eScholarship
International Journal of Comparative Psychology

Title

Deja Vu, Dialectics, and the Constancies of Controversies Involving the Nature-Nuture Issue: On Reviews of Final Solutions: Biology, Prejudice, and Genocide

Permalink
https://escholarship.org/uc/item/14v67694

Journal
International Journal of Comparative Psychology, 6(3)

ISSN
0889-3675

Author
Lerner, Richard M

Publication Date
1993

DOI
10.46867/C4QC7G

Copyright Information
Copyright 1993 by the author(s).This work is made available under the terms of a Creative Commons Attribution License, available at https://creativecommons.org/licenses/by/4.0/

Peer reviewed

eScholarship.org Powered by the California Digital Library
University of California

International Journal of Comparative Psychology, Vol. 6, No. 3, 1993

DÉJÀ VU, DIALECTICS, AND THE CONSTANCIES OF CONTROVERSIES INVOLVING THE NATURE-NURTURE ISSUE: REFLECTIONS ON THE REVIEWS OF *FINAL SOLUTIONS: BIOLOGY, PREJUDICE, AND GENOCIDE*[1]

Richard M. Lerner
Michigan State University

My first book, *Concepts and Theories of Human Development,* was published in 1976. A few months later the first review of the book appeared. It was negative and, to me, devastating.

A very senior colleague (who, as I recall, was about the age I am at this writing and, as a consequence, does not seem quite so senior now) tried to console me. He argued that the content of the review was less important than the fact that the book was reviewed per se. He suggested that, within a short time, people would forget the content of the review, but that they would recall that they had seen my work mentioned. In addition, he added, other reviews would appear and, he promised, there would be diversity of opinion.

He was right. Several other reviews were published, and I was relieved when the majority of them were favorable. I knew some people (for example, my mother) would focus only on the positive essays. Still, I feared some people would only read or remember the negative reviews (for example, members of a tenure and promotion committee).

I wished that in some way, one that would not be construed as either self-serving or self-promotional, someone would put all the reviews together in one place. Readers of these pieces would recognize that the diversity of opinion that (probably inevitably) existed across the reviews meant that my mother's opinion, that my scholarship was of unquestionable value, was not tenable. However, readers would also see that books such as *Concepts and Theories of Human Development,* ones that dealt with the core issues of development and most centrally with the nature-nurture issue, elicited from equally competent and distinguished

Address correspondence to Richard M. Lerner, Unit 2, Paolucci Building, Michigan State University, East Lansing, MI 48824, USA.

© 1993 International Society for Comparative Psychology

scholars a range of often contradictory assertions and evaluations. I wished, therefore, that readers would see that the contradictions formed a dialectic. Perhaps they would then conclude that a key point in ascertaining the value of a book that engaged the nature-nurture issue was whether it provided a synthesis that moved the controversy to a higher plane of clarity, precision, or scientific or societal utility.

Now, some 17 years and more than 25 books later, much of my wish has been fulfilled. I am honored that so many renowned scholars have taken the time to comment on, clarify, and offer corrections to my presentation in *Final Solutions*. I am grateful to all of them, even those with the most negative opinions of the book. Indeed, it is only because I am fortunate enough to have both the negative and the positive appraisals of my work appear in the same journal issue that the value of the book—as a vehicle moving the controversies surrounding the nature-nurture issue to a more superordinate, integrated level of discussion—can be evaluated. Of course, it is for readers of the book *and* of the preceding reviews to decide if *Final Solutions* has this value. However, in the hope of facilitating this evaluation it may be useful for me to point to, if not synthesize, the contradictions in appraisals that are found across the reviews.

However, this is where my wish is not realized. By my own estimation, I am afraid that it is not readily evident that a higher level of synthesis has in fact been attained. To those familiar with the history of the debates involved in the nature-nurture issue (see, for example, Aronson et al., 1970; Gottlieb, 1983, 1992; Gould, 1980; Kuo, 1976; Lehrman, 1953; Lerner, 1976, 1986; Schneirla, 1956, 1957; Tobach, 1981; Tobach et al., 1974; Tobach & Greenberg, 1984) it will seem that the range of comments applied to *Final Solutions*—that is, the character of the controversy surrounding the nature-nurture issue in 1993—is not markedly discrepant from the quality of the debates that occurred in 1976, when *Concepts and Theories of Human Development* appeared. Indeed, because of this constancy it is apt to note the oft-cited phrase of Yogi Berra that "it seems like déjà vu all over again."

The preceding reviews of *Final Solutions* raise at least five issues that were addressed in 1976 in *Concepts and Theories of Human Development* and in the second edition of the book, published in 1986. Several of these issues are closely interrelated. However, for purposes of exposition it is useful to treat them separately.

Issue 1: Genetic Activity and the Question "How?"

The first, and perhaps superordinate, issue pertains to how genes function, how genes contribute to behavior and development (Anastasi, 1958; Schneirla, 1956). Both editions of *Concepts and Theories of Human Development* (hereafter labeled *Concepts*) present a developmental contextual view of genetic functioning. This perspective is based on Schneirla's (1957) levels of integration notion and on his and Tobach's concepts of organism-context interaction (Tobach, 1981; Tobach & Schneirla, 1968), or fusion (Tobach & Greenberg, 1984), and constitutes a "developmental systems" (Ford & Lerner, 1992) view of genetic activity. Both editions of *Concepts* and *Final Solutions* adopt this developmental systems perspective.

This view is the antithesis of genetic determinism; this latter view is—as explained in both *Concepts* and *Final Solutions*—a conception that rests on the beliefs that: (a) genes and context are separable levels of organization; and (b) genes, as compared to context, exert an independent and primary influence on behavior and development. In opposition to genetic determinism, a developmental systems perspective emphasizes that genes and context are part of an inseparable, or fused, system. This perspective has recently been described by Gottlieb (1992):

> The ultimate aim of dissolving the nature-nurture dichotomy will be achieved only through the establishment of a fully developmental theory of the phenotype from gene to organism (p. vii). . . . Individual development is characterized by an increase of complexity of organization—i.e., the emergence of new structural and functional properties and competencies—at all levels of analysis (molecular, subcellular, cellular, organismic) as a consequence of horizontal and vertical coactions among its parts, including organism-environment coactions. . . . Horizontal coactions are those that occur at the same level (gene-gene, cell-cell, tissue-tissue, organism-organism), whereas vertical coactions occur at different levels (gene-cytoplasm, cell-tissue, behavioral activity-nervous system) and are reciprocal, meaning that they can influence each other in either direction, from lower to higher, or from higher to lower, levels of the developing system. . . . The cause of development—what makes development happen—is the relationship of the two components, not the components themselves. Genes in themselves cannot cause development any more than stimulation in itself can cause development. When we speak of coaction as being at the heart of developmental analysis or causality what we mean is that we need to specify some relationship between at least two components of the developmental system. (pp. 161–163)

> The facts of genetic activity support this development systems perspective: The actual role of genes (DNA) is not to produce an arm or a leg or fingers, but to produce protein (through the coactions inherent in the formula DNA ↔ RNA ↔ protein). The protein produced by the DNA–RNA–cytoplasm coaction then differentiates according to coactions with other cells in its surround. Thus, differentiation occurs according to coactions above the level of DNA-RNA coaction (i.e., at the supragenetic level). (Gottlieb, 1992, pp. 164–165)

The developmental systems perspective can find no middle ground—no compromise or synthesis—with conceptions that rest on the separation of genes from context. Both sociobiology and behavioral genetics are, at their core, such conceptions.

It is my representation of this core, and my belief in the impossibility of compromise with it, that evoke the criticisms found in the reviews of *Final Solutions* by Kaye, by Lamb, and by Siegel and Crowley. Each of these scholars expresses strong reservations about my views of genetic activity and, as well, about my descriptions of genetic determinism—both in general and specifically in regard to sociobiology and behavioral genetics.

Kaye seems annoyed by what he regards as my intemperate and moralistic writing, a product he believes of "political correctness." He sees my characterization of the genetic determinist position as "distorted and caricatured" (p. 147). Similarly, Lamb and Siegel and Crowley believe that my ideas about either sociobiology (Lamb) or behavior genetics (Siegel and Crowley) are too categorical, too superficial, and insufficiently nuanced.

Lamb argues that I fail to appreciate that sociobiology is "a broad approach to the study of behavior, not a single simple ideology," but then goes on to define this entire area of study in general terms, that is, as involving an

> . . . attempt to understand how biological factors (most notably, inclusive fitness) influence behavior. They are especially concerned about evolutionary processes that have taken place over long periods of time and might have produced tendencies evident at the level of large groups of populations. (p. 151)

I cannot disagree with Lamb's generalization about the focus of sociobiology. His words are virtually identical with those I use in *Final Solutions* to make the same point, but he does not make reference to the fact that evolutionary biologists and population geneticists, such as Gould (1980) and Lewontin (1981; Gould and Lewontin, 1979) have criticized sociobiologists' treatment of evolutionary processes as imaginary reconstructions of evolutionary history—what Gould (1980) has labeled "Just So Stories." He also does not note that the other evidentiary bases for sociobiology—the identification of homologies and the reliance upon heritability analyses—have been seen as equally problematic (Hirsch, 1970, 1990; Lerner & von Eye, 1992; Wilson, 1980).

In this regard, reviewer Hirsch notes that "It is good that Lerner articulated his version of the language of sociobiology, thereby enabling readers to appreciate why so many of its claims are incorrect" (p. 138). However, it is precisely in regard to one of these claims—involving the use of the results of heritability analyses as evidence for sociobiological interpretations of genetic activity—that Siegel and Crowley find fault with *Final Solutions*.

Siegel and Crowley indicate that my treatment of the topic of heritability analysis and thus my "characterization of contemporary behavioral genetics theory (subsumed under the paradigm of biological determinism) is too simplistic" (p. 159). They imply that I fail to understand that scholars such as Robert Plomin and Sandra Scarr

> . . . are not making simple genotype-environment correlational statements. Rather, they argue that individuals select and construct their own environments based upon heritable characteristics. (p. 159)

Siegel and Crowley's characterization of the work of Plomin and Scarr is precisely correct, but, as a consequence, the reviewers are "hoisted on their own petard": it is exactly because behavioral geneticists use what they themselves (Plomin, DeFries, & McClearn, 1980) depict as a *descriptive,* sample-dependent, non-generalizable, population statistic for *explaining* individual behavior and intraindividual change that the reliance of behavioral geneticists on heritability estimates is an egregiously flawed procedure (Hirsch, 1970, 1990; Lewontin, Rose, & Kamin, 1984).

Thus, the comments about the issue of genetic activity forwarded by Kaye, Lamb, and Siegel and Crowley are simply wrong. The reviewers are mistaken if they believe there is—at least from a developmental contextual, or developmental systems, perspective—any scientific merit in theory or research based on views of genetic activity (e.g., sociobiology, behavioral genetics) that separate genes from context (Gottlieb, 1970, 1983, 1992; Lehrman, 1953; Schneirla, 1956; Tobach, 1981). Simply, genes "do not work," genetic activity does not occur, in the manner that would be required to validate the conceptual bases of sociobiology or behavioral genetics. In other words, these fields are grounded on a counterfactual view of genetic functioning.

Although many social and behavioral scientists have yet to understand or accept the basic facts of genetic activity, or grasp the implications of these facts for invalidating extant sociobiological or behavioral genetic approaches, among geneticists the developmental systems character of genetic activity is "common knowledge." Thus, reviewer Greenberg, commenting on the flawed reasoning and scholarship involved in genetic determinist conceptions, notes that:

> Although geneticists have abandoned such simplistic ways of thinking about genetic influences, this "genetic essentialism" and its serious social implications still form the basis of much thinking about behavior. (p. 134)

In support of Greenberg's view about the rejection of genetic determinist conceptions by geneticists, it is useful to note that molecular geneticist Mae-Wan Ho (1984, p. 285) has indicated that:

> Forever exorcised from our collective consciousness is any remaining

> illusion of development as a genetic programme involving the readout of the DNA "master" tape by the cellular "slave" machinery. On the contrary, it is the cellular machinery which imposes control over the genes. . . . The classical view of the ultraconservative genome—the unmoved mover of development—is completely turned around. Not only is there no master tape to be read out automatically, but the "tape" itself can get variously chopped, rearranged, transposed, and amplified in different cells at different times.

Similarly, molecular and cell biologist R. C. Strohman (in press), in a review of *Final Solutions* and of Gottlieb's (1992) above-noted book, prepared for another journal, notes that:

> 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?

I must agree with Strohman (in press) that it is still not clear when psychologists will fully incorporate a developmental contextual/developmental systems perspective about genetic activity into their theory and research. Certainly, however, some psychologists have done this (e.g., Gottlieb, 1992; Tobach, 1981; Tobach & Greenberg, 1984), and as a consequence of their work I must also agree with Gottlieb's (1992) conclusion about the developmental systems nature of human development:

> In fact, there is no other way to envisage the manner in which development must occur if a harmoniously functioning, fully integrated organism is to be its product (pp. 165–166). . . . [G]enes are part of the developmental system in the same sense as other components (cell, tissue, organism), so genes must be susceptible to influence from other levels during the process of individual development (p. 167) . . . [and] another important feature of developmental systems is that causality is often not "linear" or straightforward. (p. 169)

Issue 2: Are Genetic Determinist Ideas Viable in Contemporary Science and Society?"

Across the two editions of *Concepts* I stressed that genetic determinist views of human behavior and development were influential in contemporary social and behavioral science. I discussed several instances of such views, ones associated with what Gottlieb (1970, 1983) terms a predetermined epigenetic perspective. However, I placed emphasis on theory and research in the study of racial differences in intelligence (or, more precisely, in IQ test scores), and focused on the work of Jensen (1969, 1973) and Burt (1966), as well as on (what were then) more recent contributors to the genetic/family resemblance literature (e.g., Bouchard & McGue, 1981). As a consequence of this first emphasis, I also stressed the heritability literature. Finally, in my attempt to explicate the work of T. C. Schneirla to what I presumed would be a readership largely from the field of human development, a third emphasis was on the work of Konrad Lorenz (e.g., 1940, 1963). Sociobiology was treated only briefly in the two editions.

One reason that I decided to write *Final Solutions,* however, was to expand on my discussion of sociobiology and to draw what I saw as the link between this field and those on which I had focused in *Concepts.* In my view, this link was predicated on a common adherence to genetic determinism. In addition, as had the other instances of genetic determinist thinking that I had discussed, I believed that sociobiology was not only influencing the work of scientists but, as well, was influencing both science policy and social policy. A key purpose of *Final Solutions* was, then, to discuss this *current* instance of the association between genetic determinist ideas and social policy.

It appears that on precisely this point the reviewers divide. However, the very fact that there is diversity of opinion among such a distinguished set of scholars, representing several distinct disciplines—about the contemporary relevance of biological determinist thinking in general, and about sociobiology in particular—supports the argument in *Final Solutions*: genetic determinist thinking/sociobiology is still controversial within contemporary scholarship. Indeed, and as evidenced in some of the preceding reviews, there is validity for Greenberg's observation about "the emotional and passionate attachment many still have to genetic determinism" (p. 133).

Thus, and to illustrate the diversity of perspective among the reviewers, while Kaye contends that "Lerner does not realize that the straw men which he seeks to bludgeon—Social Darwinism, ethology, and sociobiology as proto- or crypto-fascist ideologies of biological determinism—have been dead for years" (p. 147), Greenberg describes my work as "A critique of genetic determinism espoused by contemporary ethology and sociobiology" (p. 133), and goes on to say that "Lerner's characterization still forms the basis of much thinking in modern ethology and sociobiology, particularly in its new guise of 'Evolutionary Psychology' " (p. 133).

Other divisions exist among the reviewers in regard to the importance of genetic determinist/sociobiological thinking for contemporary science and society. For instance, Siegel and Crowley do not believe it was necessary for me to devote any attention to the sociobiological ideas of Rushton (e.g., 1988), a Canadian psychologist who—as I describe in *Final*

Solutions—claims that Africans represent an evolutionary atavism. Siegel and Crowley indicate that "The ideas put forth by J. P. Rushton are so ludicrous that they hardly deserve notice, let alone lengthy treatment" (p. 161).

I agree that Rushton's ideas are ludicrous. However, I also believe they are dangerous. Indeed, it is precisely the point of my historical analysis that we cannot let such ideas go unchallenged; we cannot let silence be misinterpreted as tacit agreement. Ludicrous and even lunatic ideas have found their way into social policy before—recall the history associated with Hitler's (1925) *Mein Kampf.* I agree with the point emphasized by Hirsch at the end of his review: "The price of liberty is eternal vigilance" (p. 140).

Moreover, several other comments by the reviewers provide support for the importance of my historical analysis for contemporary science and society. For instance, Lamb indicates that:

> Lerner's argument is provocative, clearly reasoned, and demands consideration by social scientists, humanists, and those who would avoid both the repetition of the past and our ignorance of its costs and lessons. (p. 149)

Lamb goes on to note that:

> The timeliness of the book is underscored by the current spectacle of genocidal mayhem in Bosnia, complete with the specter of officially endorsed rape in the service of ethnic hatred and racial pollution. (p. 149)

Rogers concurs with Lamb's assessment, observing that "The book is indeed valuable reading, particularly in the present climate in which genetic determinism is, yet again, being co-opted for social/political purposes" (p. 157).

Issue 3: Is My Account of the History of Genetic Determinist Thinking Appropriate?

If diversity of opinion existed across the reviews on the contemporary relevance for science and for society of sociobiological thinking, then no less of a division occurred with respect to the legitimacy of my analysis of the presence across history of ideas of genetic or, more broadly, biological determinism. For instance, Kaye, once again critical of my work, employs some of my own writing in *Final Solutions* to indicate that:

> To lump together Plato, Spencer, Haeckel, Freud, Lorenz, Wilson, and Herrnstein, Social Darwinists, Eugenicists, Nazi racial hygienists, ethologists, and sociobiologists, with no appreciation of either the context or specific content of their work or the profound differences in

their perspectives, is bad *social* science and constitutes "serious violations of the rules of scientific debate." (p. 148)

In turn, however, Kalikow sees both validity for, and merit to, my analysis of historical continuities in genetic determinist ideology. She notes that "In each generation biological determinism was presupposed as correct, and failures in the human genome were attributed to different causes" (p. 142).

She goes on to stress that:

> When every generation brings a new reason for decline and a new social movement to capitalize on it, this is a clue that we have entered the realm of ideology, here defined as the set of presuppositions underlying theories and world views. Teasing these presuppositions out is a useful exercise. (pp. 142–143)

Among all the instances across history of the links between genetic determinist ideology in science and in society, one is of particular concern to Kalikow: the science and politics of Konrad Lorenz. The career of Lorenz is also a focus in *Final Solutions*. A discussion of the reviewers' comments regarding my presentation about Lorenz is pertinent to the general issue of the appropriateness of my historical analysis. However, because both *Final Solutions* and its reviews focus separately on Lorenz, it is useful to treat his work as an independent issue.

Issue 4. Is My Discussion of the Career of Konrad Lorenz Appropriate?

There are at least two dimensions to the comments of the reviewers regarding my discussion of Konrad Lorenz: Science and Politics. The reviewers divide in regard to whether I have accurately represented one or both of these dimensions.

Hirsch, for example, sees Lorenz's Nazism as a personal shortcoming, but one that did not affect the quality of his science. He states that what I depict in my account of Lorenz "is a human tragedy, not a scientific one" (p. 137). Nevertheless, Hirsch goes on to note that I was *not critical enough* of Lorenz's science, that is, that I failed to note an important scientific shortcoming in Lorenz's ideas:

> A serious omission from Lerner's discussion is an appreciation of the important correction to Lorenz's too pessimistic ideas about vertebrate (especially primate, including human) aggression that has been provided by Frans de Waal's (1989) superb exposition in *Peacemaking among Primates*. (p. 137)

Lamb also criticizes Lorenz's science. Citing an earlier appraisal of the validity of Lorenz's science, he notes that:

Konrad Lorenz, an Austrian biologist whose contributions to the understanding of animal behavior earned him a Nobel Prize in 1973, even though, as Rajecki, Lamb, and Obmascher (1978) wrote shortly thereafter in an appraisal of his most widely cited scientific work: "[Lorenz's] provocative notions stimulated an enormous amount of research, the result of which is that all of Lorenz's postulates on imprinting can be viewed as incorrect." (p. 418)

From my discussion in *Final Solutions* and, earlier, in the two editions of *Concepts,* as well as in my above remarks regarding "Issue 1," it is clear that I believe that Lorenz's science—insofar as it rests on genetic determinist conceptions—is irreparably flawed. In addition, I believe I have also been clear that I think Lorenz was personally flawed as well.

Kaye disagrees. He notes that:

> Konrad Lorenz, despite his Nazi past, is not the advocate of killing and ruthless oppression that Lerner portrays him to be (compare Lerner's patched-up quotation from pages 251 and 48 in *On Aggression* with the actual text). (p. 147)

Kalikow, however, has a different opinion of what may be gleaned about Lorenz's character from *On Aggression.* She indicates that:

> *On Aggression* unleashed a media blitz of 'naked apeism,' whose underlying thesis was that we had to preserve our instinctive emotional and behavioral equipment in the face of civilization's threats to it. (These threats could be construed to include the Civil Rights and women's movements.) (p. 142)

She goes on to describe that:

> In the 1960's the notoriety of *On Aggression* (and clones by other authors) again signalled that Lorenz's claims echoed what many people were ready to hear. While opponents thought that his ideas were horribly wrong, argued against the determinism implicit in them, pointed out connections with the Nazi genocide, and so on, the commercial success of writers like Desmond Morris, Robert Ardrey and Lorenz himself showed what side the popular world view favored. (p. 142)

Despite this division among the reviewers about Lorenz's character, I believe that there are two facts that make the argument far from moot. First, as discussed in *Final Solutions* and earlier in several papers by Kalikow (e.g., 1978, 1983), Lorenz was a Nazi. Second, he published *several* papers between 1938 and 1943 calling for precisely the sorts of actions to which Kaye says he was not committed—despite the Nazism that Kaye acknowledges. These papers are also discussed in Kalikow (1978, 1983) and in *Final Solutions.*

I must be very blunt here. I do not apologize to my colleagues if I cannot forgive a person—despite whatever level of scholarly eminence he might have achieved—who willingly associated himself with a regime committed to and enacting genocide. Accordingly, I find fault with Siegel and Crowley's comment that:

> Things are black and white. Lorenz wears a black hat—no ifs, ands, or buts. Because he maintained the importance of genetic contributions to behavior, Lerner would have us conclude that *all* else is suspect. (p. 160)

These reviewers fail to understand that it was not Lorenz's genetic determinist thinking that was the basis for the "human tragedy" that Hirsch depicts. Rather, it was his Nazism. I do believe that Lorenz's science was "wrong", but, and as I argued in *Final Solutions,* independent of his science was his politics. And Lorenz's politics were evil. In my world view, there are no "good Nazis."

My stance here, and my phrasing, may strike some readers as inappropriate for a scientific journal. However, I must agree with Greenberg that "It is not only not improper, it is imperative that science have a social conscience" (p. 133).

My position here leads to another fault I find with the views of Siegel and Crowley. These reviewers indicate that my

> . . . suggestions that (1) the link between Lorenz and Nazi ideology is clear and coherent, and (2) Nazi ideology is hodgepodge, are inconsistent and the result of an annoying selective levelling and sharpening. (p. 159)

Nazi ideology was a motley set of ideas that, together, were even more poorly reasoned and ludicrous than those of Rushton (1988), which Siegel and Crowley thought I would have done better to ignore. However, the nature of Nazi ideology made it all the more easy to find some way to link one's own ideas with this ideology, especially when—as was the case with Nazism and Lorenz's ideas—a common overlap with then contemporary views about biology existed.

Accordingly, and for a host of possible reasons, Lorenz commingled his ideas, ones that certainly had roots in his work prior to the beginning of the Nazi regime in 1933, with this Nazi ideology, and he published these "integrated" ideas in "journals" that were political tools of the Nazi State. Nothing about this series of events strikes me as inconsistent or, in fact, surprising, if one stresses—as I choose to do—that the person involved in this history was a willing, card-carrying member of the Nazi Party.

The review by Rogers supports my stance. She agrees with my contentions that:

> Lorenz used the terminology of the Nazis during their era and that his thinking was congruent with Nazi ideology . . . [that Lorenz was] clearly following Nazi ideology of the time . . . [and that] there was an

> evident Nazi-era/post-Nazi-era continuity in the writings of Lorenz. (p. 155)

Issue 5. Is Developmental Contextualism a Scientifically Useful Theory of Human Development?

Much of the diversity in the reviewers' opinions about *Final Solutions* rests, I believe, on their evaluations of the scientific use of developmental contextualism. If so, I am not surprised. The range of opinions expressed in reviews of both editions of *Concepts* reflected different perspectives about the theoretical and empirical use of this perspective. Indeed, the range experienced in respect to *Concepts* is evident in the present set of reviews.

For example, Greenberg indicates that:

> The significance of this approach to understanding behavior is that it is heuristic and empirically testable, as Lerner's work has shown over the years; it is, as well, parsimonious. (pp. 134–135)

Although I am grateful for this characterization of my work, it is necessary to point out that quite an alternate appraisal is also found in the reviews. While labeling developmental contextualism as "the circumplex model," Siegel and Crowley claim that developmental contextualism is a completely dispersive model (but see Lerner & Kaufmann, 1985), one that is too inclusive and, by implication, empirically unfalsifiable. They state that:

> The reader should be warned a priori that it reflects a dispersive metaphor and stresses scope and comprehensiveness, rather than precision. Like the Health Belief Model that drives so much of the research in behavioral medicine (you need to at least mention it to get funded), the circumplex model is so inclusive that you can't possibly disagree with it; so vague, that virtually any research study can be conducted as long as you mention that "... we know that we are looking at one only small piece of a larger model ..." in the discussion section. (p. 161)

It is hard to reconcile this portrayal of developmental contextualism with either Greenberg's appraisal or with the several lines of work, from laboratories around the world, that are productively using this theoretical view. That is, ideas associated with developmental contextualism are being used to derive and test numerous models, ones making precise (and falsifiable) predictions about how changing *relations* between specific aspects of person and context should and should not relate to specific features of behavior and development. Some exemplary cases in point are found in Baltes (1987), Brooks-Gunn (1987), Clausen (1993), Eccles (1991), Elder (1974, 1979), Magnusson (1988), Perlmutter (1988), and Stattin and Magnusson (1990).

CONCLUSIONS

I began *Final Solutions* by describing a sad and frightening incident, one involving a story told to me by my Grandmother. It may be appropriate to end my discussion of the reviews of the book with a story about a more pleasant interaction I had with her.

> During the summers my Grandmother and Grandfather rented a cottage in the Catskill region of upstate New York. I often visited them for all or major parts of the summer. I spent many hours watching them play pinochle with people vacationing in nearby cottages. They played the card game almost every afternoon and often in the evenings as well. Yet, although they won their share of the hands, win or lose my Grandmother would get into intense and protracted arguments—with her partner (my Grandfather) or with their "opponents" (my aunts or uncles, mostly).
>
> One day, I must have been about 10, I asked my Grandmother why she liked playing pinochle if all it did was aggravate her so much that she constantly got into arguments. "Darling," she replied, "I don't care for pinochle at all. It's the arguments I love."

No scholar likes having his or her work criticized. However, if we did not enjoy intellectual disagreement or debate we would not "play the game"; we would go into some other line of work.

I believe it is fortunate that scholars enjoy debate. In my view, debate is the cornerstone of scientific progress. Scientific advances, I believe, derive more often from differences among scholars in the interpretation of facts than from the facts themselves.

I am grateful for having had the chance to engage in debate with so distinguished a set of scholars. As I indicated at the beginning of this essay, I feel honored by having been given this opportunity. In addition, I have thoroughly enjoyed it. My hope is that the readers of these essays will find reason to conclude that some progress has been made in the understanding of the issues that the reviewers and I have discussed. If so, then a key goal of *Final Solutions* and, I am certain, of all of the essays in this issue will have been attained.

ACKNOWLEDGEMENT

Preparation of this manuscript was supported in part by a grant from the W. K. Kellogg Foundation.

REFERENCES

Anastasi, A. (1958). Heredity, environment, and the question "how?" *Psychological Review, 65,* 197–208.

Aronson, L. R., Tobach, E., Lehrman, D. S., & Rosenblatt, J. S. (Eds.). (1970). *Development and evolution of behavior: Essays in memory of T. C. Schneirla.* San Francisco: W. H. Freeman.

Baltes, P. (1987). Theoretical propositions of life-span developmental psychology: On the dynamics between growth and decline. *Developmental Psychology, 23,* 611–626.

Bouchard, T. J., Jr., & McGue, M. (1981). Familial studies of intelligence: A review. *Science, 212,* 1055–1059.

Brooks-Gunn, J. (1987). Pubertal processes and girls' psychological adaptation. In R. M. Lerner & T. T. Fosch (Eds.), *Biological-psychosocial interactions in early adolescence* (pp. 123–153). Hillsdale, NJ: Erlbaum.

Burt, C. (1966). The genetic determination of differences in intelligence: A study of monozygotic twins reared together and apart. *British Journal of Psychology, 57,* 137–153.

Clausen, J. A. (1993). *American lives: Looking back at the children of the Great Depression.* New York: The Free Press.

de Waal, F. (1989). *Peacemaking among primates.* Cambridge, MA: Harvard University Press.

Eccles, J. S. (1991). Academic achievement. In R. M. Lerner, A. C. Petersen, & J. Brooks-Gunn (Eds.), *Encyclopedia of Adolescence* (Vol. 1, pp. 1–5). New York: Garland.

Elder, G. H., Jr. (1974). *Children of the great depression.* Chicago: University of Chicago Press.

Elder, G. H., Jr. (1979). Historical change in life patterns and personality. In P. B. Baltes & O. G. Brim (Eds.), *Life-span development and behavior* (Vol. 2, pp. 117–159). New York: Academic Press.

Ford, D. H., & Lerner, R. M. (1992). *Developmental systems theory: An integrative approach.* Newbury Park, CA: Sage Publications.

Gottlieb, G. (1970). Conceptions of prenatal behavior. In L. R. Aronson, E. Tobach, D. S. Lehrman, & J. S. Rosenblatt (Eds.), *Development and evolution of behavior: Essays in memory of T. C. Schneirla* (pp. 111–137). San Francisco: W. H. Freeman.

Gottlieb, G. (1983). The psychobiological approach to developmental issues. In P. H. Musson (Series Ed.) & M. M. Haith & J. J. Campos (Vol. Eds.), *Handbook of child psychology: Infancy and biological bases* (4th ed., Vol. 2, pp. 1–26). New York: Wiley.

Gottlieb, G. (1992). *Individual development and evolution: The genesis of novel behavior.* New York: Oxford University Press.

Gould, S. J. (1980). Sociobiology and the theory of natural selection. In G. W. Barlow & J. Silverberg (Eds.), *Sociobiology: Beyond nature/nurture* (pp. 257–269). Boulder, CO: Westview Press.

Gould, S. J., & Lewontin, R. C. (1979). The spandrels of San Marco and the Panglossian paradigm: A critique of the adaptationist programme. In J. M. Smith & R. Holliday (Eds.), *The evolution of adaptation by natural selection* (pp. 581–598). London: Royal Society of London.

Hirsch, J. (1970). Behavior-genetic analysis and its biosocial consequences. *Seminars in Psychology, 2,* 89–105.

Hirsch, J. (1990). Correlation, causation, and careerism. *European Bulletin of Cognitive Psychology, 10,* 647–652.

Hitler, A. (1925). *Mein Kampf.* Trans. R. Manheim. Boston: Houghton Mifflin.

Ho, M.-W. (1984). Environment and heredity in development and evolution. In M.-W. Ho & P. T. Saunders (Eds.), *Beyond neo-Darwinism: An introduction to the new evolutionary paradigm* (pp. 267–289). London: Academic Press.

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 and Row.

Kalikow, T. J. (1978). Konrad Lorenz's 'brown past': A reply to Alec Nesbett. *Journal of the History of the Behavioral Sciences, 14,* 173–179.

Kalikow, T. J. (1983). Konrad Lorenz's ethological theory: Explanation and ideology, 1938–1943. *Journal of the History of Biology, 16,* 39–73.

Kuo, Z.-Y. (1976). *The dynamics of behavior development: An epigenetic view.* New York: Plenum Press.
Lehrman, D. S. (1953). A critique of Konrad Lorenz's theory of instinctive behavior. *Quarterly Review of Biology, 28,* 337–363.
Lerner, R. M. (1976). *Concepts and theories of human development.* Reading, MA: Addison–Wesley.
Lerner, R. M., & Kaufmann, M. B. (1985). The concept of development in contextualism. *Developmental Review, 5,* 309–333.
Lerner, R. M., & von Eye, A. (1992). Sociobiology and human development: Arguments and evidence. *Human Development, 35,* 12–33.
Lewontin, R. C. (1981). On constraints and adaptation. *Behavioral and Brain Sciences, 4,* 244–245.
Lewontin, R. C., Rose, S., & Kamin, L. J. (1984). *Not in our genes: Biology, ideology, and human nature.* New York: Pantheon Books.
Lorenz, K. (1940). Durch domestikation verursachte störungen arteigen verhaltens. *Zeitschrift für Angewandte Psychologie und Charakterkunde, 59,* 2–81.
Lorenz, K. (1963). *On aggression.* New York: Harcourt, Brace & World.
Magnusson, D. (1988). Individual development from an interactional perspective. In D. Magnusson (Ed.), *Paths through life* (Vol. 1, pp. 3–31). Hillsdale, NJ: Erlbaum.
Perlmutter, M. (1988). Cognitive development in life-span perspective: From description of differences to explanation of changes. In E. M. Hetherington, R. M. Lerner, & M. Perlmutter (Eds.), *Child development in life-span perspective* (pp. 191–217). Hillsdale, NJ: Erlbaum.
Plomin, R., DeFries, J. C., & McClearn, G. E. (1980). *Behavioral genetics: A primer.* San Francisco: W. H. Freeman.
Rajecki, D. W., Lamb, M. E., & Obmascher, P. (1978). Toward a general theory of infantile attachment: A comparative review of aspects of the social bond. *Behavioral and Brain Sciences, 3,* 417–464.
Rushton, J. P. (1988). Do r/K reproductive strategies apply to human differences? *Social Biology, 35,* 337–340.
Schneirla, T. C. (1956). Interrelationships of the innate and the acquired in instinctive behavior. In P. P. Grasse (Ed.), *L'instinct dans le comportement des animaux et de l'homme* (pp. 387–452). Paris: Mason et Cie.
Schneirla, T. C. (1957). The concept of development in comparative psychology. In D. B. Harris (Ed.), *The concept of development* (pp. 78–108). Minneapolis: University of Minnesota Press.
Stattin, H., & Magnusson, D. (1990). *Pubertal maturation in female development.* Hillsdale, NJ: Erlbaum.
Strohman, R. C. (in press). The powers of development. *Integrative Physiology and Behavioral Science.*
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 own development: A life-span perspective* (pp. 37–68). New York: Academic Press.
Tobach, E., Gianutsos, J., Topoff, H. R., & Gross, C. G. (1974). *The four horsemen: 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: Erlbaum.
Tobach, E., & Schneirla, T. C. (1968). The biopsychology of social behavior of animals. In R. E. Cooke & S. Levin (Eds.), *Biologic basis of pediatric practice* (pp. 68–82). New York: McGraw-Hill.
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 Press.