NEEDS AND PRIORITIES FOR RESEARCH IN EDUCATION*

Robert L. Ebel/Michigan State University

A short while ago the Vice President for Research at Michigan State University sent a memo to the Dean of the College of Education. "What research," he asked, "are you doing to shed some light on reasons for the decline in college admissions tests scores?" The Dean turned to me for help. I'm afraid that I gave him very little. I couldn't point to any research in progress that bore more than a very tangential relation to the question. I couldn't even suggest any feasible studies that we ought to undertake. For the decline seems to me to have an obvious explanation in the modes of instruction which have been popular and prevalent in recent years. Such data related to alternative explanations as is readily obtainable has already been obtained.

The Vice President for Research is a very intelligent and well-informed biological scientist. I doubt very much that he could believe that scientific research holds the key to solving operational educational problems like that of declining test scores. My hunch is that he was simply trying to nudge our college toward doing something to reverse the decline; toward production of more cognitively oriented, cognitively capable, and cognitively productive teachers. But that may be mere wishful thinking on my part. I am a firm believer in the power of knowledge and rational thought as solutions to problems of teaching and learning, though I know some educators who claim that love in the classroom is all that is required.

Back in 1945, as part of a Ph.D comprehensive examination in educational psychology, I was invited to discuss the so-called "Scientific Movement" in education. Though I suspected that the framer of the question was no friend of testing (my specialty) or of educational research, I was no smarter then than I am now in avoiding provocative pronouncements. At the risk of failing (I did pass), I told him that a science of education was the hope of the future.

Now, thirty years later, I'm not so sure. Like a host of others, I have been disappointed in what educational research seems to have accomplished. I think I know why that hope of the future is fading, and what can be done to resurrect it. To present these insights, or opinions, for your thoughtful consideration is the purpose of this paper.

First, a bit of history. In 1857, when the National Educational Association was formed, educators were beginning to see the need for better information about schools and schooling—enrollments, costs, curricula, and so on. Toward the end of that century, Joseph M. Rice began to use tests to collect data on achievements in spelling. The need for data, and the possibility of getting some easily via the newly developed objective tests and survey techniques, led to the establishment of research bureaus in many of the larger cities. Directors of these bureaus took to meeting at the annual NEA conventions and shortly they established an American Educational Research Association, mainly to facilitate communication among themselves on their common problems.

But research suggests science. Before long educators began to dream of a science of education that would solve some of their problems and show them how the job really ought to be done. Science involves theory and hypotheses to be tested. In a field where measurements tend to be imprecise, and where influential variables tend to be numerous and complexly related, statistics are required—inferential statistics, not just descriptive statistics. During the first three quarters of this century, statistical methods have been developed rapidly, and elaborated almost incomprehensibly, to meet the apparent needs.

In the 1960's the appropriation of substantial funds from the federal government gave educational research a tremendous boost. A myriad of projects were proposed, funded, carried out, and reported, often in ponderous tomes. The training of research workers, generously funded, also accelerated. Regional and specialized centers for research on educational problems were set up.

Then in the 1970's funds for these activities began to run out. Governments are less affluent now. They are also less persuaded that research on education is a good investment. A report to Congress from the General Accounting Office claimed that more than $200 million in educational research had produced "little evidence of significant impact in classrooms."[1]

There are, of course, other, more positive views of the value of educational research. Here are several examples. From Nicholas Fattu, "—only by inspired, sustained, and systematic research in education similar to that which has graced the other sciences can education become truly effective."[2] From Julian C. Stanley, "If we are to advance beyond the dark ages of educational pre-science, we must emulate the experimental proficiency and zeal of colleagues in other behavioral sciences."[3] From Lindley J. Stiles, "The promise of excellence in education rests on the willingness of the nation to support a comprehensive program of educational research and development to improve schools."[4]

Where, in 1977, is there evidence of that willingness? Is it that our fellow citizens are blind to our achievements and our potential? Or is it that we have had our turn at bat and struck out? Even among educators there is doubt about or outright opposition to educational research. Too many ill-conceived and poorly designed studies have been conducted. Too many questionable conclusions have been reported. In too many cases the major finding is that more research on the problem is needed. In the words of Andrew Halpin, "As I watch the shenanigans that take place in our colleges of education (in the name of educational research), I am continually amazed at our capacity for self-deception."[5]

Listen to the assessment of C. D. Hardie, an Australian educational philosopher. "—(research in education) is taken seriously mainly by those who are engaged in it."[6] Hear what Tom Lamke, research specialist at the University of Northern Iowa, had to say. "—if the research in the previous three years in medicine, agriculture, physics, and chemistry were to be wiped out, our life would be changed materially, but if research in the area of teacher personnel in the same three years were to vanish, educators and education would continue much as usual."[7] One is entitled to wonder how long it will take a tidal wave of evidence on the futility of much research in education to sweep the notions of its potency and productivity back into the sands from which they were built.

Before proceeding further with an examination of these contrasting views of the value of educational research, perhaps I should try to make sure that you and I are thinking about the same thing when we speak of educational research. Some advocates of research in education define it so broadly that it encompasses any thought or expression of any ideas on any aspect of education. Now I am not opposed to thinking about educational problems. If the ideas are expressed cogently and gracefully I welcome writing about educational problems. But when I speak of research in education I have something a bit narrower and more specific in mind. My definition can be expressed in these words: competent, careful investigation of some aspect of education aimed at the discovery and interpretation of facts.

The definition can be made more explicit by identifying three distinct types of educational research: scientific research, operational research, and summary research. Scientific research has as its aim the understanding of natural phenomena via discovery of the laws of nature. It is sometimes called basic research. Lee Cronbach referred to it as conclusion-oriented research.[8] Operational research has as its aim the improvement of a process so as to obtain the maximum effect from the available resources. It is sometimes called applied research. Lee Cronbach referred to it as decision-oriented research. Summary research has as its aim the collection, assessment and integration of data from diverse studies of a particular educational problem. It could be called library research. *Articles in the Review of Educational Research*, especially since 1969, in the *Encyclopedia of Educational Research*, and in similar sources exemplify the results of summary research.

In the eyes of some, the only educational research worthy of the name is the scientific or basic variety. Others, of whom I am one, point to the obvious failure of efforts to discover or develop a scientific structure of concepts, laws, and theories in education. They, and I, believe that such efforts are bound to fail because what educational research scientists have set out to discover about instruction simply isn't there to be discovered.

Scientific investigation is a process of obtaining information about natural phenomena. Classroom instruction is not a natural phenomenon. It is a human enterprise. Natural phenomena are stable. The solar system today is the same as it was when Ptolemy and Kepler and Copernicus studied it. The behavior of a pendulum is the same today as it was in the days of Archimedes. The fumes of burning sulfur are as noxious to us as they were to the ancient alchemists.

Classroom instruction is not like that. Its procedures are not fixed eternally. We can make them whatever we choose to make them, whatever works in our particular situation. Which procedures will work best depends on the students, the teacher, the subject, and the fashion of the times. To attempt to discover laws which regulate or determine outcomes of such a free and undeterministic operation is to undertake a virtually impossible task.

Fundamentally, of course, a science of the biology of learning must be possible. Remembering, thinking, and learning must surely involve physics, chemistry, and biology. These fundamentals may someday be understood better than they are today as a result of basic scientific research. But what reasons have we to believe that any scientific discoveries in that realm will have more than the slightest impact on how classroom teachers go about trying to facilitate learning?

Effective teaching is an art than can, within limits, be learned through creative problem solving, and by copying the methods of other effective teachers. Teaching is no more a science than writing good poetry can be a science, or beating Notre Dame in football, or presiding successfully, as Jimmy Carter hopes to do, over our national government. Of course classroom instruction can be studied scientifically, but the result is almost certain to be mainly of local and ephemeral interest. It is almost certain *not* to be a *science* of instruction. If we think that the methods of science which have yielded such amazing understandings of natural phenomena will be equally productive in solving the problems of effective teaching, we are badly mistaken. Effective teaching is not a natural phenomena to be understood. It is an undertaking that has been devised by man using his creative imagination to suit his purposes. It needs development and perfection, not scientific investigation.

At this point let me pause to acknowledge that some of you may be having difficulty in accepting the conclusions I have reached about the productivity of basic research in education. Like myself, you may have been indoctrinated with the belief that science was the key to progress, and that basic science was the most powerful form of science. If you were thoroughly indoctrinated with that belief, as I once was, it may be that I have now undertaken a difficult, if not impossible task—the task of reasoning you out of a belief you were never reasoned into in the first place.

When I first published views like these in the *Phi Delta Kappan* in 1969, I was conscious of expressing a minority opinion. This predicament has never bothered me as much as it probably should have. Nonetheless, I welcome all the support I can get. I have been encouraged recently to read others who have expressed similar views. There are Halpin and Lamke whose words were quoted earlier. In the excellent AERA sponsored book of readings, edited by Broudy, Ennis and Krimerman,[10] there are supportive articles by J. A. Easley, Jr.,[11] C. D. Hardie,[6] Hugh G. Petrie,[12] and C. L. Stevenson.[13] Here is a quotation from Easley. "In education, we have now had a half century of so-called scientific research. Yet almost everyone I discuss this research with is convinced that the results are not only vulnerable, but mostly useless, or even quite misleading in relation to recent educational innovations." Here is another from Hardie. "Research in education, if interpreted in the usual sense of seeking knowledge with which to understand our experience, has been quite unsuccessful, and there is no reason to suppose that it will be successful in the forseeable future."

Those committed to the power and ultimate success of basic research in education have several explanations for its current shortcomings. It is a relatively new science they say. But why should it be new? The problems of effective teaching have been present and fully recognized for as long as the problem of understanding planetary motions or chemical changes. Another explanation is that research has not been funded generously enough. True, we seldom have access to all the money we could use. But the relatively generous funding of the 1960's brought no notable successes.

A third explanation is that the research has been too fragmentary, done on a scale far too small to be effective. Perhaps so, yet the foundations for the physical sciences were laid on the results of fragmentary, small scale, individual investigations. A fourth explanation is that educational research workers as a whole tend to be poorly trained and inept. I deny that. The programs for training graduate students in research are the most thorough and demanding of any in our college. I suspect this is true also of other colleges of education. And they tend to attract the most capable students.

I think something more fundamental is wrong. It is the basic inappropriateness of scientific research methods to the design and improvement of classroom instructional procedures. And it is improved instruction, leading to more effective learning, that is our most urgent need.

What, then, can account for our persistent faith in basic, scientific educational research? Part of it may be our pure, rather blind, faith, resulting from the general halo that Science, with a capital S, wears. Part of it may result from our earlier indoctrination concerning the supposed universal omnipotence of scientific research. Another part may be the satisfaction we enjoy in using the experimental and analytic tools which we learned how to use, at some cost, as graduate students. This exemplifies Kaplan's Law of the Hammer, which states, "Give a small boy a hammer, and he will proceed to find things that need hammering." An important part may be the financial rewards. So long as government agencies or private foundations are willing to support basic scientific research projects, researchers will continue to submit proposals to be funded, persuading themselves, and others, that a vital educational need is being met.

I have no objection to scientific educational research as an avocation, or even as a vocation, so long as someone with the necessary financial resources is willing to foot the bill. Efforts to satisfy intellectual curiosity, which is often cited as the primary justification of basic scientific research, can be absorbing and often personally rewarding. But we should be wary of claims that such research has much to contribute toward the improvement of instruction. We should oppose the diversion of any significant portion of our educational resources into such an unproductive channel.

That leaves us, if you agree, with operational research and summary research. Both of these seem to me likely to be much more fruitful than basic, scientific research. In George Mouly's list of 21 significant educational research studies,[15] including the work of Binet, of Tyler, of Hartshorn and May, of Morphett and Washburne, of Starch and Elliott, and of Wichman, all were essentially operational research studies.

It may not surprise you to know that most of my own modest research efforts have been concerned more with operations than with theories. Many of these studies have been directed toward improving the operations of measuring educational achievement. Here are a few examples:

Some effects of irrelevant data in physics test problems (1937).

The use of item response times in achievement test construction (1947).

Estimation of the reliability of ratings (1951).

Maximizing test validity in fixed time limits (1953).

Characteristics and usefulness of rate scores on college aptitude tests (1954).

The effect of varying the number of alternatives per item on multiple-choice vocabulary test items (1957).

The relation of item discrimination to test reliability (1967).

Can teachers write good true-false test items (1975)?

Those who are faced with the problem of making educational decisions, and that includes all of us who are involved in teaching, or in directing the educational enterprise, can benefit from operational research. And many of us can conduct it too! Classroom teachers can examine the effectiveness of different instructional strategies they have devised or learned about. Doctoral students can collect and interpret data from experiments or from surveys on a broader scale. Research bureaus in school systems, universities, or state departments of education can gather information relative to decisions that must be made. Note that the focus of these operational research studies is not on discovery of some eternal Truth, but on the resolution of some immediate, and probably temporary, practical problem.

One other point of considerable importance—we err greatly if we think that research itself, even operational research, will make decisions for anyone automatically. The best it can do is to improve the basis for decision making. Consider the judgments that juries, councils, legislatures, boards, and committees must make. Consider how much evidence must be gathered, weighed, tested, and integrated. Consider how many principles, guidelines, and limiting conditions must be kept in mind, and in perspective. Consider how many possible consequences must be assessed for probability of occurrence, and potential for good or harm. Then consider how little even the best research can do to solve any of these problems for us. Good decisions are a consequence of well informed, deliberate judgment, whether made by an individual or a group. Further, many of the necessary ingredients for wise decision making are not specific to the particular problem that needs to be solved. They are a part of the general, liberal education of the decision maker, and of his developed wisdom. The making of good decisions requires at least four things: (1) thorough knowledge of the specifics of the particular problem, (2) broad knowledge of the context in which the problem occurs, (3) a mind trained to synthesize evidence and analyze inferences, and (4) a temperament inclined toward reflective deliberation. No wonder the decisions we make are not always wise. No wonder we turn so eagerly, if somewhat blindly, to research as an appealing substitute for difficult, fallible, human judgment in making decisions.

What of summary research? We need it badly to collate the findings of experimental research and reports of practical experience. Done well and successfully, it produces dependable generalizations that can be useful guides in decision making. It helps to overcome the impotence and tentativeness of fragmentary research studies, which almost all the research undertakings in education inevitably is. It helps to integrate experience with experiment. Each of these has unique virtues. Experience is realistic, practical, and comprehensive. Experiment is planned, focused, and controlled. Neither deserves exclusive rights to our attention and respect. Well-done summary research can help to deliver the results of our efforts in usable form to the decision makers. To do it well requires not only hard work in assembling the evidence, but also perceptiveness and wisdom in summarizing it. Let us continue to support summary research as an essential component of our research efforts.

To conclude and summarize after all these words, what is my perception of needs and priorities for research in education? The list is short, but not, I suspect, either bland or conventional.

1. More emphasis on decision-oriented operational research, and on summary research. Less emphasis on conclusion-oriented scientific research, on attempts to discover natural laws governing learning, instruction, and education generally.

2. Clearer recognition of the limited but essential role of operational research and summary research as adjuncts to sound decision making. Human judgment must make the decisions. Research can help to inform the judge.

3. In training research workers, and educational leaders generally, more concern for breadth of knowledge of education, for soundness of educational philosophy, for general liberal education; less concern for specialized knowledge of experimental designs and sophisticated techniques of statistical analysis.

Several years ago the officers of AERA met with Senator Walter Mondale. We were hoping that he would help us get more money for educational research. He was hoping that we would give him and others in Congress more help in getting good educational laws passed. It was a standoff. Neither side got what it wanted. Both sides probably entertained unrealistic hopes. We hoped for more money than we were likely to get. The Senator hoped for more guidance and support than even the best research is likely to give, for the making o of crucial decisions always involves substantial uncertainties and hazards, even when all relevant data have been made available.

So the hopes were unrealistic. But it seems to me that past support from the research community for constructive educational legislation has indeed fallen short of reasonable expectations. If we can discard the illusion that we are pure scientists, bent on discovering Truth, if we concentrate on decision-oriented operational and summary research, then I believe our enterprise will prosper.

The contemporary world is full of pressing educational problems. Let us dedicate our research efforts to helping educational decision makers solve some of those problems.

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