

# We Are Not Testing Epstein's Ideas: A Response to Richard McQueen

MARTIN G. BROOKS

**B**rain growth periodization, and its potential application to educational settings, provides rich material for debate. If Epstein's periodization theory is accurate, the ramifications for education may be significant. If, as McQueen asserts, Epstein's theory is faulty, districts would be well advised to resist the temptation to base curriculum and operational decisions on it.

This debate, however interesting it may be, is speculative at this point because there are no hard data in the

educational arena that either definitely support or refute Epstein's theory, and there are no measures or research methods that can test it. For these reasons, much of the criticism leveled at Epstein is directed at his research and data collection methodologies, not his ideas.

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We presented Epstein's ideas in our article, "Cognitive Levels Matching" (May 1983) because, if accurate, they lend biological support to the observations and theories of Piaget, on whose work the CLM project is based. The Cognitive Levels Matching project, however, does not test Epstein's ideas. The thrust of our project is the process of cognitive development and how we, as educators, may facilitate this process through carefully planned interventions with our students. □

## Brain Growth and Cognitive Development: A Response to Richard McQueen

The Cognitive Levels Matching Project is entirely independent of brain studies. However, brain growth stages paralleling mental growth stages is an experimentally demonstrable fact.

HERMAN EPSTEIN

**I**am grateful for this opportunity to comment on an article that is so critical of some aspects of my analyses of brain development. The article itself is mainly irrelevant, partly wrong, and, in a small part, correct.

Most of what McQueen says is in the realm of anecdotes based on unpublished communications with a number of individuals. Such material should not

be published since it is hearsay and there is no way for anyone to verify that such comments were made, let alone intended for use in the given context. Nor is an educational journal the place for discussing brain data. While the

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education community may need to hear this criticism, I restrict my comments to items that relate to verifiable information.

McQueen's remarks about my use of the Shuttleworth data show the kind of erroneous thinking about which I always warn when discussing those data. It is inferred that just because there is differential brain growth at some age,

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there must necessarily be a differential development of some particular brain function. I have always taken care to point out that the gender difference in brain growth around age 11 is *not* paralleled by what is found in the mental age data. Any proposed connection between brain and brain function is necessarily only a working hypothesis that has to be tested.

Gould's comment astonished me when I first heard about it. It turns out that my remark about head size and intelligence could be misread to mean that I thought there was evidence for a real connection. All I intended to relate was my surprise that the only reference I had found seemed to indicate that there was some connection. I don't know nor do I care whether there is or isn't a connection. Gould went on to draw some erroneous inferences about my orientation on matters related to the Jensen and Burt episodes. This is entirely unwarranted because I have many times indicated publicly my reasons for asserting that experimental data show that Jensen was wrong. On the other hand, it is irrelevant to the *science* aspect of Burt's claims that Burt has been proven to be a fraud because almost all other studies obtained results very similar to those Burt was pushing. I hope soon to have a public retraction by Gould.

The Nellhaus data that I used were furnished by Nellhaus. He apparently had wondered about the variability in the data in his first published paper, and *he* was the one to separate the data into those from individuals who could be presumed to have developed in social groups having good prenatal and postnatal maternal care and those having poor care. I used only the data from the good care situations because they could be presumed to indicate what happens normally.

The reference to Hahn and others' shows only that the writer is not competent to judge such matters. I phoned Hahn for his wishes when I was asked to referee the paper; he felt quite comfortable with my doing that. I recommended publishing the paper, but added that Hahn's interpretation was not unique and that there were possible implications of great importance. I have been utilizing the Hahn data to design a completely new line of my research on mouse brain development.

I cannot resist stating that in my only meeting with Piaget he said it was interesting that there might be a biological basis for his stages but that he was too tired intellectually to think about such a notion. He suggested I go to his institute to talk those things over with Inhelder. Unfortunately, Inhelder was absent from Geneva and I never met with her or discussed these matters with her.

About a year before first being contacted by McQueen, I decided to analyze our Cognitive Levels Matching project to learn what aspects of brain studies were most relevant to which parts of the CLM program. It was then—to my astonishment—that I became explicitly aware that the CLM program was logically entirely independent of the brain studies, even though I had personally been led to formulate CLM as an extension of the studies of brain and learning development. I should have realized this before because I knew that Shaver and associates had independently proposed such an approach well before they had even heard about the brain data. As I have explained to McQueen, this program *in no way* depends on the validity of the brain analyses themselves. Thus, the article's irrelevancy extends also to implications about our work on Cognitive Level Matching programs for training teachers.

This is not to be taken as indicating that I think there is anything wrong with my brain study analyses: I do not think so. On the contrary, the original (1974) correlation-based *suspicion* that there are brain growth stages paralleling mental growth stages has grown, in my opinion, into an experimentally demonstrable fact. Moreover, I continue to be on the lookout for additional data on brain and skull growth, and the few additional studies which I have found continue to make me sure that the correlation holds. Of course, we take pains to inform teachers that the *logical roots of CLM* are in the work in educational psychology so that brain studies now serve only to *illuminate* the understandings and strategies developed for CLM.

It is also worth stressing that the apparent tie to Piaget is only that: apparent. Any *empirical* characterization of children's reasoning development would do as well—if there were others. This disavowal of necessary dependence on Piaget is not at all meant to imply that I think there is something seriously wrong with the Piagetian descriptors. But, my reading of “anti-Piaget” writings has led me to agree that there really are serious objections to some aspects of Piaget's two theories: concepts such as assimilation, accommodation, and equilibration; and his logico-mathematical theory of what underlies the various reasoning abilities. But, there is rather general agreement that, cross-culturally, children grow through the Piagetian sequence of schemata. Although some of my associates, including many of those who are involved in the Shoreham/Wading River project, continue to use those Piagetian theories, in my opinion *there is nothing in my version of CLM that requires any more than the experimentally found and validated sequence of schemata.*

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Our experimentation in Shoreham has steadily improved the first course in which we show teachers how to "see" the cognitive levels of children in their many actions, including the kinds of questions they ask, the kinds of jokes they understand and those they don't



**“Most Shoreham teachers have already been compelled by the logic of CLM to alter some of the content of their courses to fit more closely the predominant cognitive levels of children in their classes.”**

understand, and so on. Then we show the teachers how to get an idea of the cognitive level demand of the various instructional inputs (verbal, written, and pictorial). Teachers can then see how to match input to receptive competence.

The follow-up course has been the subject of not a little debate and experimentation. The current version is a hands-on course in which we work with teachers in the classroom to show how to recognize opportunities for CLM and then how to do the matching. Thus, we

have not yet begun a systematic revision of curricula, although most teachers have already been compelled by the logic of CLM to alter some of the content of their courses to fit more closely the predominant cognitive levels of children in their classes.

The evaluation of this work at Shoreham has been begun by obtaining the services of a group of outside researcher-evaluators. My judgment is that, because of the extended period—now about four years—during which we have worked at Shoreham, we are in a poor position for evaluation because even the few remaining “control” teachers have been heavily contaminated by hearing many discussions of our program. Thus, the design of the evaluation has of necessity been of a less-than-optimal nature. For that reason, I have been looking for another school district in which to replicate the whole program under conditions optimal for evaluation. The start of this work was carried out last year, and we hope this year to go on to course II. Perhaps in another year we will learn if our particular version of CLM has been successful. □



Suzan Stern

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