REFERENCES


ON THE VALUE OF STATUS STUDIES IN EDUCATION

JOSEPH M. SCANDURA

*University of Pennsylvania*

A number of questions arose when one of my doctoral students reported on “The International Study of Achievement in Mathematics” in our research seminar. In light of the concerns expressed there and in thinking about the study, I feel that a few remarks might be helpful in putting this type of research into better perspective.

A question we all too rarely ask of research, in my opinion, is: Of what value might the research be? In asking this question, I do not mean to imply that all research should have direct and immediate practical implications for society. There are too many counterexamples to make this possibility worth exploring.

But, I do think that in education—nay, behavioral research generally—too little thought has been given to this question. This is a question I would like to pose of the International Study of Achievement in Mathematics. For what could the obtained results be used?

As a researcher, my first thought was that perhaps in some way the research might be useful as a survey device of sorts to identify crucial problems in need of additional clarification. On further thought, however, it becomes clear that the kinds of problems identified by the study do not, for the most part, pertain to basic questions of learning and teaching mathematics, and only very indirectly to curriculum development or teacher education. Far too many variables were (necessarily) confounded in the study to clarify much of anything. To be sure, in almost any research effort of this kind, we can safely ignore the results of the study completely and be no worse off (and possibly even better off). If
we wanted to develop a new curriculum, for example, we might better simply study a wide variety of available curricula and other materials as a source of ideas than try to make sense out of the ambiguous results of the international study. With respect to research on mathematics learning and teaching, I can say even more forcefully, with confidence, that if getting ideas for research were the only reason for conducting the study it would hardly have been worth the effort.

This does not mean that the results are of no scholarly value whatsoever. I would not go that far. But, I do think that their value insofar as promoting new research, except for the same general sort of "status studies," is severely limited. One possibility is that the research might provide an interesting source of information for scholars interested in comparative education in mathematics or, for sociologists and other social scientists interested in the relationship of mathematics to various cultures. Unfortunately, however, there do not seem to be very many such scholars around.

Surely, there must be a more important justification for spending the large sums of money required to conduct the study—and, I think there are, or at least were, at the time the study was conducted.

At that time, there were many important and far-reaching decisions being made concerning mathematics education, both in and out of government, regarding the more pressing needs in the field and the most promising ways of meeting these needs. Much money, for example, was going into teacher education mainly for specific training in mathematics. Should more money be spent in this area? Should the same format be used? How is this training affecting what students actually learn in the classroom? These were just some of the questions to which answers were needed by decision makers. Survey research was the obvious way to get such answers.

When looked at in this way, the basic value of the study under review becomes reasonably clear. Its main purpose was not to provide leads for further research, not to provide basic information about mathematics education, not to determine what should be taught, or when, or how, and, particularly, not to add to our store of fundamental knowledge about how mathematics is learned.

The main purpose—or perhaps I should say justification—for the research was to provide administrators with a basis for decision making. Its value, then, must be judged not in a scholarly realm, but rather in terms of the savings or other benefits which have accrued as a result of the obtained information being in the hands of those who make decisions. The critical question, then, one which I hope some of the other reviewers will touch on, is whether the findings of the study have led to wise decisions. Other reviewers, I'm sure, will deal with more technical questions of design and whether the conclusions drawn from the study are adequately supported by the data in the first place.

That, then, is the sole and simple purpose for my review: to help clarify the aims and potential value of status studies, such as the one under review, which deal primarily with so-called administrative variables. The question that each and everyone of us should ask with respect to such research is: What do the results say to us insofar as decision making is concerned? If they provide useful information, information which leads to wise decisions, it may be reasonable to justify the expenditure of the large sums usually required. If they do not, can we justify funding them at all?

THE COMPLEXITIES IN THE LEARNING OF MATHEMATICS

WILLIAM E. LAMON

University of California, Santa Barbara

A few years ago, the academic community around the world focused its attention on an immense international attempt to study on the one hand the differences among the school systems of twelve countries and, on the other, their effect on achievement, interests, and attitudes of certain groups of school-attending youngsters. Those objectives and their findings were reported through many publications, most notable in International Study of Achievement in Mathematics (Husén, 1967). These publications have motivated me to raise some serious questions.

First, the findings reported on the 13-year-old population and their achievements reflected by observations, such as

"13-year-old Japanese children of unskilled workers have higher test scores than 13-year-old American children of college-trained professional workers"

"13-year-olds perform better in larger schools, and those entering late in school do poorer"

stimulated some critical thoughts on the topic.

A second, there is no doubt in this writer's mind that such conclusions challenge to scholars in the twelve participating countries to reflect some of their educational practices. Certainly, it encourages edu-