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The stage question in cognitive-developmental theory

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Abstract: The term "stage" appears to be used in three general senses in theories of behavioral development: (*a*) as a metaphor: (*b*) as a description of behaviors that undergo age change; (*c*) as an explanation of age-related changes in behavior. Although most existing stage models are purely descriptive, a few of them purport to have explanatory power. One such model, Piaget's stages of cognitive development, is considered in this paper.

To be viewed as potentially explanatory, a stage model must describe some behaviors that undergo age change, posit antecedent variables believed to cause the changes, and provide procedures whereby the behavioral changes and the antecedent variables can be independently measured. Piaget's stages seem to satisfy some but not all of these requirements. Piaget's stages describe many age-related changes in behavior, and some antecedent variables have been proposed. However, procedures do not exist for measuring the two factors independently. In lieu of such procedures, Piaget has outlined a "program" of five empirical criteria whereby the reality of his stages can ostensibly be verified. Some objections to these criteria are considered.

The five criteria in Piaget's program are invariant sequence, cognitive structure, integration, consolidation, and equilibration. Three of the criteria (invariant sequence, integration, and consolidation) lead to the same sorts of empirical predictions (culturally universal sequences in the acquisition of certain behaviors). Such predictions are subject to the objection that Piagetian invariant sequences are often measurement sequences. A measurement sequence is said to occur when some late-appearing behavior consists of some earlier-appearing behavior plus additional things. The cognitive structure criterion is subject to at least three criticisms: First, it yields, at most, descriptions of behavior; second, these are often nothing more than descriptions of task structure; third, they cannot be regarded as unique to the given stages for which they are posited. The fifth criterion, equilibration, generates some predictions that might be considered as prima facie evidence for the existence of stages. However, these predictions conflict with the current data base on Piaget's stages.

It is concluded that there is no compelling support for Piaget's hypothesis that his cognitive stages do more than describe age-related changes in behavior. Since explanatory statements involving stages appear with some regularity in Piagetian and neo-Piagetian writings, there are grounds for supposing this conclusion to be nontrivial.

Keywords: cognitive development, cognitive structure, developmental stages, invariant sequences, Piagetian theory.

The aim of this paper is to generate discussion on a topic that, has seemingly been frozen in cross section for the past several years: In what sense or senses is it meaningful to speak of "stages" of cognitive development? My approach is to pose this question with reference to the preeminent stage theory of our time, Piaget's (e.g., 1950, 1970a; Piaget & Inhelder, 1969). Though it will take some space to develop, my basic line of argument is quite simple. Whereas Piaget's stages are perfectly acceptable as descriptions of behavior, they have no status as explanatory constructs. This will, no doubt, seem a thoroughly unremarkable claim to many readers. But it leads to an interesting conclusion. Statements to the effect that children do or do not do such and such because they are at some given stage of cognitive development are meaningless – or, more precisely, they are circular. Since statements of this sort are often made both in Genevan writings and in neo-Piagetian theorizing, this conclusion would seem to be nontrivial.

At present, stage model-building à la Piaget is a popular pastime in the literature on human cognitive development. I sometimes have the subjective impression that the density of such models in developmental journals must average about one per issue. For this reason, it would be rash to hope that many readers will be favorably disposed toward the arguments presented below or will be inclined to accept my conclusions. However, the ultimate fate of these arguments and conclusions is unimportant. This paper will have more than served its purpose if it acts like something of a Rorschach stimulus in reawakening discussion of the stage question and prompting other writers to formulate new proposals about how to identify discrete changes in cognitive development.

General desiderata

One may distinguish three general uses of the stage construct in theories of behavioral development: aesthetic, descriptive, and explanatory. In the first case, stages are ideals that do not necessarily refer to anything definite or measurable in development. The term "stage" is used metaphorically because it tends to evoke certain images in readers. It is common to cite Erikson's (1950) theory of psychosexual development as an example of this use of stages. But G. Stanley Hall's model of mental development, with its allusions to sociocultural evolution, would do just as well. Among more recent theories, a case could be made for the conclusion that Kohlberg's (e.g., 1963, 1968) stages of moral development are primarily aesthetic. Kohlberg diagnoses these stages with a projective technique called the Moral Judgment Scale. However, the diagnosis for any given child is not intended to imply that certain moral behaviors will be observed in that child (Kurtines & Greif, 1974). Also, the model apparently does

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not predict that all, or even most, children go through all of the stages (Kurtines & Greif, 1974).

When stages are used descriptively, they refer to precise and measurable aspects of behavioral development. In this usage, which is by far the most common of the three, stage is basically a synonym for behavior, that is, given some feature of behavior that is known to change within some age range, these changes are sliced into a small number of chronologically ordered segments that are called stages. An important characteristic of descriptive stage models is that they are typically arbitrary. Decisions about how to slice up the stream of behavioral change are based on external criteria such as economy and elegance. Hence, there might be several different models that could be posited, all of which would be equally valid descriptions of changes in the organism.

Most of the examples cited in Kessen's (1962) earlier exposition of stage theories fall within the descriptive category. Since this category is large, it goes without saying that the models in it vary along a number of dimensions. Apart from the specific behaviors being described (cognitive, social, perceptual, etc.), perhaps the most notable dimension is the degree of abstractness with which descriptions are formulated. In some cases, stage-defining behaviors are described very concretely. The stages reported by Piaget for the development of concepts such as number (Piaget & Szeminska, 1941), classification (Inhelder & Piaget, 1964), quantity (Piaget & Inhelder, 1941), ordering (Inhelder & Piaget, 1964), and so on, come immediately to mind. For example: Stage I of cardinal number means that children cannot construct one-to-one mappings of the elements of one set onto the elements of another; Stage II means that one-to-one mappings can be constructed but children cannot conserve these mappings mentally when they are perceptually destroyed; Stage III means that one-to-one mappings can be both constructed and mentally conserved (Piaget & Szeminska, 1941, Part II). Stages of this sort represent the concrete end of the descriptive continuum. We begin to move toward the abstract end when emphasis is placed on isolating communalities and patterns in diverse classes of behavior. These patterns are then used to define stages. There is more than one way to go about abstracting such patterns. Factor analysis might be used (e.g., Buss & Royce, 1975), in which case the patterns would be called factors. Abstract algebra might be used (e.g., Piaget, 1949), in which case the patterns would be called operations or cognitive structures. Information processing analysis might be used (e.g., Klahr & Wallace, 1970), in which case the patterns would be called rules or rule systems. Regardless of the methodology employed, it is important to bear in mind that the results are descriptions of behavior, albeit rather abstract ones.

The reversibility rules, mental operations, and cognitive structures that Piaget uses to define his sensorimotor, preoperational, concrete-operational, and formal-operational stages are the best-known illustrations of the abstract end of the descriptive continuum. For sheer elegance and precision, however, modern information processing models are undoubtedly the state of art. These models reflect Simon's (1962) characterization of the task of stage definition as a process whereby "We select certain instants in the course of . . . dynamic change, take 'snapshots' of the system at those instants, and use these snapshots as descriptions of the system at a particular stage of development" (1962, p. 130). A rough account of the information processing approach might run as follows. First, we examine age-related changes in behavior on a problem-solving task or some family of similar problem-solving tasks. We look for stable individual differences in the patterns of behavior elicited by the tasks, which are also correlated with age. (The patterns tend to succeed each other in time.) Each isolable pattern is formulated as a system of rules. The rules are "computational," not in a numerical sense, but in the sense that they will generate desired behavioral outputs from given inputs. The final step is to realize the rule models in a program of some sort, such as General Problem Solver (Newell, Shaw, & Simon, 1960). A sophisticated realization of the models would consist of two parts: (*a*) programs describing the system of rules for each behavioral pattern and (*b*) programs describing how the program for a given rule system can be obtained by modifying the structure of the program for some other rule system.

Finally, there is the explanatory use of stages. 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, that is, they must be descriptive. Second, they must posit antecedent variables believed to be responsible for such changes that weld the stages into distinctive entities. These variables will presumably be maturational and experiential in nature, where variables of the latter sort include both influences being manipulated by the environment (e.g., reinforcement contingencies) and influences being manipulated by the organism (selective attention, motor activity, etc.). 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. It is one thing to describe a series of behavioral changes and to propose some possible causes; it is far more difficult to say how the latter may be measured without measuring the former. But if this is not done, statements of the form "children do x because they are in stage S" merely say "children do x." Psychoanalytic theory provides a textbook illustration. Consider the mother who asks why her one-year-old son sucks his thumb and is told "because he is in the oral stage." The oral stage satisfies the first two explanatory requirements. It describes a class of infantile behaviors centered on the mouth, and it describes psychodynamic processes believed to cause these behaviors. But the methods whereby oral behaviors and psychodynamic processes might be independently measured remain a mystery. Hence, the connection between orality and psychodynamics is conjectural, and "he sucks his thumb because he is in the oral stage" is simply a paraphrase of "he sucks his thumb."

Explanatory stages, unlike descriptive ones, are definitely not arbitrary. The specific behaviors assigned to a given stage are not there at the whim of the theorist. The organism now has something to say about the matter. It is the second and third criteria that make explanatory stages nonarbitrary. The behaviors in any given stage go together *naturally* by virtue of their common antecedents. For this reason, statements of the form "subjects do x because they are in stage S" are not vapid. They assert that x occurs because the antecedent conditions for S are present (and, by implication, those for $S + 1$ are absent).

Since it is far easier to describe behavioral development than it is to explain it, unambiguous examples of explanatory stages are not easy to come by. However, there are a few candidates. On the maturational side, we might consider Coghill's (1929) five-stage model of motor development in *Amblystoma* embryos. Each stage consists of behavioral descriptions (muscular contraction, flexure, coiling, reversal of flexure, and swimming) together with descriptions of neurological events whose measurement procedures are well-defined. Thus, if one asks why certain embryos contract when their skin is lightly stimulated, the statement "because they are in the flexure stage" has explanatory power because "flexure stage" refers to antecedent neurological variables that can be measured independently of contraction. On the experiential side, we might consider Bijou and Baer's (1963, 1965; Bijou, 1975) three-stage model of human psychological development. These stages, like Coghill's, have prima facie explanatory power because they consist of descriptions of both behavior and of antecedent variables. In this case, the latter are reinforcement contingencies operating in children's everyday environments. The second stage in Bijou and Baer's model, the so-called basic stage, corresponds roughly to the preschool years. During this age range, children are far less dependent on adults than during infancy. If one asks why certain children are showing increased independence from adults, the statement "because they are in the basic stage" has explanatory power within the Bijou-Baer theory. The statement says that certain antecedent events, reinforcement contingencies that parents and other adults are known to manipulate, have produced the behavioral effect. It is quite possible to measure the manipulation of these contingencies in given families by various techniques (parental interviews, observation of parent-child interaction in controlled situations, etc.) and to use this information to predict children's dependence behavior.

This brings us back to Piaget's stages of cognitive development and to the question I wish to examine in this paper: Is there any reason to suppose that these stages have explanatory power? Piaget's theory presents, I believe, an interesting example of stages that fall somewhere between pure description and true explanation.[1] On the one hand, Piaget wishes that his stages should be viewed as more than descriptive conventions: "to the extent that objectively certain stages exist (and this is indisputable in certain fields), they cannot be considered as a product of subjective cuts arbitrarily made by the research worker in a rigorously continuous development" (Piaget, 1960, pp. 12–13). But, on the other hand, his stages do not satisfy all of the criteria mentioned earlier. There are problems with both the second and third criteria. Concerning the former, it is true that Piaget has discussed several general factors responsible for cognitive growth (e.g., 1970a, pp. 719–726). However, a fine-grained analysis tying specific changes in given factors to specific stages has not been undertaken. Concerning the third criterion, procedures for measuring these factors independently of the behaviors that the theory is trying to explain are lacking. This lack is especially apparent for the factor deemed to be most important by the theory, equilibration. It should be possible to detect oscillations in equilibration and these oscillations should be functionally related to the presence of stage-defining behaviors. In Piaget's research, however, only age-related changes in behavior are studied.

Despite these difficulties, it would be premature to conclude that Piaget's stages are purely descriptive. He has outlined a "program" whereby, it is said, the reality of cognitive stages can be established (Piaget, 1960). The program consists of five empirical criteria. Piaget claims (e.g., 1960, pp. 12–13) that if data consistent with the predictions of the criteria can be obtained, we may infer that "objectively certain stages exist." I interpret this claim as follows. Although we may not know the specific antecedent variables that weld a given set of behaviors into a meaningful whole, these variables certainly exist if the predictions of the criteria can be verified. The task of sorting out the specific variables that go with each stage then becomes an empirical question. At first glance, this approach appears to save tremendous amounts of labor. We are allowed, for purposes of explaining behavior, to treat a given set of stages as "objectively certain" even though we do not yet know what the antecedent variables are or how to measure them. But if this approach is to work, we must make a key assumption, namely, the criteria in Piaget's program are sufficient to establish the existence of stages. Is this assumption actually justified? Insofar as I know, this question has not been carefully examined before.

Below, each criterion in Piaget's program is considered in turn. The principal aim is to determine whether any or all of them justify the assumption that stages exist. In each case, we shall want to know whether data consistent with a given criterion imply that stages have been identified or whether reasonable alternative explanations are available.

The program

The five criteria are these: invariant sequences, cognitive structure, integration, consolidation, and equilibration. Some of them are, for Piaget at least, more important than others. In particular, the first two apparently are regarded as more fundamental than the last three. There seems to be some consensus that Piagetian stages must stand or fall primarily on the sequence and structure criteria. Despite this fact, my approach will be the same for all five criteria. In each case, I shall ask whether the criterion provides prima facie grounds for believing that Piagetian stages constitute natural (i.e., nonarbitrary) groupings of behavioral traits. Although all five criteria are considered, the treatment of the last three criteria is very brief by comparison to the treatment of the first two. The reasons for this unequal weighting are, first, that the theory seems to rely primarily on the first two criteria and, second, that criticisms of the last three criteria have already been raised by other writers (e.g., Wohlwill, 1966).

Major criteria: sequence and structure

Invariant sequence. Judging from views expressed by other writers (e.g., Beilin, 1971; Kohlberg, 1968; Kurtines & Greif, 1974), this is far and away the most important criterion. We begin with a fairly typical formulation from Piaget's writings: "The *minimum* programme for establishment of stages is the recognition of a distinct chronology, in the sense of a *constant order of succession*. The average age for the appearance of a stage may vary greatly from one physical or social environment to another: for example, if the children of New Guinea, studied by Margaret Mead, manage to understand, like those of Geneva, certain structures of Euclidean geometry, they may do so at a much later or much earlier age. Whether older or younger is of little importance, but one could not speak of stage in this connection, unless in all environments the Euclidean structures were established *after* and not before the topological structures" (1960, p. 13).

These remarks, as well as other formulations of the sequence criterion (Inhelder, 1956, p. 85; Piaget & Inhelder, 1969), are somewhat misleading. They give the impression that it is the invariant succession of the *stages* whose chronology would be investigated. But this is clearly an erroneous impression, because it would smuggle stages into existence before any data are gathered. Since the sequence criterion is supposed to establish the existence of stages empirically, its predictions must not be directly concerned with the stages themselves; otherwise, it is a circular statement.

What the criterion actually says is that stages may be viewed as existing in some objective sense to the extent that the *behaviors associated with them* emerge in an order that cannot be altered by environmental factors. This leads to empirical predictions of the following sort. Suppose we have some set of stages S_1, S_2, . . . , S_n and procedures for measuring illustrative behaviors from each stage. Suppose we also have a sample of subjects from whatever nominal age range is spanned by the stages. If we administer tests for the behaviors to our subjects and scale the data, we should find that they appear in the order specified by the stages, that is, S_1 behaviors before behaviors from any of the $n - 1$ remaining stages, S_2 behaviors after S_1 behaviors but before behaviors from the $n - 2$ remaining stages, . . . , and S_n behaviors after behaviors from all of the $n - 1$ preceding stages. Moreover, this sequence should be culturally universal.

The verification of culture free sequences in concept development has been viewed by many (e.g., Beilin, 1971; Kohlberg, 1968; Tanner, 1956) as proof of the existence of stages. Tanner, for example, has remarked, "If I understand Mlle Inhelder and Professor Piaget correctly, one of the most cogent arguments for the existence of their developmental stages is that the sequence of them remains the same even if as a whole they are retarded or advanced . . . this seems to me a powerful argument in favour of the existence of mental stages, *and of their neurological bases*" (1956, p. 87, my italics). The reason for the acceptance of the invariant sequence criterion is, I believe, suggested by the italicized words in Tanner's comment. It is commonly supposed that if a sequence of behavioral acquisitions cannot be altered environmentally, the sequence must be under hereditary control: "Piaget's primary criterion for the theory of stages, the invariant order of structural achievement, to which Piaget accepts no qualification, suggests, in fact almost requires, an explanation defined in terms of genetic control" (Beilin, 1971, p. 178). Although Piaget has always resisted the suggestion that his theory is maturationist, he has acknowledged that, "the stages always appear in the same order of succession. This might lead us to assume that some biological factor such as maturation is at work" (1970a, p. 712).

Although he does not specify any exact maturational variables, they would presumably be neurological and hormonal events under the control of genes that are temporally linked to differentiation in the central nervous system. Finding the specific variables is an empirical question. For our purposes, what is important, is the implicit assumption that the existence of a culturally universal invariant sequence presupposes well-defined antecedent events of a maturational variety. According to this assumption, there is some chronologically ordered set of maturational events $\{M_1, M_2, \ldots, M_k\}$ and some chronologically ordered set of behavioral events $\{B_1, B_2, \ldots, B_k\}$ such that the two sets mesh as follows:

$$M_1 \rightarrow B_1 \rightarrow M_2 \rightarrow B_2 \rightarrow \cdots \rightarrow M_k \rightarrow B_k$$

Each M_j is the class of antecedent variables for the behavior class B_j. If this assumption is correct, then we can, in principle, measure these variables independently of behavior for purposes of establishing functional relationships. Some work along these lines has already been done for Piaget's concrete-operational stage (Kraft, Languis, Wheatley, & Mitchell, 1977). The assumption that invariant behavioral sequences imply sequences of antecedent maturational events was popularized by Arnold Gesell (e.g., Gesell, Thompson, & Amatruda, 1934) and, at first glance, it seems quite reasonable. If some developmental phenomenon cannot be altered by environmental influences, what is left but heredity? We may add to this initial impression the fact that substantial evidence bearing on maturational control of sequences in motor development has accumulated (for a review, see Carmichael, 1970). It is frequently the case, in lower vertebrates, that such sequences can be mapped onto maturational sequences in the central nervous system. But is this true in general? Are there behavioral sequences that do not involve underlying maturational sequences? There are, and it is such sequences with which Piaget's stages appear to be primarily concerned. To begin with, note that any statement to the effect that some sequence is culturally universal implicitly presumes that the sequence could have turned out some other way. If not, the statement is tautologous. 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.

If a behavioral sequence is not always "in the organism," in the sense of antecedent maturational events, where is it? Many times it is "in the tests," that is, it results from definitional connections between the behaviors being measured and, hence, it is guaranteed by the nature of our measurement operations. For this reason, I shall call such sequences *measurement sequences.*

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. This is because, logically, it is impossible to devise valid tests of later items that do not measure earlier items. Given two items, A and B, such that B consists of A plus some other things, there are only three possibilities: (*a*) children acquire neither A nor B; (*b*) children acquire A but not B; (*c*) children acquire both A and B and A precedes B. But it is inconceivable that some children acquire only B or that some children acquire both A and B but B precedes A. If either finding were observed, we would be forced to conclude that our A measure was prone to false negatives (subjects fail the test even though they have A) or that our B measure was prone to false positives (subjects pass the test even though they do not have B) or both.

Two arithmetic skills that children learn in elementary school, addition of integers and multiplication of integers, provide a classic example of a measurement sequence. Multiplication is defined in terms of addition. It is a special type of addition, namely, adding the same number to itself repetitively. To multiply, children must know how to add. If addition and multiplication tests were administered to large samples of elementary schoolers, we would expect to find children who could do both, children who could do neither, and children who could add but not multiply. We would not expect to find children who could multiply but not add. Arithmetic and algebra provide a plethora of measurement sequence illustrations (i.e., natural numbers vs. integers, integers vs. fractions, multiplication vs. factoring, division vs. square roots). In each case, the acquisition of the specific items needs to be explained in terms of antecedent variables, but the sequential linkage between them does not.

What about Piaget's stages? It is rarely possible to imagine predicted sequences turning out any other way. This claim is hardly unique to this writer. One finds, for example, the following remarks in a well-known paper by Flavell and Wohlwill: "Instances of this relation are both numerous and important. For example, concrete and formal operations are linked in just this way. Providing one accepts Piaget's characterization of what these operations consist of, it is logically possible for the child to be capable of the former and incapable of the latter, but not conversely. Formal operations are supposed to take products of concrete operations as their objects, and hence presuppose the capability to exercise these operations . . . the ability to multiply or coordinate two relations presupposes the ability to apprehend the two relations individually; the representation of class hierarchies implies the ability to represent a single class; and so on and on" (1969, p. 86).

A wealth of measurement sequences could be cited from Piaget's studies. However, it would probably be more useful to consider a detailed illustration involving the preoperational and concrete-operational stages. The bulk of Piaget's research on his theory of cognitive development has been devoted to describing the behavioral distinctions between these two stages (e.g., Flavell, 1963). Detailed investigations of numerical behavior (Piaget & Szeminska, 1941), classificatory behavior (Inhelder & Piaget, 1964), spatial behavior (Piaget & Inhelder, 1956; Piaget, Inhelder, & Szeminska, 1960), quantitative behavior (Piaget & Inhelder, 1941), and ordering behavior (Inhelder & Piaget, 1964) have all been reported. Several different tasks have been administered within each content area. For example, class inclusion problems, sorting problems, and matrix problems have all been administered within the classification domain. Age changes have been reported in terms of sequences of stages, with earlier ones corresponding to preoperations and later ones to concrete operations. In most cases, there are at least three stages: Stage I = preoperations; Stage III = concrete operations; and Stage II = a transition phase between preoperations and concrete operations. The stages frequently focus on two major traits – call them B_1 and B_2. Stage I is defined as the absence of both; Stage II is defined as the presence of B_1 and the absence of B_2; Stage III is defined as the presence of both. Finally, the traits are frequently linked by a measurement sequence. An example of this sort was given earlier: the stages of cardinal number development. The two focal traits were construction of a one-to-one mapping and the mental conservation of such a construction following perceptual deformation. Note that the latter requires that the subject first be able to construct a correspondence. Similar statements can be made about one-dimensional vs. two-dimensional classification, one-dimensional vs. two-dimensional ordering, identity vs. conservation, and so on.

Now, suppose that we wish to test the prediction that behaviors from the preoperational stage invariably precede behaviors from the concrete-operational stage. This would be done by selecting several skills from several domains, administering appropriate tests to large samples of preschoolers and elementary schoolers, and scaling the data. If focal traits emerged in the predicted order in all or nearly all cases, we would conclude that the invariant sequence criterion had been satisfied. But confirmatory data may be guaranteed so long as we administer valid tests.

To summarize, it is commonly supposed that a culturally universal sequence in behavioral development implies an underlying sequence of maturational events (e.g., Beilin, 1971; Gesell, Thompson, & Amatruda, 1934; Tanner, 1956). If this assumption were true in general, Piaget's claim that the steps in the behavioral sequence comprise "objectively certain stages" would not seem unreasonable. The maturational events that precede each behavioral step could be viewed as the glue that holds the stage together. Finding the glue for each stage then becomes an empirical question. Although the assumption that behavioral sequences imply maturational sequences is often true (e.g., Coghill, 1908, 1909, 1912), it is certainly not true in general. There is the matter of measurement sequences to contend with. It is possible to have measurement sequences for which there is apparently no maturational involvement. For example, I assume that no one would argue that learning how to multiply integers and learning how to differentiate polynomials are under maturational control; these skills are culturally transmitted. But their acquisition sequence is culturally universal; no one learns how to differentiate before learning how to multiply. Examples of this sort are intended to illustrate why it is difficult to accept the invariant sequence criterion for cognitive stages. Other writers (e.g., Flavell, 1972; Flavell & Wohlwill, 1969) have pointed out that behaviors belonging to different Piagetian stages are normally linked by measurement sequences. Examples from the preoperational and concrete-operational stages were given earlier. Examples for the sensorimotor and formal-operational stages have been given by others (e.g., Cornell, 1977; Siegler, 1978).

Note that it has not been suggested that research on measurement sequences is utterly trivial or that nothing can be learned from such research. We can learn a great deal. But what is gained is primarily information about measurement procedures. It tells us something about the construct validity of our tests, and it provides a potential method for estimating the type and frequency of measurement errors (Brainerd, 1977a). It has also not been suggested that all behaviors belonging to different Piagetian stages are related by measurement sequences – only that this is often the case. The fact that *any* sequences of this sort can be identified entails that the invariant sequence criterion cannot be accepted as prima facie evidence that "objectively certain stages exist." If it were accepted, then we would have to view sequences such as learning the alphabet before learning how to write, learning to add before learning how to multiply, learning to raise numbers to powers before learning how to differentiate, and so on, as evidence that cognitive stages exist.

Cognitive structure. The structure criterion is second only to sequences in overall importance. It specifies that the members of a set of stages shall each be characterized by a unique complement of cognitive structures. Piaget states the criterion as follows: "Inhelder and I, when considering the development of structures of thought, speak only of stages in connexion with the formation of total structures. We include as special cases all structures observable during a given stage which integrate with the structures of the preceding stage as necessary sub-structures. In this way the logical operations of the 'stage of formal operations' (from 11–12 to 14–15 years) constitute a total structure whose two complementary aspects are the formation of a 'lattice' (combinatory aspect) and the constitution of a 'group' of four transformations (double reversibility). However, this general structure covers, on the one hand, all the operational schemata of this stage" (1960, pp. 11–12).

To understand what it means to say that each stage has its own distinct set of cognitive structures, it is clear that we shall first have to know what a (Piagetian) cognitive structure is. Exactly how Piaget arrives at these structures has always been something of a mystery (Flavell, 1963). Below, I give an example of how one structure is presumably formulated. For now, however, all we want to know is *what* these structures are. Two general statements can be made. First, since the only empirical phenomena studied in Piaget's research are behaviors that undergo age change, the structures are at most abstractions from behavior. This point was anticipated some years ago by Bruner: "Are we any nearer an *explanation* of the child's solution to a problem to say that the solution presupposes some kind of grasp of the principle of logical implication. Is this not only a more refined and conceivably more useful way of describing the formal properties of the behavior observed" (1966, p. 3). Second, the structures are usually algebraic. Although Piaget uses symbolic logic to describe some behaviors, most of his structural descriptions are group-theoretic (Piaget, 1942, 1949). In the remainder of this section, we examine, first, a worked illustration of how a Piagetian structure is presumably isolated and then we consider why such a model is inadequate to insure that stages exist.

The structure chosen for illustration is one that, from a mathematical point of view, is among the most tractable of Piaget's models, namely, the Klein four-group (which Piaget calls the *INRC* group). As the example proceeds, it is hoped that some of the likely principles of structure formulation will become apparent; they will be summarized at the end.

The Klein group, or D_2 for short, is a set of four elements under a binary rule of combination o. The set is usually denoted $\{I, \alpha, \beta, \gamma\}$ and, usually, o is the successive composition rule, that is, if $(x, y) \in \{I, \alpha, \beta, \gamma\}$, then statements of the form "$x \, o \, y$" mean "first do x and then do y" (or the reverse, accordingly as left-hand or right-hand notation is preferred). D_2 satisfies the usual four group axioms and, like all groups with fewer than six elements, it is commutative. It is frequently the case in concrete realizations of D_2 that all the elements are operations of some sort. D_2 is completely defined by the abstract table:

$$\begin{array}{c c} & \begin{array}{cccc} I & \alpha & \beta & \gamma \end{array} \\ \begin{array}{c} I \\ \alpha \\ \beta \\ \gamma \end{array} & \left[\begin{array}{cccc} I & \alpha & \beta & \gamma \\ \alpha & I & \gamma & \beta \\ \beta & \gamma & I & \alpha \\ \gamma & \beta & \alpha & I \end{array}\right] \simeq D_2 \end{array}$$

From an algebraic point of view, Piaget's *INRC* group is a family of concrete representations of D_2. The general nature of this family has been described by Parsons (1960) and Flavell (1963). There is some task T comprised of two variables A and B. A can take on two mutually exclusive values (a and $\bar{a}$) and so can B (b and $\bar{b}$). For any such task, an *INRC* group may be defined as follows: I = leave the system as it is; N = change the value of A; R = change the value of B; C = change the value of both A and B. Parsons (1960) and Flavell (1963) observed that the class of Piagetian representations that satisfy this description may be divided into two main groups, namely, operations of propositional logic (logical *INRC* group) and operations of physical systems (physical *INRC* group). In either case, there is a simple procedure for establishing that a particular *INRC* group is a representation of D_2. First, construct the following mapping: $I \rightarrow I$, $N \rightarrow \alpha$, $R \rightarrow \beta$, and $C \rightarrow \gamma$. Second, show that the following table holds:

$$\begin{array}{c|cccc} & I & N & R & C \\ \hline I & I & N & R & C \\ N & N & I & C & R \\ R & R & C & I & N \\ C & C & R & N & I \end{array} \simeq D_2$$

Note that the internal structure of this table is identical to that of the earlier table for $\{I, \alpha, \beta, \gamma\}$.

The so-called snail problem (Piaget, 1970b, Chapter 5) provides an illustration of these general principles. A snail shell (variable A) is placed on a narrow strip of cardboard (variable B). Both are placed on a table that has a line of reference drawn on its surface. The snail can move toward the line (value a) or away from it (value $\bar{a}$); the cardboard can move, independently of the snail, toward the line (value b) or away from it (value $\bar{b}$). The *INRC* group for this system is then defined as above. Piaget reports that the snail problem is not solved until adolescence, the nominal age range for the formal-operational stage. There are many other problems belonging to the *INRC* family, some involving logic (Inhelder & Piaget, 1958, Part I) and some involving physical systems (Inhelder & Piaget, 1958, Part II), that are also not solved until adolescence. It is this datum, apparently, that leads Piaget to conclude that the *INRC* group is a cognitive structure of the formal-operational stage. Piaget then proceeds to use the structure to *explain* the behavior of different age levels on individual tasks from the *INRC* family. For example, consider the following explanation of snail problem performance: "The child at the level of concrete operations understands these two pairs of direct and inverse operations but does not succeed in combining them . . . As soon as the 4-group is acquired, however, the solution is made easy by the introduction of compensation without cancellation; that is, reciprocity (R). In this case we have $I \cdot R = N \cdot C$, in which (I) is the movement of the snail to the right; (R) the movement of the board to the left; (N) the movement of the snail to the left; and (C) the movement of the board to the right" (Piaget & Inhelder, 1969, p. 143). Note that the latter portion of the statement is simply a description of the algebraic structure of the snail problem. This, in turn, suggests that Piaget feels that subjects' solution of given problems may be explained by describing the problems' structure. Such explanations seem to require that the subject has, somehow, "internalized" this structure.

To summarize, the evidence seems to show that Piagetian cognitive structures are formulated roughly as follows. The main requirements are a class of tasks that all yield the same structural representation and are all solved within the age range for a given stage. We considered D_2 as an example. We could just as easily have considered the grouping structure of the concrete-operational stage (Piaget, 1942, 1949, 1972). The family of representations of this structure includes such things as class-inclusion problems (Inhelder & Piaget, 1964) and duration problems (Piaget, 1969). Such problems are apparently first solved during the elementary school years. We might also have considered the group of displacements of the sensorimotor stage (Piaget, 1954) and its associated tasks. In each of these cases, the structure in question starts out as a description of the common properties of some set of tasks, but it ultimately is used to explain performance on the same tasks: "Each stage is characterized by an overall structure in terms of which the main behavior patterns can be explained" (Piaget & Inhelder, 1969, p. 153).

This brings us back to stages. What objections, if any, are there to regarding Piaget's structures as evidence for stages, that is, as the glue that holds stage-defining behaviors together in a meaningful whole? Assuming that the above account is correct in its broad outlines, I believe there are three main objections. The first and most obvious one is what could be termed the behavioral isomorphism problem. These structures are, in their most basic sense, task descriptions. There is no guarantee that such descriptions correspond in even an approximate way to behavior. Certainly, Piaget has not demonstrated that such mappings always exist. In fact, he has proposed structures for which there are no extant tasks, let alone corresponding behaviors. This objection was, I think, first raised by Flavell with reference to the grouping structures of the concrete-operational stage: "Does each grouping operation really have a discoverable opposite number in ongoing intellectual activity? Do certain groupings even roughly resemble any frequently occurring operational pattern in middle childhood . . .?" (1963, p. 468) The first objection to regarding structures as criteria for stages, then, is that a task description does not necessarily have anything to do with behavior.

The second objection is that a behavioral description is not an explanation. We saw earlier that Piaget uses structures to explain the "main behavior patterns" associated with given stages. Suppose we assume, for the sake of argument, that a precise mapping can be established between the structure for a given problem and aspects of the behavior of subjects who solve the problem. This might be done for the *INRC* group, for example, by considering the following protocol reported for a subject who solved the snail problem: "Do (9;11) . . . *The snail moved forwards too, at the same time as the board . . . The card went back and the snail forward. Since the snail did this distance and the plank that, the snail still did this journey on top of the plank* (he puts one measuring strip on top of the other). *This piece* (the difference) *is the distance the snail did farther than the plank*" (Piaget, 1970b, p. 115). Do seems to be able to explain the structure of the snail problem in detail. Therefore, the *INRC* group has also become a description of Do's behavior. But we cannot now use the *INRC* group to explain Do's behavior on pain of circularity. No functional relationship has been established between the structure itself and behavior. To do this, we would require independent measures of the *INRC* group and behavior on the snail problem. The second objection, then, is this: A structural description of behavior does not explain how these behaviors originate. This point has been previously raised by Bruner (1966).

The third objection seems to me to be the most telling. If statements like "The structure consisting of a group of sensorimotor operations appears in the period of infancy" (Inhelder, 1956, p. 76), "The structure of concrete *groupements* begins in early childhood" (Inhelder, 1956, p. 76), "The structure of combined groups and lattices . . . develops between eleven and fourteen years" (Inhelder, 1956, p. 76), and so on, are to make any sense, it is obvious that we shall have to be able to regard these structures as unique to the stages for which they are posited. But it is impossible, in principle, to do this. Suppose there is some set of problems sharing some structural representation and all of them are solved during the age range for a given stage. What guarantee do we have that there is not some other set of problems sharing the same representation that is solved at some earlier or later age? No matter how large the first class of problems is, we can never rule out the latter two possibilities entirely. The first one is especially serious. In Piaget's theory, the structures of each stage are viewed as elaborations of the structures of previous stages: "These overall structures are integrative and non-interchangeable. Each results from the preceding one, integrating it as a subordinate structure, and prepares for a subsequent one, into which it is sooner or later itself integrated" (Piaget & Inhelder, 1969, p. 153). Since later structures integrate earlier ones, it might be argued that a problem class with the structural representation of an earlier stage might sometimes be solved during a later stage due to performance factors (e.g., Flavell & Wohlwill, 1969). But it should never happen that problem classes with the representations of later stages are solved during earlier stages. Under such conditions, the structural distinction between stages breaks down completely.

The objection that structures may not be unique to the stages for which they are posited is more than idle speculation. Specific instances can be cited wherein classes of problems solved at an earlier stage share the structure of a later stage. These examples appear to be most numerous for the *INRC* group, which explains its selection as an illustration. There appear to be several problem classes whose members are solved during the concrete-operational range (elementary school) but share the *INRC* representation. I shall give three examples. First, there are propositional logic problems. Piaget has shown that the sixteen propositional operations have the *INRC* structure; a unique *INRC* representation can be obtained for each operation (see Bart, 1971). But recent evidence shows that elementary schoolers solve a wide variety of problems concerned with these operations. In fact, the only propositional logic tasks routinely failed by such children are ones involving invalid forms (for reviews, see Ennis, 1975; Brainerd, 1978). Second, there are the conservation problems, which are usually regarded as the sine qua non of concrete operations. All of the standard conservation paradigms (number, length, liquid, etc.) conform rather closely to the earlier description of systems yielding *INRC* representations. There are always two variables (the two stimuli), and they can usually undergo two opposing transformations (the states of the variables). For example, consider the number conservation problem, which involves making relative numerousness judgments about two parallel rows of objects. Variable A is one row, which may be either lengthened (value a) or shortened (value $\bar{a}$). Variable B is the other row, which may also be lengthened (value b) or shortened (value $\bar{b}$). The *INRC* group is: I = leave the system as it is; N = change the transformation on A; R = change the transformation on B; C = change the transformation on both rows. Similar demonstrations are easily devised for other conservation problems. Third, there are the matrix problems used in Inhelder and Piaget (1964) to study the classification and seriation concepts of the concrete-operational stage. Any matrix yields D_2 representations (Budden, 1972). There are always two variables, the Row factor and the Column factor. If the matrix is 2×2, the Row and Column factors have two values each and an *INRC* group results. But $N \times N$ matrices also yield *INRC* representations. We merely divide the N Row values into two mutually exclusive classes (a and $\bar{a}$) and do likewise with the N Column values. The *INRC* group is then: I = leave the system unchanged; N = change the Row equivalence class; R = change the Column equivalence class; C = change the Row and Column equivalence classes.

In sum, it does not appear that the structure criterion provides prima facie evidence that a set of stage-defining behaviors forms a natural grouping. These structures appear to be primarily task descriptions. When mappings can be effected between the structures and subjects' performance on the tasks, the structures merely become abstract descriptions of behavior. Moreover, these descriptions apparently cannot be regarded as unique to the stages for which they are posited.

Minor criteria: integration, consolidation, and equilibration

Integration. This criterion asserts that each stage presupposes the immediately preceding one: "*The passage from an inferior stage to a superior stage is equivalent to an integration:* The inferior becomes part of the superior. It is easy to show that concrete operations serve as a base for the formal operations of which they are a part. The combinatorial method, for example, is based on changes of order which are possible during childhood and later develop into combinatorial operations. Proportions themselves are operations applied to operations, or operations to the power of two" (Inhelder, 1956, p. 85).

The integration criterion is sometimes formulated in terms of structures. That is, it is proposed that the cognitive structures of any given stage "integrate" those of earlier stages. But, as Inhelder indicates, the standard empirical illustration of integration involves selecting some behavior from a given stage and then showing, primarily on logical grounds, that it presupposes behaviors from earlier stages. Other familiar examples, in addition to the ones cited by Inhelder, involve the conservation and spatial concepts of the concrete-operational stage. Piaget frequently remarks that conservation presupposes ("integrates") both the identity concepts of the preoperational stage (1968) and the object permanence concepts of the sensorimotor stage (1954). Similarly, he states that the Euclidean and projective concepts of the concrete-operational stage (Piaget & Inhelder, 1956; Piaget, Inhelder, & Szeminska, 1960) presuppose ("integrate") the topological concepts of the preoperational stage (Piaget & Inhelder, 1956).

From an empirical point of view, the integration criterion appears to be a restatement, in slightly altered language, of the sequencing criterion. Therefore, it would be subject to the same objections as sequencing. The prediction that one would presumably test to verify the claim that some given stage integrates the preceding one is that behaviors belonging to the latter invariably precede behaviors belonging to the former. But this again raises the possibility of measurement sequences.

Consolidation. It would perhaps be more revealing to refer to this as the preparation-achievement criterion. According to the consolidation criterion, each stage is simulataneously an achievement phase for its own behaviors and a preparation phase for those of the next stage: "If the stage $n + 1$ is really new with respect to n, then in any stage n it should be possible to distinguish an aspect of *achievement* with respect to the stages going before and also an aspect of *preparation* with respect to the stages coming after" (Piaget, 1960, p. 13–14). Thus, the sensorimotor stage is an achievement phase for object permanence and a preparation phase for identity, the preoperational stage is an achievement phase for identity and a preparation phase for conservation, and the concrete-operational stage is an achievement phase for conservation and a preparation phase for propositional logic.

It is unclear what the consolidation criterion's empirical consequences are. For this reason, some writers (e.g., Wohlwill, 1966) have recommended dropping it altogether on grounds of superfluity. Other writers, notably Pinard and Laurendeau (1969), disagree. Pinard and Laurendeau believe that it entails the phenomenon of *horizontal décalage. Horizontals décalages* are invariant sequences in behaviors belonging to the same stage. The classic illustration involves conservation concepts. It is said (e.g., Piaget & Inhelder, 1941) that conservation of quantity invariably precedes conservation of weight, which invariably precedes conservation of volume.

Assuming it is true that the consolidation criterion implies the phenomenon of *horizontal décalage*, this criterion, like integration, becomes another restatement of the sequencing principle. There is, it is true, one important difference: The predicted sequences are for same-stage concepts rather than different-stage concepts. But the measurement sequence argument still applies. It is just as likely that a same-stage sequence is measurement-based as it is that a different-stage sequence is. This happens, for example, in the conservation illustration above. The stimulus materials used in conservation of quantity tests (clay balls) are also used in conservation of weight tests. In the latter tests, however, subjects must also know how to operate a pan balance. This knowledge does not enter into quantity tests.

Equilibration. This is probably the vaguest and most tentative of the five criteria. Historically, it is a rather recent addition to Piaget's stage program. It does not appear, for example, in Inhelder's (1956) discussion of stage criteria. Piaget's first systematic exposition of it seems to be a paper published in 1960. Piaget views cognitive development as consisting of the attainment of successive states of equilibrium, each more stable than the last. Each state is temporary and eventually dissolves into

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disequilibrium by a combination of internal and external forces. Ultimately, however, an equilibrium level is achieved that is sufficiently stable to resist further change. Piaget (e.g., 1960) says that his stages correspond to relatively long-lasting levels of temporary equilibrium in the overall process of cognitive growth. Since the direction of development is toward ever more stable equilibria, it follows that each successive stage should be more stable and less subject to perturbation than its predecessor: "the most general and the most elaborate programme for a theory of stages doubtless consists of representing the stages in the form of a series of equilibrium levels, the fields of which would always be more and more extensive and the mobility always greater, but whose increasing stability would depend precisely on the degree of integration and structuration" (p. 14).

As was the case for consolidation, Piaget and his co-workers have not spelled out the exact empirical consequences of the equilibration criterion. At first glance, however, it would seem to require that stages should be divisible into periods of rapid acquisition of the relevant behaviors (achievement phases) alternating with periods of relative quiescence (preparation phases). The idea that cognitive development is a matter of achieving and then losing successive equilibrium levels seems, logically, to demand that new acquisitions should appear in spurts (Pinard & Laurendeau, 1969). Importantly, I believe there would be some reason to suppose that Piaget's stages may do something other than describe behavior if these alternating periods of change and stability could be verified. Whenever development is observed to proceed by fits and starts, this fact suggests either some sort of maturational control, perhaps hormonal in nature, or major changes in the child's environment or both. One is reminded, for example, of the spurts in physical growth noted around pubescence (Tanner, 1970) or the spurts in linguistic and arithmetical skill that occur upon entering elementary school. These spurts are preceded and followed by periods of less rapid change. If it could be shown that the behaviors associated with a given Piagetian stage tend to emerge abruptly at the start of the nominal age for the stage, we might have reason to suppose that they share common antecedent variables. We would still be left with the problem of discovering what the antecedent variables are, but at least we would be doing something other than describing behavior.

Genevan writings occasionally give the impression that, in fact, children rapidly acquire stage-defining behaviors at the beginning of the appropriate age range. But there is no direct support for this claim. The data show that the reverse is true. There has been extensive research on the development of Piagetian conceptual skills in recent years, especially those from the concrete-operational stage. It has not been found that such skills appear rapidly On the contrary, it is generally conceded that development is smooth and gradual throughout a stage's age range (Flavell, 1970; Flavell & Wohlwill, 1969; Pinard & Laurendeau, 1969). Consider the concrete-operational stage as a case in point. Inhelder (1956) has suggested that behaviors from this stage are uniformly absent before age five or six, that they are rapidly acquired thereafter, and that virtually all of them are present by age eight. What the available data show is this. First, many concrete-operational behaviors seem to be present during the preceding stage (preoperations). Examples of these precocious traits are relational skills such as linear ordering and transitive inference plus binary classification (for a review, see Brainerd, 1978, Chapter 5). Second, the majority of concrete-operational skills appear *gradually* during the elementary school years. Here, conservation concepts are the classic illustration. Some of them (e.g., number and length) appear early, others (e.g., quantity and mass) appear somewhat later, and still others (e.g., area) appear very late. Third, there is another group of concepts that does not seem to develop until the age range for the next stage (formal operations). These late bloomers include such things as class inclusion (e.g., Brainerd & Kaszor, 1974) and a variety of Euclidean spatial concepts such as horizontality and distance.

On the whole, then, there is nothing in the data on how concrete-operational behaviors develop that would suggest clear lines of demarcation between this stage and either its predecessor or successor. Concrete-operational behaviors have already begun to appear during the preoperational stage, and they are still appearing during the formal-operational stage.

In sum, there is a rather extensive data base on age changes in Piagetian concepts that does not tend to confirm the idea of successive equilibrium levels. The data are sufficiently consistent on this point that some reviewers have concluded that concept development is a smooth, continuous process that is not given to fits and starts. A detailed examination of issues bearing on this question may be found in two papers by Flavell (1971; Flavell & Wohlwill, 1969).

Of course, we still wish to know whether the equilibration criterion provides grounds for concluding that a set of stage-defining behaviors is a natural grouping. The answer appears to be both yes and no. On the one hand, alternating phases of behavioral change and quiescence suggest correlated changes in antecedent variables. The latter may involve maturational events, as in the case of the hormonal changes producing the adolescent growth spurt (Tanner, 1970), or experiential events, as in the case of starting school, or both. Once alternating states of change and quiescence have been verified for some given stage model, it seems reasonable (*a*) to conclude that the alternations are being produced by changes in antecedent variables and (*b*) to regard these variables, as yet unidentified, as the glue that holds individual stages together. On the other hand, alternating states of change and quiescence have not been observed for Piaget's stages. Smooth behavioral change without noticeable variations in rate seems to be the rule. However, this does not preclude the possibility of verifying the equilibration criterion for some other cognitive stage model.

Epilogue

Piaget's stages fall somewhere between the poles of true explanation and pure description. Although Piaget wishes that his stages should be regarded as explanations of behavior (e.g., Piaget, 1960, pp. 12–13; Piaget & Inhelder, 1969, p. 153), he has not tied them to specific antecedent variables whose measurement procedures are well-defined. However, he has proposed a program of five criteria whereby the nonarbitrariness of his stages can ostensibly be ascertained by empirical means. These criteria were analyzed in the present paper. On the whole, the analysis did not support Piaget's optimistic view of them. Three of the criteria (invariant sequence, integration, and consolidation) lead to identical predictions, and they are all subject to the same objection (measurement sequences). The structure criterion is subject to the objection that those structures posited for any given stage apparently cannot be regarded as unique to that stage. Only the fifth criterion in the program, equilibration, leads to predictions whose verification would convince prudent investigators that stage-defining behaviors comprise natural groupings. But these predictions have not been corroborated for Piaget's particular stages.

To the extent that the explanatory power of Piaget's stages hinges on his five criteria, I conclude that there is no compelling evidence that these stages do anything other than describe behavior. This conclusion has at least one important consequence: It is improper to explain the fact that some children do one thing and some children do another by saying that their Piagetian stages differ. It happens that explanations of this sort abound in the concept development literature. Three examples from Genevan writings will be given. First, when children are trained on conservation concepts, some learn very well, some learn moderately well, and some learn poorly. This finding has been explained on the ground that the first group is at the concrete-operational stage, the second group is in a transition phase between the preoperational and concrete-operational stages, and the last group is at the preoperational stage (see Inhelder & Sinclair, 1969; Inhelder, Sinclair, & Bovet, 1974; Strauss, 1972). Second, the appearance of reproductive imagery at about age two and the appearance of anticipatory imagery at about age seven or eight have been explained on the grounds that the former results from entrance into the preoperational stage and the latter results from entrance into the concrete operational stage (e.g., Piaget & Inhelder, 1971). Third, the fact that a group of children tends to remember a picture of a seriated array better six months after they saw the picture than one week after they saw the picture has been explained on the ground that more children are in the concrete-operational stage after six months than after one week (e.g., Piaget & Inhelder, 1973). Other examples of this genre are given elsewhere (Brainerd, 1977b). The point to bear in mind is that such statements are not really explanations if Piaget's stages are purely descriptive.

Although this paper has been concerned with rather recondite conceptual questions, I should like to close by noting that the explanatory status of Piaget's stages is a critical issue to educators. In recent years, a number of early childhood curricula based on Piaget's theory have been devised and tested (for reviews, see Brainerd, 1978; Hooper & De Frain, 1974; Lawton & Hooper, 1978). The distinctive feature of these curricula is that they advocate basing instruction on Piaget's hypothesis that cognitive development is a stage-like process. Their guiding principle is that children should never be taught anything that exceeds the limits of their current stage. A variety of diagnostic procedures have been developed for teachers to use in assessing children's Piagetian stages. If it is true, as Piaget claims, that his stages are natural groupings, then instructional practices such as these may have considerable merit. But if it is true, as the present analysis suggests, that Piaget's stages are merely descriptive, the rationale for these practices appears to evaporate.

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NOTE

1. I am assuming that there is no serious objection to regarding Piaget's stages as adequate descriptive constructs. Though vagaries and ambiguities may sometimes arise, the behavioral meanings of phrases such as "Stage IV of object permanence," "the concrete-operational stage," "Stage III of classification development," and so on, would seem to be reasonably clear.

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by **John D. Baldwin** *and* **Janice I. Baldwin**

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Stages resulting from continuous underlying variables. Brainerd presents a thought-provoking critique of Piaget's theory from a logical level of analysis and cites some of the empirical evidence now accumulating that is incompatible with Piaget's formulations. We would like to broaden the present discussion by considering another example of apparent "stage" development seen in nonhuman primates and in human beings. In this example, the underlying causal factors function as continuous variables, yet often create the impression of stages at the behavioral level. It is our belief that an analysis of underlying factors and within-stage variance is more valuable than the study of stages themselves.

A review of the research on exploration and play in primates (and certain other mammals) supports the theory that exploration and play are stimulus-seeking behaviors reinforced by intermediate levels of sensory stimulation (Baldwin & Baldwin, 1977; in press). Sensory stimulation is a biologically established (i.e., primary) reinforcer across the primate order, including human beings. Generally, the more novel, complex, unpredictable, and intense a given stimulus input is, the greater the stimulative impact it has on the perceiver. Both low and high levels of sensory stimulation impact are aversive, but intermediate levels function as positive reinforcers.

Because the world is totally novel to the newborn infant, the infant primate is easily overstimulated. However, early clinging reflexes keep the infant close to its mother (a warm, soft, familiar stimulus that does not cause overstimulation). Early reflexes, especially alerting, looking, crawling, and finger movements, expose the infant to mild levels of novel stimuli, which in turn reinforce the development of operant patterns of exploration. Early exploration consists of touching the mother's body and passively watching the world. However, the processes of *familiarization* and *habituation* serve as biologically established mechanisms that promote continuous development of exploration and play activities. As the exploring infant becomes familiar with its mother's body, it ceases to find novelty and unpredictability there, hence early exploration eventually leads to aversive understimulation effects (i.e., boredom). At this point, differential reinforcement effects begin to shape the infant's behavior toward leaving the mother's body and exploring the environment away from her, if the infant leaves her to crawl in the branches and lianas, it will be reinforced by the escape from aversive understimulation and by the discovery of new sources of rewarding novelty. The infant advances from the stage of continuous maternal contact to the stage of environmental exploration. During this period, if the infant is overstimulated while exploring the environment, it will be reinforced for returning to the familiar, arousal-reducing stimuli of the mother. For example, if the infant's exploration leads it into the midst of a rowdy group of playing juveniles, the larger animals' vigorous and rapid activities will overstimulate the infant, and it will seek out the low inputs of mother's body to counteract the overstimulation.

After repeated experience while exploring, the infant habituates a step at a time to higher levels of stimulus input. Figure 1 shows the infant's general developmental course. As it familiarizes itself with broader ranges of novel, unpredictable stimuli and habituates to higher levels of stimulus quantity and intensity, the infant is reinforced for leaving the mother's side and venturing into ever more stimulating activities. Thus, in many environments, one sees infants progress through a series of overlapping stages from early maternal contact, to exploration of the nonsocial environment, to social exploration, then to social play. Within the realm of social play there is often a sequence from gentle wrestling play, to chasing and noncontact play, to play fights. The rate with which these stages appear is constrained to some

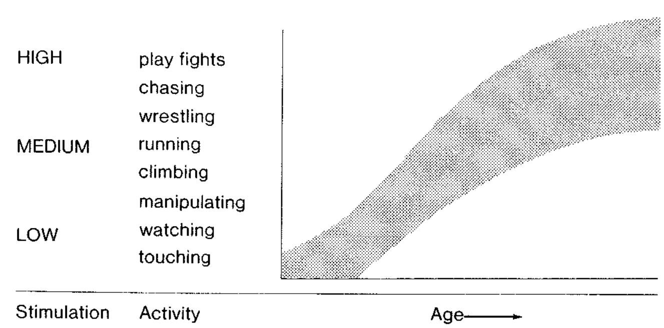

Figure 1 (Baldwin and Baldwin). The individual's optional sensory stimulation level (stippled area) rises as he familiarizes and habituates himself with more novel active and complex behaviors. Above the optimal (positive reinforcement) zone, sensory stimulation is aversively overstimulating; below, it is aversively understimulating.

degree by biological factors. First, the earliest age for each stage cannot be collapsed beyond certain limits that are determined by the maturation and growth of muscles, bones, and other behavioral "machinery": premature attempts at an advanced behavior lead to falls, hard knocks, overstimulation, and other aversive consequences that punish the behavior until the time that adequate maturation is attained. Second, there are limits to the speed with which learning can occur and skills can be acquired.

The duration and commonness of the stages of exploration and play can vary considerably within species, depending on local environmental conditions. For example, monkeys in laboratories tend to progress through the stages more slowly than conspecifics in more natural and more stimulating environments. Under some conditions, the sequence may be truncated to the point that social play does not appear. To understand the variance in stage development, one needs to turn to the underlying causal variables and determine how behaviors in each stage are conditioned in particular environments. For example, in many laboratory environments, the cages are stark and there is little novelty to reinforce the infant for leaving mother's side. In fact, mother's movements, half-hearted games, and punitive actions may provide more sensory stimulation than any other part of the environment; they hence reinforce the infant for continuing early mother-infant interactions to a much greater extent than do infants in rich sensory environments.

When the underlying developmental mechanisms are known, there is less incentive to study the stages than to focus on the mechanisms of behavioral acquisition. When the mechanisms allow a dynamic interaction of both psychobiological mechanisms (maturation, sensory stimulation, primary reinforcers, and familiarization-habituation mechanisms) and environmental determinants (patterns of sensory stimulation and other reinforcers in the environment), stage theories based on biologically determined chronological age are less likely to fit the data than theories that intertwine both maturation and experience (nature and nurture). When development in different environments is studied, the timing of stages can be quite variable, which discourages global generalizations about the stages themselves and focuses attention on the underlying causal variables. Unfortunately, stage descriptions often obscure the fact that there can be a great deal of behavioral variance within any given stage. For example, within the stage of wrestling play, animals discover countless novel patterns of movement and interaction not captured by the global concept of "wrestling." A focus on mechanisms allows one to recognize and explain these variations better than the global stage concepts do.

Brainerd's epilogue wisely points out that our behavioral theories have an impact on educational policy. If the reinforcement model of human psychological development proposed by Bijou and Baer (1963, 1965, *op. cit.*) is more valid than Piaget's model, as our data indicate, educators (and education research) will need to take a very different course than that suggested by the Piagetian model.

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Stages as descriptions, explanations, and testable constructs. Brainerd's critique of Piaget's stage theory includes assumptions about the status of the stage construct, comments suggesting that many stage sequences are untestable, and several objections to the proposition that stage sequences indicate the progressive elaboration of cognitive structures. The critique is consequently rather complex and multifaceted, and I will comment on only a few of the main points. In contrast to Brainerd, I will argue that Piaget's stages can be legitimately viewed as both descriptive and explanatory, that hypotheses about stage sequences can often be given a meaningful test, and that Piaget's claims about the structural component of stages have not yet been disconfirmed.

There seems no question but that Piaget's stages are descriptive constructs, for they describe "those structural wholes, of great abstraction and generality, which correctly identify the essence of organized intelligence at its various levels" (Flavell, 1963 *op. cit.* p. 21). What is in question is when a description becomes an explanation. Brainerd argues that all explanations are of the same form, namely, explanations of an effect in terms of its causes. However, there are other forms of explanations, one of which consists of accounting for a particular case by referring to a general principle or law (Braithwaite, 1955). As Flavell's statement implies, Piaget's stages provide the latter type of explanation. They explain any single behavior of a child by referring to the general nature of the child's cognition. While Brainerd recognizes the abstract and general character of Piaget's stages, he argues that this in itself does not distinguish them from more specific stage descriptions. This seems an error, for in psychology and in other sciences, abstract descriptions can function as explanations of particular instances.

Of course, an abstract description that merely restates what happens in particular instances is a circular explanation. Although Piaget recognized the problem of circularity, he seems to have felt that a precise definition of stages was needed before an evaluation of them as explanatory constructs could be made. To define his notion of stages, he presented the five criteria discussed by Brainerd. The criteria may be divided into three sets, corresponding to three characteristics of Piaget's definition. First, the stages are said to be distinct and objective levels of cognitive development. This characteristic leads to the criterion that there be a sequence of stages, each of which represents a level of equilibrium, and each of which is both an integration of separate aspects of previous stages and a preparation for higher stages. Second, the stages are intended as general descriptions of cognitive-developmental levels, not ones tied to specific experiences. Consequently, stage sequences should be culturally universal. Finally, each stage is intended as a formal description of the totality of cognitive functioning. The criterion that stages form structured wholes is related to this characteristic.

Though the majority of criteria apply to the first characteristic, Brainerd and most other critics have concentrated on the latter two characteristics, the universality of the stage sequences and the structured nature of each single stage. On universality, Brainerd's main criticism is that many behavioral sequences are measurement sequences. That is, they reflect the fact that later items involve earlier items plus some additional things. Therefore Brainerd argues that it would be logically impossible to have the later stage precede the earlier stage. However, there are several problems with the criticism. First, the behaviors said to indicate the earlier and later stages could be mastered at the same time, and the synchrony would be evidence against the stage theory. Though Brainerd does not mention this possibility in discussing the universality criterion, he gives several examples of it when discussing the criterion that stages consist of structured wholes. Second, it is not possible to define a measurement sequence solely on the basis of the characteristics of tasks. For example, Brainerd says that addition and multiplication form a measurement sequence because "to multiply, children must know how to add." But then the rationale for calling addition-multiplication a measurement sequence implicitly involves a hypothesis about the processes children use to multiply. Such hypotheses cannot be substantiated by logical arguments alone. If children learned to multiply by memorizing multiplication tables, multiplication might be found before addition. The basic point is one frequently made by Piaget (Inhelder & Piaget, *op. cit.* 1964, p. 282), that the child's way of relating different problems may not correspond to the logical relationship inferred by an adult. Thus an assertion that any particular sequence is a measurement sequence cannot be confidently made. Finally, even if some sequences were actually measurement sequences, the universality criterion might be supported by evidence on other sequences. On logical grounds, it seems Brainerd was incorrect in saying that the existence of *any* measurement sequences entails rejection of the universality criterion; it is only if *all* sequences are measurement sequences that the criterion is unworkable.

Brainerd's criticism of the cognitive structure criterion includes three points. The first, that the structures are sometimes purely task descriptions, seems valid. The second, that the structures do not explain a child's behavior, can be answered by referring to the earlier comments on the meaning of explanation to Piaget and Brainerd. The final point, that structures may not be unique to the stages for which they are posited, is one that requires empirical support; but Brainerd's supporting evidence appears to be weak. The contention that elementary school children can solve logical problems that, according to Piaget, require the adolescent's formal operations can be countered by noting that the younger child's concrete operations are adequate for solving some logical problems, and these may be the ones that the younger children do solve (Knifong, 1974). The suggestion that success in conservation tasks requires formal operations can also be refuted. In the section quoted by Brainerd, Piaget states that the crucial element of formal operations is the ability to use simultaneously both the negative (*N*) and reciprocity (*R*) operations. However, the child does not need to use both operations to succeed at conservation. One is sufficient. For example, on a number conservation task, the child could say that the two rows had the same number to start with, and that the row that is now longer could be shortened (negation of original transformation), and therefore that there must still be the same number in the two rows. Though each case requires its own response, these examples should indicate that conclusive evidence against the structure criterion has not yet been given.

Nonetheless, there are serious problems with the stage construct. In fact, the most common of previous criticisms of Piaget's stages was not mentioned by Brainerd. This criticism, which applies to the cognitive structure criterion, is that there is no evidence that different behaviors that are manifestations of the same structure are in fact related to one another. More specifically, the criticism is that different behaviors belonging to the same stage are acquired at very different times. When empirical research demonstrated the validity of this criticism, some writers continued to insist on the importance of simultaneous acquisition as evidence for structured wholes, though perhaps with more refined tests of the hypothesis (Pinard & Laurendeau, *op. cit.* 1969; Wohlwill, 1973). Other writers (Flavell, *op. cit.* 1971) suggested that simultaneous acquisition of different stage-defining behaviors was not crucial to the theory. However, if the hypothesis is abandoned, other evidence must be found to support the claim that stages are general descriptions of cognitive functioning at some period in development. Otherwise, explanations of particular behaviors in terms of stages can be justifiably criticized as circular. What could be used as evidence that stages are general descriptions of cognition appears to be the important, and unanswered, stage question.

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Description, explanation, and circularity. What makes an explanation circular? More particularly, what makes a stage explanation circular? Brainerd suggests that a sufficient condition for circularity would be the absence of procedures for measuring explanatory factors "*independently* of the behaviors that the theory is trying to explain," and regards this requirement as "the litmus test for explanatory stages."

Although I share Brainerd's suspicions about Piaget's stage system (particularly in the area of logic: Ennis, 1975 *op. cit.*), I urge that this requirement is too strict.

Why must there be procedures for the independent measuring of explanatory factors? Might not the explanatory factors be hypothetical constructs that help us make sense of observations? For example, the explanatory factors, atom, molecule, and gene, were invented by physicists, chemists, and biologists before there were independent ways of identifying and measuring these factors. Yet they were explanatory, and the evidence for them consisted of the descriptive facts that they explained. In the most tempting interpretation of the independence requirement there were no independent ways of measuring atoms, molecules, and genes, and thus their use as explanatory factors violated Brainerd's requirement. Hence the requirement appears too strict.

Does Brainerd mean his "independent measurement" requirement the way I just took him to mean it? Must there be a procedure for the separate positive identification and subsequent measurement of the explanatory factor? That is, I believe the most likely interpretation of his words. Furthermore, satisfaction of such a requirement would certainly guarantee that we are not dealing with a tautology, a danger in causal explanation against which Hume long ago warned us. An example of the satisfaction of his requirement is the explanation, "The snow storm was caused by the deep low pressure area to the south of us." In this explanation, the explanatory factor, the deep low pressure area, is independently measurable by a series of barometers, instruments that do not measure snowfall. Since employing the requirement as interpreted does avoid tautologous theories, and since my given interpretation does seem to be the most plausible reading of his words, that is perhaps the interpretation to attach. If so, then Brainerd is being too demanding of cognitive-developmental theory, since early twentieth-century atomic, molecular, and genetic theory also failed to satisfy that criterion. [Cf. Haugeland et al., this issue.]

However, there is some reason to think that Brainerd intended the criterion to be less strict. I shall consider this more lenient interpretation and argue that the new criterion would still be too strict as a requirement for avoiding circularity (though not too strict as a requirement for a sophisticated theory).

This weaker requirement is that there be at least two different kinds of evidence for the theory. One kind of evidence would be what he calls the "described behavior." The second kind of evidence need not be independent measurement, as demanded by the strict requirement, but rather it need only be some other kind of evidence that would be explained. Brainerd suggests this weaker interpretation when, in discussing equilibration, he says, "I believe there would be some reason to suppose that Piaget's stages may do something other than describe behavior if these alternating periods of change and stability could be verified." As I understand it, this is a more lenient approach, since no mention is made of independent measurability; rather, another kind of explained evidence (i.e., other than the satisfied behavior description) would seem to satisfy Brainerd at least partially. This other kind of explained evidence would be alternating periods of change and stability, if they actually occurred.

This more lenient requirement was satisfied by atomic, molecular, and genetic theory; although the presence of atoms, molecules, and genes was not independently measurable, there was a variety of evidence that these posited factors explained. But it still seems too strict as a criterion of explanation. Consider the following dialogue: A: "Why did the boss nearly bite my head off?" B: "Because she's in a grumpy mood today."

This answer is an explanation at a low level of sophistication, and, in the standard work contexts with which I am familiar, would not be circular. It adds two things to the request for explanation: the suggestion that the boss's behavior was not an isolated incident, and the fact that there is today some tendency by the boss to be grumpy. It is this tendency that is the explanatory factor. And the evidence for the presence of this explanatory factor might be only a series of incidents in which the boss was harsh to someone (this would be the "described behavior").

This example shows that there can be an explanatory factor, the evidence for which consists only of particular behavioral results of the operation of that factor. Such a factor can be generally described as the power or disposition to produce the sort of behavioral result used as evidence for it. I am not suggesting that one can generalize from a number of instances and have an explanation, but, rather, I am suggesting that the positing of a force or disposition to do a sort of thing can be explanatory. That is, I am not suggesting that "The boss is usually harsh" (a generalization of behavior) is explanatory; rather, I am suggesting that "The boss is grumpy" is explanatory. It posits a factor, grumpiness, that accounts for the unpleasant behavior. This must not be thought to be an empty explanation. It rules out a blunder on the employee's part as an explanation of the boss's harshness. And it rules out the harshness as a chance occurrence.

Another instructive example is an oft-challenged answer to the question, "Why do birds fly south in the winter (in the Northern Hemisphere)?" It is often alleged that the answer, "Because they have a south-flying migratory instinct," is a circular explanation. But it is not circular at all levels of sophistication. It does give some information about the responsible factors. It says that the practice is less a learned behavior than the result of an instinctive drive. It also rules out the behavior's being a chance occurrence if that is not already ruled out in the context, and rules out the birds' being influenced by some intelligent being, perhaps luring them south with created visions of food just ahead. The presence of the instinct is not implied by the question, so the answer represents some progress, albeit not as much as some would want. In a context in which it was accepted that the practice was instinctual and in which the intent of the question was to find out why they had this instinct, or what was its neurological base, the reply would be circular. But the circularity depends upon the assumed facts and the sought-after level of sophistication.

A third example is that drawn from psychoanalytic theory, about which Brainerd says, independent measurement being lacking, "'he sucks his thumb because he is in the oral stage' is simply a paraphrase of 'he sucks his thumb.'" That explanation might be defective because it presupposes a false theory, or because it presupposes an untestable one, but the lack of independent measurement of the oral stage (in the strict interpretation of "independent measurement") should not suffice to disqualify it, as I have already tried to show. Applying the requirement in the more lenient interpretation (at least two different kinds of evidence), even if the psychoanalysts had no other evidence than sucking behavior, the putative explanation need not reduce to "he sucks his thumb." I would still have been given at least the following information by the explanation: that the sucking behavior is not random, that there is some (though currently unidentifiable) internal causal factor or interaction of factors that is responsible for this identified sucking behavior (rather than that each instance was induced by some being or creature outside the child), that examples of the behavior do generally stop appearing in children sometime, and that the causal factor is inherited (the sucking behavior is not merely, if at all, the result of teaching). This is more information than is conveyed by the phrase, "he sucks his thumb." Hence the reduction is not correct.

Whether the information is informative depends on the context. But we cannot automatically condemn the explanation as circular. It does convey information and does indicate an explanatory factor.

If these three putative explanations are, as I contend, explanations in some context, then Piagetian explanations should not be judged circular on the ground that they suggest causal factors that so far only account for the kind of evidence that seems to instantiate the operation of the factor. Admittedly, explanations of the form I have been discussing are unsophisticated. But there are grounds for wondering whether our grasp of human development has yet made otiose explanations as unsophisticated as these.

In sum, the requirement that the explanatory factor be strictly measurable independently of the explained behavior, and even the more lenient requirement that there be some different kind of evidence for it, do not seem warranted. Unsophisticated explanations, though truly explanatory, might violate both the strict and lenient requirements. Piaget's stage explanations thus should not be automatically discarded on the ground that they violate either of these requirements.

I have not attempted to exhibit the explanatory features built into much of the language we call descriptive. That topic, and the related topic, theory-laden observation terms, would require more space than is here available. If pursued, the result would be the urging of even more leniency in judgments about what is to be ruled out as explanatory.

I have only attempted to defend Piaget against the strict or lenient employment of Brainerd's independence requirement. I have not tried to defend him against the suggestion that his stage claims are inconsistent with the facts or against the claim that they are untestable for other reasons.

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Some additional data relevant to considerations about the existence of cognitive-developmental stages. It is possible to appreciate Brainerd's analysis and even not to have found the flaws in it while at the same time being quite sure that it will turn out to be wrong in its strongest conclusion. The reason is that there are studies not discussed by Brainerd that seem to me to contradict his conclusion about the nonexistence of Piagetian stages. I want to mention just a few of these studies. First, however, I must point out that, for reasons that do not contradict the stage notion, behaviors can be found to appear earlier or later than the standard age.

A behavior can be *elicited* at an age earlier than normal because it is the *maturation* of a *regulation* system for behaviors that is generally responsible for the appearance of the behavior; it is not at all a great trick to be clever enough to elicit a behavior prematurely. That a behavior can appear later than normal is implicit in the finding (Dasen, 1972) that even in developed countries fewer than half the adults show formal operations, and that not all even show concrete operations. (This kind of development is presumably the consequence of an inadequate experiential development of newly created neural networks.)

The kind of experiment that bears directly on the age-linkage of the stages is that done by Webb (1974), who studied the appearance of formal operations in children of ages six to eleven who had IQs of 160; their mental ages thus range from about ten to about eighteen. His question was whether formal operations appeared according to mental age, chronological age, or some more complex reckoning. His results were that no child showed formal operations until fairly close to age eleven. This result bespeaks an age-linkage that transcends the claims even of many Piagetians. Brown's (1973) work shows a similar situation with respect to age of appearance of concrete operations.

An additional pertinent aspect of Webb's work is that he measured the maturation of the formal operations stage, finding that the very bright children reached a highly mature level of that stage in a matter of a few months, while children of normal IQ took a year or more to reach the same maturational level. One infers that children of subnormal IQ may not reach a maturational stage before the onset of the next stage, lest a mixup and perhaps inhibition of both stages result.

These results can be used to "explain away" many of the contradictions found for Piagetian stage development. In addition, they point up the need to be extremely careful about concluding that a stage is being violated when all that is being measured is one of the maturation substages of the main stage.

It is also pertinent to point out the results of the work of a group in England (Isaac & O'Connor, 1975), who have found multimodal distributions of performances on tests of intelligence and problem-solving. They show five modes, the first four of which correspond (on the anecdotal level only, thus far) to the four known major Piagetian stages. This seems to afford the possibility of inferring that persons who fail to go on to a succeeding stage may just expand their competence at the current stage in which they are stuck. It is of related interest that the English group indicates the existence of a fifth (highest) stage of problem confrontation that is seemingly parallel to the recently published first indication of a fifth Piagetian stage (Arlin, 1975).

Although I have been dealing with Brainerd's analysis in terms of what he calls descriptive theories, it is possible to adduce data on the level of his explanatory theory. It was pointed out by Fodor (1975) that the very existence of stages implies the inevitability of an externally-given expansion of a system if it is to go from one stage to another. Thus, my own work (1974 I) has shown the existence of brain growth stages in human beings, and my best current information places them at three to ten months of age, and then from two to four, from six to eight, from ten to twelve or thirteen, from fourteen to sixteen or seventeen years of age. These are the normally occurring "spurts" in brain size, and the last four occur without new cell formation, so that we are perforce talking about increases in neural network complexity. I have also been able to show correlated spurts in mental age and various other measures of intelligent behavior (Epstein, 1974 II); the connection with the traditional ages of onset of the Piagetian stages is quite obvious.

The references above are a sampling of sources of data and insight that need to be taken into account in evaluating the likelihood of the existence of intelligence growth stages.

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Structural explanation of developmental change. Two of the most important issues raised by Brainerd involve the nature of developmental explanation and the sense in which developmental change can be conceptualized in terms of stages. I will argue, first of all, that he has rejected Piaget's developmental explanations for the wrong reasons and, second, that the data on cognitive development require a concept-like stage or phase or level, even though cognitive development is probably not discontinuous or abrupt.

Nature of developmental explanation. Brainerd seems to argue that the only proper explanation of cognitive development comes from a causal theory: What antecedent conditions produce development? He thus underestimates the usefulness of another class of explanations – structural theories. (Note also that theories of antecedent conditions are only one type of causal theory. Structural theories, such as systems analyses, provide another type of causal explanation. For example, Newton's equation $F = M \cdot A$ provides a causal explanation for changes in force, even though it does not specify whether changes in force, mass, or acceleration are antecedent.) In cognitive-developmental psychology, a structural theory aims to specify a set of behavioral structures for analyzing cognitive performance, together with a set of transformation rules for specifying how one type of structure develops or transforms into another (Fischer, 1972; Piaget, 1968/1970). The strongest test of such a structural theory is whether it can predict both developmental sequences and synchronies in cognitive development. If a theory can consistently predict novel sequences and synchronies, then it most certainly does provide an explanation of cognitive development. In the same sense, the periodic table of elements provides an explanatory theory in chemistry; the Newtonian equations, such as $F = M \cdot A$, are an explanatory theory in physics, and the Mendelian rules for gene combination are an explanatory theory in genetics.

As Brainerd's examples illustrate, Piaget has attempted to formulate a structural theory to explain cognitive-developmental sequences and synchronies. But the essential argument against Piaget's theory is not that it is structural rather than causal. The essential argument is that Piaget's structural analysis does not accurately predict sequences and synchronies. As Brainerd ably indicates, Piaget's predictions of sequences are often wrong (Kofsky, 1966; Fischer, 1977) and his predictions of synchronies are usually wrong (Brainerd, 1978; Flavell, 1971; Jackson, Campos, & Fischer, 1978; Liben, 1975; Fischer, 1977). The problem with Piaget's theory is not that it fails to explain. The problem is that some of its explanations are apparently false.

The weak predictive ability of Piaget's theory is perhaps less evident than it should be because the typical methodology used in Piagetian studies does not directly test for sequence or synchrony. A sequence is typically tested by looking for differences in mean age for passing tasks in the proposed sequence, tested with cross-sectional samples. This is not an adequate test. A rigorous test of a sequence requires either scalogram analysis with a cross-sectional sample or longitudinal analysis. Many attempts to subject Piaget's sequences to these rigorous tests have failed to confirm the sequences (Kofsky, 1966; Roberts & Corbitt in Fischer, 1977).

The test typically used for synchrony is the correlation coefficient, which is also an inadequate test as normally used. Any two variables that change with age will show a significant correlation for a sample that varies in age. For example, Roberts, Corbitt, and I found a high correlation between a developmental sequence of classification skills in 2- to 7-year-olds and shoe size, $r(58) = .88$, $p < .001$ (Roberts & Corbitt in Fischer, 1977). But who would want to argue that shoe size and classification share the same structures and develop in close synchrony? Most of the correlations in the cognitive-developmental literature that are used to support predictions of synchrony are substantially lower than this correlation between classification and shoe size. More rigorous analysis with a stricter criterion indicates that, contrary to Piaget's predictions, developmental synchrony is a rare event (Flavell, 1971; Fischer, 1977).

Predicting sequence or synchrony is no easy matter. Even many of the Piagetian sequences that Brainerd attributes to measurement artifact have not been confirmed by rigorous tests. Many cognitive-developmental psychologists would be more than pleased if it were as easy to predict so-called measurement sequences as Brainerd implies. He defines measurement sequences as developmental scales where every task is composed of the task immediately preceding it plus something additional. The problem with this definition is that it assumes that analysis of the skills involved in a task is a simple matter. Yet investigators have no accepted method of task or skill analysis, and so in most cases cannot agree a priori on an analysis. Consequently they cannot readily predict developmental sequences, especially microdevelopmental sequences.

In addition, many studies fail to support the "obvious" analyses of what includes what in a series of tasks (Kofsky, 1966; Liben, 1975). Indeed, in my laboratory we have made a number of predictions based on a new system of skill analysis (Fischer, 1977) that have gone against the seemingly obvious analyses, and so far all such predictions have been confirmed. For example, in a pretending task with preschool children, a story that involved three independent agents (dolls) developed before a story that involved just two independent agents (Watson, 1977). In the development of classification skills, a task that included a simpler task plus something additional developed virtually simultaneously with the simpler task, as predicted (Roberts & Corbitt in Fischer, 1977). Several other studies have produced similar nonobvious results (Bertenthal & Fischer, 1978; Jackson et al., 1978; Watson & Fischer, 1977).

Suppose, then, that cognitive-developmental psychologists eventually do produce a structural theory that can predict and explain sequences and synchronies. The question still remains: Under what circumstances would a concept of stage be justifiably retained?

Concept of stage. As Brainerd indicates, the data on cognitive development do not support the classic concept of stage as an abrupt, discontinuous change in performance at a certain age. Nevertheless, the evidence does seem to support the existence of three different phenomena that justify the retention of some sort of stage concept.

First, development seems to produce qualitative changes in behavior – the emergence of new kinds of skills. Piaget, despite his methodological problems, has documented a large number of instances of such qualitative changes. Perhaps the most dramatic is his description of how representational ability develops out of sensorimotor action (1936/1952; see also Bertenthal & Fischer, 1978; Watson & Fischer, 1977).

Second, children seem to show an upper limit on their performance at a given age, an optimal developmental level. That is, for a given child, there seems to be a point on any specific developmental scale beyond which the child cannot perform, even with extensive training. This optimal level probably has some generality across task domains, although a particular child may have several different optimal levels in different skill areas (Fischer, in press). The more uniformity there proves to be in optimal levels in diverse areas, the more justified is a concept of stage.

Third, recent evidence indicates that, at least in infancy, development seems to produce systematic shifts in populations of skills (Feldman & Toulmin, 1975) that produce a kind of statistically defined stage (McCall, Eichorn, & Hogarty, 1977; McCall, in press). Across a wide sample of skills, infants seem to show periods of transition and periods of consolidation that look very much like a weak form of stage. The changes are not truly abrupt or discontinuous, but they do seem to show major developmental shifts in a large number of skills at certain points in development. The shifts do not support the stages suggested by Piaget (1936/1952) for infancy, but they do correspond at least roughly to the stages or levels suggested by several other investigators (Uzgiris, 1976; Fischer, 1977).

Given this evidence for stages and given the problems with Brainerd's analysis of the nature of developmental explanation, I would conclude, contrary to him, that the problem with Piaget's theory is not that it is structural but rather that it is wrong in important ways. These difficulties with Piaget's approach in no way lead to the conclusion that structural theories cannot explain development or that all concepts of stage or phase or level should be eliminated from cognitive-developmental psychology.

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Developmental stage: explanans or explanandum? It is hard to argue with some of the points made in Brainerd's paper. There is in fact a growing feeling in the field that Piaget's stage model of cognitive development is in serious trouble. Brainerd is correct in saying that "Piaget has proposed structures for which there are no extant tasks, let alone corresponding behaviors." Many Piagetian cognitive acquisitions do seem to appear either earlier or later in ontogenesis than the stage model should have it, although how "appearance" should be defined and measured is admittedly a very difficult problem (Flavell, 1977). Because of these and other difficulties, Piaget's stage model is proving increasingly less credible to developmentalists and less useful to educators. There is reason to doubt whether "stages" of the broad, Piagetian "period" variety (e.g., the concrete-operation period) will figure prominently in future theorizing about cognitive development. However much we may wish it were otherwise, human cognitive growth may simply be too contingent, multiform, and heterogeneous – too variegated in developmental mechanisms, routes, and rates – to be accurately characterizable by any stage theory of the Piagetian kind.

This does not mean that developmental psychologists will not continue to talk about "stages" of a briefer and narrower kind. We may have to give up on grand and sweeping developmental periods that try to find a single, uniform "deep structure" description of all the thinking the child does at a given age. However, we will surely need to keep looking for developmental steps or levels within a single conceptual domain or subdomain. Moreover, there is no reason to believe, contra Brainerd, that the steps we find here will be any more arbitrary or less natural than those found by any other scientist who tries to capture changes over time, for example, the geologist. One of our major tasks will continue to be the Piagetian one of simply telling interesting developmental stories, that is, of finding out what major, "natural-unit-looking" acquisitions precede and follow what others in appropriately narrow areas of cognitive functioning. It does not much matter, to me at least, whether such endeavors be called "descriptive" or "explanatory." (What exactly *does* it mean to "explain" cognitive growth?) However characterized, they are both necessary and very hard to do.

I believe Brainerd fails to distinguish clearly between two explanatory tasks concerning stages. One task is to determine what explains stages and the other is to determine what stages explain. The difference can be illustrated by the different ways that the term "process" (or "underlying process") gets used in the field of cognitive development. When we speak of the "process of development," we (vaguely) have in mind trains of events that help bring something developmentally novel into being. If that something novel is conceptualized as a stage, then we might want to say that this process of development helps explain the advent of that stage. To use one of Brainerd's examples, those antecedent neurological variables comprise the process of development that explain the advent of the *Amblystoma* flexure stage. On the other hand, when we speak of the "process" that underlies a conservation response, we are invoking a present process to explain the occurrence of a present overt behavior pattern. If this process is conceptualized as characteristic of a stage (e.g., it consists of concrete-operational compensatory thinking), then we are talking about what a stage explains rather than what explains a stage. If the Piagetians are right and their critics are wrong, Piagetian stages really do explain cognitive performance, that is, they describe very general and generalizable processes that generate ("explain," in that sense) characteristic observable behaviors. But that does not mean that the developmental emergence of those "explanatory stages" itself need have a known explanation. In short, Brainerd seems to be claiming that for a stage to be explanatory the antecedent variables that explain its provenance should be identifiable and independently measurable. Piaget's stages already seem nonexplanatory enough without adding this patently unnecessary requirement.

Finally, most of the points made about measurement sequences are very similar to those already made in Flavell (1972 *op. cit.*). It still seems to be, however, that we should treat what appear to be measurement sequences very cautiously and conservatively: "Philosophers who have recently discussed the problem . . . emphasize that the path from logical to developmental priority can be an extremely slippery one, and that psychologists may follow it only at their peril" (Flavell, 1972, *op. cit.*, p. 331). What looks like a measurement sequence to one developmental psychologist may not to another. For instance, I am far from convinced that the conservation of quantity → conservation of weight is a measurement sequence (or even if it is a completely invariant sequence). It is surely not a measurement sequence for the reason Brainerd gives: ability to conserve weight consists of ability to conserve quantity plus knowledge of how to operate a pan balance. I think it would be easy to demonstrate a regular quantity conservation → weight conservation sequence in a sample of children, all of whom were perfectly competent operators of scale business. I suspect, in fact, that this sequence could have turned out to be quantity → weight in some milieu, and if it had, that someone would have found a way to "prove" that it could not logically have been otherwise. We would do well to verify the sequentiality of at least certain putative measurement sequences empirically, and even after we have done that we should continue to ask questions about them. What, exactly, are the cognitive processes that actually mediate earlier X and later Y? Why exactly, does Y develop only after X has developed? How well developed does X have to be before Y can begin its development, and why? And so on. The discovery of a robust ontogenetic sequence, whether measurement-like or other, should be the beginning rather than the end of intensive developmental inquiry. This is true regardless of our belief or disbelief in the psychological reality of Piagetian stages.

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Cortical maturation: an antecedent of Piaget's behavioral stages. The most probable biological antecedent of the behavioral stages described by Piaget would be the maturational level of the brain, particularly of the neocortex. Extant data on cortical maturation are considerable and they have important implications for Piaget's theory. In both monkeys and human beings (Flechsig, 1927; Conel, 1939–63; Gibson 1970, 1977), nearly all areas of the cerebral cortex contain rudimentary myelin by late infancy, that is, by three months of age in the rhesus monkey and by six months of age in the human infant. Cortical myelinization continues throughout childhood and does not reach a mature stage until puberty or later. Similar maturation patterns have been described for other cortical histological parameters (Conel, 1939–63). Maturation patterns are gradual; no postinfantile period of sudden histological change has been identified. Since neurons can function prior to axonal myelinization (Ulett et al., 1944), these data suggest that each cortical area possesses incipient function in infancy; no cortical area suddenly begins functioning in later childhood, but the cortex, nevertheless, continues to mature functionally throughout childhood.

To the extent that cognitive abilities are determined by cortical function, cognitive maturation should be characterized by gradual increases of abilities already in existence in rudimentary form in infancy and not by dramatic additions of entirely new modes of cognition. In other words, cortical maturation data are consistent with Brainerd's view of behavioral development as a smooth, gradual process and with Piaget's behavioral descriptions, which posit that each stage develops from and integrates behaviors of preceding stages through a series of numerous intermediary steps. Neuroanatomical data are inconsistent both with the concept that Piaget's stages can be discretely divided from each other on the basis of cortically determined cognitive capacities wholly unique to each state and with Brainerd's hypothesis that behaviors that develop in sudden fits and starts are more likely to have a maturational base than behaviors displaying a gradual developmental pattern.

The hypothesis that cognitive maturation should be characterized by gradually increasing capacities based on cortical functions existing from infancy demands the identification of such functions. Preliminary efforts indicate that a number of cortical functions are identifiable and are compatible with Piaget's behavioral descriptions of the sensorimotor period; these are rudimentarily present in late infancy and capable of developing increased efficiency and quantitative capacity with age (Gibson, 1977). Of particular importance to Brainerd's discussion are the parameters of mobility or flexibility (the ability to inhibit one response, perception, or idea in order to move on to another), internalization (the ability to evoke images or ideas of perceptual events in the absence of the relevant environmental stimuli), and simultaneous and sequential synthesis (the ability to synthesize individual perceptions, ideas, or actions into new simultaneous or sequential synthetic wholes). Each of Piaget's stages is characterized by increasing capacities in each of these dimensions. In passing from overt sensorimotor operations, to concrete operations, to formal operations, the child is utilizing ever greater degrees of internalized thought. From the ability to merely find his way home to the ability to synthesize each step of the process into one coherent map, or from understanding the behaviors of the snail and of the cardboard individually to the ability to comprehend their interrelationships, the maturing child is demonstrating the ability to synthesize ever increasing quantities of information (Piaget & Inhelder, 1969, Piaget, 1972). Indeed, it can be argued that Piaget's stage of formal operations, characterized as it is by the emphasis on ideas and hypotheses and by the capacities of combination and double reversibility, is basically the maturational culmination of the capacities of mobility, internalization, and synthesis. Neuroanatomical data are consistent with the concept that these particular parameters are maturationally based and should be among the latest neurologically based abilities to reach maturity as they are mediated primarily by the parietal, temporal, and frontal association areas, which are the latest neuroanatomical areas to mature (Luria, 1966; Gibson, 1970, 1977).

Piaget's theory is based on the concept that each stage results from and integrates previous behaviors; as such, it appears, according to Brainerd, to be a theory of measurement sequences, that is, of behaviors in which "each item in the sequence consists of the immediately preceding item plus some new things." Contrary to Brainerd's suggestion, this does not imply that these behaviors have no maturational basis. Brainerd overlooks certain points. If it is possible to acquire a behavior in one sequence only, this implies that the method of behavioral acquisition is determined by brain function. Frequently, this will be the case even if the behavior is culturally transmitted. If a culture is to survive as a culture, it cannot transmit information arbitrarily, but must do so at appropriate maturational stages and in a manner compatible with neural processing mechanisms. Moreover, even though behavior X may consist of behavior Y plus some new things, this does not mean that once Y is acquired X automatically follows. The "new things" required by X may demand still immature abilities. Although knowledge of the alphabet is a prerequisite to writing, the possession of such knowledge does not assure comparable maturity of the motor skills essential to writing. Similarly, because a child can understand the behaviors of both the snail and the cardboard individually does not assure that he possesses the requisite synthetic capacities essential to understanding the interrelationships between the two.

Many of the measurement sequences described by Brainerd demand synthetic capacities. As these capacities are mediated by areas of the cerebral cortex, which mature slowly over time, it is probable that the ability to acquire many measurement sequences is itself under maturational control. Behaviors acquired as measurement sequences should be used as prima facie evidence neither for nor against maturational control. To demonstrate that a given measurement sequence cannot be under maturational control, it is essential to demonstrate that once behavior Y is acquired, behavior X may follow immediately thereafter with no intervening developmental time lag.

In summary, neurological data suggest that Brainerd is correct in his suggestion that Piaget's behaviors develop smoothly through a series of measurement sequences, but incorrect in his suggestion that this implies no maturational control.

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On stage: the importance of being a nonconserver. In response to Brainerd's paper on the concept of stages in Piagetian theory,[1] it seems inappropriate to reiterate traditional Genevan counterarguments, since the problem has been amply debated in the past (Tanner & Inhelder, 1956, 1960; Pinard & Laurendeau *op. cit.,* 1969, etc.). Thus, I shall not embark upon yet another discussion of the paper's numerous misconceptions of Piagetian theory. Nor will I go beyond noting in passing the narrow views expressed on scientific methodology and explanation (Strauss, 1974) and the fact that Brainerd's version of Piaget's theory is in need of substantial updating. As Beilin (1977) has stressed, the theory has undergone constant refinement by Piaget himself (e.g., Piaget et al., 1968a, 1968b; Piaget & Garcia, 1971; Piaget 1972, *op. cit.,* 1974, 1975, 1976, 1977; Inhelder et al., 1975). The importance of Brainerd's paper resides, in my view, in the challenge he offers as a justification for resuscitating the stage problem, even if he proposes no alternatives to Piaget.

It is useful to begin by clarifying the role of "stages" in Piaget's theory, since they are clearly not the core of his constructivist-interactionist epistemology, as is so often assumed. The central thread of Piaget's work has never ceased to be the search for equilibration mechanisms that engender *new* behavioral patterns (Inhelder et al., 1977). The concept of stage-like development was at no time divorced from that main endeavor. Stages were initially used by Piaget as a *heuristic* for seeking far from obvious developmental links across widely differing conceptual domains. From the outset, Piaget's stage distinctions were not based on success or failure per se, but always pinpointed intermediate, oscillatory levels that Brainerd himself recognizes as a stage-defining criterion. In all events, a distinction must be drawn between, on the one hand, the idea that there exist certain fundamental qualitative changes that might be described as "stages," and, on the other hand, the particular mathematical formalization chosen to represent them. Piaget's formalization (an adaptation of the Bourbaki system) merely reflects the state of the art in the 1950s. Since then, Piaget and his colleagues have considered other formalizations (e.g., Piaget, 1977; Piaget et al., 1968a; Céllérier, 1972; Wermus, 1976). With the shift of emphasis from logical operations to physical causality and, more recently, to the procedural aspects of goal-oriented behavior (e.g., Inhelder et al., 1976; Gilliéron, 1977), the heuristic value of the stage concept has for some time been secondary in Genevan theory and research.

It seems inappropriate at this juncture to debate the merits and demerits of any particular formalization. The central issue appears to be whether or not slicing development into stages remains heuristically valid on today's psychological scene. Due to space limitations, I shall not discuss microdevelopmental changes, but shall concentrate only on the macrodevelopmental level. Leaving aside arguments based on misunderstandings, there are two main objections to stages in Brainerd's paper: (1) the problem of horizontal décalages and (2) the fact that a stage-defining characteristic of stage n+1 is already present at stage n. The décalage problem has justifiably been one of the major criticisms levelled at Piaget's stage concept for many years. However, Bovet et al. (1975) have recently clearly demonstrated that conservation of volume, previously situated at roughly twelve years, exists in one form already at eight years, that is, at the same time as weight conservation. Moreover, as Piaget and Garcia (1971) had already shown when one differentiates weight as a quantity and weight as an action, new difficulties arise regarding another form of weight conservation, which brings the latter to a level similar to that of the later form of volume conservation. Such studies of how complex interactions develop among mass, energy, density, and so forth, will, when completed (Bovet et al., in preparation), probably represent an empirical and theoretical challenge to the horizontal décalage issue.

With regard to Brainerd's other objection, that is, the presence of n+1 defining-behavior at stage n, two counterarguments can be made. First, deep qualitative differences often underlie superficially analogous behavior. Thus a seven-year-old can indeed be taught to multiply "weight × distance" in a balance scale task, and thereby seemingly to perform like the twelve-year-old. Decenter the fulcrum and allow weights to be hung only at endpoints, and again both age groups manage to apply the multiplication procedure to make the arm balance. However, once balance is achieved with the noncentered fulcrum, the experimenter asks the child to add two more weights. Unlike the twelve-year-old, the seven-year-old will then add two *identical* weights at each end, as if he thought that he was starting from a state of static equilibrium now (Piaget & Karmiloff-Smith, in press, a.). Thus, underlying the identical external behavior in the original tasks were totally different implicit notions as to *why* one uses the "weight x distance" procedure. Physical concepts such as "weight" and "length" are filtered by the seven-year-old through representations of *instantiated parameters* of objects simultaneously having other properties; only much later can they acquire different status and become, where necessary, *abstract concepts* that have no concrete materialization.

This type of reasoning does not hold only for physical reality; many authors have claimed that seven-year-olds can cope with propositional logic, for example, with structures such as "if X then Y" (Joffe-Falmagne, 1977). A close analysis will show that in such cases the seven-year-old is working on a *concretizable statement* represented roughly by "each time X, then most likely Y," leading to a correct but only "plausible" conclusion. The twelve-year-old, on the other hand, is working on the *noninstantiated proposition* ("if") *regarding a hypothetical world*, leading to a "valid" conclusion (Johnson-Laird, 1977).

While I believe that Brainerd has misinterpreted the structural operations inherent in the INRC group, particularly with his one-to-one mapping onto task parameters in a concrete operational domain, let us accept that there are indeed stage-defining structures belonging to n+1 that are already present at the time of stage n. This may hold true at both structural and procedural levels. It has been shown recently, for instance, that seven-year-olds tend to use several different procedures in a juxtaposed fashion, but among those procedures there is also the less context-dependent, single procedure used by much older children (see Karmiloff-Smith, 1976a for examples in child language acquisition, and Piaget & Karmiloff-Smith, in press, b, for examples from children's map-reading). Does it necessarily follow that stage distinctions break down? I would tend to argue to the contrary, based on the following interpretation: when behavioral pattern X appears at stage n, it may be elicited by *data-driven* behavior and thus X is each time *recomputed* afresh by the child. The stage-defining feature of stage n+1 may therefore lie in the fact that the same behavioral pattern X now has the status of a *hypothesis-driven primitive* (a chunk) for the child. Such an interpretation could offer an account for the apparent paradox of simultaneously invoking discontinuity and continuity of cognitive growth – a core aspect of the originality of Piaget's epistemological stand.

Much of the foregoing speaks in favor of slicing development into qualitatively different stages or "cognitive Weltanschauungen," irrespective of the particular formalization one attaches to it. However, I would argue that the stage concept can only remain a valid heuristic today if the stages described represent more than an analytical tool for the observer and are shown to be *psychologically functional for the child*. In other words, a shift of emphasis is suggested from conservation-attainment to the psychological function of conservation-seeking (Karmiloff-Smith, 1976b), from the logical necessity of final levels to the psychological necessity of the stages leading to them. The typical "errors" of the nonconserver should be analyzed in terms of whether they represent powerful heuristics in development or merely shortcomings to be surmounted later. There are many telling examples in the early literature that speak to this point. In the infralogical sphere, the construction by small children of parallel and perpendicular lines (e.g., water level drawn parallel to the base of a tilted bottle, trees drawn perpendicular to mountain slope, etc., Piaget et al., 1948) may be considered essential components of the child's general geometry, even if in particular cases the child must learn to use interfigural rather than intrafigural referents. Likewise, children under three years old can be shown seemingly to conserve number because they make no eye movements towards the endpoints of the spatial display. When at roughly age four children attend to spatial layouts and then therefore fail number conservation tasks, surely the attention to spatial parameters can nonetheless be looked upon as a powerful heuristic they have constructed for coping with most daily situations of equivalence judgments? The psychological function of nonconservation behavior (i.e., attending to spatial layout, to one parameter at a time, the juxtaposition of procedures, etc.) can be interpreted as stemming from the child's constant endeavor to gain predictive control over his environments. If the child did not remain for a time at a given "stage" but was continuously trying to take new information into account, he would not have the opportunity to consolidate his procedures in the first place. Similarly, the forging of overly strong theories by children (Karmiloff-Smith & Inhelder, 1975), or even the use by the researcher of stage-defining characteristics, are still further instances of the psychological need to filter input through a stable referent, even if that referent is partially wrong. Thus, "being at a stage" allows for both simplification and unification of otherwise heterogeneous data.

It seems premature to take a firm stand now as to whether development should be sliced into three or more macrodevelopmental levels, irrespective of their formalization. Recent functional analyses of child language (Karmiloff-Smith, in press) suggest that essential developmental changes take place at around age five and again at eight. Perhaps "procedural explosions" might also be considered as stage-defining candidates. I have in mind the child's sudden and quite overwhelming tendency to "name the world," "to count the world," "to classify the world," and so forth. The shift of emphasis would thus be from the structures inherent in, say, classification behavior to the *function* that such classification behavior has for the child in gaining a grip on his complex world.

Like Brainerd, I shall conclude on a pedagogical note. If child "errors" have turned out to be powerful heuristics for development, then well-meaning learning theorists who train small children to ignore perceptual cues, to sidestep misconceptions by reciting verbal rules, and so forth, are doing these children a great disservice. They seem to lose sight of the fact that there is a profound psychological importance in being a nonconserver.

NOTE

1. This commentary should not be regarded as necessarily representative of the "Genevan" position, but merely expresses the views of the author.

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On falsifying descriptions. Brainerd's essay is lively, challenging, enlightening, and, in view of the current popularity of Piaget's theory, courageous. Many of the criticisms of Piaget's theory are cogent, particularly the one that raises the question of whether the putative invariance of his stages represents an empirical or a logical sequence (Gagné, 1968). But when Brainerd writes that, whereas Piaget's stages are perfectly acceptable as descriptions of behavior, they have no status as explanatory constructs, he is making a distinction between description and explanation that is more apparent than real. Such oversimplification should not slip past notice because it is embedded in the context of an attack on Piaget. In a related vein, when he brands as circular the statement that a child does not do such and such because he is at some given stage of cognitive development, he seems to be attacking the stage concept in general rather than Piaget's usage of it. It is to the validity of these statements that this commentary is directed because they seem to imply that establishing developmental regularities, without being able to describe the conditions that produce them, is pointless. With this implication, I take issue.

It is useful, first, to define the subject matter of the developmental psychologist as age-related behavioral change, or what Wohlwill (1973) refers to as developmental function. If the change is quantitative, the developmental function states a relationship between some quantitative aspect of behavior and time since birth; a proper analogy would be the law of falling bodies, which states the relationship between time since the body began to fall and its speed. If the change is qualitative, the developmental function describes a particular sequence of behaviors as a function of age: an appropriate analogy would be embryological stages or stages in the evolution of a particular species. True, developmental function is not causal, but it is explanatory insofar as it subsumes individual events into a general category and predicts their behavior over time. Such a descriptive function can be of a low level, in the sense that it refers to a very limited set of behaviors, measured under a limited set of conditions, but to be useful at all it must at least be generalizable to a range of individuals. Therefore, to say that a given child is doing something at time T because he is in, say, stage two of a three stage sequence is not meaningless. The statement not only places the child in his proper category but also it indicates that he was in stage one at time T_{-1} and will be in stage three at time T_{+1}. It is explanatory in the same sense that it is explanatory to say that a given baby's heart has started to beat because he is in the beginning of the foetal stage. Note that such a statement is *not* circular, even when you define the stage by the occurrence of the given behavior because the prediction of the future event that is implicit in the statement is not logically necessary; it is, in fact, empirically falsifiable.

Actually, even low-order descriptions entail abstractions whose validity may be measured by whether they reveal regularities. High-order descriptive functions, such as Piaget uses to describe his postulated stages of development, are intended to subsume a number of ostensibly different behaviors in a wide variety of conditions and are presumably applicable to all normal human beings. If these descriptions were "acceptable" then, their use as explanatory statements would surely not be tautological. To say that a child is in stage two on the basis of one type of behavior in one set of conditions would specify his behavior in a wide range of other conditions and predict what the future holds for him. If Piaget's theory fails on this count, it seems to me that it is not because such a theory has, in principle, no explanatory power, but rather because there is insufficient coordination between the theoretical constructs Piaget employs and the behavior they are designed to explain.

Of course, like Brainerd, we could all wish for the increased explanatory power incurred by knowing antecedent conditions. However, we had best be realistic about this matter, expecially when it comes to human cognitive development. First, there is little point in seeking antecedents until it is clear that there are important developmental regularities of some generality; regularities that are age- and not task-determined. Secondly, if such developmental functions can be found, they would presumably reflect changes that occur in the real world over a very long time period. Such changes would doubtless be due to some complex interaction between both maturational and long-term environmental factors, factors that are not celebrated for their amenability to specification, measurement, or experimental manipulation. Specifying these antecedents, I fear, will take more time than the talented but impatient Brainerd wants to allow.

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Maturational succession vs. cumulative learning. Brainerd characterizes Piagetian stages as a description rather than an explanation of behaviors that undergo age change, and as essentially constituting measurement sequences. This does not do full justice to the implications of Piaget's stage model for the acquisition of cognitive skill. We will discuss these implications in principle, rather than in terms of empirical findings.

Brainerd stipulates three criteria that a model has to meet in order to have explanatory status, and then demonstrates satisfactorily that Piagetian stages fail to meet these criteria. We are not compelled to accept his criteria. In fact, they do specify one kind of explanation, but erroneously exclude another.

A model can be explanatory by either shifting the level of explanation, or by organizing the phenomena in question at their own level. Coghill's model, used as illustration by Brainerd, is of the first type. Amblystoma embryos behave in certain ways at certain ages "because" the Amblystoma nervous system is in different maturational states at those different ages. This level-shifting exercise appeals to psychologists because it shifts the burden of explanation into the arenas of "hard" science – neurophysiology, developmental neuroanatomy, molecular biology, and so forth. But there is no inherent reason why those models that are explanatory at the same level as the phenomena they organize and have simplifying and predictive value should be less valued.

Piaget's stage model claims organizing value. Its generalizations explain why particular behaviors become available at a given stage – it is because they are instances of the application of the same cognitive principle. Knowing the principle, we can predict which further behaviors should and should not be available to the child at the stage in question. Whether one can predict is an empirical question. That one could is a point in principle, and illustrates the fact that the Piagetian stage model is potentially explanatory.

To think of Piagetian stages as measurement sequences ignores the obvious fact that they conform to a maturational succession of events rather than to a cumulative learning experience by a fully mature organism. Anyone who enriches his behavioral repertoire will necessarily begin with the basic concepts and then continue into increasingly complex refinements of the application of these concepts. However, if it is just a matter of acquiring the necessary information, and if the environment has this to offer, then the rate at which the material can be learned is the only limiting factor on the rate of acquisition of the additional behavioral skills.

It is quite different with Piagetian stages. The child, at some early stage of cognitive development, operates at a particular level. No amount of didactic input will enable him to function at a substantially more complex cognitive level. Substantial time must elapse before he is ready to use more complex forms of reasoning. The amount of time that this takes is determined not by what the environment has to offer, but by the internally programmed maturational characteristics of the organism. Indeed, one may hold the environment constant and find a great range of individual differences in the time it takes to move from stage to stage.

With respect to the maturation of any physical or behavioral characteristics, there are three periods: the one preceding the onset of the development, the one during which the facility develops, and the final one during which its development has reached asymptote and remains static. It is obvious enought that this is true of the development of physical characteristics of the organism. It is also obviously true of such cognitive developments as that of language. Piaget has shown that the same principles apply to the acquisition of logical operations.

Brainerd claims that from research on measurement sequences we learn primarily about measurement procedures. Apply this view to the acquisition of height by the growing child. The fact that one has to be four feet tall before one can be five feet tall is obvious from the point of view of measurement sequence. But surely we learn more by measuring height than simply information about how to measure height.

The same logic applies to Brainerd's claim that horizontal décalage "becomes another restatement of the sequencing principle." Within a stage, development is not arrested. Rather, the mental operation that has become available finds even more sophisticated application as maturation proceeds. For instance, some applications of the conservation principle are well known to become possible before others. Indeed, the same applies to a single type of conservation. Thus, conservation of number does not suddenly become available for all sets and all stimulus types. We have found children able to conserve sets of three stimuli before they could conserve sets of five, and able to conserve five before seven. Again, for a given set size, children conserve earlier if the stimuli are relevant (candy) than if they are neutral (tokens). But such phenomena cannot be explained as measurement sequences. Otherwise it would be simple to teach the child one form of conservation, given understanding of another, conservation of a greater set size, given ability to conserve a lesser set size, and so forth. But it is not. Time has to pass and, by implication, brain maturation has to progress before the child becomes able to solve the more difficult problems.

Piaget has shown that during cognitive growth, children gradually become capable of more complex mental operations. His descriptions of the types of operations of which children are capable at various "stages" in their cognitive development have sufficient generality to generate testable predictions about children's performance on various tasks. This illustrates the explanatory value of the Piagetian stage concept.

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Rages over stages. If Brainerd's goal is indeed to have us view his essay "like something of a Rorschach stimulus," then he has been successful. His argument embodies the key properties of a Rorschach: vagueness, ambiguity, and unresolved themes. Like the good Rorschach that it is, the essay contains little objective structure of any depth, and it is likely to evoke responses that tell more about the respondents than about the stimulus.

On the essay. Before I react to the projective demands of Brainerd's essay, I will give brief examples of three of its curious features: self-contradiction, misrepresentation, and naiveté.

Self-contradiction. At the outset, Brainerd establishes as his goal the stimulation of "other writers to formulate new proposals about how to identify discrete changes in cognitive development." But since he believes that the *description* of behavior is not a problem for Piaget's stage theory, it is difficult to see how better identification procedures would solve the problem he has addressed. Furthermore, the goal is predicated on one of the very assumptions that Brainerd finds dubious: that discrete changes in cognitive development occur at all.

Misrepresentation. Piaget is correctly quoted as having said ". . . This might lead us to assume that some biological factor such as maturation is at work" (1970 *op cit.*, p. 712). However, in the next sentence, *not* quoted by Brainerd, Piaget goes on to say, "But it is certainly not comparable to the hereditary neurophysiological programming of instincts." And in the same paragraph: "It would therefore be a mistake to consider the succession of these stages as the result of an innate predetermination, because there is a continual construction of novelty during the whole sequence" (p. 712).

Brainerd's selective quotation provides the basis for his subsequent argument and implies a maturational "acknowledgement" that Piaget explicitly denies.

Naiveté. In his provocative paper on process-structure distinctions in developmental psychology, Newell (1972) indicates the arbitrary nature of our characterizations of stability, change, abruptness, and continuity. What we see depends upon the grain of our measurement instruments, both in the level of aggregation – or *scope* – of our unit of analysis (consider, for example, the continuum of tasks from conservation of number, to elementary quantification, to simple reaction times) and the time scale of our observations (years–days–hours–milliseconds). Explanations are tested by choosing two different scopes or time frames (or both), and then measuring the effects of variations in the less aggregate variables on the behavior of the more aggregate.

Brainerd's essay is insensitive to these distinctions. Either all implied measurements are at the same global level and hence "a behavioral description is not an explanation," or else we are asked to create a theory capable of making the immense leap from neurological events to performance on a logical task.

Even in the "easier" realm of adult cognition, there are no theories that successfully span multiple levels. The cognitive sciences are based on the strategic bet that several intermediate levels of explanation are necessary to achieve these goals (Anderson & Bower, 1973; Klahr & Siegler, 1977; Simon, 1975). It is naive to imply, as Brainerd does, that we could go from neurology to formal operations if we would just stop being enamoured of Piaget's theory.

On the issue: theory in developmental psychology. One productive way to view the enterprise of theory construction is to characterize it as a search through a large problem space (Newell & Simon, 1972). Different theorists make different decisions about how to traverse the many paths available to them in constructing their theories and in deciding how to gather more information to help them get to the next state (i.e., the next theoretical statement). As in the search of any complex problem space, there are some states from which further progress appears to be impossible. Others appear to have the potential for success, but only at extremely high cost, that is, some subproblems are harder to solve than the higher order problem that generated them in the first place. One proceeds through such problem spaces using a variety of scientific problem-solving strategies, heuristics, and rules of thumb. The resultant searches exhibit looping and backup, as well as progress toward the goal.

Given this view, one can evaluate advances in a field in terms of the cumulation of knowledge about the nature of the problem space. Perhaps the field of developmental psychology has been reluctant to scrap the stage notion because of an apparent contradiction: On the one hand, stage theory is the most central and the most vulnerable part of Piagetian theory; on the other hand, Piaget's overall research program has made an immense contribution (Flavell, 1977, calls it "stupendous") to our current state of knowledge about cognitive development. But there should be no conflict here, for as we know from the problem solving literature (Newell & Simon, 1972), unsuccessful solution paths often provide useful information about how to search effectively for the successful path.

Brainerd's essay appears to be a hasty consolidation of the many criticisms of Piaget's stage theory that have been voiced over the past five or ten years. From them, he has fabricated a large stop sign and placed it at the stages-as-explanation node. He argues that this particular path leads to either an obvious dead end or a state from which further progress will be very costly. Perhaps he is right. Unfortunately, he offers no suggestions about the directions in which we might fruitfully pursue our search for a viable theory of cognitive development.

Flavell elegantly raised many of these objections several years ago in his two influential papers (1971, 1972, *oper. cit.*), and Wallace and I (1976) started the description of our research program by acknowledging the difficulties inherent in the stage theory viewpoint. We then proceeded from an entirely different point of departure in our attempt to formulate a developmental theory, that is, from an information processing orientation. Our attempt to deal with the stage problem is really quite different from the stages-as-descriptions characterization that Brainerd offers for the information processing approach. Since Brainerd has set the tone for repetition of old themes, let me clarify this position by offering my own refrain (1976, pp. 13–14):

"During the course of cognitive development, the human information-processing system undergoes a sequence of state changes. Changes occur in the 'hardware' – in the physical rates and capacities – and in the 'software' – in the content and organization of processes and data structures. Since we have limited access to these states, we infer them from behavior. Some of the changes over time in children's performances are so striking, so qualitatively different from Period 1 to Period 2, and so coherent across tasks presented during Period 1 or during Period 2 that we usually say the child is in a *stage* while within a period, and that he undergoes a *transition* between the two periods.

However, . . . the appealing notion of coherent stages does not withstand careful scrutiny. Performance is generated by interrelated sets of components, each of whose developmental course may be distributed over widely differing time periods. Task variations evoke unknown combinations of such component processes, producing anomalous results for those seeking chronological invariance. One can interpret the observed improvements in performance from one period to the next as the result of either a revolutionary reorganization of the system or as the completion of an incremental process of relatively localized state changes. In either case, the task of developmental psychology is to clarify the nature of both the system that generates the behavior during each period and the transition process that moves the system from one state to the next."

Recently there have been both empirical and theoretical refinements in the analysis of tasks and our understanding of the systems that accomplish them. It is becoming possible to assess independently the elementary information processes that generate more global performance (Resnick, 1976; Siegler, 1976 *op. cit.*, in press *op. cit.;* Sternberg, 1977). At the same time, proposals have been formulated that attempt to account for the self-modification of the human information processing system (Anderson, 1976; Rychener & Newell, in press; Klahr & Wallace, 1976). All of these proposals are at a very rudimentary stage of development. What is most interesting about them, in our current context, is that none of them attempts to reproduce, account for, or decry stage theory. Thus they provide a *positive* alternative to the path currently obstructed by Brainerd's stop sign. I am convinced that all these theories will be found to be incorrect in fundamental ways. However, if they can add as much to our total knowledge about cognitive development as Piaget's stage theory has, they will have been worth the effort.

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Measurability, description, and explanation: the explanatory adequacy of stage model. Let me begin by noting that I agree with both the spirit and substance of Brainerd's critique. Like the author, I feel that the question of the explanatory adequacy of stage theories is one that has not received the attention it deserves. I do not, however, entirely agree with Brainerd's view of *why* stage theories lack explanatory power and would like briefly to: a) extend his argument along lines that seem to me to provide the basis for a more radical and, at the same time, more constructive critique of existing stage theories, and b) discuss the current status of Piaget's theory in the light of this formulation. My discussion will be in two parts and organized as follows: First, I will argue that Brainerd's addition of a measurability criterion to his definition of an explanatory model is somewhat gratuitous and, perhaps more importantly, unnecessarily restrictive. Second, I will suggest that his sharp dichotomy between descriptive and explanatory stage models tends to be misleading, and that description and explanation might be more constructively viewed as two distinct but related aspects of the general phenomena of theory building.

Measurability. On the issue of what constitutes an explanatory stage model, I would like to raise two points: First, I would like to note that the author's second criterion (i.e., that a theory posit antecedent variables, which I take to mean some sort of plausible explanatory variables or causal mechanism that are at least in principle capable of operational definition) seems to be a reasonable enough theoretical requirement, and in itself provides the basis for a potentially powerful critique of most current stage theories. Second, I would like to argue that the author's third requirement (i.e., that measurement procedures be specified) is the weaker of the two possible criticisms. Measurability, I would like to suggest, is a methodological rather than a theoretical problem, and the requirement that a theory posit well-defined measurement procedures confuses the logical status of theoretical terms (e.g., intelligence, reaction time, mass, velocity) and the experimental procedures whereby the terms are operationalized (e.g., I.Q. scores, milliseconds, grams, feet-per-second). While it is true that the specification of procedures for operationalizing theoretical concepts serves to make the process of theory confirmation (or refutation) simpler, there seems to be no historical or conceptual justification for making it a mandatory theoretical requirement. For example, one would not wish to deny modern physics the explanatory power of that mysterious entity, the electron; nor modern biology the explanatory power of that, until recently, equally inscrutable entity, the gene – if only out of recognition of the fact that the eventual development and refinement of precise techniques for operationalizing theoretical concepts depends, more often than not, on technological or methodological advances (e.g., the development of a higher energy accelerator or a reliable I.Q. test) rather than on theoretical advances. Indeed, such activity makes up a large part of what Kuhn (1962) would call the process of "normal" science. Thus, the requirement that a theory posit well-defined measurement procedures for an explanatory variable is unduly restrictive in the sense that such a requirement would tend to stifle creative theoretical speculation, and the criticism that a stage model lacks explanatory power because it fails to specify measurable antecedent variables seems to me to be a weak one.

A more powerful criticism of the current proliferation of stage models, I would like to suggest, is that they lack explanatory power because they fail to posit any plausible explanatory variables at all. Piaget's theory, which by most standards would be considered among the most elaborate and widely researched of the stage models, provides a case in point. The five criteria by which Piaget proposes to ascertain the existence of his stages turn out to be, on close examination, just that – criteria for determining the existence of the stages and *not* variables or mechanisms by which the stage-related behaviors can be explained. Consider, for purposes of illustration, the two most important criteria – invariant sequence and cognitive structure. If one accepts the essential veracity of the data generated by Piagetian research, then the sequential emergence of structurally unitary stages are phenomena to be explained, not explanatory variables. The notion of an invariant sequence or a cognitive structure, at least in the way Piaget seems to use the terms, is more like what a philosopher of science might call an explanandum than an explanans (see Hempel, 1965 and Flavell, this *Commentary*). In other words, the existence of an invariant developmental sequence and cognitive structures cannot be explained by Piaget's stages; they are characteristics of the stages that have to be explained. Of the five criteria proposed by Piaget, only the notion of successive stages of equilibrium appears to provide a potentially plausible mechanism by which to account for the sequential emergence of identifiable cognitive structures. And, as Brainerd points out, the research literature on the existence of successive and identifiable stages of equilibrium is, at best, equivocal. The criticism that Piaget's theory lacks explanatory power because it fails to specify well-defined measurement procedures is thus a weak one, and a potentially more powerful criticism would be that it fails to specify any plausible explanatory variables (with the possible exception of equilibrium) at all. More important, such a criticism is clearly even more applicable to most of the stage models that currently populate the developmental literature.

It would be an error on the side of excess, however, to deny completely the theoretical utility of a stage model (particularly Piaget's) simply because it fails to specify either plausible *or* measurable mechanisms to account for developmental changes. Such a criticism, it seems to me, distorts the role of description and explanation in theory construction, and I would like briefly to consider this issue next.

Description and explanation. Brainerd's emphasis on the distinction between descriptive and explanatory stage models tends, I feel, to obscure the interaction between both in the process of theory building. Scientific theories, I would like to suggest, are not as static as the author's distinction implies, and description and explanation are both integral and interrelated aspects of the scientific enterprise. This view is not without support, and I feel that it provides a more constructive perspective on the process of theory building in the sense of suggesting that theories are descriptive *and* explanatory rather than descriptive *or* explanatory. For example, in discussing the structure of scientific explanation, von Wright (1971) argues that scientific activity has two broad aspects: one is the ascertaining and discovery of facts, the other the construction of hypotheses and theories. Scientific activity is, in von Wright's terms, both "descriptive" and "theoretical." Such a dualistic view of the scientific enterprise emphasizes the interrelationship between description and explanation in theory construction. Scientific theories, according to this view, can no more be descriptive without being explanatory than they can be explanatory without being descriptive.

Harré and Secord (1973; see also Harré's commentary on Haugeland, this issue) make the dual role of description and explanation in theory construction even more explicit. Scientific knowledge, they argue, is the product of what they term "critical description." Critical description consists of two distinct but related activities: exploration and experimentation. The role of exploration is one of extending what is known from common knowledge, and the role of experimentation is that of critically checking the authenticity of what is thought to be known. Harré and Secord further point out that while critical description often takes place with certain assumptions about the nature of the causal mechanism that generates the observed phenomena to be accounted for, critical description is possible and is sometimes undertaken with no clear idea as to a causal mechanism. The process of theory construction thus involves an interaction between description and explanation, with the role of description being at least as important as that of explanation. In keeping with the above, I would like to argue that the principal theoretical value of Piaget's stage model is that he has moved in the direction of speculating on possible explanatory variables and, in the process, the model has generated a large body of descriptive data. Such data provide a strong empirical foundation for further theoretical speculation concerning the mechanisms or variables responsible for developmental changes.

In summary, while I agree with the main lines of Brainerd's argument, I do feel that a more balanced critique of the stage question can be obtained by recognition of: a) the dual role of description and explanation in theory construction, and b) the distinction between the requirement that a theory postulate plausible explanatory variables at least in principle capable of empirical definition and the requirement that a theory specify procedures for operationalizing explanatory variables. In view of the above, I would like to suggest that Piaget's theory, while currently lacking in measurable explanatory variables, at least has the virtue of suggesting plausible explanatory variables and has generated a substantial body of descriptive data. Unfortunately, such is not the case for most of the stage models that occupy the literature. There is at present little recognition of the need to postulate plausible explanatory variables, let alone measurable ones. As it now stands, most stage models have felt the need to do little more than claim discovery of a set of developmental stages.

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"Stages" in developmental psychology. These remarks are taken, with minimal editing, from an unpublished manuscript prepared for presentation in a round-table discussion at the meetings of the Eastern Psychological Association, Boston, Massachusetts, April 6, 1967. The other discussion participants were John Flavell, Bernard Kaplan, Lawrence Kohlberg, Joachim Wohlwill, and Peter Wolff. It will be apparent that my views coincide well with those of Brainerd on the hidden traps in constructs arising within the context of a cognitive-structural-stage theory. Hence my commentary has the effect, and intent, of endorsing the Brainerd critique. Perhaps my own statement embellishes Brainerd's points in the right way in a couple of places.

It seems to me that concepts of stages in developmental psychology have served an important purpose in facilitating speculation and theorizing. The postulation of stages, however, has also tended to becloud important behavioral processes and issues that otherwise would have been investigated earlier had not such "stage" concepts been introduced into our language.

There is no denying that certain kinds of developmental theories have found the postulation of stages, usually but not always related to age, profitable as organizers of data and for abbreviatory purposes. It *is* admittedly very useful for communication purposes, if not for the eventual understanding of the underlying behavior processes, to think in terms of the preverbal versus the verbal child, the nonwalker versus the walking child, the preoperational and the concrete operations child. It is also apparent, however, that while such dichotomizations and categorizations are useful for descriptive and communication ease, they tend to obfuscate the real nature of development, particularly the importance of transitional periods, which are seldom sudden. Transitions are themselves, after all, important foci of study, especially for those of us who are interested in learning processes. While it is an interesting fact that babies generally creep before they crawl, and crawl before they walk, the mere documentation of the characteristic sequencing of such behaviors with increasing age does not tell us much, if anything, about the processes involved in the achievement of walking behavior. We cannot *explain* walking behavior by asserting that the child is in "the walking stage."

Although the age-and-stage orientation to understanding child development tends to emphasize constitutional-maturational determinants, and to belittle learning antecedents, I do not believe that the depreciating of learning processes has been the only consequence of the adoption of stage concepts in development. Certain important transitional biological processes are also often slighted by the age-and-stage orientation to human development. Hormonal changes that occur in the course of human development and that give rise to descriptions of human behavior in terms such as "presexual," "adolescent," "adult," or "mature," do not typically take place in a step-wise fashion and we may be overlooking important transitional phenomena, both behavioral and physiological, by adhering to a language that ascribes jump-wise changes in such functioning. While there is descriptive and abbreviatory value to stage concepts in human development, they place limitations of conceptualization and verbal restrictions upon us.

Like it or not, stage concepts tend to force our thinking into a certain rather rigid mold with respect to the influence of cumulative experience on the development of human behavior. Stage concepts often inhibit optimism about the potentialities of humans, especially the educational potential of children. In the area of infant learning, for example, numerous references of forty or so years ago suggested that the cortical immaturity of the newborn human was such as to prevent learning. The cerebral innervation of the child under three months of age was said to preclude learning. The reasoning was usually circular and based upon nonmorphological observations, for example, a feeble attempt at conditioning was made, no learning occurred, and it was concluded that poorly developed physiological structures precluded a positive finding.

Another case in point is that of what the educators call "reading-readiness." For some time it has been quite blatantly assumed that reading-readiness, which after all is merely a statement as to whether it is possible to *teach* a given child a certain behavior at a certain time, is either present or absent in a child. Now, it is unquestionably true that most children below a certain age (e.g., one year) probably cannot be taught to read and that most children beyond a certain age (e.g., eight years) probably can be taught to read, but it does not follow that children pass a certain momentary point in the life span beyond which reading is suddenly easy, whereas earlier it was very difficult or impossible. It should be obvious to us by now that children can be taught to read earlier than we have previously thought possible. (This is not to say, by the way, that it is *good* to teach them to read at those younger age levels, for that is another empirical question.) We now know a great deal more about the techniques necessary to facilitate reading in young children than we did even a decade ago. The important point is that readiness to do anything at any given time depends not only upon age but also upon the particular methods, procedures, or techniques that we employ in facilitating the transition.

As we gain more information about the specific processes involved in going from one "stage" to another, our understanding of such processes tends, or should tend, to eliminate the concepts of stages from our thinking. Stages do have an heuristic value in the history of our understanding of human development, but when stage explanations are superceded by *process* explanations, we ourselves have finally come of age. The more we learn about development, seemingly the more stages do we require the smaller do the steps become. We will have arrived when our statements of functional relationship with respect to developmental processes have become smooth. In my opinion, stages represent gaps in our information as scientists, not gaps in the behavior of our subjects.

A more serious objection to concepts of stages is in our tendency to *reify* these stages. The empirical hazard is that we come to regard those stages as real conditions of the organism rather than as artifacts of our observational procedures and methodologies. It has been my impression, perhaps erroneous, that subscription by psychologists to conceptualizations of development in terms of stages is usually followed closely by the adoption of a structural view of the mind. The postulation of structures is usually based upon behavioral observations, to be sure, but the language quickly becomes metaphorical. The special words, initially devised merely to abbreviate complex behavioral patterns, now become taskmasters and slaves.

The argument here is against psychological typologies, particularly when the use of typological vocabularies tends to stifle further search for underlying processes and transitional attributes. Contrary to some views, the learning-process psychologist has not entirely escaped the lure of the typological lingo. We have our anxious and nonanxious children, we have our conditioned subjects and those who failed to condition, and we have a peculiar sort of stage-concept in our use of the term "criterion." Here, no less than in the stage-type psychologies, steps and stages are artifacts: You can have learning by fiat simply by requiring that your subjects display 50 percent conditioned responses on test trials rather than 80 percent, or simply by adopting a criterion of five successive correct responses in a discrimination learning task; or you can have twice as many anxious subjects by utilizing the upper twentieth-percentile scores rather than the upper tenth on an anxiety scale. The point is that the arbitrary adoption of such criteria for the designation of subject attributes or behavioral characteristics builds into the organism's behavior a step or stage that is not really there, but is there only in our thinking *about* his behavior. The organism has not *really* become a learner at the point where we have, by verbal agreement, decreed him to be a learner. Nor have his mental structures been at all affected by our verbal agreements! Some psychological theorists seem more aware of and self-conscious about the fictional and heuristic character of their hypothetical constructs than others.

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Problems of logic and evidence. One of the central points of Brainerd's discussion is that if one is to use the stage concept as an explanation of children's behavior, then it is mandatory that evidence for the existence of the stage be independent of the behavior it is to explain. Piaget (1960 *op. cit.*) proposed several indirect criteria for establishing the existence of the stages. According to Brainerd, three of these proposed criteria are subject to the same criticism: the invariant sequence of behavior needed to fulfill the criteria could result from "measurement sequences." One point I will make below is that invariant sequences can be mistakenly identified as measurement sequences. Measurement sequences might not be as pervasive in Piagetian theory as Brainerd suggests. However, if these indirect criteria cannot be used to establish the explanatory value of stages, one could reexamine the possibility of discovering direct evidence. The hypothesis that children fail conservation tasks because they are in the preoperational stage is used to illustrate the types of problems that one might encounter in searching for more direct evidence.

Measurement sequences: logic or theory? Brainerd uses "measurement sequences" to refer to the case where solution of one task logically presupposes solution of another task. He argues that it is inappropriate for the order of acquisition of such tasks to count as evidence for stage theory. This much is beyond dispute. However, care must be taken in deciding whether or not two tasks form a measurement sequence. We devise theories (implicit or explicit) to explain obvious, well-known facts. It may be the familiarity of the facts together with the assumptions of our theories, rather than logic, that we use to argue that one task presupposes another. From this perspective, empirical facts can be misjudged to be logical truths. To illustrate that this risk is more than hypothetical, I will briefly discuss two of Brainerd's purported examples of measurement sequences.

Brainerd suggests that the order of acquisition of conservation of substance and conservation of weight comprise a measurement sequence. Contrary to Brainerd's claim, it is logically possible to conserve weight before substance. Imagine that children are first able to conserve quantities for which there exists some objective measure of the quantity independent of beliefs about conservation. These early conservations could provide a basis from which the conservation of related quantities would be derived. On this theory, conservation of liquid and mass would be acquired relatively late, since measures of these quantities (e.g., use of a measuring cup) typically presuppose conservation. The theory explains why number is conserved very early. At first, children may believe that the number of objects in a row varies with its length. However, counting provides an independent assessment of numerical quantity that enables children to correct their initial beliefs. Weight should also be conserved early since the balance scale provides the needed independent measure. Erroneous beliefs about weight can be checked against the scale. Conservation of substance could then be derived from conservation of weight. Since conservation of substance precedes conservation of weight, I am forced to admit that this theory is wrong. Brainerd's formulation may be closer to the truth, but I believe it is closer to empirical than to logical truth.

To take another example, Brainerd claims that two-way classification logically presupposes one-way classification. In order to take account of two dimensions simultaneously, it must be possible to take account of each of them separately. Is this a logical truth? Suppose we could find two dimensions, each of which enhances perception of the other. Together, these mutually facilitory dimensions create a sequence more salient to the child than is either alone. Given such a perceptual situation, would it not be possible that filling a cell in a double classification matrix would be simpler than (developmentally prior to) filling a cell in a single classification matrix? If so, then this is not a measurement sequence.

These criticisms of Brainerd's examples are not meant to argue against his claim that if something is a measurement sequence then it cannot be taken as evidence for stage theory. They are meant to suggest that we should exercise caution in dismissing Piagetian evidence as being vacuous or tautological. It may only be our implicit assumptions and theories about how tasks are solved, rather than logic, that generate the presuppositions.

Operational stage as an explanation for conservation: the problem of obtaining direct evidence. Why is it difficult to obtain direct evidence for the explanatory role of stages? I believe that there are aspects of Piagetian theory that seriously constrain the possibility of direct empirical test. To illustrate the problems, I will consider the theory that children fail conservation because they are in the preoperational stage. This theory appears to generate the empirically testable hypothesis that in the absence of each of the relevant operations, conservation should not be possible. For example, Piaget (1952, p. 12) claimed that a child who cannot compensate, who cannot "reckon simultaneously with the height and cross section of the liquids he has to compare" cannot conserve. I will discuss below why such hypotheses are difficult to test. However, let me first mention that if conservation involved concrete operations by *definition* rather than by hypothesis the theory would be vacuous. Unfortunately, Piaget sometimes writes as though this were the case. For example (1967, p. 533): "Pseudoconservation is easy to spot . . . when a child says that there will be the same quantity to drink, one should ask him to pour the same amount of liquid into two different empty glasses, one low and wide, and one tall and narrow; in the case of pseudoconservation the child will pour liquid into both glasses *up to the same level* . . . which shows clearly that his (correct) answer was not based on conservation of quantity."

What was originally an empirically testable hypothesis that compensation is necessary for conservation has been transformed into a circular argument. "Real" conservation consists of old (now "pseudo") conservation plus compensation (i.e., the pouring task). Of course, any child who can pass a "real" conservation task can pass a compensation task, but this has become true by definition. It has lost its empirical import and no longer qualifies as evidence. Here, all of Brainerd's warnings about measurement sequences are right to the point.

Perhaps we should treat this response of Piaget's as a lapse and assume the relationship between concrete operations, for example, compensation, and conservation, to be empirically testable. To test these hypotheses, measures of operations are needed. Unfortunately, any measure of an individual operation can be criticized: "a single operation is not an operation at all but only a simple intuitive representation" (Piaget, 1966, p. 34). In order to devise paradigms that are immune from this criticism, the minimum requirement would seem to be that two operations are necessary to solve the tasks. Assuming this can eventually be accomplished, the next step would be to discover the relationship between these measures of operations and conservation.

Suppose that a newly developed test of operations reveals that conservation can be acquired before (without) operations. Ordinarily this would be construed as evidence against the theory. However, it is here that the problem of horizontal décalages intrudes. Conservation of various quantities, for example, mass, liquid, weight all require the identical operations for their solution yet they are by no means acquired simultaneously. Piaget does not take these décalages as evidence against the theory but rather as characteristic of the stage. It is called "concrete" in part because of such variations in performance. So, in the hypothetical case of conservation without operations, what is to prevent an investigator from invoking "horizontal décalages" to explain discrepancies in performance? When is it legitimate to invoke horizontal décalage as opposed to acknowledging falsifying evidence? At present, there are no clear criteria for differentiating negative evidence from décalages. Without such criteria, the prospect of discovering direct tests of the theory are no greater than those of using the indirect methods that Brainerd critiques.

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Improper questions cannot be properly answered. Brainerd questions the value of a concept of stages of development as used, for example, by Piaget, on the grounds that it does not explain behavior and may even prove to be empty tautology. His analysis, in my view, reflects a misunderstanding of the intent of the concept and of the approach to theory construction from which it derives. Part of his difficulty with the stage concept stems from an unduly restrictive interpretation of what constitutes explanation (which, in turn, stems from a different approach to theory construction), and part stems from his interpretation of the nature of invariant sequences. Although these two issues are not altogether independent, they will be separated for purposes of discussion.

The nature of explanation. What Brainerd requires of an adequate explanation is a spelling out of antecedent variables, preferably in the form of a functional relation, to be expressed mathematically wherever possible. His criterion derives from a view of the process of theory construction developed for the physical sciences (e.g., Bridgman, 1927) and widely espoused by American psychologists. Its appropriateness for psychology may be questioned on general grounds as well as on the basis of failures of existing exemplars (e.g., Hull, 1952; Hull et al., 1940). Physicists can provide quantitative functions relating a dependent variable to independent variables and fixed parameters in virtue of the nature of physical phenomena: these phenomena are clearly identified in terms of measurement conditions and effective causes. Behavioral phenomena – at least at the current state of knowledge – are not that orderly. Behavior is characterized by meaning, purpose, and organization. A major problem confronting psychological theorists attempting to capture the rich variation of manifestations of behavior possessing these properties has been to establish an adequate unit of analysis. Psychological theorists who start from a narrowly, but operationally, defined unit of analysis, such as a motor response or a specific verbal utterance, have then been faced with the problem of accounting for intent or meaning, a problem typically solved through the device of tacked-on principles, processes, or mechanisms having the status of intervening variables. Perhaps the major contribution of genetic epistemology (and of other theories employing a functionally defined unit of analysis, e.g., programs, schemas, TOTEs, etc.) is to start from the defining properties of purpose, meaning, and organization in constructing the basic unit of analysis. This approach reflects a biological, rather than a physical, model of theory construction. It proceeds inductively from extensive field observations of the organism in its natural activities. The generalizations derived from these observations are not usually couched in theory-neutral data language. Rather, they reflect not only a descriptive summary of modal behavior in its context of occurrence but also an informed best guess as to the appropriate unit of analysis for the behavior as well as to the nature of its organization and determinants. This is the approach taken by Piaget, it is an approach that Brainerd, working from a different orientation, fails to appreciate ("Piaget theory presents . . . an interesting example of stages that fall somewhere between pure description and true explanation").

Invariant sequences and structure. Brainerd objects to the use of an invariant sequence of stages as a focus of investigation on grounds that it "smuggles stages into existence before any data are gathered." As I have tried to show above, his objection derives from his orientation to the enterprise of constructing behavioral theory. The assertion that development can best be characterized in terms of a succession of stages, differing qualitatively in the structure of their organization, is in itself a major theoretical evaluation of the defining properties of observational data (i.e., the basis for selection of a unit of analysis). How well the resulting theory succeeds in accounting for the organization of units so identified is an independent question, which Brainerd shoud be raising. There is no inherent circularity in definition once one goes beyond classification of similar specific behavioral instances (e.g., kicking, letting go, the schema of the shakes, and other manifestations of the third substage of the sensorimotor stage) to specify a classificatory principle (secondary circular reaction) and to differentiate it from later classificatory principles (intent to conserve consequences rather than to provoke them) in terms of manifestations and/or enstating conditions (stereotypy of behavior in contrast with variation of intensity or of component movements characteristic of tertiary circular reactions).

Brainerd, if I read him correctly, accepts only two "explanations" of invariant sequences: maturational determination, or measurement artifact. This, again, is a distorting oversimplification. The fact that biological maturation is clearly a contributing factor in behavioral maturation does not mean that (a) it is the only factor, or (b) that its effects are not also subject to moderation by environmental influences (as demonstrated, e.g., by White's (1967) work on the development of grasping). With respect to what Brainerd calls "measurement sequences," while there is an inherent structure in many complex skills such that attainment of higher levels presupposes prior mastery of lower-level component skills, that fact does not ipso facto render trivial the problem of accounting for attainment of the complex skill (as witness the epidemic of recent research on reading). Nor does the fact that development of some behaviors can be described adequately in terms of a quantitative accumulation of a graded series of components necessarily imply that all higher behaviors may be so described. There is abundant evidence of qualitative change in the organization of behavior contingent upon the attainment of a higher level of operation even for such relatively mechanical skills as telegraphy (Bryan & Harter, 1899) or typing (Book, 1908), or for a variety of forms of learning such as Pavlovian conditioning (the shift from first to second signal system, see Razran, 1971), discrimination learning (Osler & Kofsky, 1966, Gholson, Levine, & Phillips, 1972), reversal shift (Kendler & Kendler, 1962), or learning set (Levinson & Reese, 1967) to mention but a few of many possible examples.

While one may question the need for, or the utility of, characterizing each successive stage in precisely the terms Piaget and his associates have chosen to use, it is hard to deny that they have made a compelling case for the existence of levels of cognitive behavior differing qualitatively in their principles of organization. Certainly I know of no other theory of cognitive development that can encompass so richly varied an array of evidence with a single set of explanatory principles.

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Structural and developmental explanations: stages in theoretical development. What role do or should stages play in developmental theory? Is the stage construct more misleading than useful? In his article, Brainerd argues that, as set forth by Piaget, it is. Inasmuch as Piaget has put more substance into the stage construct than any other developmental psychologist, the argument, if valid, would appear to rule out any psychological use of the notion. It is important, therefore, to consider the soundness of Brainerd's case. Space limitations do not permit a point-by-point analysis; I will concentrate, instead, on what seems to me to be a central misunderstanding of the issue.

Brainerd's argument founders at the outset on his confusion of an explanation of behaviors specific to a given stage and an explanation of stage change. Consider his claim that to be explanatory "they [stages] must posit antecedent variables believed to be responsible for such changes that weld the stages into distinctive entities." This statement contains a number of unwarranted assumptions: (1) that stages must somehow contain their own antecedent conditions; (2) that these antecedent variables are responsible for stage change; (3) that it is the changes that weld stages into distinctive entities. Yet in his ensuing discussion, Brainerd switches from an emphasis on stage change to stage-related behaviors as the phenomena to be explained. (And in the end he concentrates almost completely on whether or not stages exist in a Piagetian sense.)

For example, he finds Freud's oral stage theory lacking only because there are no methods whereby oral behaviors and psychodynamic processes might be independently measured. The implication is that if we could independently determine whether a child was in the oral stage, and thereby predict thumbsucking and other oral behaviors, then that particular stage construct would be acceptably explanatory. So far, so good. A similar standard might be expected to apply to Piaget's stages. That is, if Piaget's model of cognitive structure can be independently assessed for a given stage and then used to predict other stage-determined behaviors, this should meet the standard of explanation implied. Space does not permit discussion of the success with which the Genevans have carried out this program; they have generally concentrated more on description than on testing the model. Lack of testing, however, does not make the theory any less potentially explanatory than any other empirically testable model (e.g., the Basic Stage posited by Bijou and Baer, which Brainerd seems to find so powerful, despite its manifest lack of any empirical test). This standard of explanation seems eminently defensible. Clearly, a stage in the sense of a pattern of behaviors might exist, and an explanatory model might show how and why those behaviors cohere and how they differ from earlier or later stages, without showing how they came to differ from those of earlier stages.

However, Brainerd quickly abandons this position and reverts to the demand for explanation in terms of changes in stage, that is, for a developmental explanation. Brainerd's consideration of what constitutes an explanation thus confuses levels of analysis. Piaget does offer an explanation of stage-related behaviors in terms of underlying cognitive structures; he does not, however, offer a satisfactory, that is, testable, developmental explanation. Piaget is in the position of the cognitive psychologist who offers an explanation of human adult information processing in terms of constructs such as levels of processing or structures of semantic memory without considering how those structures developed. This level of explanation of behavior obviously cuts deeper than pure description but falls short of explaining development. Theories of cognitive development are necessarily more complex than theories of adult cognition because they must account for shifts in structural descriptions, in addition to positing a series of such descriptions (Kessen, 1962 *op. cit.*). The ideal form of developmental theory-building utilizing stage constructs might proceed from a description of age-related behaviors to a stage proposal, to a series of structural principles proposed to underlie the stages, to the testing of these principles, to a proposed mechanism of change determining movement from one set of principles to the next, and finally to the testing of this mechanism. Indeed, with the exception of the last step, this describes Piaget's research program fairly well. His proposed mechanism of developmental change (the accommodation-assimilation model) has never been sufficiently well-specified to be subjected to empirical testing, except in the most trivial sense.

It is possible, although not so convenient, to describe and theorize about development without invoking stages. It may indeed be easier to get to the developmental crux of the matter – explaining change – without them. This is, however, a question of theoretical preference and success rather than an a priori criterion of adequacy.

There are to date few testable proposals about developmental change. Brainerd would like to limit the consideration of acceptable possibilities to maturation and environmental influence. It is here that he seems most clearly to misunderstand Piaget's proposals. Heuristically, Piaget's most significant contribution lies in his proposed model of organism-environment interaction, a biologically based model fitting his conception of intelligence as a biological function. Within this conception of developmental change as a function of the organism-environment system, it is not possible to consider organism and environment as separable components at any given time. Rather, organismic complexity is derived from adaptation to environmental encounters, which in turn leads to qualitatively different encounters with the objectively unchanged environment. (Contemporary systems theory would also take into account the effect of the organism on the environment.) That is to say, change can never be considered as a function of one or the other but only of the two as an interacting system. Indeed, far from being a secret maturationist, as Brainerd appears to believe, Piaget systematically neglects the role of biological maturation in considering development, either within or between stages, a neglect that is especially notable for the sensorimotor period. In fact, if one were to posit an additional contribution of biological maturation to the organization produced by assimilation and accommodation, the Piagetian account would no doubt be significantly more successful.

Clearly, the stage construct continues to be overused and abused by developmentalists; clearly Piaget's theory remains untested and no doubt wrong in many ways; clearly, his account of developmental change is unspecific and inadequate. Nonetheless, the stage notion as invoked by Piaget cannot be declared illegitimate on the grounds of explanatory inadequacy, as Brainerd has attempted to do here. It is, rather, one legitimate way of approaching developmental explanation. Its usefulness is apparent in its having revealed more about developmental change and its complexities than any previous approach. It has indeed revealed much too much to permit our ever returning to the simple form of the antecedent-consequent paradigm that Brainerd wishes to reimpose upon us, however wrong the specifics of the Piagetian theory itself may turn out to be.

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A structuralist view of explanation: a critique of Brainerd. In his critique, Brainerd argues that Piaget has constructed a descriptive theory but failed to construct an explanatory theory of intellectual development. Specifically, he focuses upon a few of the considerations that may be relevant to transitions between stages and tackles the problem of translating Piaget's structuralist descriptions into a set of independent variables manipulable by a behavioristically oriented psychologist. Not surprisingly, the enterprise fails, and, not surprisingly, Brainerd attributes that failure to Piaget. I, too, regard it as a failure, but I attribute it to Brainerd, not because he makes a poor translation but because he naively believes the translation is possible, even urgent, in cognitive psychology. I shall argue that Brainerd espouses a view of scientific explanation that the most productive branches of the human sciences – linguistics, criticism, anthropology and cognitive psychology – have abandoned. Hence, Brainerd is, I suggest, calling for a retreat rather than an advance. My brief and somewhat scattered comments are addressed to the differences between the models of explanation adopted by Piaget (and other structuralists) and that adopted by Brainerd (and other neobehaviorists).

Brainerd adopts, without criticism, both the classical distinction between a description and an explanation and the view advanced by Hempel (1965; see also Haugeland, this issue) that all explanations, whether for the natural or the human sciences, consist of causal relations between antecedent and consequent events – between independent and dependent variables. (One could adopt a more liberal view – an explanation is a description of an event in terms of the structure of the mechanism or system that produces it. To illustrate, an explanation of a phoneme is given not by specifying a series of independent variables with a causal link to the phoneme but by characterizing that phoneme in terms of the structure of the phonological system that generates it.)

Having adopted a narrow conception of explanation, namely as stating causal relations between independently specified independent and dependent variables, Brainerd goes on to review Piaget's account of transitions between developmental stages and concludes that Piaget's is not an explanatory theory. That is, Piaget does not offer a set of antecedent or independent variables that, if manipulated, would yield the consequent, dependent variable, namely, the new stage.

In fact, there is little anywhere in the Piagetian descriptions of stages specifying the independent variables or their causal relations to dependent variables. The notion of a schema, for example, specifies both conditions for activation and forms of behavior that, among other things, alter the stimulus. With Waddington (1957), Piaget views "the relations between the organism and its environment as a cybernetic loop such that the organism selects its environment while being conditioned by it" (Piaget, 1970b, p. 50). Hence it is not just stage transitions but the descriptions of stages altogether that would fall under Brainerd's judicial gavel.

That form of criticism is not new to Piagetian theory. American psychologists raised on an empiricist-positivist view of science – scientific explanations consist of empirically demonstrated causal relations between independent and dependent variables – and a behavioristic view of man – behavior should be described in terms of associated stimulus and response events – have simply regarded Piaget as nonscientific. The first step in this program was to ignore Piaget, as was done for thirty years, while the second step was to reformulate Piaget in terms appropriate to a Behavioristic epistemology, a step begun by Berlyne (1965) and by Gagné (1962) in his descriptions of the acquisition of knowledge. In both of these earlier cases, and in the present case, an attempt was made to recast the types of descriptions that Piaget had given for the associative antecedent-consequent descriptions favored by the Behaviorists. Piaget deserved better. Piaget was among the first to begin the task of describing (explaining) intelligent behavior in terms of an organized functional system or structure and the first to succeed in specifying some aspects of the nature of that structure.

The structuralist program is well advanced on several fronts in the human sciences. Frye (1957) set out to construct "a coherent and comprehensive theory of literature, logically and scientifically organized, some of which the student unconsciously learns as he goes on, but the main principles of which are as yet unknown to us" (p. 11). Again, "we have to adopt the hypothesis, then, that just as there is an order of nature behind the natural sciences, so literature is not a piled aggregate of "works," but an order of words" (p. 17). Frye's structuralist theory of literary criticism is a clear and productive alternative to those earlier *causal* theories that would explain a poem by reference to the poet's upbringing or his disappointments in love.

In anthropology, Levi-Strauss (1968), Geertz (1973), Douglas (1973), and Leach (1973), among others, have begun to describe cultures in structuralist terms: "As interworked systems of construable signs (or symbols), culture is not a power, something to which social events, behaviors, institutions, or processes can be causally attributed; it is a context, something within which they can be intelligbly – that is, thickly – described" (Geertz, 1973, p. 14). Or "Structuralist social anthropologists start off with the hypotheses that these codes are 'languages' in the same sense as spoken languages, and hence they postulate that the kind of linkage between nature and culture that has lately been emerging from the work of structural linguistics is highly relevant for social anthropology" (Leach, 1973, p. 39).

The origin of all of these forms of structural analyses arose first within linguistics. Structuralism had its formal beginnings in the Thèses presented by the Prague Linguistic Circle to the first International Congress of Slavic Philologists held in Prague in 1929. Robey (1973, p. 1) describes these Thèses as follows:

"A radically innovatory programme for the theory and methodology of linguistic study, the Thèses introduced the notion of *Structure* as the key term in a polemic against the traditional methods of the discipline, overconcerned, in their authors' view, with problems of linguistic origin, and limited to the analysis of isolated facts. Under the influence of Saussure and the Russian linguist Baudouin de Courtenay, the authors of the *Thèses* proposed language as a functional system, to be understood in the light of its *aim* (that of communication). *Structure,* in the *Thèses*, is the structure of the system, the manner in which the individual elements of a particular language are arranged for this purpose in relations of mutual dependence. Since this differs from one language to another, it follows that the separate components of a system can only be understood in the light of the system as a whole, and therefore that the primary object of linguistic study must be the structure of the system itself rather than the individual linguistic fact."

This problem was applied most successfully to phonology and syntax by such writers as Trubetzkoy (1933), Jakobson (1972), and Chomsky (1965), but has also been applied with some success to other aspects of language ranging from the lexicon (Bierwisch, 1970; Miller & Johnson-Laird, 1976) to the structure of speech acts (Searle, 1969), to poetics (Culler, 1975).

It is in this theoretical context that one should consider the stages of intelluctual development advanced by Piaget. Structuralism, for Piaget (1968) is a mode of inquiry, and the theory construction is characterized by the idea of wholeness (the whole gives meaning to the parts) transformations (adding is the inverse of subtracting), and self-regulation (an autonomous system), which has been applied to mathematics, physics, biology, psychology, linguistics, and anthropology. A piece of intelligent behavior, like a piece of a sentence, is not to be seen, interpreted, or explained in terms of a response to a set of independently specified antecedent variables to which it is causally linked, but rather in terms of a general cognitive structure, the ensemble of elements and relations, of which it is a particular examplar. To illustrate, at one point in his life, a child fails to see that active sentences are logically equivalent to passive ones, that "not more" is logically equivalent to "less," that an ascending series, or staircase, is simultaneously a descending one, and so on, because his cognitive structures lack the property of reversibility. I have noted (1970) that if a child is asked to construct an "X" pattern with checkers, he is unwilling to allow the center checker to serve simultaneously as a part of both the left-oblique and the right-oblique. Similarly, Shotwell (personal communication) has recently observed that if children are asked to build ascending and descending staircases, they are unwilling to allow the top block in the ascending series to serve also as the top block of the descending series; rather they have two top blocks side by side, one for the top of each of the ascending and descending series. Bamberger (personal communication) has similarly found that young children are unwilling to let one bell serve for two notes in producing a tune like "Ba Ba Black Sheep"–they insist upon one bell for each of the first two notes. And so on. In a Piagetian scheme, all these forms of intelligent behavior would be accounted for by the same underlying cognitive structure. Indeed, Piaget's theory is one of the few in which they would even be regarded as comparable. That is, of course, not to say that Piaget has adequately characterized that structure, but rather that he has defined the problem appropriately and productively.

More critical to the argument, such an abstract description of structure would serve at least as part of an explanation of those observed events. The central criticism of Piaget and the attempt by Brainerd to transform Piaget's into an explanatory theory therefore collapses. The major point of Piagetian structuralism is to move away from the conceptual blinders imposed by seeking explanations only in terms of causal relations between antecedent or stimulus variables and consequent, response variables. A stimulus variable cannot be defined independently of the way in which it is interpreted by the organism, that is, independent of the schema to which it is assimilated, and the response cannot be interpreted independently of the intention or goal in the service of which it is organized [cf. Bindra, BBS I:1]. That whole system of antecedent independent variables and consequent dependent variables as a model of explanation is simply abandoned in favor of organized structural systems.

Cognitive psychologists, particularly those associated with cognitive science and psycholinguistics, have known this, at least implicitly, for a long time. For many of them the major enterprise has been to try to characterize adequately and accurately the functional systems by means of which we play chess, do mental arithmetic, match sentences to situations, draw inferences, understand stories, and the like [see Pylyshyn, BBS I:1]. No particular variable causes any particular response in the system. Rather, by means of experimental techniques, different aspects of the process making up a particular structure become visible. The assumption is that enough carefully sampled observations may be sufficient to construct and validate a comprehensive model of that set of processes, whether of how conversations are managed or of how short-term memory is managed. The elaboration of the elements and their transformation rules, organized into a coherent system, would serve as an explanation of the whole range of performances generated or managed by that system.

Others have made a similar point. Chomsky (1975) has argued that any perception or action on the social and/or physical environment by a human is "structure dependent." The significance of any stimulus, whether word, gesture, or artifact, is determined by its place in the structure of which it is part. The paradigm case of this "structure dependence" is that of a phoneme, a sound difference that signals a meaning difference. But a sound may be phonemic in one language and not in another. Thus the difference between "r" and "l" is phonemic in English but not in Chinese; and these therefore constitute two distinctive stimuli in the former and only one in the latter. Bruner (1977) adds "The position of a piece on a chessboard, the function of a word in a sentence, a particular facial expression, the color or placement of a light, then cannot be interpreted without reference to the person's internalized rules of chess or language, the conventions he holds concerning human interaction, the traffic rules in force in his mind" (p. 4). The "independent" variables are not independent, but depend upon the structure of which they are a part.

There are several grounds on which one could disagree with Brainerd. I have emphasized his overreliance upon explanation in terms of antecedent-consequent or stimulus-response relations (although these may successfully characterize some relatively uninteresting aspects of human behavior such as reflexes and classical conditioning). But the line could have been drawn, and perhaps should be drawn, at some other levels. Some writers, such as Dilthey (1961), Habermas (1971), and Taylor (1971), could insist upon a distinction between the physical and the human sciences. Some would argue that Brainerd restricts himself to a form of theory tied to prediction and control rather than to understanding. Hempel, in his discussion (1965) of explanation as the relation between antecedent and consequent, pointed out that once such relations are established, knowledge of the antecedent can be used to *predict* the consequent; furthermore, the manipulation of the antecedent permits the control of the consequent. Hempel was optimistic that this was no less true for history than for the physical sciences, a view for which there is currently no optimism whatsoever. I suggest that Brainerd fails because he tries to assimilate Piaget to a more or less obsolete Baconian-Hempelian view of science as the search for antecedent, causal variables, that, once isolated, may be exploited as a means of control. Instead, he could have learned from Piaget that a more productive goal for cognitive psychology is the construction of abstract and formal descriptions or models in terms of which any particular perception or action could be interpreted.

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Species-specific acquisition vs. universal sequence of acquisition. Brainerd argues that Piaget's invariant sequence, integration, and consolidation criteria all take their apparent relevance from the unstated assumption that invariant sequences that cannot be altered by environmental factors must be hereditarily controlled. He argues, however, that Piaget's invariant sequences of development are in many cases simply "measurement sequences" whose items appear in a particular order because of the dependence of each new item on the achievement of the preceding one. Although a universal sequence of acquisition of behaviors does not imply maturation of new abilities, universal acquisition of behaviors (and universal age norms of acquisition) does suggest such maturation, and hence an explanatory stage model according to Brainerd's criterion. In focusing exclusively on sequence of acquisition, the author fails to discuss the critical issue of the evidence for universal acquisitions and age norms. Cross-cultural data on Piagetian developmental stages are inadequate and inconsistent; however, a recent unpublished study by De Avila and Pulos of 10,000 North American children from a range of cultures, using native speakers and familar materials in testing, revealed universal acquisitions, age norms, and sequences of acquisition up through formal operations (Pulos, personal communication). Similar results were obtained in Tanzania by Nyiti (1976).

These cross-cultural data, in conjunction with comparative data on Piagetian developmental sequences in monkey, ape, and human infants (Parker, 1977a, b; Chevalier-Skolnikoff, 1977), reveal species-specific levels of behavioral acquisition and suggest species-specific maturational bases for Piagetian stages. If behavioral acquisitions were simply measurement sequences, they would presumably occur in children of all ages, and in individuals of all species, given proper training. Obviously they do not.

Despite his comments on the importance of proposing and independently measuring antecedent variables, specifically neurological developmental variables, Brainerd does not discuss possible neurological correlates of Piagetian developmental sequences and acquisitions. Although very little work has been done in this area, it is obviously critical to settling the issue of the explanatory power of Piaget's model. Gibson (1977) has correlated species-specific stages of neurological development in human and macaque infants with behavioral sequences in the sensorimotor period, providing evidence for maturational bases for this development period. Monier (1960) has noted correspondences between EEG developmental stages and Piaget and Inhelder's periods of cognitive development. These EEG stages correspond to the M (maturation) levels proposed by Pascual-Leone (Ammon, 1977), suggesting maturational bases for subsequent Piagetian periods and subperiods of cognitive development.

Brainerd argues that Piaget's criterion of equilibration (stages corresponding to periods of equilibrium with development occuring through sequences of disequilibrium and establishment of new levels of equilibrium) implies the sudden appearance of new behavioral acquisitions. He believes that extensive data suggesting a pattern of gradual appearance of new behavioral acquisitions fail to support Piaget's equilibration model. The extensive data he invokes are inadequate to support his conclusion, however, because they are primarily cross-sectional data that by their very nature obscure the rate of individual development by averaging the rate of all subjects. Hence, this important empirical issue remains unresolved. Moreover, it is not clear that an equilibration model would necessarily imply sudden acquisitions, since developmental rate would depend on the number of neurological factors involved, their interrelations, and their own respective developmental rates.

The strongest claim Brainerd can make is that Piaget's five criteria are not obviously adequate to support the claim that his model is explanatory. The question needs to be addressed in terms of culturally universal acquisitions and stages of acquisition, species-specific patterns of acquisitions, and neurological correlates of acquisitions, as well as sequences of acquisition. Piaget himself espouses an epigenetic or constructionist view that he does not believe is consistent with hereditary programing, and that he believes involves only a component of maturation (1973, 1971).

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Piaget's two main stage criteria: a selective reply to Brainerd. "Piaget's stages fall somewhat between the poles of true explanation and pure description." I basically agree with this conclusion of Brainerd's; yet, after reading his empiricist critique, I realize that we understand that sentence quite differently. While Brainerd denies that Piaget's research program is sensible, I believe that it is only in need of improvement in a liberalized or neo-Piagetian direction. Since lack of space prohibits more detailed comments, I will illustrate my disagreement with Brainerd by discussing his treatment of Piaget's two main stage principles: invariant sequences and general structures.

Genetic-epistemological sequences and psychological sequences. A tacit but common method of Piaget's structuralism is to search for *genetic-epistemological sequences* of typical performances. These sequences are such that not only can the performances involved be ordered in terms of their empirically-obtained developmental traces across age-group samples (call this empirical ordering of performances a *psychogenetic sequence*), but at the same time the theoretical psycho-Logical models of these performances can be ordered on the basis of the following rule: the generative process of performances placed earlier in the theoretical sequence must be included in the generative process of later performances (call this theoretical ordering of performances a *psycho-Logical sequence*). These Piagetian genetic-epistemological sequences are called by Brainerd "measurement sequences"; yet their distinctive note is not the measurable reliability of their corresponding psychogenetic sequence, but rather the task-theoretic models which make possible the conjunct psycho-Logical sequence.

The genetic-epistemological method being as described above, it is clear that Brainerd's "measurement sequences" are actually congruent pairings of theoretical psycho-Logical sequences. This is confirmed not only by his definition of measurement sequences but also by his statement that these sequences reflect the construct validity (i.e., the theoretical foundation) of tests. Performances related in this manner are often expected (in virtue of the differentiation/integration principle of rationalist developmental psychology) to appear developmentally in this sequence, with the proviso that they not appear at the same age *and* that the psychological models prescribing the sequence be organismically valid. Surprisingly, Brainerd's analysis ignores the theoretical character of these measurement sequences as well as the possibility that these performances appear simultaneously (whether or not they do so could depend on the cognitive resources of the organism relative to the cognitive difficulty of performances involved). As a result, he repeatedly writes as if measurement sequences were metaphysically "logical" and thus compelling to Nature in some Leibnizian God-given sense, that is, unfalsifiable by experience (e.g., "the sequence property does not require explanation in terms of antecedent variables and no research is needed to verify its universality" or "confirmatory data may be guaranteed so long as we administer valid tests"). With the same God-given "logic" it could be asserted that Newton's falling apple did not require an explanation. In fact, Piaget's approach is a sophisticated extension and application of the rationalist differentiation/integration principle. He starts with the assumption (equally adopted by modern information-processing approaches) that the organism is a psycho-Logical machine of some sort. As such, its performance and patterns of growth (development, learning) could, in principle, be predicted by theoretical task analyses based on a suitable psycho-Logic. Piaget's research program attempts to infer this organismic psycho-Logic by means of a two-way psychogenetic method: (1) Use unlearned psychogenetic sequences of tasks, obtained from many different content areas, infer a psycho-Logic that generates sequences identical to observed psychogenetic sequences. (2) Verify the thus obtained psycho-Logic by constructing new tasks and predicting their psychogenetic sequence. By "unlearned" I mean a psychogenetic sequence that is cross-sectionally reliable, using concept-attainment or problem-solving tasks avoiding intratask or prior learning of the solution for any of the tasks. The measurement sequences thus obtained were often considered unsound fifteen years ago. Even those tasks whose sequencing can be regarded as commonsensical (e.g., counting versus addition versus multiplication or "free recall uncategorized" versus "free recall categorized") could not, without experience or without Piaget's theory, be assigned the average chronological age in which they can in principle be handled (i.e., by cognitively-superior children of a given subculture) without external facilitation (Pascual-Leone, 1976c). In this respect, Piaget has weakened his case by not asserting, as he should, that his theory predicts much more than a psychogenetic order; it predicts, for a given cultural environment and type of subject, a constant average passing age (if any) for any given task within the scope of the theory. (The prediction of order, however, could suffice for testing a stage theory.) Brainerd's impression that Piaget's measurement sequences cannot be empirically contradicted, shows to what extent Piaget's operational structuralism has become part and parcel of psychologists' common sense.

Piaget's models and his method of task analysis can be challenged (Beilin, 1977; Pascual-Leone, 1976a, b, c, 1977); but to conclude as some readers of Brainerd can, that Piaget's research program is unsound or scientifically trivial, is to throw out the baby with the bath water.

The principle of general state structures. Brainerd's sophisticated but mistaken theoretical view of Piaget's method is also illustrated by his remarks about general structures. He rightly points out that "structures are at most abstractions from behaviour" but does not consider that they are constructive abstractions, that is, invented theoretical descriptions that define, in a content-free manner, types of task. The types are such that they turn out to be psychogenetically ordered *and* such that; for a given cultural environment and type of subject, every type of task is mastered at a given constant average chronological age (call this characteristic "psychogenetic anchoring").

Brainerd's first objection against structures as stage criteria says that task descriptions may not be relevantly related to behaviour. But in Piaget's psycho-Logical structures, the relevance to behaviour is ensured by their being constructive abstractions derived from Piaget's two-way psychogenetic method *and* by their being psychogenetically anchored. Brainerd's second objection, that behavioural descriptions (i.e., psycho-Logical structures) do not explain how the behaviours in question originate, is unfair, given the historical relativity of "descriptions" and "explanations"; for what was explanation for a given good theory often becomes description for the theory superseding it. The point to be retained, however, is that, now in the seventies, Piaget's pioneering psycho-Logic, although still heuristically useful, is no longer sufficient, a fact that Geneva now recognizes (Inhelder, Sinclair, & Bovet, 1974; Piaget, 1975).

Brainerd's third objection to psycho-Logical structures as stage criteria, is based on a misunderstanding of Piaget's stage principles. Since in Piaget's theory the integration principle ensures that higher (i.e., later-stage) structures contain the possibilities of lower ones, it is not correct to say, as Brainerd does, that higher structures should not be applicable to tasks solved in lower (i.e., earlier-age) stages. Rather, what Piaget's theory implies is that: (1) tasks solved exclusively in higher stages do not have solution strategies which, under ordinary testing conditions, require only lower structures. (2) Tasks which can be solved in lower stages must offer a simplistic solution strategy which, under ordinary conditions, requires only lower structures. All the Piagetian tasks I know (including propositional connectors) conform to these prescriptions.

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Agnostic gauges and Genevan stages. Brainerd has vigorously indicted the explanatory value of "stage" constructs in cognitive development and he makes some telling points. Because of the great influence of Piaget's writings, *stage* has become a highly salient category of thought; it is a fashionable and often used term, but it may obscure more than inform. In its descriptive sense, a stage implies that a child now has available certain abilities, as shown by skill or lack of skill in performing, for example, on some intellectual task such as conservation. To the extent that reaching, say, Stage X (as measured by performance on task 1) will also effectively predict competence on other tasks, 2, 3, 4 . . . n, there may be some summary value in speaking of stages, *but* their meaning remains anchored in the matrix of intercorrelations among tasks 1 . . . n. In such terms, it would be remarkable if Stage X (or score on task 1) were a perfect or nearly perfect index of success on the other tasks. Behavioral variables usually relate to one another imperfectly, so that positive functions, but not total concordance, are found. Yet as currently used, descriptive stages often connote a sudden or discontinuous threshold: Before Stage X, none of these tasks is understood, but afterward, a broad family of tasks is grasped with virtually perfect finesse. In fact, empirical data often depart from such all-or-none, quantum leap, stage conceptions, and evidence of this kind calls descriptive stages of cognitive development into question, as discussed elsewhere (Rosenthal & Zimmerman, 1978). For example, in some subcultural milieus, adolescents and even adults may rarely reason in the ways expected (formal operations) by Piagetian premises about universal stages (Gholson & Beilin, 1978; Peleg & Adler, 1977).

Brainerd's major quarrel is with stages as *explanatory* constructs. He notes the lack of detailed specification of how variables operate within Piaget's stages, as well as the dearth of measures that are not intertwined with the very responses the theory seeks to explain. Perhaps more generally, a Piagetian stance would gain considerably in explanatory value (1) if the expected relationships among variables were better specified in a form that allowed more clear-cut tests, (2) if the conditions that would need to be met in order to force major changes in the theory were spelled out (i.e., what empirical evidence would be required to refute which key premises?), and (3) if uncertainties about potential choice-points in the theory were acknowledged, for example: Which suppositions are regarded as pivotal? Which are less secure? And how might empirical data of a defined sort shift the interpretive balance toward different working assumptions? As things stand, there is too much latitude for subjective interpretations about precisely what Piagetian stage theory proposes, how it can be tested fairly, and what weight to assign various types of "disconfirming" results.

Brainerd shows that a constant sequence in attaining successive skills need *not* count as support for maturational views. Serial skill dependencies (and the measurement sequences they create) are more often the rule than the exception: If a baby crawls, then walks, then learns to ride a bike, and only then learns to dance, one need not assume that the crawling-walking-biking-dancing sequence results from maturation. Nor does a fixed order of steps in developing the skills of a composer require maturational assumptions. Yet one learns to listen to music, then to read musical notation, then to understand chord and harmonic structures, and then to apply them in composing symphonies. Such serially-dependent chains of skills necessarily build in progressive steps but demand neither innate maturation nor a stage construct to account for their regular, stepwise pattern of attainment. Psychologists of whatever conceptual allegiance may acknowledge that too little attention has been given to carefully studying the hierarchial "layering" of skill families without having to subscribe to a stage view of the layering process. Other cogent caveats concerning consecutive cognitive configurations are raised in Brainerd's scrutiny of the logic of stage assumptions in Piagetian developmental theory. Put most conservatively, he concludes that major features of Piagetian theory are not proved in the light of evidence and epistemology, and that plausible alternative interpretations remain alive and well.

It has elsewhere been noted that Piagetian theory does not require some of the burdens it has placed on itself; for example, its main impact and contributions would not suffer if it were revised to better address questions of methodology and interpretation raised by contemporary research on information-processing (Rosenthal & Zimmerman, 1978). The larger impetus of Brainerd's contribution is to invite serious reevaluation of stage constructs now current in cognitive development. At the present time, "behaviorism" is in lively ferment; it appears to be actively undergoing reformulation in more symbolic, cognitive terms. It is not also timely for Genevan thought to come to grips with the leaks in its systematic plumbing? With the actual gap narrowing between "behavioral" and "organismic" viewpoints, there is now the potential for a Stage of Rapproachment (or at least Détente) if earnest conciliatory efforts were initiated.

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"Measurement sequences," Piagetian structures, and high-order rules. Let me say first off that I have worked with Brainerd on a number of projects and that I consider him to be a good friend. I also share many of his reservations concerning the currently widespread and often unthinking acceptance of the Piagetian point of view. In some ways, such blind acceptance is almost as sad a commentary on American behavioral science as was the fact that it took us approximately forty years to appreciate the really important contributions made by Piaget and his collaborators. On the other hand, friends do not always agree, and this is partially the case with the present critique of Brainerd's paper.

The general thrust of Brainerd's paper is that Piaget's stages have no explanatory power, that they are operationally circular. One of Piaget's main contentions, for example, is that the particular invariant sequences that he postulates will necessarily be invariant in *all* environmental settings. This would seem to be a theoretical statement that could be supported or refuted by empirical evidence.

Brainerd, however, essentially argues that the tasks associated with Piaget's preliminary stages are *logical* prerequisites of those tasks associated with subsequent stages. Brainerd calls these "measurement sequences," which occur "whenever each item in the sequence consistes of the immediately preceding item plus some additional things."

To illustrate the notion of a measurement sequence, Brainerd states, "To multiply, children must know how to add" because "multiplication *is* defined in terms of addition." True, it is almost always the case that children acquire these skills in the indicated order. I would propose, however, that this is a result of how our educational system (broadly defined) is organized. In general, the contention is false. A person can be taught successfully how to multiply before knowing *anything* about addition. The product 3×2, for example, is simply the numer of pairs of the two-dimensional array

	1	2	3
1	(1,1)	(1,2)	(1,3)
2	(2,1)	(2,2)	(2,3)

or 6. Notice that all one has to do is to count and the the procedure is perfectly general.

In like manner, students can be taught how to differentiate such things as Sin x and $ax^3 + bx$ without knowing anything at all about how to multiply – thus, contradicting a second of Brainerd's contentions. For example,

$$\frac{d \operatorname{Sin} x}{dx} = \operatorname{Cos} x$$

$$\frac{d\,(ax^3 + bx)}{dx} = 3ax^2 + b$$

can be learned as strictly formal symbolic manipulations with no reference whatsoever to multiplication.

More to the point, there is a basic flaw in the Brainerd argument since it implicitly assumes that there is a unique basis for solving any given class of tasks. He argues, for example, that successful performance on tasks associated with the stage of concrete operations can be achieved only where children have first acquired the capability of performing successfully on tasks associated with the preoperational stage. This type of argument simply does not follow; it is impossible to define so-called measurement sequences independently of the structures/processes that underlie them. There are any number of different ways (structures/processes) by which a given class of tasks might be solved (Scandura, 1964, 1969, 1970, 1971, 1973, 1977). Although subordinate/superordinate relationships may exist among various structures/processes, this is not true in general of tasks that may be solved by using them.

In his article Brainerd emphasizes (possibly in an attempt to circumvent earlier criticisms) that he is not suggesting that *all* behaviors associated with the various Piagetian stages are related by means of measurement sequences, but that this is often the case. He concludes, nonetheless, "The fact that *any* sequences of this sort can be identified entails that the invariant sequence criterion cannot be accepted as prima facie evidence that 'objectively certain stages exist.' "

If Brainerd means here that Piaget has not adequately operationized his stages then, of course, we would probably all agree. However, Brainerd clearly seems to be implying that there are such things as measurement sequences independent of the structures/procedures underlying task performance. As emphasized above, this assumption is patently false. Tasks themselves cannot be logically interdependent, only structures/processes for achieving them can be, and then only in the sense that using one structure/process involves using the other as a component.

Here, then, is the source of a major difference between North American empiricism and Genevan structuralism. Clearly, Piaget intends for the structures associated with his various stages to be hierarchically related. The behaviors that these structures make possible, however, may be generated in any number of ways (i.e., by any number of processes/structures). (It is a mathematical fact that if there is one rule/procedure for solving a given class of tasks, then there must be an infinitude of others that will do the same thing.) There is no guarantee, just because particular solution rules associated with different classes of tasks are hierarchically related, that this same relationship will exist between arbitrary solution rules associated with these classes. Moreover, it is impossible to determine whether or not such relationships exist in the absence of rigorous rule based analyses of the respective tasks (Scandura, 1977). In short, Brainerd's measurement sequences are a myth. Such sequences cannot be defined independently of the underlying structure/processes that generate them.

The same general misunderstanding seems to underlie the author's arguments regarding cognitive structures. For example, Brainerd argues: "It should never happen that problem classes with the representations of later stages are solved during earlier stages. Under such conditions, the structural distinction between stages breaks down completely."

This is not necessarily the case, for the reasons indicated above. In particular, problem classes associated with later stages do not have unique bases for solution. The relatively simple prescriptions preferred in North American training studies, for example, are surely not identical with the structures postulated by Piaget. Learning a structure of the latter type and successful performance on associated problem classes are not necessarily the same thing. Thus, successful performance on problems in such classes does not necessarily imply that a Piagetian-type structure has been learned.

The following seriation task illustrates this fact. In the task, a child is shown a set of sticks seriated by length, but with the relevant end of the sticks hidden by a screen. The child is given a new stick, "×," and is asked to insert it in the right position. To accomplish this, the child is allowed to ask the experimenter how the length of "×" compares with any of the seriated sticks (one at a time). According to Piaget, if one is to avoid redundant comparisons, success on this task requires the transitivity concept (structure). That is, the child must know that $a > b$ and $b > c$ necessarily entails $a > c$. (Such knowledge would avoid redundant comparisons because, given the results of any one comparison, the child would be able to eliminate other possible comparisons as logically dependent.) Nonetheless, the child could also succeed on the task by applying the following rule: Compare "×" with the first seriated stick. If "×" is shorter, put "×" before the stick and stop. If "×" is longer, compare "×" to the next stick and test "×" as above. This example also illustrates why most successful North American training studies are not directly relevant to the structure of Piagetian theory. In the example, transitivity corresponds more to the *construction* of solution rules (like the above) than to the a priori knowledge of such rules or their application. (This example was suggested by one of my doctoral students, Roland Schneider, after reading a draft of my commentary. His commentary on mine is gratefully acknowledged).

More generally, Piagetian structures appear to be related more to the construction and the choice of solution rules than to solution rules themselves (or their application). To illustrate this difference, consider an analogy between the teacher as a programmer (i.e., a constructor of solution rules) and the child as a computer (i.e., a user of solution rules). It is evident that the programmer and the computer do not need the same "cognitive" structures to succeed on a given task. Moreover, it would appear that the only kind of training experiments that could be relevant to Piagetian structure would be experiments where the child is taught how to construct and select solution rules (i.e., not only how to use given ones).

Presumably, of course, one would want a precise operational (behavioral) definition of just what a structure is. To my knowledge, Piaget has not done this, and this is an important limitation that Brainerd has reemphasized. As I have shown above, however, the arguments advanced, in themselves, are not especially damaging to Piaget's theory. Piaget's formulation is an idealization; it is a theory of what behavior would be like under certain "idealized conditions." Unfortunately, Genevan psychologists have not adequately specified just what those idealized conditions might be. Until they do, the theory will necessarily remain nonoperational. [The nature of idealized theories, and their relationship to normative ones, is discussed in Scandura, 1971, 1977, especially Chs. 1, 5, 7, 10, and 11.]) I am also inclined to agree with Brainerd that the formalism introduced by Piaget to represent knowledge is not a particularly useful one. While it may have been the best available at the time Piaget initially developed this theory, I do not believe that that is any longer the case. Indeed, if Piaget himself had had access to some of the modern tools that are presently available for representing cognitive structures and processes, I suspect that his theory might have taken a quite different turn.

More to the point, and this may come as no surprise, I suspect that the structural learning formalism (Scandura, 1971, 1973, 1977) may be especially useful in this regard. The notion of higher-order rules, or rules that operate on other rules and select and/or construct new ones, seems especially relevant. We have recently begun work in this direction and so far our results appear promising. In this regard, I do not view the Structural Learning Theory as necessarily contradictory to the mass of descriptive data compiled by Piaget and his collaborators over the years. Rather, it has the potential of providing a far more precise way to deal with developmental phenomena.

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Is Piaget a Pied Piper? Brainerd's paper provides a detailed summary of the criticisms that Piaget's stage theory has attracted over the past ten years.

Three of the arguments seem particularly worthy of reiteration. First is the point concerning measurement sequences. As Brainerd notes, a consistent developmental sequence does not necessarily indicate any psychologically meaningful relationship; instead, the sequence may be guaranteed by the logical connections among the concepts (Flavell, 1971, 1972; Flavell & Wohlwill, 1969, *oper. cit.*). The second point concerns the vagueness and ambiguity of Piaget's logical structures; there seems to be widespread agreement among developmental psychologists that these structures often have little or no direct mapping onto the behaviors they are said to explain. Third is the lack of any independent assessment procedures for the explanatory constructs within Piaget's system; as Case (1974), Siegler (1976), and Trabasso (in press) have pointed out, Piaget has not provided any way of measuring disequilibrium, the INRC group, and other explanatory variables that he proposes.

These criticisms raise an interesting question. If Piaget's stage theory is so woefully deficient along so many dimensions, why does it continue to be the dominant approach within developmental psychology? One possibility is that developmental psychologists are like rats following the Pied Piper, and that we are all well on our way out of town. This view does not explain, however, why Piaget's theory has so long outlived those of his contemporaries – Gesell, Guthrie, Hull, Tolman, and Spence – or why it has attracted such interest in disciplines as diverse as psychology, biology, physics, philosophy, and education. The remainder of this critique will be devoted to examining some reasons for the superior endurance of Piaget's approach.

Perhaps the most basic reason for the theory's staying power is the inherently interesting topics that it focuses on: children's concepts of time, speed, distance, number, causation, ordering, classification, probability, proportionality, perspective, morality, and so forth. The research program conveys a strong impression of a scientist sitting down, asking himself what the truly basic intellectual acquisition of man are, and then spending half a century systematically studying their development. Even if Piaget's stage theory were entirely worthless, its introducing these topics to developmental study might explain its prominence.

But there is more. Piaget's empirical descriptions, particularly those dealing with the sensorimotor, preoperational, and concrete operational stages, are impressively reliable and replicable; for as long as subsequent investigators have come even moderately close to matching Piaget's original experimental procedures, they have generally duplicated his results. This statement may seem like faint praise to those in the physical and life sciences where replicability is often taken for granted, but those in psychology will recognize it as a meaningful tribute. In psychological experiments, seemingly trivial variations in the wording of instructions, the stimulus materials used and the types of questions asked often profoundly influence the results. This is much less true of Piaget's work than of that of most other investigators. For example, across broad variations in instructions and procedures, five-year-olds have been found to say that the glass with the taller liquid column must have the greater amount of liquid, that the side of the balance scale with more weight must go down, that the longer row always contains more objects, that the longer bar must cast the longer shadow, and so on (Siegler, 1977).

How can we account for the superior replicability of Piaget's findings? One possibility is that he is simply an unusually careful and astute observer. An alternative, however, is that Piaget has discovered something basic about children that allows him to make accurate predictions about a wide variety of their behaviors, thus making replicability less dependent on the particulars of the experimental situation. As Brainerd notes, five-year-old (preoperational stage) children are generally said to focus on only a single dimension in making judgements, ten-year-olds (concrete operational stage) are said to consider two or more dimensions but not to know how to combine them, and fifteen-year-olds (formal operational stage) are said to consider all relevant dimensions and to know the relevant combination rules. What is intriguing to me is not whether these approaches occur in the predicted order (for, of course, if they occur at all, the order must be the predicted one), but the fact that on so many tasks, the approaches, particularly the imperfect ones, show up at approximately the same ages. Whether or not one chooses to refer to this consistent behavioral pattern as stage, there clearly is an important phenomenon to be explained.

This brings us to the topic of explanation. Brainerd is certainly correct in his criticism that Piaget has failed to provide any means for measuring the explanatory constructs within his system. The criticism is overstated, however, to the extent that it implies that such independent assessment is impossible. One need look no further than to Brainerd's own work to disconfirm it. Brainerd (1976, 1977) has demonstrated ways of independently measuring three of Piaget's most prominent explanatory constructs: compensation, identity, and reversibility. Similarly, a number of other investigators have demonstrated that encoding, an explanatory construct that resembles Piaget's equilibrium/disequilibrium notion in important ways, can be independently assessed and used to explain a variety of cognitive-developmental phenomena: Trabasso (in press) used it to explain developmental differences in performance on the class inclusion task; Sternberg (1977) used it to explain developmental differences on the transitivity task; and I (1977; in press) used it to explain developmental differences on the balance scale and projection of shadows tasks. These studies indicate that it is possible to meet reasonable explanatory standards with constructs quite similar to the Piagetian ones.

In conclusion, Brainerd has accurately identified the least satisfactory part of the Piagetian system, its efforts at explanation. It seems, however, that this may not be an "in principle" difficulty; recent work, including Brainerd's own, suggests that Piaget's explanatory constructs can be measured independent of the behavior they are said to explain. Part of the continuing allure of Piaget's theory may be just this capacity to incorporate methodological improvements while maintaining its basic conceptual coherence.

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Measurement sequences, logical necessity, and common sense. In my comment I shall focus exclusively on Brainerd's discussion of measurement sequences, since I believe these have a significance far transcending the area of developmental research. I have no comments on Brainerd's other main points. They seem to be compatible with my own current evaluation of Piaget's theory (1977).

Brainerd introduces the term "measurement sequence" to describe developmental sequences that result "from definitional connections between the behaviors being measured." He also writes: "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. This is because, logically, it is impossible to devise valid tests of later items that do not measure earlier items." I have three comments on this:

First, what Brainerd calls measurement sequences are only a special case of relationships that are logically necessary because they follow from the definitions of the concepts involved. For example, stinginess is inversely related to generosity, not as an empirically testable fact, but because the concepts are opposites. The entire field of psychology is permeated by such relationships (Smedslund, 1972, 1976).

A recent, widely influential, theory consists almost entirely of logically necessary propositions, without empirically testable content (Smedslund, 1978).

Second, the very existence of measurement sequences, and, more generally, logically necessary relationships means that, for every postulated relationship, the psychologist must ask whether it is logical or empirical. This can be answered only by means of a careful logical analysis of the relationships between the concepts involved. A relationship is empirically testable only when the concepts involved are logically independent. Psychologists in all fields have generally neglected this aspect of their work. The result is that much research must be regarded as *pseudo-empirical*, in the sense that it involves attempts to establish logical relationships by empirical methods. This is equivalent to trying to demonstrate the Pythagorean Theorem experimentally. (See the references given above.)

Third, we may seriously ask the following question: Do there exist psychological regularities that are not logically necessary, given the definitions of the concepts involved, and that are supported by data showing that they may be universally valid? My position is that there may exist no such regularities. If I am right, the entire project of psychological research must be revised. We must recognize that, in our theorizing and in our data-gathering, we are bound by the conceptual framework embedded in our natural language and in our extra-linguistic culturally defined patterns of communication. This framework, which may be called *common sense*, was acquired while we were being socialized into adult "normal" members of society, and, hence, is anterior to and organizes our observations and our thinking as psychologists. For descriptions of common sense psychology, see particularly Heider (1958), Smedslund (1972), and Laucken (1974). Common-sense formulations are logically necessary and hence, not empirically testable.

In the new paradigm, psychological theory will be a formal discipline, similar for example, to geometry, stating explicitly our basic conceptual framework. It may be of great help to the practitioner in the same way the geometry is of help to the topographer or navigator. Since there are no universal psychological regularities, empirical work will focus on practical problems, including practically relevant local regularities. "Local" is here meant as "limited to cultural/technological conditions and subject to change as culture/technology changes." Recognition that observed regularities may be local rather than universal is apparent in the recent discussion of cohort changes in developmental studies (Baltes & Nesselroade, 1972). This also means that the affinity between psychology and history becomes prominent (Smedslund, 1973; Gergen, 1973, 1976).

In conclusion, the reader is asked to consider whether he knows any universally valid relationships between logically independent variables (psychological "laws"). If he finds only measurement sequences and other logically necessary relationships, he may find the time ripe for a true revolution in our conception of psychology.

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Holistic aspects of the stage notion. Assuming that psychology is not in a looking glass world, it may not rely on Humpty Dumpty's straightforward method for establishing the legitimate meaning of its major concepts. It is necessary to review from time to time the meanings accrued by concepts through usage as well as those bestowed upon them by the theoretical system from which they come. Brainerd's discussion of the concept of stage in Piaget's theory continues a long series of probings into the status of this notion and is welcome for attempting to stimulate further reflection on the implications of a stage conception of cognitive development. My comments will be directed to a few aspects of the question that may have received insufficient consideration in the paper.

First of all, it seems to me important to separate consideration of the theoretical implications of the concept of stage from consideration of any empirical evidence bearing on those implications. It makes considerable difference whether a notion is rejected because it is empty or because it is false. It is the theoretical implications of the stage concept that need clarification, and Brainerd's discussion stays mostly at the theoretical level, although in a few instances he short-circuits the argument by bringing in empirical findings. Thus, in the discussion of equilibration, the data on the continuous acquisition of concrete-operational behaviors are used to question the equilibration criterion. Yet the notion of equilibration is so central to Piaget's theory, and the relations between the notions of equilibration and of developmental stages so much in need of explication that, at this point, it seems premature to judge the equilibration criterion on the basis of empirical data. The same point holds for the criterion of structure. Once the implications of the notion of stage are clarified, it is likely that the need for studies different from those already available will become apparent and the task of collecting significant evidence can get underway.

In discussing Piaget's five criteria for establishing stages in cognitive development, Brainerd considers each of them individually. It seems to me that they have to be taken conjointly, since each gains clarity from being considered in relation to the others. What does invariant sequence refer to without the notion of cognitive structures? What meaning does integration and consolidation have without the notion of sequence? How is equilibration to be discussed without the notion of cognitive structure? Taking these criteria as a totality does not amount to bringing in stages through the back door, but it does suggest certain issues that may merit brief mention.

One issue has to do with the relation between any given task performance and cognitive structure; no one-to-one relation seems to be assumed by Piaget. This being the case, we face the problem of determining when an individual can be said to have constructed certain cognitive structures. I have raised this problem previously (1976a, b). It seems to me that it is related to several points made by Brainerd in his paper.

The question of circularity with respect to the explanatory use of the stage concept is clearly related to this issue. If a task performance is used to determine stage level and then stage level is used to explain the task performance, the circularity is obvious. The case is no better when chronological age is used to determine stage level and it is in turn used to explain task performance. However, theoretically, stage level can be determined independently of a specific task performance by evaluating performance on a number of other tasks. This is the course followed in more sophisticated Piagetian research (e.g., Inhelder et al., 1974). The problem is deciding what constitutes an acceptable procedure for establishing the stage level in cognitive functioning.

The fact that many sequences described by Piaget fit what Brainerd calls a "measurement sequence" is taken to be a major limitation of the invariant sequence as well as of the integration criterion. The argument holds if performance in one task is taken as diagnostic of the level of a cognitive structure. However, if we consider performance in a number of tasks related to that cognitive structure, then expectations for sequence acquisition need not be confounded with the measurement operation. For me, the crux of the problem has to do with establishing the realm of a cognitive structure and determining the tasks relevant to it. Given horizontal décalage, it is certainly to be expected that a cognitive structure would be manifest in some task but not others. What should not happen in view of the invariant sequence criterion is that evidence for a higher level cognitive structure be obtained prior to any evidence of the lower level structure within the same realm. And there's the rub. The chore of establishing, through logical analysis, the domain of tasks relevant to various cognitive structures remains to be accomplished.

Moreover, even the so-called measurement sequences may be important if one considers them not only from the standpoint of sequence, but also in terms of the criterion of integration. Brainerd states that "a measurement sequence occurs whenever each item in the sequence consists of the immediately preceding item plus some new things." This statement creates the impression of an additive sequence. However, with respect to stages the critical matter is that the understanding of *A* changes once *B* is acquired and integrates *A*. Maybe the question should not be whether *B* can be seen without *A*, but whether an understanding of *A* changes once *B* is acquired.

Another issue relates to the specification of the set of cognitive structures characterizing a stage. When the criterion of structure is considered singly, it seems very serious that structures may not be uniquely related to stages. However, when the criteria of integration and consolidation are considered as well, it seems that some evidence for higher level structures should be found at earlier stages and remnants of lower level structures should be found prior to consolidation of the higher stage. The problem consists of specifying when the shift from a lower to a higher stage has taken place. Piaget has stated: "since the preparation of later acquisitions can involve more than a stage (with various overlappings among certain preparations, some shorter and others longer), and since there are various degrees of stability in the completions, in any series of stages there must be a distinction between the *process of formation,* or of birth, and the *final forms of equilibrium* (in the relative sense). Only the latter constitute the whole structures" (1974, pp. 52–53, italics in the original). It is these structures of the whole *(structure d'ensemble)* that are said to characterize a stage. The evidence that is required seems to pertain to the formation of this structure of the whole and not to the presence of constituent cognitive structures. Thus, specification of the structures needed to constitute the whole becomes a serious task.

Finally, in view of Piaget's structural-constructivist theory of development, it seems to me that the question that might merit discussion is whether a notion of invariant stages in cognitive development is crucial for the theory. The alternative position is not necessarily either lack of organization or of sequence. If the interplay of biological factors, social factors, and self-regulatory factors in development is to be taken seriously, the possibility of certain alternative constructions must be entertained. Proposing a system of stages with some possible alternatives would not make the stages any more arbitrary, but would demand even more work to conceptualize clearly such a system.

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In defence of circularity. Sydney Smith is said once to have observed two women carrying on a heated argument from windows on opposite sides of a street and remarked to a companion, "These women will never agree, they are arguing from different premises." A similar comment may be made on the continuing argument between Piaget and his collaborators and psychologists of the American school, carried on from laboratories on opposite sides of the Atlantic!

Brainerd seems to me to typify the American approach according to which what Piaget is doing is not merely wrong but wrong a priori (i.e., "unscientific"). He has not simply failed to find satisfactory explanations on cognitive development, but has in effect adopted methods that preclude success. For Brainerd, explanation requires "procedures whereby the antecedent variables can be measured independently of behavioral changes"; Coghill (1929 *op. cit.*) explained the developing behaviour patterns of tadpoles in terms of independently observable neurological changes; Bijou (1975 *op. cit.*) explained the behaviour of children in his basic stage in terms of independently observable contingencies of reinforcement; these count as explanations. Piaget, however, observes only behaviour, and the structures he proposes can therefore be "at most abstractions from behavior," not explanations.

Piaget does not believe that his stages are the consequence of independently observable antecedent behavioural variables. If they are the consequence of age-related neurophysiological changes, these are certainly not independently observable in the present state of the art. He is therefore precluded from offering "explanations" in Brainerd's sense and offers, instead, "criteria." Brainerd's arguments purport to rule out Piaget's first four criteria a priori and the fifth on empirical grounds. According to Brainerd's conception of science, Piaget cannot hope to succeed in doing what he is trying to do. What worries me is that Brainerd seems wholly unaware that his conception of science might be challenged. It amounts to a version of Humean empiricism that asserts that we are wholly dependent on our observations ("impressions of sense"), having no grounds for supposing that there may be a reality beyond our observations that is causally responsible for them. Empiricism holds that it is illegitimate to speculate on what kind of a thing it might be that could be causally responsible for the fact that what we observe occurs in the manner in which we observe it to occur, with one set of observations regularly preceding another, and so forth. Instead, we must limit ourselves to deriving laws from the "constant conjunctions" we encounter in our sequences of observations.

Psychology was the first science to attempt consciously to model itself on the practice of other established sciences. It adopted empiricism because this was said to codify the practice of these other sciences. Unfortunately the other sciences paid only lip service to empiricism out of respect for the philosophers of the time. The actual practice of scientists was "realist" not "empiricist." They "speculated on what kind of a thing it might be . . . etc.," which is what Piaget does. It is ironic that Piaget appears to be doing more or less successfully what physical and biological scientists do and have always done, whereas Brainerd is doing only what scientists said they were doing when psychology first established itself as an independent discipline. Bhaskar (1975) has shown that the successful practice of experimental science is in fact inconsistent with empiricist assumptions. The scientists' instinct proved more reliable than philosophical analysis. I have elsewhere (1977) tried to show why it was that the misconception had no serious consequences for any science except psychology, where it resulted in the methodological behaviourism to which Brainerd appears to subscribe.

Space does not permit any attempt to explain here why empiricism proved so fatally attractive to psychologists. In part, the cause was a simple verbal error; it was assumed (wrongly) that *empiricism* must be the appropriate theoretical basis for an *empirical* science. The conclusions I wish to draw in the present context are that Brainerd's arguments will rightly cut no ice with Piaget because Piaget is not trying to do what Brainerd thinks he ought to be trying to do and that Brainerd ought to consider carefully whether anything useful can be done in the conceptual straight jacket within which he has chosen to confine himself. Psychology has at present all the trappings of a genuine experimental science but little of the real content; perhaps the fault lies in a mistaken conception of the nature of scientific activity.

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Which comes first – describing or explaining? Current data show that there is something wrong with Piaget's stage hypotheses and with the general idea that cognitive development takes place in reorganizations of competence expressed through coordinated changes in performance. But I am not satisfied with this picture of the problem, which says that Piaget is simply offering descriptions. Furthermore, I believe the Piagetian stage hypotheses have some scientific value and some value for educators even though there is something wrong about them and even though they do not prescribe causal explanations.

The heart of the argument here is that Piaget's stages "fall somewhere between the poles of true explanation and pure description" or, even more serious they may be "purely descriptive." Consider two purely descriptive hypotheses about a set of phenomena, neither of them offering a causal explanation. Must the two be equal and empty, or is there a chance that one description might be superior to the other? If there are better and worse descriptions, then it might be a scientific achievement to find a better description, and it is even conceivable that that better description might be of practical value for such people as educators. How might one description be better than another?

It seems useful to approach this question indirectly. There is evidence that one does not need much scientific description of children in order to come up with nuclear ideas in Piagetian theory.

Romanes (1889) argues that human mental life goes through a *receptual stage,* a wisdom of action that man shares with the animals, a *preconceptual stage,* an imagistic kind of ideation built by reflection upon actions, and a *conceptual stage,* in which a formal kind of thinking is built by reflections upon images. Sechenov's (1935) *Essentials of Thought* intended to integrate the theoretical writings of Helmholtz and Spencer. He argued that a child's mental development goes from *automatic sensory thinking* to *concrete object thinking* to *abstract thinking.* Baldwin (1895) proposed "three great stages of adaptation": *biological adaptations, reflex attention,* and *conscious selection of ends and their pursuit by adaptation.* Baldwin proposed later (1906–15) a hierarchical series of nine genetic "modes" and "objects" of thought. These were then construed as an ascending series of five logical forms of thought, from the "pre-logical" to the "extra-logical." The first three logics look like the Piagetian stages and the last two, perhaps, like adult stages of thought.

The developmental stage theories of Romanes, Sechenov, and Baldwin have significant contemporary parallels in the writings of Herbert Spencer, John Hughlings Jackson, Sigmund Freud, and G. Stanley Hall. But perhaps enough has been set forth to offer an argument about the Piagetian stages.

The resemblance of Piaget's theory to these early stage theories seems obvious. There is much that he says that the early theorists are *not* saying. They are not saying that there are across-the-board reorganizations of children's competence. They do not propose age boundaries. Nevertheless, they are with Piaget in asserting that there are several alternative organizations of human knowledge, and that children will show these organizations in a regular sequence as they develop. These anticipations are not casual. The authors are a distributed set of the more creative and original men of their time. They write at some length about the stages of children's thought. It is clear that the subject interests them, they feel they have something to say about it, and they are concerned to work out their ideas in some detail.

At the time when these formulations were produced, there was very little organized empirical research on children. Miscellaneous observations and writings on childhood did exist, and these were collected together by Hall under the umbrella of the Child Study Movement (Wilson, 1975). These authors make no appeal to that kind of data base. Baldwin did do some observations of infancy, and these were important in organizing his point of view, but his theory goes far beyond his observations in scope. Whatever these authors were doing, they were not describing children in any simple sense. They knew a lot about children, as any human being knows a lot about children. But they proceeded without regard to that elaborate body of observations and experiments associated with Piaget's writings. How could they produce scientific theories about childhood in advance of any formal observations? They seem to be writing from a mass of late 19th-century information about evolution, the organization of the brain, comparative linguistics, embryology, biology, and anthropology. These, together with the work of analytic philosophy, seemed to dictate some hypotheses about the structure of human knowledge. In their writings about children, these authors seem to be trying to invent a description of the development of children's thought that would bring that body of phenomena into alignment with a body of evolutionary thinking about the nature of the human mind.

None of these historic authors tells you when and how to look at children in order to see the stage processes they write about. Piaget tries to do that, and many of the problems with his theory seem to arise from the fact that he is forcing his descriptions, overinterpreting his observations to bring them into conformity with an expected pattern. If this analysis is sound, there is a "deep structure" to Piagetian theory that does not rest first and entirely on direct observations of children. That deep structure rests in some broad biological and philosophical conceptions of thinking that form the point of view. Piaget is not simply trying to explain cognitive development. He is trying to describe it. He is working back and forth between a body of assumptions and a body of empirical phenomena, trying to create a detailed correspondence between theory and phenomena. He is, in a sense, trying to work simultaneously on the creation of description and explanation.

The critique offered here assumes the traditional philosophy of science as applied to psychology, which assumes that empirical and immediate proof is the only touchstone of adequate scientific theorizing. The organization of scientific work is toward the maximization of the "principles of proof." That kind of philosophy of science works best when theory arises out of one data base and makes reference to causal explanations that may be perceived and tested within the unique data base. But psychological knowledge seems to arise out of disparate data bases distributed within and without the discipline of Psychology. Psychology is multiparadigmatic (White, 1976, 1977). Sooner or later, a philosophy of science appropriate for Psychology is going to have to account for the continuing importance of Darwin and Freud and the clearly growing importance of Piagetian theory. It is not so sure that the "principle of proof" is the only thing that guides scientific work; there is a "principles of consistency" as well. Some theories align phenomena with a fairly narrow body of other scientific data, others align phenomena with a broader body of other scientific phenomena. Other things being equal, a descriptive form that brings more data into consistency may be superior to one that takes a narrower view of a body of phenomena. It aligns phenomena with a broader body of data bases within which one can search for, if not immediate explanations, then reductions of uncertainty about where explanations can be found. In that sense, one scientific description can be superior to another.

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by **Albert Yonas** *and* **Lawrence R. Carleton**

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Conjoint construct validation. We see two limitations of Brainerd's approach. First, his concept of testing for the presence of a theoretical construct is too narrow. Piaget's theoretical construct, stages, does not purport to link factors that bring about a stage (Brainerd's "antecedent variables") with the behavior characteristic of the stage. However, in general, it is not true that in order to be explanatory a theoretical construct must relate the factors that cause it to the consequences of its presence. Psychology is replete with such constructs as "intelligence" and "schizophrenia." Of course, it is essential to Piaget's theory that stages appear sequentially. But the failure of this theory to explain what brings about a stage does not render the construct inadequate as an explanation.

Second, Brainerd finds Piaget's five criteria for existence of stages individually inadequate. He considers the criterion of "constant order of succession" inadequate on the grounds that this constant order could be due to the nature of the tests, that is, the tests form a "measurement sequence." Brainerd has not addressed the possibility that Piaget's criteria, even if individually inadequate, might be adequate in combination. In particular, he has not considered the combination of the criteria of "constant order" and of "cognitive structure." A minimal implication of the notion of cognitive structure is that the structures characteristic of one stage would predict, across a wide variety of tasks, a different set of behaviors than would the structures of another stage. Even if we assume that the sequence of acquisition of skills on each of these tasks is logically necessary, consistency in the age at which an individual advances in the predicted way would provide convincing evidence that a change in a common underlying process is at work. Converging operations in this way could be used to validate the reality of the construct. Whether this has been accomplished for Piaget's construct is certainly open to question.

Finally, the problem with the notion of stage, as used by Piaget, is not that such a construct is an inadequate explanatory vehicle without the independent specification of antecedent variables, but, rather, that the theory is overly abstract and that the links between the construct and the observed behaviors are too loose. This makes it difficult to establish whether a stage construct is even an adequate description of a piece of behavior.

Author's Response

by **Charles J. Brainerd**

Invariant sequences, explanation, and other stage criteria: reflections and replies

Although the commentaries published in this issue deal with a myriad of matters, three themes recur with sufficient regularity to be called categories within which comments can be grouped: (*a*) culturally universal sequences in cognitive development; (*b*) the logic of scientific explanation; (*c*) additional criteria for stages. Since invariant sequences have long been viewed as the main phenomena on which stages must stand or fall, it is not surprising that the largest number of criticisms and addenda seem to be in category (*a*). Here are some examples: **Rosenthal** notes that measurement sequences are not unique to cognitive development and that they can be found in most other areas of behavioral development; **Markman** observes that the decision as to whether two concepts are linked by a measurement sequence is often difficult to make in practice and that we must scrupulously avoid classifying true invariant sequences as measurement sequences; **Baldwin & Baldwin** discuss a possible invariant sequence in primate social behavior; **Smedslund** argues that measurement sequences are a special case of psychological theorists' tendency to rely on predictions that may be logically guaranteed by the nature of our measurement operations; **Parker** reviews evidence that suggests that it is sometimes difficult to confirm invariant sequence predictions in cross-cultural studies.

The many comments in category (*a*) have prompted a few new thoughts on invariant sequence research. Since the existence of culturally universal invariant sequences seems to be the key empirical issue and since these ideas take some space to develop, the first section of this paper deals exclusively with the invariant sequence question. I argue that, measurement sequences aside, there may be no empirical datum that could convince us that invariant sequences do or do not exist. Some selected points from categories (*b*) and (*c*) are considered in the other two sections.

Measuring invariant sequences

Most comments in category (*a*) are concerned, in one way or another, with whether any of the invariant sequences predicted by Piaget have been confirmed, and with whether measurement sequences invalidate the invariant sequences criterion for stages. All these comments seem to presuppose that the existence of culturally universal sequences in concept development is an empirical question answerable by data. I do not believe that invariant sequence data are interpretable, however. By this I mean that we cannot use such data to make the sorts of theoretical inferences required by the invariant sequences criterion. Data on invariant sequences are so thoroughly confounded with measurement error that positive findings are incapable of convincing us that concept sequences exist in the population, and negative findings are incapable of convincing us that they do not. Thanks to measurement error, the invariant sequence criterion does not appear to make predictions that can be confirmed or disconfirmed by data. The main evidence for this statement is contained in a technical appendix to this response (Brainerd, 1978a) and consists of a few equations summarizing the effects of measurement error on the probability distributions of the random variables used to test null hypotheses in concept development studies.

The interpretability dilemma. Recall that the invariant sequence criterion stipulates that concepts from earlier stages should always develop before concepts from later stages. Take a well-worn example: children should acquire the identity concept (preoperational stage) before they acquire the conservation concept (concrete-operational stage) (Piaget, 1968, *op. cit.*). Suppose that we have two concepts, A and B, from different stages, and we are interested in testing the prediction that they develop in a fixed order. Three types of designs have been used in studies conducted to date: cross-sectional, longitudinal, and training experiments. In cross-sectional studies (Kofsky, 1966), which include cross-cultural studies as a special case, concept tests are administered once to subjects from the age range corresponding to when the concepts are believed to emerge. Longitudinal studies (Achenbach & Weisz, 1975; Kramer et al., 1975) begin with the low end of the age range; concept tests are administered repeatedly as subjects pass through the range. In training experiments (Brainerd, 1974; Litrownik et al., 1978), concept tests are administered from the low end of the age range. Subjects who fail all or nearly all the items are retained for training. They are assigned to training conditions, one for each pretested concept, and a treatment of some sort is administered. The subjects in each condition are then posttested for the trained concept and, perhaps, for others as well.

In all three designs, invariant sequence predictions are evaluated by testing null hypotheses about the relative difficulty of the concept tests at given assessment points. That is, (1) sample estimates of the expectation of some random variable of total test performance are computed, (2) significance tests are performed to determine whether or not the estimates for different tests differ significantly, and (3) easier tests are said to measure earlier-emerging concepts while equally difficult tests are said to measure synchronously emerging ones. I do not believe that the inferences involved in (3) are *ever* sound. To show why this is so, we shall have to examine the implications of such inferences in detail.

Consider two concepts, A and B. Let $P(A)$ be the unconditional probability that any subject from some well-defined population (i.e., age range) has concept A, and let $P(B)$ be the unconditional probability that any subject from the same population has concept B. Statements to the effect that A develops before B or B develops before A or the two concepts do not develop sequentially are statements about these unconditional probabilities. Specifically, they imply $P(A) > P(B)$ or $P(B) > P(A)$ or $P(A) = P(B)$ in the population. (The converse of this implication does not hold—i.e., the inference that $P(A) \neq P(B)$ does not imply sequentiality and the inference that $P(A) = P(B)$ does not imply synchrony.) The problem with basing inferences of this sort on evidence about comparative performance on concept tests is that test performance will normally be infected by false positive and false negative measurement errors. Sample statistics of overall test performance are biased estimators of the population parameters, $P(A)$ and $P(B)$, about which the invariant sequence criterion requires us to make inferences, and the biasing factors are measurement errors.

Let A and B be concept tests. For simplicity, assume that there are k items per test. The sample space consists of all possible sequences of correct and incorrect responses on each test. (The exact definitions of "correct response" and "incorrect response" are unimportant in the present context.) Let $a_1, a_2, \ldots a_k$ be a sequence of response random variables defined over the sample space such that $a_i = 0$ if a protocol shows a correct response for the ith item on test A and $a_i = 1$ otherwise. Let $b_1, b_2, \ldots b_k$ be a sequence of response random variables defined over the sample space such that $b_i = 0$ if a protocol shows a correct response for the ith item on test B and $b_i = 1$ otherwise. Let $P(a_i = 0)$ and $P(b_i = 0)$ be the unconditional probabilities of a correct response on the ith items of tests A and B, respectively. Assuming that test A is a valid measure of concept A, $P(a_i = 0)$ is a function of three variables: the unconditional probability that any subject has concept A, $P(A)$; the *conditional* probability that any subject who does not have concept A gives a correct response on the ith item, $P_i(0|\bar{A})$; and the conditional probability that any subject who has concept A gives an incorrect response on the ith item, $P_i(1|A)$.

The exact expression is

$$P(a_i = 0) = P(A) + P(\overline{A})P_i(0|\overline{A}) - P(A)P_i(1|A), \qquad (1)$$

where $P(\overline{A}) = 1 - P(A)$. The conditional probabilities $P_i(0|\overline{A})$ and $P_i(1|A)$ are, respectively, the false positive and false negative error rates for the ith item on test A. These values will increase with increases in the number of ways a subject can give a correct response without having concept A or an incorrect response despite having concept A. Similarly, the unconditional probability of a correct response on the ith item of test B is

$$P(b_i = 0) = P(B) + P(\overline{B})P_i(0|\overline{B}) - P(B)P_i(1|B), \qquad (2)$$

where $P(0|\overline{B})$ and $P(1|B)$ are, respectively, the false positive and false negative error rates for the ith item on test B. Since the expectation of any random variable is the sum of the products of its possible values and their respective unconditional probabilities, equations 1 and 2 give the expectations of the response random variables $a_1, \ldots a_k$ and $b_1, \ldots b_k$.

To compare subjects' performance on A and B, we would first need to define some statistic of test performance so that we could compute sample values from data. The exact statistic chosen will depend on our assumptions about the underlying scale of measurement. If we assume that the underlying scale is ratio or interval, we shall undoubtedly use expected total correct responses or expected proportion of correct responses. In a study, we would compute mean total corrects or mean proportion correct for tests A and B and test the null hypothesis that the means do not differ significantly (Brainerd & Hooper, 1975; Elkind & Schoenfeld, 1972). If we assume an ordinal or a nominal scale, the statistic will probably be an expected number of subjects who equal or exceed some arbitrarily high number or proportion of correct responses. In a study, we would classify subjects as "pass" or "fail" on each test according to some criterion of total test performance and test the null hypothesis that the observed numbers of passes on the two tests do not differ significantly (Kofsky, 1966; Wohlwill, 1960).

Whatever statistic we decide to use, it will have to be a moment of the probability distribution of some random variable defined over the sample space. In other words, the random variable will be some method of grouping possible outcomes of the sequences $a_1, \ldots a_k$ and $b_1, \ldots b_k$ into equivalence classes. This means that the rule that defines the random variable's probability distribution will be derived from equations 1 and 2. But this fact has an unfortunate consequence from the standpoint of the invariant sequence criterion. Observed differences between test A and test B performance on sample estimates of expectations can be explained in at least four ways: (a) $P(A) \neq P(B)$; (b) the false positive error rates differ for the two tests; (c) the false negative error rates differ for the two tests; (d) some combination of a–c. (I am excluding sampling error for the sake of simplicity.) A formal proof of this statement for the statistics mentioned earlier is given in the technical appendix to this response (Brainerd, 1978a).

In the first section of the technical appendix, I prove the statement for the first class of studies in which invariant sequence hypotheses have been tested, namely, studies in which an interval or ratio metric is assumed. In such studies, concept tests are administered to subjects and the mean total number of correct items is computed for each test. The concepts measured by the tests are said to develop sequentially if differences in mean performance are observed. It is shown that this datum does not justify the inference of sequentiality and, worse, that it does not lead to any theoretically interesting inferences at all. The proof turns on three points. First, the sequentiality inference for two concepts A and B implies $P(A) \neq P(B)$ in the population. Second, this latter inference is sound only if the observed proportion of responses for given items on Test A and Test B are unbiased estimates of $P(A)$ and $P(B)$, respectively. Third, observed proportions of correct responses per item are actually biased estimators of $P(A)$ and $P(B)$, and the biasing factors are measurement errors. Therefore, when statistics such as mean total correct items are used, $P(A) \neq P(B)$ is not a plausible inference when means are observed to differ, and $P(A) = P(B)$ is not a plausible inference when means are the same.

In the second section of the technical appendix, the proof is repeated for the second class of studies in which invariant sequence hypotheses have been tested, namely, studies in which an ordinal or a nominal metric is assumed. The difference between the first class of studies and the second is that the total number of correct items on any given test is used to assign subjects to discrete performance categories. Concepts are said to develop sequentially when differences in the distributions of subjects in the performance categories are observed. Specifically, tests that result in larger numbers of subjects in higher categories are said to measure earlier-emerging concepts. By repeating the proof for appropriate random variables, this inference is also shown to be unjustified. It follows that the measurement error problem cannot be avoided by switching to statistics that presuppose different scales of measurement.

Now, let us turn to the question of how these proofs make contact with the observations on invariant sequences made by the commentators. For the most part, these observations assume that the invariant sequence criterion makes predictions that can be submitted to empirical tests. The assumption is explicitly stated by **Scandura** when he notes that an invariant sequence prediction is "a theoretical statement that could be supported or refuted by empirical evidence." But this view is not supported by the formal analysis. When a line of investigation fails to produce evidence of a predicted sequence, a theory-saving explanation based on measurement error can immediately be devised. Alternatively, when evidence supporting a predicted sequence is obtained, a theory-contradictory explanation based on measurement error is immediately available. An unfortunate consequence of this situation is that it appears to negate most of the statements made by the commentators about invariant sequence research:

Fischer claims that predictions of same-stage synchrony have been consistently refuted, but the findings in question can be dismissed by proponents of the predictions on the grounds that the tests used to generate the findings involve different rates of measurement error. **Rosenthal** makes a similar claim, and it is subject to the same objection. When **Markman** warns us against mistaking true invariant sequences for measurement sequences, she assumes that after measurement sequences have been set aside there are some sequences left over that can be examined empirically. However, measurement errors make the resulting data uninterpretable. Similarly, when **Neimark** argues that measurement sequences and maturational sequences do not exhaust the list of invariant sequences, she makes the same assumption as does Markman. **Pascual-Leone** argues that even measurement sequences can be empirically disconfirmed. But the putative disconfirmations that he mentions are themselves subject to measurement error confounds. In fact, the only remarks about invariant sequence research by the commentators that find support in the analysis of measurement error confounds are **Smedslund**'s conjectures.

Unworkable solutions. Evidently, comparing subjects' concept test performance via statistics of the sort we have considered does not provide any basis for making inferences about the developmental ordering of concepts. In fact, we can formulate an algorithm for proving that *any* statistic of the type that has traditionally been used to test invariant sequence predictions is a biased estimator of the relevant theoretical parameters. First, define a random variable of concept test performance over the outcome space. Second, derive the expectation of this random variable. Third, show that the expectation may be expressed as a function of equations like 1 and 2.

If we are going to test invariant sequence predictions, it is obvious that we shall have to find some method of obtaining unbiased estimates of parameters such as $P(A)$ and $P(B)$. Unfortunately, there is no guarantee that such methods exist. As a rule, the problem of biased estimators is resolved either by using some other statistic of the data or by obtaining independent estimates of the biasing factors and correcting statistics currently in use. It does not appear that either of these strategies will work in the present case.

According to the first strategy, it might be possible to test predictions about concept ordering by using *other* moments of the probability distributions of test performance random variables. So far, we have only considered predictions about expectations. Although these are the only predictions that have been investigated to date, we might be able to show that the invariant sequence criterion makes predictions about other moments as well, for example, variance or skewness or kurtosis. The problem with this approach is that other moments are just as sensitive to measurement error as are expectations. This is due to a fact mentioned earlier, namely, the probability distribution of any test performance random variable will be a function of equations like 1 and 2. This is easily seen in the case of the random variables considered in the preceding section. For example, the function rules for total errors are shown in the third section of the technical appendix (Brainerd, 1978a). Note that these rules are defined by equations 1 and 2, respectively. Since the function rules for total errors are defined in terms of equations 1 and 2, it is apparent that the expressions for any of their respective moments will contain terms that refer to the probabilities of making false positive and false negative errors. Consequently, sample estimates of such statistics must necessarily be contaminated by measurement error. Similar statements can be made about the probability distributions of other random variables of concept test performance.

According to the second strategy, we might be able to correct observed values of the expectations of random variables such as total correct, proportion correct, and so on, for the effects of measurement error. Of course, this would require independent estimates of the false positive and false negative rates for individual concept tests. It is difficult to see how such estimates could be obtained. If only one type of measurement error were operating, one could administer parallel forms of differential difficulty to the same subjects. It would be reasonable to assume that performance differences between the forms were attributable to that particular measurement error. In the present case, however, there are two types of measurement error to contend with. What is more, they operate in opposite directions. Therefore, it is impossible to say whether observed differences between parallel forms are the result of differences between false positive rates or differences between false negative rates or both.

Remark. It seems, therefore, that we cannot interpret concept development data in theoretically meaningful ways. Although such data may tell us something about the relative difficulty of tests, they do not tell us anything about invariant sequences in the concepts that the tests profess to measure. As suggested above, whenever we fail to confirm a sequence that is predicted by the theory (see **Parker**'s paper for some cases in point), it can always be argued that our tests are prone to measurement error. Whenever we find a sequence that is proscribed by the theory (see **Fischer**'s paper for illustrations), it can likewise be argued that our tests are prone to measurement error. The fact that automatic, theory-saving explanations exist for disconfirmatory data is bound to provoke much fruitless controversy over the "correct" interpretation of concept development data. Interestingly, such controversies already exist in the literature. For example, the theory-contradictory sequences in same-stage concepts cited by **Fischer** are known to be less visible with the conservative tests used by Genevan researchers than with the more liberal tests used by many North American investigators (e.g., Brainerd & Brainerd, 1972; Brainerd & Hooper, 1975; Hooper & Toniolo, 1977; Rybash et al., 1975). I have interpreted (1975, 1977b) this finding as implying that the sequences in question exist, but it is hard to find them with Genevan methods because these methods are prone to false negative error. Larsen (1977) has countered with the interpretation that the sequences do not exist, and the data produced by non-Genevan methods are the result of false positive error. On the basis of the reasoning in the appendix, there may not be any way to decide between these interpretations with concept development data.

This brings us back to measurement sequences. Despite the demonstrable noninterpretability of invariant sequence data, common sense suggests that some things invariably precede others during development. Measurement error confounds notwithstanding, who would argue that crawling does not precede walking? Or, more to the point, who would argue that the concept of object permanence does not precede the concept of conservation? Since we appear to know these things quite independently of data, we might ask ourselves what the basis for our knowledge is. We are evidently applying an external, nonempirical criterion of some sort. If we asked working developmentalists to describe such a criterion, the answer would probably be, "I am most confident that one thing invariably precedes another when the later thing is vastly more complex than the earlier thing." For "vastly more complex," read "linked by a measurement sequence."

The logic of scientific explanation

Perhaps the most intersting and thought-provoking points raised by the commentators are those in category (*b*). These points are essentially matters of philosophy of science, not data, and some of them are as old as philosophy itself (see **Buss**'s* and **Bates**'s* papers). Since the chances of obtaining closure on any of these issues are remote, it might have been prudent to avoid them altogether in this reply. However, I should like to add some follow-up remarks on three points that are especially relevant to evaluating the theoretical adequacy of the stage construct, namely, the measurability criterion, explanation versus description, and the so-called "structuralist" approach to explanation.

Measurability. To a large extent, my paper was prompted by the possibility that the stage explanations of cognitive development that have been emanating from Geneva for many years may be circular statements cloaked in obscure language. I noted that a developmental construct, to be viewed as presumptively explanatory, should be concerned with real age changes in behavior, should specify some antecedent variables that are believed to cause these changes, and should provide methods whereby these variables can be measured independently of the changes they purport to explain. I also claimed that the third criterion, measurability, is necessary to ensure that stage explanations are not circular. Only one commentator, **Wetherick**, does not appear to be bothered by circular explanations.

Some commentators, especially **Kurtines** and **Ennis**, feel that the measurability criterion for explanatory stages is too severe. Before replying, I shall try to summarize the points at issue. An explanation, at least as I understand the term, is a proposition that asserts a lawful relationship between two sets of entities. One set (sometimes called the *explanans*) is said to explain the other (the *explanandum*). (This characterization might not be accepted by those who advocate "structural" explanation, but more on this later.) My original argument was that explanatory processes and explananda should be independently measurable to preclude the possibility of circularity. That is, without procedures for independently measuring explanatory variables and explananda, we lack the means of critically testing the hypothesized relationship and, hence, we cannot rule out the possibility that the explanatory variables are merely paraphrases of the explananda. The basic (and familiar) counterargument is that science often uses explanatory constructs that are not measurable in any strict sense: "most scientific theories contain explanatory components (e.g., electron, space curvature, waves, etc.) which, for either practical or theoretical reasons, lack overt procedures for empirical operationalization" (**Kurtines**). And "the explanatory factors, *atom, molecule,* and *gene,* were invented by physicists, chemists, and biologists before there were independent ways of identifying and measuring these factors" (**Ennis**). These entities do not seem to pose a circularity problem, and we do not seem to be any the worse off for having used them. Indeed, all of them have led to new programs of research that have produced important advances in our understanding of physical and biological phenomena. The essential claim, therefore, is that explanatory constructs often serve a heuristic function that the measurability criterion would tend to suppress.

I am in complete agreement with the spirit of this claim. Who could disagree? Theory construction is a creative process that should not be hampered by artificial rules and arbitrary dicta. Concepts that fail to meet rigid criteria of measurability, but produce fruitful new lines of research, are not to be scorned. Nevertheless, I do not believe that these statements nullify either the circularity criticism of Piagetian stage explanations or the relevance of the measurability criterion to such explanations. To maintain otherwise is to suggest that the stage-behavior relationship posited in Piaget's theory is equivalent, from a logical point of view, to the relationship between atoms, genes, and so on, and the facts they profess to explain. There are important differences. First, the circularity problem is less evident with genes and atoms because these concepts represent levels of analysis that are clearly different from the things they explain. Specifically, they seem to be more fundamental and penetrating levels of analysis. The problem with the stage construct, on the other hand, is that it does not seem to be a more fundamental level of analysis than behavior. In fact, it can be argued that stages are a more superficial level of analysis, that is, stages are behaviors with the fine detail erased. There does not seem to be any compelling evidence, either logical or empirical, that Piagetian stages do more than paraphrase conceptual behavior. Given such admittedly special circumstances, I think it is both reasonable and prudent to invoke the measurability criterion. I also think that the criterion is applicable to some of the illustrations presented by the commentators. For example, **Ennis**'s illustration that "birds fly south in the winter . . . because they have a south-flying instinct" comes perilously close to circularity. On its face, it *is* a circular statement resembling explanations of human behavior that were once favored by faculty psychologists. "South-flying instinct" does not seem to be a more basic level of analysis than "birds fly south." Nowadays, however, the statement can be saved from circularity if we are allowed to substitute the ethologists' definition of south-flying instinct for "south-flying instinct." It is in situations such as these that the measurability criterion can be most useful.

A second and more important difference between Piaget's stages and concepts such as molecule, gene, atom, and so on, is that the latter were clearly intended by their originators to be potentially measurable entities. By "potentially measurable" I mean only this. Although it is true that direct measurement procedures did not exist when these constructs were first used, it is also true that investigators believed that these entities could be measured and, more important, that their eventual measurement was a question of technology, *not of theory*. A gene, for example, had specifiable biochemical properties. The task was to find a molecule with the appropriate properties. Similarly, an electron had specifiable physical properties (e.g., a negative charge, a mass). The task was to find a particle with such properties. I think it can be reasonably argued that the property of potential measurability may be the chief reason that "unmeasurable" entities such as electrons are often powerful stimuli to new research. By developing the appropriate technology or by modifying existing technology, there is hope of making substantial new discoveries. The same hope does not exist with concepts that are not potentially measurable. In this connection, it is instructive to consider a truly unmeasurable construct that was introduced about the same time as the notion of an atom: Maxwell's Demon. Maxwell's Demon was proposed to explain some properties of gas molecules. By definition, however, the Demon was not subject to the laws of physics and, consequently, could not be measured independently of the effects he allegedly produced. History records that, unlike the electron and the gene, Maxwell's Demon was not a major impetus to new research.

Returning to Piaget's stages, it is not clear what one could conceivably measure, apart for the conceptual behaviors that the stages are supposed to explain, to confirm their existence. Curiously, Piaget has resisted attempts to equate his stages with phenomena at more basic levels of analysis. **Chevalier-Skolnikoff**,* for example, notes Piaget's long-standing resistance to grounding his stages in neurophysiological substrata. The ostensive reason for this resistance is that neurophysiological definitions of stages smack of maturationism, but it has never been clear why maturationism is to be avoided (Beilin, 1971, *op. cit.*). In any event, it is not clear that Piagetian stages are even potentially measurable. It seems that their measurement is not simply a matter of technology but, rather, will require large amounts of theoretical reformulation.

The how and the why of it (description and explanation). Some commentators (e.g., **Fischer, Kinsbourne, Kurtines, Neimark, Olson,** and **White**) have lodged objections to my emphasis on explanatory power as a means of evaluating the adequacy of Piaget's stages. Although many points are raised, there seem to be two underlying themes. First, the emphasis on explanatory power underplays the descriptive function of theories. Second, it may be that descriptions *are* explanations, that is, it may be that explanation consists in answering the question "how," not the question "why." The first theme is central to some of Kurtines's remarks and to all of White's paper. The second theme is most clearly evident in Olsons' and Fischer's observations on the structuralist mode of explanation, but I sense that Neimark and White have something similar in mind.

The first theme is easily disposed of. It was not my intention to imply that precise description is not an important trait of theories or that description is, somehow, less important than explanation. Good descriptions are obviously essential to good explanations because they anchor theories in the data of experience. If the earlier definition of an explanation is accepted, then it is clear that both the explanatory variables and the explananda are, at bottom, descriptive variables. On this view, an explanation is a proposition that asserts a lawful relationship between two sets of entities. The more precise our descriptions of these entities are, the greater is the probability that our explanations can be confirmed or disconfirmed in data. Clearly, then, we cannot have sound explanations without good descriptions. However this may be, it is important to recognize that being a systematic body of explanatory processes is often taken to be the hallmark of theories. It is probably the chief thing that distinguishes scientific theories from articulate common sense. While common sense is capable of precisely describing a set of facts (e.g., how many days the moon is full, the flow of the tides, the time of sunrise), theories aim at explaining why the facts are as they are and what their relations are to each other.

The second and more radical criticism of my emphasis on the explanatory adequacy of stages is that I may have adopted an invalid definition of explanation. Specifically, I failed to consider the possibility that descriptions are explanations. The question of whether description equals explanation or whether theories do more than simply describe has long been debated by philosophers of science. The "description equals explanation" position is typified by some remarks that E. W. Hobson made in 1923:

"The very common idea that it is the function of Natural Science to explain . . . cannot be accepted as true unless the word 'explain' is used in a very limited sense. The notions of efficient causation, and of logical necessity, not being applicable to the world of physical phenomena, the function of natural Science is to describe conceptually the sequences of events which are to be observed in Nature; but Natural Science cannot account for the existence of such sequences, and therefore cannot explain the phenomena in the physical world, in the strictest sense in which the term explanation can be used. Thus Natural Science describes, so far as it can, *how*, or in accordance with what rules, phenomena happen, but it is wholly incompetent to answer the question *why* they happen (quoted from Nagel, 1961, pp. 26–27)."

If it is true that good theories are simply well-articulated descriptions, then explanatory power may not be an appropriate measure of the stage construct's adequacy. However, it is not clear that the "description equals explanation" view is correct. There are two main objections. First, this view seems to be partly based on a misunderstanding or misrepresentation of what the question "why" is all about. Hobson's observations are founded on a very restrictive definition of what it means to answer the question "why." The specific definition seems to be one of logical necessity, that is, the question has not been satisfactorily answered unless the explanatory variables confer the status of logical necessity on the explananda. But this assumes that logical necessity is what most theorists have in mind, and that only logical necessity produces answers to "why" that most of us would regard as satisfactory. Neither assumption is warranted. Concerning the former, few theorists since Hume have assumed that the laws of science do more than make explananda highly probable. Concerning the latter, there are many examples of explanatory statements that, to most of us, would seem to be perfectly acceptable answers to "why" despite the fact that they do not confer logical necessity. Consider: "My chairman is grouchy today because the dean cut his budget." This statement certainly appears to do more than describe the dean's pecuniary behavior and the chairman's emotional behavior. It says that a lawful relationship obtains between the two that is probably capable of empirical confirmation. Nevertheless, grouchiness is not an invariable concomitant of budget cutting. It may not even be a frequent concomitant of budget cutting if the dean announces budget cuts only after dispensing tranquilizers.

The second objection to the "description equals explanation" view is more profound. The view presumes that putatively explanatory statements can always be translated into purely descriptive terminology, that is, a "why" answer can always be translated into a "how" answer. There is no evidence that this translation can invariably (or even usually) be done. Such translations would require parallel descriptive languages for scientific theories that do not exist. Several examples of explanatory statements that evidently cannot be translated into parallel descriptive terminology can be found in Nagel (1961, pp. 117–29). Readers interested in the "how" versus "why" issue are directed to Nagel's excellent technical discussion. The essential obstacle to translation is that the explanations of science are *idealizations*. Explanatory processes and explananda are indefinitely large classes of similar events, not singular phenomena. It is difficult to delineate the membership of such classes in descriptive language. What seems to be required is a function of some sort that maps the set of explanatory variables onto the set of explananda. It is in just this sense that explanations are more than answers to the question "how." It is the formulation of a functional relationship, often exceedingly abstract, that makes the explanations of science relevant to the question "why."

Note on structuralist explanation. It has been proposed in certain commentaries (e.g., **Olson**'s and **Fischer**'s) that Piaget subscribes to a species of explanation different from the one we have been considering. The species in question is one associated with the structuralist movement in linguistics and some social sciences. The most eloquent presentation of the structuralist approach appears in Olson's paper: "Structuralism . . . is a mode of . . . theory construction characterized by the idea of wholeness . . . transformations . . . and self-regulation. . . . A piece of intelligent behavior, like a piece of a sentence, is not to be seen, interpreted, or explained in terms of a response to a set of independently specified antecedent variables to which it is causally linked, but rather in terms of a general cognitive structure . . . of which it is a particular exemplar."

I have four replies. First: It is not all that clear that Piaget is an uncompromising advocate of structuralist explanation or that his theory is a product of conscious adherence to this approach. His major work on the subject, *Le Structuralisme*, was written many years after his theory was well-developed. What is more, the book contains much criticism of the structuralist movement, especially linguistic structuralism. Second: I argued in my paper that Piagetian cognitive structures are at most descriptions of behavior and, frequently, are only task descriptions. If this argument is valid, then in those instances when they can be mapped onto behavior, Piaget's "general cognitive structures" are superfluous descriptions, and when they can be mapped only onto tasks, they are irrelevant. Third: The structuralist mode of explanation obviously assumes the correctness of the "description equals explanation" hypothesis. The emphasis is entirely on describing common attributes of tasks and behaviors. No attempt is made to say where the behaviors themselves come from. Although this approach might be justified if the "description equals explanation" hypothesis were correct, we have seen that it probably is not.

The fourth reply is, for me, the most important. I think it not unlikely that "structuralist explanation" is an ex post facto attempt to rationalize bad theory and thereby avoid having to change it. (Certainly, there can be no doubt in the case of Piaget's theory that the theory came before the rationalization, or that the theory resists change.) This seems a poor strategy. It is most improbable that such explanations will be regarded as satisfactory by working scientists. Developmentalists, for example, want to know *why* behavior develops as it does and when it does. It is unlikely that they will be satisfied by structural descriptions of behavior, no matter how abstract or opaque. Consider a randomly chosen example. It is known that during the elementary school years children become progressively more likely to solve discrimination-learning problems on the basis of abstract attributes (dimensions) and progressively less likely to solve them on the basis of perceptible attributes (values of dimensions). In the present state of our knowledge, this fact seems to have important implications for both cognitive development and instruction. What we should like to know is why this trend exists. Is it a consequence of advances in verbal mediation, as Kendler and Kendler (1962) maintain? Is it a consequence of improvements in selective attention, as Zeaman and House (1963) maintain? Is it a consequence of improvements in hypothesis testing skills, as Levine (1975) maintains? Clearly, we want to know the antecedent variables responsible for children's increasing reliance on abstract attributes in discrimination learning. Judging from the many advances in our knowledge of children's learning that have accrued from research prompted by the desire to isolate antecedent variables, this desire generates productive lines of research that eventuate in theoretical hypotheses apparently providing satisfactory answers to "why."

Other stage criteria

Although Piaget's major stage criteria were examined in my paper, it might be that other valid criteria could be given. That is, there might be verifiable properties of cognitive development, other than the ones I considered, that do call for a stage viewpoint. Some additional criteria are described by certain commentators, but I do not see too much promise in these; they tend to be either nonempirical or contradicted by extant data. **Fischer**'s suggestion that qualitative change and "cognitive ceilings" imply stages may be used as a case in point. The idea of qualitative change is a notoriously slippery one. No one seems to be able to say what a qualitative behavioral change is. When specific examples are put forward, they invariably pose the "how much more is different" dilemma. I have discussed this dilemma at length elsewhere (Brainerd, 1978b). It is concerned with the fact that there are no rules for deciding how big a change must be before it equals a change in kind rather than amount. Concerning "cognitive ceilings," this is the old readiness-to-learn doctrine in new language. The main idea is that we cannot teach children concepts that are substantially beyond their current cognitive stage. In the case of Piaget's theory, however, there is an extensive data base on the learning of concrete-operational concepts that, to say the least, does not tend to confirm this idea. Children's ability to learn conservation concepts, for example, has not been found to depend on their pretraining stage classifications (Brainerd, 1977a, Table 1). Further, conservation learning has been demonstrated with children far below the nominal age range for the concrete-operational stage (Brainerd, 1977a, pp. 931–34).

There is one criterion, however, that is often associated with stages and about which I am extremely optimistic, namely, the idea that developmental changes in conceptual behavior occur in an abrupt, discontinuous manner. Since the appearance of two influential papers by Flavell (1971, *op. cit.;* Flavell & Wohlwill, 1969, *op. cit.*), it has become fashionable to dismiss the possibility of discontinuous change and to assert that cognitive development is a smooth, continuous process. In the earlier of these papers, for example, Flavell and Wohlwill conclude that "The recent Piagetian literature strongly suggests that development is normally gradual rather than abrupt . . . The conservation concepts, to take everyone's favorite example of a cognitive form, seem to show a rather extended interval between first-in-competence and always-in-performance. The process of stabilization and generalization of these and perhaps all competence items appears to be a relatively slow one" (p. 79). A reiteration of this conclusion may be found in Fischer's commentary.

There are two remarkable things about the widespread acceptance of the hypothesis that concept development is a gradual process. First, contrary to the above quotation, there is no sound empirical basis for the hypothesis. There have been no attempts to test models that assume discontinuous acquisition of concepts against models that assume continuous acquisition, although Flavell and Wohlwill (1969, *op. cit.*) have proposed a model of the latter sort, resembling the linear difference equation of stimulus sampling theory. The evidence mentioned in the quotation shows that it requires several years to acquire most of the concepts associated with Piaget's concrete-operational stage. But this could simply be a case, similar to familiar examples from physics and biology, where large-scale continuity masks small-scale discontinuity; the macropicture may be the result of strictly discontinous changes at the level of individual concepts. Large-scale continuity is *not* inconsistent with the view that individual concepts are acquired in an all-or-none manner. Second, a technology, namely, Markov models, already exists for putting the gradual change hypothesis to the test. Twenty years ago, learning theorists were confronted with a very similar question. They wanted to know whether college students learned paired-associate items, affirmation rules, and so on, gradually or in a sudden, all-or-none manner. Finite Markov models were vigorously developed to allow this question to be investigated. It is not difficult to apply these models to the question of whether or not children acquire concepts gradually. The only assumption that needs to be made is that concept acquisition can be equated with changes in the probability of a correct response on some test. An elaborate mathematical development and theoretical rationale for the applicability of Markov models to the question at hand can be found in Brainerd (1979).

Piaget often speaks of the acquisition of specific concrete-operational concepts as consisting of three discrete stages. To illustrate, development in the classificatory, relational, and number areas is described as a three-stage process (Beth & Piaget, 1966). For any given concept (e.g., class-inclusion, seriation, number conservation), the Stage I probability of a correct response on the appropriate concept test is zero. During Stage III, the probability of a correct response on the same test is one. During Stage II, the probability of a correct response is somewhere between zero and one. Let $S1$, $S2$, and $S3$ denote the three stages. Let $S2_E$ denote an error on the appropriate concept test while the subject is in Stage II, and let $S2_C$ denote a correct response on the test while the subject is in Stage III. (Recall that only errors and corrects occur in Stage I and Stage II, respectively.) Now, suppose that, for any given concept test, these three stages are not arbitrary slices of a continuous stream. Explicitly, suppose that (a) subjects can occupy only these three performance states, (b) the probability of a correct response in State $S2$ is some *constant* p, and (c) transitions between states occur in an abrupt, all-or-nothing manner. These assumptions imply that changes in the probability of a correct response on the test in question can be described by a three-state Markov process whose starting vector and transition matrix are:

$$P\{S3(1), S2_E(1), S2_C(1), S1(1)\} = \{t, (1-s-t)r, (1-s-t)(1-r), s\};$$

$$\begin{array}{l} \\ S3(n) \\ S2\ (n) \\ S2\ (n) \\ S1(n) \end{array} \begin{array}{c} \begin{array}{cccc} S3(n+1) & S2_E(n+1) & S2_C(n+1) & S1(n+1) \end{array} \\ \left\{ \begin{array}{cccc} 1 & 0 & 0 & 0 \\ d & (1-d)(1-p) & (1-d)p & 0 \\ c & (1-c)(1-p) & (1-c)p & 0 \\ ab & a(1-b)(1-p) & a(1-b)p & \end{array} \right\} \end{array}$$

All the parameters of equation 3 are probabilities. The starting vector gives the probability of beginning in each of the possible states of the process for some subject population. The values in the transition matrix are conditional probabilities. Each cell gives the probability that the process is in some state S_j on trial $n+1$ given that it was in some state S_i on trial n. The model in equation 3 turns out to be quite mathematically tractable. It is relatively easy to derive numerical predictions that can be used to test the model's fit to performance data (Greeno, 1968). It is also possible to derive interesting theoretical hypotheses about relationships between parameters in the transition matrix (Brainerd, 1979).

Equation 3 has a total of eight free parameters that need to be estimated before the model's fit to a set of performance data can be assessed. Since models analogous to equation 3 have proved to be important in many simple learning situations (e.g., paired-associate learning, discrimination reversal, classical conditioning), much work has been done on parameter estimation schemes. Procedures for obtaining maximum likelihood estimates of the parameters of equation 3 may be found in Greeno (1968), Halff (1976), and Brainerd (1979).

Since parameter estimation is not problematical, equation 3 can easily be applied to a set of performance data to decide whether or not those data can be described by three discrete states. The model is perhaps most readily applicable to the data of training experiments and longitudinal studies. Suppose we are training children on some concrete-operational concept (for example, conservation), and we train them to a fairly strict criterion of learning (for example, eight to ten correct responses in a row). We are now in a position to determine whether or not the change in correct response probability as a function of training consists of three discrete stages. The training trials data are used to estimate the parameters of equation 3. These parameter estimates are then used to compute values of observable statistics of training trials data such as total errors to criterion, trial number of last error, number of errors before the first correct response, number of errorless protocols, and so on. Finally, standard goodness-of-tests (χ^2 or Kolmogorov-Smirnov) can be used to decide whether or not the predicted values of these stastistics differ significantly from the observed values. Much the same procedure can be used when applying equation 3 to longitudinal data. About the only change in the usual longitudinal design that seems to be required is that subjects should be followed until they have met a strict criterion of concept acquisition (for example, one testing session without any errors).

I have already reported some preliminary findings suggesting that the process of learning a conservation concept consists of three discrete stages (Brainerd, 1979, Table 1), and more extensive data will be reported in the near future. However, the main point is that by virtue of the existence of a well-articulated literature on Markov models, developmental researchers can begin to come to grips with the question of gradual versus abrupt change. Since these models are readily adapted to certain kinds of developmental data, especially the data of learning experiments and longitudinal studies, all that remains is to run the experiments and fit the models. I believe, therefore, that there is reason to be optimistic about our prospects for determining whether or not children's acquisition of concepts is normally a stage-like process. The chances are good that this question will be resolved during the next few years. I find this an exciting prospect.

EDITOR'S NOTE

Asterisks indicate that these commentaries will appear in the Continuing Commentary section of a later issue.

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