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The Place of Theory in Educational Research[1]

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In every modern society, the education of its citizens, young and old, is a major concern. In some developing countries, the educational activities of the government consume as much as a third of the national budget. In the United States today, it is estimated that educational activities require at least a hundred billion dollars a year. Most educational activities in this country and elsewhere are like other forms of social and economic activity in society in that only a slight effort is made to study the character of the activities and to understand them as intellectual, economic, or social processes. It is true that there has been a longer tradition, even if a fragile one, of studying the character of education, but I think all members of this Association are very much aware that educational research is a minor activity compared with education as a whole.

All of us probably feel on occasion that there is little hope that educational research, given the small national effort devoted to it, will have any real impact on education as a whole. Such pessimistic thoughts are not historically, I think, supported by the evidence, especially when we look at the evidence outside of education as well as inside. By looking outside education I digress for a moment to examine some instances of the impact of science on society. All of the characteristic features of electronic communication and rapid transportation of our society are unique products of the long tradition of science and technology, and the case is especially strong that the changes that have taken place recently, for example, the widespread introduction of color television, have depended in a direct way on prior scientific research.

It might be useful to mention eight outstanding recent cases that have been studied for the National Science Foundation (Battelle Report, 1973), because the listing of these cases gives a better sense of the diversity of important recent contributions to society arising from specific scientific work. The eight cases all represent developments that almost certainly would never have taken place simply on the basis of either enlightened common sense or some approach of bare empiricism. The eight cases range across a variety of scientific theories and technologies and a variety of segments of society in their applications. They are the heart pacemaker; the development of hybrid grains and the green revolution; electrophotography, which led to office copiers or, as we say in ordinary parlance, Xerox machines; input-output economic analysis developed originally in the thirties by Leontief; organophosphorus insecticides; oral contraceptives, which rest on relatively delicate matters of steroid chemistry; magnetic ferrites, which are widely used in communications equipment and computers; and videotape recorders, which depended upon a confluence of electromagnetic and communication theory and the technology of audio recording. Compared with the impact of some of these scientific and technological developments, the initial cost of research and development has been relatively minor.

As these examples illustrate, research can have an impact in our society, and it certainly does in many different ways. To a large extent, education pays more lip service to research than do other main segments of the society. Every large school system has as part of its central office staff some sort of research unit. The schools and colleges of education associated with institutions of higher education throughout the country are all charged with research responsibilities, some of which are specifically written into the legislative charter of the institution.

When the Office of Education was established by federal legislation more than a hundred years ago in 1867, the first section of the Act defined the chief purpose of the new bureau, later called the Office of Education, as one of "collecting such statistics and facts as shall show the condition and progress of education in the several states and territories, and of diffusing information respecting the organization and management of schools and school systems and methods of teaching." There is not in this charge to the Office of Education a serious thrust of theory, and it is fair to say that most of the efforts of the Office of Education have not been directed toward the nurturing of educational theory, but rather to the more mundane and empirical matters of collecting statistics and facts and of disseminating information about the nation's schools.

The point I am making in leisurely fashion is that for at least a hundred years there has been a serious respect for facts and statistical data about education and also for many empirical studies, often of excellent design and execution, to evaluate the learning of students, the effectiveness of a given method of instruction, and so forth. At least until recently, the empiricism of education has been more enlightened and sophisticated than the empiricism of medicine, which represents an investment comparable to education in our society.

The period running from the beginning of this century to the onset of World War II has sometimes been described as the golden age of empiricism in education. Certainly it was marked by a serious effort to move from a priori dogmas and principles of education to consideration of empirical results and even experimental design of inquiries to test the relative efficiency or power of different approaches to a given part of the curriculum. Detailed analysis of the nature of tests and how to interpret the results was begun, and serious attempts, especially by Edward Thorndike and his collaborators, were made to apply a broad range of results from educational psychology to actual problems of learning in the classroom.

Unfortunately, this golden age of empiricism was replaced not by a deeper theoretical viewpoint toward educational research, but by a noticeable decline of research. To some extent, the overenthusiastic empiricism of the 1920s promoted a negative reaction from teachers, administrators, and parents. Opposition to achievement tests, to standardization, and to too much 'objectivity' in education became rife. A summary of many of the disappointments in the empirical movement in education may be found in the 1938 *Yearbook of the National Society for the Study of Education.* Although in many respects John Dewey can be identified with the development of the empirical tradition, it is important to note that his work and that of his close collaborators is not notable for the sophistication of its scientific aspects; Dewey himself, it can properly be said, continually stood on shifting ground in advocating empirical and innovative attitudes toward teaching. In fact, one does not find in Dewey the emphasis on tough-minded empirical research that one would like, but rather a kind of hortatory expression of conviction in the value of methods of inquiry brought directly to the classroom, and indeed more directly to the classroom than to the scientific study of what was going on in the classroom.

Beginning in the 1950s and especially since Sputnik, we have had a new era of a return to research, and without doubt much valuable work has been done in the last two decades. It is also important to recognize, of course, that much of the thrust for curriculum reform and change in the schools has been bolstered by one form or another of new romanticism untouched by sophisticated consideration of data or facts.

This superficial sketch of the historical developments over the past hundred years leads to the conclusion that research, let alone any theoretically oriented research, has occupied almost always a precarious place in education. It might therefore be thought that the proper theme for a presidential address would be the place of *research* in education and not the more specialized and restricted topic of the place of *theory* in educational research. However, as the examples I have cited from the National Science Foundation study indicate, there is more than meets the eye on the problems of developing an adequate body of theory in educational research, and success in developing such a body of theory can impact significantly on the place of research in education. I would like to turn to this question in more detail as my first point of inquiry.

1. Why Theory?

There are five kinds of argument I would like to examine that can be used to make the case for the relevance of theory to educational research. The first is an argument by analogy, the second is in terms of the reorganization of experience, the third is as a device for recognizing complexity, the fourth is a comparison with Deweyean problem solving, and the fifth concerns the triviality of bare empiricism. I now turn to each of these arguments.

Argument by analogy. The success of theory in the natural sciences is recognized by everyone. More recently, some of the social sciences, especially economics and psychology in certain parts, have begun to achieve considerable theoretical developments. It is argued that the obvious and universally recognized importance of theory in the more mature sciences is strong evidence for the universal generalization that theory is important in all sciences, and consequently, we have an argument by analogy for the importance of theory in educational research.

However, since at least the eleventh century, when Anselem tried to use an argument by analogy to prove the existence of God, there is proper skepticism that an argument by analogy carries much weight. Although the argument that the success of the natural sciences in the use of theory provides an excellent example for educational research, it does not follow that theory must be comparably useful as we move from one subject to the other.

Reorganization of experience. A more important way to think about the role of theory is to attack directly the problem of identifying the need for theory in a subject matter. In all cases where theory has been successful in science I think we can make an excellent argument for the deeper organization of experience the theory has thereby provided. A powerful theory changes our perspective on what is important and what is superficial. Perhaps the most striking example in the history of physics is the law of inertia, which says that a body shall continue uniformly in its direction of motion until acted upon by some external force. Aristotle and other ancient natural philosophers were persuaded that the evidence of experience is clear: A body does *not* continue in motion unless it is acted upon by force. We can all agree that our own broad experience is exactly that of Aristotle's. It was a deep insight and represented a radical reorganization of how to think about the world to recognize that the theory of motion is correctly expressed by laws like that of inertia and seldom by our direct commonsense experience.

A good example in education of the impact of theory on reorganizing our way of thinking about our discipline is the infusion of economic theory that has taken place in the last decade with such vigor and impact. (A good survey is to be found in the two-volume reader edited by Blaug, 1968, 1969.) The attempt, for instance, to develop an economic theory of productivity for our schools can be criticized in many different ways, but it still remains that we have been forced to think anew about the allocation of resources, especially of how we can develop a deeper running theory for the efficient allocation of resources to increase productivity and, at the same time, to develop a better theory for the measurements of input and output and the construction of production functions.

Let me give one example from some of my own discussions with economists, especially with Dean Jamison. Starting from the economists' way of looking at output, it is natural to ask how we can measure the output of an elementary school, for example. What I find striking is the lack of previous discussion of this problem in the literature of education. (Exceptions are Page, 1972, and Page & Breen, 1973.) Even if we restrict ourselves to measurements of academic skills, and indeed only to the academic skills assessed on standard achievement tests, we still have the problem of how to aggregate the measurement of these skills to give us an overall measure of output. If one accepts the fact, as most of us do, that academic achievement alone is not important, but that a variety of social and personal skills, as well as the development of a sense of values and of moral autonomy, are needed, one is really nonplussed by even crude assessments of these individual components. There is, of course, the well-worn answer that the things that matter most are really ineffable and immeasurable, but this romantic attitude is not one for which I have much tolerance. I am simply struck in my own thinking by the difficulty of making a good assessment, and my sense of the difficulties has been put in focus by trying to deal with some of the theoretical ideas economists have brought to bear in education.

Recognition of complexity. One of the thrusts of theory is to show that what appear on the surface to be simple matters of empirical investigation, on a deeper view, prove to be complex and subtle. The basic skills of language and mathematics at any level of instruction, but primarily at the most elementary level, provide good examples. If we are offered two methods of reading it is straightforward to design an experiment to see whether or not a difference of any significant magnitude between the two methods can be found in the achievement of students. It has been progress in education to recognize that such problems can be studied as scientific problems, and it is a mark of the work of the first half of this century, the *golden age* of empiricism as I termed it earlier, to firmly establish the use of such methods in education. It is an additional step, however, and one in which the recognition of theory is the main carrier of progress to recognize that the empirical comparison of two methods of teaching reading or of teaching subtraction, to take an example that has been much researched, is by no means to provide anything like the theory of how the child learns to read or learns to do arithmetic.

A most elementary perusal of psychological considerations of information processing shows at once how far we are from an adequate theory of learning even the most elementary basic skills. It is a requirement of theory, but not of experimentalism, to provide analysis of the process by which the child acquires a basic skill and later uses it. It is a merit of theory to push for a deeper understanding of the acquisition and not to rest until we have a complete process analysis of what the child does and what goes on inside his head as he acquires a new skill.

The history of physics can be written around the concept of the search for mechanisms ranging from the reduction of astronomical motions to compositions of circular motions in the time of Ptolemy to the gravitational and electromagnetic mechanisms of modern physics. It has been to a partial extent, and should be to a greater extent, a primary thrust of theory in educational research to seek mechanisms or processes that answer the question of why a given aspect of education works the way it does. This should be true whether we consider the individual learning of a child beginning school or the much broader interaction between adolescents, their peer groups, and what is supposed to take place in their high school classrooms. For educational purposes we need an understanding of biosocial mechanisms of influence as much as in medicine we need an understanding of biochemical mechanisms for the control of disease in a host organism. The search beyond the facts for a conception of mechanism or of explanation forces upon us a recognition of the complexity of the phenomena and the need for a theory of this complexity.

Why not Deweyean problem solving? The instrumental view of knowledge developed by Peirce and Dewey led, especially in the hands of Dewey, to an emphasis on the importance of problem solving in inquiry. As Dewey repeatedly emphasized, inquiry is the transformation of an indeterminate situation that presents a problem into one that is determinate and unified by the solution of the initial problem. Dewey's conception of inquiry can be regarded as a proper corrective to an overly scholastic and rigid conception of scientific theory, but the weakness of replacing classical conceptions of scientific theory by inquiry as problem solving is that the articulation of the historically and intellectually important role of theory in inquiry is neglected or slighted. In any case, even if we accept some of Dewey's criticisms of classical philosophical conceptions of theory, we can argue for the importance of the development of scientific theories as potential tools for use in problem solving. It would be a naive and careless view of problem solving to think that on each occasion where we find ourselves in an indeterminate situation we can begin afresh to think about the problem and not to bring to bear a variety of sophisticated systematic tools. This sounds so obvious that it is hard to believe anyone could disagree with it. Historically, however, it is important to recognize that under the influence of Dewey educational leadership moved away from development and testing of theory, and Dewey himself did not properly recognize the importance of deep-running systematic theories.[2]

The newest version of the naive problem-solving viewpoint is to be found in the romantics running from John Holt to Charles Silberman, who seem to think that simply by using our natural intuition and by observing what goes on in classrooms we can put together all the ingredients needed to solve our educational problems. To a large extent these new romantics are the proper heirs of Dewey, and they suffer from the same intellectual weakness—the absence of the felt need for theoretically based techniques of analysis.

The continual plague of romantic problem solvers in education will only disappear, as have plagues of the past, when the proper antidotes are developed. My belief about these antidotes is that we need deep-running theories of the kind that have driven alchemists out of chemistry and astrologers out of astronomy.

Triviality of bare empiricism. The best general argument for theory in educational research I have left for last. This is the obvious triviality of bare empiricism as an approach to knowledge. Those parts of science that have been beset by bare empiricism have suffered accordingly. It is to be found everywhere historically, ranging from the sections on natural history in the early *Transactions of the Royal Society* of the seventeenth century to the endless lists of case histories in medicine, or as an example closer to home, to studies of methods of instruction that report only raw data. At its most extreme level, bare empiricism is simply the recording of individual facts, and with no apparatus of generalization or theory, these bare facts duly recorded lead nowhere. They do not provide even a practical guide for future experience or policy. They do not provide methods of prediction or analysis. In short, bare empiricism does not generalize.

The same triviality may be claimed for the bare intuition of the romantics. Either bare empiricism or bare intuition leads not only to triviality, but also to chaos in practice if each teacher is left only to his or her own observations and intuitions. Reliance on bare empiricism or bare intuition in educational practice is a mental form of streaking, and nudity of mind is not as appealing as nudity of body.

2. *Examples of Theory in Educational Research*

There are good examples of theory in educational research. I want to consider a few and examine their characteristic features. After surveying five main areas in which substantial theories may be found, I turn to the general question of whether we can expect developments of theory strictly within educational research, or whether we should think of educational research as applied science, drawing upon other domains for the fundamental theories considered, on the model, for example, of pharmacology in relation to biochemistry, or electrical engineering in relation to physics.

Statistical design. The bible of much if not most educational research is a statistical bible, and there is little doubt that the best use of statistics in educational research is at a high level. It is sometimes thought by research workers in education that statistical design is simply used in experimental studies and that it does not represent a theoretical component, but I think a more accurate way of formulating the situation is this. When the substantive hypotheses being tested are essentially empirical in character and are not drawn from a broader theoretical framework, then the only theoretical component of the study is the statistical theory required to provide a proper test of the hypotheses. As a broad generalization I would claim that the best-developed theory used in educational research is the theory of statistical design of experiments. The sophisticated level that has been reached in these matters by the latter part of the twentieth century is one of the glories of science in the twentieth century, and the dedication to insisting on proper organization of evidence to make a strong inference has been one of the most creditable sides of educational research over the past fifty years.

The opprobrium heaped on matters statistical in educational circles arises, I think, from two main sources. One is that on occasion the teaching traditions have been bad and students have been taught to approach the use of statistics in rote or cookbook fashion, without reaching for any genuine understanding of the inference procedures and their intellectual justification. The second is that the mere use of statistics is not a substitute for good theoretical analysis about the substantive questions at hand. There is no doubt that excellent statistical methods have been used more than once to test utterly trivial hypotheses that could scarcely be of interest to anyone. Neither of these defects, however, makes a serious case for the unimportance of statistical theory.

Test theory. My second example is closely related to the first, but is more specific to educational matters. The educational practice of basing decisions on tests has a long and venerable history, the longest and most continuous history being the examinations for mandarins in China, running from the twelfth century to the downfall of the empire at the end of the nineteenth century. The great traditions of testing in Oxford and Cambridge are famous and in previous years notorious. As tradition has it, students preparing for the Mathematical Tripos at Cambridge worked so intensely and so feverishly that many of them went from the examination room directly to the hospital for a period of recuperation. The position that a man achieved in the Mathematical Tripos at Cambridge in the nineteenth century was one of the most important facts about his entire career.

The competitive spirit about examinations for admittance to college or graduate school in this country is not at all a new phenomenon, but rather it represents an old and established cultural tradition. What is new in this century is the *theory* of tests. In all of that long history of 700 years of Chinese examinations there seems to have been no serious thought about the theory of such tests or even a systematic attempt to collect data of empirical significance. It is an insight that belongs to this century, and historically will be recorded as an important achievement of this century, to recognize that a theory of tests is possible and has to a considerable extent been developed. By these remarks I do not mean to suggest that the theory of tests has reached a state of perfection, but rather that definite and clear accomplishments have taken place. It is in fact a credit to the theory that many of the more important weaknesses of current tests are explicitly recognized. Certainly the concepts of validity and reliability of tests, and the more specific axioms of classical test theory, represent a permanent contribution to the literature of educational theory. (Lord & Novick's systematic treatise, 1968, provides a superb analysis of the foundations of the classical theory.)

Learning theory. In the March 1974 issue of the *Educational Researcher*, W. J. McKeachie has an article entitled "The Decline and Fall of the Laws of Learning." He examines what has happened to Thorndike's Law of Effect and Law of Exercise, especially in the more recent versions of reinforcement theory advocated by Skinner.

McKeachie is right in his analysis of the decline and fall of classical laws of learning, but I think that over the past two decades the specific and more technical development of mathematical models of learning that have not made sweeping claims as being the only laws of learning or as being adequate to all kinds of learning have accomplished a great deal and represent a permanent scientific advance. Moreover, the development of mathematical models of learning has not been restricted to simple laboratory situations, but has encompassed results directly relevant to subject-matter learning ranging from elementary mathematics to acquisition at the college level of a second language.

It is not to the point in this general lecture to enter into details, but because a good deal of my own research is in this area, I cannot forbear a few more remarks about what has been accomplished. In the case of mathematics, we can give a detailed mathematical theory of the learning of elementary mathematical concepts and skills by students. The details of the theory are a far cry from the early pioneering work of Thorndike. In fact, the mathematical tools for the formulation of detailed theory were simply not available during the time of Thorndike. I would not want to claim that the theories we can currently construct and test are the last word on these matters. The analysis of specific mathematical skills and concepts has been achieved by moving away from the simple-minded conception of stimulus and response found in Skinner's writings. In a previous paper given to this Association, I criticized in detail some of the things Skinner has had to say about the learning of mathematics (Suppes, 1972). I shall not repeat those criticisms, but rather in the present context, I shall emphasize the positive and try to sketch the kind of theoretical apparatus that has been added to classical stimulus-response theories of learning in order to have a theory of adequate structural depth to handle specific mathematical concepts and skills.

As many of you would expect, the basic step is to postulate a hierarchy of internal processing on the part of the student—processing that must include the handling at least in schematic form of the perceptual format in which problems are presented, whether they are arithmetic algorithms or simple problems of a geometric character. An internal processing language is postulated and the basic mechanism of learning is that of constructing subroutines or programs for the handling of particular concepts and skills (Suppes, 1969b; Suppes & Morningstar, 1972, Ch. 4; Suppes, 1972).

There is one important theoretical point about such work that I would like to make, because I think that ignoring this theoretical point represents a major error on the part of some learning psychologists and also of physiological psychologists. The point is that it is a mistake to think of precisely one internal processing language and one particular subroutine for a given skill or concept being learned in the same form by each student. What we can expect in an area like mathematics is behavioral isomorphism, but not internal isomorphism, of subroutines. It is important to think about the theory in this way and not to expect a point-for-point confirmation of the internal programs constructed by the student as he acquires new skills and concepts. To assume that the physiology of human beings is so constructed that we can infer from the physiology how particular tasks are learned and organized internally is as mistaken as to think that from the specification of the physical hardware of a computer we can infer the structure of programs that are written for that computer. It is one reason for thinking that the contributions of physiological psychologists to educational psychology are necessarily limited in principle and not simply in practice. This seems to me worth mentioning because currently physiological psychology is the fashion, and if we are not careful we will begin to hear that the next great hope in educational psychology will be the contributions we can expect from physiological psychology. I am making the strong claim that in principle this may not be possible, and that we can proceed independently within educational research to develop powerful theories of learning without dependence on the latest news from neurophysiology.

The kind of examples I have sketched for elementary mathematics can also be extended to language skills and to the important problem of reading. Much of my own recent work has been concerned with first- and second-language acquisition, but I shall not try to expand upon these matters except again to say that what is important about current work in these areas is that specific theories of considerable structural depth, using tools developed in logic for semantics and in linguistics for syntax, have been constructed to provide a richness of theory and a potential for subsequent development that has not existed until the past decade or so (Smith, 1972; Suppes, 1970, 1971, 1974; Suppes, Smith, & Léveillé, 1972). I am sanguine about the possibilities for the future and believe that substantive contributions of importance to education may be expected from learning theory throughout the rest of this century.

Theories of instruction. One of the most interesting and direct applications of modern work in mathematical models of learning has been to the burgeoning subject of theories of instruction. A theory of instruction differs from a theory of learning in the following respect. We assume that a mathematical model of learning will provide an approximate description of the student's learning, and the task for a theory of instruction is then to settle the question of how the instructional sequence of concepts, skills, and facts should be organized to optimize for a given student his rate of learning. My colleague, Richard Atkinson, has been successfully applying such methods for the past several years, and some of the results he has achieved in beginning reading skills are especially striking (Atkinson, 1972, 1974; Atkinson & Paulson, 1972). The mathematical techniques of optimization used in theories of instruction draw upon a wealth of results from other areas of science, especially from tools developed in mathematical economics and operations research over the past two decades, and it would be my prediction that we will see increasingly sophisticated theories of instruction in the near future.

Continuing development of computer-assisted instruction makes possible detailed implementation of specific theories in ways that would hardly be possible in ordinary classrooms. The application by Atkinson and his collaborators that I mentioned earlier has this character, and some of my own work in elementary mathematics is of the same sort. In the case of the elementary-school mathematics programs, what we have been able to do is to derive from plausible qualitative assumptions a stochastic differential equation describing the trajectory of students through the curriculum, with the constants of the solution of the differential equation corresponding to unique parameters of each individual student (Suppes, Fletcher, & Zanotti, 1973). The fits to data we have achieved in this effort are about as good as any I have ever achieved, and I think we can now speak with confidence in this area of student trajectories in the same spirit that we speak of trajectories of bodies in the solar system. But again, I emphasize that this in only the beginning, and the promise of future developments seems much more substantial.

Economic models. As I have already remarked, economists' vigorous interest in education over the past decade has been one of the most salient features of new theoretical work in educational research. Some of us may not like thinking about education as primarily an investment in human capital, and no doubt the concepts of economics introduced into discussions of educational policy in the past few years are alien to many people in education, including a goodly number of educational researchers. Measurements of productivity, for example, that depend mainly on a measurement of output that counts only the number of bodies that pass through a given door to receive accreditation rightly raise questions in the minds of many of us, as do other measures the economists use, sometimes with apparently too much abandon. Moreover, the theoretical tools from economics that have been brought to bear in the economics of education are as yet not thoroughly developed. It is too often the case that an economic model for a particular educational process actually consists of nothing more than an empirical linear-regression equation that has little, if any, theoretical justification back of it. (See, for example, the otherwise excellent articles of Chiswick & Mincer, 1972, and Griliches & Mason, 1972.)

All the same, it is my feeling that the dialogue that has begun and that is continuing at an accelerated pace between economists and the broad community of educational researchers is an important one for our discipline. The broad global concepts that economists are used to dealing with provide in many respects a good intellectual antidote to the overly microscopic concerns of educational psychology that have dominated much of the research in education in past decades. I do not mean to suggest by this remark that we should eliminate the microscopic research—I have been too dedicated to it myself to recommend anything of the sort—but rather to say that it is good to have both kinds of work underway, and to have serious intellectual concentration on the broad picture of what is happening in our educational system. The sometimes mindless suggestions of outsiders about how priorities in education should be reallocated or how particular functions should be reduced is best met not by cries of outrage, but by soberminded and careful intellectual analysis of our priorities in allocation of resources. Economic theory, above all, provides the appropriate tools for such an analysis, and I am pleased to see that a growing circle of educational researchers are becoming familiar with the use of these tools and are spending a good deal of time thinking about their applications in education.

3. Sources of Theory

I promised earlier to examine the

more general question of whether theory in educational research is chiefly a matter of applying theories developed in economics, psychology, sociology, anthropology, and other sciences close in spirit to the central problems of education. I firmly believe that such applications will continue to play a major role in educational research as they have in the past, but I also resist the notion that theoretically based work in educational research must wait for the latest developments in various other scientific disciplines before it can move forward. Other areas of applied science show a much more complicated and tangled history of interaction between the basically applied discipline and the fundamental discipline nearest to it. Physics is not just applied mathematics, nor is electrical engineering just applied physics. These disciplines interact and mutually enrich each other. The same can be said for education.

In the earlier history of this century it was difficult to disentangle progress in educational psychology from progress in more general experimental psychology, and recently some of the best young economists have claimed the economics of education as the primary area of economics in which they will develop their fundamental contributions. The role of educational researchers should be not merely to test theories made by others, but, when the occasion demands and the opportunity is there, to create new theories as well. Some areas, like the theory of instruction, seem ripe for this sort of development. Another area that I like to call the theory of talking and listening, or what we might call in more standard terms, the theory of verbal communication, seems ripe also for developments special to education, and I do not propose that we wait for linguists and logicians to set us on the right theoretical tracks. What is important is not the decision as to whether the theories should be made at home or abroad, but the positive decision to increase significantly the theory-laden character of our research.

Another point needs to be made about these matters of the source of theory. One of the favorite economic generalizations of our time is that this is the age of specialization. Not every man can do everything equally well, as most of us know when faced with the breakdown of a television set or a washing machine or some other modern device of convenience. This same attitude of specialization should be our attitude toward theory. Not everyone should have the same grasp of theory nor the same involvement in its development. Physics has long recognized such a division of labor between experimental and theoretical physics, and I have come to believe that we need to encourage a similar division in educational research. Ultimately, the most important work may be empirical, but we need both kinds of workers in the vineyard and we need variety of training for these various workers, not only in terms of different areas of education, but also in terms of whether their approach is primarily theoretical or experimental. It is a mark of the undeveloped character of current educational research that we do not have as much division of labor and specialization of research technique as seems desirable.

According to one apocryphal story about the late John von Neumann, he was asked in the early fifties to put together a master list of unsolved problems in mathematics comparable to the famous list given by Hilbert at the beginning of the century. Von Neumann answered that he did not know enough about the various branches of mathematics as they had then developed to provide such a list. I shall be happy when the same kind of developments are found in educational research, and when not only inquiring reporters but also colleagues across the hall recognize that the theoretical work in learning theory, or theories of instruction, or the economics of education, or what have you, is now too richly developed and too intricate to have more than amateur opinions about it.

It is often thought and said that what we most need in education is wisdom and broad understanding of the issues that confront us. Not at all, I say. What we need are deeply structured theories in education that drastically reduce, if not eliminate, the need for wisdom. I do not want wise men to design or build the airplane I fly in, but rather technical men who understand the theory of aerodynamics and the structural properties of metal. I do not want a banker acting like a sage to recommend the measures to control inflation, but rather an economist who can articulate a theory that will be shown to work and who can make explicit the reason why it works (or fails). And so it is with education. Wisdom we need, I will admit, but good theories we need even more. I want to see a new generation of trained theorists and an equally competent band of experimentalists to surround them, and I look for the day when they will show that the theories I now cherish were merely humble way stations on the road to the theoretical palaces they have constructed.

Notes

[1] Presidential address to the American Educational Research Association, Chicago, April 17, 1974. Some of the research reported in this article has been supported by National Science Foundation Grant NSFGJ-443X.

[2] The most detailed expression of Dewey's (1938) view of scientific inquiry as problem solving is to be found in his *Logic*. A critical, but I think not unsympathetic, analysis of this work is to be found in my account of Nagel's lectures on Dewey's logic (Suppes, 1969a).

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