A RESEARCH BASIS FOR

MATHEMATICS EDUCATION

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Report No. 49
November 10, 1969

STRUCTURAL LEARNING SERIES

Mathematics
Education
Research
Group

Graduate School of Education
University of Pennsylvania
Philadelphia, Pennsylvania

Published in The High School Journal, 1970
A RESEARCH BASIS FOR MATHEMATICS EDUCATION

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For the past fifteen years, most of the research activity in mathematics education has been concentrated in curriculum development. During that time, only a small handful of people have been actively engaged in basic research on mathematics learning.

Today, the situation is changing rapidly. While there are still only a few centers actively engaged in fundamental research on mathematics learning, mathematics educators are turning, more and more, to basic research as a basis for further development. This is, it should be noted, in direct contradistinction to the earlier views of some of those engaged in curriculum development -- those who made a special point of downgrading the importance of basic research.

In this paper, I shall: (1) attempt to explain and account for the new interest in research in mathematics education during the last few years, (2) identify some of the kinds of information which every good mathematics teacher needs to know, (3) describe some of the basic research currently underway or being planned at the University of Pennsylvania, and (4) describe some of the teacher education materials we have developed which are based largely on this (basic) research.

*I would like to thank Christopher Toy for his general assistance in the preparation of this paper. His participation was made possible by a graduate research training grant to the author by the U.S. Office of Education.
Unfortunately, space prohibits discussion of any of these topics in the
depth they really deserve. I do, however, hope to convey two main points
which recur repeatedly throughout this paper: (1) the tremendous breadth and
complexity of the problems which are involved and (2) the great promise of
current and future research as a basis for solving these problems.

(1) WHY THE INCREASED INTEREST IN BASIC RESEARCH IN MATHEMATICS EDUCATION?

Let us first turn to the question of why the increased interest in
basic research in mathematics education. We can answer this question best, I
think, by tracing the history of basic and developmental research in this
country over the past four decades.

It seems that successful scientific and technological development re-
quires the presence of two vital ingredients: first, an adequate scientific
base usually obtained through basic research; second, adequate financial
and social support, most frequently by governmental agencies.

As a case in point, consider the space program. Here, a well worked-
out scientific base goes as far back as Newton's Theory of Mechanics and
includes, as well, Goddard's more recent work (1922) on liquid-fuel rocket
technology. Yet, in spite of the ready availability of this ground-breaking
work, full-scale development of space technology didn't come about for many
years. Such development began only after considerable governmental pressure and
concomitant economic pressure were brought to bear. Thus, the U.S. did not move
into space seriously until the launching of Sputnik by the Soviet Union was
coupled with the supposed "missile gap" of the 1960's. In short, it was
public pressure and continued Soviet accomplishments that prompted President
Kennedy's pledge for a moon landing before 1970.

And today, almost a decade later, after we have seen the spec-
tacular achievement of Apollo 11, it is questionable whether the nation is
willing to commit itself singlemindedly to landing men on Mars in the near future, despite the technological feasibility of such a goal. At the present time, domestic issues have taken priority with a large percentage of the American public and, as a result, much of the money that would normally be earmarked for space projects will undoubtedly be diverted to other areas.

In the late nineteen-fifties, the situation in mathematics education was much the same as it was in space research. Sputnik also gave realization to the American people that mathematics education in this country was woefully inadequate.

Furthermore, fundamental advances in mathematical research in the previous fifty to one-hundred years did, in this instance, provide a more than adequate scientific base for revolutionizing the content of school mathematics. (It is worth noting in this regard that the so-called "new mathematics" was not an invention of curriculum developers of the last decade.)

More recently, though, major improvements in mathematics education have come more slowly; it has been much harder to come up with new programs which are really better than what we already have. At best, the most recent programs have simply been refinements of other programs and, at worst, they have been unrealistic and/or philosophically indefensible. Some educators, for example, would have us teach high school students the same material that is at the present time offered in the junior and senior year to students at our better universities. Clearly, if one is dealing with extremely gifted students, this idea is completely feasible and perhaps even desirable. But, it can only be applied to the teaching of our more numerous "top twenty percent" by overemphasizing the relative role of mathematics in the high school curriculum.
One obvious reason for the slowdown is the economic pressure of the Vietnam war. This has resulted in greatly decreased support for research and development and has made it extremely difficult, if not impossible, to maintain the pace of the late fifties and early sixties. Perhaps the most fundamental reason, however, is that we lack an adequate base in the behavioral sciences (and educational philosophy) for making significant further improvements in curriculum development and, more specifically, here in teacher education.

In effect, when adequate basic knowledge is available, the relative gains from developmental activity are likely to be greater than those from basic research. However, after development has progressed for a period of time, the payoff from basic research is likely to be much greater. Figuratively speaking, developmental activity, without basic research, is much like living it up on past savings without any concern for the future. This is the situation in which we find ourselves today in mathematics education. We have largely exhausted our reservoir of knowledge about mathematics teaching and must, in my opinion, begin to build up a new body of knowledge before we can expect further breakthroughs in development of the sort to which we have become accustomed. This is particularly true, I think, in the areas of teaching methodology and the assignment of values to various objectives which might be included in a mathematics curriculum.

The situation is not unlike that in the field of atomic energy where on the basis of the knowledge provided by Einstein's Theory of Relativity and the work of other pioneering physicists, like Rutherford and Fermi, scientists and engineers were able to produce in the late 1930's the world's first sustained chain reaction. Later, given the added impetus of World War II,
scientists were able to create the atom bomb. Later still, on the basis of the same basic know-how, they were able to produce the hydrogen bomb and even to go on to harness the atom for peaceful purposes. However, when it came to harnessing hydrogen power (the fusion process) the situation was quite different. In spite of the billions of dollars that have been spent on development, the field is at a relative standstill and many scientists believe that we will not succeed in taming hydrogen power until we know much more about the processes operating inside the nucleus of the atom. In other words, development, here too, is dependent upon basic research.

(2) IDENTIFYING THE INFORMATION A GOOD MATHEMATICS TEACHER NEEDS TO KNOW

We now turn to some of the basic questions which must inevitably be asked (and answered through research) if teacher education in mathematics is to progress from its present sorry state. To conserve space, I shall not attempt to deal with the important subject of practical experience.

I will focus, instead, on the intellectual aspects of teacher education. This does not mean that I am suggesting (or feel) that practice teaching, micro-teaching, or classroom observation, for example, can be dispensed with, but rather that, while work in these areas is being actively pursued, there is very little work going on which deals with the conceptual aspects of teacher training. So, I will put my emphasis there.

The basic question we want to ask here is, what is it that the teacher needs to know? Now, it is obvious that the teacher needs to know something about mathematics — but, what mathematics? What should the level be? The emphasis? Should we teach arithmetic or number theory? Geometry or topology? Questions like these, about which we all have intuitive feelings, have, I fear, hardly begun to be dealt with in a systematic way.
Clearly, the teacher also needs to know something about teaching methodology. In this case, we are even worse off, however, because it is not merely a question of what methods to teach but whether there is really anything new known about methods which is worth teaching.

In the recent past, we have tended to emphasize such things as the history of modern curriculum development in mathematics or tended to make vacuous statements about how to motivate children or how to use audio-visual aids. About the best we have been able to do is to talk about particular approaches to the teaching of specific topics such as the multiplication of signed numbers to specific kinds of students. The alternative has been to espouse misleading doctrines concerning discovery teaching, inquiry training, or the like.

While I do not quarrel with most of these ideas themselves, I do question whether this sort of approach really helps teachers to understand what mathematics education is all about. I have observed far too many classroom teachers who talk about such things as commutative and associative laws without having the foggiest notion of what all of this might mean in the broader context of mathematics education. Many of our more recent graduates -- even our so-called better trained elementary school teachers -- are merely replacing one set of terminology with another. And, this is certainly not mathematics teaching at its best. I feel that we must do better if we are ever going to provide our children with the kind of education we would all like them to have.

To do this, the first thing we must do, in my opinion, is to avoid
the archaic and artificial dichotomy which separates instruction in mathematics, per se, from the methods one might use to teach mathematics. (This dichotomy originally developed because of poor communications between departments of mathematics and departments of education at our universities and colleges. Too frequently neither department has been willing or qualified to do the work of the other. However, I am happy to say that as more and more well-trained mathematics educators enter the field, this communication gap is slowly closing.)

It is of as little value, to give elementary school teachers empty statements about nonexistent theories of instruction as it is to teach them high-level mathematics which they cannot fathom. And it is still worse to separate the mathematics that teachers learn from the methods they will use to teach others. For one thing, teachers who learn mathematics in isolation from methodology frequently have considerable difficulty translating the material into a form which can be taught to children.

Furthermore, there is simply not much that one can say, at present, about methods which do not include specific content. Even if general principles are found -- and I think that some will be -- the teacher must still learn how to apply those principles to specific mathematical topics.

What we need to do is to conceive of the teacher's job in more operational terms than we have in the past. We should, in fact, reformulate our question of what the teacher should know to read, "What are the capabilities which the teacher needs in order to teach mathematics effectively?"

In this context, consider the subject matter. In my opinion, the teacher
must know, in fairly explicit terms, what kinds of mathematical behavior to expect of her students. She needs to know, too, something about the competencies required to elicit these behaviors. This will involve such things as the ability to compute (in arithmetic), the ability to make simple discoveries and, even the ability to prove a simple theorem. At the present time, there is relatively little that is known about these things.

For instance, the question of how best to characterize the knowledge which underlies some given universe of behaviors associated with a particular subject matter like mathematics has hardly been asked. It is true that some promising beginnings have been made in the field of formal linguistics, but, again, almost nothing has been done in mathematics or any of the other school subjects.

In addition to knowing something about the general nature of mathematical knowledge and mathematical behavior, I feel that the skilled elementary school teacher also needs to know the objectives of elementary school mathematics and to know how they relate to the rest of the curriculum. For another thing, the teacher should have some rationale or general set of criteria for selecting and modifying the objectives in any given curriculum. The goals for a good mathematical education in affluent suburbs may be very different from the objectives in the inner city slums of our metropolitan centers or in farm communities across the country. In short, what might be a sound curriculum in Palo Alto, California, or Scarsdale, New York could be untenable in North Philadelphia or Zap, North Dakota. The basic question is how and why these objectives should change. We need a general philosophical framework within which to make such changes and modifications in curriculum. Without such a framework, we can only state opinions and are
unable to subject those opinions to the scrutiny of others.

Teachers also need to know more than a grab-bag of teaching techni-
quess and interest-getting devices. They need to acquire abilities which pertain
to the teaching and the learning of mathematics generally. And, they need
practice in the application of these principles to a wide variety of mathem-
atical topics.

For example, teachers should know something about how to identify oper-
ationally the objectives in text materials and exercises. Many textbooks
simply do not coordinate their content with the exercises they offer. While they may talk a good deal about teaching students how to discover, for
example, their exercises often amount to little more than simple applications of
what is discovered.

Another ability which all teachers should have is the ability to identify
the prerequisites for learning any particular task. The teacher should
be able to determine in logical, systematic steps what it is that the student
needs to know before he can be expected to learn or perform the desired
task. Figuratively speaking, the teacher ought to be able to answer the
question of "why Johnny can't multiply." What we want, then, is a super-
diagnostician who can identify the source difficulties under any and all
circumstances.

In addition, teachers should have the ability to assess (determine) the
knowledge that a given student has. Unless the teacher is able to construct
test items which will get precisely at what particular children know, she
can never determine for sure whether her students have the necessary requisites
for achieving what she wants them to achieve. Indeed, she will be unable to
determine whether, in fact, they have learned what she intended to teach them.

The final ability we shall consider is the ability to motivate, the
basic requisite for all learning. To date, the best we have been able to do
in this area is to give the teacher specific techniques for teaching specific topics or, more specifically, for motivating children to learn these topics. Sometimes we give the teacher general advice like, put the mathematics in the context of a game or, make up problems which deal with things in which the child has an interest. Although advice like this sometimes succeeds, it fails as often.

There is a real need, I think, to talk more about basic principles and, even more important, to provide the teacher with a general frame of reference for thinking about motivation. Hopefully, the teacher will then be in a better position to make up her own mind and to evaluate her own techniques for motivating children to learn in the variety of situations which confront her everyday.

On too many occasions, I have heard professionals (like myself) throw up their hands in dismay and say something like, what we need in mathematics education is to develop "teacher proof" texts. This is nonsense, of course. Good teachers can save even the worst texts and poor teachers frequently render the best texts incomprehensible. Since we are a long way off from replacing the teacher — if, indeed, we ever should — we must train teachers to use textbooks effectively so that the textbooks do not use them.

(3) NEW DIRECTIONS FOR BASIC RESEARCH IN MATHEMATICS EDUCATION

Obviously, we do not have all of the necessary information. There is much we need to know. I am happy to say, however, that progress is being made and that the future looks quite promising indeed.

Rather than attempt to review all of the research which bears directly or indirectly on mathematics education, I will limit myself in this paper to the research in which I have taken part or have discussed elsewhere.
my completed research on the learning of mathematical rules, the ability
treatment-interaction question and problem solving so there is no need to
go into that here.* Instead, what I would like to do is to describe some
of the things that we are now doing and plan to do as part of our new research
project in mathematics and structural learning.** While I can do no more
here than simply scratch the surface, I would like to call particular attention
to the new directions we plan to follow.

One of the major concerns of the project is the problem of how to
characterize mathematical knowledge. By that I mean that we want to know
how to account for the behaviors which knowing mathematics makes possible.
And, we want to account for these behaviors in a way which is both (1) mathematically viable — that is, compatible with the way mathematicians think
about their subject—and (2) behavioral in nature.

While very little research has been done on this problem directly, there
is much to be learned, I think, in this regard by considering some of the
limitations of related research. Current work on operationalizing objectives, for
example, obviously deals with behaviors, but the research has a distinctly
post hoc flavor and does not provide a viable characterization of contemp-
orary mathematics. This is true not only because of the almost exclusive
emphasis on what might be called "rote rules" — but, also because very little

*For a summary of much of this research see my recent articles in Acta
Psychologica: New Directions for Theory and Research on Role Learning.
I. A Set - Function Language. 1968, 28, 301 - 321. II. Empirical
Research 1969, 29, 101 - 133. III. Analysis and Theoretical Direction 1969,
29, 205 - 227.

**This research is being supported through a grant from the U.S.
Office of Education to a committee of the National Academy of Sciences for
basic research in education.
Attention has been given to such questions as how to characterize the higher order intellectual skills involved in actually doing mathematics as well as how to characterize knowledge of the obvious relationships which we know exist between different mathematical rules. I shall say more on this below. Furthermore, no distinction has been made between the behaviors a subject might elicit, and the knowledge which might account for that behavior. This is an extremely important distinction in planning a curriculum because there are many different ways in which a person might learn to elicit any given class of behaviors. The specific form of the curriculum itself will depend, for example, on whether we want to teach children to subtract, say, by borrowing or by the method of equal additions. Similarly, we can teach the so-called three cases of percentage separately or as simple variants on a common theme.

A second line of research related to the problem of characterizing mathematical knowledge, is associated with formal linguistics. The methods that linguists use to analyze language, that is, to identify the competencies underlying language behavior, are very similar to the kind of methodologies we need to use to identify the competencies underlying mathematical behavior. The specific kinds of competencies which are most critically involved in language behavior, however, are simply not the same as those associated with mathematics. For example, although linguists such as Chomsky have argued the need for higher order competencies, which deal with relationships like that between passive and interrogative forms of a sentence, there is by no means uniform agreement on the point. One could hardly find a mathematician on the other hand, who did not feel that such higher order relationships are the very essence of mathematics -- not to even mention what appear to be important differences in the nature of these
higher order competencies.

To make matters worse, linguists have expended most of their research efforts at the syntactic level, that is, with grammatical relationships between, for example, words and sentences treated as entities themselves, and have almost disregarded the level of semantics or meaning. In ordinary mathematics, one can hardly get started without having to deal with both levels.

Thus, while it is true that we can gain many valuable insights into our problem from the methods of research used in formal linguistics, it is equally clear that we must be ready to extend and even depart from that tradition where necessary.

There is also much to be learned from work in the foundations of mathematics, but, here, we must be even more careful. Specifically, in the foundations of mathematics the basic problem is one of clarifying the nature of the relationships between different kinds of mathematical objects. The sine qua non of research in the area is a formal and precise characterization of the mathematical objects in question. Now, this characterization, as it is usually expressed, is designed primarily to make (do) the (meta-) mathematics easier and has little to tell us about the way in which one might characterize mathematical knowledge.

Although I have been working in this area for a number of years, it is only recently that I have begun to feel that we are finally beginning to get at the heart of the problem. Stated simply it is how can we account for complex mathematical behaviors; more directly to the point, how can we evaluate alternative characterizations of the same behaviors? The truth of the matter is that there may be any number of ways of accounting for the same behaviors. The important thing is that certain accounts are undoubtedly better than others, and our goal, obviously, is to come up with
the best one possible. For example, one might conceivably characterize a given mathematical curriculum by listing all of the separate competencies which can be identified. One competency, for example, might make it possible to add and, another, to find the areas of certain geometric figures.

This is basically the approach taken by those who would reduce curricula to sets of discrete operational objectives. There may be other ways of accounting for the same behaviors, however, which are in some sense both more powerful and/or more parsimonious. By "more powerful," I mean that the set of competencies makes it possible to generate more of the desired behaviors than the original (set). Even where two different sets of competencies account for all of the initial behaviors, one of them may be more powerful in the sense that it accounts, in addition, for behaviors outside the initial class. And by "parsimonious," I mean that the characterizing list of competencies is in some way shorter or more highly organized and interrelated.

Suppose, for example, our original aim was to account for the ability to add and the ability to subtract. In this case, one might simply identify one competence which accounts for addition and another for subtraction. Another way of dealing with the same behaviors, however, might be to introduce the same competency for addition and a higher order competency which deals with the relationship between any binary operation of which addition is just one example, and its inverse. Here, we would get subtraction "free", so to speak, since it can be generated by applying the higher order competency to addition. More important, given any other binary operation whatever, such as performing one permutation on a set of objects followed by another, one can automatically generate (account for) the inverse capability as well. In effect, while each characterization contains two comp.
etencies, the latter is far more powerful since it allows one to generate behaviors outside the original class in a way which the former never could.

Although we have hardly begun this line of research, it is already clear that current approaches to curriculum development, based on operationalized objectives, are entirely too fragmented. Most such curricula consist of nothing more than long lists of discrete objectives. While we must be careful not to underestimate the difficulty of the task, I would contend that curriculum development and teacher education, to name just two areas, cannot help but gain from systematic research along the lines just described.

While on the subject, I might also discuss some of our work on identifying mathematical processes. Let me first say what I mean by a mathematical process. A mathematical process is a (usually very general) intellectual skill (or competency) which while essential in doing mathematics is not normally considered to be part of mathematics (content). The fact that such skills act behaviorally in very much the same way as other competencies was shown in a recent study by Roughead and Scandura.* In that study, it was shown that the skills subjects learn by discovery can sometimes be specified in details and presented directly by exposition with equivalent results... I would also like to say something about a scheme we have developed for classifying such processing skills but space does not allow and I must refer you to my forthcoming book which deals with the problem.

It appears that all such skills may be classified in one of four bidirectional and mutually exclusive categories.

The first is the ability to detect regularities and the inverse ability of particularization. The second is the subject's ability to describe what he knows and its inverse of interpretation. The third is deductive reasoning. This category involves drawing inferences, on the one hand, and, on the other, axiomatization -- the ability to identify key ideas (in some class of ideas) from which all others may be deduced. Finally, we consider storage and retrieval processes which are associated with memory. We are just beginning a major project in this area aimed at identifying and determining the utility of specific processing skills and I hope to have more to say on the subject in the next couple of years.

Another area in which research is badly needed concerns the development and application of criteria for selecting from among the large number of available topics in mathematics those which are most appropriate for any given group of students. This is a very difficult problem area in which to work because, in addition to raising mathematical questions about internal consistency, and behavioral questions concerning feasibility, it raises some very basic questions of priority and value. We are just beginning to explore this problem, but one thing has already become clear. There are many paradoxes that we are going to have to deal with. For example, we are going to have to resolve such dilemmas as this: Although it would obviously be desirable to both maximize the learnability of the material we present and, at the same time, maximize the generality of the same material, these two goals are not compatible.
They clash head-on with some of our own research findings which tell us that in order to maximize learnability one must increase specificity, and vice versa. Therefore, any decisions we make along these lines will necessarily be value decisions. Though this is still largely virgin territory, it is an area of great importance and we are hoping to interest "enlightened" educational philosophers in our work.

All things considered, we are probably most deeply engaged in work in still a third major problem area. Simply, the problem is to determine how mathematical knowledge is put to use and, how such knowledge is learned in the first place. Here, again, I break sharply with tradition. I do not feel that applied educational research, for example, while of proven value in dealing with certain kinds of problems, is going to tell us very much about how mathematics is learned, no matter how well designed the studies may be. As a basis for this statement, I can refer to my dissertation, The Teaching-Learning Process: An Exploratory Investigation of Exposition and Discovery Modes of Problem Solving Instruction, Syracuse University, 1962.

There, it was shown that very minor within-method differences can have a greater effect on experimental results than differences in the basic methods themselves. Even more important, the basic variables that seemed to be involved are not subject to manipulation in the usual way. Different research methods are called for.

I reject, as well, current theorizing in so-called "academic" psychology. For one thing, the type of theory that people have been concerned with in this area is essentially probabilistic -- that is, rather than attempting to make specific predictions about the way individuals will act in

given situations, such theories talk, instead, about the probability of occurrence of various events. While this is worthwhile information to have, it is not the sort of thing we need most to know, say, in trying to teach students how to solve quadratic equations.

The problem, as I see it, is that theories of this sort treat everyone on the same plane. First, assumptions are made about how individuals do things. Then after introducing random variables into the picture, predictions are made concerning group statistics, such as means and variances. Next, experiments are run with groups and the results are compared with the predictions. So far, so good. It is the last step that causes the difficulty. As a result of the comparison, inferences are typically drawn about how individual subjects actually do things. *

While this may be a reasonable sort of assumption to make with "naive" rodents or with human subjects, when dealing with simple reflex behaviors, it simply does not apply to the learning of more complex material like mathematics. What humans do on any non-trivial situation depends in a very direct sense on what they already know and can do. Any attempt at theorizing which ignores or otherwise bypasses individual differences, cannot, in my opinion, hope to provide a viable model for education.

By own present approach goes in quite a different direction. Rather than treating individual differences as unwanted or uncontrollable experimental variables, I have, during the past year and a half, been heavily engaged

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*This basic fallacy is widely recognized in psychology, but for one reason or another, few psychologists seem sufficiently concerned to try to do anything about it. Apparently, it has been the sort of thing which is much easier to sweep under the rug and forget.
in deterministic theorizing about individual processes. Obviously, this is not the place to go into detail about such matters but let me at least outline some of my concerns. On problem areas in which one of my doctoral students, John Durnin, and I have been deeply engaged concerns assessing the behavior potential of individual subjects. More particularly, we are attempting to determine which items in a given class a subject will be able to perform successfully on and which he will not, on the basis of his (prior) performance on a small finite number of test items. At a strictly theoretical level, we have been able to prove theorems about the number of test items needed, with respect to any given class, and, even more important, how to go about selecting these test items. At the time of this writing, we have just begun empirical work on the problem but the results, so far, seem extremely promising.

Another line of research is concerned with the problem of explaining and predicting "what a subject will do next." More particularly, given some general context, relative to which a subject has learned a number of relevant rules, the question is why does he select the rule that he does use? This turns out to be a very complex problem and, oddly enough, it turns out to have much in common with the perennial problem of motivation. Although our work in the area is also just beginning, preliminary evidence suggests that, in general, subjects tend to select the path of "least resistance." The major problem, of course, will be how to characterize a path of least resistance.

The final line of research I will mention concerns the mechanisms that people use to solve problems. To date, most of my work in the area has been of a theoretical nature, but by a stroke of luck, it turns out that one of my earlier experiments** bears more or less directly on the major hypothesis.

*Francis E. Ruicott is working with me.
**Scardura, J. M., Learning verbal and symbolic statements of mathematical rules. 1967 (Journal of Educational Psychology. 58, p. 135).
involved. Loosely speaking, the hypothesis is that if a subject does not have a rule available for achieving a given goal, central automatically shifts to the higher-order goal of defining such a rule. While it is basically a very simple idea, this hypothesis has provided an adequate base for analyzing some fairly involved mathematical problems, such as those discussed by Polya in his many books on mathematical problem solving. Needless to say, we are planning further work in the area.

In doing this research, I have found it useful to distinguish between what might be called "memory free theorizing", such as that described above, and theorizing which involves, as well, the limited capacity of human subjects to process information. This distinction has helped lead me to the beginning of what seems to be a rather simple (but apparently adequate) theory of complex learning and behavior. Although it is much too soon to determine the ultimate value of such theorizing, I am hopeful that research of this kind will have very important implications, both for the sort of future research that will be done in psychology and more importantly, for educational practice in our schools.

(4) SOME MATERIALS FOR TEACHER EDUCATION DERIVED FROM RESEARCH

While we have barely scratched the surface in this research, our theorizing has motivated several very practical projects in teacher education. Our first project is aimed at content for elementary school teachers. The materials we have developed are different from existing materials in that they emphasize what might be called the concrete behavioral foundations of mathematics. Furthermore, they integrate certain aspects of methodology with the content in a rather unique way. In particular, they call for what might be called mathematical processes or higher-order intellectual skills, which are involved in doing mathematics, but which are not typically associated with mathematical content.
The processes we have singled out fall into three of the general categories mentioned above. The first is the ability to detect regularities. The second is the ability to reason deductively and the third, is the ability to interpret verbally presented information — or to learn by exposition. The text we have prepared develops each of these processing skills individually in the first chapter. Then the reader is asked to apply these skills in working with the mathematical content in the remainder of the book.

Chapter 2 Fundamental Mathematical Ideas
sets, relations, functions and special cases thereof

Chapter 3 Logic and Set Operations

Chapter 4 Mathematical Systems, Theories, and Relationships between Systems

Chapter 5 The System of Natural Numbers

Chapter 6 Systems of Numeration and Arithmetical Algorithms

Chapter 7 The System of Positive Rationals

Chapter 8 The System of Integers

Chapter 9 The System of Real Numbers and Further Extensions

The final chapter ends, showing how algebra, the study of discrete entities, meets up with geometry, the study of continuous variation, through the limit process.

This text is essentially complete and is being used in one of our classes experimentally before the final version is published.

We are, at the moment, involved in fine-tuning the text and developing exercises for it.

Most of the material in the book is traditional and, yet, the book is not
traditional in many ways. For one thing, it is the first attempt that I know of to discuss mathematical processes in a systematic way. Furthermore, it is also the first attempt to apply such processes to a wide variety of mathematical content. The text and the exercises are designed to have the reader learn the mathematical processes which are useful in the various content sections by actually identifying and using such processes. There is in the book, too, a strong emphasis on the relationship of mathematics to the real world, an emphasis that does not exist in other texts for elementary school mathematics teachers.

We have also tried to emphasize the relationship between the various mathematical topics treated in the book. That is, we have attempted, in our presentation, to build on the similarities and differences between the various topics. This, I think, not only gives the reader deeper insight into the material, but it also provides for continual review by encouraging the comparison of new material to material which has already been learned.

It should be remembered, though, that this is only a small beginning and certainly not the full flowering which I envision in this area. In fact, we are now planning a far more ambitious project which will attempt to deal systematically with each of the areas I mentioned in section (2).

In particular, we want a text which deals with the nature of mathematical knowledge. We have already done most of the needed research*, our problem is to translate these ideas into a form that will be suitable for teachers. Another goal is to identify the specific objectives of the elementary school mathematics curriculum and to devise a sound rationale for their existence.

*To be reported in a monograph I am editing on Mathematics and Structural Learning, to be published by Prentice-Hall next year.
and/or modification. Finally, we hope to identify, describe, and illustrate general teaching principles which the teacher will be able to use in a wide variety of situations. In this work, we plan to draw heavily on our own theorizing about complex structural learning. Since teaching refers to what one does to promote learning, however, we shall have to translate this theorizing (which only tells us things about learning) into a practical form that teachers can use. In particular, we need to ask what one must do in order to promote learning. In this sense, we need to look at teaching, you might say, as a mirror image of learning.¹

In outline form, then, what I have attempted to do in this paper is to (1) indicate why basic research in mathematics education is badly needed (2) to identify some of the kinds of information which every good mathematics teacher needs (3) to describe some of the basic research which we have under way and also to mention some of the implications of this research for further development in mathematics education and behavioral research generally, and (4) to describe some of our current developmental activities in teacher education, in mathematics.

In conclusion, I want to emphasize that our developmental activity has been motivated by two things: first, a concern for needed improvements in courses in teacher education and second, the conviction that basic research in mathematics and structural learning can and will provide the basis for such improvements.