TOWARDS A BEHAVIORAL PSYCHOLOGY OF MATHEMATICAL THINKING

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Some fundamental concepts that stand uncertainly on the border of mathematics, philosophy, and psychology are explored in this paper. What I have to say is not yet fully worked out, and I must ask the reader's indulgence of its preliminary character. On the other hand, if the direction in which the ideas in this paper are moving is correct, then what I have to say does, I think, have some direct significance for attitudes towards the teaching of mathematics.

There are five main ideas in this paper, discussed in the following order: the rather vague concept of *abstraction*, much used but seldom defined by mathematicians; the related topic of *imagery* in mathematical thinking; the *psychological nature of mathematical objects;* the nature of *meaning in arithmetic*, particularly as reflected in the beginning experience of children; and finally, the psychology of *algorithms in arithmetic.*

Abstraction

Some mathematicians say that the concept of set is too abstract to furnish an appropriate place for the elementary-school child to begin work in mathematics. Such a statement, I think, results from confusion about the meaning of *abstract* in mathematics. It has long been customary, although probably less so now than previously, to talk about abstract set theory or abstract group theory. To a psychologist or philosopher concerned with the nature of mathematics, it is natural to ask, What is the meaning of "abstract" in these contexts? There is, I think, more than one answer to this query. One answer is that "abstract" often means something very close to "general," and the meaning of "general" is that the class of models of the theory has been considerably enlarged. The notion of abstractness comes in because the class of models is now so large that any simple imagery or picture of a typical model is not possible. The range of models is too diverse.

In group theory, for example, one intuitive basis was the particu-

lar case of groups of transformations. In fact, the very justification of the postulates of group theory is often given in terms of Cayley's theorem, which states that every group is isomorphic to a group of transformations. It may be rightly maintained that the "basic" properties of groups of transformations have been correctly abstracted in the abstract version of the axioms, just because we are able to prove Cayley's theorem. So we can see that another sense of *abstract*, closely related to the first, is that certain intuitive and perhaps often complex properties of the original objects of the theory have been dropped, as in the case of groups, sets of natural numbers, or sets of real numbers, and we are now prepared to talk about objects satisfying the theory which may have a much simpler internal structure. This meaning of *abstract*, it may be noted, is very close to the etymological meaning.

Under still another, closely related sense of the term, a theory is called "abstract" when there is no one highly suggestive model of the theory that most people think of when the theory is mentioned. In this sense, for example, Euclidean plane geometry is not abstract, because we all immediately begin to think of figures drawn on the blackboard as an approximate physical model of the theory. In the case of group theory the situation is different. It would indeed be interesting to ask a wide range of mathematicians what is called to mind or what imagery is evoked when they read or think about, let us say, the associative axiom for groups or the axiom on the existence of an inverse? Or what sort of stimulus associations or imagery do they have in thinking about the axiom of infinity in set theory? It is my own suspicion that the combinatorial, formalist way of thinking is much more prevalent than many people would like to admit. Many mathematicians, particularly those with an algebraic tendency, have as the immediate sort of stimulus imagery the mathematical symbols themselves and think very much in terms of recombining and manipulating these symbols. In any case, knowledge of the distribution of imagery among mathematicians need not be available in order to make sense out of the presentation of simple notions like that of a set to elementary school children. The reason for this, I think, is the following.

The very character of a theory that is said to make it "abstract," in the sense of admitting a wide diversity of models, also permits an introduction to the theory in terms of extremely simple and concrete illustrations. This is particularly true of set theory. No doubt a case can be made to the effect that the general notion of set, particularly when one wishes to think of sets of high cardinality, is a difficult thing from an intuitive standpoint. This is certainly not the case when one is thinking of sets of a small finite number of elements. In introducing the child to the concept of set, we may give the most concrete and everyday sorts of examples. Objects may be picked up in the environment surrounding him, and he may be told that these constitute a collection, a family, or a set of things. This simple set of things is a good deal more concrete and vivid to him than the much more devious notion of number. Using, in fact, the distinctions given above, we can see that in one sense the notion of number is clearly more abstract than the notion of set; for twoness, for example, is a property of many diverse sets, and any particular set that has the property of twoness is more concrete and less abstract than twoness itself. By taking the next step and introducing explicit operations and notation for operations on sets, we can easily proceed to develop mathematical laws very close to those of arithmetic, but at a more concrete level. I have in mind particularly the union of disjoint sets and difference of sets, where the set which is taken away is a subset of the original set. The operation of union, thus restricted, is a complete concrete analog of addition, and the operation of difference of sets an analog of subtraction. I won't say a lot more about these matters, for I have written about them extensively elsewhere. The point of the present discussion is to emphasize that one must distinguish clearly between an abstract theory and the degree of abstractness of any particular model of that theory. I shall have more to say about abstraction in the next section dealing with imagery.

Imagery

Mathematicians classify each other as primarily geometers, algebraists, or analysts. The contrast between the geometers and algebraists is particularly clear in folklore conversations about imagery. The folklore version is that the geometers tend to think in terms of visual geometrical images and the algebraists in terms of combinations of symbols. I do not know to what extent this is really true, but it would be interesting indeed to have a more thorough body of data on the matter. To begin with, it would be desirable to have some of the simple association data which exist in such abundance in the experimental literature of verbal learning. Such association data would be an interesting supplement to the kind of thing discussed and reviewed in Hadamard's little book on the psychology of mathematics.

I tend to think of the concepts of imagery and abstraction as closely related. I could in fact see attempting to push a definition of abstraction as the measure of the diversity of imagery produced by a standard body of mathematics and stimulus material in a given population. I am not yet prepared, however, to push any particular

systematic definition of abstraction. There are many preliminaries yet to be clarified.

As one kind of investigation connected with imagery in abstraction, the following sort of modification of the standard association experiment is of considerable interest. With a standard body of mathematical material we would set students to work proving theorems from the axioms of different mathematical systems. It would, of course, be interesting to take axioms from different domains, for example, to compare Euclidean geometry and group theory. As the subjects proceeded to prove theorems we would at each step ask for their associations. Two sorts of questions would be of immediate interest. What is the primary character of the associations given? Secondly, what kinds of dependence exist between the association given at different stages in the proof of a given theorem, or in proofs of successive theorems of a given system? As far as I know, no investigations of this sort have yet been conducted, although some of the members of this conference may indeed have references to the literature that will demonstrate my ignorance. On the other hand, such experiments should not be difficult to perform and the results might be of interest.

There is one aspect of such experiments that I am not yet clear on how to handle. I have in mind the kind of thing that may arise with respect to a system like the axioms for group theory. When I use the phrase associative axiom, the association I have (not to make a pun) is just of the axiom itself written in the folowing form.

$$xo(yoz) = (xoy)oz$$

In other words, my association to the concept of associativity or to the words "associative axiom" is just a symbolic formulation itself of the property of associativity. It would be important in any investigation of associations in mathematical contexts to design the experiment in such a way as to permit this sort of association as an answer. I find these associations very prevalent in my own thinking, but I know from conversations with several mathematicians that there are rather strong differences in these matters. Let me give just one or two other personal examples as illustrations of the kind of thing I think would be desirable to pursue systematically. When I think of the general quadratic equation

$$ax^2 + bx + c = 0$$

my most direct and immediate association is the standard way of writing the solution of this equation:

$$x = \frac{-b \pm \sqrt{b^2-4ac}}{2a}.$$

On the other hand, when I mention certain geometrical notions my associations are not so verbal or symbol-oriented. For example, when I think of three points, I associate immediately to an equilateral triangle, whose sides I subjectively think of as being about 1½ inches in length. On the other hand, as I think of this triangle I also tend to add immediately after the symbol of the triangle the phrase "noncollinear" and what is interesting is that the condition of noncollinearity is not represented by a geometrical image, but by the word.

I have undoubtedly described too simply the associations that subjects might have at each stage of writing down a mathematical proof. The main problem is to distinguish between associations that play an essential and important role in obtaining the proof, and those which are more or less accidental accompaniments of finding the proof. For example, a person may read a theorem about geometry, written in English words, and as he begins to search for a proof of this theorem, he associates to simple geometrical figures—in particular, to the sort of figure useful for setting up the conditions of the theorem. At the same time that he has this geometrical association, or if he goes through the process of drawing such a figure, he may be having associations about his wife, his mother, or his children, etc. We would not want to think of these latter associations as playing the same sort of role in finding the proof. In other words, we want to see to what extent a chain of associations may be identified, a chain that is critical for the heuristic steps of finding a proof. It is also important, I am sure, to separate the geometrical kind of case from the other extreme—as a pure case, the kind of thinking that goes on when one is playing a game like chess or checkers. What kind of associations are crucial for finding a good move in chess, checkers, or, to pick a different sort of example, bridge?

An experiment we have conducted in our laboratories has some bearing on the matters I am discussing. This experiment concerned the possible differences between learning rules of logical inference in a purely formal way or as part of ordinary English. The three rules studied were:

Det	Sim	Com
$\dfrac{P \to Q \quad P}{Q}$	$\dfrac{P \wedge Q}{P}$	$\dfrac{P \wedge Q}{Q \wedge P}$

Group 1 received the formal part first (FA) and then the interpreted logic in ordinary English (IB). Group 2 reversed this order: IA, then FB. Note that I use A for the early part of the experiment and B for the later part. Schematically then:

Group 1 FA + IB
Group 2 IA + FB

The formal (F) and interpreted (I) parts of the experiment were formally isomorphic.

Some of the results are shown in tables 1 and 2. The subjects were fourth-graders with an IQ range of 110 to 131; there were 24 subjects in each group.

Table 1.—Comparisons of errors on different parts of logic experiment

Comparison	t	df	Significance
FA > FB	1.94	46	.1
IA > IB	3.28	46	.01
FA ≠ IA	1.47	46	----------
FB ≠ IB	.08	46	----------
FA + FB ≠ IA + IB	1.15	94	----------
FA + IB ≠ IA + FB	1.07	94	----------

Table 2.—Vincent learning curves in quartiles for logic experiment

Group	Probability of error in each quartile			
	1	2	3	4
FA	.40	.36	.39	.24
IB	.32	.32	.30	.19
IA	.48	.41	.33	.28
FB	.21	.21	.28	.14

Perusal of tables 1 and 2 indicates that the order of presentation, formal material first or last, does not radically affect learning. There is, however, some evidence in the mean trials of last error that there was positive transfer from one part of the experiment to the other for both groups. For example, the group that began with the formal material had a mean trial of last error of 14.1 on this part, but the group that received this material later had a smaller mean trial of last error of 10.9. In the case of the interpreted part, the group beginning with it had a mean trial of last error of 18.3, but the group that received this material after the formal part had a mean trial of last error of 7.7, a very considerable reduction. Now, one way of measuring the amount of transfer from one concept or presentation of mathematical material to a second is to consider the average mean trial of last error for both concepts in the two possible orders. If we look at the logic experiment from this standpoint, there is a significant difference between the group beginning with the formal material, completely uninterpreted as to meaning, and the group beginning with the interpreted material. The average trial of last error on both parts of the experiment for the group beginning on the formal part is 10.9 and for the group beginning on the interpreted part is 14.6. In a very tentative way these results favor an order of learning of mathematical concepts not yet widely explored in curriculum experiments.

Psychological Nature of Mathematical Objects

A proper psychology of mathematical thinking should throw new light on old controversies about the nature of mathematical objects. The three classical positions on the foundations of mathematics in the 20th century characteristically differ in their conception of the nature of mathematical objects. Intuitionism holds that in the most fundamental sense mathematical objects are themselves thoughts or "ideas." As Heyting puts it, the subject of intuitionism is "constructive mathematical thought." For the intuitionist the formalization of mathematical theory can never be certain of expressing correctly the mathematics. Mathematical thoughts, not the formalization, are the primary objects of mathematics.

The view of mathematical objects adopted by the formalists is quite the opposite. According to an often quoted remark of Hilbert (at least I believe this remark is due to Hilbert), formalism adopts the view that mathematics is primarily concerned with the manipulation of marks on paper. In other words, the primary subject matter of mathematics is the language in which mathematics is written, and it is for this reason that formalism goes by the name of "formalism."

The third characteristic view of mathematical objects is the Platonistic one: that mathematical objects are abstract objects existing independently of human thought or activity. Those who hold that set theory provides an appropriate foundation for mathematics usually adopt some form of Platonism in their basic attitude toward mathematical objects.

In spite of the apparent diversity of these three conceptions of mathematical objects, there is a fantastically high degree of agreement about the validity of any carefully done piece of mathematics. The intuitionists will not always necessarily accept as valid a classical proof of a mathematical theorem, but the intuitionists will, in general, always agree with the classicist on whether the theorem follows according to classical principles of construction or inference.

There is a highly invariant content of mathematics recognized by all mathematicians, including those concerned with foundations of mathematics, which is absolutely untouched by quite radically different views of the nature of mathematical objects. It is also clear that the standard philosophical methods for discussing the nature of mathematical objects do not provide appropriate tools for characterizing this invariant content.

The attitude that philosophical discussions of the nature of mathematical objects are, from a mathematical standpoint, somehow not serious is well expressed in the following quotation from a paper entitled "Nominalistic Analysis of Mathematical Language" by Leon Henkin (in *Logic, Methodology and Philosophy of Science, Proceedings of the 1960 International Congress*, Stanford University Press, 1962):

> In a congress where logic and methodology play so prominent a role, it is perhaps permissible to preface one's talk with a brief meta-talk and even to indulge in a modicum of self-reference.
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> Let me begin, then, by confessing that, although from the beginning of my studies in logic I have been intrigued by philosophical questions concerning the foundations of mathematics, and have often been to a greater or lesser extent attracted or antagonized by some particular viewpoint, I have never felt an overwhelming compulsion to decide that any given theory was "right" or "wrong." On the contrary, I have had a strong continuing feeling that "working mathematics" comes first, and that differing approaches to the foundations of the subject were largely equally possible ways of looking at a fascinating activity and relating it to broader areas of experience.
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> My interest in foundational theories has rather been directed toward the extent to which these are themselves susceptible of mathematical formulation, and toward the solution of technical problems which may arise from such analyses. In particular, this has characterized my approach to modern efforts to obtain a nominalistic interpretation of mathematical language.

The question posed by an attitude like that expressed by Henkin is, whether it is possible to characterize in a psychological way the activity of the working mathematician. Although I have entitled this section "Psychological Nature of Mathematical Objects," what I mean to suggest is that the classical philosophical discussions of the nature of mathematical objects may fruitfully be replaced by a concentration, not on mathematical objects, but on the character of mathematical thinking. It is, I think, by a concentration on mathematical thinking, or activity, that we can be led to characterize the invariant content of mathematics or, to use Henkin's phrase, to get at the nature of working mathematics without commitment to a particular philosophical doctrine.

By a concentration on mathematical thinking, as opposed to the nature of mathematical objects, new kinds of clarification about the nature of mathematics seem possible. Concentration on mathematical thinking is the proper emphasis for those concerned with understanding how mathematics is learned or how it should be taught. In my own work in elementary-school mathematics, I have been struck by the tension in my own thinking between the use on the one hand of classical mathematical language to describe the mathematics we are teaching—the ordinary mathematical talk about sets, numbers, binary operations on numbers, and the serious and systematic application of the language of learning theory to the analysis of what the child is learning in acquiring mathematical knowledge. Looked at from a learning-theoretic standpoint, the mathematical language has a scholastic ring about it. What I say, in fact, to my psychological friends is that what seems to be from a psychological standpoint the important thing about the kind of program I have worked on in elementary school mathematics is that the children are becoming tremendously sophisticated in using a new language. From a psychological standpoint, this ability to use the symbolic language of mathematics in an accurate and facile way is much more fundamental than learning talk about sets or learning constructive algorithms for performing operations on numbers. If this is right, then a good deal of what most of us, including myself, have to say about the learning of mathematics is itself written in the wrong kind of language. The difficult thing is that I do not yet see clearly what the right sort of language or conceptual framework is.