The Influence of Research on Educational Practice [1, 2]

Fred N. Kerlinger*
University of Amsterdam

How does research influence educational practice? I know of no satisfactory empirical answer to this question. It has recently been answered systematically, empirically, and competently in medicine (Comroe & Dripps, 1976), but, as far as I know, not in education. Virtually all answers in education have been speculation and opinion, sometimes based on research, sometimes not.

In this address, I will defend the following three propositions. One, there is little direct connection between research and educational practice. Two, bodies of research aimed at theoretical understanding of psychological, sociological and other behavioral scientific phenomena of possible relevance to educational thinking and practice may have beneficial though indirect effects on educational practice. A corollary is that basic research is more important than applied research in its potential effect on education. And three, two major obstacles to research influencing educational practice in the long term are the pragmatic-practical notion that research should pay off and that it should be relevant to contemporary social and educational problems.

To defend these propositions, I will discuss the basic purpose of scientific behavioral research, the assumed validity of the payoff and relevance notions, and how research can and perhaps does influence practice. Finally, I will recommend what we, as educators and as educational researchers, can and should do to maximize the fruitful outcome of our efforts.

The Purpose of Scientific Behavioral Research

The basic purpose of scientific research is theory. This rather enigmatic statement means that the purpose of scientific research is to understand and explain phenomena (see Braithwaite, 1953). A theory presents a systematic view of phenomena by specifying relations among variables, with the purpose of explaining and predicting the phenomena. Theory is held in high esteem by behavioral scientists – and rightly so. The high esteem springs from science's basic purpose, and theory is the vehicle for expressing the basic purpose. Science, then, really has no other purpose than theory, or understanding and explanation.

Many people think that the purpose of rersearch is or should be to improve the lot of mankind. Not so. Either men improve man's lot or it doesn't get improved. The misunderstanding in many people's minds about research and its presumed ameliorative purpose arises in part from confusing science with engineering and technology. Engineering is a set of applied disciplines that depend mostly on science but that are themselves not science. It is the job of the engineer to devise technical solutions to practical problems. In so doing, he uses technology, which likewise often arises from science but is not itself science. Technology comprises technical methods and materials devised to achieve practical objectives. This is quite different, of course, from the purpose of science.

This is a hard argument to digest. So let me give an example to show what I mean. Suppose a theory of learning has been found to be empirically valid, and rather successfully explains the learning of concepts. The research to test the theory is scientific research because it explains some aspects of human learning. It may or may not have implications for teaching concepts to children. Whether it does or does not has nothing to do with its status as scientific research. A teaching expert now devises a method of teaching concepts based on the theory. He is an engineer, a technologist. Although based on scientific research, what he does is not itself scientific research. Of course he may test the efficacy of his method using techniques devised by scientists. His research is applied research which is in this case inspired by the original basic research. Actual teaching using the

* Herdruk uit de *Educational Researcher*, september 1977, Vol. 6, no. 8, pp. 5–12, met toestemming van de uitgever en de auteur.

method is partly engineering, partly art. It is certainly not science. There is no such thing as a science of teaching or a science of education.

In this talk I emphasize strongly the nature and purpose of basic scientific research and say little about applied research. The reason is that I feel that a basic scientific research approach to educational phenomena has been in general neglected and is increasingly jeopardized by the values, attitudes, and practices of important decision making and funding agencies in the United States (National Science Board, 1976). I am not saying that applied research is unimportant. But I believe and will try to show that basic research has greater ultimate impact on practice. At the least, I am trying to stimulate consideration of a better balance between basic and applied research in education.

Misconceptions of Scientific Research

There are in the Western world today three or four related ways of thinking about research, especially in education, that are inimical to research and that diminish its potential healthy influence on practice. I will examine two of these in some detail so that we can better understand the main problem. They are the pragmatic-practical misconception and the misconception that research in education should be relevant.

The Pragmatic-Practical Misconception

Most people assume that educational research can solve educational problems and improve educational practices. The assumption is false. And it creates expectations that cannot be fulfilled. Educational research does not lead directly to improvement in educational practice. The solution of a research problem is on a different level of discourse than the solution of an action problem. The outcome of a research problem is usually the establishment of a relation of some kind between two or more phenomena. This is true even of applied research problems. Take a relatively simple applied outcome like that in an experiment by Clark and Walberg (1968), who studied the relative effects of massive reinforcement and regular reinforcement on the reading achievement of under-achieving children. Their experiment showed that massive reinforcement had a fairly substantial effect on the reading achievement of the children who received it.

Can these results be applied directly to educational practice? On the surface, it would seem so. If a research study shows that massive reinforcement helps underachieving children to read better, then encourage teachers teaching such children to use massive reinforcement. Unfortunately, things are not so simple. Does massive reinforcement work with children of other ages? What difference does massive reinforcement make when used by different kinds of teachers? More subtle, is it possible that the prolonged use of massive reinforcement might have a deleterious effect on some or even all children? Might it, for example, have the effect of ultimately crippling children's internal motivation and initiative?

So even a seemingly obvious and simple outcome of research that is more applied than basic turns out to be removed from practice. If we take the results of many basic research studies that seem to have implications for educational practice we find an even greater gap. In most such studies the gulf between study findings and practice is wide and deep.

Studying relations and taking action are on two different levels of discourse which one cannot easily bridge. Scientific research never has the purpose of solving human or social problems, making decisions, and taking action. The researcher is preoccupied with, and schould be preoccupied with, variables and their relations. He should never be required to think about or to spell out the educational implications of what he is doing or has done. To require this is to require a leap from an abstract relational level of discourse to a much more concrete and specific level. This cannot be done directly; it is not possible to do a research study and then have practitioners immediately use the results.

The expectation that research should lead immediately to change in practice springs in good part from the well-known pragmatic and practical orientation of people who conceive the purpose of research as the improvement of the lot of mankind. Research, in this view, must pay off; there must be a return on the investment in research. Practical answers and problem solutions are demanded of science and scientists. Most educational research funding seems to be based on this expectation.

The roots of the expectation are strong in American history and life. We are a practical people; we want results. This pressing practical attitude has paid off handsomely; we have built a new world in a relatively short time. Part of the price we have paid for it, however, is anti-intellectualism. An acute observer of American life, the historian Hofstadter, has amply documented nineteenth century American impatience with intellectual matters (Hofstadter, 1962). We still have a strong current of it and the pragmatic orientation it springs from.

A strong pragmatic attitude virtually forces focus upon outcomes and getting things done. What is good is what works! There is relatively less emphasies on why things work; most important is that they work. This is a defeating attitude because, as Thomson (1960) has pointed out, 'The best way to make advances in technology . . . turns out to be to understand the principle' (p. 997). He has also pointed out that this idea is a recent discovery and has probably only recently become true. No wonder it is hard to understand!

Educators have little patience with what they conceive to be 'impractical', 'ivory tower' research. They want research to be put to practical work. The net effect of their impatience is a pervading anti-intellectualism that has a devastating effect on research in education. One of the unfortunate manifestations of this general attitude toward research is the urgent desire and demand for research to yield quick returns on our investment in it. Two talk about research for the sake of knowledge seems to many of us foolish, even pathetic. We must have payoff!

This is a forlorn and futile expectation. Scientific research does not pay off in any simple way because it is not and cannot be aimed at practical problems (Brain, 1965; Brooks, 1971; Dubos, 1961; Townes, 1968; Waterman, 1966). Indeed, our insistence on research leading to targeted and programmatic outcomes can have and has had deleterious consequences. One of these is reinforcement of our latent anti-intellectualism. Is it any wonder that educational research has not been distinguished for high quality? Another serious consequence is that our talented young men and women are led into dead ends, into fruitless and virtually meaningless searches for immediate solutions of educational problems.

Some of you may agree with my argument but may ask: How about applied research? Methods used by scientists are of course used in applied research. But the purpose of applied research is to help in making decisions and these decisions are ordinarily tied to relatively specific problems, even though they may be large problems. So applied research can of course be used to help solve such problems, but this problem solving does not ordinarily lead to understanding of the complex phenomena behind educational practice. While indispensable, especially when done in a milieu in which basic research is strong, its power and general applicability are limited.

Take reading. Answers to reading problems lie not in many researches aimed at telling teachers how to teach reading. They lie in research aimed at understanding the many aspects of human learning and teaching connected with reading. Such understanding is arrived at, if it is ever arrived at, by invoking psychological and other theories related to reading and doing research over long periods directed at understanding reading-related phenomena. Study of reading in and of itself is almost invariably unproductive. We must study reading in the context of perception, motivation, attitudes, values, intelligence, and so on. In other words, the goal should not be the improvement of reading! It should be understanding of the relations among the many complex phenomena related to reading. Research directed toward improving anything but minor skills is doomed to triviality, frustration, and defeat. To improve something as complex as reading requires understanding of reading and many related phenomena, a very difficult task indeed. And there is of course no guarantee of improvement in children's reading, even if basic research on phenomena related to reading is done.

It is unrealistic, therefore, to ask how a piece of scientific research will produce such-and-such educational results. This demand has probably weakened educational research more than any other single cause. We force our doctoral students to tell us how their theses will change educational practice, and the poor things comply with our demand when in reality the demand is impossible to meet for a simple reason. *People* make decisions and solve problems. Of course, the results of research may be suggestive: they may suggest that if you do thus-and-thus, such-and-such may happen. But that is all. They only suggest; they never demonstrate the certainty of practical outcomes. If we are to understand the influence that research can have on educational practice, we must understand how misguided the pragmatic-practical view is. No amount of Congressional, government, university, or student actions and demands can change the stubborn fact that scientific research of any consequence never pays off directly.

The Demand for Relevance

Like the pragmatic demand for payoff from research, the demand for relevance has highly deleterious consequences. Both demands are also hard to deal with because they are so plausible. What is more plausible than to ask that research should be relevant, that it should be directed to social and educational problems of worth and consequence? The problem of relevance is important and subtle, subtle because it is so plausible and because it can be used in the cause of ideology. It is the common argument of European Marxists and of both conservative and liberal American educators, for example. Both demand relevance from research.

The argument for relevance seems to say, in effect, that the substance and direction of research must be guided by significant social and educational problems. It is remarkable that European Marxists and American educators come to the same conclusion about research – but from different ideological bases and goals. A certain neo-Marxist group has said, for instance, that research in psychology must be relevant, and this means emancipation of powerless groups in the society – this is called 'emancipation research'![3] In other words, research is to be used for human and political purposes. It is no accident that one of the subfaculties that is almost completely radicalized in my university is education. Marxist, neo-Marxist, Maoist, and other radical students and instructors gravitate to those disciplines that are perceived as having potential relevance to the solution of social problems: sociology, education, psychology, political science.

The inevitable consequences are frightening: Basic research in education is almost entirely neglected except by a few individuals. It is not immediately relevant to social and human problems. Moreover, it supports the decadent, bourgeois, imperialist status quo. Thus, it should not be supported.

Arguing from a less sophisticated theoretical base, Congressmen, government officials, educational administrators, teachers, and educational researchers call for relevance, even though the word itself may not be used. The net effect of this call, together with the closely related payoff psychology mentioned earlier, is to cut off financial and moral support for basic research in education. At present, the support for basic research in the National Institute of Education budget, for example, is virtually nil. Overwhelming proportions of the budget go to projects that are conceived to be relevant and that promise solutions to pressing social and educational problems (AERA, 1976; NIE, 1976).[4]

But NIE policy and practice simply follow a deeper American philosophy of pragmatic return on the dollar, payoff in other words. As a professional staff member for the Senate Labor, Health, Education and Welfare Appropriations Subcommittee said last year, 'We want N.I.E. to show us that we are getting a bang for the bucks we are spending on educational research' (McNett, 1976). The relevance part of it springs not so much from ideological sources, as with the neo-Marxists, but from the need to conform to recent payoff trends and demands. This has resulted in a virtually exclusive focus on applied research. The effect is to choke off the most important part of educational research.

The demand for research to be relevant has three serious weaknesses. The first is: Who defines relevance and what is relevant? You? Me? Professors? Government officials? Politicians? Students? When we demand relevance we are in the midst of politics because competing claims of relevance have to be resolved. Research and politics and ideology do not mix well because, as Nisbet (1975) has said, 'In science, ideology tends to corrupt; absolute ideology, absolutely' (p. 46), and because research problems and goals cannot be decided democratically or autocratically. Research problems are decided by basic researchers pursuing theoretical explanations of phenomena, or by applied researchers seeking answers to questions of what will work and how it will work.

A second difficulty with the demand for relevance is that no one can really tell whether a line of research will lead to worthy practical outcomes or to socially desirable ends (DuBridge, 1969; Thompson, 1969; Townes, 1968). The demand for relevance puts the choice on politically chosen ends and forecloses other research possibilities. I shudder to think of our loss if the present demand for relevance had been as strong as it now is when Thurstone and Pavlov were doing their work!

In a remarkable report published last year and directed to assessing the relative effects of basic and applied research on medical practice, Comroe and Dripps (1976) show, clearly and unmistakeably, that basic research has been much more important than applied research in ultimate influence on applied modern clinical practice. This is the strongest empirical evidence I have yet seen supporting the great importance of basic research (see, also, Griffiths, 1967; Thompson, 1969; Townes, 1968). The Comroe and Dripps report also makes it clear how indirect the influences are.

Comroe and Dripps asked 40 psysicians to list the advances in medical practice that they considered the most important for their patients. They sent the selected advances to a large number of specialists and asked the specialists to vote on the list. The votes decided ten advances in medicine in the last thirty years. The authors, with 140 consultants, then identified the essential bodies of knowledge that had to be developed so that the advances could be made.

From some 2,500 research reports that were especially important to the development of one or more of the essential bodies of knowledge identified, they and consultants selected more than 500 essential or key articles for careful study. A 'key' article was one that had an important effect on subsequent research and development, reported new data or new ways of regarding old data, a new concept or hypothesis, and so on. In other words, it was a key article if it led to one of the ten clinical advances.

Comroe and Dripps classified the articles as: (1) basic research unrelated to the solution of a clinical problem; (2) basic research related to the clinical problem; (3) studies not preoccupied with basic mechanisms; (4) reviews; (5) developmental work or engineering to create, improve, or perfect apparatus or a technique for research; (6) the same as (5) but for use with patients.

The results were clear: Basic research was responsible for almost three times as many key articles as other types of research and almost twice as many articles as non-basic research and development taken together! (The figures were: basic: 61.7% not basic: 21.2%; development: 15.3%; review: 1.8%.) This remarkable research into research corrects distorted ideas of the contributions of basic and applied research to practice and strongly affirms what many scientists have been saying for the last thirty or more years: Basic research done not for payoff or relevance ultimately has greater effect than so-called programmatic or targeted research. Even if one can quarrel with this statement, it is at least evident that faith in the plausibility of the relevance argument must be shaken.

The third weakness of the relevance argument is the most fundamental one. Even if we had unanimous agreement on what is relevant, the argument misses the main point and purpose of science and scientific research, and, if accepted, leads to erosion of science. For if social amelioration is substituted for disinterested pursuit of understanding and explanation, science will lose the very things that have made it unique and powerful in advancing man's knowledge of the world and of himself: objectivity, disinterestedness, and universality.

I have been negative long enough. Do I have anything positive to say? Yes, I think so.

How Does Research Affect Education?

How does research influence and change education and educational practice? The effects of research are indirect and deep and are felt only over appreciable periods of time. Deeper understanding of underlying phenomena is relatively slow, even reluctant, because it has to combat or displace fixed sets of beliefs. Larger trends in theoretical thinking and series of research studies geared to answering general theoretical psychological and sociological questions have the greatest probability of having an impact. Applied research studies, virtually by definition, have less chance of having long-range and deep impact because they are aimed at specific and relatively narrow goals. Theoretically oriented studies aimed at understanding phenomena are general, abstract, and applicable in principle to many different problems and situations, if they are applicable at all.

Take attribution theory and attribution studies. In a provocative study by Harvey and Kelley (1974), one of the questions asked was: What conditions affect an individual's sense of his own competence? The researchers found that conditions of stability and instability of situations in which judgments were made affect pupils' sense of their own competence.

In another study stimulated by attribution theory, Jones and his colleagues (Jones, Rock, Shaver, Goethals, & Ward, 1968) were interested in the effects of initial success and failure on observers' judgments of ability. They had their subjects tackle a series of problems which were presented in such a way that observers saw some subjects first succeed and then fail and other subjects first fail and then succeed. The observers judged those who first succeeded more able than those who first failed, despite later performance.

Series of studies such as these should increase our understanding of attribution, a general phenomenon or process of potential importance to education and teaching. We may gain increased insight into teacher judgments of pupils and the conditions and traits of teachers that affect such judgments, for example. We will probably also pick up bonuses on the way. The serendipity of theoretical exploration is often surprising and rewarding. For example, is it possible that the Harvey and Kelley study is an opening wedge into a highly important but little explored aspect of motivation: sense of competence?

Neither of these studies by itself means much if anything for educational practice, though they are suggestive. A body of such studies, on the other hand, may help to change the thinking of psychologists, sociologists, and educators about an important area of human behavior, making judgments and other attributions. Such gained insights can have an impact on educational practice – though there is never any guarantee that there will be significant and beneficial impact.

Another example of long-range research that is already changing education in Europe and America is the series of developmental-epistemological studies of Piaget and his colleagues, reinforced by developmental studies done in the United States over many years. Curiously enough, developmental studies seem to be taking us back to some of the precepts and practices recommended by John Dewey. For example, Dewey said that the child has an intellectual life of his own, a way of thinking about reality quite different from an adult's. The child is not just a small adult. Piaget found ample evidence for the validity of this belief. Understanding by educators of the child's conception of reality is likely to change educational practice profoundly. Series of studies like these, then, will probably make a difference.

When we think of influence on knowledge, understanding, and practice, we rarely think of the influence of methodology. This is strange because methodology has already had a profound influence on behavioral scientific knowledge.

Methodological advances make a difference mainly because they change our ways of thinking about what we can study and how we can study it. They broaden our approach and perspective on research problems, in other words. Before the 1930's, for example, experiments were mostly two-variable affairs. After the invention of analysis of variance, however, more realistic and more theoretically interesting experiments could be done using two or more independent variables. Moreover, the important phenomenon of the interaction of two or more variables could be studied. In educational research, for instance, methods of teaching could be studied in conjunction with other variables, like ability, aptitude, sex, and attitude. Research using better and more appropriate methodology leads to results that are more generalizable and enlarges both experimental and nonexperimental research approaches and problems.

I believe, indeed, that we are in the midst of a revolution in research thinking due largely to methodological development. I want to give a rather complex example called analysis of covariance structures (Jöreskog, 1971, 1974), a general formulation of different methods of analysis in a highly sophisticated multivariate analysis framework. It integrates factor analysis, including hypothesis-testing factor analysis, multivariate analysis, study of change, and path analysis, for example, in a framework explicitly oriented to theory and hypothesis testing. In fact, it is explicitly aimed at complex testing of theory, and superbly combines methods hitherto considered and used separately. It also makes possible the rigorous testing of theories that have been very difficult to test adequately. Examples are theories of intelligence like Guilford's and Guttman's.

Although a long way from the classroom, its influence will ultimately be felt, just as the influence of factor analysis and multiple regression is now being felt. The recent past and present theoretical and research work of sociologists of education using path analysis is an example. By using path analysis, sociological explanations of educational phenomena have been strengthened. But path analysis will change profoundly because it has been shown to be a special case of covariance structure analysis. The latter will make path analysis much more powerful than it now is. This will change sociological explanations of educational phenomena and ultimately educational thinking and practice.

Yes, I think that methodology has a profound if indirect and oblique influence on practice. Methodology is, after all, different ways of doing things for different purposes. Change methodology and you change, to some extent at least, the problems we attack. Perhaps more important for educational research, problems that have seemed intractable because of their complexity are now becoming tractable and amenable to scientific scrutiny and attack.

The most important source of influence on practice is theory. I am thinking of theory at two levels. One is the larger kind of theory, for example, gestalt, behavioristic, psychoanalytic, and cognitive theories in psychology. Such theories change viewpoints on children and their learning, among other things. Sometimes they interact to produce change. It is not unlikely, for instance, that psychoanalytic theory interacted with behavioristic theory to produce a more open and permissive view of the child.

The other kind of theoretical influence is the more specific theory, such as attribution theory, reinforcement theory, and theories of intelligence. Theories of intelligence can change educational thinking and practice. The implications of environmental and hereditarian theories of intelligence can lead to quite different educational systems and practices, for instance. Reinforcement theory's influence has already been felt because of its strong emphasis on positive reinforcement. Teachers are more likely to use reward than punishment because their training cannot have helped but be influenced by reinforcement theory. They know that in their work the weight of evidence is on the side of positive reinforcement.

Conclusion

One of the most significant things about scientific research is the system of values behind it. When research is strong, an open atmosphere of critical inquiry is fostered, which in turn fosters openness and critical inquiry in our teaching. We are more likely to appeal to evidence in what we tell students, and we are more likely to require students to do the same. Theoretical explanations and empirical testing of theory become the underlying structure of our work and teaching. The university is plagued as much as other institutions by superstition, prejudice, and dogma. The healthiest antidote to such social diseases has been science and scientific ways of thinking and working because there is a constant appeal to empirical public evidence and a constant challenge of generalizations unsupported by evidence. As Monod (1971) puts it, science subverts mythology and dogma.

Science and scientific research change our ways of thinking about ourselves, others, and children, and about learning, motivation, intelligence, and the many psychological and sociological determinants of learning, achievement, and adjustment. A profession, once thoroughly exposed to science, can never be the same again. The effects of scientific behavioral research in education, then, should be strong though indirect and slow. Applied research should undoubtedly have effects, but they will probably not be as strong and far-reaching as the long-range effects of basic research. More germane to my main points in this talk, if applied research is emphasized and supported at the expense of basic research, then the results will be unfortunate. The more important research and thinking aimed at basic understanding of educational problems and processes cannot help but suffer, even weaken and die. So my question is: Can scientific research in education be strong? Will the current, partially irrational, attacks on scientific research have the effect of further weakening research in education? Basic scientific research has been neglected, sometimes denigrated, even in university schools of education. It is puzzling and frustrating that in universities faculty members and doctoral students – the present and future intellectual leaders of education – have been and are deficient in research knowledge and understanding of science. Add to this the apparent ignorance of national and local officials and policy makers of what research can and cannot do and how it is done, and we have little real promise for obtaining the knowledge needed for adequate understanding of education.

What Can Be Done?

What can be done to improve educational research to maximize the probability of it positively affecting educational practice? First, I doubt the efficacy of planned programs to improve schools and education through research. Such phenomena as action research, targeted research, programmatic research, and, in Europe, emancipation research are mostly bizarre nonsense, bandwagon climbing, and guruism, little related to what research is and should be. Indeed, such movements have serious negative effects because they distract us from adequate research and because they substitute superficial and mediocre activities for the hard coin of scientific research.

Second, we should not make promises we can't keep. I agree strongly with Frankel (1973) when he says, in his brilliant essay on irrationalism and rational inquiry, 'Considerable damage has also been done by scientists, among whom social scientists are perhaps the most notable, who exaggerate the amount of sound and applicable knowledge they have and who offer confident solutions to social problems–solutions that, when tried, turn out to be only a mixture of pious hope and insular moral judgments' (p. 931). We should refuse to inflate the currency of educational research. This means that we should not create futile expectations of what educational research can and will do. When we talk to Congressmen and other influential policy makers and to school people and parents, we should not promise great improvements. The job of educating policy makers and the public is very difficult, but we should at least try to do it properly and with complete honesty.

Third, there should be a judicious balance between basic and applied research. The present overemphasis on applied research and neglect of basic research is shortsighted and ultimately detrimental to educational research and educational practice (Panel, 1960; Waterman, 1966). To foster and maintain such a balance should be a prime duty of the National Institute of Education and the American Educational Research Association. I believe that roughly one-quarter to one-half the budgets of federal educational research funding agencies should be allocated to basic research.

Fourth, as the Panel on Basic Research and Graduate Education of the President's Science Advisory Committee (1960) has pointed out, research that is not excellent has no place in science: 'In science the excellent is not just better than the ordinary; it is almost all that matters' (p. 1814). Mediocre research is bad research. We must always aim, therefore, to do excellent research. To do this, we have to give educational researchers the best theoretical, mathematical, and methodological training possible in order to maximize the probability of excellence. Conceptual and technical competence should be our first training goal. Cutting off federal funds for research training programs, therefore, strikes me as irresponsible. So do mediocre research training programs in schools of education. The main source of basic research in education should be university schools of education. They must therefore have high quality research training programs.

Fifth, education research leadership should come from educational researchers and not from officials and agencies, federal or state. I am puzzled and chagrined by NIE and the Congress, for example, setting broad and general research goals for the whole country. Congress has even mandated NIE concentration of resources on five research goals or needs (*Congressional Record-House*, 1976). I am also deeply concerned when I read in the National Science Board's (1976) important report, *Science at the Bicentennial*, of the dismal and deleterious effects of government pressure for applied rather than basic research and its overregulation of research all over the country and evidently in all fields. We should try to minimize the influence of government and foundation research goals, which are often dictated by political and other extraneous considerations. This may of course mean giving up federal funds. My answer to that is that such funds will not do much good anyway. Indeed, they distract us from much of what we should be doing.

Sixth and last, we should try to create and maintain in our universities and laboratories the open atmosphere of free inquiry characteristic of science at its best. It is mainly in such an atmosphere that excellent and creative research is done. We should be extremely wary of proposals and actions that would limit this freedom directly or indirectly. One reason I am so suspicious of 'save-the-world' proposals is not just because they are essentially phoney, but also because, with their financial and prestige resources and rewards, they distract young men and women of promise from the real and fundamental tasks of research.

There are many obstacles to and distractions from doing research, especially in education. One of the most potent is closing the open atmosphere of free inquiry by special appeals to improve education through research and by channeling resources and support to special 'virtue' projects and special ways of doing research that promise social and educational improvement. One of the most deleterious effects of the general acceptance of alluring and 'special' research activities is the lack of social, financial, and psychological support for basic research, which is made to appear less attractive, less alluring, and more demanding. I do not mean, of course, that we should not encourage innovation and new developments. I simply ask for a better balanced and more open environment and for critical examination of proposals, especially those involving large sums of money and those that obviously promise more than they can deliver.

I am both optimistic and pessimistic. There are hopeful signs of health in educational research. For example, some of the most promising of recent developments in theory and methodology come from individuals working in educational research or closely connected with it. But there are also influences hostile to research: the demands for payoff and relevance, attacks on objectivity, educator and policy maker lack of understanding of science and scientific research, and the general lack of a congenial atmosphere for research. I am inclined to believe that increased understanding and acceptance of research are inevitable. But how long will it take? Until research is understood and accepted, there will be little change in educational practice based on tested theory and empirical evidence. Instead, we will have to depend on the conflicting claims of men and women with greater or lesser amounts of magical power and charisma.[5]

Notes

1. Presidential address, AERA Annual Meeting, April 1977, New York City.
2. I am grateful to the following individuals for reading and crticizing the first draft of this address: H. Beilin, L. Beilin, D. Griffiths, G. Mennenbergh, R. Owen, E. Pedhazur, and W. Russell.
3. You may be interested to know that in Germany my book, *Foundations of Behavioral Research*, was translated by members of this group. One thing they did

was substitute for the whole of my Chapter 1 a chapter written by a neo-Marxist! (I knew nothing of this until the book was published.) In the substituted chapter, the importance of relevance was brought out. In the Preface it was implied that I wasn't with it-and needed the translator's help.

4. The original plan for NIE was an admirable balance between basic and applied research concerns. The change came from Congressional and public pressure for payoff and relevance. With such strong pressure, the original excellent conception of NIE seems to have been forgotten.
5. Just before giving this address, I received a draft of a report prepared by a group of consultants to the National Institute of Education, 'Fundamental Research Relevant to Education.' The reasoning of the report and its conclusions and recommendations are similar to those of this address. Had I had this report earlier, I would certainly have cited it. It is an important document that deserves the careful study of the members of AERA, indeed of the whole education community.

References

American Educational Research Association. AERA info memo on governmental and professional liaison. 6 February 1976.

Brian, W.R. Science and antiscience. *Science*, 1965, 148, 192-198.

Braithwaite, R. B. *Scientific explanation*. Cambridge: Cambridge University press, 1953.

Brooks, H. Can science survive in the modern age? *Science*, 1971, 174, 21-30.

Carey, W. D. Basic research and Congress. *Science*, 1976, 192, 743.

Clark, C. A., & Walberg, H. J. The influence of massive rewards on reading achievement in potential urban school dropouts. *American Educational Research Journal*. 1968, 5, 305-310.

Comroe, J. H., & Dripps, R. D. Scientific basic for the support of biomedical science. *Science*, 1976, 192, 105-111.

Congressional Record-House. Sec 403, September 27, 1976.

Dubos, René. Scientist and public. *Science*, 1961, 133, 1207-1211.

DuBridge, L. A. Science serves society. *Science*, 1969, 164, 1137-1140.

Frankel, C. The nature and sources of irrationalism. *Science*, 1973, 180, 927-931.

Griffiths, D. E. The ten most significant educational research findings in the past ten years. *Executive Action Letter*, 1976, 6, 1-10.

Harvey, J. H., & Kelley, H. H. Sense of own judgmental competence as a function of temporal pattern of stability-instability in judgment. *Journal of Personality and Social Psychology*, 1974, 29, 526-538.

Hofstadter, R. *Anti-intellectualism in American life*. New York: Vintage, 1966.

Jones, E. E., Rock, L., Shaver, K. G., Goethals, G. R., & Ward, L. M. Pattern of performance and ability attribution: An unexpected primacy effect. *Journal of Personality and Social Psychology*, 1975, 32, 767-773.

Jöreskog, K. G. Statistical anlysis of sets of congeneric tests. *Psychometrika*, 1971, 36, 109-133.

Jöreskog, K. G. Analyzing psychological data by structural analysis of covariance matrices. In D. H. Krantz, R. C. Atkinson, R. D. Luce, & P. Suppes (Eds.), *Contemporary developments in mathematical psychology* (Vol. II): *Measurement, psychophysics, and information processing*. San Francisco: Freeman, 1974.

McNett, I. E. R&D can help with the ABC's. *New York Times*, July 18, 1976.

Monod, J. *Chance and necessity*. New York: Knopf, 1971.

National Science Board. *Science at the bicentennial: A report from the research community*. Washington: U.S. Government Printing Office, 1976.

Nisbet, R. Knowledge dethroned. *New York Times Magazine*, pp. 34, 36, 39, 41, 43, 46.

Panel on Basic Research and Graduate Education of the President's Science Advisory Committee. Scientific progress and the Federal government. *Science*, 1960, 132, 1802-1815.

The National Institute of Education. Washington, D.C.: U.S. Department of Health Education and Welfare, National Institute of Education, 1976.

Thompson, P. TRACES: Basic research links to technology appraised. *Science*, 1969, 163, 374–375.

Thomson, G. The two aspects of science. *Science*, 1960, 132, 996-1000.

Townes, C. H. Quantum electronics, and surprise in development of technology. *Science*, 1968, 159, 699-703.

Waterman, A. T. Federal support of science. *Science*, 1966, 153, 1359-1361.