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Vestiges of Logical Positivism: Critiques of Stage Explanations

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Abstract. This article reviews the historical emergence and basic tenets of logical positivism, and demonstrates that a number of these positivistic tenets still exert a deeply hidden and deleterious influence on developmental psychology. The demonstration is primarily in terms of a case study concerning a critique of tests of Piagetian stage hypotheses. The analysis illustrates and demonstrates a number of important points concerning the vestigial influence of logical positivism: (1) such influences do occur; (2) they can be deeply implicit, and correspondingly difficult to recognize and understand, and (3) they can be seriously damaging of one's understanding of developmental processes in general, and of Piaget's theory in particular. The discussion leads to a reformulation of some conceptual issues in developmental psychology, and some suggestions for other areas where vestigial positivistic influences seem to be operating.

Logical positivism dominated the philosophy of science from the 1920s into the 1960s. During this period, it exerted an enormous influence on the development of psychology, with many psychologists eagerly adopting this attempted rigorous formulation of the scientific enterprise as a set of normative standards for their own scientific efforts. Even during this period, however, some of the most important prescriptions adopted as part of the foundation of scientific psychology were not tenets of logical positivism per se, but were rather distortions of logical positivist principles that were repudiated and rejected by the logical positivists themselves. A primary example of this is the dogma of operational definitionism [*Hempel,* 1965].

Furthermore, the fundamental flaws in logical positivism became more and more apparent over time until it became clear that it could not simply be 'corrected', but needed to be radically replaced at its foundations.

This move toward replacement and the corresponding search for alternatives became dominant among philosophers of science in the 1960s and has been underway since that time [*Suppe,* 1977]. Psychologists, on the other hand, have in general kept in touch with these developments only distantly and tenuously, with a correspondingly restricted assimilation.[1]

The limitations and distortions of the most explicit theme arising out of this logical positivist era, radical behaviorism, have become clear and obvious past the point of debate for most psychologists, though what to replace it with is far from being so consensual. But other tenets, such as operational definitionism, still permeate much psychological literature and culture as if nothing had happened since 1930. Psychologists have benefited greatly from such nonbehaviorist or explicitly antibehaviorist influences as *Chomsky, Piaget,* and computer modeling. Behaviorism, however, was just one of the legacies of logical positivism, and other such legacies still persist with great strength. Furthermore, some of these vestiges persist with an implicitness that makes it difficult to recognize their presence and influence, and still harder to explicate them and their consequences.

[1] Widely known among psychologists is *Kuhn's* [1962, revised 1970] concept of a scientific paradigm, which gained popularity in the 1960s. *Kuhn* was among the early rebels against logical positivism, and his thesis was explicitly intended as an alternative to it, but the implications of his position for the core tenets of scientific psychology were never widely understood, and his actual influence on the practice of psychology has been minimal. Furthermore, the most popularly exciting parts of his thesis were precisely those that attracted the most severe criticism, and with respect to which *Kuhn* was eventually forced to concede [*Lakatos and Musgrave,* 1970; *Suppe,* 1977].

In this article, we will examine an instance of such vestigial logical positivism, arguing both for its existence and for its harmfulness, with four major intentions: (1) a critique and rebuttal of the position examined; (2) a demonstration that such implicit and vestigial influences of logical positivism do in fact exist; (3) an illustration of the distortions and damage that such influences can wreak on one's understanding of developmental processes in general, and (4) an illustration of the damage that can be done to one's understanding of *Piaget's* theory in particular. The instance that we wish to examine is *Brainerd's* conception of, and consequent criticisms of, developmental stage. Our primary focus is his 1977 article in which developmental concept learning research is subjected to severe criticism, on both conceptual and methodological grounds, and it is claimed, among other things, that the concept of developmental stage is intrinsically circular and must be abandoned. We also draw upon *Brainerd's* 1978 article for additional clarification of some of the points. Our intent is to show how *Brainerd's* positions and arguments derive from assumptions that psychology absorbed from logical positivism decades ago, and that these assumptions are false and misleading.

The bulk of *Brainerd's* 1977 article is devoted to methodological considerations, and thus might seem relatively remote from the conceptual and philosophical considerations that we wish to introduce. In fact, however, it provides us with an opportunity to trace the consequences of logical positivistic assumptions, some fairly explicit, with at least one implicit, all the way from foundations through theory to methodology and statistics. Logical positivism promulgated a number of errors, and most of them still permeate psychology. Vestiges of logical positivism are not trivial, either in their extent or in their importance, even though their consequences for psychology in general are often not obvious.

Any attempt at an explication of latent positivistic assumptions must begin with some understanding of what logical positivism *was*, and among the more revealing approaches to such understanding is the historical. We begin with an overview of the emergence and development of logical positivism, with the intent of delineating its major features, especially those that have influenced psychology. Those influences have been with regard to not only psychology's conception of science, but also psychologists' conception of the nature of psychological processes, including conceptions of the nature of cognitive development.

Logical Positivism

Logical positivism was the product of a long historical development, and expressed many themes which deeply permeated, and continue to permeate, the broader culture. Its impact on psychology was due as much to its articulation of these themes as to its arguments for those themes. Early positivism combined several themes from the Enlightenment reaction against the metaphysical excesses of medieval Aristotelianism. A fundamental desire was for 'positive' knowledge – knowledge of facts, and knowledge based upon facts – rather than metaphysical speculation. A major inspiration and model was Newtonian physics. This reaction and movement into empiricism was not an isolated philosophical movement: it expressed a sense of liberation from medieval dogma and from the institutions that perpetuated and functioned in the name of that dogma [*Taylor*, 1975], and was associated with concerns for social and political progress, for example, for *Comte* (1798–1857), the proponent of classical positivism [*Copleston*, 1977]. The theme of empiricism has been fundamental to all versions of positivism, classical and contemporary.

The rejection of medieval metaphysics included rejection of *Aristotle's* multiple forms of causality, especially final causality, in favor of a strict concern for efficient causality [*MacIntyre*, 1981; *Taylor*, 1975]. This restriction of valid explanation of action in the world to efficient causality, usually rendered as a strict antecedent-consequent causality, together with variations of and derivatives from such a restriction, has also been a dominant theme in positivism, though not always as explicit as empiricism. A historically important derivative has been the rejection of even efficient causality as 'metaphysical' in favor of a view of science as concerned solely with the lawful description of empirical regularities, with no 'speculation' about what might lie 'behind' those regularities. One of the important consequences of this shift is that a concern for 'lawful description' begins to grant language and mathematics, the means for such description, a special function in science. The need to understand and account for this function has provided a central dynamic in the evolution of neo- and logical positivism.

A third theme has been inductivism, an epistemology consistent with the focus on empiricism and empirical regularities. The Enlightenment rejection of metaphysics and consequent flight into empiricism tended to involve a rejection of a priori rational thought as a source of knowledge, leaving only induction from empirical facts as a valid foundation for knowledge. The view of science as concerned with lawful descriptions of empirical regularities only strengthened this view, for an empirical regularity seems to be *constituted* as multiple instances of a pattern, and the collection of empirical observations of such patterns would seem the only way to discover or support the existence of such regularities.

A fourth theme has been an emphasis on continuous, quantitative measurement as fundamental to science. This has often been as much a bias as an argued position, but the available models of quantitative empirical regularities in physics have remained a powerful influence, as has the triumph of the calculus as the language of physics – calculus is a mathematics of continuous variables.

The positivist rejection of a priori sources of knowledge would be problematic in the best of circumstances, but the clear importance of mathematics in science, and in the positivistic interpretation of science, focused this tension ever more strongly: mathematical truths are prime candidates for a priori truths, and their functions in science would seem to undermine the strict empiricism of positivism. At times, this tension was 'resolved' with a strict inductivist interpretation of logic and mathematics.

At the end of the 19th century, the dominant philosophies of science in Germany were a neo-Kantianism associated with *Helmholtz* and *Cohen,* and later with *Cassirer,* and a neopositivism associated primarily with *Mach.* The neo-Kantianism viewed science as yielding knowledge of the world in terms of webs of logical relations which were exemplified in sensory experience. The focus on sensory experience was consistent with positivism's empiricism, but the understanding of logical relations allowed for a priori elements of knowledge. The neopositivism of *Mach* rejected such a priori elements in favor of a view of the principles of science as being 'nothing but abbreviated descriptions of sensations' [*Suppe,* 1977, p. 10]. Later, this was modified to allow for 'the inclusion of an a priori element in science, but construing it as being a conceptual element without factual content' [*Suppe,* 1977, p. 10].

Neither of these positions could accommodate developments in physics in the early part of this century, specifically relativity and quantum mechanics, and there ensued a sense of crisis concerning how to understand science given these new developments. The most influential response to this crisis was to emerge from *Mach's* neopositivism, and led to the received view of science as a philosophy of science, and to logical positivism as a broader philosophy of language. Relativity and quantum mechanics served as the joint impetus for such a change; *Whitehead and Russell's Principia Mathematica* (1910–1913) and *Wittgenstein's Tractatus Logico-Philosophicus* (1922) served as the models and inspirations for that change. The *Principia* provided a model for the axiomatic derivation of mathematics from logic. A major reason for its influence was that, in (purportedly) reducing mathematics to logic, it provided a way of incorporating mathematics into the fundamental empiricist program without admitting a priori elements: mathematics was 'just' logic, and logic was 'just' convention – logic was just a matter of syntax. Mathematics, then, was available for the expression of empirically observable relationships without being understood to introduce any empirical content itself.

The influence of the *Principia* went far beyond this basic motivation, however. Most fundamentally, the axiomatic form of the *Principia* was taken as the normative ideal for any well developed theory. Just as axiomatic derivation from logic was understood to introduce mathematics without adding any content beyond the conventions in the axioms, so also was derivation from axioms understood to introduce theoretical terms of science without adding any content beyond the empirically grounded statements at the axiomatic level. The empirical content at the axiomatic base, in turn, was provided by rules of correspondence between the fundamental axiomatic statements and pure theory-neutral empirical statements. In this way, theoretical terms could be introduced without admitting meaningless metaphysical 'nonsense' into science. Theories, then, were construed as structures of conventional abbreviations for relationships among empirical observations.

Under the influence of *Wittgenstein's* proposal in the *Tractatus* for a logically perfect language, a language that would be incapable of expressing metaphysical nonsense without violations of its syntax, this conception of the nature of the empirical content of scientific theories was expanded into a thesis about the cognitive content of language in general. Thus was born the verificationist theory of meaning of logical positivism: the cognitive content of language is provided entirely by logical derivation from, i.e., conventional abbreviations for, empirical observation statements. The meaning of a statement is the observational means by which the truth value of a statement can be verified. All language that does not conform with this condition is about covert metaphysical entities, and is without cognitive significance (e.g., it is emotive, or serves some other noncognitive function).

The verificationist theory of meaning was the central tenet of the early logical positivism. It was elaborated, modified, and eventually abandoned by the central figures of the field – *Schlick, Ayer, Carnap, Reichenbach, Hempel, Chisholm* – but it remained something of an ideal form to which a workable approximation was hoped to be found. Since all meaningful language is reducible to statements about empirical observations, the verificational meaning of language is correspondingly grounded on the verificational meaning of observational statements, i.e., the methods of their verification. For the verification of singular statements about particulars of experience, it was proposed that the observation language be about material things and physical properties, and that the perception of such physical realities posed no epistemological problems. For the verification of generalizations about experience, the logical positivists turned to the hoped for development of an inductive logic, just as did the classical positivists.

The axiomatic logical structure, thus, allowed for mathematics without a priori elements, provided for theoretical terms without leaving empiricism behind, inspired a verficationist theory of language meaning in general, led to a perceptual physicalism, and connected with the inductivist theme in classical positivism. It had still further consequences, however. For example, since the only moves that are legitimate within an axiomatic logical system are deductions from the axioms, it follows that prediction and explanation have exactly the same deductive form, and differ only in their temporal relationship to the event being predicted or explained. Additionally, since those deductions cannot introduce any new content, and since they must begin with factual empirical statements, there is no valid way for issues of value to be introduced into scientific reasoning, and issues of fact and science on the one hand and value on the other must involve strictly distinct and at most marginally related realms of consideration. As still another consequence, the axiomatic construction of language on observational primitives became not only a logical structure, but also an epistemic structure: such a construction on primitives was proposed as a model for language learning, and such a construction on observational facts was proposed as the way science develops. Inductivism, thus, became not just a logical method for verifying general statements; a Baconian or 'magpie' inductivism [*MacIntyre*, 1981] became a model for individual and scientific development.

A historically important derivation from the verificationist theory of meaning is operational definitionism. The verificationist theory of meaning proposed to reduce the meaning of statements to the methods by which their truth value could be observationally determined. Operational definitionism proposed to reduce the meaning of terms to their methods of measurement (with a strong bias toward continuous variable, quantitative measurement). This was inspired by the correspondence rules of verificationism, but differed in critical and untenable ways. First, it violated the thesis of the 19th-century logician *Frege* that the basic unit of language meaning is the sentence, not the word, so that word meaning is derivative from its participation in sentences, rather than sentence meaning being built of word meanings. Second, it could make no sense out of multiple methods of measurement for the same term, e.g., the differing methods of measurement of temperature for differing ranges of temperature. Third, it shared with standard rules of correspondence an inability to accommodate dispositional terms. And so on. Operational definitionism, for all these reasons, was never accepted within the primary logical positivist circles. Its primary influence has been in psychology and sociology.

A related derivation was methodological behaviorism. In its general form, this was an attempt to carry out the logical positivist program of defining all theoretical terms within an axiomatic system founded on observational (behavioral) statements. In its stricter form, it was the exclusion of all theoretical terms as being not operationally definable. There was a tension between these two versions throughout the ascendancy of behaviorism.

Logical positivism and the received view of science faced severe difficulties from the beginning. Within logical positivism per se, verificationism gave way to falsificationism, which in turn led to a general abandonment of the attempt at a strict empiricist theory of language meaning. Within the received view, the reduction of the theoretical to the observational proved difficult to carry out, the assumption of a theory-neutral observation language came to be severely criticized, no sound version of induction was found, Baconian inductivism as the process of science proved to be historically false, the purported symmetry between prediction and explanation failed for some sciences (e.g., evolutionary biology, meteorology, etc.), and so on. As the difficulties and criticisms mounted, post-received view philosophies of science were proposed by *Hanson, Kuhn, Feyerabend,* and others, which were in turn subjected to telling criticism and replacement.

Philosophy is now in at least the second generation of post-received view philosophies of science. Furthermore, there are major philosophical schools that developed contemporaneously with and in opposition to logical positivism whose relevance to psychology is just beginning to be appreciated – at least in the United States –, such as hermeneutics (e.g., *Betti* and *Gadamer*) and critical theory (e.g., *Adorno, Horkheimer,* and *Habermas*). Psychology, however, has in many ways still not freed itself from logical positivism [*Kitchener,* 1983]. Behaviorism shows up frequently, and operational definitionism is still rampant. Induction, the building of the complex and abstract out of the simple and concrete, is taken as a model not only for scientific development, but even for cognitive development. The divorce of issues of fact from those of value creates bewilderment about how to approach the higher realms of human nature as well as conceptual chaos in clinical psychology and other applied areas. And the restriction of explanation to efficient (antecedent-consequent) causality, the bias for quantitative measurement, and the presumed deductive equivalence of prediction and explanation remain buried and hidden, with few psychologists (though a growing number) aware that there are problems in these areas.

Criticisms of Stages

Brainerd [1977] offers a number of logical, methodological, and philosophical criticisms of the concept of developmental stage and of stage-related research. In particular, he criticizes stage models of concept learning on three basic grounds: (1) that the very concept of developmental stage fails as an acceptable explanatory construct; (2) that the explanatory unacceptability of the concept of stage requires a basic shift of conceptualization and statistical methodology in developmental studies, and (3) that the statistical methodology used in many developmental studies of concept learning is flawed. In a subsequent article [*Brainerd,* 1978], he elaborates and refines these criticisms. We agree with *Brainerd* that the conceptualization and methodology of developmental stage research requires further careful attention, but we wish to contend that *Brainerd's* arguments contain errors of logic, methodology, and philosophy, and thus that they do not support his particular conclusions. Most fundamentally, we wish to show that a number of those errors derive explicitly or implicitly from incorrect assumptions of logical positivism.

Brainerd's [1977] discussion consists of four major points: (1) He reviews and makes a basic methodological criticism of a number of studies which have attempted to train stage theoretical concepts, usually some form of conservation. He argues that these studies have demonstrated only correlations between test scores before and after training, and that such results are insufficient to support a stage interpretation. (2) He reanalyzes data from these studies and suggests alternative statistical procedures for future studies. The reanalysis evaluates the relation between pretest scores and change scores (pretest to posttest), and the resulting lack of a positive correlation is interpreted as a failure to support stages. Further, he argues that the use of a change score is not ideal, and proposes that regression techniques be used in future studies. (3) He criticizes the stage concept as being explanatorily circular because the explanation and the explicandum refer to the same variable. He concludes that the concept of stage as it is currently used in developmental theory is unacceptable. (4) Finally, he suggests that by focusing on the 'pseudo-issue' of stage versus learning, current investigations of cognitive development have missed the real issue: whether development is a consequence of rule-governed learning or contingency-shaped learning. With regard to this latter proposal for restructuring the issues involved in developmental research it should be noted that, although early in the article *Brainerd* [1977, p. 930] suggests that his methodological alternatives are useful for 'estimating the learning-stage relation', it is clear from his later discussion that the methodological proposals are to be understood as appropriate for investigating the issue of rule-governed versus contingency-shaped learning [p. 937].

The validity of *Brainerd's* arguments depends on three major requisites: (1) the validity of his criticisms of the stage concept as being explanatorily circular; (2) the coherence of his structuring of the relevant issues in terms of stage versus learning and rule-governed versus contingency-shaped learning, and (3) the appropriateness and statistical soundness of his methodological proposals. In what follows, we will contest the validity of these arguments at all three points – the criticisms of stage, the structuring of the issues, and the methodological proposals – finding errors of (or errors derived from) tenets of logical positivism (along with a few others) at each of those points.

Circularity, Explanation, and Cognitive Stages

In this section we first critically examine the argument that the concept of stage, as it is currently used, is circular. Then we suggest that this argument for the circularity of stage explanations is derived from a restricted, antecedent-consequent causal definition of explanation, and we provide evidence that this is the definition of explanation adhered to in *Brainerd's* [1977, 1978] articles. That valid explanation must be of the antecedent-consequent form is an easy and not uncommon conclusion from positivism's attempt at a rigorous empiricism. We then discuss one important consequence which can arise from the antecedent-consequent definition: a concept of cognitive stage as explanatory only if stages are the consequence of neural maturation. Finally, we comment on some alternative definitions of explanation and how they might apply to stages.

Brainerd [1977, p. 920] contends that 'the central explanatory construct, stage or level of cognitive development, is logically unacceptable. Any attempt to use it to explain observed differences in children's learning suffers from blatant circularity'. His argument, basically, is that:

> 'To avoid circularity in a learning experiment, we must have procedures for measuring our explanatory constructs that are independent of our procedures for measuring learning ... In all of the conservation experiments [reviewed], the only evidence about subjects' stages of cognitive development has come from scores on conservation tests. The only evidence that learning has occurred has come from scores on the same tests. Therefore, to say that some children learn more about conservation than others because they are at more advanced stages of cognitive development is not an explanation at all. It is a circular statement masquerading as an explanation [*Brainerd,* 1977, p. 936].

This conclusion is based on two errors: an inappropriate application of the concept of circularity, and the use of an overly restricted concept of explanation.

Circularity

Brainerd makes reference to *Hempel* [1965], *Nagel* [1961], and *Ebel* [1974] in advancing his concept of circularity, a concept that can be summarized by the statement, 'The most basic rule is that an explanation and its explicandum must not refer to the same variable' [*Brainerd,* 1977, p. 935]. However, the assertion that a circular explanation is one in which the explanation and the explicandum refer to the same variable is not borne out by either *Hempel* [1965] or *Nagel* [1961]. The condition that an explanation not be circular is, in fact, the condition 'that the explicandum does not logically imply the [explanatory] premises' [*Nagel,* 1961, p. 36]. A logical implication is not at all the same thing as referring to the same variable. It is impossible, in fact, for a set of locially valid explanatory premises *not* to refer at some point to the same variable as that which is to be explained. If reference to a variable were contained nowhere in the explanatory premises, then it could not be validly introduced in any logical deduction from those premises, and they, therefore, could not serve as an explanation of any fact which did contain essential reference to that variable.

Given *Brainerd's* definition of circularity, the conservation experiments are criticized because the explanatory assignments and the to-be-explained learning both refer to the same variable, that is, to the particular conservation test under consideration. But, examined from the other direction, if reference to the same variable in an explanation and an explicandum *did* constitute logically circular tautology, then test-retest reliability would be tautologically guaranteed. Similarly, an explanation of current air pressure that made any reference to previous air pressure would be circularly invalid, and the same would be true for any other variable which might be substituted for air pressure: any variable whose current value is in fact causally dependent on past values of that same variable could not, within this conception of circularity, be explained in terms of any of those past values upon pain of circularity.

Perhaps the concept of a variable has been confused with that of a measurement; perhaps an explanation that referred to the same measurement as its explicandum would in fact be circular: to attempt to causally explain a given measurement in terms of that same measurement certainly has a superficial appearance of circularity. Such a revision of *Brainerd's* concept of circularity, however, no longer supports his criticisms of the stage concept, and furthermore, the revision is still not a valid definition of circularity. 'Sometimes an event is explained by means of hypotheses for some of which the fact of its [the event's] occurrence affords the only available evidential support ... An explanatory argument of [this form] is not for that reason circular or pointless' [*Hempel,* 1965, p. 372f.]. The point that *Hempel* is making depends on a distinction between an explanation *that* an event occurred and an explanation of *why* it occurred. In particular, that it occurred may be part of the evidential support for the explanation of why it occurred. *Hempel* uses the example of spontaneous combustion in a haystack for which the fact that the haystack is burning provides the evidential support for the hypothesis that the conditions for spontaneous combustion occurred. To conclude, the criticism of the stage concept as being explanatorily circular is invalid, because it depends on a definition of circularity that is invalid.

Antecedent-Consequent Explanation

On what might this invalid definition of circularity be based? What could elicit it and make it seem reasonable? We submit that it is dependent on a particular conceptualization of explanation, namely, the strict antecedent-consequent form. Although the antecedent-consequent causal form of explanation is the most common form in psychology, it is not the only model of valid explanation. It is an inappropriate model for the explanatory claims of Piagetian cognitive stages, and so such a model can result in unfortunate distortion of the Piagetian position. More appropriate models of explanation for *Piaget's* theory will be discussed at the end of this section.

Nowhere does *Brainerd* explicitly define his conception of explanation. However, the assumption that he restricts valid explanation to the antecedent-consequent causal form provides coherence to some of *Brainerd's* other definitions and discussions as we will explicate below. First, the assumption is consistent with *Brainerd's* definition of circularity: the simplest causal explanatory paradigm involves one antecedent variable and one consequent variable, and *Brainerd's* definition of circularity can be seen as an attempt to accept only extensions of that paradigm.

Second, in support of the assumption that the restriction to the strict antecedent-consequent causal form of explanation is implicit in *Brainerd's* arguments, we note that this form of explanation is consistent with his specific criticisms of the stage concept. Stages are not causally antecedent variables, nor are they defined in terms of such variables, and so stages violate the causal model. From the perspective of a strict causal paradigm, it might well appear circular to explain scores on a concept learning (conservation) test partly in terms of scores on an earlier administration of the same test. An even more general case for the claim that the charge of circularity derives from an implicit assumption of antecedent-consequent explanation can be made by examining explanations in which scores from a test of one concept are explained in terms of scores from a test of another concept, i.e., when the explanation and the explicandum are neither the same variable nor the same measurement. This is the form of explanation when scores on a conservation task are explained in terms of scores on a different task hypothesized to assess the same stage of development. In such cases, *Brainerd* [1977, p. 936] claims that 'Any attempt to explain observed differences in learning by invoking the stage construct amounts to explaining scores on one concept test by appealing to scores on another concept test. Since the choice of which scores are to serve as the explanation and which scores are to serve as the explicandum is arbitrary, an explanation of this sort is circular. The circularity is merely less blatant than in experiments in which scores on the same test are both the explanation and the explicandum.' The use of the concept of circularity in this passage is not consistent with *Brainerd's* own definition, but it is consistent with the concept of circularity as a violation of the strict causal paradigm in which none of the concept test scores represents a cause of any of the others. To *Brainerd* [1977, p. 936], conservation test scores represent 'dependent' (consequent) variables for which stages are supposed to be explanatory, but stages are not antecedent in any causal sense. 'Although Piaget wishes that his stages should be regarded as explanations of behavior, he has not tied them to specific antecedent variables whose measurement procedures are well defined' [*Brainerd,* 1978, p. 36].

A third point consistent with *Brainerd's* interpretation of stages as needing to be causally antecedent in order for them to be explanatorily acceptable is the espousal of a neurological interpretation of stages. 'The circularity problem will not be satisfactorily disposed of unless a neurological basis can be found for cognitive stages' [*Brainerd,* 1977, p. 938]. A neurological interpretation of stages would provide the 'necessary' causal antecedent interpretation of stages. *Brainerd's* commitment to a neurological interpretation is also evident in his presentation of a strict neural-maturation readiness model (toilet training) as capturing 'the essence of the developmental approach to concept learning' [*Brainerd,* 1977, p. 920] and, in a somewhat more complex manner, in his discussion of 'some methodological caveats about conservation-learning experiments with preschoolers' [*Brainerd,* 1977, p. 933].

Brainerd draws a contrast between an inherent inability to learn and an inability due to the lack of various prerequisites to learning, and suggests that it is impossible to test for an inherent inability to learn unless the possibility of a lack of prerequisites is adequately controlled for. He proposes some methodological means for such control.

> 'Studies designed to examine conservation related behavioral aptitudes have repeatedly shown that sophisticated attentional, linguistic, and perceptual skills are prerequisites for passing items on conservation tests. Obviously, children must possess these aptitudes if they are to benefit from training ... This assumption is rarely justified with preschoolers. Consequently, one of these three strategies [which either compensate for or avoid such deficiencies] must always be adopted in conservation-learning studies with preschoolers ... (Otherwise) any failure to produce conservation learning may be reasonably interpreted as having resulted from an absence of prerequisites, rather than from any inherent inability to learn' [*Brainerd,* 1977, p. 933f.].

The argument, then, suggests compensating for absences of 'sophisticated attentional, linguistic, and perceptual' prerequisites in order to better test stage-related hypotheses concerning an 'inherent inability to learn'. There are actually two puzzles here, one of interpretation and one of logic. It is the interpretive puzzle that connects most directly to a neural conception of stages, so it will be addressed first.

'Sophisticated attentional, linguistic, and perceptual' prerequisites are in fact cognitive prerequisites, so *Brainerd* is in effect arguing for a cognitive prerequisite model. The puzzle derives from the fact that *Piaget's* model is itself a cognitive prerequisite model: 'Learning a concept belonging to any one of the stages is said to consist of learning how to apply cognitive structures to new content. If the structures appropriate to a given stage are present, learning processes can operate to induce the concept; otherwise they cannot' [*Brainerd,* 1977, p. 920]. So *Brainerd* is arguing that the possibility of cognitive prerequisites must be compensated or controlled for in order to test the hypothesis of cognitive prerequisites; the methodology assumes the truth of what it proposes to test.

The puzzle is resolved, however, if 'inherent inability to learn' is understood as *neural* inability to learn – a maturational inability: 'Piaget has always resisted the suggestion that his theory is maturationist' [*Brainerd,* 1978, p. 12]. Under this interpretation, an inability to learn due to a lack of stage-structured neural prerequisites is contrasted with an inability to learn due to a lack of cognitive prerequisites, and some form of methodological control for the alternative explanation would seem to be appropriate. Thus, *Brainerd's* proposals make sense given his neural interpretation of stages, but are inappropriate as stated given a Piagetian conception of stages, and are consequently inappropriate for testing a Piagetian stage hypothesis. It might be countered that *Brainerd's* prerequisites are not necessarily of the same kind as *Piaget's,* and that is true: a further distinction needs to be made here, but it is not to be made in terms of 'neural' versus 'cognitive'. An approach to this issue is suggested in the section on 'structuring of the issues'.

The second, logical, puzzle derives from the fact that the prerequisites and aptitudes that *Brainerd* discusses are circular, by *Brainerd's* definition, in exactly the same sense as are stages: neither are causal antecedents of what they are taken to explain. It has been argued that stages must be given a neural interpretation in order to avoid circularity. If this argument were valid, then it would apply equally well to cognitive prerequisites, and the methodological proposals would be left without any 'neural' versus 'cognitive' contrast to compensate for – both would be neural. On the other hand, if the argument were *in*valid, then still the contrast would not be present: the argument against stages would fail, and both would be cognitive. The puzzle, then, is that *whether or not* the argument against stages is valid, the neural versus cognitive contrast fails – the posing of the contrast involves a logical inconsistency. The reason for the puzzle, however, is clear: developmental psychology cannot be understood within a strict antecedent-consequent causal framework. Alternative forms of explanation are required, and if they are not allowed in stages, then they will reappear in guises such as 'cognitive prerequisites'.

Thus, we have presented a reconstruction of this argument in which a positivistic, exclusive commitment to the causal model for valid explanation has led to an incorrect definition of circularity, an unacceptable view of stages as explanatorily invalid, a non-Piagetian, exclusively neurological interpretation of *Piaget's* stages, and an inconsistent set of methodological 'caveats'.

Although *Brainerd* does not explicitly define his conceptualization of explanation, he comes quite close in the following:

'To be viewed as legitimate explanatory constructs, stages must satisfy at least three criteria. First, they must specify some target behaviors that undergo age change – i.e., they must be descriptive. Second, they must posit antecedent variables believed to be responsible for such changes which weld the stages into distinctive entities ... Third, procedures whereby the antecedent variables can be measured *independently* of behavioral changes must also be specified. This last requirement, which is essential to avoid circularity, is the litmus test for explanatory stages' [*Brainerd*, 1978, p. 5].

Taken alone, it is not clear from this passage whether or not the required stage antecedent variables must be *causally* antecedent or, for example, if *logical* antecedence in the explanatory deduction suffices. In the context of our discussion to this point, however, it seems clear that *Brainerd* is requiring a strict antecedent-consequent causal model as the paradigm for valid explanation. Furthermore, he cites approvingly *Ebel* [1974], [*Brainerd*, 1977, p. 935], in which we find: 'What is meant by "explain" in this context is to account for a specific instance in terms of a general law of this type: if-then always. Such laws relate a sufficient cause to its invariable effect' [*Ebel*, 1974, p. 486]. Here we have an explicit statement of the restriction of valid explanation to causal explanation. In addition, *Ebel* goes on to a discussion of explanatory inadequacy that has strong similarities to *Brainerd's* definition of circularity, though *Ebel* himself does not explicitly offer such a definition. Thus, although neither *Hempel* [1965] nor *Nagel* [1961] provide a source for *Brainerd's* conceptualizations of explanation and circularity, *Ebel* [1974] provides a source for both. With a source identified, and with the resultant strong coherence introduced into *Brainerd's* discussions of circularity and cognitive stages, the case for a vestigial assumption of a strict causal explanatory paradigm would seem to be made.

The difficulty, of course, is that causal explanations are not the only valid form of explanation. One important class of noncausal, but nevertheless potentially valid, explanations is dispositional explanations. Examples would include explanations involving such dispositional terms as brittle, soluble, malleable, electrical conductor, or magnetic [*Hempel,* 1965].

> 'Magnetization of an iron bar can manifest itself by the fact that iron filings will cling to its ends; but also by the fact that one of its ends will attract the north pole, the other one the south pole of a compass needle; and no less by the fact that if the bar is broken in two, each of the parts will display the two kinds of disposition just described for the whole bar... These two symptom sentences imply the general statement that any iron bar which satisfies the compass needle condition also satisfies the iron filings condition: and this surely is not a definitional [tautological, circular] truth, but a statement that has the character of an empirical law' [*Hempel,* 1965, p. 460f.].

Furthermore, even gravity is not 'tied to specific antecedent variables'; it is instead a dispositional property of matter and spacetime.

Causal and dispositional explanations do not exhaust the range of potentially valid forms of explanation. Indeed, *Brainerd* himself recognizes an instance of still another class, although he dismisses it with the term 'measurement sequence'. He argues that developmental assessments can yield behavioral sequences because the items used to measure the behaviors assure the sequence; such sequences are 'in the tests', not 'in the organism' – they are guaranteed by the nature of the measurements. 'A measurement sequence occurs whenever each item in the sequence consists of the immediately preceding item plus some new things. When behaviors are related in this manner, the only way that they can be acquired is in an invariant sequence' [*Brainerd,* 1978, p. 14]. This represents an instance of explaining why something is the case by showing that it could not *logically* be otherwise. 'If it is impossible even to imagine a sequence turning out some other way, *the sequence property does not require explanation in terms of antecedent variables and no research is needed to verify its universality.* The fact that the behaviors are acquired at all clearly *does* require explanation, but the fact that they are acquired sequentially does not' [*Brainerd,* 1978, p. 14].

If the possibility is recognized of there being 'structural sequences' analogous to *Brainerd's* 'measurement sequences', in which the sequencing is 'in the underlying cognitive structures' rather than 'in the tests' (e.g., the structures of formal operations operate upon the structures of concrete operations, and, therefore, cannot precede them), then we have available to us the form for an essentially Piagetian interpretation of *Piaget's* model in which the stages are explanatory. Specifically, each stage consists of certain underlying cognitive structures which manifest corresponding classes of behavioral dispositions, and the stages are related to each other in a logically necessary developmental structural sequencing.

We conclude that *Brainerd's* arguments against the explanatory acceptability of the concept of stage fail for three reasons: they are based upon an unacceptably narrow conception of explanation, an incorrect conception of circularity, and a misunderstanding of the nature of Piagetian stages. The three reasons, however, are not independent of each other: in particular, the positivistic restriction of valid explanation to causal explanation motivates the definition of circularity, which in turn yields the argument for the explanatory invalidity of stages and the corresponding neurological interpretation of *Piaget's* model. Next we will consider *Brai-*

nerd's structuring of the relevant issues in terms of stage versus learning and rule-governed versus contingency-shaped learning.

Structuring of the Issues

Brainerd [1977] asserts that the circularity problem renders studies which investigate the concept of cognitive stage inconclusive at best. For the reinterpretation of past studies and for future research he offers an alternative for the developmental approach to concept learning, suggesting that 'The issue is the distinction between rule-governed and contingency-shaped learning ... At present, [such] lines of investigation ... would seem to be a more fruitful route for students of children's learning to follow than would the question of stage versus learning. Until the circularity problem is cleared up, stage versus learning will remain a pseudo-issue' [*Brainerd,* 1977, p. 937].

Our objection to this structuring is simply that the issues do not fall into mutually exclusive categories in the way that *Brainerd* describes. We will argue first, that a stage approach subsumes a learning approach, and second, that a rule-governed learning approach subsumes a contingency-shaped learning approach. Thus we will argue that the oppositions of one position versus another that *Brainerd* proposes are not fully coherent, and therefore that the conclusions he bases upon those oppositions do not follow. We will conclude this section by proposing an alternative set of oppositions that may be used in examining the issues *Brainerd* addresses.

We have already seen that *Brainerd's* stage versus learning opposition is one in which neural prerequisites to learning ('inherent inability to learn') are contrasted with 'behavioral' prerequisites to learning ('absence of prerequisites'). If we adopt a more accurate cognitive interpretation of Piagetian stages, and accept the cognitive nature of the 'sophisticated attentional, linguistic, and perceptual' prerequisites [*Brainerd,* 1977, p. 933], the opposition as *Brainerd* states it disappears. Stages are not in opposition to learning; they are precisely cognitive prerequisites to and foundations for learning. *Brainerd's* impressions to the contrary are directly traceable, via the neurological interpretation and circularity criticism of stages, back to the strictly causal explanation paradigm. A genuine oppositional position might be to contend that learning involves no cognitive prerequisites whatsoever, which is not really a viable alternative, or that those prerequisites are not organized as stages. Although this latter position is a viable alternative to cognitive stage models, it is not one that *Brainerd* develops. Thus, *Brainerd's* stage versus learning issue is a pseudo-issue, but not because of the circularity of stages.

Our objection to *Brainerd's* opposition between rule-governed and contingency-shaped learning is similar: the opposition does not exist in the way *Brainerd* suggests. Specifically, a rule-governed learning model does not deny the facts of contingency-shaped learning. On the contrary, rule-governed learning models incorporate the facts of contingency-shaped learning as the learning of certain (rather simple) rules. It is not clear what contrast *Brainerd* is aiming for here, but it seems most likely that he has in mind some sort of 'mental contents' learning versus 'associative' learning (as if associations were *not* 'in the mind'), which, if true, constitutes not just a vestige of logical positivism, but of that particular positivistic legacy, behaviorism.

Rule-governed learning models conflict with contingency-shaped models only in the sense that rule-governed learning models contend that human behavior cannot be *fully* accounted for without taking into consideration more complicated rules than those which exclusively contingency-shaped learning models are capable of modeling. Similarly, stage models conflict with learning models only in the sense that they contend that human behavior cannot be *fully* accounted for without taking into consideration the stage structuring of cognitive prerequisites to behavior. In neither case is there an intrinsic opposition of one model with an incompatible alternative; rather, there is a claim that one model must be incorporated into the framework of the other. In both cases the only potential opposition involved is with a position which opposes the necessity or the coherence of such an incorporation.

Thus, a more acceptable oppositional structuring of relevant issues is as follows: (1) learning is of complex rules as well as of simple rules, or learning is only of simple rules; (2) learning does or does not have cognitive prerequisites; (3) those cognitive prerequisites are or are not themselves rules, and (4) those cognitive prerequisites are or are not organized as stages.

Clearly, explications of these oppositions and of the concepts involved in them are required. The point at hand, however, is the failure of *Brainerd's* contentions that the stage versus learning opposition is a pseudoissue, and, therefore, that the relevant opposition is that of rule-governed versus contingency-shaped learning. That is, even if *Brainerd's* contentions against stages were accepted, his delineations of the crucial issues still would not follow: The conceptual framework within which he criticizes stages is the *same* as that within which he derives his understanding of the crucial issues, and it is in this fundamental framework that we find the deepest influences of vestigial logical positivism. We turn next to the methodological proposals which *Brainerd* has made.

Methodological Proposals

Brainerd's methodological comments and proposals derive in many respects from his conceptualizations and criticisms of stages, and thus share the same limitation of a strictly causal explanatory paradigm. There is an additional positivistic error introduced here, however: a conception of measurement as restricted to continuous quantitative scales. Such a conception of measurement is present in positivistic writings, but *Brainerd's* assertions are more directly a vestige of operational definitionism. We also comment on a few additional aspects of *Brainerd's* discussion that deserve attention.

First, *Brainerd's* methodological comments are examined as they apply to the influence of stages on learning, the topic to which *Brainerd* originally directed his comments. (This issue is the fourth described at the end of the preceding section: Are cognitive prerequisites for learning organized as stages?) However, toward the end of his article *Brainerd* [1977, p. 937] asserts that available data cannot answer this question and that the techniques that he is proposing instead are appropriate for determining whether concept learning is rule-governed or contingency-shaped learning. As *Brainerd* construes this question, it is a combination of the first and second issues described at the end of the previous section: Does concept learning involve learning complex as well as simple rules, and does concept learning have cognitive prerequisites? Therefore, we also examine *Brainerd's* proposals with respect to these two issues.

In examining *Brainerd's* methodological comments, we begin with his specific critique of and suggestions for the studies reviewed. *Brainerd's* most general critique of the statistical method employed in these investigations (investigations of the relation between stage of development and benefit from training) seems to be well founded: a simple correlation between level of knowledge before training and level of knowledge after training is not acceptable evidence for stage effects on learning. It is, in fact, a general result to be expected on strictly *methodological* grounds, independent of its relationships to *any* theories of learning, cognition, *or* development.

Contrary to the results of such simplistic analyses, *Brainerd* concludes in his ensuing reanalysis of the data that there is in fact *no* evidence of a relationship between stage classification prior to conservation training and susceptibility to such training. *Brainerd's* reanalysis, however, suffers from its own problems. In presenting his reanalysis of these studies, using change scores to assure that the stage assignment measure is independent of the measure of amount of learning, *Brainerd* is careful to note that this change score approach is not ideal. In addition to commenting on the inherent statistical problems with the change score approach, he also attempts to address the problem of ceiling effects in the reanalysis. In the two studies for which raw data were still available, observed scores were compared to the theoretical maxima on the test. However, this is an inadequate test for ceiling effects. A psychological ceiling often exists at some point below the theoretical maximum score. In the case of the data *Brainerd* reanalyzed, multiple trials were employed with young children, and it is quite probable that their attention wandered on some trials. Thus, some of these children's scores may have been below the theoretical maximum and yet they may have fully possessed the skills which the test was designed to tap.

More troublesome than potential ceiling effects is the problem of regression to the mean, which *Brainerd* did not discuss. When any test whose reliability is less than 1.0 is employed, it is to be expected that any subgroup selected for being below the mean on that test will on the average show an improvement on the subsequent test. Thus, the mean improvement scores of the stage I subjects, who were selected for being low on the pretest, may be inflated relative to the stage II subjects. A direct method for avoiding this problem is to select groups of subjects using one pretest but assign each subject a pretest score based upon a second pretest. Of course, *Brainerd* is not responsible for the design of the studies whose data he has reanalyzed, and he urges caution in interpreting his own reanalysis. Our discussion provides still further reason for such caution. We agree with *Brainerd's* [1977, p. 930] conclusion that the data from his reanalysis 'do not indicate that children's susceptibility to conservation training depends on their pretraining stage', but the potential ceiling effects and the problem with regression to the mean mitigate the importance of this apparent failure to confirm the Piagetian hypothesis.

Our most serious objections, however, concern not these comments of *Brainerd's* concerning past studies, but rather his proposals for future studies. To avoid the statistical problems with change scores, *Brainerd* offers the procedure of comparing the slopes of the regression equations for the pretest and posttest scores of experimental (training) and control groups. This procedure would also eliminate the problem of regression to the mean, although not the concern about ceiling effects.

However, if we ignore the statistical problems, the viability of *Brainerd's* reanalysis and procedural recommendations to test *Piaget's* theory (and stage theories in general) depends on his claim that the theories predict that 'the higher the stage, the greater the learning' [p. 921]. This is not the same as the Piagetian claim that, for a particular type of concept, training experiences are more likely to have an impact on children who are at an intermediate level of thinking [*Inhelder* et al., 1974; *Piaget,* 1970]. The disagreement is not simply that *Brainerd's* simplification suggests that the potentiating effect of development for learning from these training experiences is predicted to increase without bound. Rather, from our perspective, the important point in the Piagetian prediction that *Brainerd's* simplification fails to capture is that something very specific is meant by children at an intermediate level of thinking. In particular, Piagetian theory posits a transition period of high disequilibrium in which the old understanding of a concept or method for solving a problem has been detected as being inadequate, but a more advanced alternative understanding is not yet fully developed. Such disequilibrium is presumed to constitute a state of high susceptibility to new learning. The relevant issue, then, is not so much 'who learns the most' (as with change scores and their conceptual equivalents) as 'who is most susceptible to new learning'. *Brainerd* can find support for his essentially quantitative version of the Piagetian prediction among the writings both of some Piagetians and of other interpreters of *Piaget,* but we believe that this interpretation is wrong and is the result of imprecision of writing and/or translation. Both *Brainerd's* change score reanalysis of old studies and his regression analysis proposal for future studies suffer from this misinterpretation of *Piaget.*

This error in interpretation of the Piagetian prediction is strongly related to another major problem in *Brainerd's* proposal: the attempt to impose a continuous quantitative scale (implicit in the proposal to use regression techniques) on a developmental sequence which the theory being tested hypothesizes to change with qualitative shifts. To test this aspect of Piagetian theory, the scale used must divide subjects according to the qualitative stages both for initial assignment and assignment after training. If this were not done and some quantitative scale were used on which the within-stage variance were high, then the Piagetian hypothesis might be inappropriately rejected. In general, the problems both in some of the work *Brainerd* criticizes and in *Brainerd's* alternative proposal seem to result from translating *Piaget's* theory, which attempts to address the qualitative structure of what is learned, into a theory which predicts quantitatively how much is learned. A more appropriate test of *Piaget's* theory is to compare the number of subjects in each stage who improve by moving to a higher stage. This directly tests the question of which group is more likely to benefit rather than the question of which group benefits more. The psychometric problem of making veridical stage assignments may be troublesome, but no more so than imposing a quantitative scale, and the result is information that is appropriate to the theory under investigation.

In addition to the points above, one can question *Brainerd's* analysis of training effects. *Brainerd* did not consider the nature of the training in making his predictions about what the effect of training should be within different theoretical systems. However, central to the Piagetian approach is the notion that the training techniques must be structurally matched to the child's current cognitive level. Thus, the Piagetian prediction is that, given appropriate training, a transition group of children is more likely to benefit than a nonconserving group. However, with training which focused exclusively on the inconsistencies in the nonconservers' solutions to conservation problems and which does not provide the necessary range of experience for developing the complete concept of conservation, a viable Piagetian prediction is that the nonconservers would be more likely to benefit.

Toward the end of his article *Brainerd* argues that the type of data he reviewed is not appropriate for assessing stages in cognitive development, but is appropriate to assess whether Piagetian concepts are an example of (complex) rule learning or of contingency-shaped learning. 'If learning the concept is an instance of rule-governed behavior, the correlation (between pretest and amount learned) should be positive ... But if learning the concept is an instance of contingency-shaped behavior, there is no reason to expect a positive correlation' [*Brainerd,* 1977, p. 937]. In this proposal, *Brainerd* explicitly conflates the issue of cognitive prerequisites for concept learning with the issue of the involvement of complex rules in concept learning, accepting evidence of the former (correlations between pretest and amount learned) as evidence for the latter (rule-governed learning). If we rectify this conflation, however, we can ask if *Brainerd's* methodological proposals are more appropriate for testing these issues than they were for the stage issue. We suggest again that there is a measurement problem. *Brainerd* [1977, p. 927] argues, 'Since it is assumed that subjects must know a rule or rules in order to learn, the probability that the relevant rule or rules are known increases as a function of pretest performance.' However, by the same argument used in discussing the stage issue, if the amount of variance accounted for on the pretest by the relevant prerequisite knowledge is small, then an inappropriate rejection of its importance might result. We have argued that *Brainerd's* approach cannot be used to reject the specific hypothesis that there are cognitive prerequisites for concept learning that are organized in stages. By the same reasoning, this approach cannot be used to reject the more general claim that there are cognitive prerequisites for concept learning.

Conclusions

We have critically examined *Brainerd's* argument that the concept of stage is circular as it is currently used in discussion of concept learning, his restructuring of the issues involved in concept learning, and his methodological critique of and recommendations for investigation of concept learning. At each point we have uncovered problems. We have demonstrated that the argument for circularity is flawed, the restructuring of the issues is in need of modification, and the methodological recommendations are inappropriate.

More fundamentally, we have found two major incorrect assumptions, both historically derived from logical positivism, that account for the majority of these errors. One is an exclusive reliance on quantitative conceptions of measurement, with a consequent misconstrual of the essentially qualitative predictions involved in Piagetian stages. The other much deeper assumption is that of a strictly causal paradigm for valid explanation. This yielded an invalid and inconsistently applied definition of explanatory circularity, an incorrect criticism of stages as being circular, a non-Piagetian neurological interpretation of stages, invalid and inconsistent methodological caveats, an incoherent structuring of the issues involved in developmental research, and, eventually, provided support to the non-Piagetian quantitative interpretation of Piagetian stage predictions.

The specific rebuttals of *Brainerd's* arguments and positions are worth doing for their own sake. The explications that underlie those rebuttals, however, contain a much more important message: that vestiges of logical positivism still persist, that they can be implicit and non-obvious, that they can have extensive ramifications, and that they can be destructive in their consequences.

Several recent discussions in the developmental literature demonstrate that the issues raised here have relevance beyond *Brainerd's* particular conceptions of stages. One example is found in the dispute concerning the use of correct explanations as criteria in assessing conservation: the argument against explanations seems to view the issue from the perspective of an implicit operational definitionism, and, therefore, focuses primarily on psychometric considerations. The rebuttals have correctly seen that this position is inadequate, but have failed to address the presuppositions underlying that inadequacy. Because these issues have not become explicit in the debate, it has had something of the air of two sides talking past each other, and, understandably, has produced no resolution [see, e.g., *Brainerd,* 1973; *Pinard,* 1981]. Another legacy of logical positivism that has bedeviled developmental psychology is a naive inductivism that asks for proof of hypotheses, rather than for their testability. (This is parallel to the demand of operational definitionism for observable measures of concepts, rather than for the testability of hypotheses involving them.) An example is the concern for the empirical demonstration of stages and their unique sequencing [e.g., *Flavell,* 1977], rather than for whether or not they lead to testable hypotheses [see *Campbell and Richie,* 1983]. Other examples permeate developmental psychology (and psychology in general), but, as reflected in this discussion, the clearest examples seem to come from cognitive development because of a more explicit focus on *explanations* of development and its sequencing.

We have disputed the substance of *Brainerd's* argument at many points, but our critical review does not diminish the importance of the major thrust of his article. He set out to question the appropriateness of the current approach to concept learning, and to argue that more care is needed at both the theoretical and empirical levels. We hope that our comments will be viewed as supporting, and even strengthening, this thesis.

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