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Some Proposals for Reviving the Philosophy of Mathematics

Hersh's essay begins the challenge to foundationalism:

> The present impasse in mathematical philosophy is the aftermath of the great period of foundationist controversies from Frege and Russell through Brouwer, Hilbert and Gödel. What is needed now is a new beginning . . .
>
> Many of the difficulties and stumbling blocks in the philosophy of mathematics are created by inherited philosophical prejudices which we are free to discard if we choose to do so.

Hersh presents the case from the point of view of mathematicians. For him, philosophy of mathematics is primarily the working philosophy of the professional mathematician. In so far as that philosophy is restricted to the usual mix of foundational ideas, Hersh charges, it is generally inconsistent, always irrelevant and sometimes harmful in practice and teaching.

There are difficulties in each of the foundational theories and Hersh discusses several of these. However, his main concern is to understand how the preoccupation with foundations came about. At present, Hersh suggests, the best explanation of foundational concerns is in terms of the historical development of mathematics which he summarizes. Along the way, he isolates some of the basic presuppositions of foundation studies: "that mathematics must be provided with an absolutely reliable foundation" and "that mathematics must be a source of indubitable truth." Hersh's point is that it is one thing to accept the assumption when, like Frege, Russell or Hilbert, we feel that the foundation is nearly attained. But it is quite another to go on accepting it, to go on letting it shape our philosophy, *long after* we've abandoned any hope of attaining that goal.

Very well, if the concerns of foundations of mathematics are the wrong concerns, then how do we philosophize about mathematics ? Hersh's answer is clear: we begin with the ongoing *practice* of mathematicians. This is a deep

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and important point that will be returned to again and again throughout this anthology. The emphasis on mathematical practice is not just a mathematician's chauvinism. It is the practice of mathematics that provides philosophy with its data, its problems and its solutions. At the turn of the century it seemed as if foundationalism could capture the essence of mathematical practice and no wonder. As we've noted, foundations programs changed that practice. But in the last half century, foundational research and ordinary mathematical practice have evolved along quite different lines. To revive the philosophy of mathematics, we must return to its source for a fresh look.

If we view mathematical practice with an unjaundiced eye, Hersh suggests, we will observe prominent features that have been ignored by traditional philosophy. We might note, for example, that mathematical knowledge is inherently fallible and no foundation can make it infallible. When informed of Russell's Paradox, Frege is alleged to have said "Arithmetic totters." Hersh might agree but add that arithmetic doesn't totter too much and besides, everything totters. Mathematical knowledge is "fallible, corrigible, tentative and evolving as is every other kind of human knowledge."

In a similar vein, we might note that mathematical practice is essentially a public activity, not a private one. This obvious point is at odds with the standard foundational attitude that mathematics is essentially a private affair, taking place in a mind, and that public practice is only a symptom of it. The emphasis on mathematical practice, in our time, brings with it an emphasis on the mathematical *community* as the ultimate source of mathematical activity.

Hersh concludes his paper with a brief sketch of the new vista in philosophy of mathematics. It is not without flaws. Professional philosophers will be disturbed by the free and easy use of 'idea' as a basic explanatory notion. After two thousand years of philosophical reworking, the idea of 'idea' has become rather vague. Indeed in comparison the platonist's 'set' or the formalist's 'symbol' can look like a positive advance in clarity. In Hersh's framework idea takes on a more substantial meaning, however, very like 'cultural product of the mathematical subculture.' Of course this interpretation is likely to raise more questions than it answers from both mathematicians and philosophers. What accounts for the striking differences between mathematical products and other cultural products? Is mathematical creativity as unconstrained as artistic creativity? Hersh suggests some answers, but more importantly, he asks deep questions.

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By "philosophy of mathematics" I mean the working philosophy of the professional mathematician, the philosophical attitude toward his work that is assumed by the researcher, teacher, or user of mathematics. What I propose needs reviving is the discussion of philosophical issues by working mathematicians, especially the central issue—the analysis of truth and meaning in mathematical discourse.

The purpose of this article is, first, to describe the philosophical plight of the working mathematician; second, to propose an explanation for how this plight has come about; and third, to suggest, though all too briefly, a direction in which escape may be possible. In summary, our argument will go as follows:

(1) The philosophical notions about mathematics commonly held by the working mathematician are incompatible with each other and with our actual experience and practice of mathematical work. Many practical problems and impasses confronting mathematics today have philosophical aspects. The dearth of well-founded philosophical discourse on mathematics has observable harmful consequences, in teaching, in research, and in the practical affairs of our organizations.

(2) The present impasse in mathematical philosophy is the aftermath of the great period of foundationist controversies from Frege and Russell through Brouwer, Hilbert, and Gödel. What is needed now is a new beginning, not a continuation of the various "schools" of logicism, formalism or intuitionism. To get beyond these schools, it is necessary to go back in history to their origin, to see what they had in common, and how they were rooted in the mathematics and philosophy of their day.

(3) Many of the difficulties and stumbling blocks in the philosophy of mathematics are created by inherited philosophical prejudices which we are free to discard if we choose to do so. Some of our philosophical difficulties will then simply evaporate; others will become tangible problems which can be investigated systematically, with reasonable hopes for progress.

Each statement will be amplified and argued at some length below.

1 THE PHILOSOPHICAL PLIGHT OF THE WORKING MATHEMATICIAN

Most writers on the subject seem to agree that the typical "working mathematician" is a Platonist on weekdays and a formalist on Sundays. That is, when he is doing mathematics, he is convinced that he is dealing with an objective reality whose properties he is attempting to determine. But then, when challenged to give a philosophical account of this reality, he finds it easiest to pretend that he does not believe in it after all.

We quote two well-known authors:

> On foundations we believe in the reality of mathematics, but of course when philosophers attack us with their paradoxes we rush to hide behind formalism and say, "Mathematics is just a combination of meaningless symbols," and then we bring out Chapters 1 and 2 on set theory. Finally we are left in peace to go back to our mathematics and do it as we have always done, with the feeling each mathematician has that he is working with something real. This sensation is probably an illusion, but is very convenient. That is Bourbaki's attitude toward foundations. (Dieudonné [8].)
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> To the average mathematician who merely wants to know his work is securely based, the most appealing choice is to avoid difficulties by means of Hilbert's program. Here one regards mathematics as a formal game and one is only concerned with the question of consistency . . . . The Realist position is probably the one which most mathematicians would prefer to take. It is not

until he becomes aware of some of the difficulties in set theory that he would even begin to question it. If these difficulties particularly upset him, he will rush to the shelter of Formalism, while his normal position will be somewhere between the two, trying to enjoy the best of two worlds. (Cohen [4].)

(Throughout the paper, the term "formalism" is used, as it is in these quotations from Dieudonné and Cohen, to mean the philosophical position that much or all of pure mathematics is a meaningless game. It should be obvious that to reject formalism as a philosophy of mathematics by no means implies any critique of mathematical logic. On the contrary, logicians, whose own mathematical activity *is* the study of formal systems, are in the best position to appreciate the enormous difference between mathematics as it is done and mathematics as it is schematized in the notion of a formal mathematical system.)

We will shortly offer an analysis of this supposed alternative of Platonism and formalism. At present we merely record this as a generally accepted fact about the mathematical world today: Most mathematicians live with two contradictory views on the nature and meaning of their work. Is it credible that this tension has no effect on the self-confidence and self-esteem of people who are supposed above all things to hate contradiction?

The question of what is interesting in mathematics is a practical question of the highest importance for anyone who is active in research or who is involved in hiring and promoting people who do research. Is it not astonishing that there is no public discussion on this question, no vehicle for public discussion of it, hardly even a language or viewpoint which could be used for such a discussion?

This is *not* to say that there can or should be explicit, agreed-upon standards of mathematical taste. On the contrary. Precisely because tastes differ, discussion on matters of taste is possible and necessary. Our very existence as a single profession, and our ability to agree in practice that certain deeds in mathematics are deserving the highest praise and reward, prove that there are common standards of excellence which we use as criteria for evaluating our work. To make these criteria explicit, to bring them into the open for discussion, challenge, and controversy, would be one important philosophical activity for mathematicians. Our inability to sustain such a public discussion on values in mathematics is an aspect of philosophical unawareness and incompetence.

The problems of truth and meaning are not technical issues in some recondite branch of logic or set theory. They confront anyone who uses or teaches mathematics. If we wish, we can ignore them. To do so, however, is to leave oneself the prisoner of one's unexamined philosophical preconceptions. It would be surprising if this had no practical consequences.

Let us pause to consider two possible examples of such practical consequences. The last half-century or so has seen the rise of formalism as the most frequently advocated point of view in mathematical philosophy.[1] In this same period, the dominant style of exposition in mathematical journals, and even in texts and treatises, has been to insist on precise details of definitions and proofs, but to exclude or minimize discussion of why a problem is interesting, or why a particular method of proof is used.

It would be difficult or impossible to document the connection between formalism in expository style and formalism in philosophical attitude. Still, ideas have consequences. One's conception of what mathematics *is* affects one's conception of how it should be presented. One's manner of presenting it is an indication of what one believes to be most essential in it.

Another example is the importation, during the '60's, of set-theoretic notation and axiomatics into the high-school curriculum. This was not an inexplicable aberration, as its critics sometimes seem to imagine. It was a predictable consequence of the philosophical doctrine that reduces all mathematics to axiomatic systems expressed in set-theoretic language.

The criticism of formalism in the high schools has been primarily on pedagogic grounds: "This is the wrong thing to teach, or the wrong way to teach." But all such arguments are inconclusive if they leave unquestioned the dogma that real mathematics *is* precisely formal derivations from formally stated axioms. If this philosophical dogma goes unchallenged, the critic of formalism in the schools appears to be advocating a compromise in quality: he is a sort of pedagogic opportunist, who wants to offer the student less than the "real thing." The issue, then, is not, What is the best way to teach? but, What is mathematics really all about? To discredit formalism in pedagogy, one must challenge its philosophical base: the formalist picture of the nature of mathematics. Controversies about high-school teaching cannot be resolved without confronting problems about the nature of mathematics. In the end, the critique of formalism can be successful only through the development of an alternative: a more convincing, more satisfactory philosophical account of the meaning and nature of mathematics.[2]

Mathematicians themselves seldom discuss the philosophical issues surrounding mathematics; they asume that someone else has taken care of this job. We leave it to the professionals.

But the professional philosopher, with hardly any exception, has little to say to the professional mathematician. Indeed, he has only a remote and inadequate notion of what the professional mathematician is doing. Certainly this fact is not discreditable; it is to be expected, in view of the formidable technical prerequisites for understanding what we do.

Still, it has to be said that if a mathematician, uncomfortable with his philosophical confusion, looks for help in the books and journals in his library, he will be badly disappointed. Some philosophers who write about mathematics seem unacquainted with any mathematics more advanced than arithmetic and elementary geometry. Others are specialists in logic or axiomatic set theory; their work seems as narrowly technical as that in any other mathematical specialty.

There are professional philosophers of science who seem to be reasonably conversant with quantum mechanics and general relativity. There do not seem to be many professional philosophers who know functional analysis or algebriac topology or stochastic processes. Perhaps there is not need to know such things, if mathematics can really be reduced to logic or arithmetic or set theory. But such a presumption is itself a philosophical stand which is (to put it mildly) subject to challenge.

There are a few penetrating comments on mathematics in Polanyi's "Personal Knowledge." But then, Polanyi was really a chemist. And there is the beautiful work "Proofs and Refutations" by I.M. Lakatos [17]. This dissertation, written under the influence of Karl Popper and George Pólya, is the most interesting and original contribution to the philosophy of mathematics in recent decades. The fact that Lakatos' work remains almost unknown to American mathematicians in a striking illustration of our intellectual blinders.

There are, indeed, occasional philosophical comments by leading mathematicians whose interests are not confined to set theory and logic. But the art of philosophical discourse is not well developed today among mathematicians, even among the most brilliant. Philosophical issues just as much as mathematical ones deserve careful argument, fully developed analysis, and due consideration of objections. A bald statement of one's own opinion is not an argument, even in philosophy.

In the usual university mathematics curriculum, the only philosophical questions considered are those raised by the various foundationist schools of 50 years ago. In regard to these, it is mentioned that none of them was able to carry out its program, and that there is no real prospect that any of them can resolve the problem of "foundations."

Thus, if we teach our students anything at all about the philosophical problems of mathematics, it is that there is only one problem of interest (the problem of the foundation of the real number system), and that problem seems totally intractable.

Nevertheless, of course, we do not give up mathematics. We simply stop thinking about it. Just *do* it. That, more or less, is the present situation in the philosophy of mathematics.

2 HOW DID WE GET HERE?

This dilemma of Platonism versus formalism, of a vacillation between two unacceptable philosophies, is a characteristic of our own historical epoch.

How did it come about?

I would like to suggest a historical schema—a conjecture, which perhaps could be investigated by a suitably qualified historian.

Even as an impressionistic conjecture, it may help give us an orientation on our present situation.

Until well into the nineteenth century, geometry was regarded by everybody, *including mathematicians,* as the firmest, most reliable branch of knowledge. Analysis derived its meaning and its legitimacy from its link with geometry.

I do not say "Euclidean geometry," because the use of the qualifier became necessary and meaningful only after the possibility of more than one geometry had been recognized. Before that, geometry was simply geometry—the study of the properties of space. These existed absolutely and independently, were objectively given, and were the supreme example of properties of the universe which were exact, eternal, and knowable with certainty by the human mind.

In the nineteenth century, several disasters took place.

One disaster was the discovery of non-Euclidean geometries, which showed that there was more than one thinkable geometry.

A greater disaster was the development of analysis so that it overtook geometrical intuition. The discovery of space-filling curves and continuous nowhere-differentiable curves were stunning surprises which showed the vulnerability of the one solid foundation—geometric intuition—on which mathematics had been thought to rest.

The situation was intolerable because geometry had served, from the time of Plato, as the supreme exemplar of the possibility of certainty in human knowledge. Spinoza and Descartes followed the "more geometrico" in establishing the existence of God, as Newton followed it in establishing his laws of motion and gravitation. The loss of certainty in geometry was philosophically intolerable, because it implied the loss of all certainty in human knowledge.

The mathematicians of the nineteenth century, of course, proved equal to the challenge. Led by Dedekind and Weierstrass, they turned from geometry to arithmetic as the foundation for mathematics.

Gradually it became clear that in reducing the continuum to arithmetic, one required a kind of mathematics which had hitherto gone unnoticed—set theory.

Set theory at first seemed to be almost the same thing as logic, and so the hope then appeared that instead of arithmetic, set theory-logic could serve as the foundation for all mathematics. It was not to be. As Frege put it in his famous postscript, "Just as the building was completed, the foundation collapsed." That is, Russell communicated to him the Russell paradox.

This was the "crisis in foundations," the central issue in the famous controversies of the first quarter of this century. Three principal remedies were proposed:

The program of "logicism," the school of Frege and Russell, was to find a reformulation of set theory, which could avoid the Russell paradox and thereby save the Frege-Russell-Whitehead program of establishing mathematics upon logic as a foundation.

The work on this program played a major role in the development of logic. But it was a failure in terms of its original intention. By the time set theory had been patched up to exclude the paradoxes, it was a complicated structure which one could hardly identify with "logic" in the philosophical sense of "the rules for correct reasoning." So it became untenable to argue that mathematics is nothing but logic—that mathematics is one vast tautology.

> I wanted certainty in the kind of way in which people want religious faith. I thought that certainty is more likely to be found in mathematics than elsewhere. But I discovered that many mathematical demonstrations, which my teachers expected me to accept, were full of fallacies, and that, if certainty were indeed discoverable in mathematics, it would be in a new field of mathematics, with more solid foundations than those that had hitherto been thought secure. But as the work proceeded, I was continually reminded of the fable about the elephant and the tortoise. Having constructed an elephant upon which the mathematical world could rest, I found the elephant tottering, and proceeded to construct a tortoise to keep the elephant from falling. But the tortoise was no more secure than the elephant, and after some twenty years

> of very arduous toil, I came to the conclusion that there was nothing more that I could do in the way of making mathematical knowledge indubitable. (Bertrand Russell, "Portraits from Memory.")

The response of Hilbert to this dilemma was the invention of "proof theory." The idea was to regard mathematical proofs as sequences of formal symbols, rearranged and transformed according to certain rules which correspond to the rules of mathematical reasoning. Then purely finite, combinatorial arguments would be found to show that the axioms of set theory would never lead to a contradiction. In this way, mathematics would be given a secure foundation—in the sense of a guarantee of consistency.

This kind of foundation is not at all the same as a foundation based on a theory known to be *true,* as geometry had been believed to be true, or at least impossible to doubt, as it is supposed to be impossible to doubt the law of contradiction in elementary logic.

The formalist foundation, like the logicist foundation, tried to buy certainty and reliability at a price. As the logicist interpretation tried to make mathematics safe by turning it into a tautology, the formalist interpretation tried to make it safe by turning it into a meaningless game. The "proof-theoretic program" comes into action only after mathematics has been coded in a formal language and its proofs written in a way checkable by machine. As to the *meaning* of the symbols, that becomes something extra-mathematical.

It is important to realize that Hilbert's writings and conversation display full conviction that mathematical problems are questions about real objects, and have meaningful answers which are true in the same sense that any statement about reality is true. If he nevertheless was prepared to advocate a formalist interpretation of mathematics, this was the price he considered necessary for the sake of obtaining certainty.

> The goal of my theory is to establish once and for all the certitude of mathematical methods. . . . The present state of affairs where we run up against the paradoxes is intolerable. Just think, the definitions and deductive methods which everyone learns, teaches and uses in mathematics, the paragon of truth and certitude, lead to absurdities! If mathematical thinking is defective, where are we to find truth and certitude? (Hilbert [12].)

As it happened, certainty was not to be had, even at this price. Gödel's incompleteness theorems showed that the Hilbert program was unattainable—that any formal system strong enough to contain elementary arithmetic would be unable to prove its own consistency.

Instead of providing foundations for mathematics, Russell's logic and Hilbert's proof theory became the starting points for new branches of mathematics. Model theory and other branches of mathematical logic have become an intrinsic part of the whole structure of contemporary mathematics—and as much or as little in need of foundations as the rest of the structure.

The third famous school that competed with the logicist and the formalist was the intuitionist. Brouwer's position was that the natural numbers were reliable and needed no deeper foundation; and that the only acceptable parts of mathematics were those that could be derived from the natural numbers "constructively." His notion of constructivity was strict enough to exclude the real number system as it is usually understood. As a consequence, even though his opinions were accepted at least in part by such men as Hermann Weyl and Henri Poincaré, the vast majority of mathematicians continued to work nonconstructively.

(Some aspects of the intuitionist viewpoint are still attractive to mathematicians who are seeking an alternative to Platonism and formalism; in particular, the insistence that mathematics be meaningful, and that mathematics be viewed as a certain kind of human mental activity. One can accept these ideas, while rejecting the dogma that any mathematics which cannot be obtained "constructively" from the natural numbers is deficient in meaning.)

This story is probably too long and familiar for many readers. But it makes the point: All three foundationist schools shared the same presupposition. For us today, in view of their common failure, the common presupposition is more important than the much-emphasized differences. By bringing out and challenging this presupposition, we can escape from the quagmire where mathematical philosophy has been trapped for fifty years.

The common presupposition was that mathematics must be provided with an absolutely reliable foundation. The disagreement was on strategy, on what had to be sacrificed for the sake of the agreed-on goal. But the goal was never attained, and there are few who still hope for its attainment.

At this point we can see the reason for the "working mathematician's" uneasy oscillation between formalism and Platonism. Our inherited *and unexamined* philosophical dogma is that mathematical truth should possess absolute certainty. Our actual experience in mathematical work offers uncertainty in plenty. Platonism and formalism, each in its own way, provide a nonhuman "reality" where one might imagine absolute certainty dwells.

Pick some familiar theorem: for example, the uncountability of the continuum; Cauchy's integral formula; the fundamental theorem of algebra.

Is it a true statement about the world? Does one *discover* such a theorem, and does such a discovery *increase* our knowledge?

If you answer yes to such questions, you may be called a *Platonist* (or a "realist"). You will then be faced with the next question: to what objects or features of the world do such statements refer? One does not meet roots of polynomials (or uncountable sets) or integrals of analytic functions while walking down the street, or even while traveling in outer space. Where, outside of our thoughts, can one encounter roots of polynomials, or uncountable sets?

Perhaps such things do not have any real existence after all, and the conviction that they exist and are objectively knowable is merely an illusion in which we indulge ourselves. Perhaps a theorem is nothing more than a formula that can be derived by the rules of logic from some given set of formulas (*axioms,* if you will).

If you prefer to retreat to this modest disclaimer, you may be called a formalist. Since you have now renounced any claim that mathematics is meaningful, you are no longer under the difficulty of analyzing its meaning. But this does not leave you free from philosophical difficulties. On the contrary. You now may be asked, how is it that all three of the examples we have given were known, understood and used long before the axioms on which they are "based" had been stated? If we say that a theorem has no meaning except as a conclusion from axioms, then do we say that Gauss did not know the fundamental theorem of algebra, Cauchy did not know Cauchy's integral formula, and Cantor did not know Cantor's theorem?

The basis for Platonism is the awareness we all have that the problems and concepts of mathematics exist independently of us as individuals. The zeroes of the zeta function are where they are, regardless of what I may think or know on the subject. It is then easy for me to imagine that this objectivity is given outside of human consciousness as a whole, outside of history and culture. This is the myth of Platonism. It remains alive because it corresponds to something real in the daily experience of the mathematician. Yet it remains alive only as a halfhearted, shamefaced Platonism, because it is incompatible with the general philosophy or world-view of most scientists—including mathematicians.[3] Platonism in the full sense—*belief in the existence of ideal entities, independent of or prior to human consciousness*—is of course tenable within a religious world-view (belief in a divine Mind.) For those whose general world view excludes mysticism, Platonism in the full sense is very difficult to maintain once the full force of scientific skepticism is focused on it.[4]

At this point the alternative becomes formalism. Instead of believing that our theorems are (or should be) truths about eternal extra-human ideals, we say instead that they are merely assertions about transformations of symbols (formal derivations). This viewpoint also involves an act of faith. How, indeed, do we *know* that our latest theorem about diffusion on manifolds is formally deducible from Zermelo–Fraenkel set theory? No such formal deduction is ever written down. If it were, and it were checked by a human reader, the likelihood of error would be greater than in checking an ordinary (not formalized) mathematical proof.

Platonism and formalism, each in its own way, falsify part of the reality of our daily experience. Thus we speak as formalists when we are compelled to face the mystical, antiscientific essence of Platonic idealism; we return to Platonism when we realize that formalism as a description of mathematics has only a distant resemblance to our actual knowledge of mathematics.

The claim I wish to advance in this paper is that we can abandon them both, if we abandon the search for absolute certainty in mathematical truth. What we can have instead is a philosophy that is true to the reality of mathematical experience, at the price of violating some ancient philosophical dogmas.

3 ANECDOTES AND GOSSIP

Let us clear our minds by turning away from the philosophical alternatives we are accustomed to, and turning instead to our actual experience.

Anyone who has ever been in the least interested in mathematics, or has even observed other people who were interested in it, is aware that mathematical work is work with ideas. Symbols are used as aids to thinking just as musical scores are used as aids to music. The music comes first, the score comes later. Moreover, the score can never be a full embodiment of the musical thoughts of the composer. Just so, we know that a set of axioms and definitions is an attempt to describe the main properties of a mathematical idea. But there may always remain an aspect of the idea which we use implicitly, which we have not formalized because we have not yet seen the counterexample that would make us aware of the possibility of doubting it.

The fact is that it is sometimes extraordinarily difficult to achieve understanding, certainty, or clarity in mathematics.

In every branch of contemporary mathematics, one hears a version of the following story (always by word of mouth, never in print).

"Many of the most important theorems of our subject were first discovered by the great Professor Nameless. His intuition was so powerful that he was able to come to his conclusions by methods that no one else was able to understand. Years later, others were able to find proofs of his results by arguments that could be followed by all the workers in the field. Of course, it turned out that (with perhaps one or two exceptions) all of Nameless' formulas and theorems were true. It was just that no one was quite able to follow his explanations of how he discovered them." I am certainly not going to violate tradition by filling in the missing name. The same story is told by probabilists, by partial differential equators, by algebraists and by topologists—only the name of the hero changes. This kind of knowledge *before* complete proof is inexplicable in terms of the formalist account of mathematics.

To give another instance—in an invited talk at an International Congress of Mathematicians, a famous professor describes some of his latest results. He adds that the correctness of these results is not quite certain, because there has not yet been time for other specialists in his area to check them, and of course, until you have checked with other people, you can never be quite sure you haven't overlooked something.

Even the greatest mathematicians make mistakes, sometimes important ones, and these may be found even in famous papers which have been well known for a long time.

In the *Proceedings of the American Mathematical Society,* September 1963, there appeared an article entitled "False Lemmas in Herbrand," by Dreben, Andrews, and Aanderaa. They showed that certain lemmas in a thesis published by Herbrand in 1929 are false. These lemmas are used in the proof of a theorem which has been well known and influential in logic for fifty years. The authors show how Herbrand's theorem may be proved by replacing the false lemmas with correct ones.[5]

In the *Bulletin of the American Mathematical Society,* March 1975, there appeared an article by S. Hellerstein and J. Williamson, entitled "Derivatives of Entire Functions and a Question of Pólya." They wrote: "In 1914, Pólya asked: If an entire function f and all its derivatives have only real zeroes, is f in U_0? (the Pólya-Laguerre class). In 1, 2] M. Alander proved that the answer to Pólya's question is affirmative for all f in U_{2p} with $p \leq 2$ and in [3] purported to have extended this result to arbitrary p. However, in a famous survey article on zeros of successive derivatives, Pólya refers to Alander's papers [1] and [2] but not to his more general result [3]. The first author of this announcement, while a graduate student under the direction of A. Edrei, brought this curious omission to the latter's attention. In response to Edrei's subsequent query, Pólya replied in a letter that he was aware of Alander's more general "proof" but was never convinced by it nor could he show that it was fallacious! Alander's proof involves a study of level curves of harmonic functions associated with functions in U_{2p}. Avoiding such geometric considerations, and using instead direct analytic arguments, we have succeeded in proving a stronger version of Alander's 'theorem.' "

Notice that both Alander's and Herbrand's theorems were true—even though their proofs were defective. This is the most typical case. Why is it so?

A very interesting article by Philip Davis [6] contains, among other things, a discussion of errors in mathematical publications, with some famous names and examples.

Davis suggests that the length and interdependence of mathematical proof mean that truth in mathematics is probabilistic. I think his argument shows something else: that mathematical *knowledge* is fallible, and in this respect similar to other kinds of knowledge.

Let us mean by "intuitive reasoning" or "informal reasoning" that reasoning in mathematics which depends on an implicit background of understanding, and which deal with concepts rather than symbols, as distinguished from calculation, which deals with symbols and can be mechanized. Then the checking of an analytic-algebraic proof, as actually done by a mathematician, is primarily a piece of intuitive reasoning. But there are many different kinds of intuitive reasoning. The proof that the angle sum of a Euclidean triangle equals two right angles can be written in a formal language and deduced using only modus ponens. To *understand* such a proof, the reader would have to supply a meaning to these statements—that is, he would have to reason intuitively. On the other hand, if the proof is given by drawing the familiar diagram, there is a different kind of intuition in which several steps of the symbolic proof are merged into a single insight. We have a choice, not between an intuitive fallible mode of reasoning and a formal, infallible mode, but between two modes of reasoning (verbal and diagrammatic) both of which are intuitive and fallible. (Parenthetical aside: The reasoning by words can be formalized, and this formalization itself can be studied for certain purposes. But it is entirely likely that the drawing of diagrams can also be formalized; see [7].)

All this is not to deny the existence of an interpersonally verifiable notion of "correct proof" at the intuitive level of the working mathematician. It is merely to point out that this notion is not very similar to the model of formal proof in which correctness can always be verified as a mechanical procedure.

We do not have absolute certainty in mathematics; we may have virtual certainty, just as in other areas of life. Mathematicians disagree, make mistakes and correct them, are uncertain whether a proof is correct or not.

Faced with these obvious facts, one has three choices. The commonest is hypocrisy. That is, pretend not to notice the gap between preaching and practice.

If we renounce hypocrisy, then we have to give up either the myth or the reality. Either say that mathematics as practiced every day by mathematicians is not what mathematics really ought to be, or else say that the theory, that mathematical proof is really (or approximately or in principle) a mechanical procedure, is not quite right.

A common response is to say, "True, we aren't always as careful or thorough as we should be, but that doesn't detract from the ideal."

In one sense this is unarguable. Certainly, we should try our best not to make mistakes. But if it is meant that we really ought to (if we only had the time and energy) write our proofs in a form that could be checked by a computing machine, then the point *is* certainly arguable. Especially by anyone with experience debugging programs!

It just is *not the case* that a doubtful proof would become certain by being formalized. On the contrary, the doubtfulness of the proof would then be replaced by the doubtfulness of the coding and programming.

What really happens every day is that the correctness of a formal proof (i.e., of code written for a computing machine) is checked by a human being who uses his understanding of the *meaning* of the steps of the computation to verify its formal correctness.

As it has become commonplace to use very large, complicated programs, it has become recognized that it is essential to write these programs in a manner to be readable by human beings—that is, to be understandable, not just formally correct. True, we cannot give a formal definition of "understandable." Nevertheless, it turns out in practice that it is *understanding that verifies the correctness of formal computation—not only the other way round.*

4 WHERE DO WE GO FROM HERE?

The discussion in Sections 2 and 3 was intended to make two points:

(1) The unspoken assumption in all foundationist viewpoints is that mathematics must be a source of indubitable truth.

(2) The actual experience of all schools—and the actual daily experience of mathematicians—shows that mathematical truth, like other kinds of truth, is fallible and corrigible.

Do we really have to choose between a formalism that is falsified by our everyday experience, and a Platonism that postulates a mythical fairyland where the uncountable and the inaccessible lie waiting to be observed by the mathematician whom God blesses with a good enough intuition? It is reasonable to propose a new task for mathematical philosophy: not to seek indubitable truth, but to give an account of mathematical knowledge as it really is—fallible, corrigible, tentative and evolving, as is every other kind of human knowledge. Instead of continuing to look in vain for foundations, or feeling disoriented and illegitimate for lack of foundations, we can try to look at what mathematics really is, and account for it as a part of human knowledge in general. That is, reflect honestly on what we do when we use, teach, invent, or discover mathematics—by studying history, by introspection, and by observing ourselves and each other with the unbiased eye of Martians or anthropologists.

Such a program requires a philosophical position which is radically different from the three classical points of view (formalist, Platonist, intuitionist). The position I will try to present differs from all three of them in the following sense. It denies the right of *any* a priori philosophical dogma to tell mathematicians what they should do, or what they really are doing in spite of themselves or without knowing it. Rather, it takes as its starting point the attitude that mathematics, as it is being done now and as it has evolved in history, is a reality which does not require justification or reinterpretation. What has to be done in the philosophy of mathematics is to explicate (from the outside, as part of general human culture, rather than from the inside, within mathematical terms) what mathematicians are doing. If this attempt is successful, the result will be a description of mathematics which mathematicians will recognize as true. It will be the kind of truth that is obvious once it is said, but up to then was perhaps too obvious for anyone to bother saying.

There is a comparison with the philosophy of science. At one time philosophers of science wrote elaborate rules of inductive discovery which scientists were supposed to follow. The fact that one could hardly find a scientist who had made a discovery in such a fashion seemed quite irrelevant to them. More recently, K. Popper and M. Polanyi have described science in a different manner, more closely related to a real knowledge of how science develops, and not so much based on the traditional philosophizing of Francis Bacon or John Stuart Mill. These writings of Popper and Polanyi are not completely ignored by practicing scientists. On the contrary, some scientists have testified that their work has benefited by the insights they received from these works on the philosophy of science.

We can try to describe mathematics, not as our inherited prejudices imagine it to be, but as our actual experience tells us it is. Certainly our experience does not tell us that it is a game with symbols (formalism) nor that it is a direct perception of ideal entities (Platonic idealism).

What would be the most straightforward, natural answer to the question, what is mathematics?

It would be that mathematics deals with ideas. Not pencil marks or chalk marks, not physical triangles or physical sets, but ideas (which may be represented or suggested by physical objects). What are the main properties of mathematical activity or mathematical knowledge, as known to all of us from daily experience?

(1) Mathematical objects are invented or created by humans.

(2) They are created, not arbitrarily, but arise from activity with already existing mathematical objects, and from the needs of science and daily life.

(3) Once created, mathematical objects have properties which are well-determined, which we may have great difficulty in discovering, but which are possessed independently of our knowledge of them. (For example, I define a function as the solution of a certain boundary-value problem. Then

the value of the function at some interior point is determined, although I may have no effective way of finding it out.)

These three points are not philosophical theses which have to be established. They are facts of experience which have to be understood. What has to be done is to analyze their paradoxes, and to examine their philosophical consequences.

To say that mathematicial objects are invented or created by humans is to distinguish them from natural objects such as rocks, X rays, or dinosaurs.

Recently, certain philosophers (Korner, Putnam) have argued that the subject matter of pure mathematics *is* the physical world—not its actualities but its possibilities. To exist in mathematics, they propose, means to exist *potentially* in the physical world. This view has the merit that it does permit us to say that mathematical statements have meaning, can be true or false. It has the defect, however, that it attempts to explain the clear by means of the obscure. Consider the theorem $2^c < 2^{(2^c)}$, or any theorem in homological algebra. No philosopher has yet explained in what sense such theorems should be regarded as referring to physical "possibilities."

The common sense standpoint of the working mathematician is that the objects of algebra, say, or of set theory, are just that—part of a theory. They are human ideas, of recent invention. They are not timelessly or tenselessly existing either as Platonic ideas or as latent potentialities in the physical world.

We may ask how these objects, which are our own creations, so often turn out to be useful in describing aspects of nature. To answer this specifically in detail is important and complicated. It is one of the major tasks for the history of mathematics, and for a psychology of mathematical cognition which may be coming into birth in the work of Piaget and his school. The answer in general, however, is easy and obvious. Human beings live in the world and all their ideas ultimately come from the world in which they live—refracted through their culture and history, which are in turn, of course, ultimately rooted in man's biological nature and his physical surroundings. Our mathematical ideas fit the world for the same reason that our lungs are suited to the atmosphere of this planet.[6]

Once created and communicated, mathematical objects are *there*. They become part of human culture, separate from their originator. As such, they are now objects, in the sense that they have well-determined properties of their own, which we may or may not be able to discover.

If this sounds paradoxical, it is because of a habit of thinking which sees in the world only two kinds of reality: the individual subject (the isolated ego) on the one hand, and the exterior world of nature on the other.

The existence of mathematics is enough to show the inadequacy of such a world view. The customs, traditions, and institutions of our society—all our nonmaterial culture—are aspects of the world which are neither in the private "inner" nor the nonhuman "outer" world.[7] Mathematics is also this third kind of reality—a reality that is "inner" from the viewpoint of society as a whole, yet "outer" from the viewpoint of each individual member of society.

That mathematical objects have properties which are well determined is as familiar as the fact that mathematical problems often have well-determined answers.

To explain more fully how this comes about is again a matter for actual investigation, not speculation. The rough outlines, however, are visible to anyone who has studied and taught mathematics.

To have the idea of counting, one needs the experience of handling coins or blocks or pebbles. To have the idea of an angle, one needs the experience of drawing straight lines that cross, on paper or in a sandbox. Later on, mental pictures or sample calculations prepare the ground for other new concepts.[8] A suitable shared experience of activity—first physical manipulation, later on, paper and pencil calculation—creates a common effect.

Of course, not everyone experiences the desired result. The student who never catches on to how we want him to handle the parentheses in our algebraic expression simply doesn't pass the course.

Why are we able to talk to each other about algebra? *We have been trained to do so, by a training that has been evolved for that purpose.* We can do this *without* being able to verbalize a formal definition of polynomials. Polynomials are objective, in the sense that they have certain properties, whether we know them or not. That is to say, our common notion has implicit properties. To unravel how this is so is a deep problem comparable to the problem of linguistics. No one understands clearly how it is that languages have mysterious, complicated properties unknown to the speakers of the language. Still, no one doubts that the locus of these properties is in the culture of the language speaker—not in the external world nor in an ideal other world. The properties of mathematical objects, too, are properties of shared ideas.

The observable reality of mathematics is this: we see an evolving network of shared ideas which have objective properties; these properties are ascertained by many kinds of reasoning and argument. These kinds of valid reasonings, which are called "proofs," are not universal; they differ from one branch of mathematics to another, and from one historical epoch to another.

Looking at this fact of human experience, there certainly is matter for explication.

How are mathematical objects invented?

What is the interplay of existing mathematics, ideas and needs from other branches of science, and direct mirroring of physical reality?

How does the notion of proof develop, becoming more refined and subtle as new dangers and sources of error are discovered?[9]

Does the network of mathematical ideas and reasoning, as part of our shared consciousness, have an integrity as a whole that is more than the strength of any one link in the reasoning, so that the collapse of any one part can affect only those parts closest to it?

These sorts of philosophical questions can be studied by the historian of mathematics—if we allow, as we should, his field of study to extend up to yesterday and today. The famous work of Thomas Kuhn is a paradigm of the kind of insight in the philosophy of science that is possible only on the basis of historical studies. Such work has yet to be done in the philosophy and history of mathematics.[10]

Such studies will never make mathematical truth indubitable. But then, why should mathematical truth be indubitable?

In daily life, we well know that our knowledge is subject to correction, is partial and incomplete. In the natural sciences, it is accepted that scientific progress consists of enlarging, correcting, and sometimes even rejecting and replacing the knowledge of the past. It is the possibility of correcting errors by confronting them with experience that characterizes scientific knowledge. This is precisely the reason why it is essential that we share our ideas and check each other's work.

This account of mathematics contains nothing new. It is merely an attempt to describe what mathematicians actually are doing and have been doing for centuries.

The novelty, if any, is the conscious attempt to avoid falsification or idealization.

SUMMARY AND CONCLUSION

The alternative of Platonism and formalism comes from the attempt to root mathematics in some nonhuman reality. If we give up the obligation to establish mathematics as a source of indubitable truths, we can accept its nature as a certain kind of human mental activity.

In doing this, we give up some age-old hopes; we may gain a clearer idea of what we are doing, and why.

> Could it be that in mathematics too we need a new Consciousness? . . . A new consciousness stressing the exchange, communication and experience of mathematical information, a Consciousness where mathematics is told in human words rather than in a mass of symbols, intelligible only to the initiated; a Consciousness where mathematics is experienced as an enlightening intellectual activity rather than an almost fully automated logical robot, ardently performing simultaneously a large number of seemingly unrelated tasks. (P. Henrici, *Quart. Appl. Math.* (April 1972), 38.)

A world of ideas exists, created by human beings, existing in their shared consciousness. These ideas have properties which are objectively theirs, in the same sense that material objects have their own properties. The construction of proof and counterexample is the method of discovering the properties of these ideas. This is the branch of knowledge which we call mathematics.

COMMENTS ON THE BIBLIOGRAPHY

The present article is strongly influenced by Lakatos' critique of formalism presented in the first few pages of [17] and accepts his aim [15] "to exhibit modern mathematical philosophy as deeply embedded in general epistemology and as only to be understood in this context."

No attempt is made here to discuss in detail the issues raised by intuitionism and constructivism. These were presented by Bishop, Stolzenberg, and

Kopell at a symposium published in *Historia Mathematica* 2 (November 1975). The spokesmen for the "classical" viewpoint at that symposium were remarkably unwilling to deal with the philosophical issues raised by Bishop. A conscientious evaluation of intuitionism from the classical point of view has been given by a physicist; see Bunge [3].

A "Platonist" viewpoint is espoused by Steiner [25], and a formalist one by Dieudonné [8]. Monk [18], Cohen [4], and Robinson [22] discuss the Platonist–formalist duality in the light of Cohen's results on independence of the continuum hypothesis and the axiom of choice. Putnam's "modal-logic" version of realism is presented in his recent book [21].

NOTES

1. See, e.g., [8].

2. These issues are developed by Thom [26, 27] and Dieudonné [10].

3. Two whole-hearted Platonists are R. Thom ("Everything considered, mathematicians should have the courage of their most profound convictions and thus affirm that mathematical forms indeed have an existence that is independent of the mind considering them. . . . Yet, at any given moment, mathematicians have only an incomplete and fragmentary view of this world of ideas" [26].) and K. Gödel ("Despite their remoteness from sense experience, we do have something like a perception also of the objects of set theory, as is seen from the fact that the axioms force themselves upon us as being true. I don't see any reason why we should have less confidence in this kind of perception, i.e., in mathematical intuition, than in sense perception. . . . They, too, may represent an aspect of objective reality" [11].). Thom's world of ideas is geometric, whereas Gödel's is the set-theoretic universe.

4. "I cannot imagine that I shall ever return to the creed of the true Platonist, who sees the world of the actual infinite spread out before him and believes that he can comprehend the incomprehensible" (Robinson [22]).

5. I am indebted to Rohit Parikh for the information that for many years Herbrand's thesis was not physically accessible to most logicians. Presumably his errors would have been corrected much sooner in normal circumstances.

6. "I have met people who found it astonishing that the cats have holes in their furs exactly at the places where the eyes are." (I am indebted to Wilhelm Magnus for this quotation from Lichtenberg, an 18th-century professor of physics at Göttingen.)

7. Related ideas are advocated by Popper [20] and especially by White [28]. They are implicit in the well-known writings of R.L. Wilder on mathematics as a cultural phenomenon. In a different sense, they are also implicit in the writings on "heuristic" of George Pólya and their philosophical elaboration by Imre Lakatos.

8. The work of Piaget [19] is little read by professional mathematicians, perhaps in part because some of his comments on groups and other abstract mathematical structures seem naive or misinformed. Nevertheless, one cannot overestimate the importance of his central insight: that mathematical intuitions are not absorbed from nature by passive observation, but rather are created by the experience of active manipulation of objects and symbols. The full import of this insight for mathematical epistemology has yet to be appreciated.

9. "Historically speaking, it is of course quite untrue that mathematics is free from contradiction; non-contradiction appears as a goal to be achieved, not as a God-given quality that has been granted us once for all. . . . There is no sharply drawn line between those contradictions which occur in the daily work of every mathematician, beginner or master of his craft, as the result of more or less easily detected mistakes, and the major paradoxes which provide food for logical thought for decades and sometimes centuries." (N. Bourbaki, "Foundations of Mathematics for the Working Mathematician," *J. Symbolic Logic 14* (1949), 1–8.)

10. "Under the present dominance of formalism, one is tempted to paraphrase Kant: the history of mathematics, lacking the guidance of philosophy, has become *blind*, while the philosophy of mathematics, turning its back on the most intriguing phenomena in the history of mathematics, has become empty" (Lakatos [17]). However, recent work in the history of mathematics shows an increasing interest in philosophical issues. See, for example, the articles on historiography in *Historia Mathematica 2* (November 1975).

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A point of view similar to that expressed here was put forward recently in a very interesting paper by three computer scientists, R.A. DeMillo, R.J. Lipton, and A.J. Perlis, "Social Processes and Proofs of Theorems and Programs," presented at the SIGPLAN Conference, Principles of Programming Languages, May 1977, Los Angeles (Proceedings, pp. 206–214).

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