The Reasonable Ineffectiveness of Research in Mathematics Education

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You may recognize the title above as a play on the title of an article by Richard W Hamming, "The Unreasonable Effectiveness of Mathematics," which appeared in the *American Mathematical Monthly* in February 1980 In his article, Hamming asks the question, "Why is mathematics so unreasonably effective?" He offers some partial explanations but ends by saying that these explanations are so insufficient "as to leave the question essentially unanswered" [p 82] I am going to follow the same strategy but with a different question. My question is, "Why is research in mathematics education so ineffective?" I shall offer some tentative thoughts on this question, but you will soon discern that I, too, must leave my question "essentially unanswered." In addressing my question, I shall make two claims: (1) much of the ineffectiveness of research in mathematics education is more perceived than real, and (2) most of the perceived ineffectiveness is reasonable

The effectiveness of research in education

Before asking whether research in mathematics education is really ineffective, I should like to broaden the context and ask whether research in education is, or has been, effective.

In 1978, the National Academy of Education published a thick book — 672 pages — edited by Suppes [1978], whose title suggests the positive claims that are made in its pages: *Impact of research on education: some case studies* The book contains nine studies that purport to show how educational research has contributed to school practice. Scriven [1980] has argued that the book does not address the "payoffs" from educational research; it

> turns out to be. concerned with such questions as whether the results of basic research in psychology and the social sciences trickle down to the educational research journals (which, you will not be surprised to hear, they do). That does not show that educational research *benefitted* from that other research and, more importantly, it does not show that educational research *benefitted anything else* [p. 10, italics in original]

Whether or not you agree with Scriven's analysis, the somewhat defensive tone Suppes adopts in the book's preface makes it clear that even in his view the effects of educational research have not been overwhelming:

> I like to think of the case studies presented here as representing only a sampling on the beginning of educational research in its early years of development. What the future holds should be brighter and better, because it will be able to build on the kind of work reported in this volume and, as that building takes place, the impact on practice should become more marked. [p. xvi]

Some of the difference of opinion between Scriven and Suppes concerns whether one should talk about *effects* (impact) or *benefits* Scriven is pushing a much more demanding criterion than is Suppes. But even if one limits the question to effectiveness, one has only to glance at the pages of journals such as the *Educational Researcher* over the past couple of years or attend some of the sessions at the annual meetings of the American Educational Research Association to sense that there is a crisis of faith in educational research Have we been doing the wrong things? Have we failed to make contact with school practice? Who, if anyone, is listening to what we have to say? Is it all an empty exercise? These are the kinds of questions one senses are below the surface of the articles and talks Many of these same questions undoubtedly occur to researchers in mathematics education, but we do not seem to be addressing them in even the oblique fashion of our colleagues in educational psychology

The issue of whether or not educational research is effective seems to get tangled up, in some people's minds, with the distinction between pure and applied research What is the difference between pure research and applied research in education? Which is more likely to have a greater impact on educational practice? Two models seem to be implicit when people make the distinction between pure and applied research.

The first model is *hierarchical* Basic research is at the top — so high in status that it is often seen as "up in the clouds." Below it is applied research, and below that somewhere is the mundane world of practical affairs The simplest version of this model is what Greeno (1978) calls "the pipe-line model":

> According to this model, fundamental knowledge and theories are like crude oil, which gets pumped out of the ground in basic research Basic knowledge is shipped to applied research settings where it is transformed into something more useful; this is like shipping crude oil to refineries, and transforming the product into useable forms. Finally, results of applied research are shipped to developers and disseminators who use the knowledge in making products for use in school and send the stuff around to school users. This is analogous to sending refined gasoline to filling stations, where customers can drive up and fill their tanks [pp. 7-8]

Greeno argues that this model is at best a weak reflection of the relation between science and technology in any field, and that it is grossly misleading in education. He argues that basic and applied research are not hierarchical; they overlap substantially, each contributing to the other, and the significant research questions are both basic and applied I shall return to this argument after further consideration of the hierarchical model.

People who do not adhere to the oversimplified pipeline model may still view basic research as higher in status be-

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cause it is theory-oriented and because it aims at generalization, whereas applied research, with its lack of theory and specificity, is necessarily lower in status. Suppes [1967], in an article written for a booklet that was the forerunner of the *Journal for Research in Mathematics Education*, seemed to adopt this hierarchical view. He argued that basic research could have a direct impact on practice, that we needed more basic research in mathematics education, and that we needed both theoretical and empirical basic research. He seemed to be suggesting that basic research in our field should be largely concerned with how students learn mathematics, that applied problems in our field include more effective ways of organizing the curriculum, and that basic research will necessarily be more helpful than applied research in addressing such curriculum questions. I should not have to point out that this is a somewhat restricted view.

Tyler [1981] seemed to assume the same sort of hierarchy between basic and applied research recently when he spoke to some directors of projects sponsored by the National Science Foundation. Where he uses "science education," one can read "mathematics education":

> Research needed in science education is not only basic research which results in widely generalizable concepts and principles but also applied research, that is, inquiries focused on particular situations and particular kinds of students, teachers, and institutions, which furnishes information of importance in improving science education in the particular circumstances where efforts to improve science education are being made. [p. 6]

Again, one has the connection between basic/general and applied/specific and, it seems to me, an implied status differential.

The other dominant model for thinking about basic and applied research might be called the *complementarity* model: the two types of research are seen as complementary, each with its own domain and its own agenda, and equal (supposedly) in status. When one sees basic research characterized as "conclusion-oriented" and applied research as "decision-oriented," one can be fairly sure these types of research are being viewed as separate but equally valid — for their own domain of relevance. Other ways to assert their complementarity are to characterize basic research as descriptive and applied research as prescriptive, or basic research as being concerned with learning and applied research with teaching.

Adherents of the complementarity view [Gelbach, 1979; Greeno, 1978] seem agreed that the distinction between the two types of research has become increasingly blurred in recent years. Gelbach argues that "it is now almost impossible to discriminate" [p. 9] between basic and applied research because of: (1) new capabilities in research methodology (such as multivariate statistical methods) and design that permit the investigation of practical instructional problems in natural classroom settings with the same "scientifically respectable levels of precision" [p. 9] that one has in investigating basic research problems; and (2) important arguments recently advanced by people such as Glaser, who claim that our theory-building should be prescriptive, not descriptive. Snow has argued that we should abandon attempts at general theory construction in favor of less ambitious, but more achievable, "local" theories; for example, "theories that apply to the teaching of arithmetic in grades 1-2-3 in Washington and Lincoln schools in Little City, but perhaps not to the two other elementary schools in that town" [Snow, quoted by Gelbach, 1979, p. 9]. Gelbach criticizes this view, saying that "local theory development should be our *last resort* rather than our next move" [p. 9, italics in original]. Certainly, Snow's position implies a convergence between basic and applied research.

I propose a third model for the difference between basic and applied research that can be used to address the question of the effectiveness of research. Although it may appear to conflict with the other two models, I think it is equally valid. Recall the story of the married couple who went to the rabbi to help resolve their quarrel. The wife went in to the rabbi and told her story, whereupon the rabbi nodded and said, "You are right." Then the husband told his side of the story to the rabbi, who nodded and said, "You are right." After the couple had gone on their way, the rabbi's wife, who had heard both exchanges, said to her husband, "They cannot both be right." And the rabbi nodded and said, "You are right." It is in this sense that I claim my model is right.

When people talk about research as being "basic" or "applied," "conclusion-oriented" or "decision-oriented," "descriptive" or "prescriptive," and so forth, they are referring not to anything that can be considered an intrinsic quality of the research study itself, but rather to either the researcher's purpose in conducting the study or the uses to which the study is put. In other words, the same study can be either basic or applied, depending on who is doing the labeling and for what purpose. Basic research is not defined by whether it is conducted in a laboratory rather than a school, nor is it defined by whether analysis of variance is used rather than chi-square. One cannot unambiguously label a piece of research as either basic or applied; one can only ask what connection the research study appears to have to theory and what connection to practice. Both of these, to a large extent, are in the eye of the beholder.

Consider a hypothetical situation: a researcher conducts a study and then writes a report. Her purpose may have been to understand and explain some phenomenon that has to do with the learning of mathematics. She may hope to derive some generalization from the results. Her purpose is basic research — conclusion-oriented research. That is her purpose, but that does not mean the study was basic research in some intrinsic sense. We are speaking only of her purpose. When the report is written, of course, her view will be woven, more or less explicitly, into the account. A reader of her report brings his own frame of reference to it. Although it is important to distinguish between a research study and the report of the study, especially when writing about one's own work, there is a sense in which, for the reader, the study *is* the report. The reader ordinarily has no firsthand knowledge of a study other than what is contained in the report. More precisely, for the reader, the study consists of the report plus the frame of reference in which he embeds it. A given reader may see our researcher's study as applied research, despite her avowed purpose. She may have failed to make a clear link with theory, even though she intended to do so. The reader may have a practical problem to solve and may be able to use the results of the study to help solve it

With respect to a particular piece of research, which can only be known to a public through some report, the classification of basic versus applied depends upon the perspective of the reader of the report. This model might be called a *lens* model — a study may be basic or applied depending upon the lens you use in reading a report of it. This lens ought to be understood as incorporating your purposes and intentions in extracting information from the report. If the report helps you formulate a theory, then the study is functioning as basic research, regardless of the author's intentions. If the study helps you solve a practical problem, then the study, for you, has been applied research.

This point of view has implications for the effectiveness of educational research in general and research in mathematics education in particular. From this point of view, effectiveness, too, is relative. It is relative to both the criterion used and the person using the criterion. Consider the argument presented by Getzels [1978] for the effectiveness of basic research in education. He characterizes basic research as follows:

> Basic research comprises studies in which the investigator formulates his own problem regarding a phenomenon or issue, and his aim is primarily to conceptualize and understand the chosen phenomenon or issue and only secondarily, if at all, to do anything about it. The work is theory oriented rather than action oriented. Although the distinction here is not foolproof, in this view basic research (like fine art) deals with "discovered problems" and applied research (like commercial art) deals with "presented problems." [p. 480]

This is a nice formulation, but it hinges on the "aim," the intention, of the investigator. It characterizes the research study as seen by the investigator, but not necessarily as seen by others. Getzels continues as follows:

> Despite the belief that basic or theory-oriented research has little effect on practice — a belief on which the sacrifice of basic research for other activities is founded — the fact is that basic research can have powerful effects on practice. [p. 480]

Getzels then gives several specific examples of such effects.

How is it that someone like Getzels can so confidently assert that basic research can have powerful effects on practice, when our own experience as practitioners has suggested to most of us that such effects are rare, if not unknown? Is it a difference between mathematics education and the rest of education? Or is it a difference attributable to our perspective, as opposed to Getzels'? Here, too, I shall argue that both are right.

The ineffectiveness of research in mathematics education

Let us turn to research in mathematics education and ask why it appears, to many, to be ineffective. One reason may be that, despite what appears to be a flood of research in our field, we actually have very little in the way of research to go on in drawing implications for practice. As many observers [e.g., Greeno 1978] have noted, the amount of money spent on research in education by the federal government in the United States is a small fraction of that spent on other areas of research such as national defense, space research, or atomic energy. Moreover, the amount spent on research in education compared to the amount spent on education in general is an even smaller fraction; one estimate is that it is less than 0.4% [Getzels, 1978, p. 478]. Mathematics education, although it has done comparatively well, has shared in this dearth of funding. Again, it is a matter of perspective. Viewed one way, a lot of money has been spent by the US federal government over the last decade or so to support research in mathematics education. Viewed another way, however, the funds have not been nearly enough to do the job properly.

We face another paradox, too. It sometimes seems as though, amid the frantic activity of research in mathematics education, we must have more than enough data to answer important questions that face us. Begle [1979] surely had this impression as he undertook the massive research synthesis that resulted in his book, *Critical variables in mathematics education*. Yet one can convincingly argue that we do not have enough data — certainly not enough of the right sort of data. As Bauersfeld [1979] noted: "We have a shortage in the midst of abundance" [p. 210]. Sanders [1981], speaking of educational research as a whole, recently argued that we lack

> a body of systematically observed, factual knowledge about the way education operates in its natural settings. ... Despite rampant empiricism, there is no extensive "figuration of facts" ... observed regularities of the empirical world which must be accounted for by scientific explanations. Although we have large quantities of census-type data, achievement and other test data, there is very little trustworthy data representing the facts of the educating process. [p. 9]

Sanders calls for more case studies and naturalistic investigations "to redress this weakness" (p. 9). His argument applies as well to research in mathematics education as to educational research in general.

Another partial explanation for the apparent ineffectiveness of research in mathematics education has to do with our lack of what Bauersfeld [1979] termed our "self-concept." Researchers in mathematics education do not constitute a true community. In North America, we have the Canadian Mathematics Education Study Group, the Special Interest Group for Research in Mathematics Education, the North American branch of the International Group for the Psychology of Mathematics Education, the Research Council for Diagnostic and Prescriptive Mathematics, the informal networks spawned by the Georgia Center for the Study of Learning and Teaching Mathematics, the research sessions at the meetings of the National Council of Teachers of Mathematics, the *Journal for Research in Mathematics Education*, and so on. Despite all of these activities — and perhaps because of all of them — we lack a strong common identity; we are not truly a community.

You have undoubtedly heard the refrain that most of the research studies in our field are conducted as part of the requirement for a doctorate and that most of these are done by people who will never do another piece of research. It is an old refrain, but unfortunately it seems to be as true today as it ever was. The annual surveys of research that have been published in the *Journal for Research in Mathematics Education* during the last decade show that, although the growth in the number of dissertations cited may have lagged a little behind the growth in the number of journal articles, there are still something like two dissertations for every article. Further, it appears that the overwhelming majority of the dissertations do not come from departments of mathematics education, nor are they conducted under the supervision of people who are recognized researchers in mathematics education. They may be good dissertations, and they may come from worthy programs in worthy institutions. But they do not arise from what one might call "the research community in mathematics education." They do not partake of issues that concern this community; they do not arise from common concerns, shared knowledge, mutual interaction. Is it any wonder that collectively they do not add up to very much?

Insufficient funds, insufficient support, insufficient knowledge, insufficient collegiality — are these not good reasons for the perception of research in mathematics education as ineffective? Is it not reasonable that research conducted under such conditions would fail to influence school practice? There are two additional reasons, however, that appear most compelling of all: (1) our lack of attention to theory, and (2) our failure to involve teachers as participants in our research.

Attention to theory

I recently examined the 35 (out of 38) articles in the ten issues of the *Journal for Research in Mathematics Education* from July 1979 to May 1981 whose authors had affiliations with US institutions only. I looked at each article to see if an attempt had been made to link the question under investigation to some theoretical context. For 20 of the articles, I could find no such attempt. I may have been too harsh in some of my judgments, and I may have been somewhat hasty, but this and other observations convince me that a lack of attention to theory is characteristic of US research in mathematics education. This conclusion may not apply to research done in some other countries, but the problem is not unique to the United States.

Why is this lack of attention to theory such a serious problem? I contend that it is only through a theoretical context that empirical research procedures and findings can be applied. Each empirical research study in mathematics education deals with a unique, limited, multi-dimensional situation, and any attempt to link the situation considered in the study with one's own "practical" situation requires an act of extrapolation. Extrapolation requires, however, that one embed the two situations in a common theoretical framework so that one can judge their similarity in various respects. As the old adage has it, "There is nothing so practical as a good theory." Kerlinger [1977] has argued that "the basic purpose of scientific research is theory" [p. 5] and that "there is little direct connection between research and educational practice" [p. 5]. The effect of research on educational practice is *indirect*; it is mediated through theory. As Kerlinger points out, two factors that in the long run hinder the effectiveness of educational research are the twin demands for payoff and relevance. Such demands short-circuit the theory-building process.

Les us consider some examples of how theory has, or has not, affected practice in mathematics education. A frequently cited example [Cronbach & Suppes, 1979; Resnick & Ford, 1981] is E. L. Thorndike's influence on the teaching of arithmetic during the early years of this century. There is no doubt that Thorndike, through his research, his teaching, and, most especially, his analysis of the psychology of arithmetic, substantially influenced the teaching of school arithmetic in the United States. He was one of the few educational theorists to be actively concerned with the nuts and bolts of curriculum building. His theoretical ideas had an impact in the classroom largely because he himself (and his students) analyzed textbooks in the light of his theory and made concrete suggestions for changes. His theory was his hammer; he looked around and saw the arithmetic curriculum as something to pound. One should perhaps note that he did not have much competition at the time and that he was extremely energetic in his efforts to apply his theoretical ideas. He was not, strictly speaking, a mathematics educator, and his research, strictly speaking, was not research in mathematics education, but we put it there quite happily. He is a notable exception to the charge that researchers do not influence practice in our field.

A second example is Piaget, also — needless to say — not a mathematics educator. Groen [1978] has assessed the impact of Piaget's theoretical ideas on educational practice, and he devotes one section of his assessment to mathematics. Groen begins by noting that "the hard core of Piagetian theory is replete with mathematical analogies" [p. 299], and consequently, "it is not surprising that there are many parallels between mathematics education and Piaget's own ideas" [p. 299]. Groen contends that, usually, rather than Piaget influencing the teaching of mathematics, it was the other way round — mathematics influenced Piaget's thinking. Groen then raises the issue of discovery learning and argues — with considerable justification — that on this issue the influential theorist has been not Piaget, but Polya. Further, he argues that with respect to "the notions of mathematical competence underlying the 'new math' curricula" [p. 300], the applied research done under the Piagetian influence dealth with highly specific problems and was difficult to generalize from. Groen concludes with an analysis of Copeland's book for elementary school teachers on the teaching of mathematics. He claims Copeland gives a one-sided view of Piagetian theory that emphasizes its static aspects and that tends to confuse mathematical structure with Piaget's more dynamic view of structure.

One might reasonably conclude from Groen's assessment that Piaget's ideas had not had much influence upon mathematics teachers. A more valid conclusion is that Piaget's ideas, as the teachers understand them, have had a profound impact, but this impact is often difficult to discern clearly. In countless classrooms today, mathematics teachers are dealing with children and teaching their subject matter in the light of what they believe to be Piaget's ideas. It is part of the professional baggage they picked up in college that is still with them, and it is heavily reinforced by the professional culture in which they live. Although Groen apparently could not find much of an overt Piagetian influence on mathematics education, the influence has been substantial, but largely covert and indirect.

Let us consider a final example of the influence of theory on practice in mathematics eduction. Several years ago, Stake and Easley directed a series of case studies of science and mathematics teaching for the National Science Foundation [see Fey, 1979]. They found a number of secondary school mathematics teachers who offered, as justification for teaching their subject, the argument that the study of mathematics improves one's ability to think logically:

> "I can teach them to think logically about real problems in their lives today"
>
> "Mathematics can teach the student how to think logically and that process can carry over to anything To be able to start with a set of facts and reason through to a conclusion is a powerful skill to have" [quoted in Fey, 1979, p 498]

These teachers had clearly rejected Thorndike's findings concerning the lack of transfer of the disciplines — if indeed they had ever heard of these findings — and had adopted a view that has echoes of faculty psychology Presumably this view was not dominant in their preservice education program, which doubtless gave them much sounder, more scientific justifications for the teaching of mathematics These justifications either had not survived or had never been accepted The educational psychology textbooks are fairly clear on this issue: one cannot train logical reasoning ability through specific school subjects like mathematics This is part of the received wisdom of the school-of-education culture, and these teachers must have been taught it. We have here a case in which current theories have not had much impact on teachers' thinking, and presumably their practice.

These three examples are intended to illustrate some of the various and perhaps perverse ways in which theory influences practice in mathematics education As Kerlinger and others have noted, the influence is primarily indirect Unless someone forceful and dominant such as Thorndike acts on the system, one must look hard to detect how the influence is occurring A common procedure is for the theorist to set forth his views and then for a transmitter, such as Copeland, to provide a simplified, and perhaps somewhat garbled, version for a larger public of teachers. The transmission network, however, is complex A Piaget introduces a new idea, which resonates for someone else, who incorporates it into a talk, paper, or book, and other mathematics educators begin to use it in their speaking or writing Gradually, the idea comes into the culture of mathematics education and is picked up by teachers in practice. Sometimes the idea is banned from colleges of education — like faculty psychology — but lurks in the culture like a virus to strike down the receptive practitioner

Sometimes the force of theory is felt merely by providing a name for a construct that people have been grappling with but have not articulated. Attribution theory and expectation theory seem thus far to have contributed to mathematics education in this fashion; researchers in mathematics education are intrigued by the constructs, but they have not been much concerned with following out the ramifications of the theories Naming, however, is a powerful force, as Adam must have discovered. Hadamard [1947], in discussing Newton's contributions to the calculus, said it aptly:

> The creation of a word or a notation for a class of ideas may be, and often is, a scientific fact of very great importance, because it means connecting these ideas together in our subsequent thought [p. 38]

Pimm [1981, p. 48] quotes Higginson's anagram, "renameing is remeaning", from which it follows that "nam(e)ing is meaning". We need the constructs and networks of theory to help us think about things — about the phenomena we confront as mathematics educators We ought to be giving more serious attention to the theoretical underpinnings of our work, and we need to make more explicit and coherent the assumptions we are making, the point of view we are adopting, and the frame of reference that surrounds the picture we are trying to paint. As long as we ignore the theoretical contexts of our research work in mathematics education, it will remain lifeless and ineffective.

Teachers as participants in research

Consider now the teacher's role in research. First, we should quickly note that "research" should be given the broadest possible connotation; we should not limit it to controlled experimentation or even to empirical research, as is often done Research in mathematics education should include historical studies, philosophical studies, and analyses of curriculum topics, as well as surveys, case studies, clinical studies, and the like. What makes a study research is not the methodology but the attempt to be systematic and to put the study in a larger context of theory, if possible. (This is true even for what some would term "applied research") "Disciplined inquiry" is perhaps a better term in some respects than "research" since it emphasizes the process and not just the form.

From this perspective, one can see that much of what mathematics teachers do every day comes close to being research; it is just not quite so deliberate, systematic or reflective As Alan Bishop [1977] has pointed out, teachers can borrow three things from researchers: their procedures, their data, and their constructs. What do researchers do when they do research? If they are conducting an empirical study, they might observe, formulate hypotheses, observe some more, and try to test their hypotheses If possible, they try to vary the situation systematically to see what the effects of variation might be They formulate constructs and models of how these constructs might be related, and then they gather data to test the constructs and models They develop instruments to help them gather data. These are activities that teachers can do They can borrow these procedures and use them to study their own teaching. They can also borrow researchers' data As Bishop points out, you do not have to gather data yourself for them to be of value to you The value of data is in the process of understanding and interpreting them Teachers can interpret the data from a research study in the light of their own situation and experience.

Teachers can also borrow a researcher's constructs and the accompanying models and theories Bishop refers to the work of George A. Kelly, the developer of the psychology of personal constructs, which is a theory of personality functioning. In Kelly's theory, we are all researchers, creating constructs as interpretations of our world and testing the predictive validity of these constructs. When we teach, we are concerned with the students we are teaching and with the ideas we are trying to teach them Our behavior is shaped by the constructs we have about the students and the ideas. The students, in turn, have their own constructs about us and about the ideas as they understand them. Kelly sees behavior as an experiment. To understand a child's behavior, says Kelly, try to figure out what question she is asking of the world. What hypothesis is she attempting to test? To change the child's behavior, try to figure out ways of getting her to form new constructs. To change one's own behavior as a teacher, try to create alternative constructs for interpreting the world. If you cannot create such constructs, try borrowing some. The great value of the work of theorists such as Piaget, Dienes, and Gagne is in the interpretive lenses they give us for looking at familiar phenomena in new ways.

Too many mathematics educators have the wrong idea about research. They give most of their attention to the results. They think it is primarily important for teachers to know the results of the research on a given topic. They give a high priority to summarizing and disseminating research results so that teachers can understand them. In a nontrivial sense, however, the results are the least important aspect of a research study. Note that Bishop did not include results among the things to be borrowed from researchers.

The most important aspect of a research study is the constructs and theories used to interpret the data. A landmark research study is one that confronts us with data analyzed and organized so as to shake our preconceptions and force us to consider new conceptions. A researcher makes a contribution to our field by providing us with alternative constructs to work with that illuminate our world in a new way, and not simply by piling up a mass of data and results.

This view suggests why teachers should be active researchers, why they should develop a research attitude. Teachers should not stop at being borrowers; they should become collaborators. Research is not something to be left to people who understand randomized block designs and analysis of covariance. Research in our field is disciplined inquiry directed at mathematics teaching and learning. It is stepping out of the stream of daily classroom experience and stopping to reflect on it. It is becoming conscious of the constructs we are using and then trying other constructs on for size.

Research in mathematics education has increasingly been moving out into the classroom. This has been, in general, a healthy move. It would be better, however, if teachers were working more closely with researchers in formulating their problems and interpreting their findings and not simply in helping them gather data. The teachers would benefit, with respect to both their professional attitudes and their effectiveness, and so would the researchers.

Sanders [1981] has suggested that in no other profession is the community of researchers more sharply differentiated from the community of practitioners than in education. Researchers tend to identify with, and publish for, communities that do not include practitioners, and vice versa. The self-correcting mechanisms of science, however, require that the knowledge it claims is reliable be presented to a community of peers for review and correction. If incorrect or incomplete theories become institutionalized, asks Sanders, where will the impulse for correcting them come from? At present, the theory builders in our field, such as they are, do not see the consequences of their ideas in prectice; and the teachers, who have been trained to depend on experts for answers, have little impetus to correct these ideas and improve their own understanding.

Certainly the interests of the teacher and the researcher are not necessarily congruent. Neither one should expect too much from the other [Phillips, 1980], but this by no means invalidates the argument that each can profit from a closer association with the other.

A contrast between mathematics and research in mathematics education

Hamming [1980] offers four partial explanations for the effectiveness of mathematics that may help to explain further the ineffectiveness of research in mathematics education. Hamming argues, first, that we see what we look for; "we approach ... situations with an intellectual apparatus so that we can only find what we do in many cases" [pp. 88-89]. The phenomena we see arise from the tools we use, and mathematics has been highly creative in inventing tools. Research in mathematics education, on the other hand, has not. Hamming relates a parable he attributes to Eddington: Some men went fishing in the sea with a net and, upon examining what they caught, found that there was a minimum size to the fish in the sea. In research in mathematics education, our nets have been rather coarse; our instruments, rather blunt.

Second, we select the kind of mathematics to use, and "it is simply not true that the same mathematics works every place" [p. 89]. When the mathematics we have does not work, we invent something new. Hamming gives the illustration of how, when scalars did not work for representing forces, vectors were invented, followed by tensors. In research in mathematics education, we have not shown the same ingenuity in adapting our tools to our problems.

Third, science in fact answers comparatively few questions. "When you consider how much science has not answered then you see that our successes are not so impressive as they might otherwise appear" [p. 89]. If one considers the questions associated with truth, beauty, or justice that mathematics cannot answer, says Hamming, one sees that almost none of our experiences fall under the domain of mathematics. Applying this same argument to the realm of research in mathematics education, one concludes that perhaps mathematics educators have not recognized the limits in the classroom to the kinds of questions that research might be able to answer. Perhaps one reason for the perceived ineffectiveness of research in mathematics education is that too much has been expected of it.

Fourth, Hamming contends that the evolution of man has provided the model for mathematics by selecting for "the ability to create and follow long chains of close reasoning" [p. 89]. We have been, to some extent, selected according to the models of reality in our minds. For example, we think very well about problems pertaining to things that are about our size, says Hamming, but we tend to have trouble if the problems concerns very large or very small things. Just as there are some light waves we cannot see and some sounds we cannot hear, perhaps there are some thoughts we cannot think. Although evolution has not had much chance to operate over the few generations of scientists in the history of science, perhaps there has been some selection for the ability to follow chains of reasoning The history of research in mathematics education is much shorter, and evolution has not had time to select for a research attitude. Many people who do research in mathematics education have, in fact, been "selected" — or have selected themselves — because of their mathematical abilities These abilities may not be, and probably are not, the same abilities that are needed for effective research in mathematics education

Davis and Hersh [1981] argue that the Platonist, formalist, and constructivist views of mathematics are no more than different ways of looking at the same thing. They use the analogy of how one can sit at the console of an interactive graphics system and learn about a hypercube by looking at pictures of the hypercube, rotating it so as to see how one view transforms into another. The viewer gradually builds up a comprehensive view of the thing itself out of the various partial views displayed. Similarly, one can build up a picture of mathematics itself by integrating the various pictures of it that are offered by the various philosophies of mathematics Research in mathematics education may also be something like the hypercube, except that we are just beginning to note various views of it Partial views are offered in several recent sources such as Begle [1979] and Shumway [1980], but a comprehensive image remains elusive

The parallel between mathematics and research in mathematics education ought not to be pushed too far, however Applying educational research to mathematics teaching practice is not an engineering problem like applying mathematics to a practical situation. For too long researchers have been misled by this engineering metaphor The improvement of mathematics teaching is not a technological problem; it is a human problem Kristol [1973], writing about the inability of the reforms of the 1960s to have much impact on the educational process, put it this way:

> There are some who will say that this state of affairs merely shows how obstinately conservative our "educational establishment" is. I think this misses the point. When there is so much will to change, so much dedication to effecting change, and so little effectual change, the more reasonable conclusion is that we are dealing with a network of human relationships that does satisfy, if only in a minimal way, certain basic societal needs, even if we don't quite know why or how it does.. That this should surprise us indicates how deeply our thinking about all subjects has been suffused with the technological mystique. We are inclined to believe that our power over nature and humanity is, or ought to be, limitless We tend to assume that the will to transform our human condition is a sufficient condition for such a transformation to occur Everywhere, we hear the refrain: "We can go to the moon, can't we? Well, why can't we do something equally marvelous about the ghettos or education or whatever?"
>
> The answer is, of course, that going to the moon is easy whereas improving our system of education is hard. The one is nothing but a technological problem, the other is everything but a technological problem.
>
> Doing something about education means doing something about people — teachers, students, parents, politicians — and people are just not that manipulable. They are what they are and do not become new people to suit any new ideas we might have [p. 62]

If researchers in mathematics education are to become effective in improving the practice of mathematics teaching, they should: (1) develop a stronger sense of community, which would include practicing teachers as collaborators in research; (2) create their own theoretical constructs for viewing their work; and (3) recognize the limits of their domain as well as its complexity.

Note

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