This commentary on Charles J. Brainard's "The Stage Question in Cognitive Developmental Theory" will appear together with the article in the first issue of The Brain and Behavioral Sciences.

"Measurement Sequences," Piagetian Structures, and Higher-Order Rules
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Let me say first off, that I have worked with Brainard 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 40 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 Brainard's present critique. (Nonetheless, the printed version is far more to my liking than the original manuscript submitted by Brainard and Siegler.)

The general thrust of Brainard'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.

Brainard (and presumably Siegler), however, argue (essentially) that the tasks associated with Piaget's preliminary stages are logical prerequisites of those (tasks) associated with subsequent stages. Brainard calls these "measurement sequences," which occur "whenever each item in the sequence consists of the immediately preceding item plus some additional things (p. 14)."

To illustrate the notion of a measurement sequence Brainard states, "To multiply, children must know how to add" because "multiplication is defined in terms of addition (p. 15)." 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 successfully be taught how to multiply before knowing anything about addition. The product 3×2, for example, is simply the number of pairs in the two-dimensional array

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or 6. Notice that all one has to do is to count and that 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 Brainard's contentions. For example,

\[
\frac{d}{dx} \sin x = \cos x
\]

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

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 assumes implicitly 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 only be achieved 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 which underlie them. There are any number of different ways (structures/processes) by which a given class of tasks might be solved (e.g., see 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 the published paper (possibly in an attempt to circumvent earlier criticisms), Brainerd emphasizes that he is not suggesting that all behavior associated with the various Piagetian stages are related by means of measurement sequences—only 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 operationalized his stages then, of course, we probably all would 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, 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 Northern American empiricism and Genevan structuralism. Clearly, Piaget intends for the structures associated with his various stages to be hierarchically related. The behaviors which 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 (e.g., Scandura, 1977). In short, Brainerd's measurement sequences are a myth. Such sequences cannot be defined independently of the underlying structure/processes which 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 (p. 27)."

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 "x" 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 "x" 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 also could succeed on the task by applying the following rule: Compare "x" with the first seriated stick. If "x" is shorter, put "x" before the stick and stop. If "x" is longer, compare "x" to the next stick and test "x" 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.*

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 structures 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. Piaget to my knowledge has not done this and this is an important limitation which Brainerd has reemphasized.**

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*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.

**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, contemporary 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 and 1977, especially Chapters 1, 5, 7, 10, and 11.)
also am 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 his 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 (e.g., Scandura, 1971, 1973, 1977) may be especially useful in this regard. The notion of higher-order rules, or rules which 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.