THE PSYCHOLOGICAL FOUNDATIONS OF MATHEMATICS

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1. Introduction

I would like to say to begin with that it is a pleasure to be here and to participate in this colloquium on models. For the topic of my own lecture today I am somewhat hesitant in view of the fact that Professor Piaget is sitting here and has been writing on this topic for many years. I wish that I had confidence that the kind of things I want to say will turn out to be the right things, the significant things to suggest in investigations on the psychological foundations of mathematics, but I have no such confidence. Secondly, it is perhaps paradoxical considering the subject of this colloquium and my own interests that I shall not have more to say of a direct sort about models, but it seems to me that the problems raised by the learning of mathematics provide an excellent touchstone for testing and evaluating models, particularly with respect to many of the issues we have already discussed. In the cognitive domain mathematics provides one of the clearest examples of complex learning, for the structure of the subject itself provides numerous constraints on the structure of any models that are to be considered adequate to mathematics learning. Therefore I hope to justify, in the context of the

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present colloquium, my own concern with the psychological foundations of mathematics by emphasizing the importance of the kind of learning found in mathematics for the development of complex models of learning. I would agree wholeheartedly with those two good cognitivists Frank Restle and Herbert Simon that simple stimulus models are certainly not adequate to give a very deep account of mathematics learning. On the other hand, I am equally skeptical of the cognitive models that have as yet been proposed with respect to the central problems of giving such an account, although I have a great deal of respect and appreciation for the kind of thing that Restle, Simon and their associates have been concerned with over the past few years.

Before I begin discussing particular psychological issues there is another direction of interest quite apart from models that I want to mention, and that is the relation of the psychological foundations of mathematics to foundations of mathematics in the classical sense, and by the classical sense I mean the work in foundations that has been characteristic of this century. The three main positions in the twentieth century on the foundations of mathematics 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. For the intuitionist formalization of mathematical theories can never be certain of expressing correctly the mathematics. Mathematical thoughts, not the formalization, are the primary objects of mathematics. Yet the nature of mathematical thinking has scarcely been seriously discussed from a psychological standpoint on the part of any intuitionist.

The second 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. The philosophy of Bourbaki, for example, is that of Platonism.

The view of mathematical objects adopted by the formalists is something else again. According to an often quoted remark of 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 'formalism'.

In spite of the apparent diversity of these three conceptions of what mathematics is about -- and certainly they differ extraordinarily in their conception of the proper object of mathematical attention --, there is a very high degree of agreement about the validity of any carefully done piece of mathematics. The intuitionist will not always necessarily accept as valid a classical proof of a mathematical theorem, but the intuitionist will, in general, always agree with the classicist as to whether or not the theorem follows according to classical principles of construction and inference. There is a highly invariant content of mathematics recognized by all mathematicians, including those concerned with the foundations of mathematics, which is absolutely untouched by 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. A main thesis of this paper is that the classical philosophical discussions of the nature of mathematical objects may fruitfully be replaced by concentration, not on mathematical objects, but on the character of mathematical thinking. There is reason to hope that by concentration on mathematical thinking or mathematical activity we can be led to characterize the invariant content of mathematics. Or, to put it another way, to get at the nature of working mathematics without commitment to a particular philosophical doctrine.

My original title for this paper was "Behavioral Foundations" rather than "Psychological Foundations." The reason for changing is the desire to avoid the charge of attempting to reduce mathematics to the kind of considerations exemplified in Skinner's Verbal Behavior. Moreover, it is an increasing conviction of mine that the classical concepts of behaviorism, namely, those of stimulus, response and reinforcement, are not, at least in their standard formulation, nearly adequate for any complicated behavior, and in particular, for the intellectual activity of mathematicians and scientists.

It will perhaps be desirable to make this point somewhat more explicit, particularly because of the considerable interest in this colloquium on the formal properties of models. It would be too substantial a digression to present possible formal axiomatizations of stimulus-response theory and then to analyze in this rather detailed and cumbersome framework the severe limitations on accounting for the formation of new concepts in the repertoire of a subject. The essential idea of the argument that shows how severe these limitations are can be presented within various fragments of stimulus-response theory.

The first thing to be noticed in considering the question of what does the theory say about the formation of new concepts out of old ones is that many details of the learning process are irrelevant. For example, for analysis of this problem it is not essential to know whether learning is mainly all-or-none or incremental. The second thing to note is that unless the theory has sufficient apparatus for defining new concepts in terms of old ones the theory cannot give a systematic account of how the new concepts are learned.

This logical question of definability is central to my argument, and a simple example of a purely mathematical sort may be useful in clarifying the method by which it may be shown that one concept may not be defined in terms of other concepts.

Consider first the ordinal theory of preference based on a set A of alternatives, a binary relation P of strict preference and a binary relation I of indifference, where P and I are relations on A. A triple $\alpha = (A,P,I)$ is an <u>ordinal preference pattern</u> if and only if the following three axioms are satisfied for every x, y and z in A

Axiom 1. If xPy and yPz then xPz;

Axiom 2. If xIy and yIz then xIz;

Axiom 3. Exactly one of the following: xPy, yPx, xIy.

The Italian mathematician Alessandro Padoa formulated in 1900 a principle that may be used to show in a rigorously definite way that one concept of a theory is not definable in terms of the others. The principle is simple to formulate: find two models of the theory such that the given concept is different in the two models, but the remaining concepts are the same in both models. It is easy to show that if the given concept were now definable in terms of the other concepts then it would be possible to derive a formal contradiction within the theory. (For a more detailed discussion of these matters, see Ch. 8 of my Introduction to Logic.) Thus to show that the concept P of strict preference cannot be defined in terms of the concept of the set A of alternatives and the concept I of indifference, it is sufficient to consider the following two models $\mathfrak{A}_1$ and $\mathfrak{A}_2$ of the theory.

$$A_1 = A_2 = \{1,2\}$$

$$I_1 = I_2 = \{(1,1), (2,2)\}$$

$$P_1 = \{(1,2)\}$$

$$P_2 = \{(2,1)\} .$$

Note that two trivial numerical examples of ordinal preference patterns are sufficient to establish the undefinability of the concept of strict preference. On the other hand, it is easy to offer a definition of indifference in terms of strict preference:

$$xIy \qquad \text{if and only if not} \quad xPy \quad \text{and not} \quad yPx .$$

If this example is kept explicitly in mind, it will be easier to appreciate the point I want to make about any current variant of stimulus-response theory of concept formation. One way or another the theory must be rich enough to make possible the formal definability of the new concept to be learned. I can see no other way of giving a formal account of learning the new concept. If the machinery does not exist within the theory for characterizing the new concept, then the theory cannot give an adequate account of how the new concept is formed by the subject. In this connection it is important to emphasize how incomplete are all standard learning-theoretic accounts of concept formation. Current theories simply do not postulate mechanisms of concept formation which are adequate to even the most primitive and simple concepts, let alone ones of any mathematical complexity.

To illustrate this failure, we may consider some examples of the sort often studied experimentally. In line with earlier remarks, I shall ignore detailed assumptions about learning and give a schematic account that is compatible with any one of several fully worked-out learning models. As a matter of notation, let S be the basic set of stimuli, and given concepts may be represented as partitions $C_1, \ldots, C_n$ of S. In general, each C_i is a partition of S, although in many familiar experimental examples the concepts are just two-valued and thus lead to concepts that may be represented as subsets of S. Let new concepts be represented as partitions $K_1, \ldots, K_m$ of S. The first general point to note is that if we are simply given an m+n+1-tuple $\mathcal{S} = (S, C_1, \ldots, C_n, K_1, \ldots, K_m)$ then no questions about generating the concepts K_j from the given concepts C_i can be definitely settled. It is necessary also to specify what operations may be performed on the given C_i, or what additional structure is imposed on the basic set S of stimuli. It is a matter of the postulated psychological theory of concept formation to impose this additional structure.

In familiar experiments on concept identification it is assumed that the intersection, union and complement of two-valued concepts can be formed, but these Boolean operations are weak. Certainly they are not adequate to give an account of the formation of any complex mathematical concepts. For example, if we assume in an experiment, for purposes of theoretical analysis at least, that an individual has the concepts of shape, size and color, with indefinitely many values for each concept, we cannot in terms of the Boolean operations, or their generalizations to partitions, define or characterize any of the intuitively simple comparative concepts of greater size, more saturation of color, etc.

Although the matter cannot be pursued in detail here, it should be all too obvious to those familiar with the psychological literature of concept formation that the structures of the mechanisms of concept formation as yet proposed are far too simple, as a direct application of Padoa's method will show, to account for the formation of the great variety of mathematical concepts.

Because of their central importance for any theory of concept formation in mathematics, the three topics I shall concentrate on the remainder of this paper are <u>abstraction</u>, <u>imagery</u> and <u>algorithms</u>.

2. Abstraction

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 possibility 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 theory is now considered abstract because the class of models of the theory is so large that any simple imagery or picture of a typical model is not possible. The range of models is too diverse.

In the case of group theory, for example, one intuitive basis was the particular 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 that every group is isomorphic to a group of transformations. It has been 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 very 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 an interesting question 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 as another suitable example, what sort of stimulus associations or imagery do they have in thinking about the axiom of infinity in set theory? It is my own conjecture 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.

It is interesting to note that the classical search for a representation theorem for a theory can very well be thought of as an effort to make the abstract theory more intuitive. The formal idea of a representation theorem can be put as follows. We begin by discussing the class or category M of all models of the theory. We then seek a subclass or subcategory R of models of the theory such that given any model in M there exists an isomorphic model in the representing class R. We may of course always obtain a trivial representation theorem by simply taking R = M, but the satisfying representation theorems are just those that are able to select as the class R an intuitively clear and relatively simple class of models. Cayley's theorem is a good example. Another classic example is Stone's representation theorem for Boolean algebras. Many of us would have had a feeling that we did not quite understand what the abstract theory of Boolean algebras came to if Stone's theorem had proved not to be true. The motivation for Boolean algebras is mainly thought of in terms of the algebra of sets, but if the abstract theory has models of Boolean algebras that are not isomorphic to algebras of sets, what indeed are we to make of the structure of these abstract algebras? Stone's theorem shows that we do not have any worries on this score, but in the history of mathematics and science many negative examples can be mentioned, in which the move to a more abstract theory was not buffered by the proof of an appealing representation theorem, but these matters cannot be pursued in further detail here.

3. 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 combination 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 exists 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 abstractio as the measure of the diversity of imagery produced by a standard body of mathematics and stimulus material in a given population.

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 associations 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. On the other hand, such experiments should not be difficult to perform and the results might be of interest.

I have undoubtedly put the matter too simply. One 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 the central activity 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, he may have associations about his wife, his mother, or his children. We would not want to think of these latter associations as playing the same sort of role in finding proofs. In other words, we want to see to what extent a chain of associations may be identified, which 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 such as 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 laboratory has some bearing on these questions. This experiment concerned the possible differences between learning rules of logical inference in a purely formal way and as part of ordinary English. The three rules studied were

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

(Here $\rightarrow$ is the sign of implication and $\wedge$ the sign of conjunction, but subjects were not told this when they began the formal part of the experiment.) Group 1 received the formal part first (FA) and the interpreted logic in ordinary English (IB). Group 2 reversed this order: IA then FB. Note that A stands for the first part of the experiment and B for the second 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 I.Q. range from 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

	Probability of error in each quartile			
Group	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 who received this material as the second part of their experiment 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 uninterrupted 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 that 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 very widely explored in curriculum experiments.

4. <u>Algorithms in Arithmetic</u>

I conclude this paper with consideration of a pedagogically important and theoretically interesting example of a problem that needs deeper psychological analysis, namely, the problem of how the first algorithms in arithmetic are learned.

As an initial model for thinking about algorithms, I would like to propose the following. We have in mind a given collection of problems that we wish the child to be able to solve. To make our analysis definite at this point, let us consider a set of arithmetical problems. They might be in the form of $8 - 5 = \_\_\_\_$, $8 + \_\_\_\_ = 10$, $10 - \_\_\_\_ = 4$, $\_\_\_\_ - 3 = 5$, etc. The machinery needed to solve these problems can be roughly divided into two parts. One part consists of direct storage in memory of certain elementary facts. Exactly what these elementary facts are will vary from stage to stage in the curriculum. Towards the beginning of arithmetic, it might consist of storage of the elementary addition facts: $1 + 1 = 2$, $1 + 2 = 3$, $2 + 1 = 3$, $1 + 0 = 1$, $2 + 0 = 2$, $3 + 0 = 3$, $0 + 3 = 3$, etc. The second part of the machinery consists of algorithms, or constructive rules, for transforming the elementary facts in memory into new elementary facts or, what is probably more important, transforming new stimulus presentations into one of these elementary facts stored in memory.

An immediate problem of psychological importance with respect to a given body of problems is how much should be stored in memory and how much should be carried by the algorithmic rule. It is seldom the case that for a given set of problems we want all the answers stored directly in memory -- it is certainly contrary to the usual spirit in teaching mathematics, but it is also unusual to want to store in memory only a minimal set of facts. For illustrative purposes, let me describe in some detail a way of teaching arithmetic that would consist of storing in memory a small number of facts and transferring the larger part of the load to the algorithmic rules. I emphasize that the example chosen is not one that is meant to have direct pedagogical applications. This system for computing sums is clearly not the sort of system we would wish to teach.

Let us suppose that our set of problems is just the following thirty

$0 + 0 = n$	$0 + n = 0$	$n + 0 = 0$
$0 + 1 = n$	$0 + n = 1$	$n + 1 = 1$
$1 + 0 = n$	$1 + n = 1$	$n + 0 = 1$
$1 + 1 = n$	$1 + n = 2$	$n + 1 = 2$
$2 + 1 = n$	$2 + n = 3$	$n + 1 = 3$
$1 + 2 = n$	$1 + n = 3$	$n + 2 = 3$
$3 + 1 = n$	$3 + n = 4$	$n + 1 = 4$
$1 + 3 = n$	$1 + n = 4$	$n + 3 = 4$
$2 + 2 = n$	$2 + n = 4$	$n + 2 = 4$
$4 + 1 = n$	$4 + n = 5$	$n + 1 = 5$

We put the following four facts in memory

$$1 + 1 = 2$$

$$2 + 1 = 3$$

$$3 + 1 = 4$$

$$4 + 1 = 5$$

We have the following four rules of operation:

1. Use the four facts in memory to replace equals by equals.
2. Replace a term of the form $a + (b + c)$ by $(a + b) + c$, or vice versa.
3. Replace a term of the form $a + b$ by $b + a$.
4. Cancel an equation of the form $a + n = a + c$ to get $n = c$.

These four rules are then used to transform a problem, step by step, until we reach an expression of the form $n = c$. Thus,

$2 + 2 = n$	Problem
$2 + (1 + 1) = n$	by (1)
$(2 + 1) + 1 = n$	by (2)
$3 + 1 = n$	by (1)
$4 = n$	by (1)

or, similarly,

$3 + n = 5$	Problem
$3 + n = 4 + 1$	by (1)
$3 + n = (3 + 1) + 1$	by (1)
$3 + n = 3 + (1 + 1)$	by (2)
$3 + n = 3 + 2$	by (1)
$n = 2$	by (4)

There are several immediate criticisms to be made of this set-up, as I have described it. First, I have not been really explicit about parentheses in connection with rule (1). And I have not really made clear the role of the associative law, i.e., rule (2). More importantly, I have not written down a genuine algorithm for the set of problems. The four rules are four rules of proof, not an algorithm for solving any one of the thirty problems.

To convert the four rules into an algorithm, it is necessary to specify an order in which they are to be applied, and this order, to be efficient, should vary with the particular problem. Not only is it necessary to specify an order, but it also is necessary to show that the algorithm can be given to a machine and automatically used to solve any of the thirty problems.

To convert the present four rules into a genuine algorithm is somewhat tedious. Let me describe another simpler system that may be used to solve the same thirty problems.

We put in memory the following five definitions:

1 = /

2 = //

3 = ///

4 = ////

5 = /////

Our algorithm is then the following:

(1) Replace all Arabic numerals by their stroke definitions and delete all plus symbols.

(2) If there are strokes on both sides of the equal sign, cancel one-by-one starting from the left of each side until there remain no strokes on one side. Ignore n in cancelling.

(3) On the one side still having strokes, replace the strokes by an Arabic numeral, using the definitions in memory.

The solution in the form n = c or c = n will result.

Let us apply this algorithm to the two problems previously considered.

First problem:

2 + 2 = n	Problem
// // = n	by (1)
4 = n	by (3)

In this case no cancelling is required.

Second problem:

3 +	n = 5	Problem
///	n = /////	by (1)
//	n = ////	by (2)
/	n = ///	by (2)
	n = //	by (2)
	n = 2	by (3)

It should be clear from these examples how the algorithm may be applied to solve the other twenty-eight problems in the original set, and moreover, how simply by adding new definitions in memory we may, without changing the algorithm, move on to similar problems involving larger numbers.

From a logical standpoint this algorithm is perhaps as simple as any to be found, and is very close in spirit to a direct characterization of the operation of counting. Consideration of its possible use by children takes us out of the domain of elementary mathematics -- the theory of algorithms for simple mathematical systems -- into the domain of psychology. Let me try to state some of the problems we encounter as we enter this domain.

(1) It seems highly unlikely that any children, without training, actually use the algorithm just described. The perplexing question is: what algorithms do they in fact use? At the level at which this problem is often discussed, the obvious answer is that they use the algorithms taught in the classroom and presented in their textbooks. But even casual inspection of the curriculum shows the inadequacy of this response, for

algorithms for the thirty problems listed above (or with the numerical variable "n" replaced by a blank or box) are not explicitly taught, although some partial hints in terms of counting may be given. A typical curriculum instruction to teachers is to let the children find the answer "intuitively" by working with the numbers. Parenthetically, the use of the word "intuition" in its nominal, adjectival or adverbial form by a curriculum builder, reformer, planner or evaluator should be a signal to the psychologist that unexplained and ill-understood learning behavior is about to be mentioned, and, unfortunately, often described as if it were understood.

So the problem remains, how do children in the fourth, fifth, or sixth month of the first grade, solve problems like those in our set of thirty?

(2) A proposal often heard is that children solve such problems by simple rote learning. This is a possible response when any single set of twenty or thirty simple problems is considered. It does not seem nearly as plausible when we look at the larger set of problems from which our thirty have been drawn. There are 55 ordered pairs of numbers summing to 9 or less ($0 + 0 = 0$, $0 + 1 = 1$, $1 + 0 = 1$, etc.). There are then 165 problems of the same type as our thirty ($n + 0 = 0$, $0 + n = 0$, $0 + 0 = n$, etc.). And the number of problems is increased considerably further by adding triplets of the form $1 + 2 + n = 4$, $1 + n + 2 = 4$, etc. It is extremely doubtful that this large stock of problems is held in memory, available for direct access. The child solves them by applying some sort of algorithm. Some of the possibilities are the following.

(a) The child counts off the necessary number words, aloud or in silent speech. Thus, the solution to "$4 + 5 = n$" is obtained by counting off five number names after "four", namely "five, six, seven, eight, nine." The solution to "$4 + n = 9$" is obtained by counting off number names after "four" until "nine" is reached and then judging the cardinality of the set of number names counted off. Even without detailed analysis it is clear that the second kind of problem is harder than the first. The third kind of problem is still harder. The solution to "$n + 5 = 9$" is obtained by counting off enough number names such that five more take the child to "nine." It seems doubtful to me that the algorithm can be successfully applied in this form to the third kind of problem. Notice that no advantage has been taken of the commutativity of addition. Serious training on this property would enable the child to reduce problems of the third kind to those of the second kind. The relatively greater difficulty almost all first-grade children have with the third kind of problem, when the unknown is at the far left, indicates that if the algorithm just described is used, it is not augmented by the commutative law.

For a great many different reasons it seems improbable that the algorithms actually used require very many closely-knit steps to obtain an answer. The counting algorithm just described is realistic for problems of the form "$4 + 5 = n$" and not out of the question for problems of the form "$4 + n = 9$." For problems of the form "$n + 5 = 9$" the child may, without being explicitly conscious of it, make rough estimates of n and test the guess by counting. He remembers, say, that $5 + 5 = 10$, and "nine" is close to "ten," so he trys 3 or 4. Or, he may remember, that is, have in immediate storage, that $4 + 4 = 8$, and he uses this fact to guess 3, 4 or 5.

(b) In many ways the above discussion sells the counting algorithm short, because of the seeming difficulty of counting a set of number names like "five, six, seven, eight" pronounced aloud or in silent speech, because the trace of "five" may have departed before "eight" is said. When the algorithm is externalized and applied in terms of physical objects (even the fingers) it seems much easier. I have seen something like the following used quite successfully in Ghana with harder problems than those we are now discussing.

The child has a counting set of pebbles on his desk. To solve the problem "$4 + 5 = n$" he first counts out 4 pebbles from his pile. He stops, and then counts out five more. This counting is done by simultaneously saying the number names "one, two, three, four" and pulling one pebble from the pile as he says each name. After counting out the set of four, and then counting out the set of five, he now counts the separated set of nine pebbles and gets the answer. He solves the problem "$4 + n = 9$," by first counting out a set of nine pebbles and then taking four away, that is, by counting off a set of four from the set of nine. (It is to be emphasized that each of these counting operations is a highly physical thing.) After taking away the set of four, he then counts the remaining set of five to obtain the answer. Notice that the act of taking away four from the set of nine pebbles can be clearly and succinctly taught even though the subtraction symbol has not been introduced. As already remarked, lots of people have observed that for American children the "$n + 5 = 9$" sort of problem is harder than the

"4 + n = 9" sort. For the counting algorithms just described they would seem to be on an equal footing. I think, but do not have real evidence at hand, that the Ghanaian children have the same sort of relative difficulty. The explanation is most likely to be found in the decoding required to pass from the written problem to the physical execution of the algorithm. The detailed analysis of how the stimulus arrangement expressing the problem sets off the algorithm shall not be gone into here, but I may say in passing that this kind of example provides an excellent opportunity to analyze the behavioral semantics of the simplest sort of language. Briefly put, I interpret a problem format like "4 + n = 9" as a command in the imperative mood. The symbol "9" standing by itself to the right of the equals sign means for the pebble model "Count out a set of nine pebbles." The symbol "4" means "Count out a set of four pebbles from the set of nine." And, roughly speaking, the remaining phrase " + n" means "Count the remaining set of pebbles and record the answer." For this kind of semantic the classical notion of truth is replaced by that of a response, or class of responses, satisfying a command. What I have sketched here in the roughest sort of way can be made precise by using with only slight modification the standard methods and concepts of formal semantics.

From the standpoint of the usual way of characterizing algorithms, the pebble-counting algorithm is unusual, for the operations of the algorithm are performed on the pebbles and not on the number symbols themselves. In this case the number symbols have meaning and this meaning is used to give instructions for performing the algorithm. It would seem that it is this sort of algorithm many people now advocate in arithmetic in order to avoid development of great facility with algorithms defined wholly in terms of the number symbols and which may thus be learned without "understanding numbers."

In order to give a concrete sense of some of the complexities that arise in understanding how children learn and perform algorithms, I would like to review briefly two pertinent experiments.

In the first experiment children in the first, second and third grades (ages 6, 7 and 8 years approximately) were asked to give the correct answers to the 63 problems of the form $1 + 2 = n$, $1 + n = 3$ and $n + 2 = 3$, with sums ranging from 0 to 5. The problems were shown on a screen by a slide projector in the form $1 + 2 =$ ____, $1 +$ ____ $= 3$, etc., and the subjects responded by pushing one of six buttons marked 0, 1, 2, 3, 4, 5; the buttons were arranged linearly. A timer also measured the response latency from the appearance of a problem on the screen to the pushing of one of the six buttons. In a given daily session a subject was presented with each of the 63 problems for a total of 63 trials. One group of first graders had six sessions; the remaining subjects had three sessions. The only data we shall examine here are those resulting from summing over all grades, days and subjects. This summation yields a total of 280 responses for each of the 63 problems.

In line with the general discussion of possible algorithms, the following simple model is proposed for analyzing the rather complex data of this experiment. The fundamental operation, it is postulated, is that of counting. For problems of the type $a + b = m$, where a and b are given numbers and m is to be found, the time required is $(b+1)\alpha + \delta$. Here δ is a constant of the sort familiar in reaction time studies;

α is the time it takes to count one step for problems of this type (hereafter called Type I); $b + 1$ rather than b steps are called for, because "0" is the first possible answer, "1" the second, etc. In the case of Type II problems, whose form is $a + m = b$, the only change is to replace the timing parameter α by β. Thus the time required to solve $a + m = b$ is $(m+1)\beta + \delta$. Note that here m replaces b, because in all cases we think of counting up to the sum. For problems of Type III, that is, problems of the form $m + a = b$, we introduce a third parameter γ, and the time required to solve $m + a = b$ is $(m+1)\gamma + \delta$. Also in line with the earlier discussion it is natural to postulate that $\alpha < \beta < \gamma$. Concerning errors, it is also natual to postulate a parameter θ such that for the three types of problems the probability of an error on each counting step is $\theta\alpha$, $\theta\beta$ and $\theta\gamma$ respectively. Thus for n-step problems of Type I the probability of an error is $1 - (1-\theta\alpha)^n$, which in first approximation is simply $n\theta\alpha$, because, $0 < \theta\alpha < 1$. (It is assumed for simplicity that the probabilities of an error on the successive steps are statistically independent and that successive errors will not cancel each other out.)

The detailed analysis of this model will not be pursued here. The model goes badly awry in a number of its detailed predictions, but several qualitative features are well confirmed without requiring statistical estimates of the five parameters α, β, γ, δ and θ. Here are some predictions and the supporting or disconfirming evidence.

1. For the three problem types, the order of increasing difficulty both in response error and latency is I < II < III. The data are shown in Table 3, with the distribution of the three types given for the first

21 with least errors, the second 21, and the third 21, and corresponding data for the first 21 problems in speed of response, the second 21 and the third 21. For each entry the error data are shown first and the latency data second.

Table 3. Error and latency distributions for three types of problems in rank-order blocks of size 21.

	I	II	III
First 21	11, 11	5, 6	5, 4
Second 21	6, 6	8, 6	7, 9
Third 21	4, 4	8, 9	9, 8

The evidence that Type I problems are easiest is good, both in terms of errors and latencies, because they are concentrated in the first 21 problems in both distributions, but the discrimination between Types II and III is subtle and does not strongly favor the hypothesis that $\beta < \gamma$, even though other pedagogical evidence does.

2. For problems of a given type, the speed and accuracy is greater when the number of counting steps is less. Thus we may begin by looking at matching pairs, such as $3 + \_\_\_\_ = 5$ and $2 + \_\_\_\_ = 5$ to see if indeed the first is easier. To be more explicit, let each problem be defined by a triple (x, y, z) of numbers such that $x + y = z$. A matching pair then consists of two problems (x, y, z) and (y, x, z) of the same type, i.e., with the blank in the same spot. For example, $\_\_\_\_ + 1 = 5$ and $\_\_\_\_ + 4 = 5$ form a pair. There are 27 pairs with $x \neq y$. For simplicity, I restrict myself to the latency data. The prediction of

the model is that the response will be faster for the member of each pair having the smaller number to find. Thus the response to ____ + 4 = 5, which should take $2\gamma + \delta$ seconds, should be faster than the response to ____ + 1 = 5, which should take $5\gamma + \delta$ seconds. These predictions for the matching pairs are pretty well borne out by the data. The prediction is true for 20 of the 27 pairs. Four of those for which it is not are problems of the form ____ + 0 = a; when these special cases of adding zero are eliminated, the results are even more favorable to the model.

3. The problems with the smallest error rate and latency are consistent with the model, namely 0 + 0 = ____, ____ + 0 = 0 and 0 + ____ = 0. On the other hand, there are some striking anomalies, hard to explain from nearly any standpoint. On only four problems is the error rate greater than for 1 + 1 = ____, which was missed 26% of the time! Part of the explanation may be that the subjects responded very fast to this problem, -- it ranked fourth immediately after the three "zero" problems just mentioned --, and thus made many careless errors. The mean latency for 1 + 1 = ____ was 3.3 seconds; it was 2.6 seconds for 0 + 0 = ____ as the minimum of the set of 63 problems and 7.0 seconds for 4 + ____ = 5 as the maximum. Whatever the explanation, only a quite complicated model seems likely to fit this surprising error rate into the scheme of things. Other aspects of the response errors are not well-explained by the model, but shall not be considered here.

The analysis presented has been necessarily very sketchy. A more detailed quantitative assessment will be made elsewhere of the family of models suggested by the simple five-parameter model examined here.

The preliminary results seem to be encouraging enough to warrant such investigations in greater depth.

I want now to move on to a second experiment that has some interesting bearing on the complexity of understanding the learning of algorithms. Roughly speaking, the significance of the experiment I want to describe is related to the fact that we probably have been and will continue to be much misled by the mathematical structure of algorithms, so that we are deceived into thinking that young students learn the material very much in the way it is formulated from a mathematical standpoint. As in most areas of complex learning, what is actually going on is undoubtedly a good deal more subtle. The experiment is one performed with 9- and 10-year old children who had already been given extensive instruction on the commutative, associative and distributive laws of arithmetic. They had had verbal instruction as to the significance of these laws, and they had performed and executed presumably correctly, but without detailed check on the part of the teacher, a great many exercises applying the laws. Many of the exercises emphasized the fact that the commutative, associative and distributive laws are central to the justification of the more complex algorithms of elementary arithmetic -- multiplication of two-digit numbers, the algorithm for long division, etc.

The experiment was conducted as part of the program of the Computer-Based Laboratory we have constructed in the last two years at Stanford; the experiment was performed adjacent to the school classroom in a very small room in which was located a teletype that was connected to the computer at Stanford. The school itself is approximately 20 kilometers south of Stanford. The children participating in this experiment had used the teletype for at least a month for the purposes of review and drill in elementary mathematics, and were fully familiar with the instrument and the experimental setting. We had noticed in earlier work that the students were having difficulty with the commutative, associative, and distributive laws and that they particularly had difficulties with exercises that called for a rapid shift from one law to another.

We decided to perform a fairly simple learning experiment on the mastery of this material. We broke up the types of problems into 48 categories; shortage of time prohibits me from giving a description of these 48 categories. They depend on which particular law is involved and where the blank occurs. The equations $5 + 3 = 3 + \_\_\_\_$ and $5 + 3 = \_\_\_\_ + 5$ would be two instances of two categories exemplifying the commutative law of addition. The subject got 24 problems a day and they cycled through the entire 48 types every two days. The students were given 10 seconds to answer each problem. If an answer was not given in that time interval, the program returned control of the teletype to the computer, and the teletype printed out "Time is up" before repeating the problem again. After the second appearance and failure to respond correctly or within ten seconds, the correct answer was printed out, the problem was repeated for a final time, and the program moved on to a new problem.

The results for the first six days of the experiment are shown in Table 4.

Table 4. Mean learning data for the first six days of the computer-based teletype experiment on the laws of arithmetic.

Day	Prob. Correct	Prob. Wrong	Prob. Time-Out	Mean Time in Sec.
1	.53	.22	.25	630
2	.56	.33	.11	520
3	.74	.21	.05	323
4	.72	.23	.05	390
5	.77	.18	.05	355
6	.91	.08	.01	279

For the first day the mean proportion of correct responses was .53, the mean proportion of errors .22, the mean proportion of time-outs .25 and the mean completion time 630 seconds for the entire set of 24 problems. So the students began by being rather slow and by making lots of errors. The second day we see an increase, and an increase each thereafter, except on the fourth day, until on the sixth day of the experiment the mean proportion correct is .91, the number of time-outs is very slight and there is a very considerable reduction in mean completion time from 620 seconds to 279 seconds. From the standpoint of learning we get very clear mean results. If I had the time I would show you the results for the best student and the worst student in the class on the first day. Both of them showed considerable learning and one of the pleasing things about the experiment is that every student showed marked improvement in performance. Now one next question to ask is about how to analyse the difficulty of items. At the moment it appears that the best way is not in terms of the mathematical law involved, for example, the distributive law, but in terms of the kind of patterns required in the answer. These patterns can be defined fairly directly in a psychological rather than in a mathematical fashion. For instance, regardless of whether we are concerned with an associative, commutative or distributive law, if the student must fill in a single blank on the right of the equation, using a stimulus pattern or a numeral already occurring on the left, then the problem is relatively easy. What appears to be the case psychologically is that the students are perceiving the kind of pattern required without regard to the mathematical meaning of the law involved. Data for this sort of problem are labeled Type A in Table 5. Of next order of difficulty are the problems requiring that two blanks be filled in on the left, but using numerals that occur on the right; for example,

$$5 \times (6+3) = (5\times\_\_\_) + (5\times\_\_\_).$$

Data for this kind of problem are labeled Type B in Table 5.

Finally, most difficult are the problems that require the use of a number not shown on the right-hand side of the equation, which in the present experiment were essentially examples showing that neither subtraction nor division is commutative:

$$12 - 7 = 7 - \_\_\_.$$

Data for this sort of problem are labeled Type C in Table 5.

Table 5. Learning data on the three types of problems in the computer-based teletype experiment.

Days	Type A			Type B			Type C		
	Prob. Correct	Prob. Wrong	Prob. Time-out	Prob. Correct	Prob. Wrong	Prob. Time-out	Prob. Correct	Prob. Wrong	Prob. Time-out
1-2	.63	.21	.16	.37	.35	.28	.06	.80	.14
3-4	.89	.09	.02	.62	.36	.02	.26	.53	.21
5-6	.93	.06	.01	.75	.24	.01	.23	.55	.22

Admittedly a deeper sort of theory is required to explain the data of Table 5 than that exemplified in the simple five-parameter model discussed earlier. On the other hand, this experiment as well as the earlier one should make evident that a theory of how mathematics is learned and mathematical concepts are formed will not fall out of the consideration in any direct or simple way of the structure of mathematics itself as it is usually thought of by mathematicians. A new and rather subtle psychological theory of learning is clearly necessary. The present paper has merely tried to delineate what may be important aspects of such a new theory.