

# **Nature *and* Nurture**

The Complex Interplay of Genetic  
and Environmental Influences  
on Human Behavior and Development

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## Genes and the Promotion of Positive Human Development: Hereditarian Versus Developmental Systems Perspectives

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Many experimental biologists outside of the biomedical-industrial complex are just now coming (back) to grips with the facts of epigenesis; with the profound mystery that developmental biology is, with the poverty of gene programs as an explanatory device; and with a crisis defined by the realization that an increasingly deficient theory of developmental genetics is the *only* theory currently available. The question remains: if biologists are starting to learn this lesson, will the psychologists be far behind?

—Richard C. Strohman (1993a, p. 101)

Genes are part of the developmental system in the same sense as other components (cell, tissue, organism), so genes must be susceptible to influence from other levels during the process of individual development.

—Gilbert Gottlieb (1992, p. 167).

Contemporary theories of human development are predicated on dynamic, relational, and systems perspectives (Lerner, 1998a, 1998b). The complexity of these theories can be daunting to scholars, both in regard to the conceptual difficulties involved in integratively understanding the multiple levels of organization fused

within the developmental system and in respect to the methodological challenges involved in using such theories as a frame for research.

If challenging to scholars, such theories are often seen as virtually impossible to grasp by nonspecialists, for instance, by the "Person in the Street" (to use the term suggested by Horowitz, 2000, p. 8), by media representatives, or by policy makers who influence the allocation of funds to programs aimed at promoting health and human development. These groups may gravitate to "single-variable stories" (Horowitz, 2000, p. 3) about human development—such as "genes cause behavior" (e.g., see Rushton, 1999)—to understand, communicate about, or support policies and programs to improve people's lives, respectively.

Such a simplistic—indeed a distortingly simplistic—alternative to developmental systems theories of human development is embodied in hereditarian views of behavioral development, that is, views that "split" nature from its relation to nurture (Overton, 1998) and that reduce the complexity of the human developmental system to mechanistically acting genetic determinants (e.g., Plomin, 1986, 2000; Rowe, 1994; Rushton, 1999, 2000). Fields such as human sociology and behavior genetics are examples of such hereditarian positions. However, because it is often the case that sociobiologists (e.g., Rushton, 1999, 2000; Wilson, 1980) claim that data derived from behavior genetics research pertinent to the concept of heritability provide key evidence in support of the validity of their ideas, behavior genetics constitutes an important sample case for the evaluation of hereditarian thinking. Accordingly, we may note the observations of Horowitz (2000) in regard to the behavior genetics approach to theory and research about human development. Horowitz (2000, p. 3) indicated:

Against the media popularity of single-variable stories, the science itself is moving inexorably toward greater and greater data-driven, integrative theoretical complexity. An exception to this is behavioral genetics. In contrast to the dynamic nonlinear interactive models full of reciprocity between and among levels and variables, behavioral genetics presents a relatively non-dynamic linear additive model that tries to assign percentages of variance in behavior and development that can be attributed to genes. The enterprise rests on the assumption that genetic influence can be expressed as a value accounting for a portion of the variance in a nondynamic linear equation for predicting behavioral functioning and furthermore, that the individual experiences of shared and nonshared environments can be assessed inferentially by the degree of biological relatedness of individuals without empirical observations of experience (Hoffman, 1991; Horowitz 1993, p. 3).

Behavioral genetics involves a relatively simplistic approach when compared with the kinds of dynamic systems theories currently being elaborated. Perhaps that is why, in the mode of wanting simple answers to simple questions, behavior genetic reports are so media attracting.

What then is the view of human development presented by behavior genetics? Why is this view not a viable, nature alternative to dynamic and integrative developmental system conceptions of human development? What is the frame offered

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by behavior genetics for application to public policies and social programs aimed at reducing or preventing the problems of young people or promoting their positive development? And, if behavior genetics fails as a useful model for understanding human development, what potential harm is done to the children, adolescents, and families of our nation if this instance of hereditarian thinking influences policies and programs? Can potential harm be counteracted by forwarding a developmental systems perspective as a frame for applications to policies and programs? To address these questions, it is useful to define the field of behavior genetics.

### DEFINITION OF THE FIELD OF BEHAVIOR GENETICS

According to Robert Plomin (2000), a prolific contributor to the behavior genetics literature, "Behavioural genetics is the genetic study of behaviour, which includes quantitative genetics (twin and adoption studies) as well as molecular genetics (DNA studies) of human and animal behaviour broadly defined to include responses of the organism from responses measured in the brain such as functional neuroimaging to self-report questionnaires" (p. 30). Plomin, DeFries, and McClearn (1980) indicate that "behavioral genetics lies at the interface between genetics and the behavioral sciences" (p. 12), and Plomin (1986) notes that "behavioral geneticists explore the etiology of individuality, differences among individuals in a population (p. 5). He further explains that "the three basic methods used in human behavioral genetics are family, twin, and adoption studies" (p. 11).

Across all these methods, the goal of behavior genetic analysis is to separate (partition) the variation in a distribution of scores (e.g., for a personality trait, temperamental characteristic, or intelligence) into the proportion due to genes and the proportion due to the environment. Although behavior geneticists admit that genes and environments may be correlated and/or may interact, they most typically seek to compute a score (termed a "heritability coefficient") that in its most frequently used form denotes the independent contribution of genetic variance to the overall differences in the distribution of scores for a given individual characteristic.

For such heritability scores to be meaningful, the methodologies of behavior genetics rest on a model of gene function that sees as possible genetic contributions that are independent of (not correlated or interactive with) the context within which genes exist. Genes, however, do not work in the way that behavior geneticists imagine.

### Fatal Flaws in the Behavior Genetics Model of Gene Function

As illustrated in the epigraphs by Strohman (1993a) and Gottlieb (1992), as well as in the writings of other molecular geneticists (e.g., Elman, et al., 1998; Ho, 1984; Muller-Hill, 1988; Venter, et al., 2001) and cell biologists (McF., 1997, 1998, 1999; Meaney, Aitken, Berkel, Bhattacharya, & Sapolsky, 1988) more generally, mo-

lecular biologists do not place credence in the model of genetic function involved in behavioral genetics. In fact, Venter and his colleagues (2001), the group that successfully mapped the sequence of the human genome, emphasize that there are two conceptual errors that should not be made in the face of the advances they and other scientists are making in understanding the structure and functional consequences of the human genome. They stressed:

There are two fallacies to be avoided: determinism, the idea that all characteristics of the person are "hard-wired" by the genome; and reductionism, the view that with complete knowledge of the human genome sequence, it is only a matter of time before our understanding of gene functions and interactions will provide a complete causal description of human variability. (p. 1348)

Contemporary thought in molecular genetics thus rejects the idea that genes are structures that act on supragenetic levels; instead, these scientists adopt the dynamic, developmental systems view noted in the epigraph by Gottlieb (1992; see also Ford & Lerner, 1992; Lerner, 1998b; Lewis, 1997; Magnusson, 1990, 1995, 1996, 1999a, 1999b; Magnusson & Stattin, 1998; Smith & Thelen, 1993; Thelen & Smith, 1994, 1998). This view emphasizes the integration—or fusion—of genes with the other levels of organization that comprise the person and his or her context. In such dynamic systems, the specific features of the interactions of the processes associated with these multiple levels create both the individuality of behavior at any point in time and the integrated character of human functioning that gives behavior its generality and cross-time predictability (Lerner, 1978; Smith & Thelen, 1993; Thelen & Smith, 1998).

In essence, then, we have in the field of behavior genetics (e.g., Plomin, 1986, 2000; Rowe, 1994) the use of a model of genetic structure and function that is specifically rejected by those scientists who study the action of genes directly. This rejection occurs because the field of behavioral genetics not only employs a counterfactual and scientifically atavistic conception of the role of genes in human development (Elman, et al., 1998; Ho, 1984; Strohmman, 1993a, 1993b; Venter, et al., 2001) but also because behavior genetics is a viewpoint with a conceptually flawed and empirically deficient view of developmental process and, as well, involves the conflation of description and explanation.

For instance, in regard to process, the structural account of genetic action behavior genetics offer suffers from the flaws of all structural accounts of development: that is, as explained by Thelen and Smith (1994, 1998; Smith & Thelen, 1993), such conceptions are inherently incomplete. These views do not explain individual behavioral performance (actions), other than to express empirically unsubstantiated confidence that in some way genetic structures translate—through the levels of cells, tissues, organs, the individual, and his or her actual context—into real-time actions.

For example, without any specification of the pathways of influence from genes to behaviors, Rowe (1994) asserted:

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Genes can produce dispositions, tendencies, and inclinations, because people with subtly different nervous systems are differently motivated ... [and] given enough environmental opportunities [for selection of environments], the ones chosen are those most reinforcing for a particular nervous system created by a particular genotype ... the direction of the growth curve of development, and the limit ultimately attained, is set in the genes. (p. 91)

Strohmman  
Version

However, because behavior geneticists believe that genetic structure transcends and is independent of real-time actions, an adequate, empirically verifiable account of actual individual-in-context behavior is beyond theoretical range (Smith & Thelen, 1993). Moreover, because of the inability to explain individual performance of actual individual-in-context behavior, behavior genetics, like other structural theories (Smith & Thelen, 1993), cannot explain the global order of behavior or developmental change itself.

In turn, in regard to the conflation of description and explanation, behavior genetics describes variability in trait distributions in a specific sample and then explains the distribution it has observed by reference to a label. It has applied to one (or the other) of the sources of the variability—genes or environment. Not only is this reification an instance of the nominal fallacy, but—to paraphrase the parody of structural explanations presented by Smith and Thelen (1993, p. 159)—the cause of the distribution of interindividual differences in a trait distribution is merely an abstract description of the trait distribution itself. Behavior genetics describes the variability in a distribution, labels it with a fancy source term (i.e., heritability), and then imputes that there is a gene, or set of genes, that explains the distribution.

To illustrate, Rowe (1994) notes that "understanding the growth and development of a single individual has been confused with understanding the origin of different traits in a population" (p. 3). However, this confusion about the distinction between interindividual differences and intraindividual change, as well as the problem of the conflation of description and explanation, exists in behavior genetics. On the basis of heritability data, writers such as Rowe seamlessly slide from talking about descriptive sources of variation within a trait distribution into talking about the genetic basis of individual development, that is, about the "causal influence on such child outcomes as intelligence, personality, and psychopathology" (p. 1).

One key basis of the lack of an adequate treatment in behavior genetics of performance, of developmental sequence and process, as well as of the distinction between description and explanation, is that these conceptual problems are coupled in behavior genetics with a lack of an adequate theoretical understanding both of supragenetic intraorganism processes (Gottlieb, 1991a, 1991b, 1992, 1997; Gottlieb, Wahlsten, & Lickliter, 1998) and of extraorganism contextual or ecological processes (Bronfenbrenner & Ceci, 1994; Horowitz, 2000; Lewis, 1997; Magnusson, 1999a, 1999b; Sameroff, 1983; Thelen & Smith, 1998). Accordingly, behavior genetics fails to adequately measure the environment, or ecology (Hoffman, 1991) of human development. In short, to paraphrase Goldberger (1980), in his discussion of Hearnshaw's (1979) account of the scientific fraud be-

havior geneticist Cyril Burt perpetrated regarding the study of the heritability of intelligence, behavior geneticists have methods that give them a lot of numbers but very little sensible or useful data about human development.

## BEHAVIOR GENETICS AS THE EMPEROR'S NEW CLOTHES

That these egregious conceptual and methodological problems exist is not news, not even in psychology. Hirsch (e.g., 1970, 1976a, 1976b, 1990a, 1990b, 1997a, 1997b) has written repeatedly about these problems for about a quarter of a century, and Schneirla (1956, 1957), Kuo (1967, 1970, 1976), Lehman (1953, 1970), Tobach (1981; Tobach & Greenberg, 1984; Tobach & Schneirla, 1968), Gottlieb (1970, 1983, 1992), Bronfenbrenner (1979, 1989; Bronfenbrenner & Ceci, 1994), Collins, Maccoby, Steinberg, Hetherington, & Bornstein (2000), Ford and Lerner (1992), Horowitz, (1993, 2000), Lerner (1978, 1984, 1986, 1991), Lewis (1997); Magnusson (1999a, 1999b; Magnusson & Stattin, 1998), Overton (1998), and Thelen and Smith (1994, 1998; Smith & Thelen, 1993) have contributed consonant commentaries both prior to and during the period of Hirsch's still ongoing work.

Yet, despite this criticism by their colleagues in the field of psychology, as well as by the lack of credence given to behavior genetics by molecular geneticists—as well as by eminent population geneticists (e.g., Feldman & Lewontin, 1975) and evolutionary biologists (e.g., Gould, 1981, 1996)—many psychologists continue to act as if behavioral genetics provides evidence for the inheritance of behaviors as varied as intelligence (Jensen, 1969, 1998), parenting (Scarr, 1992), morality (Wilson, 1975), temperament (Buss & Plomin, 1984), television viewing (Plomin, Corley, DeFries, & Faulker, 1990), and even the role in human development of the environment (Harris, 1998; Plomin, 1986, 2000; Plomin & Daniels, 1987; Rowe, 1994)! It should be noted that "environment" is the too general, and now outmoded, term used by behavior geneticists to refer to the integrated, multilevel context, or the ecology, involved in the dynamic system of person-context relations that characterizes human development (e.g., Bronfenbrenner, 1979; Bronfenbrenner & Ceci, 1994; Bronfenbrenner & Morris, 1998; Thelen & Smith, 1998).

The breadth and depth of these continuing criticisms of behavior genetics have been somewhat invisible or, at least, ignored by Plomin (2000), who claimed that "The controversy that swirled around behavioural genetics research during the 1970s has largely faded. During the 1980s and especially during the 1990s, the behavioural sciences became much more accepting of genetic influence" (p. 30).

This view is wrong in at least two ways. First, the controversy regarding the legitimacy of behavioral genetics—both as a conceptual frame for understanding the role of genes in behavioral development and as a methodology for studying the role of genes in behavioral development—has not diminished at all. One need only note the controversy associated with the publication of *The Bell Curve* (Herrnstein & Murray, 1994; e.g., see Goldberger & Manski, 1995; Hirsch, 1997a) or the criticisms leveled at the hereditarian views of J. Philippe Rushton (1996, 1997a, 1997b, 1999; e.g., Lerner, 1992a, 2002), which rely heavily on information de-

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rived from behavior genetics, to recognize that Plomin's (2000) "declaration of victory" is an inadequate attempt to either ignore or deny the persisting flaws of behavior genetics theory and method identified by scientists from numerous disciplines (e.g., see the critiques published throughout the 1990s and into the twenty-first century by Collins, Maccoby, Steinberg, Hetherington, & Bornstein, 2000; Gottlieb, 1997; Hirsch, 1997a, 1997b; Horowitz, 1993, 2000; Lerner & von Eye, 1992; Lewontin, 2000; Peters, 1995; Strohmman, 1993a, 1993b; Winston, 1996, 1997a, 1997b).

To illustrate, in a critique of the explanatory model and method associated with behavior genetic analyses of parent behaviors and of the effects of parenting on child and adolescent development, Collins, et al. (2000) noted:

Large-scale societal factors, such as ethnicity or poverty, can influence group means in parenting behavior—and in the effects of parenting behaviors—in ways that are not revealed by studies of within group variability. In addition, highly heritable traits also can be highly malleable. Like traditional correlational research on parenting, therefore, commonly used behavior-genetic methods have provided an incomplete analysis of differences among individuals. (p. 220)

Accordingly, Collins, et al. (2000) concluded:

Whereas researchers using behavior-genetic paradigms imply determinism by heredity and correspondingly little parental influence (e.g., Rowe, 1994), contemporary evidence confirms that the expression of heritable traits depends, often strongly, on experience, including specific parental behaviors, as well as predispositions and age-related factors in the child. (p. 228)

Second, Plomin rewrites history by stating that it was not until the 1990s that behavioral science accepted the role of genes in behavioral development. For well more than a half century (e.g., Anastasi, 1958; Maier & Schneirla, 1935; Novikoff, 1945a, 1945b; Schneirla, 1956, 1957), genes have been accepted as part of the developmental system that propels human life across time. The issue is not the one that Plomin points to, then, that of accepting that genes are involved in development. Instead, the issue is how do genes contribute to development. Plomin's (2000) approach and that of other behavior geneticists (e.g., Rowe, 1994) involves a split, nature-reductionist treatment of this issue (Overton, 1998). Most contemporary developmental scientists take an integrated, relational developmental systems approach to the issue (Lerner, 1998a, 1998b; Overton, 1998).

In fact, Plomin (2000) conceptually approaches the vacuity of the behavior genetics approach, at least as it has been pursued through the twentieth century. Although he maintains that "twin and adoption research and genetic research using nonhuman animal models will continue to thrive" in the twenty-first century (p. 30), Plomin perhaps admits to the serious flaws in this approach to understanding the role of genes in behavioral development when he acknowledges that "the greatest need is for quantitative genetic research that goes beyond heritability, that is,

beyond asking whether and how much genetic factors are important in behavioral development (p. 31). Moreover, he then goes on to ask a series of important questions about the role of genes in behavioral development: "How do genetic effects unfold developmentally? What are the biological pathways between genes and behavior? How do nature and nurture interact and correlate?" (p. 31). Unfortunately, he seeks answers to these questions through the flawed model and methods of behavior genetics and never explores the potential usefulness of developmental systems approaches. Nevertheless, such exploration would be very useful because Plomin admits that it would be a major mistake:

To think that genes determine outcomes in a hard-wired, there's-nothing-we-can-do-about-it way. For thousands of rare single-gene disorders, such as the gene on chromosome 4 that causes Huntington's disease, genes do determine outcomes in this hard-wired way. However, behavioral disorders and dimensions are complex traits influenced by many genes as well as many environmental factors. For complex traits, genetic factors operate in a probabilistic fashion like risk factors rather than pre-determined programming. (p. 33)

Thus, ultimately, Plomin (2000) admits that a probabilistic, nature-nurture relation is involved in accounting for the role of genes in behavioral development. Still, his views about single-gene disorders reflect an ahistorical conception of such problems of human development. That is, in respect to other such single-gene disorders, for example, as involved with phenylketonuria (PKU), genetic research has found means to counteract the problems produced by the genetic inheritance and has thus shown that a hard-wired genetic influence is not that hard-wired after all (Scriver & Clow, 1980a, 1980b). As such, Plomin maintains a narrow view of the probabilistic developmental system; it apparently does not include the ingenuity of scholars who capitalize on the plasticity within the developmental system to demonstrate that what might seem to be hard-wired is in reality amenable to change as a consequence of the embeddedness of genes within a dynamic system. Nevertheless, in admitting to the importance of a probabilistic system in behavioral development, Plomin (2000) is in actuality defeating his own split approach to the nature and nurture of behavioral development.

Moreover, other scholars are not as convinced as is Plomin (2000) that the various methodologies he associates with behavior genetics will generate useful data. For example, Collins, et al. (2000) noted:

One criticism is that the assumptions, methods, and truncated samples used in behavior-genetic studies maximize the effects of heredity and features of the environment that are different for different children and minimize the effects of shared family environments ... A second criticism is that estimates of the relative contributions of environment and heredity vary greatly depending on the source of data ... heritability estimates vary considerably depending on the measures used to assess similarity between children or between parents and children ... The sizable variability in estimates of genetic and environmental contributions depending on the

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paradigms and measures used means that no firm conclusions can be drawn about the relative strength of these influences on development. (pp. 220-221)

Similarly, and again counter to Plomin's (2000) assertion that the controversy surrounding behavior genetics faded by the 1990s, Horowitz, in 2000, noted:

One sees increasing skepticism about what is to be learned from assigning variance percentages to genes ... The skepticism is informed by approaches that see genes, the central nervous system and other biological functions and variables as contributors to reciprocal, dynamic processes which can only be fully understood in relation to sociocultural environmental contexts. It is a perspective that is influenced by the impressive recent methodological and substantive advances in the neurosciences. (p. 3)

The cutting-edge study of the neurosciences within the developmental systems perspective noted by Horowitz (2000) is exemplified by the work of Suomi (1997, 2000; Bennett, et al., in press) who sought to identify how genes and context fuse within the developmental system. Because of the close genetic similarity of rhesus monkeys to humans, he studied such organisms as a means to provide a model for the investigation of this system. In one recent instance of this long-term research program, Suomi (2000; Bennett, et al., in press) found that young rhesus monkeys show individual differences in their emotional reactivity (or "temperament"). Some young monkeys are highly reactive; for example, they become quite excited and agitated when they experience environmental stress, for instance, separation from their mothers; other monkeys show low reactivity in such situations, for instance, they behave calmly in the face of such separation. Suomi (2000; Bennett, et al., in press) discovered that these individual differences in behavior are associated with different genetic inheritances related to the functioning of serotonin, a brain chemical involved in neurotransmission and linked to individual differences in such conditions as anxiety, depression, and impulsive violence.

Accordingly, to study the interrelation of serotonergic system genes and environmental influences on behavioral development, Suomi (2000; Bennett, et al., in press) placed high or low reactivity rhesus young with foster rhesus monkeys that were also either high or low in emotional reactivity. When young monkeys with the genetic inheritance marking high reactivity were reared for the first six months of life with a low reactivity mother, they developed normally and, despite their genes, did not show high reactivity even when removed from their foster mothers and placed in a group of peers and unknown adults. In fact, these monkeys showed a high level of social skill; for example, they took leadership positions in their group. However, when young monkeys with this same genetic marker for high reactivity were raised by high reactivity foster mothers, they did not fare well under stressful conditions and proved socially inept when placed in a new social group.

Moreover, Suomi (2000; Bennett, et al., in press) found that the interaction between the serotonin transporter genotype and early experience not only influences rhesus monkey behavior but, as well, brain chemistry regarding use of seroto-

min. Despite have a high reactivity genotype, the monkeys whose early life experiences were with the low reactivity foster mothers had brain chemistry that corresponded to monkeys with a low reactivity genotype. Accordingly, Suomi (2000, p. 31) concluded:

The recent findings that specific polymorphisms in the serotonin transporter gene are associated with different behavioral and biological outcomes for rhesus monkeys as a function of their early social rearing histories suggest that more complex gene-environment interactions actually are responsible for the phenomenon. It is hard to imagine that the situation would be any less complex for humans.

### Behavior Genetics Constitutes the Maintenance of a Scientific Fiction

Clearly, many human developmentalists do not believe the causal story line of behavior genetics. Nevertheless, research in behavior genetics—studies that, in effect, involve obtaining samples of people with differing degrees of biological relatedness and applying, typically, state-of-the-art measures of traits and inadequate measures of the ecology of human development (Bronfenbrenner & Ceci, 1994; Hoffman, 1991)—is well-funded and widely disseminated, both through articles in the best scientific journals and in books produced through excellent publication houses.

But, behavior genetics is really like the story of the emperor's new clothes. Despite the positive regard some researchers hold for this area, there is actually nothing there. The naked truth is that conceptual errors and misapplied models—no matter how often repeated or published—do not by dint of their numbers make for an adequate contribution to science. The conceptual problems of the split, nature-mechanistic model of human development of behavior genetics and the several limitations of the scientific methods it uses to try to support this model, for example, in regard to the computation and interpretation of heritability estimates, are well known (e.g., see Lerner, 2002, for a recent review) and will be discussed in other chapters in this book. Together, these sources indicate the technical (statistical) problems associated with heritability research and underscore the myriad conceptual and methodological shortcomings of behavior genetics and why any purported evidence it presents for the split, hereditarian view of behavior development is more apparent than real. Nevertheless, despite scientific limitations that should reduce a field to scholarly irrelevancy, behavior geneticists continue to pursue their "research," to obtain government and foundation funding for it, and to promulgate their ideas about the import of their work for public policies and social programs pertinent to young people and their families (e.g., Rowe, 1994; Rushton, 1999, 2000).

Such extensions of flawed ideas to the arena of public policy and social programs can be dangerous to human welfare, social justice, and civil society (Lerner, 1992a, 1992b, 2002). To illustrate, in the mid 1960s, T. C. Schneirla wrote about the social policy implications of Konrad Lorenz's hereditarian ideas about the ex-

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istence of a human instinct for aggression. In a review of Lorenz's *On Aggression* (1966), Schneirla (1966) wrote:

It is as heavy a responsibility to inform man about aggressive tendencies assumed to be present on an inborn basis as it is to inform him about "original sin," which Lorenz admits in effect. A corollary risk is advising societies to base their programs of social training on attempts to inhibit hypothetical innate aggressions, instead of continuing positive measures for constructive behavior. (p. 16)

Despite such risks, hereditarians adopting a behavior genetics approach to human behavior advise society about the social import of their counterfactual views of genetic contributions to development. It is important to illustrate and evaluate such applications.

### THE CONCEPTUAL FRAMES PROVIDED BY BEHAVIOR GENETICS AND DEVELOPMENTAL SYSTEMS FOR HUMAN DEVELOPMENT APPLICATIONS

The purported uses for applications to policies and programs pertinent to human development of behavior genetics rest on the secondary (or even epiphenomenal) role assigned within this perspective to the ecology of human development in contributing to the causal bases of individual functioning. If context is not of primary significance in the determination of behavior and development, then any policies or programs aimed at enhancing the context of human development (e.g., prevention programs pertinent to youth risk behaviors or policies aimed at changing the family, school, or community experiences of poor children to promote their positive life chances) would be at best of only secondary importance. If context is, however, not at all causal—or if purported contextual influences on human development are seen as only illusory or epiphenomenal influences that can be reduced completely to genetic influences (e.g., Rowe, 1994; Rushton, 1999, 2000)—then attempts to change the context to enhance human development are irrelevant, misguided, and wasteful exercises. They could be construed, in fact, as inhumane exercises that falsely elevated the hopes of people whose problematic plights were due not to their social circumstances (e.g., social injustice or the absence of opportunity, equity, or democracy) but rather to their fixed and immutable genetic inheritances.

Such a view of the impotence of the ecology of human development as a source of plasticity in behavior would result then in applications directed to the only causal source of variance in human development, that is, genetic ones. If the genes that caused the problems afflicting the human condition could not be changed through antenatal repair, then the only policies and programs that would make scientific and societal sense would be ones aimed at, in the short term, diminishing the chances of possessors of the problematic genes from reproducing and thus passing their affliction onto another generation (e.g., see Lorenz, 1940a,

1940b, 1943a, 1943b; cf. Gould, 1981, 1996; Lerner, 1992a; Proctor, 1988). The long-term, or final, policy or program solution would be the elimination of the genes from the human genetic pool. The view of context associated with hereditarian conceptions (e.g., see Rowe, 1994; Rushton, 1999, 2000) contrast significantly with the fused conception of person and context variables found in developmental systems perspectives about the bases of human development (Lerner, 2002).

### The Dynamic, Developmental Systems "Alternative" to Behavior Genetics

The developmental process envisioned in the dynamic developmental systems perspective stands in marked contrast to the hereditarian view of developmental process found in behavior genetics. As Gottlieb (1992) explained, in a developmental systems view of process, the key "conception is one of a totally interrelated, fully coactional system in which the activity of genes themselves can be affected through the cytoplasm of the cell by events originating at any other level in the system, including the external environment" (pp. 144–145). As such, Gottlieb (1997, 1997) and other developmental systems theorists (e.g., Thelen & Smith, 1998) emphasized that neither genes nor context by themselves cause development. The fusion among levels within the integrated developmental system means that relations among variables—not splits between nature and nurture—constitute the core of the developmental process.

Accordingly, although hereditarians argue that biological contributions are isomorphic with genetic influences (e.g., Rushton, 1999), this equivalence is not seen as veridical with reality from the perspective of developmental systems theory. For instance, although some hereditarians see constitutional variables (e.g., relating to brain volume, head size, size of reproductive organs, and stature) as all based on heredity (Rushton, 1999), within developmental systems:

"Constitutional" is not equivalent to "genetic," and purposely so. Constitutional includes the expressed functions of genes—which, in themselves require some environmental input—but constitutional includes the operations of the central nervous system and all the biological and environmental experiences that impact organismic functioning and that make constitutional variables part of the dynamic change across the life span as they affect the development of and the decline of behavior. (Horowitz, 2000, p. 8)

In short, developmental science and developmental scientists should stop engaging in the pursuit of theoretically anachronistic and counterfactual conceptions of gene function. Indeed, significant advances in the science of human development will rest upon embedding the study of genes within the multiple, integrated levels of organization comprising the dynamic developmental system of person-context relations.

As Thelen and Smith (1994, 1998; Smith & Thelen, 1993) noted, pursuing this dynamic interactionist, developmental systems perspective will surely be an ardu-

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ous path, one likely filled with conceptual and empirical difficulties, mistakes, and uncertainties. Nevertheless, there is more than sufficient reason to continue to pursue this approach to behavioral development.

First, the nature-mechanistic approach of behavior genetics fails completely as an adequate theoretical or empirical approach to understanding human development. Second, and to paraphrase the epigraph by Strohmman (1993a, p. 101) that opened this paper, we have no better option available than to pursue a dynamic developmental systems approach. And third, great progress is being made. To appraise this progress it is useful to return again to the ideas of Horowitz (2000).

#### The Contributions of Horowitz to Understanding the Importance of Developmental Systems Theories

Summarizing the status at the beginning of the twenty-first century of theory and research pertinent to developmental systems perspectives, Horowitz (2000) noted that there exists:

extremely important information about structural plasticity in neuro-psychological function. Most critically, this structural and functional plasticity across developmental time is being tied directly to the amplifications and constraints of the social/cultural contexts that determine the opportunities that children and adults have to experience and to learn. (p. 3)

To help frame these data, Horowitz (2000) introduced a model of the dynamic developmental system that she notes corresponds to those of other developmental systems theorists (e.g., Gottlieb, et al., 1998; Lerner, 1998a, 1998b, in press). As such, Horowitz indicated:

In this model, as in some of the others, the assumption is made (supported by data) that from the moment of conception development is influenced by constitutional, social, economic and cultural factors and that these factors, furthermore, continue in linear and nonlinear relationships, to affect development across the life span, with development broadly defined to accommodate both the increase and decrease in ability and function. (p. 4)

Moreover, in the context of presenting her model of the human developmental system, Horowitz (2000) compared the approach to developmental analysis represented by hereditarian approaches to behavior development, such as behavior genetics, with the approach pursued in the sorts of theories represented in her model. While recognizing the attractiveness to the "average person" and the media of the simplistic answers provided by nature-oriented theorists, Horowitz (2000) observed:

The conundrum for many is to explain the regularities of the postnatal emergence of the normal universal species-typical behaviors in each individual child despite the

seeming variations in the gross nature of environments. The nativists answer is recourse to instincts, to predetermined, architecturally and genetically driven explanations both for the species as a whole and for the individuals in particular (Chomsky, 1965; Pinker, 1994; Spelke, Breininiger, Macomber, & Jacobson, 1992; Spelke & Newport, 1998). To the Person in the Street these explanations seem to provide the simple answers to simple questions though the nativist position is by no means simplistic and the position is often supported by very interesting data.

The alternative view and, I believe, the more compelling view is to consider that within all the gross environmental variations there is present the essential minimal experience necessary for the acquisition—the learning—of the basic universal behaviors of our species. There is a growing agreement that universal behaviors and physical structures are not built into the organism but that humans are, at the very least, evolutionary primed to take advantage of the transactional opportunities provided by what Brandstätter (1998) sees as the universal physical and social ecologies available to all normal human organisms—the kinds of transactional opportunities so beautifully analyzed by Thelen and her colleagues with respect to early motor development (Thelen & Ulrich, 1991). As a result of these transactional experiences, the forms and function of the universal developmental domains are constructed, whether as described in Thelen's dynamic systems approach to motor development (Thelen & Smith, 1994; Thelen & Ulrich, 1991), or in Katherine Nelson's (1996) powerful analysis and synthesis of the role of language in cognitive development or in Kurt Fischer's notion of the "constructive web" and his attempts to document the linear and nonlinear mechanisms involved in the construction and development of the hierarchies of skills (Fischer, 1980; Fischer & Bidell, 1998). (p. 5)

In short, given the myriad theoretical and methodological problems associated with behavior genetics, little can be gained either for advancing the science of human development or for adequately informing or serving Horowitz's (2000) "Person in the Street" by continuing to invest resources in the behavior genetics approach. There seem to be compelling reasons to make human and financial investments elsewhere given, on one hand, the counterfactual view of genetic activity inherent in behavior genetics and the several insurmountable conceptual and computational problems involved in the derivation of heritability estimates and, on the other hand, the availability of the theoretically rich and empirically productive developmental systems alternative to hereditarian approaches such as behavior genetics.

I believe, then, that both science and society may be well served by embarking on the scholarly path envisioned by Horowitz (2000). To both enhance understanding of human development, and to best promote its healthy progression across ontogeny, we should begin to devote our theoretical and research efforts to the exploration of the dynamic developmental system depicted by her and others (e.g., Collins, et al., 2000; Ford & Lerner, 1992; Lerner, 1991, 1996, 1998, 1998a, 1998b, 2002; Levine & Fitzgerald, 1992; Lewis, 1997; Sameroff, 1983; Smith & Thelen, 1993; Thelen & Smith, 1994, 1998). Such an initiative would be important because the hereditarian and the developmental systems viewpoints have quite different implications for policies and programs pertinent to the promotion of positive human development.

## Contrasts Between Hereditarian and Developmental Systems Perspectives About Human Development Policies and Programs

Table 1.1 presents one view of the different implications of hereditarian and developmental systems ideas for promoting positive human development through policy and program initiatives. The table displays a 2x2 contingency table that contrasts (A) beliefs about whether the hereditarian, split conception is believed to be either (1) true or (2) false; and (B) public policy and social program implications that would be associated with the hereditarian split position were it in fact (1) true or (2) false under either of the two belief conditions involved in "A."

The format for this table was suggested by Jensen (1973) in a discussion of what he saw as the social and educational policy dangers that might arise from viewing his genetic differences hypothesis (Jensen, 1969) about the source of racial and socioeconomic status variation in IQ scores as false when it might in fact be true. The dangers Jensen (1973) saw are among those presented in cell A2, B2 of Table 1.1 (the cell that would be followed in the developmental systems perspective were believed to be true).

Table 1.1 displays as well the severe implications for the treatment of some people that would derive from policies and programs if the hereditarian position were accepted as true. These implications would occur whether it was in fact the case that the hereditarian position was veridical with reality. In turn, the table presents (in cell A2, B2) the implications for positive human development that would derive from policies and programs if the hereditarian viewpoint was generally accepted to be what it in fact is, that is, false, and if the developmental systems alternative was used instead as a frame for human development policies and programs.

Though I have argued that the hereditarian position is counterfactual, I have also acknowledged that beliefs about its falsity are not unanimous. Given the quite negative human development policy and program implications of the belief in the truth of a hereditarian position, it is important to do more than just (a) appreciate the contrasts between hereditarian and developmental systems perspectives in their respective visions for the sources of influence that may be engaged to improve human development, or (b) note that behavior genetics merges a counterfactual view of gene action with a naive and impoverished understanding of the ecology of human development. In addition, as Lewontin (1992) has cautioned, it is crucial for human welfare that scholars remain vigilant about the presentation of hereditarian ideas potentially pertinent to human development policies and programs and, as well, about the actual use of these ideas in policy statements and program recommendations. A key example of this need for vigilance occurs in regard to the book by David Rowe (1994), *The Limits of Family Influence: Genes, Experience, and Behavior*.

## ROWE'S HEREDITARIAN IDEAS ABOUT THE IRRELEVANCE OF CONTEXTUAL INFLUENCES ON HUMAN DEVELOPMENT

Rowe's (1994) central idea is that "broad differences in family environments, except those that are neglectful, abusive, or without opportunity—may exert little in-

TABLE 1.1  
Policy and Program Implications if Hereditarian "Split" Conception  
of Genes (A) Believed True or False; (B) In Fact True or False

B. Hereditarian "split" conception in fact:	
1. True	2. False
<ul style="list-style-type: none"> <li>* Repair inferior genotypes, making them equal to superior genotypes</li> <li>* Miscegenation laws</li> <li>* Restrictions of personal liberties of carriers of inferior genotypes (separation, discrimination, distinct social tracts)</li> <li>* Sterilization</li> <li>* Elimination of inferior genotypes from genetic pool</li> </ul>	<ul style="list-style-type: none"> <li>* Same as A1, B1</li> </ul>
1. True	
<ul style="list-style-type: none"> <li>* Wasteful and futile humanitarian policies</li> <li>* Wasteful and futile programs of equal opportunity, affirmative action, equity, and social justice</li> <li>* Policies and programs to quell social unrest because of unrequited aspirations of genetically constrained people</li> <li>* Deterioration of culture and destruction of civil society</li> </ul>	<ul style="list-style-type: none"> <li>* Equity, social justice, equal opportunity, affirmative action</li> <li>* Celebration of diversity</li> <li>* Universal participation in civic life</li> <li>* Democracy</li> <li>* Systems assessment and engagement</li> <li>* Civil society</li> </ul>
2. False	
A. Hereditarian "split" conception believed:	

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fluence on personality development over the life course" (p. 1). However, he also claims that his "book's thesis [is] shared family environments have little effect on developmental outcomes" (p. 4).

Thus, to Rowe (1994), it does not matter (except in extreme circumstances) whether family environments are, across children, different or common (i.e., shared). It is his thesis that in neither case does environment influence developmental outcomes (see also Scarr, 1992). There are several problems with this stance, ones prototypic of the behavior genetics approach to the environment.

A key problem is that Rowe (1994) fails to recognize that family influences can strongly influence individual development, perhaps especially when there is no variation in them, that is, when they are shared equally across children. Elder's (1974) classic work on the effects of family economic hardship on children developing during the Great Depression is one excellent case in point. Another is provided by the recent work of Conger, Elder, and their colleagues (e.g., Conger et al., 1991; Conger et al., 1992; Ge et al., 1992) linking family economic hardship to adolescent distress, adjustment, and problems of substance use. Yet, because such invariant ecological influences contribute nothing to the variance across children or adolescents, their effects may be underestimated—especially when the ANOVA analytic techniques used in behavior genetics are ill-advisedly employed to estimate environmental contributions—a point that Hebb (1970) and Feldman and Lewontin (1975) made.

Another example of the problematic view of the context of human development Rowe (1994) used occurs when he explains what he means by the term "socialization science." Here, Rowe appears to be referring to an area of social and behavioral science that is predicated on the view that characteristics of the ecology of human development, such as culture, socioeconomic status, family milieu, and nonnormative life events, may have some causal influence on human development. He claimed that socialization science "may miss entirely which experiences are influential for personality development, and in many cases these may be experiences we cannot grasp to change our children's lives" (p. 5).

Of course, causal influences may not be identified in a given study. But, such omission would seem to be especially likely when—as is typically the case in behavior genetics—the context of human development is represented by, or better reduced to, a single score on a personality or intelligence test. For instance, consider behavior genetic studies using adoption designs. Here, assessments of the relative contributions to child outcomes of (a) the conception through point-of-adoption family context provided by a biological parent versus (b) the family context of an adopting parent to child outcomes, are indexed by differences in the relations (e.g., by differences between correlations) in trait scores for the biological parent and child compared to the adopting parent and child (cf. Hoffman, 1991). To illustrate, in discussing how the influence of selective placement in adoptions can be understood, Rowe (1994, p. 39) noted that "its quantitative strength is the correlation between the trait as measured in the biological parent (usually the unwed mother of the adoptee) and as measured in the adoptive par-

ent." All contextual influences associated with families are reduced, therefore, to a single score, for example, to an IQ score.

Such adoption studies underestimate the possible contribution to the variance of context and of dynamic person-context relations, and, at the same time, overestimate the contribution of genetic variance because:

1. Variance due to intrauterine contextual influences (and typically also to post-birth, preadoption contextual influences) is not measured and is attributed instead to genetic variance; in fact, Rowe (1994) admits that "all the accidents of embryological development are unshared; they can affect siblings differently, because each child has a different birth history" (p. 34);
2. Data sets of trait correlations involving different degrees of biological family resemblance represent findings to-be-explained; the presence of correlations does not prove anything about the role of genes or of genetic differences in causing the correlations; and
3. The multiple levels of organization comprising the ecology of human development (Bronfenbrenner, 1979, 1989; Bronfenbrenner & Ceci, 1994) cannot even begin to be measured by a score for a single trait measure, or even by a multivariate array of scores from several trait measures. To the contrary, and as again illustrated by the work of Elder (1974) and Conger (e.g., Conger, et al., 1991, 1992; Ge, et al., 1992), multilevel-multivariate representations of the context are needed to adequately represent the ecology of human development.

The reason Rowe (1994) omitted these theoretical and empirical contributions to the study of the context of human development is that behavior genetics is theoretically anachronistic. In splitting genes from environment, in divorcing the genes from the developmental system in which they are fused (Gottlieb, 1997; Horowitz, 2000; Overton, 1998; Thelen & Smith, 1998), Rowe focused on the environment of human development though a lens that reduces the scope of his vision and has him looking at past theories instead of contemporary ones (Lerner, 1998a). That is, to Rowe (1994), the theories that are seen as relevant to the context, or ecology, of human development or, in his terms, that represent the breadth of "socialization science" are Freudian theory (pp. 9-10), early behaviorism (pp. 10-12), and social learning theory (pp. 12-13), as represented by two references to the work of Bandura (1965, 1971). Bandura's (1986) later work—which is dynamically interactional and developmental—is not mentioned.

Indeed, nowhere in Rowe's (1994) book, or in the more recent hereditarian books published by Rushton (1999, 2000), is there an appreciation of this complex ecology of human development or, even more surprising, of the major theoretical contributions that have occurred over about the last 30 years in the understanding of context in human development and of the dynamic systems linking the individual to his or her multilevel context (e.g., Bales, Lindenberger, & Staudinger, 1998; Bales, Staudinger, & Lindenberger, 1999; Brim & Kagan,

1980; Bronfenbrenner, 1979, 1989; Bronfenbrenner & Morris, 1998; Elder, 1980, 1998; Elder, Modell, & Parke, 1993; Ford & Lerner, 1992; Gottlieb, 1983, 1991a, 1991b, 1992, 1997; Horowitz, 2000; Lerner, 1978, 1986, 1991, 1996, 2002; Levine & Fitzgerald, 1992; Lewis, 1997; Magnusson, 1995, 1996, 1999a, 1999b; Sameroff, 1975, 1983; Thelen & Smith, 1998; Wapner & Demick, 1998).

Moreover, the absence of reference to dynamic developmental systems theory is a particularly striking omission, given Rowe's (1994) biological orientation and the fact that it was in biology (von Bertalanffy, 1933) and later in comparative psychology (e.g., Gottlieb, 1976, 1991, 1992; Kuo, 1976; Schneider, 1957; Tobach, 1971, 1981) where such perspectives had much of their genesis and (and continue to have) influence. The errors of scientific omission and commission in hereditarian positions, such as the one Rowe (1994) presented, have important implications for the ways in which such positions are applied to human development policies and programs. As I have suggested, these implications and their developmental systems alternatives are important to consider from the standpoints of both science and human welfare.

### Hereditarian Versus Developmental Systems "Pathways" From Science to Social Policy and Social Action

I have argued that the conceptualization of genes and, superordinately, of nature as separable from nurture, found within behavior genetics is known—at least among molecular geneticists and some developmental and/or comparative scientists—to be counterfactual. Yet, I have noted that the field associated with this conceptual mistake continues to flourish. Indeed, despite a continuing failure by hereditarians to demonstrate the scientific validity of their nature-mechanistic ideas, new versions of the same flawed ideas continue to arise and attract research funding and, as described by Horowitz (2000), public, political, and media attention (e.g., Herrnstein, 1971; Herrnstein & Murray, 1994; Jensen, 1969, 1998; Lorenz, 1943a, 1943b, 1965; Rushton, 1987, 1996, 1999, 2000; Wilson, 1975).

Scholars in the field of human development must, therefore, confront several questions as a consequence of this curious situation: How did the biological, social, and behavioral sciences that attempt to contribute to the understanding and enhancement of human behavior and development arrive at this point? Why do we not just declare that the "emperor has no clothes?" Why, instead, why do we award grants and journal space to work having this fatal conceptual flaw?

Most important, why do we allow such mistaken reductionistic and mechanistic thinking to influence both science policy and social policy? In turn, why do we not more generally embrace policies informed by the scientifically valid alternative, developmental systems models of the role of biology-context relations?

In response to these questions, I believe that we can acknowledge, on one hand, that behavior genetics *has* helped social and behavioral science recall that both biology and context must be considered in any adequate theory of human development. On the other hand, however, I believe it is appropriate at this point in

the history of the field of human development to reject the oversimplified and incorrect view of context and of biology, respectively, found in behavior genetics. We are at a point in the science of human development where we must move on to the more arduous task of understanding the integration of biological and contextual influences in terms of the developmental system of which they are a dynamic part. This change in scientific attention is important for reasons of both the production of adequate developmental scholarship and the generation of useful social policy.

To illustrate, I may note that for Rowe (1994), as for other behavior geneticists as well (e.g., Plomin, 1986, 2000), as long as variance can be partitioned, there is a belief that genes can be shown to give rise to any aspect of human functioning—even the environment in which the individuals—the “lumbering robots” (Dawkins, 1976)—housing the genes are embedded. For instance, Rowe (1994) asserted that “the measures we label as environmental (including such central ones as social class) may hide genetic variation” (p. 5). And how do genes create the environment? To Rowe, “the answer is that the genes may construct a nervous system—and that hormones and neurotransmitters may then motivate behaviors resulting in the dramatic redesign of an environment. The way a beaver will restructure its environment is as genetically shaped as its flat tail and keen hearing” (p. 90). Thus, Rowe’s (1994) answer, which is his description of a process termed “niche picking,” illustrates not only the mechanism and reductionism of behavior genetics but, as well, the acontextual, asysemic, superficial, and even magical, thinking about developmental process that exists within the field of behavior genetics.

Moreover, it is in the incautious dissemination of work based on such thinking wherein pernicious implications for social policy can arise (Lerner, 1992a, 1992b). Rowe (1994) argued: “My thesis here is that social class may capture not variation in rearing and environmental social background, but instead variation in genes. This idea returns genes to socialization science by a back door—by the very *variable (social class) thought to have liberated social science from hereditarian thinking*” (p. 135).

Rowe’s (1994) idea is redolent of the late-nineteenth- and early-twentieth-century social Darwinists in America and Europe (Proctor, 1988; Tobach, Giannatos, Topoff, & Gross, 1974). In particular, his idea is consistent with the thinking involved in the German racial hygiene movement during this period (Proctor, 1988). Here, writers such as Alfred Ploetz, Wilhelm Schallmayer, Karl Binding, and Alfred Hoche maintained that members of low socioeconomic status groups—among other weaker members of society (e.g., the chronically sick or the lame)—were in their respective societal niches because of the inheritance of particular (i.e., inferior) genes (Lerner, 1992a; Proctor, 1988).

At this point, however, similarity to the thinking of Rowe (1994) disappears. This is because these racial hygienists went on to recommend—in a manner consistent with Cells A1, B1 and A1, B2 of Table 1.1—that, if the overall health of society were

to be improved, then policies must be instituted to rid society of these inferior genes. For instance, social programs and health care could be denied to the people possessing these genes and, as a result, they would have neither the economic, social, nor medical resources to long survive on their own (Proctor, 1988). As a consequence, it was thought that these policies would—in perhaps only a generation or two—eliminate poverty as well as weak, medically fragile, or handicapped persons. Simply, then, it was argued that the overall health of the German people would be enhanced because the carriers of the inferior genes would not be present to reproduce.

While again underscoring my belief that Rowe (1994) would find such policy recommendations reprehensible, my point is that the assertion that social class differences are due to genetic differences has been used in the past to justify horrible, and indeed criminal, social policies and political actions (Lifton, 1986; Müller-Hill, 1988; Proctor, 1988). Moreover, ideas about genetic differences may influence social policy today. For example, former NIMH Director Frederick K. Goodwin drew a link between violent behavior among nonhuman primates and the presence of violence among urban males and asserted that these youth have lost the social controls humans have had imposed by civilization over thousands of years of evolution (Psychological Science Agenda, 1992).

Goodwin’s thinking is not very distant from that of Rushton (1999, 2000), who proposed that the bases for what he believes are reproductive rate and associated behavioral differences among racial groups lies in the different reproductive strategies characterizing them. Rushton (1999, 2000) describes a continuum of reproductive strategies wherein “at one end of this scale are r-strategies that rely on high reproductive rates. At the other end are K-strategies that rely on high levels of parental care” (p. 24).

The different strategies depicted across this continuum are useful in biology to depict the reproductive rates of separate species (that are trying to survive and reproduce in diverse ecological niches; Johanson & Edey, 1981). For instance, a sponge, living and reproducing on the ocean floor, will produce literally thousands of offspring during a given reproductive cycle, and this rate will increase the probability of a few offspring withstanding the harsh currents and otherwise dangerous ecology of the ocean bottom for a period sufficient for their survival and eventual perpetuation of the species. In turn, given elephants’ enormous nutritional needs during their lengthy prenatal gestation period and postnatal years, the probability of offspring survival is enhanced when a small number, most typically one, offspring, is produced during a reproductive cycle.

Thus, the r-K distinction is useful for describing differences between species in how their rate of reproduction fits the ecological niche within which they live. However, there is no validity for applying this concept to differences within a species in the reproductive rates of different individuals or groups. Yet, this is an error that Rushton (1999) makes, and in fact admits that he does! He noted that the r-K “scale is generally used to compare the life histories of different species of animals. I have used it to explain the small but real differences between the human races” (p. 24).

Hence, Rushon (1999) misapplies the r-K distinction in two ways. First, he takes a concept that describes differences between species and applies it to differences within a species without any biological evidence of the validity of such an application. Nevertheless, without any documentation, Rushon (1999) asserts that, in response to the question of whether his r-K concept applies only to differences between species and not to within-species differences, "it applies to both" (p. 103).

Second, Rushon (1999) used a descriptive concept to explain differences within a species—and his explanation is that, basically, the group he called "Blacks" represent an evolutionarily less advanced form of organism, in that their reproductive strategy is more closely aligned with more primitive, r-like organisms. Indeed, Rushon used his r-K explanation to account for purported differences between "Orientals" and "Whites," who he claims are more "K-selected" and "Blacks," who he contends are more "r-selected," in their investment in their children.

He indicated that "highly K-selected men invest time and energy in their children rather than the pursuit of sexual thrills. They are 'dads' rather than 'cads'" (Rushon, 1999, p. 24). Moreover, Rushon (1999) asserted—without any citation whatsoever to bolster his statements—that "in Africa, the female-headed family is part of an overall social pattern. It consists of early sexual union and the procreation of children with many partners. It includes fostering children away from home, even for several years, so mothers remain sexually active .... In Black Africa and the Black Caribbean, as in the American underclass ghetto, groups of pre-teens and teenagers are left quite free of adult supervision" (pp. 35–36).

Amazingly, Rushon (1999) showed no awareness (e.g., through discussion or even mere citation) of the rich literature pertinent to the African American family (e.g., Demo, Allen, & Fine, 2000; McAdoo, 1977, 1991, 1993a, 1993b, 1995, 1998, 1999; McCubbin, Thompson, Thompson, & Furrell, 1998). This literature presents data providing a point-for-point contradiction of Rushon's undocumented assertions. Accordingly, when Rushon (1999) asserted that "scientists have a special duty to examine the facts and tell the truth" (p. 105) one may wonder if he included himself within the group held to this standard.

In any case, it seems clear, from the evaluations that have been made of the quality of the "data" Rushon forwarded regarding his ideas, that the "truth" is not being told by either the data he presented or the interpretations he made of his data (see Lerner, 2002, for a review). For example, Cernovsky (1997) noted that Rushon's studies of racial differences (e.g., Rushon, 1988a, 1988b, 1990a, 1990b, 1991a, 1991b, 1995) as well as those of other researchers working to support his findings (e.g., Lynn, 1993)

are noteworthy for their excessive reliance on very low correlation coefficients from obsolete data sets to postulate causal relationships. When a given method produces findings inconsistent with their ... views, they conveniently switch to a different method. An independent statistical re-examination of the same source of data by others may produce dramatically different results. (p. 1)

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To illustrate, Cernovsky and Liman (1993) reanalyzed the data that Rushon (1990) used to demonstrate that there were significant race differences (involving what Rushon termed "Mongoloid," "Caucasoid," and "Negroid" groups) across nations in crime rates (e.g., involving homicide, rape, and serious assault). The data, Rushon (1990a, 1990b) claimed, indicated that the Negroid group had higher rates of crime than did either of the other two groups. However, Cernovsky and Liman (1993) found that the race differences Rushon (1990a, 1990b) reported were not strong and, in fact, were largely weak and inconsistent. Not only does Rushon (1990a, 1990b) not present any evidence why these small differences among races should be considered genetic in origin, but in addition, Cernovsky and Liman (1993) found that in Rushon's own data reliance on race to predict an individual's likelihood of committing a crime "would result in an absurdly high rate (99.9%) of false positives" (p. 31).

Given the numerous dimensions of critical scientific problems associated with Rushon's work, we may agree with Cernovsky's (1995) view:

Although Rushon's writings and public speeches instill the vision of Blacks as small-brained, oversexed criminals who multiply at a fast rate and are afflicted with mental disease, his views are neither based on a bona fide scientific review of literature nor on contemporary scientific methodology. His dogma of bioevolutionary inferiority of Negroids is not supported by empirical evidence. (p. 677)

In sum, given the weak science that is associated with hereditarian position and the fact that, despite its limitations, this counterfactual view of human diversity and development finds its ways into the thinking of not only the "Person in the Street" but also media representatives and policy makers (and even science policy makers), it becomes necessary for scholars with understandings of human development distinct from hereditarian ones to integrate their roles as scientists and citizens. Indeed, and to note a point of agreement with Rowe (1994), scientists, as citizens, might serve both their science and society best by working with other sectors of their community "to try to understand how things really work and what levers for change may exist in them" (p. 224).

## CONCLUSIONS

To understand how things really work will require knowledge far beyond that which could be gained from partitioning variance into genetic and environmental components. We will need knowledge about all the levels of organization that comprise the ecology of human development and, as well, and perhaps most critical, about the dynamic system of developmental relations that comprise this ecology.

To obtain such knowledge, we must go beyond the limits of any one area of scholarship. Indeed, we will have to go beyond the limits of academe. How things really work in the real world involves people from all walks of life. In the end, then, each of our perspectives is limited. To effect important and sustained social

changes through our actions, communities of scholars in concert with communities of citizens will have to coalesce to learn how desired individual, family, and societal changes can be created.

In such efforts, we would do well to heed the advice of Horowitz (2000) in regard to how, in the face of the simplistically seductive ideas of hereditarianism, we must find the will to act in a manner supportive of social justice. She noted:

If we accept as a challenge the need to act with social responsibility then we must make sure that we do not use single-variable words like genes or the notion of innate in such a determinative manner as to give the impression that they constitute the simple answers to the simple questions asked by the Person in the Street lest we contribute to belief systems that will inform social policies that seek to limit experience and opportunity and, ultimately, development, especially when compounded by racism and poorly advantaged circumstances. Or, as Elman and Bates and their colleagues said in the concluding section of their book *Rethinking Innateness* (Elman et al., 1998), "If our careless, under-specified choice of words inadvertently does damage to future generations of children, we cannot turn with innocent outrage to the judge and say 'But your Honor, I didn't realize the word was loaded.'" (p. 8)

The challenge Horowitz articulated is one that is quite real for human development scientists who have been involved with trying to provide ideas and evidence countering the behavior genetics approach to human development. As I have emphasized already, I believe that we must pay heed to Lewontin's (1992) caution that the price society must pay for the continued presence of hereditarian conceptions is the need to remain vigilant about their appearance. We must be prepared to discuss the poor science they reflect and the inadequate bases they provide for public policy and applications pertinent to improving human life (see also Schneider, 1966; Tobach, 1994). We must be ready to suggest alternatives, such as developmental systems ones, to hereditarian views of research about and applications for human development.

Given the enormous, indeed historically unprecedented, challenges facing the families of America and the world, perhaps especially as they strive to raise healthy and successful children capable of leading civil society productively, responsibly, and morally across the twenty-first century (Benson, 1997; Damon, 1997; Lerner, 1995; Lerner, et al., 2000a, 2000b), there is no time to lose in the development of such a commitment by the scholarly community.

Colleagues involved in the developmental systems approach to understanding the role of genes in human development have an opportunity through their work to serve both scholarship and the communities, families, and people of our nation and world. By informing policies and programs sensitive to the diversity and richness of the dynamic relations between individuals and the ecology of human development, we demonstrate that nothing is of greater value to civil society than a science devoted to using its scholarship to improve the life chances of all people.

## REFERENCES

- Anastasi, A. (1958). Heredity, environment, and the question "how?" *Psychological Review*, 65, 197-208.
- Baltes, P. B., Lindenberger, U., & Staudinger, U. M. (1998). Life-span theory in developmental psychology. In W. Damon (Series Ed.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (5th ed., pp. 1029-1144). New York: Wiley.
- Baltes, P. B., Staudinger, U. M., & Lindenberger, U. (1999). Lifespan psychology: Theory and application to intellectual functioning. In J. T. Spence, J. M. Daxley, & D. J. Foss (Eds.), *Annual Review of Psychology* (Vol. 50, 471-507). Palo Alto, CA: Annual Reviews.
- Bandura, A. (1965). Influence of models' reinforcement of contingencies on the acquisition of imitative responses. *Journal of Personality and Social Psychology*, 1, 589-595.
- Bandura, A. (1971). *Social learning theory*. Morristown, NJ: General Learning Press.
- Bandura, A. (1986). *Social foundations of thought and action: A social cognitive theory*. Englewood Cliffs, NJ: Prentice-Hall.
- Bennett, A. J., Lesch, K. P., Heils, A., Long, J., Lorenz, J., Shoaf, S. E., Champoux, M., Suomi, S. J., Linnoila, M., & Higley, J. D. (in press). Serotonin transporter genotype and early experience interact to influence nonhuman primate CNS serotonin turnover. *Molecular Psychiatry*.
- Benson, P. (1997). *All kids are our kids: What communities must do to raise caring and responsible children and adolescents*. San Francisco: Jossey-Bass.
- Brandstädter, J. (1998). Action perspectives on human development. In W. Damon (Series Ed.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1. Theoretical models of human development* (5th ed., pp. 807-863). New York: Wiley.
- Brim, O. G., Jr., & Kagan, J. (Eds.). (1980). *Constancy and change in human development*. Cambridge, MA: Harvard University Press.
- Bronfenbrenner, U. (1979). *The ecology of human development: Experiments by nature and design*. Cambridge, MA: Harvard University Press.
- Bronfenbrenner, U. (1989). Ecological systems theory. In R. Vasta (Ed.), *Six theories of child development: Revised formulations and current issues* (pp. 185-246). Greenwich, CT: JAI Press.
- Bronfenbrenner, U., & Ceci, S. J. (1994). Nature-nurture reconceptualized in developmental perspective: A bioecological model. *Psychological Review*, 101, 568-586.
- Bronfenbrenner, U., & Morris, P. A. (1998). The ecology of developmental process. In W. Damon (Series Ed.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1. Theoretical models of human development* (5th ed., pp. 993-1028). New York: Wiley.
- Buss, A. H., & Plomin, R. (1984). *Temperament: Early developing personality traits*. Hillsdale, NJ: Lawrence Erlbaum Associates.
- Cernovsky, Z. Z. (1995). On the similarities of American blacks and whites. *Journal of Black Studies*, 25, 672-679.
- Cernovsky, Z. Z. (1997). *Statistical methods and behavioral similarities of blacks and whites*. Paper presented at the 58th Annual Convention of the Canadian Psychological Association, Toronto, Canada, June 12-14.
- Cernovsky, Z. Z., & Litman, L. C. (1993). Re-analyses of J. P. Rushton's crime data. *Canadian Journal of Criminology*, 35, 31-36.
- Chomsky, N. (1965). *Aspects of a theory of syntax*. Cambridge, MA: MIT Press.

- Collins, W. A., Maccoby, E. E., Steinberg, L., Hetherington, E. M., & Bornstein, M. H. (2000). Contemporary research on parenting: The case of nature and nurture. *American Psychologist*, 55, 218-232.
- Conger, R. D., Conger, K. J., Elder, G. H., Jr., Lorenz, F. O., Simons, R. L., & Whitbeck, L. B. (1992). A family process model of economic hardship and adjustment of early adolescent boys. *Child Development*, 63, 526-541.
- Conger, R. D., Lorenz, F. O., Elder, G. H., Jr., Melby, J. N., Simons, R. L., & Conger, K. J. (1991). A process model of family economic pressure and early adolescent alcohol use. *Journal of Early Adolescence*, 11, 430-449.
- Damon, W. (1997). *The youth charter: How communities can work together to raise standards for all our children*. New York: The Free Press.
- Dawkins, R. (1976). *The selfish gene*. New York: Oxford University.
- Demo, D. H., Allen, K. R., & Fine, M. A. (2000). *Handbook of family diversity*. New York: Oxford University Press.
- Elder, G. H., Jr. (1974). *Children of the Great Depression*. Chicago: University of Chicago Press.
- Elder, G. H., Jr. (1980). Adolescence in historical perspective. In J. Adelson (Ed.), *Handbooks of adolescent psychology* (pp. 3-46). New York: Wiley.
- Elder, G. H., Jr. (1998). The life course and human development. In W. Damon (Series Ed.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1. Theoretical models of human development* (5th ed., pp. 939-991). New York: Wiley.
- Elder, G. H., Jr., Modell, J., & Parke, R. D. (Eds.). (1993). *Children in time and place: Developmental and historical insights*. New York: Cambridge University Press.
- Elman, J. L., Bates, E. A., Johnson, M. H., Karmiloff-Smith, A., Parisi, D., & Plunkett, K. (1998). *Rethinking innateness: A connectionist perspective on development (neural network modeling and connectionism)*. Cambridge, MA: MIT Press.
- Feldman, M. W., & Lewontin, R. C. (1975). The heritability hang-up. *Science*, 190, 1163-1168.
- Fischer, K. W. (1980). A theory of cognitive development: The control and construction of hierarchies of skills. *Psychological Review*, 87, 477-531.
- Fischer, K. W., & Bidell, T. (1998). Dynamic development of psychological structures in action and thought. In W. Damon (Series Ed.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1. Theoretical models of human development* (5th ed., pp. 467-561). New York: Wiley.
- Ford, D. L., & Lerner, R. M. (1992). *Developmental systems theory: An integrative approach*. Newbury Park, CA: Sage.
- Ge, X., Conger, R. D., Lorenz, F. O., Elder, G. H., Montague, R. B., & Simons, R. L. (1992). Linking family economic hardship to adolescent distress. *Journal of Research on Adolescence*, 2, 351-378.
- Goldberger, A. S. (1980). Review of "Cyril Burt, Psychologist." *Challenge: The Magazine of Economic Affairs*, 23, 61-62.
- Goldberger, A. S., & Manski, C. F. (1995). [Review of the book *The Bell Curve*]. *Journal of Economic Literature*, 33, 762-776.
- Gottlieb, G. (1970). Conceptions of prenatal behavior. In L. R. Aronson, E. Tobach, D. S. Lehman, & J. S. Rosenblatt (Eds.), *Development and evolution of behavior: Essays in memory of T. C. Schneirla* (pp. 111-137). San Francisco: Freeman.
- Gottlieb, G. (1976). Conceptions of prenatal development: Behavioral embryology. *Psychological Review*, 83, 215-234.

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- Gottlieb, G. (1983). The psychobiological approach to developmental issues. In M. M. Haith & J. Campos (Eds.), *Handbook of child psychology: Infancy and biological bases* (Vol. 2, pp. 1-26). New York: Wiley.
- Gottlieb, G. (1991a). Experiential canalization of behavioral development: Theory. *Developmental Psychology*, 27, 4-13.
- Gottlieb, G. (1991b). Experiential canalization of behavioral development: Results. *Developmental Psychology*, 27, 39-42.
- Gottlieb, G. (1992). *Individual development and evolution: The genesis of novel behavior*. New York: Oxford University Press.
- Gottlieb, G. (1997). *Synthesizing nature-nurture: Prenatal roots of instinctive behavior*. Mahwah, NJ: Lawrence Erlbaum Associates.
- Gottlieb, G., Wahlsten, D., & Lickliter, R. (1998). The significance of biology for human development: A developmental psychobiological systems view. In W. Damon (Series Ed.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1. Theoretical models of human development* (5th ed., pp. 233-273). New York: Wiley.
- Gould, S. J. (1981/1996). *The mismeasure of man*. New York: Norton.
- Harris, J. R. (1998). *The nurture assumption: Why children turn out the way they do*. New York: Free Press.
- Hearnshaw, L. S. (1979). *Cyril Burt, psychologist*. New York: Cornell University Press.
- Hebb, D. O. (1970). A return to Jensen and his social critics. *American Psychologist*, 25, 568.
- Hernstein, R. J., & Murray, C. (1994). *The bell curve: Intelligence and class structure in American life*. New York: Free Press.
- Hernstein, R. J. (1971). IQ. *Atlantic Monthly*, September, 43-64.
- Hirsch, J. (1970). Behavior-genetic analysis and its biosocial consequences. *Seminars in Psychiatry*, 2, 89-105.
- Hirsch, J. (1976a). Jensenism: The bankruptcy of "science" without scholarship. *United States Congressional Record*, Vol. 122, no. 73, E2671-2; no. 74, E2693-5; no. 75, E2703-5, E2716-8, E2721-2.
- Hirsch, J. (1976b). Review of "Sociobiology," by E. O. Wilson. *Animal Behavior*, 24, 707-709.
- Hirsch, J. (1990a). Correlation, causation, and careerism. *European Bulletin of Cognitive Psychology*, 10, 647-652.
- Hirsch, J. (1990b). A nemesis for heritability estimation. *Behavioral and Brain Sciences*, 13, 137-138.
- Hirsch, J. (1997a). Some history of heredity-vs.-environment, genetic inferiority at Harvard (?), and The (incredible) Bell Curve. *Genetica*, 99, 207-224.
- Hirsch, J. (1997b). The triumph of wishful thinking over genetic irrelevance. *Current Psychology of Cognition*, 16, 711-720.
- Ho, M. W. (1984). Environment and heredity in development and evolution. In M. W. Ho & P. T. Saunders (Eds.), *Beyond neo-Darwinism: An introduction to the new evolutionary paradigm* (pp. 267-289). London: Academic Press.
- Hoffman, L. W. (1991). The influence of family environment on personality: Accounting for sibling differences. *Psychological Bulletin*, 110, 187-203.
- Horowitz, F. D. (1993). Bridging the gap between nature and nurture: A conceptually flawed issue and the need for a comprehensive and new environmentalism. In R. Plomin & G. E. McClearn (Eds.), *Nature, nurture and psychology* (pp. 341-354). Washington, D.C.: APA Books.

- Horowitz, F. D. (2000). Child development and the PITS: Simple questions, complex answers, and developmental theory. *Child Development*, 71, 1-10.
- Jensen, A. R. (1969). How much can we boost IQ and scholastic achievement? *Harvard Educational Review*, 39, 1-123.
- Jensen, A. R. (1973). *Educability and group differences*. New York: Harper & Row.
- Jensen, A. R. (1998). Jensen on "Jensenism." *Intelligence*, 26, 181-208.
- Johanson, D. C., & Edey, M. A. (1981). *Lucy: The beginnings of humankind*. New York: Simon & Schuster.
- Kuo, Z.-Y. (1967). *The dynamics of behavior development*. New York: Random House.
- Kuo, Z.-Y. (1970). The need for coordinated efforts in developmental studies. In A. Aronson, E. Tobach, D. S. Lehman, & J. S. Rosenblatt (Eds.), *Development and evolution of behavior: Essays in memory of T. C. Schneirla* (pp. 181-193). San Francisco: W. H. Freeman.
- Kuo, Z.-Y. (1976). *The dynamics of behavior development: An epigenetic view*. New York: Plenum.
- Lehman, D. S. (1953). A critique of Konrad Lorenz's theory of instinctive behavior. *Quarterly Review of Biology*, 28, 337-363.
- Lehman, D. S. (1970). Semantic and conceptual issues in the nature-nurture problem. In L. R. Aronson, E. Tobach, D. S. Lehman, & J. S. Rosenblatt (Eds.), *Development and evolution of behavior: Essays in memory of T. C. Schneirla* (pp. 17-52). San Francisco: Freeman.
- Lerner, R. M. (1978). Nature, nurture, and dynamic interactionism. *Human Development*, 21, 1-20.
- Lerner, R. M. (1984). *On the nature of human plasticity*. New York: Cambridge University Press.
- Lerner, R. M. (1986). *Concepts and theories of human development* (2nd ed.). New York: Random House.
- Lerner, R. M. (1991). Changing organism-context relations as the basic process of development: A developmental contextual perspective. *Developmental Psychology*, 27, 27-32.
- Lerner, R. M. (1992a). *Final solutions: Biology, prejudice, and genocide*. University Park: Pennsylvania State University Press.
- Lerner, R. M. (1992b). Nature, nurture and mass murder. *Readings: A Journal of Reviews and Commentary on Mental Health*, 7, 8-15.
- Lerner, R. M. (1995). *America's youth in crisis: Challenges and options for programs and policies*. Thousand Oaks, CA: Sage.
- Lerner, R. M. (1996). Relative plasticity, integration, temporality, and diversity in human development: A developmental contextual perspective about theory, process, and method. *Developmental Psychology*, 32, 781-786.
- Lerner, R. M. (Ed.). (1998a). *Handbook of child psychology: Vol. 1. Theoretical models of human development* (5th ed.). Damon. New York: Wiley.
- Lerner, R. M. (1998b). Theories of human development: Contemporary perspectives. In R. M. Lerner (Ed.), *Handbook of child psychology: Vol. 1. Theoretical models of human development* (5th ed., pp. 1-24). New York: Wiley.
- Lerner, R. M. (2002). *Concepts and theories of human development* (3rd ed.). Mahwah, NJ: Lawrence Erlbaum Associates.
- Lerner, R. M., Fisher, C. B., & Weinberg, R. A. (2000a). Toward a science for and of the people: Promoting civil society through the application of developmental science. *Child Development*, 71, 11-20.

- Lerner, R. M., Fisher, C. B., & Weinberg, R. A. (2000b). Applying developmental science in the twenty-first century: International scholarship for our times. *International Journal of Behavioral Development*, 24, 24-29.
- Lerner, R. M., & von Eye, A. (1992). Sociobiology and human development: Arguments and evidence. *Human Development*, 35, 12-33.
- Levine, R. L., & Fitzgerald, H. E. (Eds.). (1992). *Analysis of dynamic psychological systems* (Vol. 1 & 2). New York: Plenum.
- Lewis, M. (1997). *Altering fate*. New York: Guilford Press.
- Lewontin, R. C. (1992). Foreword. In R. M. Lerner (Ed.), *Final solutions: Biology, prejudice, and genocide* (pp. vii-viii). University Park: Pennsylvania State University Press.
- Lewontin, R. C. (2000). *The triple helix*. Cambridge, MA: Harvard University Press.
- Lifton, R. J. (1986). *The Nazi doctors: Medical killing and the psychology of genocide*. New York: Basic Books.
- Lorenz, K. (1940a). Durch Domestikation verursachte Störungen arteigenen Verhaltens. *Zeitschrift für angewandte Psychologie und Charakterkunde*, 59, 2-81.
- Lorenz, K. (1940b). Systematik und Entwicklungsgedanke im Unterricht. *Der Biologe*, 9, 24-36.
- Lorenz, K. (1943a). Die angeborenen Formen möglicher Erfahrung. *Zeitschrift für Tierpsychologie*, 5, 235-409.
- Lorenz, K. (1943b). Psychologie und stammesgeschichte. In G. Heberer (Ed.), *Die evolution der organismen* (pp. 105-127). Jena, Germany: G. Fischer.
- Lorenz, K. (1965). *Evolution and modification of behavior*. Chicago: University of Chicago Press.
- Lorenz, K. (1966). *On aggression*. New York: Harcourt Brace.
- Lynn, R. (1993). Further evidence for the existence of race and sex differences in cranial capacity. *Social Behavior and Personality*, 21, 89-92.
- Magnusson, D. (1990). Personality development from an interactional perspective. In L. Pervin (Ed.), *Handbook of personality* (pp. 193-222). New York: Guilford Press.
- Magnusson, D. (1995). Individual development: A holistic integrated model. In P. Moen, G. H. Elder, & K. Lusher (Eds.), *Linking lives and contexts: Perspectives on the ecology of human development* (pp. 19-60). Washington, DC: APA Books.
- Magnusson, D. (1996). *The life-span development of individuals: Behavioral, neurobiological, and psychosocial perspectives. A synthesis*. Cambridge, UK: Cambridge University Press.
- Magnusson, D. (1999a). Holistic interactionism: A perspective for research on personality development. In L. A. Pervin & O. P. John (Eds.), *Handbook of personality: Theory and research* (2nd ed., pp. 219-247). New York: The Guilford Press.
- Magnusson, D. (1999b). On the individual: A person-oriented approach to developmental research. *European Psychologist*, 4, 205-218.
- Magnusson, D., & Stattin, H. (1998). Person-context interaction theories. In W. Damon. (Series Ed.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (5th ed., pp. 685-759). New York: Wiley.
- Maier, N. R. F., & Schneirla, T. C. (1935). *Principles of animal behavior*. New York: McGraw-Hill.
- McAdoo, H. P. (1977). A review of the literature related to family therapy in the Black community. *Journal of Contemporary Psychotherapy*, 9, 15-19.
- McAdoo, H. P. (1991). Family values and outcomes for children. *Journal of Negro Education*, 60, 361-365.

- McAdoo, H. P. (1993a). Family equality and ethnic diversity. In K. Altergott (Ed.), *One world, many families* (pp. 52-55). Minneapolis, MN: National Council on Family Relations.
- McAdoo, H. P. (1993b). The social cultural contexts of ecological developmental family models. In P. Boss, W. Doherty, & W. Schynum (Eds.), *Sourcebook of family theories and methods: A contextual approach* (pp. 298-301). New York: Plenum.
- McAdoo, H. P. (1995). Stress levels, family help patterns, and religiosity in middle- and working-class African American single mothers. *Journal of Black Psychology*, 21, 424-449.
- McAdoo, H. P. (1998). African American families: Strength and realities. In H. I. McCubbin, E. A. Thompson, A. I. Thompson, & J. E. Fromer (Eds.), *Resiliency in ethnic minority families: African American families* (pp. 17-30). Thousand Oaks, CA: Sage.
- McAdoo, H. P. (1999). Diverse children of color. In H. E. Fitzgerald, B. M. Lester, & B. S. Zuckerman (Eds.), *Children of color: Research, health, and policy issues* (pp. 205-218). New York: Garland Publishing.
- McCubbin, H. I., Thompson, E. A., Thompson, A. I., & Futrell, J. A. (Eds.). (1998). *Resiliency in ethnic minority families: African-American families*. Thousand Oaks, CA: Sage.
- McEwen, B. S. (1997). Possible mechanisms for atrophy of the human hippocampus. *Molecular Psychiatry*, 2, 255-262.
- McEwen, B. S. (1998). Protective and Damaging Effects of Stress Mediators. *New England Journal of Medicine*, 338, 171-179.
- McEwen, B. S. (1999). Stress and hippocampal plasticity. *Annual Review of Neuroscience*, 22, 105-122.
- Meaney, M., Aitken, D., Berkel, H., Bhattacharjee, S., & Sapolsky, R. (1988). Effect of neonatal handling of age-related impairments associated with the hippocampus. *Science*, 239, 766-768.
- Muller-Hill, B. (1988). Murderous science: Elimination by scientific selection of Jews, Gypsies, and others. Germany, 1933-1945 (G. R. Fraser, Trans.). New York: Oxford University.
- Nelson, K. (1996). *Language in cognitive development: Emergence of the mediated mind*. New York: Cambridge University Press.
- Novikoff, A. B. (1945a). The concept of integrative levels and biology. *Science*, 101, 209-215.
- Novikoff, A. B. (1945b). Continuity and discontinuity in evolution. *Science*, 101, 405-406.
- Overton, W. F. (1998). Developmental psychology: Philosophy, concepts, and methodology. In W. Damon (Series Ed.) & R. M. Lerner (Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (5th ed., pp. 107-187). New York: Wiley.
- Peters, M. (1995). Does brain size matter? A reply to Rushon and Ankney. *Canadian Journal of Experimental Psychology*, 47, 751-756.
- Pinker, S. (1994). *The language instinct*. New York: William Morrow.
- Plomin, R. (1986). *Development, genetics, and psychology*. Hillsdale, NJ: Lawrence Erlbaum Associates.
- Plomin, R. (2000). Behavioural genetics in the 21st century. *International Journal of Behavioral Development*, 24, 30-34.
- Plomin, R., Corley, R., DeFries, J. C., & Faulker, D. W. (1990). Individual differences in television viewing in early childhood: Nature as well as nurture. *Psychological Science*, 1, 371-377.

- Plomin, R., & Daniels, D. (1987). Why are children in the same family so different from each other? *Behavioral and Brain Sciences*, 10, 1-16.
- Plomin, R., DeFries, J. C., & McClearn, G. E. (1980). *Behavioral genetics: A primer*. San Francisco: Freeman.
- Proctor, R. N. (1988). *Racial hygiene: Medicine under the Nazis*. Cambridge, MA: Harvard University.
- Psychological Science Agenda (1992, May/June). APA, mental health community react to Goodwin's resignation (pp. 1, 8). American Psychological Association.
- Rowe, D. (1994). *The limits of family influence: Genes, experience, and behavior*. New York: Guilford Press.
- Rushon, J. P. (1987). An evolutionary theory of health, longevity, and personality: Sociobiology, and r/K reproductive strategies. *Psychological Reports*, 60, 539-549.
- Rushon, J. P. (1988a). Do r/K reproductive strategies apply to human differences? *Social Biology*, 35, 337-340.
- Rushon, J. P. (1988b). Race differences in behavior: A review and evolutionary analysis. *Personality and Individual Differences*, 9, 1009-1024.
- Rushon, J. P. (1990a). Sex, ethnicity, and hormones. *Behavioral & Brain Sciences*, 13, 194, 197-198.
- Rushon, J. P. (1990b). Why we should study race differences. *Psychologische Beitrage*, 32, 128-142.
- Rushon, J. P. (1991a). Do r-K strategies underlie human race differences? A reply to Weizmann et al. *Canadian Psychology*, 32, 29-42.
- Rushon, J. P. (1991b). Race, brain size, and intelligence: Another reply to Cernovsky. *Psychological Reports*, 66, 659-666.
- Rushon, J. P. (1995). *Race, evolution, and behavior*. New Brunswick, NJ: Transaction.
- Rushon, J. P. (1996). Political correctness and the study of racial differences. *Journal of Social Distress & the Homeless*, 5, 213-229.
- Rushon, J. P. (1997a). Cranial size and IQ in Asian Americans from birth to age seven. *Intelligence*, 25, 7-20.
- Rushon, J. P. (1997b). More on political correctness and race differences. *Journal of Social Distress and the Homeless*, 6, 195-198.
- Rushon, J. P. (1999/2000). *Race, evolution, and behavior* (Special Abridged Ed.). New Brunswick, NJ: Transaction Publishers.
- Sameroff, A. (1975). Transactional models in early social relations. *Human Development*, 18, 65-79.
- Sameroff, A. J. (1983). Developmental systems: Contexts and evolution. In W. Kessen (Ed.), *Handbook of child psychology: Vol. 1, History, theory, and methods* (pp. 237-294). New York: Wiley.
- Scarr, S. (1992). Developmental theories for the 1990s: Development and individual differences. *Child Development*, 63, 1-19.
- Schneirla, R. C. (1956). Interrelationships of the innate and the acquired in instinctive behavior. In P. P. Grassé (Ed.), *L'instinct dans le comportement des animaux et de l'homme*. Paris: Masson et Cie.
- Schneirla, R. C. (1957). The concept of development in comparative psychology. In D. B. Harris (Ed.), *The concept of development: An issue in the study of human behavior* (pp. 78-108). Minneapolis: University of Minnesota Press.
- Schneirla, R. C. (1966). Instinct and aggression: Reviews of Konrad Lorenz, *Evolution and modification of behavior* (Chicago: University of Chicago Press, 1965), and *On aggression* (New York: Harcourt Brace, 1966). *Natural History*, 75, 16.

- Scriven, C. R., & Clow, C. L. (1980a). Phenylketonuria: Epitome of human biochemical genetics (first of two parts). *New England Journal of Medicine*, 303, 1336-1342.
- Scriven, C. R., & Clow, C. L. (1980b). Phenylketonuria: Epitome of human biochemical genetics (second of two parts). *New England Journal of Medicine*, 303, 1394-1400.
- Smith, L. B., & Thelen, E. (Eds.). (1993). *A dynamic systems approach to development: Applications*. Cambridge, MA: MIT Press.
- Speike, E. S., Breilinger, K., Macomber, J., & Jacobson, K. (1992). Origin of knowledge. *Psychological Review*, 99, 605-632.
- Speike, E. S., & Newport, E. L. (1998). Nativism, empiricism, and the development of knowledge. In W. Damon (Series Ed.) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1. Theoretical models of human development* (5th ed., 275-340). New York: Wiley.
- Strohman, R. C. (1993a). Organism and experience. [Review of the book *Final Solutions*]. *Journal of Applied Developmental Psychology*, 14, 147-151.
- Strohman, R. C. (1993b). Book Reviews: *Final Solutions*, Richard M. Lerner, Pennsylvania State University Press, 1992, and *Individual Development and Evolution: The genesis of novel behavior*, Gilbert Gottlieb, Oxford University Press, 1992. *Integrative Physiological and Behavioral Science*, 28, 99-104.
- Suomi, S. J. (1997). Early determinants of behavior: Evidence from primate studies. *British Medical Bulletin*, 53, 170-184.
- Suomi, S. J. (2000). A behavioral perspective on developmental psychopathology: Excessive aggression and serotonergic dysfunction in monkeys. In A. J. Sameroff, M. Lewis, & S. Miller (Eds.), *Handbook of developmental psychopathology* (2nd ed., pp. 237-256). New York: Plenum Press.
- Thelen, E., & Smith, L. B. (1994). *A dynamic systems approach to the development of cognition and action*. Cambridge, MA: MIT Press.
- Thelen, E., & Smith, L. B. (1998). Dynamic systems theories. In W. Damon (Series Editor) & R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1 Theoretical models of human development* (5th ed., pp. 563-633). New York: Wiley.
- Thelen, E., & Ulrich, B. D. (1991). Hidden skills: A dynamical systems analysis of treadmill stepping during the first year. *Monographs of the Society for Research in Child Development*, 56 (1), 104.
- Tobach, E. (1978). The methodology of sociobiology from the viewpoint of a comparative psychologist. In A. L. Caplan (Ed.), *The sociobiology debate* (pp. 411-423). New York: Harper & Row.
- Tobach, E. (1971). Some evolutionary aspects of human gender. *Journal of Orthopsychiatry*, 41, 710-715.
- Tobach, E. (1981). Evolutionary aspects of the activity of the organism and its development. In R. M. Lerner & N. A. Busch-Rossnagel (Eds.), *Individuals as producers of their development: A life-span perspective* (pp. 37-68). New York: Academic Press.
- Tobach, E. (1994). ... Personal is political is personal is political .... *Journal of Social Issues*, 50, 221-224.
- Tobach, E., Giannetos, J., Topoff, H. R., & Gross, C. G. (1974). *The four horses: Racism, sexism, militarism, and social Darwinism*. New York: Behavioral publications.
- Tobach, E., & Greenberg, G. (1984). The significance of T. C. Schneirla's contribution to the concept of levels of integration. In G. Greenberg & E. Tobach (Eds.), *Behavioral evolution and integrative levels* (pp. 1-7). Hillsdale, NJ: Lawrence Erlbaum Associates.

- Tobach, E., & Schneirla, T. C. (1968). The biopsychology of social behavior of animals. In R. E. Cooke & S. Levin (Eds.), *Biologic basis of pediatric practice* (pp. 68-82). New York: McGraw-Hill.
- Venter, J. C., Adams, M. D., Myers, E. W., Li, P. W., Mural, R. J., et al. (2001). The sequence of the human genome. *Science*, 291, 1304-1351.
- von Bertalanffy, L. (1933). *Modern theories of development*. London: Oxford University Press.
- Wagner, S., & Demick, J. (1998). Developmental analysis: A holistic, developmental, systems-oriented perspective. In W. Damon (Series Ed.) and R. M. Lerner (Vol. Ed.), *Handbook of child psychology: Vol. 1. Theoretical models of human development* (5th ed., pp. 761-805). New York: Wiley.
- Wilson, E. O. (1975). *Sociobiology: The new synthesis*. Cambridge, MA: Harvard University Press.
- Wilson, E. O. (1980). A consideration of the genetic foundation of human social behavior. In G. W. Barlow & J. Silverberg (Eds.), *Sociobiology: Beyond nature/nurture* (pp. 295-305). Boulder, CO: Westview.
- Winston, A. S. (1996). The context of correctness: A comment on Rushon. *Journal of Social Distress and the Homeless*, 5, 231-250.
- Winston, A. S. (1997a). Genocide as a scientific project. *American Psychologist*, 52, 182-183.
- Winston, A. S. (1997b). Rushon and racial differences: Further reasons for caution. *Journal of Social Distress and the Homeless*, 6, 199-202.