples of such research include the studies of Sears and his colleagues in the 1950s and 1960s (e.g., Sears, Maccoby, & Levin, 1957; Sears, Rau, & Alpert, 1965) and Martin Hoffman (see Hoffman, 1970, 1975). More recently, people such as Claire Kopp (1982, 1989) have sought to outline the developmental stages in children's acceptance of parental standards. Although emotion regulation per se generally was not examined in these more recent investigations (although Kopp, 1992, looked at crying), the control of behavior as reflected in the expected behavioral outcomes of regulation (e.g., morally relevant actions or willing compliance) is an obvious focus in this work. It generally was assumed that children who resisted temptation, complied, and experienced guilt were well regulated.

Despite the fact that regulation seldom was assessed, this body of inquiry provides an intellectual basis for current thinking on emotion-related regulation. We know, for example, from the recent research of Kochanska, Murray, and Coy (1997) that inhibitory control—that is, the ability to effortfully inhibit action—is related to the development of conscience and behaviors such as resistance to temptation. Indeed, as discussed later, there is mounting evidence that inhibitory control, as well as the ability to control attention, are related to diverse potential indices of conscience, including reactive (emotionally driven) aggression, sympathy, and prosocial behavior (Eisenberg, 2000a).

Some of the earliest research in which regulatory processes were assessed somewhat more directly is Walter Mischel's research on delay of gratification. In the 1960s, Mischel, Grusec, and colleagues examined the relation of the social learning construct of modeling to the ability to delay gratification (e.g., Bandura & Mischel, 1965; Mischel & Grusec, 1966). Modeling or imitation was viewed as a behavioral mechanism that explained the supposed outcomes (e.g., taking on of adults' standards) of the Freudian-based concept of internalization. Then, in the early 1970s, Mischel published several papers on the role of attention and cognitive appraisals in children's ability to delay gratification (e.g., Mischel & Baker, 1975; Mischel & Ebbesen, 1970). In some studies, Mischel and his colleagues used manipulations, such as having children look or not look at the re-

wards when testing their ability to delay gratification. Voluntary waiting time was greater when children could not attend to the rewards during the waiting period (Mischel & Ebbesen, 1970). Similarly, thinking about nonconsummatory qualities of the rewards increased children's abilities to delay (Mischel & Baker, 1975). Of most interest, individual differences in children's abilities to shift their attention from a tempting commodity, as reflected in their delay of gratification and resistance to temptation in such experimental settings (especially when they were not explicitly provided with a means of diverting their attention; Shoda, Mischel, & Peake, 1990), has been linked to social and cognitive competence in adolescence and early adulthood (Mischel, 2000; Mischel, Shoda, & Rodriguez, 1989). It is likely that children who shifted their attention lessened their emotional reactions to the desired commodities—what Metcalfe and Mischel (1999) labeled the "hot, emotional 'go' system"—so that regulation of internal emotional states (as well as behavior) was reflected in Mischel's measures.

Thus, there has been a long line of individuals interested in the question of how children become capable of behaving in socially proscribed ways. Much of this work has concerned morally relevant or socially desirable behaviors, which sometimes, but certainly not always, involved the management of emotion. Links between this line of investigation and current research on emotion-related regulation often are not evident, although the research of Kochanska (1993; Kochanska et al., 1997) and Mischel, and a few of our own studies (e.g., Eisenberg, Fabes, Karbon, et al., 1996), are exceptions to the general rule.

The Early Parent-Child Relationship

Another historical root of contemporary study of emotion-related regulation is theory and research on the early parent-child relationship. Bowlby (1969), who himself was highly influenced by the writings of Freud as well as ethologists, noted that when infants were securely attached, they were able to use their mothers as secure bases from which to explore the world. In a sense, mothers of securely attached children, by their presence, serve to regulate their children's anxiety in novel situations. According to Bowlby (1973), "whenever an individual is confident that an attachment figure will be available to him when he desires it, that person will be much less prone to either intense or chronic fear than will an individual who for any reason has no such confidence" (p. 359).

More important for recent conceptualizations is Bowlby's notion that children develop working models of relationships. His ideas in this regard may have been influenced by Freud's discussion of how one's inter-

nal world can guide their behavior: "The yield brought to light by scientific work from our primary sense perceptions will consist of an insight into connections and dependent relations which are present in the external world, which can somehow be reliably reproduced or reflected in the internal work of our thought and a knowledge of which enables us to 'understand' something in the external world, to foresee it and possibly alter it" (Freud, 1940/1963, p. 53).

Bowlby (1973) hypothesized that people construct an internal representation of the world that includes feelings about the self's worth and what one can expect from others:

> In the working model of the world that anyone builds, a key feature is his notion of who his attachment figures are, where they may be found, and how they may be expected to respond. Similarly, in the working model of the self that anyone builds a key feature is his notion of how acceptable or unacceptable he himself is in the eyes of his attachment figures. On the structure of these complementary models are based that person's forecasts of how accessible and responsive his attachment figures are likely to be should he turn to them for support. (p. 203)

Based on the assumption that the working models that people develop early in life affect their perceptions of how likely others are to provide emotional support, more recent investigators have hypothesized that internal working models affect individuals' expression and regulation of their emotions. In fact, attachment relationships and styles have been viewed as reflecting strategies for regulating emotion in interpersonal relationships (Bridges & Grolnick, 1995; Cassidy, 1994; Sroufe, Schork, Motti, Lawroski, & LaFreniere, 1984). For example, securely attached infants, who have experienced sensitive responding by parenting figures when distressed, are viewed as learning that it is acceptable to exhibit distress and to actively seek out the assistance of others for comfort when stressed. In contrast, the avoidant infant, due in part to parents' nonresponsiveness to the infant's distress signals, appears to learn to inhibit emotional expressiveness as well as other-directed self-regulatory strategies (e.g., contact-seeking and -maintaining behaviors; Bridges & Grolnick, 1995; Braungart & Stifter, 1991) and to hide sadness (Spangler & K. E. Grossman, 1993). Although empirical evidence is limited, initial data support these expectations, for infants as well as for 10-year-old children (K. E. Grossman, K. Grossman, & Schwan, 1986; Zimmerman & K. Grossman, 1996; see Cassidy, 1994).

Also emerging from both psychoanalytic theory and attachment theory are theoretical perspectives that place the emergence of children's emotion regulation in the nature of their actual interactions with the mother-

ing figure early in life. Erikson (1950/1963), when discussing the infant stage of trust versus basic mistrust, said "The [infant's] experience of a mutual regulation of his increasingly receptive capacities with the maternal techniques of provision gradually helps him to balance the discomfort caused by the immaturity of homeostasis with which he was born" (p. 147). In discussion of the second psychosocial stage, autonomy versus shame and doubt, Erikson emphasized the importance of firm, reassuring outer control at this stage for structuring and regulating children's early emotions. Erikson felt that this stage was "decisive for the ration of ... freedom of self-expression and its suppression" (p. 264). He argued that "From a sense of self-control without loss of self-esteem comes a lasting sense of good will and pride; from a sense of loss of self-control and of foreign overcontrol comes a lasting propensity for doubt and shame" (p. 264). Thus, for Erikson, a sense of self-control, derived from reassuring outer control, contributes to the toddler's self-esteem and the experience of emotions such as pride, doubt, and shame.

In the early 1960s, Sander (1964), a psychoanalytically oriented theorist influenced by Erikson, proposed that the mother-child dyad jointly negotiates the development of a number of early adaptations, including the modulation of affect in social interactions (with a focus more on the activation than inhibition of affect). This perspective is consistent with the importance of sensitive maternal responding in attachment theory and with psychoanalytic stage theories such as Erikson's in which children's sense of themselves and their abilities to manage their own impulses are profoundly influenced by the quality of early mother-child interactions.

This theme of dyadic regulation of infants' responding, including emotional reactivity, was picked up again by the psychoanalytically oriented psychiatrist, Daniel Stern in the 1970s and 1980s. He discussed how, through games such as "peek-a-boo" or "I'm going to getcha," the child experiences high levels of excitation or fear—cycling up and down—that are the creation of the dyad, not the child alone: "The infant is with an other who regulates the infant's own self-experience. In this sense, the other is a self-regulating other for the infant. In games like peek-a-boo, it is the regulation of the infant's arousal that is mainly involved" (Stern, 1985, p. 102). Not only is such regulation viewed as the primary source of regulation for the young infant, but in certain situations, the infant is believed to experience both the affect and its regulation as aspects of the self (Stern, 1985). Thus, children's sense of their abilities to self-regulate may emerge partly from dyadic caregiver–child interactions. Similarly, Tronick and his colleagues, building on the ideas of Stern, have argued that if they have sensitive interaction partners, infants participate in self-regulation of emotion by eliciting regulatory reactions from their partners through their own expression of emotion. Mismatches between infants' displays and

adults' responses are viewed as compelling infants to develop their own self-regulatory skills, which (when developed) are useful for coping with prolonged or exaggerated forms of stress in interactions. However, it also is argued that infants' development of self-regulatory skills can be derailed if the adult-child interaction is chronically aberrant (Gianino & Tronick, 1988). These are examples of work stemming from psychoanalytic and attachment perspectives that provide the foundation for current efforts to understand how early parent-child interactions affect the emergence of emotion regulation.

Temperament

Temperament is a construct that existed in Greek and Roman times. The Greeks assumed that a balance among the four humors of yellow and black bile, blood, and phlegm created an inner state responsible for the observed variations in rationality, emotionality, and overt behavior. Through the centuries, various conceptions of how biology contributes to personality have been popular, and many of these included reference to inherited emotional tendencies (see Allport, 1937/1960; Kagan, 1998).

Most early conceptions of temperament in psychology did not explicitly include the notion of regulation. For example, in 1937, Allport defined temperament as "the characteristic phenomena of an individual's emotional nature, including his susceptibility to emotional stimulation, his customary strength and speed of response, the quality of his prevailing mood, and all peculiarities of fluctuation and intensity in mood; these phenomena being regarded as dependent upon constitutional make-up and therefore largely hereditary in origin" (1937/1960, p. 54). Sometime later, in the 1960s, temperament was defined by Thomas, Chess, and Birch (1968) as "the *behavioral style* of the individual child—the *how* rather than the *what* (abilities and content) or *why* (motivation) of behavior. Temperament is a phenomenologic term used to describe the characteristic tempo, rhythmicity, adaptability, energy expenditure, mood, and focus of attention of a child independently of the content of any specific behavior" (p. 4). Thus, this definition of temperament included aspects of functioning related to the regulation of emotion—focus of attention and adaptability.

In the early 1980s, in a landmark chapter, Rothbart and Derryberry (1981) explicitly defined temperament not only as reactivity, including emotional reactivity, but also as an aspect of functioning that serves to regulate reactivity. Reactivity was defined as "characteristics of the individual's reaction to changes in the environment, as reflected in somatic, endocrine, and autonomic nervous systems" whereas self-regulation was defined as "processes functioning to modulate this reactivity, e.g., attentional and behavioral patterns of approach and avoidance" (p. 37).

Ego Control

Another relatively recent line of inquiry relevant to the construct of emotion-related regulation is Jack and Jean Block's work on the construct of ego control and resilience (J. H. Block & J. Block, 1980). The Blocks developed these terms when attempting to integrate aspects of psychoanalytic theory with Lewin's theorizing about the dynamics of motivational states in the individual (e.g., boundary permeability, elasticity; Lewin, 1938; see J. H. Block & J. Block, 1980). As they discussed in their 1980 chapter, the Blocks were interested in ego-functioning structures that help individuals control and modulate their impulses.

The Blocks defined and examined the constructs of ego control and ego resilience. Ego control refers to the "threshold or operating characteristic of an individual with regard to the expression or containment of impulses, feelings, and desires" (J. H. Block & J. Block, 1980, p. 43). It varies on a continuum from ego overcontrol on one extreme to ego undercontrol on the other. Ego undercontrol involves insufficient modulation of impulses, the inability to delay gratification, immediate and direct expression of motivations and affects, and vulnerability to environmental distractors. In contrast, ego overcontrol refers to the containment of impulses, delay of gratification, inhibition of actions and affect, and insulation from environmental distractors.

Based on the Lewinian construct of elasticity, the Blocks defined ego resilience as "the dynamic capacity of an individual to modify his/her modal level of ego-control, in either direction, as a function of the demand characteristics of the environmental context" (J. H. Block & J. Block, 1980, p. 48). High resilience involves resourceful adaptation to changing circumstances and flexible use of problem-solving strategies; low resilience involves little adaptive flexibility, an inability to respond to changing circumstances, the tendency to perseverate or become disorganized when dealing with change or stress, and difficulty recouping after traumatic experiences. Although the Blocks thought that ego control and resilience were statistically independent, they also said that "extreme placement at either end of the ego-control continuum implies a constancy in mode of behavior that, given a varying world, can be expected to be adaptively dysfunctional" (p. 44).

The construct of ego control obviously overlaps with various constructs related to behavioral regulation. An important contribution of the Blocks is their focus on the fact that individuals can be overcontrolled, and may not be able to effortfully relax this control. These ideas feed into recent study of differences between effortful or active types of regulation and types of control that are more reactive and involuntary. I return to this point later.

Coping

Some work on coping, like that on ego control and resilience, has roots in psychoanalytic conceptions of the ego. People such as Haan (1963, 1977) and Valliant (1977) in the 1960s and 1970s developed elaborate systems of coding ego-related coping behaviors. As was stated by Haan (1977), "Taking the Freudian definition of defence very seriously,... I thought to derive each coping function as a counterpart of a defensive one" (p. 3). According to Haan (1977), coping involves "purpose, choice, and flexible shift, adheres to intersubjective reality and logic, and allows and enhances proportionate affective expression" (p. 34). Coping was differentiated from ego defenses, which were viewed as more rigid and irrational, and as involving covert expression of impulses.

Lazarus (1966), working at the same time as Haan on the construct of coping, was influenced more by prior work on stress and cognitive appraisals than by psychoanalytic concepts (see Lazarus & Folkman, 1984). Lazarus and Folkman defined coping as "constantly changing cognitive and behavioral efforts to manage specific external and/or internal demands that are appraised as taxing or exceeding the resources of the person" (p. 141). His ideas about coping have dominated clinical and prevention psychology in recent decades. Among the methods of coping he outlined are means involving the use of cognition and attention to manage negative reactions to stress (e.g., positive cognitive restructuring, distraction) and more active attempts to control stressful situations through planning, problem solving, and following a plan of action. In more recent work by people who have devised measures based on Lazarus' ideas, constructs such as cognitive avoidance are even more clearly defined (e.g., Sandler et al., 1994). Links between coping and regulation have been noted by several writers over the years (e.g., Eisenberg, Fabes, & Guthrie, 1997; Gianino & Tronick, 1988), and in a recent paper, Compas, Connor, Saltzman, Thomsen, and Wadsworth (2001) defined coping as "conscious volitional efforts to regulate emotion, cognition, behavior, physiology, and the environment in response to stressful events or circumstances" (p. 89).

Neurophysiological Work on Inhibition and Approach

In recent work on regulation and associated constructs, investigators are identifying neurophysiological structures believed to underlie inhibition and approach tendencies, as well as their integration. This work has its roots in physiologists' concern about what counters excitation physiologically. For example, in 1874, Brunton (see S. Diamond, Balvin, & F. R. Dia-mond, 1963) discussed vagal inhibition of the heart—neurological functioning related to current notions on respiratory sinus arrhythmia, viewed

as an index of physiological emotion regulation (Porges, Doussard-Roosevelt, & Maiti, 1994). People such as McDougall in the early 1900s and then Pavlov also were interested in the broader concept of physiological inhibition (see S. Diamond et al., 1963). In 1890, William James noted the role of attention in inhibition: "We see that the mind is at every stage a theatre of simultaneous possibilities. Consciousness consists in the comparison of these with each other, the selection of some, and the suppression of the rest by the reinforcing and inhibiting agency of attention." (Vol. 1, p. 288; quoted by S. Diamond et al., 1963, p. 154). Freud, in the *Interpretation of Dreams,* also discussed the suppression or inhibition of thoughts and excitation (1938/1900).

As discussed previously, attentional processes are of importance in current theories and work on emotion regulation. Moreover, as noted by S. Diamond et al. in 1963, behavioral choice and thought rest on inhibitory capacities, including inhibitory processes in the cortex and the limbic systems.

An important more recent contribution to theory on the physiology of behavioral inhibition and activation was made by Gray (1975, 1987). He proposed that there is a Behavioral Inhibition System (BIS), which is activated in situations involving novelty and stimuli signaling punishment or frustrative nonreward, and a Behavioral Activation System or BAS, which involves sensitivity to cues of reward and cessation of punishment. These systems are believed to be primarily subcortical, with numerous projections into the neocortex. Fowles (1987), Patterson and J. P. Newman (1993), DePue and Collins (1999), and others have proposed modifications on Gray's ideas, but the notion of separate (albeit related) social withdrawal and social facilitation or approach systems still is important in theory and measures related to temperament (Rothbart & Bates, 1998), personality, and clinical problems (DePue, Luciana, Arbisai, Collins, & Leon, 1994; Watson, Wiese, Vaidya, & Tellegen, 1999).

In regard to attentional regulation, Luria's observations in the early 1970s provided the basis for some recent work. Luria (1973) distinguished between two attentional systems. The first one is largely involuntary or reflexive: "It is well known to psychologists that those features of the most elementary, involuntary attention of the type which is attracted by the most powerful or biologically significant stimuli can be observed very early on, during the first few months of the child's development" (p. 258). Posner and colleagues have argued that this form of orienting depends on brain structures in the posterior section of the brain (e.g., Rothbart, Posner, & Boylan, 1990). The second type of attention noted by Luria (1973) was voluntary: "These higher forms of voluntary attention do not become possible immediately. Long study has shown that the reverse is the case: the formation of voluntary attention has a long and dramatic history, and

the child acquires an efficient and stable, socially organized attention only shortly before he is due to start school" (p. 259). Luria (1966), building on Vygotsky's (1966) work, also argued that language, a symbolic capacity, can be used to override basic sensorimotor response inclinations and allows for self-directed control. Luria believed that controls of this sort are initially conscious but can become automatic with practice (see Rothbart, 1989, for further discussion of this issue).

Although Luria believed that higher level, voluntary attention was acquired through social interaction and was not biological in character, Posner, Rothbart and colleagues have shown that this sort of attention, called executive or effortful attention, likely is based primarily in the prefrontal cortex, especially the anterior cingulate gyrus. It is involved in volition and awareness of input. As I address shortly, executive attention also seems to be involved in aspects of emotion-related regulation and is related to a variety of important developmental outcomes.

TODAY

The bodies of thinking and research just reviewed provide the foundation for contemporary conceptualizations of emotion-related regulation. For example, the Blocks were instrumental in promoting thinking about the dysfunctional nature of overcontrol and the importance of flexible, optimal levels of control for successful adaptation. Temperament and coping theory, as well as neurophysiological findings, more recently have highlighted the need to examine different modes of control or regulation, including attention, physiological, and behavior domains, and to attend to the distinction between effortful control and more passive/reactive modes of control (e.g., Derryberry & Rothbart, 1997; Posner & Rothbart, 1998). In addition, investigators who have focused on the role of the caregiver–child relationship and on socialization have provided the foundation for current research on the role of attachment and early dyadic interactions in the development of emotion regulation and the processes through which socializers' behaviors and attitudes affect children's social functioning (e.g., through their influence on regulatory processes; Eisenberg, Gershoff, et al., 2001; Gottman, Katz, & Hooven, 1997). Thus, these various traditions have jointly influenced current directions in the field.

In the remainder of this chapter, I focus primarily on the notion of effortful versus less effortful modes of control and the relation of control/regulation to children's social functioning (including adjustment). Issues pertaining to the quality of parent-child interaction and socialization, albeit important, are not considered further.

In framing contemporary thought and research on emotion-related reg-

ulation, it is helpful to make some conceptual distinctions. I have already discussed the heuristic distinction between emotion regulation and emotion-related behavioral regulation. In addition, we have found it useful also to differentiate between regulation and control, with the latter defined as inhibition or restraint. In my view, successful regulation involves optimal levels of control. Like a number of other investigators (including the Blocks, 1980), we believe that well-regulated individuals are not overly controlled or undercontrolled; well-regulated people have the ability to respond to the ongoing demands of experience with a range of responses that are socially acceptable and sufficiently flexible to allow for spontaneity as well as the ability to delay spontaneous reactions as needed (Cole, Michel, & Teti, 1994). Whereas regulation generally is viewed as adaptive, it can be differentiated from the level of a child's *control* of behavior, which may be adaptive or maladaptive, depending on its flexibility and if it can be voluntarily managed.

Effortful or Voluntary Versus Passive (Reactive), Less Voluntary Control

A key distinction in thinking about regulation and control may be voluntary or effortful versus passive or less voluntary control. Well-regulated children would be expected to be relatively high in the ability to voluntarily control their attention and behavior as needed to respond in an adaptive manner. Voluntary control overlaps substantially with what Rothbart has labeled as effortful control. Rothbart has defined *effortful* control as "the ability to inhibit a dominant response to perform a subdominant response" (Rothbart & Bates, 1998, p. 137). Occasionally Rothbart and Posner have defined effortful control so that it might appear to involve only attentional regulation. However, measures of effortful control (including Rothbart's) typically involve attentional regulation (e.g., the ability to voluntarily focus attention as needed) and/or inhibitory control, or the ability to effortfully inhibit behavior as appropriate. Although not included in most measures of effortful control, the ability to activate behavior when one does not wholeheartedly desire to do so (e.g., get moving on a task in the morning when it would be nicer to stay in bed) also would seem to be part of the construct of voluntary control (it is unclear if it is an aspect of effortful control). Recall that effortful control is believed to involve executive functioning in the prefrontal cortex (Mirsky, 1996), which is directly related to awareness of one's planned behavior (Posner & DiGirolamo, 2000). Indeed, Posner (Posner & DiGirolamo, 2000) has argued that the frontal structures in the cortex, especially the anterior cingulate gyrus, are related to our subjective feelings of voluntary control of our thoughts and feelings and come into play when resolving conflict, correcting errors, and planning new actions (Posner & Rothbart, 1998).

Executive attention, which involves effortful control, is believed to show some development in the early months of life and at approximately 18 months of age; however, it is still quite immature at 24 months of age. There is dramatic change in the ability to use executive attention in the 3rd year of life (Posner & Rothbart, 1998). Similarly, the ability to effortfully inhibit behavior on tasks such as "Simon says" appears to emerge at about 44 months of age (Posner & Rothbart, 1998) and is fairly good by 4 years of age (Reed, Pien, & Rothbart, 1984). Findings in studies with children age 5 and older demonstrate that the anterior cingulate is involved in the ability to perform tasks involving attentional control (Casey, Trainor, Giedd, et al., 1997). Moreover, the prefrontal cortex has been implicated in MRI research on children's ability to perform on tasks requiring them to withhold pressing an apparatus in response to an X while responding to non-Xs (Casey, Trainor, Orendi, et al., 1997; Nigg, 2000). Although effortful control may be fairly well developed by 4 or 5 years of age, there is evidence that regulation in general improves across childhood and into adulthood (Murphy, Eisenberg, Fabes, Shepard, & Guthrie, 1999; Williams, Ponesse, Schachar, Logan, & Tannock, 1999).

In contrast to effortful types of regulation, there are aspects of control, or the lack thereof, that seem to be involuntary or so automatic that they are not usually under voluntary control. These might include some types of impulsivity or, at the other extreme, very low impulsivity as in over-controlled children who are timid, constrained, and lack flexibility in novel or stressful situations. Derryberry and Rothbart (1997) noted a difference between active (effortful) and more passive types of control and suggested that behavioral inhibition (i.e., inhibition to novel or challenging situations) involves reactive anxiety or fear and is a good example of less voluntary overcontrol. Nigg (2000) and Mezzacappa, Kindlon, Saul, and Earls (1998) made similar distinctions between executive control and what they called motivational control (including behavioral inhibition).

According to Gray (Pickering & Gray, 1999), impulsive (uncontrolled) behavior is associated with high BAS and relatively low BIS functioning, whereas the BIS system inhibits behavior. Such reactive systems appear to be substantially seated in subcortical systems (e.g., the amygdala for inhibition and mesolimbic dopamine pathways for approach; Cacioppo, Gardner, & Berntson, 1999; Pickering & Gray, 1999) discussed by Gray (1987) and others. The BIS/BAS systems are likely to play a major role in reactive (less voluntary) control. Thus, these aspects of responding can be seen as being based in physiology and temperament, and as reflecting a dispositional tendency toward particular actions such as fearful avoidance or interested approach.

Of course, given the complexity of the human brain, there are many connections between the cortical and subcortical functions and structures.

For example, approach and withdrawal systems are linked to asymmetries in frontal brain activation (Tomarken & Keener, 1998), with right frontal brain activation associated with behavioral inhibition in children (Calkins, Fox, & Marshall, 1996) and left brain activation associated with positive emotionality and heightened functional activity of the mesolimbic dopamine system. Thus, it is likely that effortful and less voluntary (i.e., reactive) aspects of control are mediated by somewhat different parts of the brain, and that the more voluntary and effortfully controlled regulatory processes sometimes modulate the expression of less voluntary, reactive functioning (see Derryberry & Rothbart, 1997). Consistent with this view, Patterson and J. P. Newman's (1993; J. P. Newman & Wallace, 1993) work suggests that disinhibited individuals (who tend to be impulsive) are not only high in approach, but also have difficulties in at least two other ways: (a) orienting to aversive events that have implications for changes in action, and (b) making an effortful switch from automatic processing under the expectancy of reward to controlled processing of situational factors when there is the unexpected interruption of approach-related activity. Also suggestive of different neurological bases of effortful and passive control, vagal modulation of respiratory driven, high-frequency heart-rate variability has been associated with executive control (i.e., effortful control) on behavioral tasks, whereas motivational control (e.g., passive avoidance, avoidance of punishment, and low reward dominance) has been correlated with sympathetic modulation of heart-rate variability (Mezzacappa et al., 1998).

In our thinking, passive control, like effortful regulation, likely includes control (or the lack thereof) of internal states (including emotion) as well as of behaviors associated with emotion-related internal states. Examples of reactive functioning reflecting passive behavioral control include BAS/BIS-related impulsivity and overcontrol of behavior. Involuntary focusing on recurring thoughts (rumination) and lack of effortful control over intrusive thoughts are examples of involuntary control in the attentional domain, although it is unclear how much these responses are linked to automatic versus effortful processing and which parts of the brain are involved most (see Nigg, 2000).

Although reactive, passive modes of control likely are relatively involuntary and automatic, the meaning of the term *involuntary* is not quite the same as "automatic." As suggested by Luria, some aspects of effortful control may become relatively automatic with frequent use. Thus, it is important to consider the possibility that automatic modes of control can actually include initially effortful types of control that, through practice, occur in a relatively automatic manner. The part of the brain involved in processing may change as an effortful behavior becomes more automatic (Posner & DiGirolamo, 2000), and although the given aspect of control

may function as if it is involuntary, effortful control of the response may be readily available if the need for it arises. Thus, an automatic response may not be one that is difficult to regulate voluntarily.

Current Status of Measurement of the Construct of Emotion-Related Regulation or Control

As alluded to earlier, lack of regulation of emotional experience or emotion-related behavior is believed to contribute to children's social competence and moral functioning (J. H. Block & J. Block, 1980; Eisenberg & Fabes, 1992; Pulkkinen, 1982). Thus, in the past decade, numerous investigators have examined the relations of various indexes of control and children's social appropriateness, popularity, empathy/sympathy, low levels of aggression and negative emotionality, and measures of conscience (e.g., Caspi, 2000; Eisenberg, Fabes, Guthrie, & Reiser, 2000; Kochanska, Coy, Tjebkes, & Husarek, 1998; Kochanska et al., 1997; Pulkkinen & Hamalainen, 1995; Rothbart, Ahadi, & Hershey, 1994). In much of this work, however, it is difficult to know if a given measure of "regulation" taps a combination of emotionality and regulation, effortful control, some combination of effortful control and reactive control processes, or another construct due to confounding of items.

Consider the following example. Derryberry and Rothbart (1997) suggested that the Blocks' construct of ego control refers "to an emotion-related (fear) system that is associated with categorical, restrictive control of behavior. The child who is anxiously watching may be locked into this system and unable to flexibly construct a strategy to reduce anxiety" (p. 645). I agree with Derryberry and Rothbart that very high levels of ego overcontrol likely reflect reactive (passive) control (e.g., high BIS responding), whereas high levels of ego undercontrol reflect high BAS responding and/or lack of sufficient BIS responding. However, moderately high scores on measures of ego control probably partly reflect effortful control, perhaps combined with some anxiety-related BIS-type control. The situation is further complicated by the manner in which ego control often is assessed. Although J. H. Block and J. Block (1980) used both behavioral measures and adults' reports to assess children's ego control, many investigators have used only the latter. Typically under- or overcontrol is measured with the long list of Q-sort items developed by the Blocks (J. H. Block & J. Block, 1980) or with similar descriptive items. Unfortunately, these items often tap aggression, social competence, emotionality, and a host of other aspects of functioning (e.g., self-esteem). Thus, often the construct of ego control, as operationalized in empirical research, has measured social behavior and emotionality more than control/regulation.

Similarly, we do not know enough about what many behavioral meas-

ures of control/regulation tap. Kindlon, Mezzacappa, and Earls (1995) collected a number of behavioral measures of "impulsivity" and obtained two clusters in a factor analysis: (a) an inhibitory control factor (reflecting responses to a Stroop task, times the child required redirecting back to tasks, the ability to stop behavior in response to a signal, and the ability to inhibit a strong competing response on the trail-making task) and (b) a factor believed to reflect insensitivity to punishment or nonreward, that is, BAS/BIS types of responding (e.g., the relative failure to exhibit nonresponse within a motivationally salient context of earning money or points). Olson, Schilling, and Bates (1999) also obtained two factors using somewhat similar tasks: one that seemed to reflect inhibitory control (e.g., the ability to inhibit motor behavior on command in walking and drawing tasks, as well as reflective performance on the Matching Familiar Figures Task [MFFT]), and one that reflected the ability to wait patiently when confronted with a reward (gift). The child's stated willingness to wait for a larger but delayed reward did not load on either factor, perhaps because it likely taps effortful control (if children intend to distract themselves), reward-based reactive impulsivity (if they are "pulled" by the immediate rewards), or both. However, White et al. (1994) found that delay of gratification, a task involving inability to inhibit playing a game in the face of losing rewards, and other measures believed to reflect inhibitory control (Stroop errors, circle tracing) all loaded together when factored with other-report measures of undercontrol, motor restlessness, lack of persistence, and impulsivity (which loaded on a second factor). However, Olson, Bates, and Bayles (1990) found that whereas measures of inhibitory control (MFFT performance, motor inhibition) tended to load on one factor, rated task orientation (which likely partly reflected inhibitory control) and delay of gratification (attempts to open a gift prematurely) tended to load on the other. The lack of consistency across studies is understandable because these studies differed not only in the tasks used, but in the age and age range of the children involved. Moreover, although it has been argued that tasks in which individuals have the option of stopping in the face of diminishing rewards and increasing punishment tap BAS/BIS responding, they also may assess the ability to make an effortful switch from automatic processing under the expectancy of reward to controlled processing of situational factors when approach activities are interrupted (Patterson & J. P. Newman, 1993). Thus, tasks involving rewards or punishment may reflect primarily effortful control for some children (especially those who behave in a regulated manner), a preponderance of reactive BIS/BAS responding for other children, or some of both for many children.

In addition to behavioral measures of inhibitory control or impulsivity, parents or teachers sometimes have been asked to rate children on aspects of effortful control (e.g., attention focusing, inhibitory control) or impul-

sivity, often using subscales from Rothbart's Child Behavior Questionnaire, a measure of temperament (e.g., Eisenberg et al., 1993; Eisenberg, Fabes, et al., 2000; Rothbart et al., 1994). Adults' reports of attentional or inhibitory control tend to be positively and significantly related to observed performance on tasks likely to tap effortful control (Eisenberg, Fabes, Guthrie, et al., 1996; Kochanska et al., 1997) or some combination of effortful control and impulsivity (Eisenberg et al., 1995; Eisenberg, Fabes, Shepard, et al., 1997). Behavioral measures of inhibitory control also tend to relate negatively to adults' reports of children's impulsivity (e.g., Eisenberg et al., 1995; White et al., 1994). Thus, there seems to be some general consistency among behavioral and adult-report measures of control/regulation, despite the lack of precision in some of the measures.

Theoretical Expectations

Based on the Blocks' theorizing, personality and developmental psychologists have attempted to identify groups of overcontrolled, undercontrolled, and resilient children and adolescents in several countries, including the United States, Iceland, Germany, and New Zealand (e.g., Asendorpf & van Aken, 1999; Hart, Hofmann, Edelstein, & Keller, 1997; D. L. Newman, Caspi, Moffitt, & Silva, 1997; Robins, John, Caspi, Moffitt, & Stouthamer-Loeber, 1996). However, these groupings generally have been constructed based on items that tap social and emotional functioning more than regulation or control per se. Moreover, these groups of investigators generally have not discussed whether the construct of "control" in their work is effortful regulation or reactive, passive control.

Nonetheless, it is reasonable to assume that different aspects of control, as well as different styles of coping, are associated with overcontrolled, undercontrolled, and resilient (or optimally regulated) personality styles (Eisenberg & Fabes, 1992; Eisenberg, Fabes, et al., 2000). Specifically, we have hypothesized that highly inhibited individuals are high in involuntary behavioral inhibition (e.g., inhibition to novelty, rigidity of behavior); low to average in the abilities to voluntarily or effortfully inhibit or activate behavior as needed; low in voluntary, effortful attentional regulation (especially attention shifting); and low in instrumental, problem-focused, or engagement-coping strategies, including proactive coping that requires action. Thus, these individuals are seen as somewhat low in effortful regulation—especially attentional regulation that relates to the control of internal emotional states—and high in involuntary or passive control. They are expected to be prone to internalizing problems, especially if they are predisposed to negative emotionality.

In contrast, undercontrolled individuals are predicted to be low in all types of emotional and effortful behavioral regulation, including inhibi-

tory, attentional, and activational control, as well as low in adaptive (i.e., planned) problem-focused and proactive coping behavior. They also are hypothesized to be low in involuntary inhibition and high in reactive approach tendencies—that is, they are expected to be impulsive and low in behavioral inhibition. In general, people with this style of control are predicted to be relatively low in social competence and prone to externalizing behavior problems.

Finally, optimally regulated individuals are hypothesized to be high in various modes of adaptive effortful regulation, and in regard to involuntary control, neither overcontrolled nor undercontrolled. They are expected to be relatively high in engagement (active) coping and well adjusted, socially competent, and resilient to stress.

An important issue is whether effortful and reactive modes of control relate differently to developmental outcomes; they are more meaningful constructs if they provide some differential prediction. If we are correct in our theorizing, one would expect externalizing problem behaviors such as aggression, defiance, stealing, and the like to be linked to low effortful control and high impulsivity (or to the other end of the reactive continuum—low reactive inhibition control). Thus, both effortful and reactive control are expected to be negatively related to externalizing behavior. In contrast, internalizing problems such as social withdrawal and anxiety would be expected to correlate with *low* effortful control and with high reactive control (or low impulsivity). Thus, although measures of impulsivity and effortful control tend to be substantially negatively related, we would expect them both to be negatively related to internalizing and to provide some unique prediction.

Empirical Data

Contemporary empirical findings generally provide support for the role of control/regulation in the quality of children's social functioning, including their social and moral behavior and adjustment. The data also support, at least to some degree, the distinction between effortful and less effortful types of regulation.

Externalizing Problems and Social and Moral Competence. There is mounting evidence that indexes of both effortful control and impulsivity—or low reactive control—predict externalizing problems. For example, Rothbart et al. (1994) found that a composite of parent-reported effortful control was negatively related to 7-year-olds' aggression and negativity, and positively related to their empathy and guilt. Surgency, which included impulsivity (along with high-intensity pleasure, smiling and laughter, activity level, and low shyness) was also related to aggression and low guilt.

Kochanska (Kochanska et al., 1997; Kochanska, Murray, Jacques, Koenig, & Vandegeest, 1996) has repeatedly found that her behavioral measures of inhibitory control and parents' reports of this construct during the toddler, preschool, and early school years predict internalized compliance, moral behavior (low cheating), rule-abiding behavior, and/or low selfish/antisocial solutions to hypothetical dilemmas in the early school years (Kochanska et al., 1997). Similarly, Kochanska, Murray, and Harlan (2000) found that similar measures of effortful control at 22 months and at 33 months of age predicted lower anger and compliance with a prohibition at 33 months of age. Cole, Usher, and Cargo (1993) noted that a composite of measures of executive functioning, including those tapping attentional and inhibitory control, was associated with behavioral control when children were told not to touch a jar of toys and candies while left alone. As was discussed earlier, the ability to delay gratification has been found by Mischel and colleagues to predict a host of positive outcomes over a decade later, including academic and social competence and the ability to cope with frustration and stress (Mischel, 2000; Mischel, Shoda, & Peake, 1988; Mischel et al., 1989). Moreover, other measures of delay of gratification have been linked to low externalizing problems (Krueger, Caspi, Moffitt, White, & Stouthamer-Loeber, 1996; Olson et al., 1999). Externalizing problems also have been predicted by low scores on stop-go tasks of inhibitory control (e.g., Oosterlaan & Sergeant, 1998), lack of persistence on a task (Eisenberg, Guthrie, et al., 2000), and a composite of measures reflecting performance on a Stroop task and tasks involving shifting orientations and inhibiting actual behavior (Mezzacappa, Kindlon, & Earls, 1999).

Interestingly, fearfulness as assessed by behavioral inhibition in an unfamiliar laboratory setting and/or maternal ratings also tends to relate to a variety of measures of conscience for children with mothers who deemphasize power in their discipline (e.g., Kochanska, 1991, 1993, 1997), but not for children whose mothers are prone to use power in disciplinary interactions. No such moderation has been reported for children high in inhibitory control, so it is likely that inhibitory control predicts outcomes regardless of maternal discipline. Thus, it seems that reactive behavioral inhibition and effortful control both relate to measures of compliance and conscience, although moderators of this effect may vary for different types of control.

Consistent with findings for behavioral measures, adults' reports of children's low effortful control and high impulsivity have been associated with externalizing problems. For example, Lengua, West, and Sandler (1998) found that mother-reported attention focusing was negatively related to mother- and child-reported conduct problems, whereas mother-reported impulsivity was positively related to both (also see Huey &

Weisz, 1997, for relations of ego control to externalizing behavior). White et al. (1994) found that both a behavioral measure of low effortful control and adult- and self-reported impulsivity/undercontrol correlated with self-reported delinquency at age 10 and age 12–13, but the relations were considerably stronger for the reports of impulsivity/undercontrol, especially teacher- and self-reports, than for behavioral measure.

In our own longitudinal work, teacher-reported attentional control at age 4–6 years predicted social competence (a composite of socially appropriate behavior, popularity, and low aggression and disruptive behavior) at school at age 8–10. Mothers' and teachers' reports of attentional and behavioral effortful regulation, combined with low impulsivity, at age 6–8 also predicted socially competent behavior at school and levels of parent-rated externalizing problems concurrently and 2 and 4 years later. Significant relations often were obtained within a given context (school or home), albeit not solely within reporter (e.g, findings often were across teachers or across parents; Eisenberg, Fabes, Shepard, et al., 1997; Murphy, Shepard, Eisenberg, & Fabes, 2000). Some of the correlations were substantial; for example, teachers' reports of attentional control at age 4–6 correlated .44 with the composite measure of school competence at age 8–10 and .28 with social competence at age 10–12. The correlation from age 8–10 to age 10–12 was .56 (Murphy et al., 2000).

Thus, it is fairly well established that both low effortful control and high impulsivity are associated with externalizing problems and low social competence. Findings are somewhat less clear for children who are comorbid for externalizing and internalizing problems. O'Brien and Frick (1996) did not find that an index of impulsivity or reward dominance (children's stopping playing a game when punishment increased and rewards decreased)—which likely taps reactive control and perhaps executive attentional abilities—was related to externalizing problems for children who were comorbid (i.e., were high in anxiety). Further, in another study, performance on this task was related to disruptive behavior disorders only when the effect of anxiety was controlled (O'Brien, Frick, & Lyman, 1994). In contrast, Krueger et al. (1996) compared groups of externalizing and comorbid children with nondisordered children and found that the former groups (combined) were lower in delay of gratification. Although, based on the mean delay scores, the comorbid group was clearly lower in delay than the nondisordered group (but higher than pure externalizers), this comparison was not tested. Similarly, Huey and Weisz (1997) found that comorbid clinic children were lower in ego control than nonclinical children, but less undercontrolled than pure externalizers. Anxiety (an aspect of internalizing) is believed to reflect reactive behavioral inhibition, which may counter the disinhibitory tendencies (e.g., BAS responding) associated with externalizing problem behaviors.

Eisenberg, Cumberland, et al. (2001) compared children with at least borderline levels of externalizing (with or without comorbid internalizing problem behaviors) with nondisordered children. The former group was lower than nondisordered children on parents' (usually mothers') and teachers' reports of effortful attention shifting, attention focusing, and inhibitory control, and higher in reports of impulsivity, as well as on some behavioral measures of control. These findings generally held across reporters. There were few consistent differences between comorbid and externalizing only children in regulation or impulsivity. Thus, although children with severe anxiety may be less reward driven or impulsive than children with externalizing problems or comorbid children, it appears that comorbid children are less effortfully regulated and sometimes more impulsive than children without disorders. Perhaps more severe anxiety or internalizing problems have a more marked attentuating effect on impulsivity than was found in Eisenberg, Cumberland et al.'s study, which did not involve many diagnosed or clinic-treated children.

Although it is clear that both impulsivity and low effortful control predict externalizing problems, even over time, researchers seldom have controlled for earlier levels of externalizing problems when predicting later levels. Thus, it is difficult to draw even tentative conclusions about causality from most data. In a study in which investigators did control for initial levels of the outcome variable, Colder and Stice (1998) found that self-reported temperamental impulsivity did not predict high school seniors' delinquency over the school year when delinquency at the initial assessment was controlled. However, there was high consistency in reported impulsivity over time, which could account for the lack of an effect. In research conducted with young school-age children, Eisenberg, Guthrie, et al. (2000) used structural equation modeling to test prediction of externalizing problems from both attentional control (attention shifting and focusing) and behavioral control (i.e., items from Block's ego control scale and a measure of persistence on a task when working toward a reward rather than cheating). The construct of behavioral regulation likely reflected both effortful control and low impulsivity. At Time 2, which was a 2-year follow-up, both attentional and behavioral regulation had unique effects on externalizing problem behavior, even when the relation between earlier and later problem behavior was controlled in the analysis. The negative relation of attentional control to externalizing problems was moderated by negative emotionality, such that it was stronger for children prone to negative emotions. Similar moderation was not significant in the structure equation model for behavioral regulation, although the relation between behavioral regulation and externalizing problems was moderated at T1 and T2 in a similar manner when we tested for moderation using teacher-report (but not parent-report) data in regression analyses.

Internalizing Problems. As discussed previously, because low levels of both reactive/passive control and effortful control are both expected to predict externalizing problems, it is difficult to use relations between these constructs and externalizing outcomes to test the notion that effortful control and impulsivity are two distinct, albeit related, constructs. However, the case is more interesting in regard to internalizing problems. Children with internalizing problems are expected to be particularly prone to behavioral inhibition and low impulsivity—that is, to be high in the reactive aspects of control—but are expected to be relatively low in effortful control, especially attentional control (which is expected to be involved in regulating negative emotions such as anxiety). Thus, the two aspects of control are hypothesized to relate in different directions to internalizing problems.

There is substantial evidence that young children who are prone to behavioral inhibition—who are constrained when dealing with novel or stressful situations—are prone to developing anxiety problems over time (Biederman et al., 1990; Kagan, Snidman, Zentner, & Peterson, 1999; Rosenbaum et al., 1993; see Rothbart & Bates, 1998). There is less research examining the relation of effortful control or the other extreme of reactive control (i.e., impulsivity) to internalizing problems. Using a delay of gratification measure that probably reflected BAS tendencies as well as attentional regulation, Krueger et al. (1996) found no difference between children with internalizing problems and nondisordered children. In contrast, Huey and Weisz (1997) found that teacher-, but not self- or parent-rated internalizing problems, were positively related to ego overcontrol. However, Oosterlaan, Logan, and Sergeant (1998), in a meta-analysis, found that anxious children were nonsignificantly slower to stop on a task (the Stop Task) that likely taps executive control.

Eisenberg, Cumberland, et al. (2001) examined the differences between internalizing, externalizing, and nondisordered children (based on a cutoff score of T = 60 on Achenbach's parent and teacher measures; Achenbach, 1991) on various types of regulation or control. Internalizing children were consistently higher than externalizers in adult-rated inhibitory control, but did not differ much from nondisordered children on this dimension. Internalizers, in comparison to externalizers, tended to express less negative emotion when disappointed (an index of regulation) and were somewhat more persistent when working on a task toward a reward; they were only slightly less regulated on some tasks (marginal findings for one sex or the other) than control children. Internalizers tended to be lower than nondisordered children in effortful attention shifting or focusing, and higher than externalizing and comorbid children on these aspects of effortful control. They also were lower than both nondisordered and externalizing children in impulsivity. Thus, internalizing children were

quite high in reactive control (as tapped by low impulsivity) but were low in effortful attentional control. Whether they were actually lower in inhibitory control than nondisordered is difficult to know for sure because adult raters may confuse reactive inhibition with effortful inhibitory control. However, internalizing children were only slightly less controlled on behavioral measures than control children, suggesting that they have at most slight deficits in behavioral control. Thus, effortful and reactive control appeared to be functionally different in their relations to internalizing problems; nor did effortful inhibitory and attentional control relate to the same degree to internalizing problems.

Relations of Impulsivity and Effortful Control to Resilience. If reactive and effortful aspects of behavioral control are different, they might also be expected to relate differently to ego resilience. Logically, one would expect children high in effortful control, because they can adjust control as needed, to be resilient. In contrast, children high in reactive control—overcontrolled children—would be expected to be rigid and nonresilient. Children moderate to low in reactive control, who are a bit spontaneous and impulsive, should be more likely than overcontrolled children to deal well with stress and change. These predictions are consistent with J. Block and Kremen's comment (1996) that "the human goal is to be as undercontrolled as possible and as overcontrolled as necessary. When one is more undercontrolled than is adaptively effective or more overcontrolled than is adaptively required, one is not resilient" (p. 351).

In our most recent work, we assessed ego resilience with seven of J. H. Block and J. Block's (1980) items that were rated by a panel of experts as most representative of ego resilience (e.g., "Can bounce back or recover after a stressful or bad experience"). In a study of preschool children (Cumberland, Eisenberg, & Reiser, 2001), we found that impulsivity and effortful control were both positive correlates of ego resilience and each provided some unique prediction of resilience. As alluded to already, this may be because resilience reflects, in part, a healthy degree of spontaneity, as well as the ability to regulate oneself as needed.

The preschool sample in the initial study was small and contained few children at risk. Thus, we also examined the relation of impulsivity and effortful control to resilience in our sample that was the largest and most diverse in regard to problem behaviors. We examined these relations when the children were 4.5 to nearly 8 years old and then 2 years later. Again, there were modest significant positive correlations between impulsivity and resiliency at Time 1, $rs = .21$, $p < .004$, for teachers' reports, and $r = .17$, $p < .014$, for mothers' reports. Two years later, these correlations were positive, but weaker and nonsignificant. However, at both ages, there was a quadratic relation between teachers' (but not parents') reports

of impulsivity and resilience, such that children at the mean level of impulsivity were more resilient than children low in impulsivity, and children average and high in impulsivity were similar on resiliency (Eisenberg, 2000b). In this sample, measures of effortful modes of regulation also tended to be positively related to resiliency (within and sometimes across reporter); *rs* ranged from *.27* to .52 for teacher data and from .15 to .39 for parent data, with the correlations being somewhat stronger at the older age. No quadratic relations were marginally significant or significant at either time for attention shifting, focusing, or inhibitory control. Thus, resiliency in children seems to be related to both effortful control and impulsivity (which we assume reflects low reactive control), although the latter relation is modest, may decrease with age, and is curvilinear for teacher-report data. These findings are further evidence that level of effortful and reactive control often may not be related to outcomes in the same manner.

FUTURE DIRECTIONS

Initial data are consistent with the view that effortful control and reactive, less voluntary control are related but have different physiological bases and relate somewhat differently to some developmental outcomes. However, initial work on this topic is only suggestive. There is some disagreement in regard to the exact neurological bases of each type of control (e.g., Mirsky, 1996); neuropsychologists are making considerable strides in pinpointing portions of the brain and may resolve this issue in the next decade. Moreover, our measures for assessing the behavioral manifestations of different aspects of control are crude. For example, it is unclear the degree to which numerous tasks, such as delay of gratification tasks and tasks in which participants have the option to stop playing a game when they start to lose rewards, tap reactive versus more effortful modes of control. In addition, measures of reactive inhibition such as fear of novelty in laboratory situations may not be well suited for older children, and it is not clear how much dispositional measures of impulsivity tap BAS responding or low BIS responding (some may even measure effortful control to some degree).

To move forward in understanding the role of emotion-related regulation and control in development and developmental outcomes, it will be important for developmentalists to draw upon work in neuropsychology in thinking about distinctions among various aspects of control. Moreover, we will need to be more thoughtful about our measurement of various measures of control. New measures of effortful regulation being adapted from work on attention in cognitive and neuropsychology are promising

(e.g., Nigg, Hinshaw, & Halperin, 1996). For example, Wilson (2000) found that the ability to shift attention on a computer-based task differed for rejected/aggression and less aggressive/popular children. A related, difficult issue is how to deal with the confounding of items that often occurs across scales of regulation/control and measures of social outcomes or emotionality. Procedures such as using experts' ratings or confirmatory factor analyses to eliminate confounding of questionnaire measures due to overlapping items are promising. However, because scales can lose the majority of items when such procedures are applied (Lengua et al, 1998), it is difficult to know what is being assessed in the end. In addition, the field would benefit from more nuanced measures of behavioral problems and social competence. For example, externalizing problems driven by unregulated emotion may differ somewhat in their origins and correlates from those that are more calculated and driven by pragmatic outcomes or are more covert (Dodge, Lochman, Harnish, Bates, & Petit, 1997), and may relate differently to measures of regulation/control.

Similarly, there may be adaptive and maladaptive forms of impulsivity, with the former relating more closely to resilience. Dickman (1990) found that he could differentiate between dysfunctional impulsivity (the tendency to act with less forethought than most people of equal ability when this tendency is a source of difficulty) and functional impulsivity (the tendency to act with relatively little forethought when such a style is optimal). Specifically, functional impulsivity involved the tendency to make decisions quickly when appropriate and to prefer situations that are fast paced and involve quick decisions. Dysfunctional impulsivity tended to reflect acting without thinking or considering the consequences and not reasoning carefully about problems or decisions. These two tendencies were only modestly positively correlated with one another in adults and related differently to some personality characteristics (e.g., orderliness and cognitive structure) and performance on a task. Dickman's findings, along with our findings on the relation of impulsivity to personality resilience, suggest that there may be different types of impulsivity or that the ability to effortfully modulate one's level of impulsivity as a function of the context may be an important dimension to examine in children.

Another important task for the future is to clarify similarities and differences among related constructs such as coping, regulation, and resilience. For example, do regulation and coping overlap primarily in regard to stressful situations, with regulation but not coping also occurring in nonstressful contexts? Is coping always effortful as asserted by Compas et al. (2001)? Conceptual clarity would contribute to clearer predictions of relations between control/regulation and socioemotional functioning and to more precise operationalization of measures. Moreover, clarification of such issues might help to identify situations in which proactive versus re-

active regulation/coping occurs, which in turn could spur much needed research on proactive coping and regulation in children.

The current state of the literature on regulation in children can be viewed as building on both theory in the past millennium (especially from the last 30 years) and methodological and technical advances. Ideas regarding involuntary mental processes and coping processes are reminiscent of Freud's theorizing. The importance of the Blocks' seminal ideas and research concerning over- and undercontrol and the flexibility of control (i.e., resiliency) has been fully recognized and integrated into conceptual models and empirical research only in the last 5 to 10 years. Indeed, the ideas of the Blocks, Lazarus, Vygotsky, Gray, and others examining regulatory systems, coping, and attention continue to provide intellectual fodder for developmental, personality, and clinical investigators. In addition, technological advances in neuropsychology have allowed researchers to search for and identify likely neurological bases for various aspects of control/regulation. Freud would be very gratified to learn about the links between anatomical sites in the body and interrelated attentional, cognitive, and emotional processes. Researchers assessing temperament, coping, emotion-related behavioral regulation, and resiliency continue to refine prior methods, although the early measures used by the Blocks, Mischel, Thomas and Chess, and others are similar to some contemporary methods and provide examples of useful approaches. Similarly, theorists and researchers examining the role of caregiver-child interactions and early socialization frequently harken back to the work of Bowlby, Ainsworth, Erikson, and others for conceptual and sometimes methodological scaffolding. Thus, there is continuity between prior work on emotion-related regulation and current directions in the field, although the links sometimes have been temporarily forgotten or unrecognized. However, many important questions have not yet been addressed fully by either past or contemporary investigators, including issues to do with the origins of emotion-related regulation; processes involved in its expression, development, and modification; and variables that moderate its expression and quality. Clearly, there is much to be done in the next decade and millennium, although the advances in theory and knowledge in recent decades bode well for our success in tackling these issues in coming years.

ACKNOWLEDGMENTS

This research was supported by a grant (1 R01 MH 60838) and a Research Scientist Award from the National Institute of Mental Health (K05 M801321), as well as a grant from the National Institute of Drug Abuse (DA05227).

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6

Children Develop in History: So What's New?

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DEVELOPMENT AND HISTORY

"So what's new?" The second half of our title is meant to suggest that our tone in this essay is provocative and insistent. The first part of our title even indicates that we take it for granted that our readers have already incorporated the message—perhaps Vygotsky's (1978) or Bronfenbrenner's (1979), or Sheldon White's (White & Siegel, 1984)—that children's development takes place in context, and that contexts change, often rapidly enough that children develop in a world that differs from the one in which their parents came of age.

Vygotskians have argued forcefully for the social quality of children's activity as they develop. They tell us that children do not discover wholly new ways of doing or thinking things for themselves, but instead use the scaffolding provided by more advanced practitioners, the components of which are sociocultural in origin. Those who scaffold children's development, moreover, are not *just* experts. They also participate in a more extensive, often emotionally laden relationship with the learner, a relationship subject to passionate variation and elaboration. Development does not just happen to the learner: It happens to the relationship as well (Fogel, 1993; Hartup, 1986; Rogoff, 1990).

We feel comfortable presuming that, now, many and even most developmentalists can and will make the distinction between "environment," which is simply "out there," external to the workings of development, and

"context," which transacts, back and forth, with developing actors (Rogoff, 1990). Having done so, we hope, they will bear with us as we seek to explore here some of the implications for developmental study of the embeddedness of context, in history in a broader sense.

But there are historical corollaries of an awareness of the importance of context for development to which even contextual developmentalists may have given little thought. These transactions with context may be radically different at different historical periods (events at other levels affecting contexts, for instance). Moreover, transactions with context may feed back into history. Through cumulation or other processes they may set in motion events affecting transactions, or affecting superordinate levels that are contexts to proximal transactions. We need a sense of history that recognizes how change often occurs from the aggregation of individual actions (Riley, Johnson, & Foner, 1972), through the process of emulation, or through a modification of life trajectory induced by what previous actors have done to affect the circumstances within which further action is taken.

Michael Cole (1996), of course, is only the most prominent developmentalist to show the fallacy of presuming that at the surface and even beneath the surface, children's development looks alike in different cultures. Developmentalists have profited from Cole's admonition by seeing the developmental meaning of "context" more clearly. But the correction that Cole and his fellow comparativists have provided has been more methodological than substantive, more epistemological than ontological. Cole asserts that one cannot crudely apply identical *measures* across cultures and expect to derive adequate comparative evaluations of *concepts*.

To begin this essay, we examine in preliminary fashion the role of contexts in history, development, and their interrelationships. This discussion sets the stage for considering the scope of our argument, as organized around four propositions and their research examples:

1. Children develop within the social arrangements of a given moment.
2. These arrangements are changed by events and trends.
3. Developing individuals change history.
4. Cultures make sense of the ways of development.

Research on planning in late adolescence provides other examples of the analytic value of taking this perspective, as well as evidence of the limitations and misrepresentations that result from its neglect. John Clausen's work on planful competence provides us with an example of the problems that crop up when developmental accounts neglect history, and pay some special attention to his neglect of change in gender role in the 20th century. We close by noting that the personal qualities of planful compe-

tence influence the life course of individuals, their development, and their social institutions, culture, and collective history.

CONTEXTS IN HISTORY AND DEVELOPMENT

We assert that historical *alterations* in context are not annoyances, to be grudgingly recognized in the interest of discounting their effects when we seek to describe development. Rather, *change in context is an integral part of the story of development, and development itself plays a part in the story of contextual change* (Cairns, Elder, & Costello, 1996). We can put this another way, more nearly in the language of the life course (Elder, 1998a, 1998b): The developmental trajectories and social pathways that lives entail are coherently structured by contexts, which, in turn, are subject to change, both from the impact of the broader contexts in which they are embedded, and from the force of the aggregation of lives that follow these pathways.

Even a contextualized view of development that describes how members of *our species* grow up will not do, because species views are timeless or set in extra*historical* time. Children develop in a briefer historical time frame that intersects with the developmental timetable of the individual. A species view will not do even if the species is seen under the rubric of evolutionary change, for children like adults are (sometimes) self-regarding actors, and they are (sometimes) aware of history but never of evolution as they plan, enact, and evaluate their actions.

Children are often aware of historical change, and they are sometimes themselves a part of that change (Werner, 2000). But our argument is not primarily about *event-centered* history. It is more about the kind of less dramatic, more *structural* history that surrounds us all the time: The life courses of father and son, mother and daughter are embedded in these historically evolving structures (Kinser, 1981). The life course represents a changing historical context for the individual and it is nested in the historical time and space of social institutions and societies. And yet, it does not just happen to children, for children plan and act, appropriate, discard, and modify as they develop, and they do so with regard to their social situations (including, important to note, one another), in consequential ways (Baker-Sennett, Matusov, & Rogoff, 1993). When children's imagined contexts are not what we imagine them to be (perhaps because of our sometimes reified scientific concepts), we cannot comprehend their agency, and, hence, their development (Modell, 1999). Thinking historically can help us here.

A while back, we collaborated with Ross Parke in a series of workshops that were designed to bring together the perspectives of social history and human development. We learned a great deal in doing this work, and it

enabled us to assemble a set of worthwhile papers in *Children in Time and Place* (Elder, Modell, & Parke, 1993), along with a discussion of how challenging we found the collaboration between practitioners of these two remarkably remote specialties. Reflecting upon the workshops, the social historian Michael Zuckerman (1993) called for "a historical social science in which considerations of context could challenge our sense of our situation, ... a more active construction of context, in which it actually impinged on our agendas [for research] themselves,... a history that was ingredient to our analytic purchase on our problems" (p. 232). He called, that is, for developmentalists to incorporate an account of history that did *not* mainly provide instances of development that we can understand within our present theories, but rather an account that suggests that those very theories could only be "right" for a particular kind of society. The theories were designed to explain child outcomes by a certain kind of biology and a certain kind of environment.

When contemporary developmentalists have incorporated historical context, Zuckerman asserted, they have usually incorporated contexts that provide essentially confirmatory instances of what developmentalists already knew. But historical documents—contemporaneous perspectives read with contemporary eyes—sometimes surprise, and these surprises, when not disregarded or interpreted away, offer the possibility of disconfirmatory confrontation with the unruly world—most challenging of all, with some unruly worlds and not others.

Such disconfirmations, in Zuckerman's view, had not challenged the developmentalists at the workshop as he had hoped they would, because the documentation was unavoidably open to discrepant interpretation. "But historians have always endured and indeed accepted acrimonious disputation and irreconcilable antagonism. Our extended experience with such symptoms of the epistemological specter that now haunts all the social sciences doubtless conditions our receptivity to the proliferation of perspectives that attended the emergence of the new social history" a generation ago (Zuckerman, 1993, p. 239). Zuckerman called on developmentalists to think like historians if they wish to understand development in context. We now intend to apply something resembling Zuckerman's perspective to the question of what happens to our ideas when development is recognized as occurring in history, when those seeking to explore patterns of individual development find that they need context to explain them, and, looking insistently, they see that context is "history and development."

Historical context is a moving target—a target that is moving not just because it recedes and because its parameters change but because the changes in its parameters alter the way it affects development. It is also a moving target because social change provides the occasion for fresh theorizing, and a forgetting of older categories for understanding develop-

ment, not excluding those that once made powerful good sense. The historicist challenge to the species view says that when developing individuals take an active hand in their own development, they act upon a *meaningful* world that has come into being in history and will pass; and they act accordingly, playing a part—sometimes consciously—in a dialectic of imperfect maintenance and marginal transformation.

Both development and history take place in institutions, cultures, and populations. Social institutions are publicly recognized, more or less formally organized, more or less rationally structured social settings wherein mutual but not identical goals are sought through the coordination of routine behavior. The family, for instance, is an example of an institution, and so is the school, and so is the Little League and the youth gang. Any given instance of an institution of this type is embedded in a larger social structure, both of expectations and exchanges, on which its prosperity and longevity in part depend. The multiplication of social institutions, some have argued, is one of the master themes of recent history (Powell & DiMaggio, 1991; Tilly, 1984).

Social institutions often involve distinctive populations, whether ethnically diverse or homogeneous, and often generate particular cultures shared among participants, as they participate. In the state of California, the pre-1940 years were marked largely by racial homogeneity in the Berkeley-Oakland area. "Children of the Great Depression" (Elder, 1974/1999) grew up in this historical world, but their adult years were shaped by the institutions of a far more diverse cultural world. Work opportunities in the war industries of the Second World War drew thousands of Black and Hispanic families to the region, creating dramatically greater racial and cultural diversity. An organizational society emerged from the war and the consumer demands of the postwar years. Today the continued flow of immigrants from south of the border and from Asia has strikingly increased this diversity in Southern California and especially in the city of Los Angeles, the premier immigrant metropolis (Portes & Rumbaut, 2001). Historical times in the region are distinguished by interrelated institutional, cultural, and population change.

Through the social institutions in which children's developmental trajectories are formed, along with cultural and population influences, developing children make history: Both individually and collectively, they affect families, schools, courtship arrangements, labor markets, congregations, sects, and gangs, which wax and wane depending in part on how well they attach their developing participants to themselves and their goals. These contexts, we recognize, are not as familiar as objects of study by developmentalists as are parent–child dyads. But to understand children as social-historical actors, we must be prepared to apprehend the contexts in which "history" and "development" work on one another.

Prescriptions of this kind are demanding: We, too, are struggling to figure out just what *research prescriptions* emerge from our claims about development in history. Each of us has earlier placed human development within particular historical contexts, in somewhat different ways, and without fully embodying the ideas we now articulate here. Our books, *Children of the Great Depression* (Elder, 1974/1999) and *Into One's Own* (Modell, 1989), for all their differences, are basically accounts of "history happening to development." There are pleasing exceptions: Elder's look into the children's young adult years hints at what made the 1950s "the '50s," and Modell occasionally struggles with notions of how cultural practices might, over time, establish an "inertia" that might make them normative. But in our books, the dimensions and categories of development are by and large presumed to travel unchanged through the Great Depression, and even through the whole hectic half-century surrounding it. In both books, it is mainly the *parameters* that change, not the nature of relevant quantities, as Zuckerman might propose.

So, in Elder's (1974/1999) *Children of the Great Depression,* what amounts to "achievement motivation" sits there at the end of the Depression, as granitic as David McClelland (1953) might have wished, despite the reduced opportunities and perhaps perversely changed returns to acting upon them. Correspondingly, in Modell's (1989) *Into One's Own,* marriage timing changes dramatically, but not the interpretation of the phenomenon of marriage itself despite its changing integration with premarital sex and with postmarital childbearing. Nor when Modell provided a lengthy account of the origin of the institution of dating, and its subsequent spread through the young population, did he suggest that "intimacy," an important term of his analysis, might have changed in kind, rather than just in timing.

We both wrote as though history had "outcome variables," to put a sharp edge on what we now wish to question. In posing the question "What's new," we suggest that it is altogether reasonable to think that taking history seriously calls for a developmental account in which there are no "state" antecedents and outcomes, but only continuous and reciprocal processes.

THE SCOPE OF OUR ARGUMENT

We offer four propositions that make the case for why developmental analyses need to be responsible to the fact that children develop in history in order. Our propositions are not testable hypotheses, and neither are they cookbook methods. We are offering, rather, "a way of thinking" about the children whose development we seek to analyze. We do so in the hope that

if we think about children in this way, the perspective will influence accounts of their development, and the way that others' accounts of children's developments are read, incorporated, and synthesized.

We argue each proposition, first, from a theoretical perspective, and then from a more genial perspective that expands upon a single argument, John Clausen's isolation of "adolescent planful competence" (1993) as a determinant of individual differences in adolescent and adult development. Other candidates come readily to mind: We could have pointed just as well to Bowlby's notions of attachment or the Puritans' notion of infants' wills corrupted by original sin, or to Rousseau's unblemished, natural infant.

Clausen's (1993) argument is one that is useful in a number of ways: It seems to explain his lovingly analyzed data very well, and it satisfies our own outlooks on life, as historical actors in the latter half of the 20th century.[1] But we treat it critically in order to show you, with reference to our four "what's new" propositions, that Clausen's notion is insufficient, because he employed it in a way that does not adequately consider how children develop in history. He focused on biography without linking it to social history in his quantitative analysis.

Our propositions along with historical instantiation:

> *First, a child develops within the social arrangements, institutions, and value system of a given historical moment, which possess their own histories (i.e., a widely believed version or versions of those histories) according to which they are comprehended when they are put into concrete practice.*

As an example of the way children develop within historically understood social arrangements, institutions, and value systems, we turn to the circumstances of rural Iowa children in hard times. Despite changes, their worlds are defined less by low income than by the ties that bind people together, both within and between families and communities. The Iowa Youth and Families Project provided information on some 400 families from the north central region which were followed from 1989 into the late 1990s (Elder & Conger, 2000). Seventh graders and their near siblings, along with their parents, were followed up annually from 1989 to 1995, and then again in 1996, 1997, and 2000. Over the decades, rural population change has taken the form of a marked reduction of family-based farms and a sharp increase in farm size. In the process, Iowa families in agriculture lost ground economically and faced increasing uncertainty, and yet

[1]We have each dealt at some length with Clausen's big book, Elder as author of its Foreword, and Modell as the author of an essay-review (Modell, 1994).

their children were doing remarkably well according to key developmental markers—such as academic competence, social success, self-confidence, and the avoidance of problem behavior.

A key explanation emerged in the organization and culture of farming families and their developmental implications. These families were characterized by a substantial investment across the generations, by strong family connections, and by active family engagement in the community that included the leadership of parents. The children were exposed to more mastery experiences, compared to other youth—in work life, school success, and social leadership. These young people also experienced supportive adults in their lives, more so than youth who grew up in families without ties to the land and agriculture.

Typically, families with ties to the land are engaged in farming, full- or part-time, though some had lost their farms, and other parents had grown up on farms but never farmed. Each of these developmental ecologies was compared to one with families that were two or more generations removed from agriculture. The comparison leads inevitably to a speculative question about future history: What will Iowa children be like who will have grown up after the farm crisis had expelled so many of their parents from the land?

Within the German-American heritage of north central Iowa, an *intergenerational investment* was expressed first of all in a commitment to passing on the farm to a son in the younger generation, usually the oldest son, with a warning not to value material things. In this framework, farming represents a way of life and only secondarily a business. An intergenerational investment also took form in the unusual involvement of fathers in the upbringing of children, and in the commitment of mothers to the advancement of their children, often through higher education and nonfarm careers. They represented a bridge to the outside world.

Second, families with ties to the land were distinguished by *multiple connections* among members and their interdependent lives. Children in farming families were socialized to become responsible members of family groups. They learn to be part of the whole. In the everyday round of chores, young people learn that they are needed and, in being needed, they feel connected to the family and to the adult culture of rural life. When asked how his farm life had influenced him, a teenage boy claimed that it gave him a "sense of my family, a strong bond with my brothers and sisters and Mom and Dad." Doing things together in Iowa farm families had much to do with a sense of family and with ties that bind. Beyond the farm, participation in social activities with parents added to the sense of family and connectedness.

Third, *networks of social engagement* were expressed through parents' involvement in civic groups, church, and schools. They were more involved

than parents with no ties to the land. This involvement includes leadership roles as well, such as director of the church choir and president of the Parent Teacher Association. Associational life of this kind mattered in a number of respects for children's development. Compared to the disengaged parents, the involved were more aware of opportunities for their children and they were more likely to know how to make such options available to them. Involved parents tended to be perceived as efficacious by their children, and they were most likely to have socially active children, whether in athletics and social groups or in volunteer groups within the community. Children were inspired by the leadership example of their parents.

A fourth point stresses *mastery experiences* through productive labor, academic work, and social activities and leadership. Success breeds success through social reinforcement. Youth who were employed on paid jobs tended to stress a number of values and skills that they acquired on the job. These include a sense of responsibilities, social skills or mastery, the importance of self-control, and goal clarification. Work experience told some youth that they did not want a life with their current job. Other workers discovered how much they enjoyed their job, suggesting a possible career.

A concluding theme centers on *supportive adults and the power of small worlds.* These adults included parents, grandparents and other relatives, close neighbors, teachers, coaches and ministers, along with close friends who mirrored the values of their parents. Youth with family ties to the land are more likely than other young people to live in small, overlapping worlds, centered around family, church, school, and civic life. The overlap occurs through a common pool of participants in which "most everyone knows everyone else." Young people with ties to the land are also likely to have access to the support and guidance of more significant adults outside the immediate family.

In a sense, some of these adults can be thought of as "socially redundant" when no problems beset the family; they represented a safety net, as in the case of grandparents. Overall, the culture is adult sponsored or managed in the sense that adults were actively involved in the lives of children and youth. The support that family systems can generate was noted by a grandfather who observed that "being involved with them [the children] on everything makes a big difference. The kids are more proud of what they do, you Know ... If you've got the backing from Dad and Mom and from Grandpa and Grandma, it all makes a big difference."

Second, events and trends change life's arrangements. In doing so they challenge and modify the histories that guide us as we develop and shape others' development.

To continue our example of rural transformation, we discuss Roger Barker's account of the changing contexts ("behavior settings") of children in Oskaloosa, Kansas, a county seat with a population around 700, in the 1950s and 1960s. Eastern Kansas was not experiencing a farm crisis in that period, but rather undergoing a classic modernization of the rural sector. In the larger scheme of things, Oskaloosa was becoming less important to the county's economy and social arrangements—health care, for instance. And from the perspective of children's development, Oskaloosa was becoming slightly less important, too, as television entered their lives, and as more of them gained the means of reaching other, putatively livelier towns in the vicinity. Yet, the range and variety of local behavior settings to which children and adolescents were admitted was still growing in Oskaloosa between the early 1950s and early 1960s, because of the efflorescence of new places and activities in town, so children, in theory, could have more experiences. At the same time, Barker observed a decline in the proportion of child and adolescent public activities that involved prominent roles for the children on whom all participants depended. Oskaloosa, Barker, and Wright (1955) argued, had been in exactly this sense remarkably compelling to its children. But it was becoming less so, not just or even primarily because of competition from beyond, but because more of Oskaloosa's behavior settings asked less of the children than formerly.

According to Barker's minutely observed accounts of Oskaloosa activity in 1951, children 6 to 9 years old were absent entirely from 33% of the discrete behavior settings in which other Oskaloosans participated. By 1963, the proportion had risen sharply to 55%. Children 9 to 12, excluded from 31% of the behavior settings in 1951, were excluded from 49% in 1963. Adolescents (to age 17) were less frequently excluded than younger children (from 21% of Oskaloosa behavior settings in 1963), but this proportion rose to 35% by 1963. The new behavior settings, then, were age specialized in a way that the old settings had not been. This was not a choice, exactly. It was a concomitant of the functional specialization inherent in modernization. Barker's genius was to recognize it and (not without a quantum of romanticism) think hard about its not entirely benign implications for children's development.

Children's prominent activities were increasingly focused within the institution of formal education, which, itself, assumed greater prominence and received greater prestige within the community (Barker & Schoggen, 1973). In 1957-1959, Kansas education authorities had conducted a statewide study, calling into being committees of local notables to touch and sway the grassroots, enlisting support for the modernist conclusion that "students attending these [very small] high schools are suffering from educational malnutrition.... With students who are required to take practically every subject offered in the schools it means that even in these small classes the ranges of ability and interest are extreme." Specialization, again.

State Superintendent of Schools Adel P. Throckmorton rehearsed this message in his tireless and ultimately successful campaign. He argued that "in Kansas, the biggest cause of dropouts is a lack of the courses in which potential dropouts have an interest and aptitude" (Throckmorton, 1965, p. 4). In Oskaloosa itself, school board activists were not quite persuaded of the value of school consolidation, but half-expected that it would be forced upon them from above. Local autonomy was being eroded. Within schools, the number and variety of behavior settings (and, by far, formal classes most prominent among these) increased, as newer, larger schools came into being. School, to be sure, did not have yet the "lifeline" quality that it had gained among rural Iowa children a generation-plus later. But its immanence was already altering the subjective experience of childhood, and, hence, children's development.

How educational history has affected children's development, including children's understanding of their own development, is, in a sense, the obvious story here. But the side of the developmental story that most interested Barker, the story that most moved him, even as he was writing (with Paul Gump) an empirically grounded antimodernist monograph against large consolidated schools (Barker & Gump, 1964), was the side that took place not in schools, but in the the behavior settings that were *not* intentionally organized for children: the shops, offices, churches, and streets of Oskaloosa. For it was by their engagement in these contexts that Barker explained the quietly precocious versatility of American children, as compared with those growing up in England (Barker & Schoggen, 1973) and in other lands.

Barker feared that these qualities would be lost as children were shooed into schools and other age-specialized institutions. For in proximity to those who enacted the chief roles in public behavior settings to which young people were admitted, grown-up behavior could be observed, modeled, and (if adults, entrusted kids with active roles, as they typically did in Oskaloosa, where there were so many roles called forth by behavior settings, and so few persons to carry them out) informally apprenticed. Barker found in this arrangement a kind of organic developmental setting that, he feared, modernization would largely squash, even in towns like Oskaloosa that survived.

> *Third, developing individuals make history—it does not just happen to them. Human agency is experienced in terms of individuals' efforts to conduct their lives, much of this in the form of participation in social institutions. As such, human agency also takes the form of individual and collective modifications of received institutional arrangements.*

Generalized student resistance to grading standards in the 1960s-1970s and the resulting grade inflation in schools provides a useful example of

one way "developing individuals make history." This history might be said to "start" with American children, most likely in the baby boom, who later moved into higher education in record numbers. Perhaps unmotivated in course work to an extent unseen in the past, or perhaps just poorly instructed by many of the new influx of unseasoned teachers, students viewed academic demands as pointless. Teachers were no doubt moved also by the general questioning of American institutions so characteristic of the "'60s" (Deutsch, 1979; Grant, 1988). In any case, teachers began to grade more leniently, apparently beginning with "soft" subject matter in the mid- or late 1960s. Average grades rose in colleges and shortly afterward in high schools, and not improbably in grade school, without corresponding rises in standardized tests (Ahmann & Glock, 1981; Bejar & Blew, 1981; Bellot, 1981; Dey, Astin, & Korn, 1991; Prather, 1979). Some of this trend was produced by a movement of grade-conscious students, perhaps from tough-graded courses into ones in which they believed they could achieve higher grades.

The phenomenon promptly tapped anxieties about changing standards of youthful conduct and about the declining ability of adult-sponsored institutions to exercise authority over the young people in their charge (Geisinger, 1982). Soon these anxieties stiffened into polemical assertions that students had lost their ambition, and that faculty had lost their backbone, concerns visible yet in the American "culture wars."

How this all played out on the ground, at least in a group of Northern California high schools, is examined in a fine organizational study by Natriello and Dornbusch (1984)[2]. In these schools, some teachers chose (or were able) more closely to reflect their students' performance and/or effort in the grades they assigned than did others. Where grading was soft, students followed a leisurely course, and sought relatively high grades. Even the students who in this way took advantage of their teachers' soft grading did not dismiss the importance of grades—indeed, Natriello and Dornbusch offered evidence to the contrary. But students in leniently graded classes were, on average, less diligent, and that diligence often translated into lower standardized test scores. Moreover, school grades did not feed back into students' zest for committing serious effort to academic work at all as well as did standardized test scores, even though from an administrative point of view one of the major purposes of regular grading in courses was to let students know how well they were doing.

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[2]The issues in this fine book are so clearly those raised in the polemics over grade inflation, that it is almost surprising that the authors omitted mention of that notion. Perhaps they were only being conscientious in that the phenomenon "inflation" is inherently historical and they were aware that they had investigated no history. As it stands, they have left the historical speculation to us, as we leave the serious history writing to others.

So, over the 1970s, as grades inflated, members of the newly enlarged student body may have felt less often than otherwise the goad of anxiety at school, but also less frequently the balm of credible success. They may have shifted the curricula they studied, and, by doing so, shifted the curricula of the institutions in which they studied. In so doing, they contributed to lessening the institutional distance between themselves and those who taught them. At the same time, they may well have more often sought certifiable success in other endeavors.

> *Fourth, cultures make sense of the way people grow up, by providing a particular vocabulary rather than some other vocabulary. They modify not just what external observers see, but what actors experience and what they anticipate experiencing as they take stock of what they are doing.*

This is what Michael Zuckerman's arguments teach us forcefully. Though we could readily provide an instance from within our current, or recent, scientifically based child development theory, it is well to remind ourselves that the academic enterprise has not been the first intellectual movement to provide an institutionalized vocabulary for children's development that accounts for regularities over time in children's lives—quite the contrary. Consider scholarship on early American Puritanism (perhaps not incidentally Zuckerman's initial historical focus), which provides another consequential theory of child development, what Philip Greven (1977/1979) has called the "Evangelical" path.

Greven's (1977/1979) account is especially sharply drawn. "Tenderness and indulgence were the Achilles heel of discipline in evangelical families," and discipline was necessary for children who were inherently willful, for they were not inherently godly but rather the contrary by nature. "Parents, who govern well, never suffer their children to arrive at such a pass, that nothing short of torture will coerce them. They commence the business in season, and enforce obedience by gentler methods; they master the disease [of willfulness] at its first appearance, and so avoid the necessity of desperate remedies." (Anonymous, 1814, quoted in Greven, 1977/1979, p. 33). With stern discipline organized around the straightforwardly hierarchical relationship of parent to child, modeled on the to-be-learned relationship of God to man, children could be spared the most severe rigors of the untamed will. But pride and other incursions of self into God's rightful dominance typically remained: What today we would denote adolescent egocentrism was denoted then as a series of related sins, emerging from evil self. The uneventful recession of adolescence that we characteristically seek today contrasted markedly with the public, conscience-driven but socially enjoined and supported defeat of self that most characterized the Evangelical experience: conversion.

Here is the testimony of a 16-year-old youth in 1801:

> In the midst of a solemn struggle for my soul, an impression was made on my mind, as though a voice said to me, "Thy sins are all forgiven thee."... I rose to my feet [at a revival meeting], opened my eyes, and it really seemed as if I was in heaven.... My mother raised the shout, my Christian friends crowded around me and joined me in praising God; and though I have been since then, in many instances, unfaithful, yet I have never, for one moment, doubted that the Lord, then and there, forgive my sins and give me religion. (Peter Cartwright, 1856, quoted in Graff, 1995, p. 50)

Such transitions were dramatic, to be sure. But they were not random acts of anguished conscience: In fact, they were normative transitions, commonplace in puritanical populations. So, perhaps, is "storm and stress" in populations adhering to the classical G. Stanley Hall (1904) account of adolescence. But, we maintain, they were not the same thing. Forcefully put, to think of them as identical would be to deny to Puritan young people the belief that God was a prime basis for human action and becoming.

JOHN CLAUSEN AND ADOLESCENT PLANFUL COMPETENCE

Planning represents a central theme of human agency in making choices and the life course of young people, but it does not take place in a social vacuum. Life's plans and one's planfulness have much to do with the notion that human lives and development occur in historical time and place (Elder, 1998a). In the 1980s, John Clausen, a distinguished sociologist and former director of the University of California's Berkeley Institute of Human Development, sought to put to a test the formative influence of planfulness in adolescence by tracing its influence across the lives of people who were members of a pioneering program of longitudinal studies. Clausen was trained at the University of Chicago at the end of the 1930s and, as a research assistant, was exposed to the life record data of juvenile delinquents.

Clausen's 1993 study of *American Lives* is based on the Berkeley and Oakland longitudinal studies that were launched in the late 1920s and extended into the 1980s. The book is divided into quantitative accounts of measured relationships among variables, and case study accounts of exemplary sample members. Both are serious components of his book. The biographies are not just "illustrations," but rather seek to explore in interpretive *post facto* fashion how individuals developed as they acted. These actions formed their unfolding lives or life course.

In preparing to carry out the research project, Clausen came to the reluctant conclusion that he had to move to the personality level in order to

connect "personal development to social structure and experiential variables" (Clausen, 1991a, p. 6). Only at that rather stable level could he arrive at a "quantitative assessment of the person at any time, so as to encapsulate the effects of those antecedent variables that affect both current functioning and future tendencies most strongly" (p. 6).

He sought, however, not a deeply latent dimension, but rather one that fits well with his "preferred conception of the life course," one that entails "negotiation by a reflexive self of a set of potentially available roles that are interlinked and to which persons commit themselves in varying degrees at different periods of their lives" (Clausen, 1991b, p. 806). Clausen was determined to write an account in which his characters developed as they made choices and acted: They were not merely passive vessels, acted upon, nor organisms motivated by internal imperatives. *Planful competence* at adolescence was his nominee for this personality construct.

Planful Competence and Development at Midcentury

Planful competence "entails knowledge, abilities, and controls." Clausen (1993) proposed "that individuals who by late adolescence have a realistic view of their abilities, know in a general way what they want, and consider the consequences of their choices are more likely to make smooth transitions and adaptations and to remain satisfied with their decisions" (p. 19). We are not going to worry you about matters of measurement here: Others have remarked, and we quite agree, that the way Clausen chose to measure adolescent planful competence is a bit of a congeries. Rather, we want to ask you to take the concept seriously, and value Clausen's findings.

Clausen was determined to recognize what Elder (1974/1999) had earlier shown with the same data: that the developmental contingencies individuals encounter are situated in history, but where Elder operationalized a key element of historical context—the economic collapse—and systematically examined its effect in children's development, history is nowhere operationalized in Clausen's quantitative sections. It is confined, instead, to the individual biographies.[3]

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[3]To be fair, one should compare Clausen's and Elder's differently historicized accounts to Jack Block's excellent but entirely ahistorical *Lives Through Time* (1971), also based on the Institute of Human Development (IHD) data. Block, in describing the "developmental setting" of the sample's early years, mentioned the Great Depression and World War II, believing it was called for to alert readers to this fact, just as he did to the relative comfort of the subjects' physical surroundings. But he neither asked how this history might be relevant to his subjects' developmental patterns, nor did he describe any of the intermediate behaviors that families or individuals might have performed in response to economic downturn or war. Personality stability and change, in Block's book, happen outside of history. He needed not even to speculate whether it might be enlarged or limited by the dramatic historical events he named.

Within family and institutional settings, Clausen's individual cases are very much affected by depression and war, and by the prosperity that followed. Some of his subjects—whom he admired—were planful, and, having planned and finding circumstances that called for action, acted, whereas others drifted. Perhaps they acted according to other personality-level imperatives that Clausen did not comprehend as readily as planful competence. These imperatives might have been less "modern" than planfulness (whether "traditional" or "postmodern"), the kind of unruly truths that are often invisible where assumption is put in the place of historical thought.

Clausen's book is, among other things, a paean to individuals who were able to develop planful competence by adolescence. He did not make the error of assuming that planful competence in adolescence is the only way individuals can develop soundly into and through adulthood, or that all who are planfully competent are guaranteed the good life. But he also insisted that just as planful competence emerges in adolescence as the outcome of a number of broad (and historically set-in-place) aspects of children's circumstances, upbringing, and objective prospects, it so affects the way children come to address the world that planful, competent adolescents succeed often, even decades beyond adolescence, in such diverse realms as formal education, occupation, marriage, and lifecourse stability.

In *American Lives* (Clausen, 1993), we are shown not just that the planfully competent adolescents were able to find greater scope for their various talents and were well rewarded for their exercise, but how what they did fit the times. The portraits of the planful are unforgettable, although highly conventional except in detail, because of the depth of the author's feelings and psychological interpretation, and the richness of the record on which he draws. But his sketchy accounts of the adolescent drifters seem almost to *presume* their frequent adult failure, and do not explore how those failures fit their society and times. The mechanisms that punished the losers are ignored; that which separates the sheep from the goats is naturalized. History disappears again.

Why, in a more theoretical sense, might planful competence in adolescence matter so? Because an essential characteristic of modernity is the loosening hold of tradition, and the corresponding opportunity—and burden—of each individual to decide for him or herself. This was no less true for Clausen in 1993 than it had been in 1940. As he put it, by adolescence, "to a significant degree, a person's life course is his or her personal creation.... The [modern cultural] script demands choices, and the choices men and women make early in adult life have consequences for success or failure, stability or change over the rest of their lives" (Clausen, 1993, p. 4).

If Robert Bellah and his associates (Bellah, Madsen, Sullivan, Swidler,

& Tipton, 1985) are right in their sense that American individualism is moving toward a spiritual dead-end, the planfully competent will even then be better able than others to seek and find "both freedom and attachment, spontaneous expression and responsible control" (Clausen, 1993, p. 12), even if not community. And to the skeptics who might claim that the data are too old, Clausen offered informal evidence, based on the children of his sample, that the planfully competent among them, too, fared better than others of "a generation that has known drugs and easy sex, as well as greater uncertainty in the economic sphere" (Clausen, 1993, p. 3).

Planfulness in various guises appears in descriptions of the "modern" person where the qualities of those adapted to the trend of the 20th century have been compared with "traditional" people who seem to make out less well in the setting that flourished then (Doob, 1988; Inkeles, 1983). In general, findings in the American context have found planfulness associated with a variety of things that academics usually find virtuous and desirable. But we may ask whether modernity is the natural terminus of history, as so many social science accounts including Clausen's assume, the point at which human potential is uniquely fully realized, or, rather, a moment in history, and subject to change.

Planful Competence, Gender, and Historical Change

Clausen (1993) separated the lives of men and women in his analysis, and at one point even admitted that planful competence represents "a malecentered perspective" (p. 519). But when we bring Clausen's treatment of the two together, we see that adolescent planful competence makes its very considerable contribution to personality continuity for women just as it does for men, but not identically. The correlation coefficients linking planful competence to personality consistency are only slightly lower for women than for men.

During the historical period of which Clausen wrote, marriage and parenthood were more at odds with continued formal education for women than for men, and even though adolescent planfulness was commonly associated with the eventual attainment of more education among both men and women, this was less the case for women. We know, too, that as Clausen's sample became adults, women's route to advantaged employment was even more constrained relative to men's. In Clausen's data, women's planful competence in adolescence made only a small contribution to their eventual occupational attainment, but a very substantial one to men's.

Clausen's observation that planful competence promotes individual stability is a key part of his overall view of modern life, and of success in it. His account underscores the notion that life is lived in relationships, a prin-

ciple of life-course study, and it is lived in institutional arrangements. He understood that "success" and satisfaction rest importantly on the steadiness of personal and institutional relations. Planful individuals maintain more stable personal and institutional relations not only because they are less governed by impulse, but because their course of development is structured and regulated by relationships with the people in their lives (see Hartup & Laursen, 1999). Caspi, Elder, and Herbener (1990) found similar results in their study of couples and adults alone or in broken marriages.

Given this sensitivity, it is noteworthy that Clausen took little interest in his findings about gender differences in the role of planful competence. The gendered quality of American society in the period did not become a real part of Clausen's social history. This historical limitation feeds back into his developmental account.

We must ask whether women's routes to education and occupational attainment were mainly the product of chance or idiosyncrasy (Bandura, 1982) or, instead, were more systematically related to prior attributes. Clausen's tabulations[4] indicate that parents' socioeconomic status mattered more for women than it did for men, whereas personality in adolescence mattered more for men than for women. A likely reason for this pattern illustrates the interplay of developmental and historical analysis.

Women's agency in the educational and occupational spheres during the period under study was disvalued to the point where the major determinant of how far they would go in life was the material resources from their parents. But young men's agency mattered, and so it was aspects of their capacity, character, and drive that counted most—their planful competence. A large part of their own ticket involved their personalities. Their planfulness was admired and rewarded. Women could be quite competent, quite stable, but not gain that same reward.

[4]These are simple correlations. Clausen's presentation of data does not allow comparison of more complex models. Sources: Clausen (1993, pp. 263,264,446,536,538,540).

	Males	*Females*
Correlation of adolescent planful competence and		
Consistency of personality profile between high school and early adult years	0.53	0.56
Consistency of personality profile between high school and later adult years	0.36	0.25
Consistency of personality profile between high school and older years	0.55	0.41
Educational attainment	0.68	0.40
Occupational attainment	0.71	0.20
Correlation of parental socioeconomic status (SES) and		
Educational attainment to parental SES	0.44	0.62
Occupational attainment and parental SES	0.20	0.43

Today one would not expect the importance of women's planful competence for their subsequent career formation to be that much less than for men. A gendered path to formal education and occupations has faded some (Buchmann, 1989), even as the proportion of women attending college has exceeded that of men's and women's participation in the labor force has approached men's. Some of the energy for women's issues in the last half-century no doubt stemmed from the lack of concrete rewards for the (culturally prescribed) planfulness that so many women manifested in their persistence and inner locus of control.

We are not saying, however, that it is "only natural" that the discrepancy between men's and women's rewards for planfulness has lessened. Such an explanation (as well as its variant form that presumes universalism as a concomitant of modernity) would be ahistorical, just as was Clausen's neglect of gender inequality in the 1930s-1960s. In fact, we will present suggestive (but hardly conclusive) evidence that part of the convergence of girls' upon boys' adolescent planfulness and its life-course effect can be explained as much by a *decline* in planfulness for males as by an increase for females. This convergence, in turn, not improbably derives from the way young Americans have responded to the increasing centrality of formal education in job placement, and the sharp movement of women into education and gainful employment.

Elder's work (Shanahan, Elder, & Miech, 1997; see also Shanahan & Elder, in press) with the Terman longitudinal study offers an instructive analogy, dealing with California youth, like Clausen's subjects (although only males), but born slightly earlier. In these people's lives, adolescent planful competence also predicted favorable adult socioeconomic status—but to a significant degree only among the younger (1911-1917) birth cohort, not among the older (1904-1910) cohort. Was the reason the inexorable expansion of a modern perspective, so that planfulness came to be more widely rewarded? Hardly. The answer, instead, is *historical.*

Educational achievement, on average, was actually greater among the older cohort, and was more uniform, because the Great Depression intersected their lives in such fashion that many among them extended their education as a way of avoiding the harsh Depression labor market. But the younger cohort faced happier choices, and *their* planfulness—indirectly, through differentially extended education—*was* well rewarded. The older cohort possessed just as much planfulness as a quality of personality as did the younger cohort: But the institutional configuration that gave planfulness scope for the younger cohort (and, presumably, for Clausen's males) was lacking. Things do change.

We have examined attitudinal items in the annual, national high school senior survey, "Monitoring the Future," of which six items (from two bat-

teries) allow the construction of a planfulness index.[5] These items have been used consistently over the 22-year period from 1977 to 1998. We have taken the first three of these years and combined them into a "late 1970s" period; 1986-1988 to stand for the "late 1980s"; and 1996-1998 to stand for the "late 1990s." Among the six items, the average interitem correlation is .164 and the alpha reliability coefficient .540, with all nine years combined. The alpha coefficients rise slightly over the period. These figures are on the light side, but the items have a pleasing face validity, and display interesting individual patterns over time.

Over the period, high school seniors became a little *less* likely to subscribe to the idea of planfulness. More seniors began to maintain that good luck, not hard work, explains success, and that those "who accept their condition in life" are happier than those who try to change things. The trend varied by gender. It was boys, not girls, who increasingly told the survey researchers that they believed that planning leads to unhappiness because plans usually fail, and that planning ahead does not make things better. Girls became more certain that they could make their plans work out, but boys did not. On the six-item index, girls moved ahead of boys in their outlook on planful behavior.[6]

The worlds of males and females, once so different, now are less so, and in ways relevant to the kinds of plans one might execute. As these aspects of context have changed, so has the way young women and men contemplate action, and the meaning of "planfulness" to each. The changed relations of planfulness to gender fits with our discussion of the time-

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[5]The data have been supplied to the authors by the Interuniversity Consortium for Political and Social Research, University of Michigan. The Consortium bears no responsibility for the use of the collection, nor for inferences or interpretations based thereon. Three of the High School and Beyond "planfulness" indexes are from one battery and are reversed:

1. Good luck is more important than hard work for success. (var5304)
2. Planning only makes a person unhappy since plans hardly ever work out anyway. (var5308)
3. People who accept their condition in life are happier than those who try to change things. (var5309)

Three are from a subsequent battery, and are not reversed:

4. When I make plans, I am almost certain that I can make them work. (var5312)
5. I believe a person is master of his/her own fate. (var5318)
6. Planning ahead makes things turn out better. (var5322)

[6]Girls' planfulness scores declined slightly in the 1980s, then rose back to their starting point in 1990s.

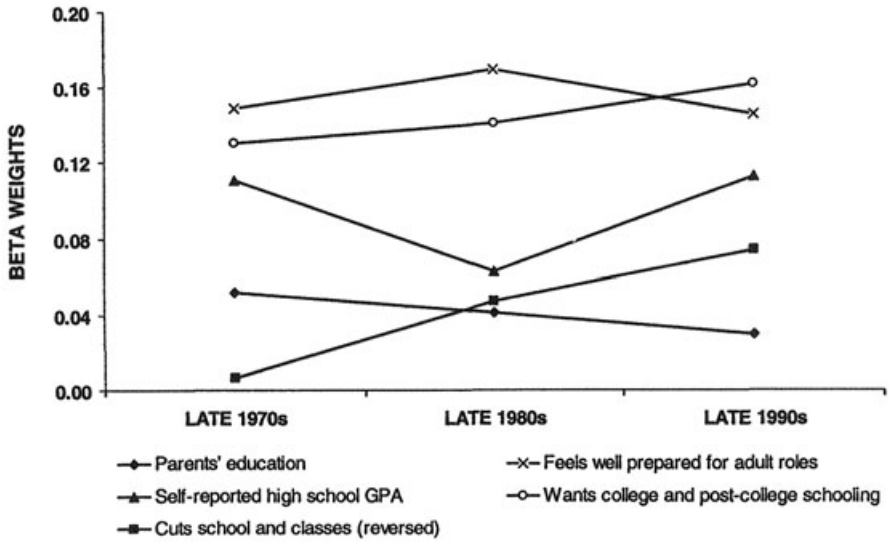

FIG. 6.1. Planfulness in relation to selected characteristics: late 1970s to late 1990s; regression coefficients in standard form.

bound qualities inherent in Clausen's notion, for "adolescent planful competence" then and there "worked" so much better for male than for female sample members. But one must also ask whether it works all that well anymore for members of either gender. Is "planfulness" as a "personality characteristic" bound up with young people's successful development today, in the same sense as it was when Clausen's young people grew up?

Apart from the striking gender trend, we also note trends in the determinants of high school seniors' orientations to planfulness. As planfulness became slightly less widespread among these adolescents, its correlates shifted sharply (see Fig. 6.1)[7]. Note first here the striking decline in the relevance of parents' education to planfulness: This usually important structural feature (especially for women's lives, according to Clausen's findings) decreasingly affected the planfulness of adolescents.

One important determinant was unchanging in its considerable impact: the index of seniors' responses to questions that asked how well prepared they had been in their background and experiences to carry out the

[7]All relations shown in Fig. 6.1 are significant at the 0.001 level, except for the insignificant relation in the 1970s of cutting school and classes to planfulness.

roles of spouse, parent, and worker. But other factors did change. Grade point average (GPA) displayed an interesting pattern: The 1980s were not just a midway point between the 1970s and the 1990s, but had a quality of their own, fitting with a considerably lower self-reported GPA, though momentary. Skipping school and class, on the other hand, showed a notably increasing negative relationship to planfulness. Choosing to be at school and in class was increasingly connected with how eagerly students embraced planning. Most important, anticipating 4 years of college, and anticipating graduate study, have become more prevalent over time and more closely linked to current planfulness.

Planfulness has thus become increasingly the realm of young people who sensed that their futures are tied up with formal education. Over the period, adolescents have been subject to an ever more intense drumbeat of education as the pathway to adult success. Alternative ways of being a successful adult have increasingly paled before this standard route. If this future has challenged young folks' (and particularly boys') sense of their ability to plan, it has done so by increasingly limiting those who trust in plans to those who accept the "school route," both behaviorally in the present, and in their near-term aspirations. And it has produced a more dedicated clutch of skeptics, differentiated in word and deed from their planful, education-oriented fellows.

Does this mean that adolescent planful competence is out of date, with more up-to-date personality characteristics—joyful spontaneity, perhaps—providing the best adult developmental outcomes? We answer speculatively, yet guardedly "Yes and no." We have strong evidence in favor of this interpretation from trends in planfulness for boys and girls who were in different high school programs: Here, for the sake of simplicity and because it captures most of the relevant information, we dichotomize these programs into college and noncollege tracks. Table 6.1 shows the trends.

We find a more rapid movement into the college track for girls than for boys. Over the period, boys in the college-preparatory track lost a degree of commitment to planfulness, but this decrease was dwarfed by the startling loss of a planful orientation to the future among boys in the other high school tracks. Among girls, there was also a modest decline in planfulness among those in the college track, but *no decline* among the admittedly not very planful (but more planful than like-situated boys) girls outside the college track. That is, an academic education became increasingly the agency for boys' planfulness over the two decades, whereas girls' own enlarged view of an occupational future may have given them a heady feeling that sometimes supported planfulness.

Not implausibly then, the routes of young men to the good life have increasingly become dominated by formal channels, particularly educational.

TABLE 6.1
Mean Planfulness Scores of U.S. High School Seniors Separately by Program and Gender

	Boys			*Girls*		
	1970s	*1980s*	*1990s*	*1970s*	*1980s*	*1990s*
College track	0.66	0.61	0.50	0.64	0.40	0.42
Other tracks	-0.28	-0.50	-0.78	-1.17	-0.4	-0.56
Difference	0.94	1.11	1.28	1.80	0.87	0.98
% in college track	45.5	50.7	52.1	53.4	58.1	60.6

These channels alone support the orientation that, according to Clausen, is expressed in "planful competence." The world beyond school, on the other hand, may no longer respond quite as it once had to a character trait like planfulness. If this is so, the rock-solid but clearly admirable notion of "planfulness" as embodied in Clausen's biographical sketches may emerge in the 1990s and 2000s looking more like "compliant diligence," and, as such, perhaps becoming less attractive to youth and adult institutional actors alike.

Whether this pattern holds or not, we are historians enough to insist on the historical embeddedness of the concept of planfulness. Just as the particular configuration of social organization and cultural notions that we call "modernity" happened in history, so also will it pass. For this reason, among others, research on the developmental paths of individuals must attend to the historical trajectories of the contexts within which they were formed.

Planful Competence in History Makes History

We return for the moment to our first prescription for understanding of development within history—that one's development is profoundly affected by the "deep structure" of contemporaneous institutions and beliefs. Clausen's account of planful competence in the second and third quarters of the 20th century makes this case. Planfulness could not be better suited to understanding development in the United States in 1930-1970, in part because it was more likely to be a part of the moral, evaluative vocabulary of socialization.

Planfulness, of course, does not simply emerge full-blown at adolescence, but develops from children's experience of environments that are challenging enough to sustain interest, but not so overwhelming that they

induce a sense of futility and lack of control on the part of the child. These children tend to come from families who have raised them with an eye to encouraging autonomy rather than obedience, a sense of the worth of the child's own accomplishments rather than the fulfilling of forms provided by others (Bronfenbrenner, 1958; Miller & Swanson, 1958; and see Bandura, 1995.) The connection of this kind of upbringing to the middle class has often been remarked, most incisively in the work of Clausen's collaborator of many years before, Melvin Kohn (1969/1977). Over the past two decades and more, Kohn has worked to examine the ways that market labor of different kinds organizes the orientations of men and women toward large realms of life, including childrearing (Kohn & Schooler, 1983). We can take advantage of Kohn's work to give some further thought to our third proposition, that children's development plays back into history.

Put briefly, Kohn found in studies of American populations in the third quarter of the 20th century that the cognitive complexity and self-orientation of more-favored occupations typically encouraged an active, exploratory, self-reliant orientation in workers, not just toward the work itself but to realms beyond. Such work also encouraged parents to promote in their children a similar capacity for self-direction. This finding provides a socialpsychological and developmental understanding both of the lack of perfect intergenerational social mobility, and of why people from different social strata often seem to be so different from one another.

Kohn and his colleagues have taken admirable care to replicate these findings in other societies, mainly based on data from the 1970s. A recent report (Kohn, Naoi, Schoenbach, Schooler, & Slomczynski, 1990), compares three strikingly different societies—then-socialist Poland, capitalist United States, and capitalist but hardly free-market Japan. The authors concluded that although the effect of social class differs in the three societies, the organization of work produces parallel psychological differentials within the populations of the three societies. "In all three countries occupational self-direction ... [played] a crucial role in explaining the psychological effect of social class.... The effects of social class on parental valuation of self-direction, on self-directedness of orientation, and on ideational flexibility prove to be in very substantial degree a function of the varying opportunities for occupational self-direction enjoyed by men at various locations in the class structure" (Kohn et al., 1990, p. 1004).

Significantly for our discussion here, self-direction is a construct that incorporates trustfulness, the absence of authoritarian conservatism, self-deprecation, and fatalism, and the presence of "personally responsible standards of morality" (Kohn et al., 1990, p. 982). This is not exactly planfulness, but it is surely close. And when we look closely at models for the United States, Japan, and Poland, we find a similar *direction* but a marked difference in *slope.* Individual self-directedness is more closely tied to

social class and to job structure in the United States than in Japan and Poland.[8]

Why? The authors were mainly concerned about the generality of their finding, and thus did not explore national differences. So we are free to venture an interpretation.[9] Planfulness seems to have had a special significance for the United States at mid-20th century by comparison with its importance in a country like Japan (with a value system that in many ways differs from that of the United States), or in a still-socialist country like Poland.

Arguably, the state in socialist Poland, just as the broad obligations of employer to employee in Japan, provided fallbacks that were largely absent in the United States, where success and failure were uniquely clearcut outcomes. The rewards of planfulness in the United States may have been more prominent than in Japan or Poland. Planful Americans of Clausen's generation may well have had a specially profound reason to celebrate and prescribe their children's development. If so, we may well understand planfulness not merely as a trait of personality, an individual quality with large developmental consequences, but as a value that has long pervaded American life (Hofstadter, 1955); a value that affects the ethical orientations that underlie political orientations, and the way that the polity works to structure American social institutions. It is not farfetched that a widespread sense of one's own planful competence could make possible the popular decisions that permitted the "Reagan Revolution" of the 1980s and the dismantling of the federal welfare structure in the 1990s—the sense that one's own personal efforts will suffice to avoid personal harm. In the views of U.S. youth, unfettered capitalism was the *right* way to organize the economy, because it called upon everyone to work hard, conscientiously, *planfully.*[10]

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[8]There also seems to be a strong correlation between formal educational attainment in the United States and personal self-direction that is lacking in the other two societies.

[9]Our speculation is just that. The differences in slope, perfectly plausibly, could stem from samples that in the case of both Japan and Poland were not fully representative of the whole societies.

[10]The World Values Survey of 1981 and 1990 provides some evidence (see Inglehart, 1997). Asked how well they agreed with assertions like "In the long run, hard work generally brings a better life" and "hard work doesn't bring success—it's more a matter of luck and connections" and whether "there [are] people in this country who live in need ... because of laziness and lack of willpower" rather than some structural or merely chance reason, U.S. youth expressed the faith more than did their counterparts in Canada, Britain, Germany, Italy, France, the Netherlands, or Sweden, that hard work was what it took to succeed and a lack of willpower is what determined failure. In a series of questions about values that "you personally think are important in a job," by comparison with Canadians and British, with Germans and Italians and French, with Netherlandish and Swedish youth, American young people looked ahead when they evaluated a job. Promotion and security were the most distinctive American job concerns.

SUMMARY AND CONCLUSION

The 20th century brought dramatic social change to the lives and developmental studies of children, including research designs that address their influence. But if children develop in history, as we commonly assert, this observation has too seldom altered the research we do and our theoretical interpretations. We think of history in terms of both cultural and social change or continuity, as in belief systems, economic cycles, and a rising divorce rate. At the very least, sensitivity to history places child development in context, but we ask more of historically informed research. We ask it to link contextual variation and its developmental relevance to research on children's development. This variation is a constituent of development.

We have offered four propositions about this relevance and then have explored them in depth through commentary on John Clausen's innovative, longitudinal study of planful competence among Californians who were born in the 1920s. The first proposition asserts that children develop within the social arrangements, institutions, and value system of a given historical moment. This sociocultural system has its own history. To illustrate, we referred to rural children on farms who grew up amidst an economic crisis, whereas their parents experienced economically rewarding times during their childhood. In earlier periods, the modernization of farming occurred gradually, without crisis or rapid economic growth. In either case, the social context for rural children of the same age tended to vary sharply according to whether their families were engaged in farming or not.

Our second proposition holds that historical change through new events and trends alters the life arrangements, social institutions, and value context of children. This impact shapes their development in particular ways. To continue with the rural example, postwar modernization of farming in the Midwest brought an expansion of acreage for some families, and greater prosperity. Later on, mounting indebtedness from declining land values pushed some farm families into bankruptcy and the loss of their farm. Residential change to nearby communities followed, along with a period of extreme economic and emotional distress, and, for children, transitions to new schools and classmates. Rural changes of this kind were accompanied by the growth of consumption aspirations and the outmigration of young people. Such changes in rural society produced change in the content and course of development.

Historical change affects the developmental course of children, as stated in the previous proposition, but individuals also make history through their choices and actions, including those that in part direct their own development. Our third proposition, then, asserts that developing

individuals make history, in large part through collective adaptations. Examples of such adaptations appear in birth cohorts of Midwestern children. The 1980s farm crisis adversely affected countless families and this prompted emergency actions by state governments, such as the establishment of "hot lines" for counseling. The emotional problems of children in distressed families also led schools as well to provide more counseling and emotional support. One of the most vivid cases of "individuals making history and their own life course" involves the outmigration of rural families and young people during the farm crisis of the 1980s. Iowa counties with an agricultural economy lost up to 20% of their population by the 1990s.

Our fourth proposition acknowledges that the culture of developmental language and knowledge has itself varied across historical time. When and where Puritan Protestantism dominated, for example, the character of children was shaped by harsh discipline, based on a hierarchical relationship between the believer and God. Nurture entailed an upbringing that was designed to break the willfulness of children.

Each of these propositions is relevant to John Clausen's (1993) *American Lives,* a study of individual differences in planful competence during late adolescence and their impact on the life course of development for men and women. A distinguished social psychologist, Clausen was influenced by the study of life histories and its emphasis on individuals as producers of their own life course and development. In *American Lives,* he applied this perspective to the lives of Californians who were born in the 1920s, and he did so by investigating the link between adolescent planfulness and a more stable, successful life course.

He found much evidence of this link among men and even women, but what about the imprint of changing historical times? The study members grew up in the Great Depression, experienced different roles in World War II, and generally benefited from the prosperity of the postwar years. This historical experience is expressed in the six biographies of individual sample members that Clausen offered, but it is ignored in all quantitative analyses. Here, a sense of historical timelessness prevails.

But consider the many ways in which history left its imprint on this study and its findings. First of all, neither the Great Depression nor World War II appeared to make a difference in Clausen's quantitative findings, but he had not framed his study to capture these influences. For example, we know that history left its mark on the consequences of planfulness among Americans in an older generation, born around 1911—members of Lewis Terman's study (Shanahan et al., 1997). They belonged to two birth cohorts, 1900 to 1910 and 1911 to 1920. The older men experienced more disruption and less opportunity in their life course, and their planfulness in adolescence did not predict their level of education. A significant num-

ber of these men remained in school during the 1930s, because good jobs were not available. By contrast, planfulness in the younger cohort played an important role in determining high levels of education, when compared to other men. The timing of their lives enabled them to carry out their plans—opportunities were available for them at the time. Structured opportunities in history clearly matter in determining the lifetime relevance of planful competence in adolescence.

A second illustration of the relevance of history involves the concept of planful competence and its relevance to males in this time period, an era of the organizational society and organization men. Personal agency was highly prized in the lives of men, but less so among women. In Clausen's study, planful competence was more predictive of life-course achievement and stability among men than it was among women. But the gender difference is not placed in the context of gender role change, which reflects a great revolution in the 20th century; nor does it inform *American Lives.*

Today the evidence suggests that young men and women are becoming more similar in their planfulness between the 1950s and century's end, but how has this convergence occurred? From a national sample of high school seniors across the past 30 years, we find that young men have become less planful, whereas women show a trend toward more planfulness. And in both cases, the educational path has become increasingly prominent as a correlate of planfulness.

Historical context can inform developmental studies as pathways of the life course, embedded in social institutions, and as a particular historical time and place with its social institutions, cultures, and populations. Trends and major events typically alter the sociocultural context of a time and place, and with it, life's pathways and their developmental implications. Through collective adaptations, as in a population or cohort, these developmental influences can change institutions and cultures. Children develop in history, and their collective adaptations are a source of change in communities and the larger society. Mass migration is one illustration of this "making of society" and the managerial experience of World War II veterans represents another.

Should all this be granted, developmentalists will perhaps seek the company and even the collaboration of social historians. On the whole, no doubt, this will be a step in the right direction (certainly inasmuch as it will somewhat reduce historians' startling lack of insight into and curiosity about developmental issues, despite their obvious pertinence to the way society functions and perpetuates its patterns). But we also fear that particular concession, for we found in organizing *Children in Time and Place* that history can all too readily become an account of the past, a prologue to "the action," rather than an account of what happens when developmental processes occur. Developmentalists, perhaps, will have to do

the history themselves in order to become truly responsible to the historical embeddedness of their developmental accounts. Or they will, at least, have to collaborate firsthand in the historical effort rather than assigning it to the historian on grounds of efficient specialization.

At minimum, we think, developmentalists need to be challenged to supply a set of hypothesized "upward lines" for the histories of development they (co-) create—that relate what children do as they come of age to the institutional settings within which they grow up, and that give direction to that growing up. Historians would rarely think of these links, accustomed as they are to rendering children as passive objects. And the intellectual discipline of thinking about the "upward links" for developmentalists will have two important consequences for their own thinking. They will need to synthesize accounts of normative development and individual difference, for both make a difference in the renegotiation of context. And they will need to understand context less as a stolid "environment" and more for what it is, a composite, embedded, and (viewed up close) dubiously stable.

The central message of this essay is an old one that has been stated in part by developmentalists on numerous occasions. For example, over 50 years ago, Urie Bronfenbrenner (1958) recognized the consequences of a changing society for children's development when he authored an essay entitled "Socialization and Social Class Through Time and Space." He discovered that studies of class and childrearing could not be merely synthesized without regard for their historical reference. Their findings simply did not make any sense. But a coherent story emerged when he arranged the studies in historical time.

From the 1930s to the 1950s, middle- and working-class parents had changed their childrearing practices. Middle-class parents had shifted toward more permissiveness, whereas working-class parents had become more restrictive. Bronfenbrenner (1958) brought an important perspective to developmental studies with his essay and we have extended it with an eye toward bringing history into our research designs, studies, and theories. The life journey of children tells a story of who they are. As developmentalists, we are challenged to investigate and understand what it can tell us.

ACKNOWLEDGMENTS

We acknowledge support by the National Institute of Mental Health (Grants MH 00567, MH 51361, and MH 57549), research support from the MacArthur Foundation Research Network on Successful Adolescent Development Among Youth in High-Risk Settings, and a Spencer Foundation Senior Scholar Award to Elder.

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Notes Toward a Philosophy of Science for Developmental Science

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In 1894–1895—quietly, without much soul searching or systematic writing—a scientific study of human psychological development got going. A series of questionnaires were sent out from Clark University asking about children's feelings, thoughts, and activities—*Anger; Dolls; Crying and Laughing; Toys and Playthings; Folklore Among Children; Early Forms of Vocal Expression; The Early Sense of Self; Fears in Childhood and Youth; Some Common Traits and Habits; Some Common Automatisms, Nerve Signs, etc.; Feeling for Objects of Inanimate Nature; Feeling for Objects of Animate Nature; Children's Appetites and Foods; Affection and Its Opposite States in Children; Moral and Religious Experiences.* Questionnaire series like these were to be sent out for 10 years, in the heyday of the program. A first-order developmental science came into existence, organized so as to create a cooperative naturalistic study of children (Siegel & White, 1982; White, 1990) There were no elaborate prospectuses or methodological treatises associated with this program, certainly no systematic formulations resembling what we have today come to think of as a philosophy of science.

This first-order program of developmental science came to an end by the time of World War I. There were two later establishments of developmental science in the United States: the child development movement housed in a number of child development institutes and centers, beginning in the 1920s and largely ended by 1950 (White, 1995) and the the rise of developmental psychology in the 1960s (White, 1991) that continues today.

Each of the three *establishments* of developmental science consisted of a cooperative research group together with outside people, allies, who provided resources (positions, funds, journals, space, societies, access to subjects, political support) that sustained the group's program. The allies most likely lent support to the program because they saw it as valuable for their interests and/or for society at large. Why three establishments? The long, slow, waxing and waning pattern of growth of work on developmental science in the 20th century may have reflected, simply, the relatively slow and effortful growth of all the later maturing, nonexperimental psychologies in the early decades of this century. The "growth waves" of developmental science must certainly have been influenced by a periodic heightening of public interests in programs for children.

THREE ESTABLISHMENTS

There was no articulated philosophy of science associated with any of the three establishments of developmental science. Nevertheless, each establishment had a reasonably well formed scientific program. Each had an institutional place in American society, and there were hopes of social utility that mobilized support for the work.

G. Stanley Hall's *child study questionnaires* were designed to offer guidance for child study, and to serve as a basis for a cooperative naturalism of child development. The questionnaires look like methodologically imperfect forms of 20th-century research methods—psychological tests, survey research, or Piaget's clinical method of inquiry—but they were conceived in a 19th-century spirit. In part, they were supposed to give teachers, parents, and the growing number of adults working with children tools that might help them look at child development in a richer and more complex way—to help adults with child study. In part, the questionnaires were intended to be instruments for a cooperative naturalism. Questionnaire studies like Hall's were pursued in a small number of psychology departments and schools of education. Hall hoped such studies would provide a scientific foundation for education, and he founded the *Pedagogical Seminary* (the "Pedagogical Seminar") as an outlet for the work.

The *child development movement* of the 1920s and 1930s centered its research work on psychometric and normative studies of children that, it was hoped, would provide quantitative indications of a range of healthy, normal child development. The movement was housed in a series of interdisciplinary child development institutes and centers founded between 1917 and 1927, and was largely supported by the Laura Spelman Rockefeller Fund in a program conceived and directed by Lawrence K. Frank. Frank's training had been as a cost accountant and it seems likely that

ideals of "scientific management" and mental health guided his approach. The hope was that scientific findings of studies of the normal and natural processes of child development, taught in programs of parent education, would produce better parenting and that such "preventive parenting" would make a positive contribution to mental hygiene, citizenship, and social life in the United States.

Developmental psychology, beginning in the 1960s, included several theoretically or practically oriented streams of research on children, but what brought this establishment together and gave it force and coherence at the outset was a surge of interest in Jean Piaget's program of research on children's cognitive development. What stimulated the growth of the third establishment were some dramatic educational and social initiatives of American society at large. During the 1950s, stimulated by *Sputnik,* there was a movement toward the building of new scientific curricula for schools, and an interest in what a growing child understands about space, time, number, geometry, causality, probability, and scientific reasoning. The work of Jean Piaget in Geneva was addressed to these questions, and it was probably the impetus of the science curricula that first began to bring Piaget's work across the water. During the 1960s, stimulated by the War on Poverty, there was a new interest in the education of disadvantaged children. Could appropriately designed preschools, schools, and television programs foster children's cognitive development and "close the gap" between more- versus less-advantaged children? Again, the methods and theory of Jean Piaget seemed to provide a way to approach such questions. For the most part, the developmental science of the third establishment came to be housed in Psychology departments, and in this period developmental psychology became a major subdiscipline of American psychology.

Although the three establishments of developmental science differed, there was a felt unity linking them. They have seemed to developmental scientists to form one lineage in the United States. The people and activities of the consecutive establishments overlapped. There have been accumulations across the three periods—accumulations of methods, of theories, of clarification of issues, and of data—so that most contemporary developmental scientists today understand that their work rests upon a century-old knowledge-building process.

The three establishments devoted to the building of developmental science represent a core tradition of research on the patterns and organization of human development situated in the midst of an emergent pluralism of work on children, sometimes single studies, sometimes organized programs. G. Stanley Hall began his university-based questionnaire program in the midst of a large and diverse array of child study writings coming from diverse scientific, professional, literary, and popular sources.

Bibliographies compiled by Clark University's librarian (Wilson, 1975) give some sense (I think an underestimate) of the size and diversity of this array of work. Later, in the 1920s, the research of the child development institutes and centers coexisted with other research: (a) research in which experimental psychologists compared the behaviors of animals and children behavior within the traditional framework of comparative psychology, (b) accounts of childhood maladjustment and psychopathology in the *American Journal of Orthopsychiatry;* published by psychiatrists, psychologists, and social workers of a rapidly burgeoning child guidance movement, (c) a body of work on testing and human learning associated with the *Journal of Educational Psychology,* and (d) in the 1920s, with the work of Hermine Hug-Hellmuth, Anna Freud, and Melanie Klein, the beginning of what would be a substantial body of work on the psychoanalytic study of the child. Contemporary research in developmental science—as represented, say, in the programs of the biennial meetings of the Society for Research in Child Development, or in the journal *Child Development: Abstracts and Bibliography*—reflects a number of scientific and professional programs (White, 1983).

The third establishment of developmental science, the present period, might almost be regarded as two periods, one right after the other. The Piagetian genetic epistemology program dominated the 1960-1980 period. From the 1980s forward, various neo-, semi-, or post-Piagetian inquiries remain active, but inquiries in developmental science now seem to flow in two streams: a biologically oriented inquiry into infant development, and a cultural-historical inquiry into the development and education of older children. The infant work has been stimulated by dramatic new increases in the power of microbiological research techniques. The cultural-historical has been immensely stimulated by the work of L. S. Vygotsky and the work of a generation or two of Soviet developmental scientists who have followed and developed his ideas, but I believe it also represents a fulfillment of over a century of American work on human social development—powerful theories in the late 19th century followed by the slow building of a research base in the 20th (White, 2000).

PSYCHOLOGY'S "CRISIS"

The issue of articulating an appropriate philosophy of science for psychology arose first in experimental psychology in the 1920s, because of changes in the work and scientific programs of experimental psychology amounting to what some called a crisis. Developmental science stood to the side. This was at a time when the second establishment of developmental science was pursued away from psychology departments, in interdisciplinary child development institutes and centers, and when the psy-

chological work of the centers was largely descriptive, devoted to measurement, and normative.

Experimental psychology had been the best-known activity of the New Psychology in America until the first decade of the 20th century and, generally speaking, experimental psychologists dominated the laboratories and departments of the nascent discipline. Forty-one brass-instruments laboratories were opened in American colleges and universities between 1883 and 1900 (Hilgard, 1987). The laboratories were largely modeled on Wundt's Leipzig laboratory and continued its program of the study of the physical dimensions of consciousness[1] through studies of psychophysics, sensation and perception, and reaction time studies. But in the decade after 1900, rather rapidly, interesting new forms of psychological science were set forth—by the animal psychologies of Edward L. Thorndike and Ivan Pavlov, by the mental testing of Binet and Simon, by the developmental science of G. Stanley Hall and James Mark Baldwin, by the mental testing of Binet and Simon, by Freudian psychoanalysis, and by the social psychology of William McDougall.

The work of the first-generation brass-instruments laboratories had been critical to Psychology's standing as a science in the late 19th century. The argument that there could be a science of psychology had been disputed before the discipline was organized and so early writers of the New Psychology had bravely faced some distinguished opposition:

- William James (1890/1981) devoted the first six chapters of his monumental *Principles of Psychology* to what he called "the physiological preliminaries." Then, turning in chapter 7 toward the methods of psychology, he confronted Auguste Comte. Comte's enormously influential Positive Philosophy had given no place to a science of psychology. James quoted Comte at some length, then John Stuart Mill arguing to the contrary, eventually siding with Mill's defense of scientific psychology.
- The classic presentation of the scientific program of the brass-instruments laboratories was Edward Bradford Titchener's (1901,1905) detailed and scholarly laboratory manuals. There were four volumes, a student's manual and an instructor's manual for qualitative experiments and a student's manual and instructor's manual for quantitative experiments.[2] With the use of quantitative experiments, Titchener argued, psychology became truly scientific:

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[1] This is Boring's (1963) retrospective expression of the mission of the brass-instruments laboratories, reflecting Titchener's understanding of their program and, perhaps, Wundt's.

[2] Compare Sanford's (1897–1898) incomplete set of laboratory manuals which, as far as they go, show a similar program.

> Immanuel Kant (1724-1804)—not a mediaeval philosopher but the author of the *Critique of Pure Reason* and one of the most influential thinkers of modern times—declared roundly in 1786, and found no reason later to change his opinion, that psychology could never attain the rank of a true science. (1905, p. xxiv)

> Titchener held that it was the work of Fechner's psychophysics, establishing mathematical connections between psychological and physical events, that proved Kant to be wrong.[3]

With the waning of the brass-instruments laboratory, the first justifications of a science of psychology lost their force. Compounding the problem was the fact that psychology seemed to be breaking apart into schools. American authors—Carl Murchison (1926,1930) in his *Psychologies of 1925* and *Psychologies of 1930,* Edna Heidbreder (1930) in her *Seven Psychologies,* and Robert Woodworth (1931) in his *Contemporary Schools of Psychology*— published books describing the several psychologies of their time, disagreeing somewhat as to their number.

Edna Heidbreder (1930,) began her *Seven Psychologies* with the remark: "It is something of a paradox that systems of psychology flourish as they do on American soil. Psychology, especially in the United States, has risked everything on being science; and science on principle refrains from speculation that is not permeated and stabilized by fact. Yet there is not enough fact in the whole science of psychology to make a single solid system" (p. 3). For Heidbreder, a truly scientific psychology ought to be built upon a single, solid system "permeated and stabilized by fact." Was it a lack of facts that was causing Psychology to come apart into systems? Had Psychology "risked everything on being science" and lost?

Some European psychologists went beyond Heidbreder, describing Psychology's situation in apocalyptic terms. Hans Driesch wrote a book titled *The Crisis in Psychology* in 1925. Karl Buhler wrote *Die Krise der Psychologie* in 1927. L. S. Vygotsky wrote an early monograph titled *The Historical Meaning of the Crisis in Psychology* in 1927.[4] William McDougall—living in America but, I think, taking a European approach to Psychol-

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[3]For Kant it was fundamental that the data of any scientific discipline be susceptible to mathematical treatment. For a lucid discussion of Kant's reservations about the possibility of a scientific psychology, see Danziger (1990).

[4]The analysis of Psychology's crisis set forth in Vygotsky's monograph was a cornerstone of his developmental psychology. Curiously, although Vygotsky's developmental psychology has had a very large influence in the United States, for a good many years the monograph was known in the United States only through secondary accounts of it (Kozulin, 1990; Van der Veer & Valsiner, 1991). Vygotsky's (1927/1997) monograph has only recently appeared in English.

ogy's situation—wrote a paper titled "The Present Chaos in Psychology and the Way Out," in 1930.[5]

Psychology's status as a science had to be asserted. The knowledge of the discipline had to consist of something more meaningful than the disputations of a set of medieval schools. Psychologists of the late 1920s and early 1930s had to be interested in scientific prospectuses that might point the way toward some meaningful unity of their discipline. The search for some possible unification of a pluralistic discipline of Psychology was a significant—and, I think, seriously underestimated—factor in the commitment of many American psychologists toward methodological behaviorism in the 1930s.[6]

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[5]German intellectuals may have tended to pose fundamental issues as "crises." Ermarth (1978), in his study of Wilhelm Dilthey, remarked: "Dilthey's thought cannot be understood apart from this context of prolonged, even endemic, crisis. The period through which he lived was marked by controversy and upheaval in almost all levels and sectors of existence. He lived through the *Hegelstreit, Religionsstreit, Materialismusstreit, Darwinismusstreit, Pesimismusstreit,* and the *Methodenstreit;* it was, as one observer noted in retrospect, 'an endlessly strife-racked age' (p. 16).

[6]The pluralism experienced by psychologists in the 1930s was not new but in part political, the felt loss of a core or leading element of the discipline.

The new (scientific) psychology had been plural from the beginning. In a landmark study of the psychological experiment as a social institution, Danziger (1985, 1987) analyzed the contents of the eight major psychological journals—two French, two German, and four American—between 1879 and 1898. We customarily date the beginning of modern psychology as 1879, the date of the opening of Wilhelm Wundt's Leipzig laboratory, and so Danziger began his analysis then. Danziger found differences in the forms of the research being reported by the eight journals, corresponding to different social arrangements that were being made to create psychological data. (We do not ordinarily think of a psychological experiment as a cooperative arrangement between people, but of course it is exactly that.)

Depending on the journal, the psychologist might be called the *experimenter, observer, operator,* or *attendant;* the subject might be called the *subject, reactor, percipient, individual under experiment, observer,* or *experimentee.* Danziger ultimately concluded that there were three models of research practice to be found in the earliest psychological journals, what he called the "Leipzig model," the "Paris model," and the "Clark model." The Leipzig model was the form of research being pursued by Wundt in his experimental psychology program; the Paris model was the medically based study of exceptional mental states pursued by Charcot's group; and the Clark method involved the study of characteristics of groups of subjects pursued by G. Stanley Hall and his students.

Danziger's retrospective analysis of early psychological pluralism squares exactly with James' (1890/1981) contemporary description of the three principal methods of Psychology, and is similar to Dewey's description of Psychology's methods in his textbook 3 years earlier (Dewey, 1887/1967). The pluralism of the 1920s was not a novelty; what was novel was the loss of the hegemony of the brass-instruments laboratories. The narrative tension in the story of the history of psychology in the 20th century has been, I believe, the field's political vicissitudes in finding and accepting a scientific multiparadigmatism appropriate for the study of the complex hierarchical control structures within which human activities are embedded.

THE "AGE OF THEORY"

From the 1930s until the late 1950s, mainstream American psychology entered into what Sigmund Koch (1985) called an "Age of Theory." The dominant group of American psychologists, experimental psychologists, sought for and found new definitions of the scientific mission and proper direction of their subdiscipline. Prominent vehicles for those definitions—used for the education of graduate students and for discussions and debates about the proper values of the field by the more senior members of the scientific community—were writings in the history of psychology and the philosophy of science.

The History of Psychology. The first edition of Boring's (1929) *A History of Experimental Psychology* had an extraordinarily wide and deep influence on all psychologists. Histories, I believe, have the capacity to map out and make sense of contemporary social and institutional arrangements (White, 1978). Scientific psychology, Boring said, had been created in 1879 in Wilhelm Wundt's Leipzig laboratory when Wundt brought together a philosophical stream of Empiricism-Associationism with a set of procedures invented in 19th century scientific laboratories of Europe. Psychology could be regarded as philosophical epistemology being pursued by other means, an experimental epistemology. There are indications that not only experimental psychologists but all kinds of American psychologists took this history as their own.

O'Donnell (1979) argued that Boring wrote his 1929 history in part as a political act, to squelch a disturbing pluralism that was growing up around him. Conceding that Boring probably did want to do just that, it is hard to believe that Boring could have anticipated the extraordinary influence of his book; Samelson (1980), in a rejoiner to O'Donnell, offered a reasonable discussion of why Boring wrote his history. But it has been only in very recent years that the subdisciplines of Psychology other than experimental psychology have begun to write about their distinctive historical roots.[7] Developmental psychology, for example, is not a descendant of the Leipzig program. Wundt would have placed developmental psychology in his *Völkerpsychologie,* a cultural-historical research program referred to only briefly by Boring as a "history of human nature," and de-

[7]Late in life, Boring recognized that histories of other subdisciplines of psychology needed to be written. Brozek (1991) remarked that Boring wrote him in 1965 about a plan to establish a series, *Histories of Psychology,* to be published by Basic Books. There were to be volumes dealing with physiological psychology, the psychology of learning, comparative psychology, sensation and perception, child psychology, and social psychology. The project was apparently abandoned with Boring's death in 1968.

velopmental psychology has philosophical roots and a social history that are quite distinct from philosophical epistemology (White, in press).

The Philosophy of Science. A second significant organization of psychological work in the Age of Theory was mediated by an articulated body of analysis that: (a) explained how a nonmathematical psychology might be a true science, (b) suggested proper directions for the scientific development of the field, and (c) explained how a set of differing psychologies might in time attain meaningful unity as a scientific discipline.

The philosophy of science embraced by American psychologists had roots in the systematic writings of the Austrian physicist Ernst Mach, in the *operationism* of the American physicist Percy Bridgman, and in the *logical positivism* of a group of philosophers, the Vienna Circle, interested in the reform of their discipline. Useful accounts of the ideas they offered are to be found in readings offered by Marx (1951) and Feigl and Brodbeck (1953), and a monograph by Mandler and Kessen (1959). None of the constituent elements of the philosophy of science had been designed by psychologists for psychologists and what the several sources had to say to psychologists was not perfectly univocal, but it was possible to derive from them an account of a proper form of scientific psychology.

All sciences address the same physical reality, identifying and studying different aspects and relations of that reality. In this respect, there is a unity of science. The philosophers of science rejected arguments that there are fundamental differences between the natural sciences and the human sciences, arguments that had led some to propose that there ought to be two psychologies (Cahan & White, 1992). Though the sciences are alike, there is a ladder of the sciences. Some sciences, such as psychology, deal with concepts and laws that are more complexly derived from physical reality. Their concepts and laws are in principle reducible to the concepts and laws of sciences, such as physics and biology, that are more simply and directly derived from reality.

Sciences pass through a maturing process, something like a developmental sequence. The 20th-century philosophers of science century followed some ideas of Comtean Positivism in the 19th century. All sciences pass through three stages, according to Comte: the theological, the metaphysical, and the positive. In their last, positive stage they are based on empirically verifiable laws. The sciences have matured in order: first mathematics, then astronomy, physics, chemistry, and biology.

For the philosophers of science, psychology might be regarded as an "immature physics." Experimental physics offers a useful model of a mature science. For psychology to mature as a science, it would have to perfect the communications among investigators and the logic of its theoretical formulations. Concepts should be exactly defined in terms of point-at-ables—

or else they should be purely formal, logical or mathematical. Laws should be conditional universals, linking such empirically meaningful concepts. Theories should take an axiom-theorem, or "hypothetico-deductive" form.

At the center of experimental psychology in the Age of Theory, behavior theorists such as Tolman, Hull, and Skinner (for a while) tried to build theories of this ideal form. The philosophy of science held that, within Psychology, experimental psychology, with its laboratories of conditioning and learning and its very physicalistic approach to behavior change, might be regarded as the most scientifically mature of the subdisciplines. Other subdisciplines of psychology—personality, social psychology, developmental psychology—were valid scientific enterprises but their concepts, laws, and theories were relatively immature. These subdisciplines of psychology would perfect themselves as their research processes and knowledge base were progressively translated into the concepts and processes found in conditioning and learning laboratories. The Yale psychologists were particularly active in trying to build such bridges, both in laboratory work and in speculative theoretical treatises.

Curiously, most American psychologists were willing to go along with a history of experimental psychology as a general history of the field. There were no written histories of the other subdisciplines of American psychology during the Age of Theory. It is not at all clear that personality, social, and developmental psychologists were willing to subscribe to the view that their sector of psychology should be regarded as immature and that its inevitable destiny would be to flower into some sort of stimulus-response account. But there was no alternative philosophy of science. A good number of psychologists went along with these formulations during the Age of Theory and so the community of psychologists lived with a very rough definition of its past, its scientific organization, and its program. To show how seriously at least some psychologists took the program, Koch (1959-1963) edited a six-volume study of the status of psychology in terms of the standards of the philosophy of science in 1959. Ironically, the volumes emerged to stand as a memorial of the era. They appeared just as the Age of Theory ended, the "Cognitive Revolution" got under way, and Psychology again went into a time of considerable reconstruction.

DEVELOPMENTAL SCIENCE AT THE MARGINS

During the 1930s and 1940s, the developmental science of the child development institutes and centers in the United States stood apart from the systematizing and theoretical programmatics that had gripped mainstream psychology. The movement was conservative and atheoretical—

probably deliberately so, because it reacted against the florid speculation and advice giving that were emerging in professional and popular literatures about children at that time.

Lawrence K. Frank, who directed the funding of the child developmentnstitutes and andcenters for rthe Laura Spelman Rockefeller Fund, complained that the movement seemed uninteresting and "lacked theory." The comment seems ironic to modern eyes. In just the period of the child development movement, the 1920s and 1930s, brilliant theoretical works were being written in Europe—the original edition of Werner's *Comparative Psychology of Mental Development* in 1926, Piaget's earliest writings reporting his studies using the clinical method in the 1920s, and the original edition of Vygotsky's *Thought and Language*[8]—works that would be the driving force of the third establishment of developmental science beginning in the 1960s and that maintain their intellectual presence in developmental science today.

The basic ideas in those theoretical writings were by no means foreign to American soil. Werner's comparative-developmental psychology presented a sophisticated treatment of an evolutionary view of children's development that was quite common in the 19th century. James Mark Baldwin in the United States had anticipated the core of Jean Piaget's work in genetic epistemology. Late 19th-century American writings by Josiah Royce, John Dewey, George Herbert Mead, and James Mark Baldwin offer some ideas about human social and moral development that developmental science has not fully capitalized upon even today (White, 2000). But the research base of the child development movement in United States in the 1920s and 1930s—its methods, norms, and values—simply could not be meaningfully related to those theories in the 1920s and 1930s.

DEVELOPMENTAL PSYCHOLOGY AFTER THE "COGNITIVE REVOLUTION"

The "Cognitive Revolution" of the late 1950s (Baars, 1986; Gardner, 1985) had some significant effects on developmental science. It produced a genuine renaissance of work in the field. Developmental psychology became a central, fast-growing subdiscipline of Psychology. Piaget's methods and theory and, after a time, Vygotskian theory and elements of the research program built up by a generation of Soviet psychologists, broadened the research program of American psychologists. There was substantial social

[8] This was probably originally written in what Minick (1987) would designate as the third period of Vygotsky's psychology work—that is, not too long before his death in 1934.

support for research in developmental psychology because it seemed promising as a research substrate for science education, on the one hand, and programs for the education of disadvantaged children on the other.

An important side effect of the "Cognitive Revolution" has been a release of Psychology's pluralism. There is not on the horizon any scheme for the ultimate unification of Psychology within one scientific framework, but the multiplicity of psychologies does not have to constitute a "crisis." Perhaps it is a division of labor. I believe that from this time on Psychology will have to understand itself as a multiparadigmatic discipline. To be multiparadigmatic does not necessarily mean that the field has to be seen as composed of a set of contentious schools, nor does it mean that it has to be incoherent. Suppose Psychology at large is regarded as an aggregation of research programs using appropriate research methods to examine the several levels of the complex control structures that govern human activities—social and cultural factors, factors coming from the organization of the person's mind and personality, and biological factors.

A second side effect of the "Cognitive Revolution" has been a new need for the several subdisciplines of Psychology to make sense of themselves, to develop an understanding of their own history and the "philosophy of science" governing and guiding their empirical enterprise. You cannot build a meaningful scientific field out of methodology, some statistics, and the ideal of a well-crafted study. There has to be some vision of where the field has come from and where the field is moving, what goals or ideals it serves.

Particularly in view of its new commitment to a cultural-historical approach to human development, developmental science has needed a meaningful understanding of its own history. Recently (White, in press) I argued that developmental science is the creation of modern societies and I set forth three arguments about its meaning for such societies:

1. Developmental psychology as a cooperative human activity is a useful component of a modern society, since much of that society is made up of a web of social institutions and practices dealing with human health, education, and welfare. Developmental psychology participates in the perennial design processes and reconstructions that maintain the ability of that society to address the needs of children and families.
2. In contemporary society, developmental psychologists help with the establishment and management of social programs through work on *representation*—offering indications of the conditions of children and families in American society; *demonstration*—designing programs to explore the possibilities of constructive action; *idealization*— offering ideas about how programs might influence human development; and *evaluation*—designing studies and collecting data to indicate the effects of social programs and policies. These four functions have been reasonably clear in the course

of developmental psychologists' sustained involvement with Head Start (White & Phillips, 2001), and they seem to characterize a good many other participations in the programs and policies of American society.

3. Not only institutions but individual lives must be designed in a modern society. Parents in one way, and children in another, are faced with a disconcerting openness, set of options, and choices of the possible paths a child might take towards adulthood. There are not strong traditions to restrict and guide them. Parents are responsible for complex and poorly-understood negotiations and coordinations with professionals in their upbringing of children. Parents are somehow expected to somehow find "optimal child development" and a career line for the child in a "world of work" holding out some 40,000 occupations. Children are faced with school tasks whose significance, they are told, is very large but whose human meaning is not always completely clear. A substantial part of the work of contemporary developmental psychologists has been to contribute to psychological testing, counseling and guidance, and assorted remedial and clinical services, in support of a child's complex task of finding his or her way towards adulthood. We assign a quasi-medical, or "mental health," interpretation to much of this work, but that designation is wearing thin and we badly need to reconstrue the nature and meaning of such work at this time (White, 1996). But the contributions of developmental psychology towards the design of individual lives is, I have little doubt, quite real and quite necessary in a modern society.

THE SCIENTIFIC ORGANIZATION OF DEVELOPMENTAL SCIENCE

Developmental science needs, in addition to an understanding of its own history, some kind of vision of what the community of developmental scientists are doing together that we might call, provisionally, its "philosophy of science." The traditional vision of the philosophy of science aligned all scientific work with its interpretation of experimental physics and offered principles for the guidance of psychology that directed it toward physics-like laws and theories. But we have had 100 years of developmental science. What we have is clearly valuable but not physicslike. Perhaps we can begin to move toward an understanding of the processes and principles of developmental science based on that experience. It is beyond my powers to offer a philosophy of science for developmental science and, I must confess, I am inclined to believe that the elucidation of its processes would have to include elements of a sociology of science and a politics of science as well—that is, the kind of three-dimensional examination of scientific processes recently offered by Latour (1999).

I would like to offer a few theses that I believe should be included in a realistic account of what developmental science is and does in a modern society:

1. *Developmental science deals with long, slow, complex processes of human change taking place over months or years. Developmental science stands in relation to psychological science somewhat as geology stands to physics.* It has sometimes been argued in the past that because developmental science studies time, and time does not cause anything, then developmental science is scientifically empty. But no science directed at human development directly studies causes, because humans are complex systems subject at every moment to a multiplicity of causal and controlling events. You get something that looks like a causative study in a treatment-comparison experiment, where you examine whether one treatment has a different effect on a subject's activity than another. But anyone who has ever tried to replicate a treatment-comparison study knows how complex the causative and contextual factors are that have to be controlled in order to reproduce a treatment-comparison effect. Developmental science studies sequences. Within the orchestration of psychogical sciences, developmental science deals with longer term trends of change of relatively higher order organizations of human activity, changes over periods ranging from days (microgenetic studies) to months—in contrast, say, to cognitive neuroscience that addresses sensory or short-term memory events taking place on a time scale of milliseconds or minutes.

2. *The environment in which human development takes place is a constructed human environment, with objects and affordances impregnated with human intelligence.* The subjects of developmental science do not address a physical world composed of simple tones, lights, smells, touches, and tastes. William James used to argue that no human being really addresses such a world, because that kind of world is a construction produced by sophisticated physical analyses of human experience. Nor do humans address the classic Darwinian world, a world of oceans, plains, rivers, savannas, and mountains within which they must forage to satisfy "primary drives" such as hunger, thirst, and the avoidance of pain and danger. Humans in a modern society live in largely human-made world, with objects, activities, and behavior settings reflecting the designs of other human beings around them. We have only recently begun to closely examine the actual environment in which human development takes place, and the activities that define human development in such an environment (Barker & Wright, 1954; Bronfenbrenner, 1979; Wohlwill, 1985). We cannot fully understand human development unless we examine it in the environment in which it exists and to which it responds.

3. *The line between basic and applied developmental science is by no means clear.* If we are to study human development in the natural and complex environment in which it takes place, we are going to have to venture out beyond the laboratories and tabletop exercises of the university and into the everyday world of human affairs. But people in their everyday envi-

ronments are going to tolerate very little of the observations and intrusions of researchers as bystanders. Furthermore we want to try to understand subjects when they are committed to and involved with an activity and engaged with other human beings. The price of admission to the sphere of such activities is some degree of engagement with people in terms of their activities and the goals they seek. I believe we need to seriously reconsider the line between basic versus applied research in developmental science. We know that the participations of psychologists in clinical, rehabilitative, educational, programmatic, and policy-oriented activities has brought valuable observations and insights about human development in the past, and I suspect that a more open attitude toward basic versus applied inquiries might materially assist us in developmental science.

4. *Human development is not simply a matter of adapting to the environment. It is in part a process of learning to constitute, construct, and reconstruct environments.* The environment in which human activities take place is not fully an external arrangement that has to be addressed and contended with as is. It includes behavior settings that are in part constituted by the subject's own activity and whose fundamental design is, at times, redesigned and reconstructed by people. We have now before us a broad plan of a historical sequence of human evolution in which human activities have coevolved with the environments in which humans live (Deacon, 1997; Donald, 1991). Nelson (1996) began the process of construing basic sequences and patterns of human development in terms of such a coevolutionary scheme. I believe we will have to see microgenetic sequences themselves in coevolutionary terms, as transactions taking place between a changing human and a changing environment.

5. *Developmental science is fundamentally multiparadigmatic.* The traditional philosophy of science took as its premise a unity of science, but it seems reasonably clear today that psychology maintains a number of co-operative empiricisms and should reasonably be regarded as multiparadigmatic. Developmental science is similarly multipararadigmatic addressing, through a division of labor, different levels of a heterarchical control structure. We would not expect an examination of the relationship between culture and personality to use the same kind of research design as an examination of friends and enemies in the fourth grade. We commonly use similar statistics across widely different sectors of psychology, but the conventional use of statistics of inference is a convention that arose in the 1920s in psychology (Rucci & Tweney, 1980) and there is today an increasing use of qualitative methods of data analysis. The unity we can expect across the different programs of developmental science is a unity of coherence, of consistency between the findings of one sector and another—as biologists look for consistency between patterns observed at

the naturalistic and microbiological level. Although psychological researchers using different procedures, such as Skinner box studies of animal behavior versus tachistoscopic studies of human perception, cannot directly replicate one another's work and thus seek for falsification in an absolute, Popperian sense, they can come up with findings that seem inconsistent. An experiment using one procedure might suggest that space perception has a familial, genetic basis while work with the other procedure finds that space perception can be trained. The two seemingly inconsistent findings might well be reconciled. But the existence of apparent consistencies or inconsistencies across paradigms gives chances of mutual regulation, possibilities of confirmation or falsification, to investigators working in distinctly different research paradigms.

6. *Conceding that any and all cooperative empiricism depends on falsification, there is no need to design the developmental sciences so as to allow for quick, immediate falsification.* One of the most restrictive aspects of the traditional philosophy of science, and of a generation of psychological methodology that followed upon it, was its great emphasis on quick and certain falsification. Concepts had to be absolutely pure and solidly grounded on operational definitions or else there would be lack of understanding between investigators or, worse, much time wasted on the study of "pseudoproblems." The fundamental standard of effective scientific work is falsification. But the requirements set forth by the philosophy of science dictated that any assertion by one investigator be instantly and completely falsifiable by another. Perhaps this is too restrictive a requirement. To begin with, a good many studies of human development cannot be replicated because often the people, place, and occasion that served as the situation for one study cannot easily be reproduced.

It seems to me there must be falsification of some sort if a group of investigators are to share a cooperative empiricism. If one person's observations cannot regulate another's—deny them, qualify them, add to them, or set conditions for them—then there seems little meaning to the cooperative process. But it is not clear that the only useful form of falsification can be a swift, sure duel to the death between one person's findings and another's.

Consider, as an extreme case, psychoanalysis. We are in a period of active Freud-bashing and psychoanalysis, which Freud held forth as a "science" of the human mind, is constantly held forth as an example of prescientific or nonscientific inquiry. Today, a good deal of psychoanalytic theory is considered to have been invalidated by subsequent work, or else there have been translations or alternative explanations for the traditional Freudian claims. But this very activity, it seems to me, amounts to a form of falsification. Should we give more credence to Freud's claim that he was mounting a scientific inquiry through his psychoanalysis?

Because the principle of falsification is so central to cooperative inquiry, I would hope that a revised, updated philosophy of science for developmental science would give some serious attention to the limits of the principle for our scientific inquiries.

Can we, should we, write a philosophy of science for developmental science, a body of governing principles for the cooperative empiricisms that we all maintain together? I believe we should, just as I believe we should develop a convincing history of developmental science. Josiah Royce once remarked that what binds disparate people into a community is the conviction that they share an ideal past and ideal future. We need to share a common vision of our enterprise.

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8

The Institute of Child Development: Pioneering in Science and Application

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The Institute of Child Development was an idea whose time had come. The date was July 1, 1925, and John Anderson arrived from Yale to arrange for the remodeling of the old Publications Building on Pillsbury Drive, buy office and research equipment, and hire a staff. The Institute idea, however, did not originate within the University of Minnesota. Funds for the unit were not available within the institution, and neither undergraduate nor graduate students were clamoring for courses in child development. Why, then, did the Institute of Child Development originate when it did and where it did?

One must consider, first, that "the 1920s, the decade of the flapper and the bootlegger, was also the decade of the child, a unique period when trends in science and society converged to bring about the unanticipated founding of the scientific study of children" (Smuts, in press). More about that later. We stress at the outset, however, that the founding of the Institute of Child Development was an outgrowth of a movement—a movement that drew its energy from a broad surge of interest within the United States in social betterment that might be achieved through improved understanding of the child. Our account summarizes the origins of this movement and some of the institutions and individuals involved in it.

Second, the University of Minnesota's involvement in this movement is not surprising given that, since its earliest days, the institution was governed by the provisions of the Morrill Act of 1862. But the faculties of many Land Grant universities were not persuaded that the creation of a "science and technology" of child development was a scholarly endeavor that was likely to produce anything of much consequence (Bradbury & Stoddard, 1933). Why, then, was this idea translated into a research and dissemination center at Minnesota and not in other places? And why has this unit continued with increasing productivity and influence when, elsewhere, similar units have closed their doors?

The answers to these questions constitute a remarkable story, the significance of which extends beyond the university. Indeed, the existence of the Institute of Child Development exemplifies the impact of social demand on the history of science, the influence of the women's movement on science policy, and a turning point in American society's commitments to its children. The continued existence of this unit—through the Great Depression and World War II as well as through funding and personnel crises—is also a story whose significance extends beyond this institution.

In this document, we bring together information about "who," "what," "when," and "where" with respect to the Institute of Child Development. But we also want to place this information in social and historical context. Only then can the achievements of the many foundation officials, administrators, scientists, parents, and children who contributed to the unit over the years be fully appreciated. We believe that the Institute of Child Development has contributed as much as any unit in the University of Minnesota to the "search for truth" and "the welfare of the State" (words carved over the entrance to Northrop Auditorium). Although science and application have not always moved together in a neat synergism within the Institute, social demand clearly created the place, social demand supported its continuance over three fourths of a century, and social demand will sustain it into the future.

THE INSTITUTIONALIZATION OF CHILD DEVELOPMENT

The Child and Childrearing at the Turn of the Century

By the end of the 19th century, developments in medical science were providing a new focus for those interested in child care. The widely used manual, *The Care and Feeding of Children* (Holt, 1894), preached the benefits of a scientific approach to sleep and feeding practices. In 1914, the U. S. Children's Bureau began publishing the pamphlet, *Infant Care,* providing practical advice for mothers with a heavy focus on prevention and treat-

ment of illness. As Weiss (1977, 1978) pointed out, these early advice givers also offered techniques to ease the burdens of motherhood, including early toilet training (expected to be complete by the age of 1), strict scheduling of meals and naps, and putting children to bed by six in the evening to allow the mother to have a quiet evening meal. The "child" was not the complex cognitive agent we assume today, and child-care advice focused on efficient task management. Psychologists of the late 19th century had staked out the adult mind and found it to be complex terrain, but the child's mind was not considered interesting except by a few (e.g., Baldwin, 1895). Unfortunately, the new field of child guidance did not exist on very strong scientific foundations. Nevertheless, popular interest in childrearing continued to increase.

A change in the relation between science and child welfare began to occur between 1915 and 1925 (Cravens, 1985; Sears, 1975; Smuts, in press). Arguments began to be heard with increasing urgency that effective intervention in children's lives requires a sound empirical base and that scientific work should be given increased priority. By this time, the child was coming to be seen as psychologically intricate. Larger social forces were also interacting with emerging scientific endeavors in a way that motivated a closer look at the child. Mothers, for example, began to advocate for better scientific knowledge of childhood and, in the end, women were the most significant movers and shakers of the early child development movement.

The Iowa Child Welfare Research Station and the Laura Spelman Rockefeller Memorial: Visions of the Future

Cora Bussey Hillis was an Iowa mother and activist who became convinced that facilitating the study of normal growth and development was her own personal responsibility. She believed passionately that Iowans should fund child science themselves:

> Give the normal child the same scientific study by research methods that we give to crops and cattle.... Learn how the normal child develops in body, mind, and spirit and gradually evolve a science of child rearing by accumulated, comparative data and by intensive study of selected groups carried on through the years under natural conditions and in a controlled environment. (Bradbury & Stoddard, 1933, p. 7)

Echoing the widely perceived link between proper child care and the health of a democracy, she added, "I realized that help of a dependable character could come only through applied science based on study of the normal child, the most valuable asset of any nation" (p. 7). Sixteen years

were required for Mrs. Hillis and an organized network of Iowa women to convince the men in the Iowa State Legislature that this idea was worth spending money on. Ultimately, they succeeded and the Iowa Child Welfare Research Station (ICWRS), with Bird Baldwin as its first Director, was established at the State University of Iowa (Iowa City) in 1917.

A philanthropic intervention was also needed for scientific child study to move ahead. In 1918, John D. Rockefeller founded a special fund in his wife's memory, the Laura Spelman Rockefeller Memorial (LSRM), to support research and social programs to improve the lives of women and children. Beardsley Ruml became director in 1922, changing the agenda from support of Mrs. Rockefeller's charities to support of social science broadly. Lawrence K. Frank, an economist, become administrator of programs for children and parents, and he felt strongly that something needed to be done to improve the upbringing of children during the early years. Science applied to education would not bring about meaningful reform, he believed, because children were in the care of their mothers at home during the crucial character-building preschool years. What was needed in Frank's view was a program of education grounded in scientific knowledge about child development and aimed at parents (i.e., mothers): "I proposed the child study program [to Beardsley Ruml] and indicated how it could be implemented through research centers and fellowships for the training of personnel and by a concurrent program of parent education all over the country" (L. K. Frank to M. J. E. Senn, January 10, 1966, NLMA, quoted in Senn, 1975, p. 15).

THE INSTITUTE OF CHILD DEVELOPMENT

Establishing the Unit

By 1922, the LSRM had begun to fund specific research projects at the ICWRS and, in 1924, granted monies to Teachers College, Columbia University, to found a new institute there. Meanwhile, Frank began conversations with Lotus D. Coffman, president of the University of Minnesota and a former college classmate, about the possibility of a child welfare center at Minnesota, one that would be a "friendly rival" to the ICWRS (Cravens, 1993, p. 63). Coffman was actually more interested in funding for "cooperative cottage housing" for women students at the university, but the LSRM declined to get involved in that project (L. D. Coffman to B. Ruml, January 7, 1924; B. Ruml to L. D. Coffman, January 18, 1924, LSRMA). Instead, Frank turned Coffman's attention toward the idea of a child welfare research institute. He believed that land grant institutions are especially appropriate settings for such enterprises because they could function as

"counterparts of agricultural extension agents, disseminating the new [scientific] findings to parents instead of to farmers" (Smuts, in press). Ironically, just as plans for the Minnesota institute were getting underway, Cora Bussey Hillis was killed in an automobile accident in St. Cloud, Minnesota.

Establishing a child welfare institute at the University of Minnesota was opposed initially by some faculty members:

> [The idea] aroused a great deal of resistance and denunciation from various schools and departments which believed that it was a waste of money to study children when they so urgently needed funds for their own well-established research programs. It must be remembered ... that in the 1920s and 1930s there were relatively few funds available for scientific research in our American universities and a great deal of rivalry for whatever money might be available. (L. K. Frank Oral History, NLMA, quoted in Senn, 1975, p. 15)

Not surprisingly, some of the resistance at the University of Minnesota came from the newly founded psychology department (Cravens, 1985; Schlossman, 1981) where the faculty welcomed the research opportunities but were unenthusiastic about the parent education that would be mandated by the LSRM. President Coffman managed to create greater acceptance for an institute by organizing a faculty study committee "to re-educate the opponents" (Cravens, 1985, p. 443) and by promising faculty research time and funds in exchange for teaching in the institute. The reeducation effort was so successful that, then, Frank had to resist "a vigorous effort by the School of Education to get the proposed center under its jurisdiction," telling the Minnesotans that "the Memorial would finance a center only insofar as it was a semi-independent or autonomous unit within the university setting which was open for participation by any university school or department interested" (quoted in Senn, 1975, p. 15).

A grant application for support to establish an Institute of Child Welfare at the university was submitted to the LSRM in January 1925. Written by an interdepartmental committee (see Table 8.1), the application requested funds for a research and administrative staff, a nursery school for 30 children, an infant home providing round-the-clock care and observation of six infants, an extension worker, and research equipment. The staff was to be interdisciplinary including faculty recruited from pediatrics, anatomy, psychology, educational psychology, sociology, home economics, the extension division, and the psychopathic department (as it was then called) of the university hospital. The proposed budget was $56,300, including $17,500 for research assistants—a sum that was questioned by Lawrence Frank because he did not think it necessary to provide stipends for graduate students! Coffman argued that this funding was necessary but, even so, the

TABLE 8.1
Planning Committee for the Institute
of Child Welfare (1923-1925)

Frederick J. Kelly (Dean of Administration), Chair
Eula Butzerin (Public Health Nursing)
F. Stuart Chapin (Sociology)
Harold S. Diehl (Student Health Service)
Richard M. Elliott (Psychology)
Melvin E. Haggerty (College of Education)
A. S. Hamilton (Medicine)
Wylle B. McNeal (Home Economics)
Richard R. Price (General Extension Division)
Richard E. Scammon (Anatomy)
F. W. Schultz (Pediatrics)

negotiations with the LSRM resulted in taking $4,000 from the budget for graduate assistants and adding it to the budget for the extension service.

On February 21, 1925, Frank wrote to Coffman expressing this and other concerns: "The plan for education of parents is not sufficiently developed ... we do not think that merely adding on some courses in the extension department will get very far ... we would want to have some clearer indications of what was possible in Minnesota before we could accept the program submitted" (L. K. Frank to L. D. Coffman, February 21, 1925, LSRMA). President Coffman then went back to Dean Frederick J. Kelly and the committee to develop a more detailed plan and the hastily put-together revision was submitted to the LSRM on March 10, 1925. Several pages of projects became the basis for parent education activities in the Institute of Child Welfare for the next 35 years. Word was received in a letter dated April 9, 1925 that the LSRM would appropriate $245,000 over a 5-year period to the university to establish the Institute of Child Welfare (UMNA).

But Lawrence Frank continued to give advice: "If the university nursery school does not provide the best which modern science can devise ... it will not be very easy to persuade mothers generally that intelligent and enlightened child care is important" (L. K. Frank to F. J. Kelly, April 23, 1925, UMNA). He weighed in on the selection of the director, reminding administrators that the mission of the unit was to accumulate knowledge about the *normal* child. One month later, John Anderson, a student of Robert Yerkes, was recruited from Yale where he was studying the effects of diet on learning in rats (he had commanded a mental testing unit during World War I). In New York, Lawrence Frank seemed pleased: "This is very interesting news, indeed, and I am delighted to hear that you have found someone without prolonged delay" (L. K. Frank to F. J. Kelley, June 12, 1925, UMNA).

Beginnings

By the summer of 1926, John Anderson could submit an impressive report to the Laura Spelman Rockefeller Memorial (AR, UMNA). Containing a list of more than a hundred research projects that had been mounted during the first year, the report also included a roster of the people working in the unit. Anderson was successful in luring Florence Goodenough to the Institute from the Minneapolis Child Guidance Clinic, and affiliations had been worked out with Richard Scammon and various members of other departments at the university to conduct research in the nursery school and teach courses. Anderson did not regard the faculty as fully formed, however, until 3 years later when he reported to the LSRM that the organization of the Institute of Child Welfare was more-or-less complete (AR, 1928–1929, UMNA).

Small as it was, the faculty (see Table 8.2) hit the ground running: Goodenough and her students began more than 20 projects during the first year alone, and the faculty budgeted in other departments conducted dozens of other studies. Within the next few years, five best-selling books appeared including the famous *Modern Baby Book* (Anderson & Goodenough), *Child Care and Training* (Faegre & Anderson), *Nursery School Procedure* (Foster & Mattson), *Experimental Child Study* (Goodenough & Anderson), and *Developmental Psychology* (Goodenough). Taken together, the work of both the budgeted and nonbudgeted staff demonstrated convincingly that the unit would be productive.

As a multidisciplinary unit, the new Institute of Child Welfare was organized according to the commitments made in the grant application. Most of the appointments made through the Institute's budget were psychologists; faculty members from other departments contributed effort but cross-charges did not accompany them.

Like many annual reports, those written by John Anderson for the LSRM every year for 14 years are cold documents, replete with organizational charts, rosters, statements of income and expenditures, research status reports, and publications. One cannot reconstruct from these docu-

TABLE 8.2
Institute of Child Development Professorial Roster: 1929–1930

John E. Anderson, PhD, Professor and Director
Edith Boyd, MD, Assistant Professor
Marion L. Faegre, Assistant Professor and Extension Worker
Josephine C. Foster, PhD, Associate Professor and Principal, Nursery School and Kindergarten
Florence L. Goodenough, PhD, Associate Professor
Esther C. McGinnis, PhD, Associate Professor and Chair of Parent Education

ments what day-to-day life was like in the unit, especially with respect to the interaction between the psychologists and the nonpsychologists. One knows from the annual reports that nearly everyone conducted research in the nursery school. Oral histories suggest that the budgeted staff comprised an unusually cohesive unit and that Anderson was a strongminded administrator (Harris, 1996). He also had a strong-minded administrative assistant, a secretary whose name was Kathryn Bellows and who stayed from 1926 to 1968—the longest tenure any staff member has achieved in the Institute of Child Development. Tough as nails, Mrs. Bellows was the "conscience of the Institute," according to Dale Harris (Mildred Templin, personal communication, 2000).

The financial arrangements in 1925–1926 consisted of a $50,000 grant from the LSRM ($491,418 in today's dollars) plus $4,000 for equipment.[1] Not a penny was provided by the University of Minnesota, nor did support from the university exceed $8,300 in any one year for the next 14 years. John Anderson was hired for a salary of $6,000, Florence Goodenough for a salary of $3,500, and nursery school teachers for $1,500–$2,000 each, but even at these salaries, the grant could obviously not support more than a small staff.

Clouds, however, were on the horizon. Various signs suggested that the new child development institutes and the other mechanisms created by the LSRM had become Pandora's boxes: The public clamored for more and more information whereas the research laboratories could not produce it fast enough. Unhappy with this situation, the LSRM changed course in 1928, deciding to put most of its resources into research. Consequently, the budget of the Institute of Child Welfare was renegotiated that year in order to assure funding of the Institute for 10 years[2] at an initially increased rate of funding for 4 years ($90,000) but decreasing to zero by the end of the period.

Crisis and Uncertainty

Within the next 4 years, two realities began to sink in. First, the Great Depression caused consternation with academic budgets generally, and it was clear that the university could not be relied on to take up the funding burden once the LSRM funds were exhausted. Morale was given a major blow by a universitywide directive to reduce salaries in 1932-1933 by requiring workers making more than $1,200, to take, first, "payless vacation" days, and, then, salary reductions calculated by a complex progres-

[1]$202 was also collected in fees that year.

[2]Although LSRM funding was supposed to end in 1937–1938, Anderson arranged later to split the final allocation between that year and the next.

sive method (Goodenough lost roughly 20% of her salary).[3] Second, the approaching cessation of funding from the LSRM began to loom as a major crisis: "In developing our plans for the future, it seems to me that we must give consideration to the group of psychologists associated with Dr. John E. Anderson....By the close of the fiscal year of 1938 it will all be gone" (L. D. Coffman to M. Haggerty, College of Education, April 26, 1932, UMNA). President Coffman then went on to ask whether Dean Haggerty could support these individuals in the College of Education. The answer: "I see no immediate possibility.... I shall, of course, be happy to keep the situation in mind" (M. Haggerty to L. D. Coffman, April 29, 1932, UMNA). Central administration dithered for more than 10 years, supplying only nominal support from university budgets to the Institute of Child Welfare during that time whereas support from the LSRM dropped inexorably to zero.

By 1932, two sources were seen as possible ways to restore some of the lost foundation monies: (a) Fees for nursery school and parent education services, begun in 1926–1927, could be increased substantially; (b) a "special appropriation" could be requested from the state legislature. This latter idea was immediately approved by President Coffman and a request was made for $30,000 in June, 1934; $5,000 was eventually approved for the fiscal year 1935–1936 (UMNA).

Faculty development ceased during this unstable period for obvious reasons. The professorial faculty in 1939–1940 consisted of five of the same six individuals who were in place a decade earlier, the sixth (Esther McGinnis) having left in 1937 without being replaced. The Second World War then broke out, placing further constraints on the Institute's development and raising new questions about its future. Promising new instructors such as Dale Harris and William Martin left for service in the Armed Forces, although Harris was finally appointed as an assistant professor in 1943–1944. This was the first new professorial appointment in 15 years other than that of Elizabeth Fuller who replaced Josephine Foster as principal of the nursery school. The effects of the Great Depression and World War II are starkly evident in the budget book: Income reached its nadir in 1940–1941 and 1941–1942 ($41,615 in both years) and, owing to the shrunken size of the staff, the publication rate fell to barely half of what it had been a decade earlier (ABP, 1937–1947).

Oral histories suggest that these were not the happiest years at the Institute although training, research, and parent education activities continued: Marian Radke (Yarrow), Mildred Templin, and Dale Harris all took their degrees during this time. But Florence Goodenough was in failing

[3]The faculty tried to negotiate with the university for a reallocation of the reduced salaries in order "to study the effects of the Great Depression on children" (UMNA). They did not succeed.

health, and ties between the Institute and its allied departments seem to have loosened considerably. State funds, however, began to flow into the Institute once the war ended. By 1949–1950, the budget was more than double what it had been 10 years before and several new appointments had been made.

John Anderson resigned as director in 1954[4] after beginning a reorganization of the Institute's contributions to training in clinical psychology (Dale Harris worked on relations with school psychology) and moving the entire operation of the Institute to the building it now occupies. The "balance" between research and parent education activities continued under the new director, Dale Harris. When John Anderson resigned, the university administration finally asked that the Institute affiliate itself with one of the university's colleges rather than continue to be administered by the Office of Academic Administration as it had been since 1925. Although this issue might seem innocuous, it was not. The research faculty regarded themselves as scientists and, to them, a transfer to the College of Science, Literature, and the Arts (CSLA) was most desirable. It took 2 years of wrangling to reach an agreement whereby the unit would be attached to the College of Education "for purposes of administration" but that instructional programs lodged in other divisions of the university would remain there. So, on July 1, 1957, the arrangement began that continues to the present, whereby the Institute is a department of the now renamed College of Education and Human Development while simultaneously offering the fifth- or sixth-largest program of undergraduate study in the university's College of Liberal Arts.

The rancor surrounding the administrative shift contributed to early burnout for Dale Harris (Harris, 1996) and it was accompanied by increasing malaise in the parent education program and low productivity in research. Before his departure in 1959, however, Harris made two contributions: The name of the Institute was changed in 1957 to the Institute of Child Development and Welfare, and a successful training grant application was submitted in 1959 to the National Institute of Mental Health (NIMH) requesting fellowships to support graduate training in child psychology at the Institute. Although the word *welfare* was subsequently dropped in 1963, the new name and the training grant have endured. (The training grant is actually among the oldest continuing training grants currently funded by the NIMH.) Somehow, the Institute had survived the hiatus between the time when social science was funded by the great foundations and the time when the federal government assumed the main responsibility for funding research in these scholarly fields.

[4] His festchrift became the most famous book ever published by the Institute of Child Development, *The Concept of Development* (Harris, 1957).

Renaissance

Harold Stevenson became the third director in 1959–1960. Morale was at a low ebb (Mildred Templin, personal communication, 2000). Parent education continued until this time although innovation was rare and intervention studies showed mixed effects with no clear signs as to the differential efficacy of various dissemination strategies (Harris, 1996). Concerning the unit, which was now called the Institute of Child Development and Welfare, Stevenson said:

> John Anderson called to say they wanted me to be a consultant about what they should do with the Institute.... So I went and it was an unbelievable situation because they had something like 13 or 14, 15, I think it was, tenurable positions of which only two had tenured people in them and 13 had people who were, some of whom might have been tenurable but most of them were not. I mean, people teaching clinical psych with M. A.s, and people giving courses on the family with B. A.s in this institute that was supposed to be so good. I don't know what happened but anyhow they offered me the job of being the Director at a young age. (Stevenson, SRCD Oral History, 1993)[5]

Within 4 years, the parent education program was closed, all but two faculty members had resigned, and recruitment was begun of staff representing the new "experimental child psychology" as well as the new look in clinical child psychology. Among the earliest resignations was the director of the nursery school, thereby enabling the recruitment of Shirley G. Moore, "an early childhood educator who knew analysis of variance" (Stevenson, SRCD Oral History, 1993). The dozen years that Stevenson directed the Institute were, in fact, devoted mainly to staff development and improved research funding, although important innovations were also made with both the graduate and undergraduate programs, and support services were improved.

By the late 1960s, Stevenson became convinced that he did not want to spend his life managing things (SRCD Oral History, 1993) and the administrative alignment of the Institute within the university had become a contentious issue once again. Stevenson went to the University of Michigan in 1971, leaving the Institute with a budgeted professorial staff that

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[5]Years later, Stevenson was walking on the Stanford campus "and this car came screeching up behind us and out jumped Pat Sears and she said, 'How old were you when you became the director of the Institute at Minnesota?' I said, 'I think I was 33, 34 maybe I don't know, 1959, so I was 34.' And she said 'Oh, I thought so, Bob was 33 when he became director of the Institute at Iowa' and she dashed off" (Stevenson, SRCD Oral History, 1993).

included a dozen new individuals,[6] many of whom were relatively young (see Table 8.3). After being informed that most of these people had tenure, one site visitor (William Kessen) exclaimed: "My God, you've got your faculty for the next 30 years!"

Continuities and (Some) Change

Campus instability marked the transition to Willard Hartup's term (1971–1982) as director. Classes were closed down by strikes generated by the war in Southeast Asia; male graduate students disappeared without notice and never returned; students delayed taking their degrees. Members of the audience slapped one another on the way into the auditorium to hear a lecture by Arthur Jensen, who subsequently gave his talk to a private audience in the Institute. The situation eased over the decade, but slowly.

Faculty conditions continued more or less unchanged, although departures were few and recruitment slowed. By the end of Hartup's term (1982), the only two faculty members who had been appointed before 1960 (Templin and Roff) had retired, but nine appointees from the Stevenson era remained. In 1975–1976, 50 years after the Institute was founded, the academic and research budgets exceeded $1,000,000 for the first time (54% provided by the university, 43% by the federal government, and 3% from other sources).

Faculty expansion came to a temporary halt in the mid-1980s. During the time that Andrew Collins was the fifth director (1982–1989), budgets were reduced and discretionary funds largely disappeared. Based on real-locations and interdepartmental transfers, however, two major initiatives were begun: First, three faculty members were appointed with expertise in developmental psychopathology, making possible significant realignments within the university of responsibility for graduate training in clinical child psychology. Second, work in the biological bases of behavior was expanded. And, finally, international visitors appeared on the roster with increasing frequency.

These initiatives extended into the term of the sixth director, Richard Weinberg (1989–1999), when the number of full-time faculty positions reached 15. Budgets continued to be tight into the 1990s, however, until Dean Robert Bruininks recommended a substantial allotment from a "strategic investment pool" to support program enhancement. External funds then surged with the establishment of the Irving and William Har-

[6]Among these were Herbert and Anne Pick, who eventually became the first married couple at the University of Minnesota to hold tenure simultaneously in the same department. Sandra Scarr and Philip Salapatek both held tenure in the Institute a few years later.

TABLE 8.3
Institute of Child Development Professorial Roster, 1971–1972

Willard W. Hartup, Professor and Director
William R. Charlesworth, Associate Professor
W. Andrew Collins, Assistant Professor
John H. Flavell, Professor
Michael M. Maratsos, Assistant Professor
John C. Masters, Associate Professor
Shirley G. Moore, Professor
Anne D. Pick, Associate Professor
Herbert L. Pick, Jr., Professor
Merrill Roff, Professor
Philip H. Salapatek, Associate Professor
L. Alan Sroufe, Associate Professor
Mildred C. Templin, Professor
Albert Yonas, Assistant Professor
Robert Wozniak, Assistant Professor

ris Professorships (1990), the Marian Radke Yarrow and Leon Yarrow Endowment for Research in Social Relationships (1994), and the Mollie and Meyer Weinberg Endowment for Child Development and Public Policy (1995). Eight faculty members were appointed to endowed chairs (something that had not happened prior to 1990), along with the expanded support base.

These new developments notwithstanding, six individuals, originally appointed during Stevenson's term, remain on the faculty during the tenure of Ann Masten, the seventh and current Director (see Table 8.4). One can only describe faculty development during the last 40 years, then, as consisting of both substantial continuity and substantial change. As the Institute reaches its Diamond Jubilee, the total budget approaches $5,000,000 (52% in university allotments, 30% from federal granting agencies, and 18% from other sources), more than 10 times the budget for 1925–1926 calculated in today's dollars.

RESEARCH[7,8]

The various centers supported by the Laura Spelman Rockefeller Memorial adopted quite different research agendas. For example, research in par-

[7]For a complete publications list, readers should consult the annotated bibliographies that were published between 1925 and 1970 as well as the bibliographies and research summaries contained in the Annual Reports (1925–1939 and 1964–2000).

[8]The assistance of Patricia Bauer, Michael Maratsos, Anne Pick, and Herbert Pick with this section is greatly appreciated.

TABLE 8.4
Institute of Child Development
Professorial Roster, 2000–2001

Ann S. Masten, Professor and Director
Patricia J. Bauer, Professor
W. Andrew Collins, Professor
Nicki R. Crick, Associate Professor
Byron Egeland, Professor
Michael K. Georgieff, Professor
Megan R. Gunnar, Professor
Canan Karatekin, Assistant Professor
Michael P. Maratsos, Professor
Charles A. Nelson, Professor
Anne D. Pick, Professor
Herbert L. Pick, Jr., Professor
Maria D. Sera, Associate Professor
L. Alan Sroufe, Professor
Richard A. Weinberg, Professor
Albert Yonas, Professor
Steven R. Yussen, Professor and Dean

ent education was emphasized at Teachers College, normative behavioral studies at the Yale Psycho-clinic (Gesell), and broad-based longitudinal studies at the Institute of Child Welfare (later the Institute of Human Development) at the University of California, Berkeley. In contrast, the agendas at both Minnesota and Iowa were deliberately varied.

From the beginning, Institute investigators examined a wide range of topics covering everything from physical growth and the biological bases of behavior to social development and psychopathology. Early faculty and students devoted much time to normative studies and working out effective methods for assessing behavioral development—both in the laboratory and in natural settings. Looking back at these early works, a dust-bowl quality pervades many of them. Remember, though, that one could make a case then for measuring almost anything about a child or its family; most attributes had never been scientifically measured. And also, buried within these works were process-oriented arguments dealing with developmental issues ranging from gene-environment interaction to emotional regulation.

Physical Development and Physiological Processes

Richard Scammon, one of the founders, was one of the university's best-known scientists. Under his direction, everyone studying physical growth in the early days of the Institute worked on measurement—on both direct assessment techniques and formulas for deriving growth curves, velocities, and descriptions of anatomical topography (APB, 1925–1937). Substantively, these investigators and their colleagues covered enormous territory. Based on extensive observations across dozens of studies, Scammon

(1933) concluded that growth curves differ widely for different parts of the body, and those for postnatal growth could be divided into four general types: lymphoid, neural, general, and reproductive. By their own assessment, however, these investigators admitted that they did a much better job of establishing norms for physical growth than they did at understanding the processes involved in growth itself (Scammon & Boyd, 1932). Physical growth research in the Institute of Child Development, however, did not survive World War II and studies in this area have never been resumed.

Recent work on physiological development emphasizes brain-behavior linkages, and includes attempts to calibrate the brain's activity (evoked potentials) so they can be used for identifying the neural correlates of perceptual discrimination (Salapatek, 1981), determine the neural loci of both early memory and face recognition (Nelson, 1997; Nelson et al., 2000), describe the psychosocial processes that regulate the activity of stress-sensitive physiological systems in young children (Gunnar & Donzella, in press), and attempt to understand the manner in which cognitive processes go awry in mental disorders (Karatekin, 2000).

Perceptual and Motor Development

Babies have relatively little control over their bodies at birth. Within little more than a year, they sit, reach, stand, crawl, engage in other forms of locomotion, and acquire a large repertoire of both gross and fine motor skills. These dramatic changes symbolize "behavioral development" for many investigators, especially because they occur in a sequential unfolding that varies only slightly from child to child. One of the most enduring normative studies of early motor development was conducted in the Institute by Mary Shirley (1931), who examined 25 babies longitudinally in order to observe changes in postural and locomotor activity, fine motor skills, and the coordination of the eyes.

After these early successes, research on motor development in the Institute of Child Development languished for 30 years, except for some arcane studies of finger tapping and reaction times (e.g., Tinker & Goodenough, 1930). This hiatus, however, occurred in child development research as a whole. The motor development norms established in the earlier studies were widely incorporated into developmental tests and pediatric practice, but nothing else. Seemingly, researchers had discovered a developmental imperative (Thelen, 2000), and that was that.

Motor development in relation to experience, however, was the subject matter to which child development researchers returned in the 1970s. Within the Institute, the manner in which both children and adults guide their perceptual-motor activity in reaching and pointing, locomotion, way finding, and navigation was examined (H. Pick, 1993).

Modern work on infant perception began generally in the 1960s when measurement techniques were invented based on orientation (eye movements, head turning) and habituation (diminished reactivity to visual or auditory stimuli stemming from repeated exposure). The paradigm shift was first reflected in the Institute when Rachel Keen Clifton (Keen, 1964) used habituation techniques to study auditory perception. Since then, a large number of studies have been devoted to the phenomena known collectively as "perceptual learning," including perceptual differentiation and attention in a variety of situations (A. Pick, 1997) and the manner in which infants utilize two-dimensional sensory input to perceive a three-dimensional world (Yonas & Granrud, 1984).

Language Development

Some of the best-known research completed in the Institute during the first few years dealt with language development. Dorothea McCarthy's (1930) monograph contained norms based on measures that she had devised for individual sounds, first words, second words, 50 words, and so on. She also developed measures of grammatical development, including sentence mean length and the child's use of "complex" or "compound" sentences, as well as the functions of linguistic responses in relation to the environment. These norms became benchmarks in the field, although little or no concern was shown in this work for the deeper structural coherence in language, or for theories of language acquisition. Later, Mildred Templin (1957) showed that the development of articulation follows a predictable, negatively accelerated curve and that the rate of development for adequate articulation varies for different types of sounds.

A major turning point in the study of language occurred in the 1960s with Chomsky's (1969) analysis of transformational grammar including the constructs of deep and surface structure; recent work has reflected this shift. One notion that has been controversial but that has shaped the entire field of language development is that only an organism that possesses a rich, innate linguistic structure could acquire language. These ideas are reflected in Institute studies of word meaning and transformational grammar (Maratsos, 1983). Other recent work stems more directly from the ideas of Richard Whorf's and deal with the relation between thought and language (Sera et al., 2000).

Intelligence

Many of the early studies at the Institute of Child Welfare were devoted to the children's intellectual abilities and their assessment. Florence Goodenough played an important role in defining this agenda. A student of E. L. Thorndike's at Teachers College, Columbia University, she completed her

doctoral work at Stanford in 1924 with Lewis M. Terman: "A completely dedicated scientist and possessing a razor sharp mind, she was intolerant of laziness, carelessness, or lack of intellectual acuity. She was a terror to all but the most competent students. But with children she was gentle and encouraging" (Harris, n. d., p. 3). During her years at Minnesota, Goodenough explored various verbal indices of intellectual ability but also nonverbal ones—most notably human figure drawings. The *Draw-a-Man* test (Goodenough, 1926) was highly reliable, correlated well with standard measures of intellectual ability, and became world-famous.

During the late 1930s, Goodenough and her associates became involved in a series of arguments concerning the modifiability of intellectual development—an iteration of the nature-nurture controversy (Dahlstrom, 1985). Impressive enhancement of children's intellectual capacities through environmental intervention was claimed by investigators at the Iowa Child Welfare Research Station (e.g., Beth Wellman). The Minnesota investigators, however, were skeptical and a rancorous debate ensued. Goodenough (1940) published a series of articles in which she analyzed the conflicting evidence, with some of the analyses based on an adoption study conducted by one of her students, Alice Leahy (1932). In a letter to a former mentor, Leta Hollingworth at Teachers College, Goodenough wrote that she believed that Beth Wellman had "deceived herself... I have told Terman that I think the situation is entirely comparable with that of a religious fanatic who hears the wings of angels in every rustle of the dishtowels on the family clothes line" (F. L. Goodenough to L. Hollingworth, 1939, UMNA). More recently, adopted children and their resemblance to biological and adoptive family members were studied in the sociopolitical context of the 1970s (Scarr & Weinberg, 1983).

Cognitive Development

The first studies in cognitive development at Minnesota were conducted by Edna Heidbreder (1927, 1928), who compared strategies of problem solving among nursery school children, elementary school children, and adults. On the basis of these data, Heidbreder thought that logical reasoning emerged by means of a trial-and-error process that takes a number of years to mature, and not in a series of stages. Jean Deutsche (1937) then devised a large-scale study based directly on a Piagetian model, in which reasoning about physical causality was examined among children across a wide age range. Much overlapping of age groups occurred in types of answers elicited; she concluded that thinking does not develop in definite stages, and that children's explanations depend more on experience and instruction than on stages of maturational development.

Later, elaborations and extensions were conducted of other Piagetian phenomena, including the object concept. Exposing infants to various kinds

of uncertainty in the presentation of visual objects, thereby producing surprise reactions, investigators were able to describe the timetable through which object permanence emerges (Charlesworth, 1969). When John Flavell arrived at the Institute (1965), he continued work on the phenomenon that Piaget called "perspective taking."

Memory development, however, has been the problem that has engaged Institute investigators for the longest period. Josephine Foster (1928) published the first Institute study of memory development—a simple normative study of memory for prose material among nursery school children. Studies in memory development were resumed in the 1960s, when John Flavell took up the task of identifying certain processes underlying children's increasing success with various types of remembering. Generally, Flavell (1970) argued that, as children mature intellectually, they become increasingly conscious of their own mental activities and increasingly active, planful, and strategic in their problem solving. Studies in memory development now under way show that, contrary to earlier beliefs, infants and very young children create memories of events that endure over substantial periods of time—1-12 months (Bauer, Wenner, Dropik, & Wewerka, 2000). Even though these memories are measured using nonverbal techniques, children later recall them linguistically; that is, the advent of language does not make memories inaccessible that were originally encoded nonverbally (Bauer, Kroupina, Schwade, Dropik, & Wewerka, 1998).

Social Competence

Among the earliest Minnesota studies in social development, three are still frequently cited. In Goodenough's (1931) masterpiece, *Anger in Young Children,* new methodology was employed (mothers were used as recorders) with results showing that anger manifestations peak in the 2nd year, are accompanied by less frequent violence as time goes on, and are increasingly symbolic, retaliative, and accompanied by "after reactions." Soon thereafter, Helen Dawe (1934) published her famous study of 200 quarrels of preschool children showing their brevity, the centrality of physical objects in instigating them, and the increased amounts of verbal argument as children grow older. One other investigation from these early days has become a textbook fixture: Mildred Parten's (1932) observations of social participation. The observational nomenclature invented by this investigator entered the language shortly thereafter and has endured: Who does not know what "parallel play" is, or "solitary play," or "onlooker behavior"?

In succeeding decades, constructs such as peer popularity and rejection were differentiated from one another in terms of their correlates (Hartup, Glazer, & Charlesworth, 1967) and the developmental significance of peer reputations evaluated (Moore, 1967). Most recently, results show that rela-

tionally aggressive children, adolescents, and adults are at risk for concurrent and future maladjustment in the form of both peer rejection and problematic friendships (Crick, 1996). Friendships and enmities have been also been examined in studies showing how social interaction between children is modulated as a function of these relationships and which developmental outcomes are associated with them (Hartup & Abecassis, in press).

Parent-Child Relationships

Marian Radke Yarrow (Radke, 1946) conducted one of the more extensive early studies of parent-child relationships—a multimethod, multimeasure investigation focused on authority patterns in these relationships. The most lasting results show secular trends toward less severe disciplinary techniques and that parents' and children's perceptions of discipline are considerably different from one another. Succeeding decades have extended this work in studies dealing with conflicting expectancies in parent-child relations across the transition to adolescence (Collins, 1995).

Relationships between parents and children have also been examined at Minnesota in several well-known longitudinal investigations. The "Mother-Child Project" began in 1975, designed to determine the correlates of good- and poor-quality caretaking in infancy and to chart the subsequent course of development through early adulthood as a function of early-relationship histories. Continuities have been established showing that good-quality relationships between mother and child predict high-quality peer relationships in early and middle childhood as well as other indices of adaptational success (Sroufe, Egeland, & Carlson, 1999). Findings from this investigation, as well as others, were used to develop a preventive treatment program (Project STEEP) for high-risk mothers and their infants (Egeland & Erickson, 1999). Elsewhere, "Project Competence," now in a 20-year follow-up, shows that adversity that accumulates in families is often chronic and related to parenting problems; good resources are less common among children growing up in the context of adversity, but if good resources are available—especially good parenting and good intellectual skills—competent outcomes are likely (Masten & Coatsworth, 1998).

INSTRUCTION

Responsibilities for instruction have differentiated the Institute of Child Development from the other Rockefeller institutes for many years. For example, the Iowa Child Welfare Research Station offered virtually no courses to undergraduates until the early 1960s. The Institute of Child Development at Minnesota, however, has maintained an extensive program

of both undergraduate and graduate studies since 1926,[9] and its instructional programs are woven integrally into the fabric of the University as a whole.

Table 8.5 shows total enrollments in child development courses by decade, beginning in 1928–1929.[10] The large increase after World War II reflects the large increases in undergraduate enrollment occurring then in the university as a whole as well as increased interest in child development that accompanied the advent of Project Head Start. Recent decreases have been associated with shifts in student interests (especially among men) away from the behavioral and social sciences.

Undergraduate Instruction

A curriculum evolved in the Institute over the first three decades that offered some science and a lot of application. What is now the introductory course in child psychology was first offered in 1928, enrolling 931 students in the next 10 years. With undergraduate enrollments increasing greatly in the postwar period, the faculty was pressed by the College of Science, Literature, and the Arts to offer child psychology as a major program for the baccalaureate degree. With course requirements indicating that this was to be a science program, students began enrolling as majors in 1955–1956. Restructured on numerous occasions to reflect new developments in the child development field as well as liberal arts education as a whole, this major program has consistently been among the most popular in the renamed College of Liberal Arts and currently enrolls some 300 students. An honors program (with thesis) has been offered since 1963–1964, and a peer-advising system—the first in the College of Liberal Arts—has been in place since 1964.

Graduate Instruction

Developmental Psychology. Although graduate degrees have been offered in the Institute since the beginning, many students from other departments have conducted their research under the supervision of child development instructors. During the early years, students such as Dorothea McCarthy (psychology), Mildred Parten (sociology), and Willard Olson (education) completed thesis work in the Institute and are ordinarily con-

[9]Only one course, Child Training, was offered in the first year, a course that was taken over from the Department of Home Economics, which continued to cross-list it for many years.

[10]These figures do not include instruction offered by Institute faculty in departments other than Child Welfare or Child Development which, in the early years, accounted for almost a third of the instructional activity of the staff and still account for substantial enrollments.

TABLE 8.5
Total Enrollments in Child Psychology Courses
Institute of Child Development

1928–1929	520
1938–1939	1,034
1948–1949	Not available
1958–1959	1,485
1968–1969	4,200
1978–1979	4,283
1988–1989	3,766
1998–1999	3,170

sidered graduates. Child development has been the declared major, however, of more than 300 students completing PhD degrees, including Ruth Howard, the first African-American woman to receive a PhD anywhere in psychology.

Clinical Studies. Clinical child psychology has been a graduate major in the University of Minnesota since 1952. Child assessment and treatment courses originated within the Institute at the same time that clinical training was administered by the Department of Psychology. The two departments worked together under various awkward management systems until the field of developmental psychopathology began to emerge and resources for clinical child training gradually shifted between the two departments. Beginning only in the 1990s, students are being admitted to a truly joint clinical training program. Student admissions, curriculum requirements, and supervision are now overseen by an interdepartmental faculty committee. A slightly different situation prevails in the training of school psychologists who are admitted jointly to graduate study in the School Psychology Training Program as well as a relevant cognate department (the Institute of Child Development among them).

Special Programs

Like many academic units, the Institute of Child Development has sponsored instruction by radio, television,[11] correspondence, extension classes, continuing education, summer workshops, roundtables, and special institutes. Interdepartmental centers also play a major role in doctoral training and the training of other child development professionals. For example, the Center for Cognitive Sciences was founded in 1964 to support research

[11]Indeed, Dale Harris presented the university's first TV-for-credit course.

on perception, cognition, and learning. Originally called the Center for Research in Human Learning, this unit has been supported from the beginning by the National Institute of Child Health and Human Development. The Center for Early Education and Development (CEED) was organized in 1972, with major support provided by the Bush Foundation. The most encompassing such enterprise, however, is the All-University Children, Youth, and Family Consortium, which was established in 1990 with major input from the Institute. Other centers have come and gone during the long history of the Institute but these are among the most enduring.

Perhaps the best-known supplement to the instructional program is the series of events known as the Minnesota Symposia on Child Psychology. Organized originally by John P. Hill, the symposia were staged annually for 28 years and biennially since then. Approximately 200 distinguished scientists have contributed to these scholarly exchanges and the volumes based on them. Feasting and scientific discourse have always occurred simultaneously at these events but, when invited to give the evening lecture, women no longer appear in formal dress.

THE NURSERY SCHOOL

A laboratory nursery school was opened in the Institute in October 1925: "Immediately the population of the campus was increased and its spirit enlivened by the presence, in the little building near the railroad tracks, of a group of preschool children as young as two and a half years" (Gray, 1951, p. 480). Average enrollments in the early years consisted of 36 children. These children were selected to be representative of the socioeconomic levels in the general population (as mandated by the LSRM) and participation was free. Tuition was charged beginning in 1932 when funding by the LSRM began to decrease and the clientele became less representative of the general population. In order to maintain the integrity of certain study samples, however, some children continued to be admitted on a tuition-free basis until 1935. An experimental kindergarten was opened under Institute auspices in 1928, increasing the observational facilities substantially until it was transferred to the University Elementary School in 1959.

The early nursery school program was centered on physical growth, hygiene, nutrition, and routine health habits (e.g., sleep, eating, and toileting patterns): "The children are received at nine o'clock, fed at noon, put to bed for naps, and then urged into educational play once more until four o'clock. All the while the growth of the child—mentally as well as physically—is studied: its sleeping habits, the development in the use of sense organs, the modification of inherited responses, the unfolding of intelligence" (Gray, 1951, p. 480). Although many laboratory schools adopted curricula that reflected the tenets of progressive education, the program at

the Institute remained conservative. Gradually, theoretical reflections of psychoanalytic theory, social learning theory, cognitive/developmental theory, and ethology became evident and the school has been recognized for many years as a national resource. Now named the Shirley G. Moore Laboratory School, the unit enrolls 100 children each year randomly selected from a pool of applicants ranging from 2 to 5 years of age and reflecting the cultural and racial diversity of the Twin Cities.

Six women have served as principals (directors) of the nursery school. The founder, Margaret G. Wood, served for 1 year, followed by Josephine Foster, who directed the unit from 1926 until her death in 1941. She was succeeded by Elizabeth Fuller (1941–1956), Evelyn Helgerson (1956–1959), Shirley Moore (1960–1979), and Lynn Galle (1979 to present).

PARENT EDUCATION

Beginnings

In addition to the undergraduate courses, which were considered to be a contribution to the education of future parents as much as to their education in the arts and sciences, the parent education program over the years included the following: extension courses, correspondence courses (both credit and noncredit), study groups organized in cooperation with local agencies and led by Institute specialists, special lecture series, study courses offered in rural areas with the assistance of extension leaders, preparent courses offered in high schools throughout the state, radio programs, traveling libraries and exhibits, syndicated newspaper columns, communitywide parent education programs in Duluth and Brainerd, and popular articles in newspapers and magazines (143 were published in the first 9 years). Not counting the audiences reached through the media, these activities reached 89,871 persons, mostly women, between 1925 and 1939. In addition, a Parents' Consultation Service was begun in 1933 providing diagnosis and treatment of "adjustment difficulties among normal children as opposed to those reaching a pathological state" (Summary Annual Report, 1925–1939).

Parents' Magazine and the Institute

The "perils of popularization" are revealed in the story of the Institute's involvement with *Parents' Magazine* (Schlossman, 1985). This publication had its genesis in the mind of a creative but highly manipulative entrepreneur, George J. Hecht, whose own history consisted of running a family importing business and publishing, during the early 1920s, a little newspaper called *Better Times* that was well known to the New York intelli-

gentsia (Anonymous, 1975). Early in 1924, Hecht approached Beardsley Ruml with an idea for a magazine to be called *Children* that would contain scientifically based popular articles about child development and parenting. Start-up costs were needed. Would the LSRM be interested? Eventually, the president of the LSRM authorized discussions, stipulating that academic oversight of the eventual publication be assured.

Although acceptable academic oversight over the new *Children, a Magazine for Parents,* was never attained, Frank and Ruml eventually convinced Teachers College (Columbia University) to accept a $50,000 donation which, in turn, was exchanged by Hecht for stock in the new magazine. Once the Institute of Child Welfare was organized at Minnesota, similar participation (i.e., laundering) schemes were offered to that institution as well as to the Iowa Child Welfare Research Station and Gesell's Psychoclinic at Yale (also being supported by the LSRM). Consider, now, the conflicting interests of the parties: (a) Hecht wanted academic approbation for his magazine, financial support, *and* full managerial control of the enterprise; (b) the LSRM wanted to support a respectable mass communications effort in parent education but would not (could not) oversee it; and (c) the universities were suddenly endowed with venture capital (i.e., promised dividends) to accompany the grants-in-aid from the LSRM that they had already received.

The four presidents were each deeply skeptical of their involvement in a commercial dissemination effort when they knew the child development knowledge base was thin and the entrepreneur with whom they were dealing was slippery. Correspondence during the early 1930s between Walter Jessup (Iowa), Lotus Coffman (Minnesota), E. Gilmore (Columbia), and James Rowland Angell (Yale) reveals their worries about lack of proper representation on the board and lack of effective control over the contents of the magazine by their various institutions (UMNA). Nevertheless, they went ahead with the scheme. The University of Minnesota, in fact, increased its investment in the magazine when Mr. Hecht wrote to President Coffman requesting him to "ask whether the Fund would not buy 25 shares of Founders and 25 shares of Management stock ... and donate this to the University of Minnesota" (G. J. Hecht to L. D. Coffman, April 8, 1930, UMNA). President Coffman dutifully sent a request to Lawrence Frank 3 days later (UMNA), and the university ended up owning something more than one fourth of the corporation.

But embarrassing reactions to the magazine began to accumulate:

> My attention was called to the fact that the Parents' Magazine is published with the official cooperation of the University of Minnesota. After examining the magazine, I wondered first whether the authorities at the University of Minnesota know that the Parents' Magazine is published with their offi-

> cial cooperation; and second, if they do, whether they approve of the articles ... and of the general advertising policy of the magazine which seems a bit disreputable in spots. I note the names of some distinguished professors on the Advisory Board along with that of Dr. Dafoe of Dionne quintuplet fame. I do not find the name of Dr. Francis Townsend or Dizzy Dean, but doubtless this oversight will be corrected in due time....(Roscoe Pulliam, President, Illinois State Normal University to L. D. Coffman, August 6, 1936, UMNA)

Fights over board representation precipitated hastily called meetings of the four university presidents and rancorous correspondence between them and George Hecht. And even though an adequate representation plan was finally adopted, many important matters continued to be decided by Hecht himself.

But the final straw was a lawsuit initiated by Mr. Hecht against the National Congress of Parents and Teachers, arguing that its publication, *The National Parent Teacher Magazine,* infringed on his copyrights, which included "Children, the Magazine for Parents," "Children, the Parents' Magazine," and "The Parents' Magazine." Filed without the knowledge of the university co-owners of the magazine, the suit enraged Mary Langworthy, the president of the PTA (Parent-Teacher Association), who wrote to Coffman on January 13, 1936 demanding to know the extent of the university's "official cooperation" in the publication in question (UMNA). Enraged in turn (and fearing for the long-standing relationship existing between the PTA and the Minnesota College of Education), President Coffman together with the other presidents tried to rein in the publisher (UMNA). But, by the end of the 1930s, the bad blood on all sides produced an offer from George Hecht to buy out the universities which, in turn, agreed to use the funds for child development activities. This endowment supplied half the money needed to construct an addition to the Child Development Building in 1969, and continues even now to supply a small discretionary income to the Institute of Child Development.

Later Developments

Doubts accumulated in many different quarters during the 1950s concerning the efficacy of parent education undertaken directly by highly trained university staff. Countrywide, the child development community was not convinced that the knowledge base was substantial enough for the broad range of activities mounted, and other models (e.g., the training of leadership personnel) were thought to be more cost-effective. Consequently, the parent education program at the Institute of Child Development was discontinued in 1960. The Parent Consultation Service became the Child De-

velopment Clinic, an adjunct of the clinical child psychology training program that was folded into the Psycho-educational Center in 1968 and eventually phased out altogether.

Meanwhile, beginning late in the 1960s, new applications of child development knowledge were being tested in Project Head Start and other early-intervention programs for young children. Parent involvement was a highly visible component of most of these programs. Recognizing that dissemination of information to parents remains highly sought-after within American society, the Institute reconfigured its efforts in the applied fields beginning in the early 1970s. CEED was one outgrowth of this reconfiguration, involving staff members from the Institute and six other departments of the university.

A PROGRAM NOT REALIZED: THE INFANT HOME

The original grant application submitted to the LSRM included a request for funds to establish an infant home in which six babies could be studied on a 24-hour basis, an idea that had been floated earlier by Lawrence Frank to Bird Baldwin at the ICWRS. In an addendum to the Minnesota application, Richard Elliott, chairman of the Department of Psychology, was especially enthusiastic: "A strong conviction is shared by these men [of psychology] that the research scope of this laboratory would be tremendously broadened by the opportunity to observe and control material at all times." Karl Lashley, a member of the department at the time, submitted elaborate plans that depended on having round-the-clock access to babies, including the measurement of "rate of development of motor coordinations, visual fixation, grasping, sucking, swallowing, righting movements, inhibition of grapsing [sic] reflexes, Babinski, etc.; the correlation of individual differences with degree of development in later ages" (LSRMA). It took John Anderson no more than 1 year to decide that the proposed facility was too expensive to be practical (AR, 1925-1926), and it was never brought up again.

FACILITIES

The Institute of Child Development has never been housed in a modern building. Space has been occupied successively in three adjacent turn-of-the-century buildings that are among the oldest in the university: First, a small sandstone structure was remodeled for the Institute in 1925 that was built in 1888 by the Student Christian Association (later the YMCA) over the Great Northern railroad tracks. Next, the research and administrative

offices moved in 1928 to the old Law Building (now Pattee Hall), an elegant red brick building constructed in 1889. Finally, the unit was consolidated in 1954 in an orange brick building at 51 E. River Road that was constructed in 1903 for the School of Mines, gutted by a fire in 1913, and rebuilt for the College of Education and the University High School in 1914. Successive faculties have tried again and again to reconfigure the space available in this building (laboratories remain largely as designed by John Anderson in 1954), and in the addition built in 1969 with funds supplied by the National Institute of Child Health and Human Development and the Parents Institute Fund.

CONCLUSION

John Anderson's legacy does not rest on his work as an empirical scientist. A relatively small number of empirical studies are listed in his bibliography even though he wrote a large number of reviews, commentaries, expositions, and books. He was an energetic administrator who believed earnestly in the social benefits to be gained through child development research and parent education. Pioneering in science and application was an endeavor he relished, and he took a long view concerning the social benefits to be achieved through these endeavors. Among his unpublished works is the text of a speech he gave on March 5, 1927, to the Midwest Conference on Education for Parenthood in Kansas City:

> There will be problems in the year 2000 and many of them. Mothers and fathers will still be worrying about the best procedure in particular situations, there will still be child study classes, there will still be health campaigns, and research and extension organizations will still be grinding merrily away. But, and this is the important point, the general level of the great majority of children will be healthier, and happier than they are now.
>
> While society through science will never quite catch up to the problems proposed in the home and by science itself, nevertheless, there will be a steady advance and an improvement of the general level. The problems of one generation will become the commonplaces of the next. (Anderson, 1927, pp. 3-4)

One does not know whether we have come as far as Anderson envisioned. Nevertheless, the Institute of Child Development has stayed the course. We see even more problems around us than the pioneers dreamed of. Social demand for the "new news" about children is insatiable. As a result, the unit will surely survive to observe its centennial. Will there be a second one?

SOURCE NOTES

Many of the documents cited in this manuscript are available only in institutional archives. Citations to documents located in the Laura Spelman Rockefeller Memorial Archives (Boxes 45-46) are marked LSRMA in the text; those located in the University of Minnesota Archives are marked UMNA; those located in the National Library of Medicine Archives are marked NLMA.

Annual reports were written for the Institute of Child Welfare between 1925 and 1939 and are available in both LSRMA and UMNA, along with a Summary Report, 1925-1939. In the text, these documents are marked either AR with the year or SR and the year. Annual reports for the Institute of Child Development were also written between 1963 and 1990; biennial reports thereafter. These volumes are available in the Institute of Child Development, University of Minnesota. Other documents in the Institute that we used in writing this account are the printed budgets (1925-1967), and the Annotated Bibliographies of Publications of the Institute of Child Welfare (Development) that were issued at intervals from 1934 until 1970 (marked ABP in the text). Finally, we are grateful for the use of several Oral Histories from the archives of the Society for Research in Child Development, Ann Arbor, Michigan.

ACKNOWLEDGMENTS

The authors wish to thank Penelope Krosch, University of Minnesota Archives, Erwin Levold, Laura Spelman Rockefeller Memorial Archives, and Alice B. Smuts, University of Michigan, for their assistance.

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