The purpose of this paper is not to review all or even much of the past or current research in mathematics education; this has been done elsewhere. Monographs covering research in mathematics education up to about 1960, for example, are available from the U. S. Department of Health, Education, and Welfare. Some of the NCTM yearbooks also provide a good source.

I would like here to put such research into a perspective, to indicate some of the strengths and weaknesses of various approaches to educational research generally, and to describe what I see as the emerging research role of the mathematics educator.

Background

To take this task practicable, I shall limit my discussion to research on teaching and learning, the area with which I am most familiar. As you well know, the traditional methods paradigm for research on teaching and learning has been designed to assess the relative effectiveness of two or more instructional methods. A major difficulty with this sort of research is that too often, both method and content are varied simultaneously, but, not independently. Such an approach allows one to say nothing about either separately. Thus, for example, if a modern mathematics program is taught by the discovery method and a traditional program by traditional methods, then, the results on any criterion variable such as problem solving ability, attitude, and the like may be due to methods differences, content differences, or both in some unknown combination.

In other studies, the concomitant variations in content and/or method are more subtle. The variations in content, to which I refer, are those differences that can be attributed to the presentation (with resultant learning and practice) of different, but unspecified, aspects of the same material. Thus, when teaching by exposition, the instructor may state verbally that the distributive principle may be used to facilitate the calculation of $17 \times 551 + 17 \times 449$ whereas when using the discovery method, the presented material may consist of carefully sequenced examples in which the distributive principle may be used. Often, fundamental differences in method also go undetected. Expository instruction, for example, typically continues until the student can perform the required calculations, whereas discovery instruction normally proceeds until the student "catches on." In such situations as these, the possibility of detecting differences due to instructional methods depends critically on what criterion measures are used. By way of illustration, two methods of instruction may influence the amount learned to about the

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¹This paper is based on a talk given at the N.C.T.M. meeting in Minneapolis on August 21, 1964.
same degree, whereas the differences in attitude engendered by the two methods may be highly reliable. Even more critical, from the standpoint of interpretability, is the fact that in studies of this kind, it is impossible to specify the basis for whatever differences are found. Some of my own research*, for example, has demonstrated that relatively minor variations within exposition and/or discovery methods may alter the experimental outcomes drastically.

Until recently, the pattern of methods research had not changed appreciably since the 1920's and 1930's, a pattern which was largely established during the Thorndikean Era. Throughout much of this period, researchers in psychology and in education were almost indistinguishable. The rise of behaviorism in America, an approach which gave psychology a means of obtaining greater respectability in the scientific community, and the crossing of the Atlantic of the Gestalt brand of psychology, which educators found more palatable, led to an ever widening gap between research in psychology and in education. Only recently have there been signs of some reproachment.

This lack of understanding between educators and psychologists has had a profound influence on educational research. Educators, desiring results of immediate applicability, have often dealt with problems and questions with little transfer potential. Much concern has been given to such questions as, "Is it better to teach subtraction via the carrying method, or is it better to use the borrowing principle?" The fact that the results of research based on questions of this sort, had little generality, and seemed to lead nowhere as to the formulation of any theory of instruction, was disheartening. Although a few energetic souls remained active in research throughout their careers, a result of this situation was that most educational researchers quit with the dissertation and most teachers and teacher educators lost faith in educational research findings.

**Types of Research**

Hopefully for the future, educational research is presently in the process of rebirth. There are several new approaches which appear promising—I shall outline four.

**Action Research.** The first is based on the philosophy that known learning principles are inadequate to explain real school learning, and that it would be better to start over from scratch rather than to try to patch up existing theory. This philosophy, originally championed by subject matter people—particularly the revolutionary brand of mathematics educator—has resulted in a large amount of sponsored action classroom research in the late 1950's and early 1960's. By such research, I refer to the sort of thing for which the U.I.C.S.M. and the Madison Project are famous. They seek to find out what makes for good teaching by trying out various methods and seeing what "works." The approach is that of the inventor as opposed to that of the scientist—like the Edison who creates the light bulb rather than the scientist who searches for the underlying principles on which the light bulb is based. There are at least two major limitations of action research. One limitation

*Ref. 3.
reflects the fact that invention is, by nature, quite arbitrary. Whatever teaching method is proposed, as a result of action research, necessarily exemplifies only one way of reaching the educational objective in question. The results of action research do not, in themselves, lead to better understanding of the teaching-learning process and, yet, more refined teaching technologies will depend on such understanding. In fact, even the educational objectives, themselves, often remain the private property of the innovator. Action research, at best, can create sound methodologies for present day classrooms, but, it cannot uncover principles on which future instructional inventions may be based. Another limitation of action research is the difficulty of transmitting knowledge about what has been learned about teaching except in terms of situation-bound specifics or vague generalities. Two methods have been widely used to disseminate information acquired in this manner. One approach has been to bring teachers into the project center for intensive training. For the most part, this has proved a satisfactory, although expensive, method. Another approach has been to develop films and tape recordings. The use of these media is based on the assumption that teaching is an art, and that artistry is best learned by emulation. The difficulty here, of course, is to know what to emulate. Is the teacher doing a good job because the children are raising their hands or because they are jumping up and down in their seats? Is it because the teacher doesn't seem to be saying too much; because the students determine the pace at which the instruction proceeds; or is it because they are talking to one another?

Let there be no doubt that inventive action research has served a valuable function in improving mathematics education throughout the land. Nonetheless, this has only been a first step.

Now that the writing and rewriting of materials phase of the new mathematics curricula is nearing the stage of completion, at least on the gigantic scale to which we have become accustomed, there is an ever increasing pressure to evaluate these new programs. Although there are some large scale evaluative studies in progress which are quite reminiscent of early curricular research, there is also an increasing recognition that more sophisticated and intensive research is needed. Attempts to get at the rudiments of teaching and learning generally, as well as in mathematics, are underway at a number of centers throughout the country. Research is beginning or going on at Maryland, Illinois, Minnesota, Stanford, and Florida State which emphasizes the teaching and learning of mathematics and mathematical type materials. Other institutions are planning research centers along these lines.

My opinion is that such research probably will not soon improve on the master teacher, at least in the classroom situation. It may, however, provide the master teacher with a better understanding of why what he does is good. Probably more important, it may make it possible to improve teacher education by providing sound information about teaching and learning which is capable of efficient transmission.

Another point to which I will return later is that the sort of research that is needed will require competencies which are different from those possessed by most mathematics educators.
Technological Research. A second modern approach to research on teaching and learning is based on the opinion that teaching should be treated as a technology based on a science of learning. Thus, many investigators, typically those with a background in experimental psychology, have been attacking the educational problem with the tools of their trade—such psychological notions as contiguity, reinforcement, and mediation. Most of these scientists are under no delusions that variables already identified in the laboratory will be sufficient in the educational situation; they are willing to embellish, but they are not willing to disregard what is already known about learning.

There is no question that many learning variables operate in the classroom, but there is a question as to whether the technological approach, based on a relatively molecular science, is the most efficient means of understanding the educational process. There are many ways of explaining the same phenomena. Thus, the pressure on an enclosed gas resulting from changes in its volume may be explained in terms of kinetic theory. But, explanation may be more easily accomplished and the necessary measurements more readily made by referring to the familiar gas law: pressure x volume = constant.

Similarly, the motions within our solar system may be just as accurately described in terms of epicycles when the earth is taken as center of the solar system as in terms of the Copernican Theory in which the sun plays the central role. The essential difference is in the efficiency of explanation.

There is no doubt that this sort of technological research should be encouraged—it is the means by which a bridge can be built between the psychology of learning and the instructional process. Nonetheless, there is no more need to forego the direct study of teaching (and in this I include the teaching of mathematics), than there was for the early chemist to ignore chemistry and expend all his energy first trying to explain chemical phenomena in terms of the more established science of physics. One more example, this by Goodman (1964,11), will crystallize the point. Suppose you wished to synthesize a chair with molecules. "If you were to get even a quarter of an inch up one of the legs, by that time you would be in such a state of indeterminacy that you would give up in complete and utter disgust. It would be better to employ a carpenter." The question is whether it would be any more feasible to construct a theory of carpentry.

Another limitation, or advantage, of the technological approach is that, whatever the findings, implementation will require a considerable degree of instrumentation. With this in mind, there are several significant attempts underway to utilize computer technology in instruction. Perhaps the furthest developed, at this time, is Stolow's Socrates System. Socrates is designed for individualized instruction. Not only does it provide a high degree of flexibility in sequencing within programs, but it allows the computer to select the most appropriate program—from those available—for achieving a particular outcome on the basis of the individual student's response history.

Ref. 2
Ref. 5
Consideration is given to both the number of correct responses and the
time it takes the student to answer.

The Plato System, also at Illinois, is designed to teach students
how to prove mathematical theorems. In an improved version of this
system the computer will be capable of determining whether the next step
in the student’s proof follows from any one or two previous steps. The
student, however, must justify each step in his proof by stating the
underlying axiom or theorem in detail so as to be unambiguous to the
computer.

IBM's new course writer system will operate out of Yorktown, Pennsyl-
vania, and make it possible to both explore what can be done with
computerized instruction and to conduct more refined empirical research
at several locations throughout the country. One of these stations is
being installed at The Florida State University.

Many-Variable Research. Still a third philosophy underlying
modern educational research is that the educational process is extremely
complicated—that many variables are involved. Thus, some investigators,
mostly educational and psychological research methodologists who naturally
have emphasized those things they know best, have argued that more
sophisticated research designs are needed.

For many years, educational researchers have included many cri-
terion variables in their experimentation. Thus, they may not only
measure the amount learned, but retention, transfer, attitudes, and
interests. In spite of this concern with large numbers of dependent
variables, the number of independent variables in research on teaching
and learning has typically been severely limited. Proponents of the
"complex design" are quick to point out that the availability of data
processing equipment makes large-scale experimentation with many factors
(independent variables) highly practical.

Some of these advocates also have argued that interactions between
variables may be more crucial than overall main effects—and, that
interactions can only be assessed when all of the interacting variables
are included in the same experimental design. The large-scale research
of McKeechic, Isaacson, and Milholland on the teaching of psychology
is based largely on this assumption. In mathematics education, possible
interactions between method and content may be at least as important
as any effects due to variations in either content or method alone.

Perhaps the major limitation of the complex design approach is
the very ability to deal with large numbers of independent variables.
Thus, there may be a tendency for the experimenter to add variables to
his design almost indiscriminantly. The availability of a large variety of
standardized tests by which to assess personality and other character-
istics of teachers and learners further increases this temptation and
makes choice a critical factor. Such choices are necessarily subjective.

This approach also poses a difficulty for any attempt at theory
formulation. By dealing with available tests, which sometimes measure
rather ill-specified outcomes, and educational variables, such as class
size, use of television, amount of teacher preparation, and the like, which are probably of secondary importance, the generation of theory may be unnecessarily complicated. A more profitable approach may be to first identify the crucial variables underlying the teaching-learning process. We need to invent preliminary rationales, if not theories, which will have the effect of making educational research more general and cumulative, not specific and fragmentary. To be sure theory development is the result of invention rather than discovery. Since theory must relate to empirical findings, however, it is different from technological invention in which something is created to serve some practical function. Theory development, in education, will be no easy job, but a behavioral language which lends itself to precise discourse on teaching and learning will surely help. A related need is to develop taxonomies by which subject matters may be classified so that empirical findings in one context may be generalized to others. It is my personal opinion that mechanical techniques, such as factor analysis, will not identify the critical variables. Such work may, in fact, actually hinder more refined analysis by "clouding" the basic issues. However, far be it for me to discourage any sort of research in which an investigator has faith.

Naturalistic Research. There is also a growing body of psychological types who are impressed with the first mentioned approach—that of action research—but, who wish to carry such research to its natural conclusion. The philosophy underlying this fourth approach to educational research is based on the thesis that the variables studied should be abstracted directly from the situation to be understood. Thus, people like R. K. Kersh, J. M. Linn, and J. E. Anderson, like others at Illinois hope to obtain a better understanding of the whys and wherefores of the preceding action research by isolating and studying systematically those variables which appear to be of critical importance in the mathematics classroom.

At the Florida State University, several projects are underway. In order to help identify latent mathematical talent, Ralph Heimer has developed some teach-test materials. Essentially, the idea is to determine the student's ability to learn new mathematics by presenting carefully selected materials and testing to see what and how much has been learned. In some of my own research, an abstract card instrument has been used as a means of simulating the teaching and learning of mathematics while minimizing the contaminating influence of previously acquired knowledge. Currently, my graduate assistants, Merle Behr and Steve Snitz, and I are involved in a project designed to help determine relationships between previously acquired knowledge and subsequent learning. One of our findings suggests that mere presentation of prerequisite material, without practice, may severely hamper later learning. We are now trying to establish the boundary conditions for this finding. In another research project, one which is just getting underway, we will be looking for relationships between method of presentation and learning ability. We hope not only to develop techniques for teaching students how to learn, but to discover underlying principles. Whether, of course, this can be accomplished remains to be seen.

Ref. 3
I would like to conclude this paper by expressing my views on the emerging research role of the mathematics educator.

Background. The traditional role of the professor of mathematics education has been to integrate, interpret, and disseminate relevant knowledge for practitioners. Teacher training and the in-service education of teachers have been his major responsibilities. Graduate school professors also educate those professional personnel who train teachers. The preparation of texts, writing for professional journals, and activity in local, state, and national organizations compete for the remainder of his time.

I contend that the mathematics educator is equally responsible for doing research—a responsibility which in the past has been badly neglected. In view of the large number of questions being raised in this revolutionary era of curriculum modernization, of improvements in educational research methodology, and of the growth of knowledge in the behavioral sciences, continuance of this neglect is unwarranted. Further, the availability of federal funds, while a mixed blessing, makes some of the practical problems that have existed in the past no longer relevant. In addition, the pressures on college and university teacher-educators to recruit large numbers of mathematics teachers will lessen in the foreseeable future as those born during the post war "baby boom" enter the teaching profession. Research might well occupy the relinquished time.

From its inception, postdoctoral research in education has been left largely to educational psychologists and research methodologists. As for research on mathematics teaching, some of these specialists are conducting relevant research and most provide a valuable consulting function—but, they cannot do the job alone. Providing assistance with the psychological, design, and statistical phases of an investigation are but a poor substitute for thorough familiarity with the discipline (i.e., mathematics education) to be understood.

In sum, this historical division between educational researchers and teacher educators generally has resulted in neither group having been properly equipped to conduct significant curricular and instructional research in the subject matter areas. Mathematics has undergone far-reaching changes during the twentieth century; school mathematics, in particular, finds itself reorganizing after a ten-year revolution. As a result of such growth and curricular change, certain persisting developmental and methodological problems have been reborn, to be sure, but, many new problems also have emerged. Those educators traditionally charged with research typically do not have the backgrounds necessary to ask the appropriate questions, let alone answer them. On the other hand, recent technological advances in research methodology and relevant research findings in the behavioral sciences are unknown to most teacher educators and mathematicians. They normally have not been trained to conduct modern behavioral research.
As a partial solution, interdisciplinary teams, often supported by government funds, have developed in many parts of the country. Typically, these teams have consisted of scholars in mathematics, behavioral scientists, and public school teachers. Less frequently there is a professor of mathematics education. In any case, the professional mathematics educator, although often present and sometimes consulted as a matter of courtesy, typically is not making or even involved in many of the important decisions.

Armed generally with first-rate people and the knowledge, methodologies, and tools that are now available, the team approach has often resulted in work far superior to that done previously. Yet, there are misgivings. Many highly competent mathematics educators feel that, in some cases, suggestions have been made which are not feasible, not desirable, or both. Thus, mathematicians design a curriculum that is well suited to the development of research mathematicians without asking whether or not this is a good thing to do. And, some psychologists, without really appreciating the problems of education, attempt to apply laboratory findings to educational problems almost indiscriminately. In other cases, the academicians are exhorting the same things that mathematics educators have been saying for years. But now, someone is paying attention.

The Problem. One major difficulty in a separate discipline approach to research in mathematics education is poor communication. Thus, the educator and psychologist often fail to understand the mathematician and so are unable to intelligently criticize him. Similarly, the mathematician doesn't really appreciate all that the psychologist tells him—after all, "that stuff based on rats has little to do with real thinking." As for the educator, "he doesn't have much of importance to say anyway."

Lack of adequate communication is perhaps one of the major reasons that interdisciplinary teams often lead to the development of new disciplines. There is a real need for mathematics educators, with a good background in both mathematics and at least one of the behavioral sciences, who are research oriented. How, for example, can a person ask and hope to answer basic questions relating to the problems and processes of teaching, learning, and creation in mathematics without solid understanding of mathematics, psychology, research methodology, and the educational processes? The background necessary for this sort of endeavor is not acquired in any of the traditional doctoral programs in mathematics, psychology, or in education. In the past, professors of mathematics education have been selected largely on the basis of past scholarship in mathematics or on their performance as classroom teachers or administrators. The importance of such backgrounds cannot be doubted. Yet, the fact remains that the educational process involves human behavior and knowledge of mathematics alone is not sufficient for the researcher in mathematics education. Neither is teaching or administrative skill sufficient. Having been involved primarily with practice and little with theory and supporting research, the master professor-teacher typically is ill-equipped to conduct research. More important, lacking familiarity with behavioral
research, both the mathematician and the former school professional often rely on time-worn statements like, "rote learning is useless" and "understanding is the goal of all good instruction." Such statements simply do not indicate the extent of present knowledge.

Some Recommendations. Lest there be some misunderstanding, I wish to emphasize that I do not feel that all mathematics educators should conduct scientific research. The conscientious clinical educator, in most cases, has too much to do already.

The people I describe should have an appreciation for clinical problems, to be sure, but they also must be specifically trained to do research. There may be some (few) people who can conduct significant research while maintaining an active clinical role, but for the vast majority, to try is to achieve mediocrity in both. Ideally, faculty roles should be defined more clearly, as in the medical schools, to allow clinical specialists and researchers to work separately, but in close proximity—the researchers keeping the clinicians in touch with the latest developments and the clinicians making the researchers aware of practical problems and clinical findings in need of clarification.

To be maximally effective, the training of mathematics education researchers must begin on entrance to the graduate schools, must be based on new specially devised graduate programs, and must be under the supervision of qualified researchers. I feel that such training cannot continue to be solely an individual-happenstance occurrence. Appropriate research experiences must be provided during the graduate years, as is done in the academic disciplines. The dissertation must become the product of research experiences, not an introduction; it should make a substantial contribution to understanding rather than merely pay lip service to this ideal and be relegated to nonsignificant obscurity on some library shelf. The course work should provide opportunities for comprehensive understanding of mathematics and a behavioral science, thorough familiarity with research procedures and the literature relating to a significant area of research, and grounding in the foundations of education.

An interdisciplinary approach would seem essential to train the sort of person I have described. It also is my opinion that graduate training, leading to advanced degrees in mathematics education, must be clearly separated from the in-service education of teachers. I point this out only because of the unfortunate confusion that has all too frequently occurred in the more traditional clinical programs. In addition, I feel that the time of both students and staff would be best spent if fewer, but more demanding, courses were required for graduate degrees in education. The most advanced training might consist of graduate seminars devoted to research and to interdisciplinary study.

The admission of bright young persons to graduate study in mathematics education, sound interdisciplinary courses of study, and research programs
directed toward the improvement of mathematics teaching are the least that can be provided for a society struggling with educational problems. Extending research leadership will require bold and imaginative departments of mathematics education and schools of education supported by competent research personnel and adequate facilities. To continue to ignore (or minimize) research in mathematics education is to chance that in the near future there may no longer be such a function for mathematics educators to ignore.
Bibliography


