TWENTY-FIVE YEARS OF EDUCATIONAL RESEARCH*

BENJAMIN S. BLOOM

University of Chicago

INTRODUCTION

As President of The American Educational Research Association I have been trying to take stock of what we have accomplished during the past quarter of a century. Such stock taking tells as much about the person doing the inventory as it does about the field. Although the President (now Past President) of an organization may believe that his role gives him access to more information and a more objective perspective than is available to others, this is probably one of the delusions fostered by the office.

In any case, it is this writer's hope that each group of educational research workers will be sufficiently provoked by this paper to undertake a similar effort on their own to determine what has been accomplished over the past 25 years by our educational research. Such efforts should take into consideration the ways in which we have worked and should give some thought to the ways in which our efforts in the future can be increasingly effective. Each group will probably see the field from a different perspective, and it is to be hoped that sharing these perspectives and stock taking will enable us to find a better base for our work in the future.

Need for Stock Taking

This is the 50th anniversary of The American Educational Research Association. An anniversary provides a ceremonial occasion for stock taking, but it hardly warrants an effort to do more than engage in the appropriate sentimental reminiscences.

Somewhat greater motivation for the task may be derived from the rapid increase in federal funding of educational research and development. The increase from the level of support in 1960 has been of the order of 2,000 per cent. This increase has been so rapid that few of us have had the opportunity to assess the overall effects of these funds on educational research, to say nothing about the effects they have had on education. We are all aware that the increases in support are in large measure based on faith in the "magic" of research. The power of research has already been amply demonstrated in medicine, engineering, and agriculture. The effects of research in the natural sciences have also

been clearly demonstrated. The case for educational research is yet to be demonstrated. The effects of the new funding on the quantity of educational research is already quite evident. We anticipate marked changes in the quantity and quality of students in educational training programs, and we have already seen the effects of funding in bringing new breeds of workers from many related fields to educational research and development. Stock taking is an inevitable consequence of new governmental funding—let us hope that the best minds in the field of education will share in the making of the inventory.

An even more telling reason for stock taking arises from the new faith in education and the new tasks thrust on it. Education is looked to for solutions to problems of poverty, racial discord, crime and delinquency, urban living, peace, and even for the problems arising from affluence. The new tasks thrust on education require new approaches, new understandings, and a closer relation between theory, research, and practice than has ever existed before. It is not likely that education will be able to provide all the solutions that are expected of it. However, if solutions are found, they are likely to be based on research and development. Compensatory education for the disadvantaged is a case in point. Educational solutions must be found for these children—solutions which depend on more than the dedicated teacher working by himself. The educational researcher cannot ignore such problems, and he must contribute to the creation of the solutions.

To put the matter in a larger setting, there is increasing evidence that many societies are repelled by the fruits of research in the physical sciences, which seem to create more problems than they solve. Each new development of energy, power, and speed appears to bring about greater means of destruction and more anxiety than existed before. As societies seek for more positive and optimistic areas in which to put their hopes and resources, they appear to be those concerned with man and his development. Most of us are of the view that education is the one area which is the hope of mankind. If educational research is able to respond effectively to the new order of problems, this field will probably come to occupy the position hitherto held by research workers in the natural sciences.

But enough of the future. As educational researchers we need to get a clearer picture of our recent past. We tend to veer from overoptimism about educational research and its effects on education and the schools to almost complete despair and pessimism. From time to time we wonder whether anything has been produced by our efforts. Have the schools been changed in any respect by our work over the past 25 years? It is probable that the truth lies somewhere between our most optimistic and our most pessimistic picture of the state of education and educational research. We all need to find the picture for ourselves if we are to have the courage and energy to go on.
Personnel and Research Output

There are many ways in which we might take stock of where we are and how far we have come. One is to look at personnel and research output quantitatively. The number of persons awarded doctoral degrees in education has gone from about 1,500 in 1954 to approximately 2,400 in 1964. While this increase in personnel presumably equipped to do educational research is relatively small compared with the great increase in funding, it does indicate that there are likely to be more people available for research.

Membership in AERA has been increasing at the rate of about 25 per cent per year during the past five years. It is debatable whether this growth rate is an indication of increased interest in educational research or increased effectiveness of our Membership Committee.

Another indication of the strength of the field is the interest in education and educational research shown by scholars from other disciplines. An increasing number of scholars from the behavioral sciences, and especially psychology, sociology, anthropology, and economics, are turning to educational research, and many of them are making important contributions to the field. Some of them are doing much to help us see education in new terms. I am happy to report that many of these scholars from other disciplines are joining AERA. However, the significant point is that scholars from many disciplines are recognizing the central place of education in the scheme of things and are interested in making their contributions to educational research and development.

During the past three years I have had an opportunity to visit a number of schools and departments of education and to meet with many research workers through AERA activities, conferences and summer institutes. Although my impressions are highly subjective, I am convinced that the young educational research workers are as able a group of research workers as can be found in any of the social sciences. These young people represent our most valuable asset, and, if properly encouraged and supported in their research, they are likely to make major contributions to the field.

Finally, there is no lack of research output. Educational research workers have no hesitancy about writing up their studies and publishing them. During the last 25 years approximately 70,000 titles were listed in the Review of Educational Research. While there is some overlap in that the same article or book may have been listed in several issues of the Review, it is clear that we are now annually publishing about 2,500 items that the authors of the reviews regard as contributions to educational research. From the increase in research completed under Office of Education grants, it is safe to predict that publications of educational research will increase rapidly in the near future.

Quantitatively there is no doubt that educational research is a lively
and growing field. In terms of manpower and research output it is developing rapidly. There are also some indications that the quality of research personnel is improving, but this is only a highly subjective impression.

Another approach to stock taking is to make an estimate of the significant contributions to the field over the past quarter of a century. This is a highly controversial type of inventory and only the most daring or foolish individuals are likely to permit their summaries to be published. It is my hope that although I may find myself in the latter category, others will be stimulated to do the same in order to correct the record.

**Methodological Contributions**

In some ways education is one of the strongest fields in the behavioral sciences in terms of its contributions to research methodology and its use of complex techniques and technologies for research. With the possible exceptions of psychology and economics, educational research workers have contributed to and used stronger and more powerful research procedures than have other social scientists. Our colleagues in other fields have recognized this and occasionally accuse us of using "elephant guns to shoot at fleas." I do not mean to say that all of our research is characterized by precision and methodological elegance—far from it. What I am trying to say is that research workers dealing with educational problems have contributed to and used very powerful research methods and procedures and that our field does not lag behind other social science fields in this respect.

Especially with respect to statistical methods, educational researchers have pioneered, adapted, or used skillfully a great variety of complex procedures. Factor analysis, analysis of variance, multi-variate procedures, sampling methods, and research designs are some of the areas in which educational research workers have made major contributions during the past twenty-five years.

Educational researchers have contributed new computer programs and have made a great deal of use of computers and computer technology in their research. In the use of computers for statistical purposes, for research on learning, and for the simulation of individual and group processes of thought and behavior, workers in educational research have been in the forefront.

Advances have been made by educational workers in the mapping of human characteristics. The delineation of human aptitudes and abilities by factor analysis and other methods has progressed greatly in the past quarter of a century. Closely related to this have been the developments in the classification of the outcomes of education. These maps have been very useful as bases for further research, and they have helped greatly in the communication process. I regard such maps as methodological con-
tributions because they enable us to specify some of our variables with greater precision and because they provide classificatory devices for some of our research findings.

Closely related to these maps of human characteristics have been the many contributions over the past 25 years in the development of tests and testing procedures. Workers in educational research have made many advances in the evaluation of student progress toward specific objectives of education. While much of the work has centered on cognitive outcomes of learning, including creativity, some developments have taken place in the evaluation of interests, attitudes, and values. A great deal of work has also been done by educational researchers in the development of more precise instruments for the measurement of a large number of aptitudes, abilities, and specific personality and emotional characteristics.

One more type of contribution that I would regard as methodological has been the development of instructional procedures. Programmed instruction and computer assisted instruction are two of the more dramatic examples. Other instructional procedures such as those emphasizing inquiry and discovery may also be regarded as methodological contributions.

Undoubtedly, there have been other methodological contributions which might have been cited here. I leave it to my readers to amend this list and to point out important omissions.

**SUBSTANTIVE CONTRIBUTIONS**

By substantive contributions I mean contributions to new ways of viewing a particular phenomenon, new understanding of a particular topic or problem, and new ways of stating the question or problem. Methodological contributions have to do with new procedures and techniques for research while substantive contributions have to do with research which has made a difference in the way we think about education and learning, in the view of a particular educational problem, and, we hope, in the way education goes on in the school or home.

This distinction does not embody a value judgment about which is the more important—methodological or substantive. It is likely that progress in one type is dependent on progress in the other. The development of the electron microscope (a methodological contribution) dramatically affected our understanding of cell tissue and disease (substantive contribution). The development of new measures and statistical techniques (methodological contributions) are likely to be basic to the development of new insights into particular areas of learning and development (substantive contributions).

In what follows, I propose to name a few areas in which I believe certain crucial studies have altered, or are likely to alter, our way of thinking about educational phenomena. I have emphasized those areas
most directly concerned with the educational development of the student.

One group of studies has vitally affected our conceptions about the development of the individual. During the past 25 years we have gained a great deal in our understanding of developmental sequences through the work of such persons as E. Erickson, A. Gesell, R. Havighurst, and J. Piaget. These studies of developmental sequence, which have emphasized process, are in large part supported by longitudinal research of a more quantitative nature done by workers connected with the Harvard Growth Study, the Berkeley Growth Study, the Oakland Growth Study, and the Fels Institute. While these studies show great individual differences in growth, they do reveal an orderly sequence of development and the great importance of the early years of childhood for much of later development.

Another set of studies has shown us a great deal about the effects of the environment on the development of the individual. Perhaps one of the most fundamental distinctions that is emerging is the view of the home as an educative environment with its own curriculum, in contrast to an earlier view of the home as a unit in a socio-economic or social class status system. The role of parents as models has been studied in some detail by J. W. Douglas and J. Floud; the teaching style of mothers has been investigated by R. Hess and S. Stodolsky; while the language learning in the home has been studied in depth by B. Bernstein, D. McCarthy, and L. Vigotsky. The effect of the early environment on conceptual development and intelligence has been studied by M. Deutsch, A. Jensen, J. Mc V. Hunt, and R. Wolf. The effects of parents and the home on attitude formation in relation to the schools has been clarified for us by the work of J. A. Kahl, D. McClelland, and S. Smilansky. Another type of environment that has been studied is the peer group. The work of J. S. Coleman has enabled us to understand some of the effects of the adolescent subculture on the individual student.

Much research has been done on the predictability of human characteristics. It has become increasingly evident that school achievement and other characteristics particularly relevant to the work of the schools can be predicted with greater precision than was previously thought to be the case, especially when the home and school characteristics are put in as part of the predictor variables. The work of A. Payne, F. Peters, and L. Tucker bears directly on this problem.

Quite in contrast to the research on the prediction of human characteristics is the work on the modifiability of human characteristics. In one sense this is the central task of education and much of our research on education is concerned with this problem. Some of the more crucial studies which seem most pertinent to this problem as it relates to young children have been done by S. Kirk, S. Gray, M. Deutsch, and M. Smilansky.
During the past twenty-five years there has been a tremendous amount of research on a great variety of teaching methods and instructional strategies. It had been difficult to see any generalizations emerging from this research until models for the study of instruction, such as those developed by J. Carroll, J. Ginther, and L. Siegel, were available. What appears to be emerging is that a great variety of instructional methods yield essentially equal outcomes in terms of student achievement of lower mental processes, such as knowledge or simple skills. Large class, small class, T. V. instruction, audio-visual methods, lecture, discussion, demonstration, team teaching, programed instruction, authoritarian and non-authoritarian instructional procedures, etc. all appear to be equally effective methods in helping the student learn more information or simple skills. This does not mean that each use of an instructional approach is equally good with every other use of the same approach. There is still good and bad teaching, good and bad programed instruction, etc. We need quality-control studies to insure that a particular example of an instructional strategy is of the appropriate quality. However, we are free to use a great variety of instructional methods—if the goal of instruction is the acquisition of information. In contrast to the evidence about the great variety of instructional approaches which are relevant to the learning of information, is the lack of clear evidence about the instructional approaches which are effective in bringing about significant changes in the higher mental processes. While the work of P. Dressel, H. M. Chausow, E. M. Glaser, R. Suchman and others suggests that dialectic (rather than didactic) approaches appear to be more effective in producing changes in higher mental processes, the research evidence is far from complete and convincing on this point.

The effect of individual differences in learners has always been one of the central concerns of educators. Much of educational research attempts to bring in individual differences as a major variable in the investigation. Some especially pertinent research on the role of personality in learning has been reported by such workers as T. M. Newcomb, N. Sanford, S. B. Sarason, and G. G. Stern. Research on the effects of independence vs. dependence in learners has been carried on by C. Houle, C. McCollough, W. J. McKeachie, H. Thelen, and E. L. Van Atta.

During the past quarter of a century a great deal of work has been done on the principles of learning. The role of learning cues, student involvement and participation, and reward and reinforcement have been clarified by the work of J. S. Bruner, J. Dollard, E. Hilgard, N. E. Miller, B. F. Skinner, and R. W. White.

As a final area in which I believe crucial investigations have been reported during this period is the work on sequence in learning. Especially noteworthy in revealing some of the considerations involved in learning
sequences is the research of N. A. Crowder, R. M. Gagne, and B. F. Skinner.

I am sure that my readers will take issue with my classifications of areas of research that have been most significant as well as with the particular work (or researchers) I have named.

In Retrospect

As I indicated earlier, approximately 70,000 studies were listed in the Review of Educational Research over the past 25 years. Of these 70,000 studies, I regard about 70 as being crucial for all that follows. That is, about 1 out of 1,000 reported studies seem to me to be crucial and significant, approximately 3 studies per year. It is likely that a somewhat more relaxed criterion would increase the number of studies regarded as crucial by the order of 3 times the present list. I doubt if anyone would increase the list as much as 10 times. Even with the threefold increase, this would mean no more than 9 crucial studies a year (out of approximately 2,500 per year). Perhaps this is all that we should expect in educational research, and it may be about the level expected in any field of research. However, it is my opinion that we need much more in education, and I am confident that we can get a great deal more if we are willing to make the effort and if the proper research strategies are available to us.

Some Suggestions for the Future

One way in which we can get more and better research is to increase the amount of time available for research. Very few persons in the United States give the largest portion of their time to educational research. Administrative work, teaching, committee responsibilities, speaking engagements, etc. all conspire to reduce the time most of us devote to research. It is possible that the increase in funding of research will enable many of us to buy more time for research, but research is difficult, and there are many tempting distractions. We will devote more of our time to research as we become convinced of the need for research in education, and as we become more fully aware of the contributions that research can make to education. Perhaps this is only another way of saying that as the demand increases, we will find ways of increasing the supply.

Another way in which we can improve the quality of educational research is to improve the qualifications of educational research workers. All of us must constantly seek to upgrade our research competence and ways must be found to provide opportunities for each of us to secure further training as needed. The use of brief training sessions is widespread in most fields of research, and we must find better means of providing such opportunities in educational research. The pre-session workshops and conferences provided by AERA this year represent a small
move in the right direction. We can also improve the quality of educational research workers by improving our graduate training programs. The new grants for training provided by the Office of Education give us an opportunity to attract the best students and to improve our training programs simultaneously. Let us hope that our schools of education will take advantage of both of these possibilities. The increased interest in education and educational research makes it possible to attract to educational research some of the outstanding scholars in other disciplines. We have already seen this taking place. We must find ways of involving our colleagues in other fields in the attack on educational research problems as well as in helping us in the training of our graduate students.

We need advances in our theoretical and conceptual schemes, but it is difficult to do anything directly which will result in improvements in theory and model building. We also need improvements in our research methodology and especially in the training of research workers in the use of research methods. As I have indicated earlier, it is my opinion that we do not lag behind other social sciences in this area. Advances in this area will be beneficial to us all, but I suspect that this is not the direction from which we will secure great increases in the production of crucial research contributions to education.

What is especially needed, in my opinion, is the development of basic new research strategies which are analogous to those which have resulted in the rapid strides made in such fields as medicine, biology, and physics. Some suggestions for this may be found in the article "Strong Inference" by John Platt in *Science*, October 16, 1964. Platt, in attempting to explain the rapid growth of crucial investigations in molecular biology and other fields, believes that it is the use of strong inference which is responsible. There are several features of the strong inference research strategy that I believe to be most important for educational research.

1. There is a need for a clear map of the present state of the field. Such a map should indicate the most promising alternative pathways for future research as well as the alternatives that have been found to be inadequate or incorrect. Platt points out that in molecular biology the different research groups appear to have worked up trees of knowledge in which they graphically represent the present state of the field and the branches show the pathways that have been most fruitful as well as the ones which have not. In such a scheme the research view of the home environment as a curriculum and instructional approach might be contrasted with the less adequate (for education) research view of the home as a sociological status unit. This paper represents one effort to sketch the nature of such a map (or tree).

2. There is a need for rapid communication throughout the country.
and world among the researchers dealing with a particular portion of the map. It is claimed that research workers throughout the world in medicine and in some areas of physics and biology are able to get word of important discoveries within 24 hours. I am of the opinion that it takes months for educational researchers to learn about what has been found by other workers and that it takes years before a “discovery” is recognized as such. We need to find procedures for speeding up the communication process and for developing “invisible colleges” in which educational researchers throughout the world can be in close touch with their colleagues working on closely related problems. We need communication at various stages of the research process, but especially at the stage where each new finding must be related to the overall picture or map.

It is likely that the development of maps and closer communication would do much to discourage research which is a mere repetition of something already clearly determined by previous research. The creation of R and D Centers and Regional Laboratories will probably result in improved maps of the field as well as better communication. The publication of lists of research in progress will help researchers find colleagues currently interested in related problems. The increased use of summer centers, training programs and conferences of educational research workers should do much to bring communication about educational research to the level now found in some of the other fields of research.

3. A major element in strong inference, according to Platt, is the search for crucial ways of asking the question and for research procedures which will yield clearer and more definitive results. He suggests the use of multiple hypothesis procedures and the use of research designs that permit the testing of several hypotheses simultaneously, in contrast with the slower method of taking one hypothesis at a time. Some of the newer developments in research design and multi-variate statistical procedures (see Gage, *Handbook of Research on Teaching, 1963*) appear to me to be relevant to this feature of strong inference.

*Some Possible Consequences*

It is likely that one major result of the use of some of the elements of “strong inference” would be a reduction in the amount of redundant research. There is much repetition in educational research, and this is particularly apparent in any careful scrutiny of the research summarized in the *Review of Educational Research* over the past twenty-five years. It is this redundancy that in part explains why there are so few examples of crucial research in the period under consideration.

Another possible effect of strong inference approaches to educa-
tional research could be a greater emphasis on the research problems of education rather than the methods of research. Each of us becomes addicted to favorite methods of doing research, and we keep looking for problems to which our methods may be applied. Perhaps we should turn it around and seek the important problems of research and then select the methods we find to be relevant.

It is possible that this way of looking at educational research would help us to view educational research as something which is important in itself. Research would be for "real" rather than for the gaining of points in a rating system related to academic rank, salary, prestige, etc.

Finally, it is to be hoped that the use of strong inference approaches would result in a rapid sequence of fundamental discoveries which could then be supported by further replication and demonstration under a wide range of conditions.

Let us hope that more powerful research strategies will enable us to produce in the next five years at least as many crucial substantive pieces of research as we produced in the last 25 years.

(Received March, 1966)