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MATHEMATICS AS AN OBJECTIVE SCIENCE

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1. Introduction. Morris Kline has written that “mathematics is a body of knowledge. But it contains no truths.” [13, p. 9] Views of this general kind, which deny that mathematics has objective scientific content, are widely held by mathematicians and are disseminated in classrooms and in popular books such as Kline’s. I believe that such views are false and that their dissemination does no good for our own or others’ respect for our subject. Below I shall examine four views which, though they do not exhaust the current range of opinion in the philosophy of mathematics, are nevertheless sufficiently representative to raise what seem to me to be the main issues about the objectivity of mathematics. I shall argue that each of these views arises from an oversimplification of what happens when we do mathematics.

2. Surfacism. In order to bring out some of the features which the views I want to oppose have in common, let me begin with an imaginary analogous view in the philosophy of physics. Many of the qualities we associate with material objects—such as definite shape, hardness, color—can be thought of as qualities of their surfaces. Consider a philosopher who is misled by this simple observation and believes that all qualities of material objects are qualities of their surfaces. He holds, let us say, that material objects are not solid, as we usually suppose, but instead are infinitely thin surfaces. It is meaningless, on his view, to speak of the inside of a material object. Since no one would refer to his own position as “superficialism,” we may imagine that our philosopher calls his view “surfacism.” Asked to explain the fact that when we cut into an object we do not just find a void, our surfacist says that the edge of the knife pulls on the surface to which it is applied, thereby stretching that surface so as to create two new surfaces. Asked to give an account of a quality which is difficult to treat consistently as a quality of surfaces, such as weight, he asserts that the quality is illusory. What is actually going on, he claims, is that certain qualities of the surfaces of our bodies, or of our interactions with other surfaces, are being projected into the external world. For example, suppose we consider the case of weight more carefully. The weight of an object is really just the difficulty I have in lifting it. That difficulty must, strictly speaking, be located in those points at which the object and my body interact. Hence the weight must reside in the common surface of the object and my body. It is a gratuitous oversimplification to think of the weight as a quality of the material object in and of itself.

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We need not suppose that our surfacist philosopher is always on the defensive. He may maintain, for instance, that the conventional view first violates the principle of parsimony by creating an entirely unnecessary entity—the inside of the object—and then goes on to give that entity absurd qualities. The inside, for one example, is supposed to be material but invisible. Why should insides be so different from outsides? Whence this asymmetry? If there really is space inside material objects, would it not be more reasonable to suppose that, like the space outside material objects, the space inside is filled with air? Occasionally, we may suppose, our surfacist complains about the unscientific and superstitious character of his opponents' views. Belief in the solid inside of a material object, he asserts, is a remnant of belief in the immortal soul, which was the "solid" inside of a human being. As a matter of fact, he argues, the usual account is simply incomprehensible. Who can visualize a material object except by visualizing its surface? Who, when visualizing a material object, can visualize anything in addition to its surface?

It seems to me that the views about the nature of mathematics that I wish to discuss are forms, more or less disguised, of surfacism. Hence it will be useful for me to consider how one might refute surfacism in the pure form just described.

The purpose of having a view about the nature of material objects is to order our experiences of those objects in a way which is useful in our dealings with them. Such a view is a social artifact which serves a variety of social functions. Material objects are themselves public in character, and most of my interactions with my material environment are, directly or indirectly, also interactions with my social environment. It follows that the most important function which such a view must serve is to facilitate both those of our interactions with material objects which have public significance and those of our interactions with each other which are mediated by material objects which concern material objects. Hence a view about the nature of material objects which is intended to be more than a debating position should satisfy the following Principle of Objectivity: Anything which is practically real should be taken as objectively real.

Let me make this clearer. When I say that an attribute like weight is practically real, I mean that the attribute plays a role, and that there exists a consensus that the attribute does play a role and should play a role, in our interactions with the objects that have the attribute. It will follow that there is at least a rough consensus on the degree or kind of presence of the attribute in a particular object. For, to repeat what I said above, our interactions with objects are generally also interactions with each other. On the other hand, when I say that an attribute is taken as objectively real, I mean that it is taken to reside in the observed object rather than in the subjective experience of the observer or in the subjective relationship between the observer and the observed object. A theory about the nature of material objects, then, is only serious if it accepts as its data all those attributes which have a commonly accepted role in our ordinary social dealings with the objects. It must take those data and unite them into a coherent account, explaining some in terms of others no doubt, but not explaining any of them away. In particular, a theory will undermine our ordinary activities, rather than support them, if it treats attributes which are important in those activities as mere subjective illusion. Of course one can find examples in which entities that formerly appeared to play a role in our practical activities were later shown not to exist. Nevertheless, in an argument about the objective reality of something whose practical reality is evident, the whole burden of proof should fall on the proponent of the negative position. After all, the simplest explanation for the apparent practical importance of an entity is that the entity actually exists and actually plays a role in our practice. In the absence of a strong argument to the contrary, then, the presumption must be that anything practically real is objectively real.

To avoid possible misunderstandings, let me consider a case in which the principle of objectivity is satisfied. An argument one sometimes hears against taking physics literally is that in the world of the physicist there is no such thing as yellow. If that were true, it would be a powerful argument. Fortunately, it is not true.
First of all, it is important to distinguish our experience of yellow from the color itself. What is relevant to our public dealings with a material object is not how it appears to this observer or that observer under these conditions or those conditions. What is relevant is the actual color of the object—roughly speaking, how it appears to a normal observer under standard conditions. Thus it cannot be the task of physics, as opposed to psychology, to give an account of our experience of yellow.

It remains, however, that physics does not take color as an ingredient in its description of the world. Nevertheless, the usual account in terms of wave lengths of light does give objective content to talk about yellow. Colors are not denigrated or explained away. They are not made to reside in our eyes or in our minds. On the contrary, our ability to deal with color is enriched. Not only does the theory account for the observed properties of colors, but it makes possible their manipulation in new ways. The physicists have even found new colors (for example, in the infrared) which we cannot see.

Thus in this case the principle of objectivity is amply satisfied. The merely private aspects of our experience of color are dismissed as subjective. The practically real color itself, on the other hand, is supplied with objective content.

The principle of objectivity, then, may be used to refute surfacism as follows. The weight of a homogeneous material object is proportional to its volume and not to its surface area. It is reasonable to conclude that the weight of the object is distributed through it. Hence the surfacist must hold that weight is merely an illusion—not objectively real. But since weight is important in our dealings with material objects, and since it can be measured in a way which is interpersonally valid, the surfacist who declares weight to be illusory thereby trivializes his theory.

In order to apply these ideas to the philosophy of mathematics, we must observe that mathematics is a public activity. It occurs in a social context and has social consequences. Posing a problem, formulating a definition, proving a theorem are none of them private acts. They are all part of that larger social process we call science. A functioning mathematician is aware of the work of other mathematicians, publishes his own work, and expects other mathematicians to take his work into account. Thus a philosophy of mathematics is closely analogous to a view about the nature of material objects. Its main function should be to facilitate the ongoing social process of doing mathematics. It follows that a serious philosophy of mathematics must satisfy the principle of objectivity. That is, it must not deny objective reality to any aspects of mathematical activity which have practical reality.

3. Formalism. No one who observes the behavior of mathematicians can fail to notice that they manipulate symbols in accordance with rules. Thus our first attempt at a philosophy of mathematics might be to hold that mathematics is the rule-governed, or formal, manipulation of symbols and nothing else. (The phrase "and nothing else" is the mark of the surfacist.) This view is often called formalism. Positions more or less like this may be found in Haskell Curry [5], Abraham Robinson [17], and Paul Cohen [4]. (The views of David Hilbert, though often called "formalism," are quite different from the position we are discussing here, since Hilbert takes at least the finite, combinatorial part of mathematics to be meaningful and true. See, for example, Hilbert [12] or Kreisel [15].) An example of a different sort is provided by some computer scientists interested in artificial intelligence. They naturally want to think that human intelligence is not in principle different from what their computing machines are doing. Thus the human brain is assimilated to a computer, theories are assimilated to programs, and thought is assimilated to the operation of a Turing machine. After all, says the formalist, what else could mathematics be? Can you imagine a mathematician working in any way other than by manipulating symbols?

To make this somewhat more concrete, let us imagine asking a formalist what he takes to be the content of the fundamental theorem of arithmetic. If he is really a strict formalist, he must reply that, standing alone, it has no content at all. The theorem is, after all, just a string of symbols. What makes us feel that it has content is only that it plays a definite role in certain
activities we engage in. It is like a frequently encountered position in chess. If we give a more precise description of our symbolic activities, say by giving a particular formal system which codifies some part of mathematics, then we can also give a precise account of the role of the fundamental theorem of arithmetic. We might specify one or more formal proofs of the theorem in our system, and we might give some examples of uses of the theorem in formal proofs of other theorems. For the formalist, however, the theorem has no meaning apart from its role in our symbolic activities. For the strict formalist, the theorem does not make any assertion about natural numbers, since for him no such objects exist.

Now I agree that mathematics almost always involves the formal manipulation of symbols. I agree that a mathematician can usually be viewed as working inside some formal system. This seems to me an important insight. There is a branch of mathematical logic whose subject is just this aspect of mathematical activity. I mean the theory of recursive functions. That theory has contributed more than any other part of mathematical logic to our understanding of the inherent limitations of mathematics. Let me state this quite strongly. I do not believe that mathematicians will ever compute a nonrecursive function, solve a recursively unsolvable problem, or work in a theory which is not recursively axiomatizable. But all of that is not to concede that human minds are algorithmic devices in the sense of recursive function theory. Rather, it is analogous to the harmless concession we might make to surfacism that no one will ever see a material object without a surface.

It is easy to understand how a philosopher who never actually did any mathematics might hold a formalistic view of its foundations. After all, what is there for him to see but the outer play of symbols? On the other hand, I must admit that I find it difficult to understand when, as happens occasionally, a creative mathematician is a formalist. Introspection shows that when I am actually doing mathematics, when I am wrestling with a problem that I do not know how to solve, then I am hardly dealing with symbols at all, but rather with ideas and constructions. Some of the hardest work a mathematician does occurs when he has an idea but is, for the moment, unable to express that idea in a formal way. Often such ideas first manifest themselves as visual or kinesthetic images. As the mathematician becomes clearer about them, as they become more formal, he may discover that they manifest considerable internal structure which is, so to speak, not yet symbolically encoded. This point is hard to discuss in a way which avoids purely psychological categories not directly relevant to the epistemological point I am trying to make. Still, mathematicians customarily talk about ideas, constructions, and proofs in a way which makes it clear that they have in mind something other than the symbols they use. Thus mathematicians may discuss whether two distinct papers embody the same idea, whether two distinct strings of symbols express the same construction, or whether two distinct lectures expound the same proof. Every mathematician knows that the same construction can be used in quite different parts of mathematics and that, if you find a new proof of an old theorem, you had better check that it is not just an old proof in a new form.

As has been customary since Brouwer, let me use the word “construction” to refer generically to all of these entities which lie behind the symbols the mathematician writes and which give those symbols life and content. I think there can be no doubt that constructions are practically real in the sense I introduced above. Mathematicians discuss them constantly, agree on their general properties, and agree that they are what is important in mathematical creation. It follows that an adequate philosophy of mathematics cannot just treat constructions as subjective illusion. Most formalist philosophers, however, either do not mention them at all or else dismiss them under some such name as “heuristics” without giving any account that would explain the properties that mathematicians agree constructions have. Indeed, the formalist cannot give a theory of constructions, since he denies they exist. For example, even if there could be a program which could recursively recognize whether or not two strings of symbols embody the same idea, the formalist could not admit that that is what the program does. What could it even mean to say that a computing machine had an idea for a proof but was having trouble formalizing it?
In order to state this argument more carefully, let me introduce the word "intuitive." In the sense that is relevant here, "intuitive" is used to contrast with the word "formal." Thus an argument may be called intuitive if it is natural and easy to follow. This is roughly the sense in which the word "intuitive" seems to be used in intuitionism. Thus an intuitive proof, in that context, is one which is unformalized, independent of symbols, and perhaps not even entirely communicable. At any rate, there certainly are constructions which are intuitive, in the sense that they are not formal and not symbolic, but which do have internal structure, do enable us to see new facts, and can be formalized so as to give correct proofs.

Now my argument may be summarized as follows. Intuitive constructions are practically real. They are vital to the practice of mathematics. It is of the essence of formalism that it denies their objective reality. Therefore, by the principle of objectivity, formalism cannot be an adequate philosophy of mathematics.

4. Intuitionism. If formalism must be rejected because it neglects the intuitive content of mathematics, then it is natural to make a second attempt at a philosophy of mathematics as follows. Let us hold that mathematics consists of intuitive constructions, of the formal manipulation of symbols which is their external expression, and of nothing else. This seems to me to be the essence of the view usually called intuitionism. It was worked out by L. E. J. Brouwer and Arend Heyting. A good introduction is Heyting [11]. A more recent introduction is Dummett [6]. Perhaps the clearest general statement by Brouwer himself is his [3]. A related, but definitely distinct, point of view is that of Errett Bishop [2]. I should say that very few of my remarks about intuitionism apply directly to Bishop's philosophy of mathematics, since Bishop has little of Brouwer's subjectivistic tendency.

It is characteristic of intuitionism that it denies the existence of any mathematical reality external to the mathematician or even of any mathematical truth beyond what the mathematician has actually proved or could actually prove. Mathematical objects exist for me only as the results of my constructions, and mathematical facts are true for me only insofar as they are the conclusions of arguments I can make. Thus the sequence of natural numbers, being infinite and hence not surveyable, is only potentially real. Statements which have so far been neither proved nor refuted, like Fermat's conjecture, have no definite truth-value. The logical law of the excluded middle, which asserts that every statement is either true or false, is rejected as inapplicable to statements about infinite sets, and indirect proofs of such statements are rejected as invalid.

To take an example, let us again consider the fundamental theorem of arithmetic. The intuitionist, unlike the formalist, does not take this to be a mere string of symbols. The theorem has a meaning. Nevertheless, he also does not take the theorem to be a truth about an externally existing domain of natural numbers. Rather he thinks of it as expressing a certain ability that we have—namely, our ability to factor an arbitrary natural number into primes and to see, given two such decompositions, that they consist of the same primes with the same multiplicities. Like the formalist, the intuitionist takes the meaning of the theorem to reside in our practice, not in any external reality to which the statement might refer.

Let us examine Brouwer's rejection of the law of the excluded middle somewhat more closely. Brouwer does not have available any concept of truth which could be used to justify, or even to explain, a truth-functional interpretation of the logical connectives. Moreover, for Brouwer it only makes sense to assert a mathematical statement as the conclusion of an intuitive proof. But a proof that either A is true or B is true ought to contain an indication as to which of the two alternatives is being proved. Otherwise we could assert the existence of a number n such that if n = 0 then A, and if n = 1 then B; but we would not know the value of any such number. Surely, however, we know the value of a number we have actually constructed. Thus we would be asserting the existence of a number without having constructed it. Hence a proof that the Fermat conjecture is either true or false would have to contain either a proof or a refutation of
the conjecture. Since I can supply neither, it follows from Brouwer's point of view that I am not in a position to assert that the conjecture is either true or false. Thus the law of the excluded middle is "refuted" not by finding a third possibility but by making an additional demand. An assertion is only to be considered justified if an intuitive construction can be supplied which justifies it.

As an intellectual movement, mathematical intuitionism is similar to other positions, like existentialism, which emphasize our isolation from each other and which conclude from that isolation that we are epistemically reduced to our own individual resources. That is to say, it is characteristic of all of these views that they hold that our inner experience, as such, is the only source of knowledge available to us and that they deny that our inner experience essentially entails an external reality to which it refers. In consequence, these views tend to collapse into irrationalism and solipsism. When Brouwer emphasizes the absolute freedom of the creative subject in mathematics, he is taking a stance related to that of the existentialist emphasizing the absolute freedom of that same creative subject in aesthetics, in ethics, or in politics.

Looked at in our context, however, intuitionism is a fairly typical form of surfacism. Its characteristic rhetorical gesture is to ask what a mathematician could possibly have access to other than his own constructions. Put differently, try to think something other than one of your thoughts, or try to visualize something other than one of your images.

As in the case of formalism, it seems to me important not to overlook the contributions that intuitionism has made to our understanding of the practice of mathematics. The writings of the intuitionists are a rich source of ideas about the internal process of mathematical creation. Here again there is a branch of mathematical logic devoted to trying to extract and develop the precise content of these insights. The various realizability notions, functional interpretations, Kripke structures, and the like, seem to me to give promise of a mathematical theory, perhaps yet to come, of the experience of doing mathematics.

I myself have been attracted by intuitionism. But I have gradually come to see that, in the long term, strong intuitionistic convictions undermine one's actually doing mathematics. By embracing intuitionism the mathematician is giving up the most powerful motivation for his work—the search for publicly validated truth. Mathematics, after all, is a part of science. The main purpose of doing mathematics is to discover new truths. If that conception is given up, as it is in intuitionism, then mathematics is reduced to an esoteric art form—to a kind of play. There is a sense in which intuitionism is inadequate in its own terms, for it overlooks what is introspectively obvious: that I am interested in my constructions not for their own sake but for the new truths they enable me to find. The constructions derive their significance from their epistemic role. Who would be interested in a proof that established nothing? Just as the constructions lie behind the symbols and give them their interest and meaning, so there is something behind the constructions—mathematical truth.

In this respect mathematical creation is not at all free. A mathematical argument often gives a feeling of inevitability. The concept of rigor, which plays such a great role in the mathematician's talking and thinking about his work, is a restriction on his freedom which he accepts in order that his theorems may be true and in order that his arguments may genuinely establish their truth.

Mathematical truth, unlike a mathematical construction, is not something I can hope to find by introspection. It does not exist in my mind. A mathematical theory, like any other scientific theory, is a social product. It is created and developed by the dialectical interplay of many minds, not just one mind. When we study the history of mathematics, we do not find a mere accumulation of new definitions, new techniques, and new theorems. Instead, we find a repeated refinement and sharpening of old concepts and old formulations, a gradually rising standard of rigor, and an impressive secular increase in generality and depth. Each generation of mathematicians rethinks the mathematics of the previous generation, discarding what was faddish or superficial or false and recasting what is still fertile into new and sharper forms. What guides
this entire process is a common conception of truth and a common faith that, just as we clarified and corrected the work of our teachers, so our students will clarify and correct our work.

In order to formulate a more careful argument, I need to say a few words about the concept of rigor. It is widely believed that this notion changes. Arguments that seemed rigorous to Euler seemed inadequate to Cauchy. Arguments that seemed rigorous to Cauchy seem to us to contain obvious gaps. But it is not really the case that the concept of rigor has changed—only the standard of rigor. That is to say, a rigorous argument is always an argument which suffices to establish the truth of its conclusion. As our insight grows, we see that more is required to establish truth, and therefore arguments that once seemed rigorous are now seen to have gaps. But the concept of rigor itself has not changed since at least the time of Euclid.

More is true than that the concept of rigor presupposes the concept of truth. Actually, when we evaluate a mathematical argument, we do not check to see whether it accords with some set of rules taken, let us say, from a logic text. Rather, we try to determine whether the argument works—that is, whether it convinces us, and ought to convince us, of the truth of its conclusion. Thus the concept of mathematical truth is directly involved in the practice of mathematical rigor. It functions as an indispensable ingredient in the very criterion of rigor.

Now I may formulate my argument against intuitionism as follows. Mathematical truth is practically real. Indeed, without the practical reality of mathematical truth, there would be no such thing as mathematical rigor. But it is of the essence of intuitionism that it denies the objective reality of mathematical truth. Therefore, by the principle of objectivity, intuitionism cannot be an adequate philosophy of mathematics.

5. Logicism. If we reject intuitionism because it neglects mathematical truth, then we may be led to make a third attempt at a philosophy of mathematics as follows. Let us hold that mathematics consists of certain truths, of the arguments that establish these truths, of the constructions underlying those arguments, of the formal manipulation of symbols that expresses those arguments and truths, and of nothing else. It seems to me that this is the central thrust of what has traditionally been called logicism. Views of this sort have been advocated most prominently by Gottlob Frege and by Bertrand Russell. Classical statements of logicism may be found, for example, in Frege [7] or Russell [18]. A somewhat more recent statement is in Hempel [9].

A logicist, unlike a formalist or an intuitionist, would take the fundamental theorem of arithmetic as a truth whose content is quite independent of our activity. For the logicist, however, there are no natural numbers which exist as independent entities and which happen to have the property expressed by the theorem. Instead, the theorem is to be understood on the basis of a long sequence of definitions. When all the expressions used in the theorem are expanded out in accordance with these definitions, then, according to the logicist, the theorem will turn out to be merely a very complex logical truth. The fundamental theorem of arithmetic, for a logicist, is on a par with an assertion like, "if all $A$'s are both $B$'s and $C$'s, then all $A$'s are $C$'s."

What the logicist denies is that there is any subject matter for mathematical truths to be about. Mathematical terms, for the logicist, do not refer—or at least do not refer uniquely. It follows that mathematical truths are not true by virtue of successfully describing any actual state of affairs. They are empty of factual content. Hence mathematical truths must be true solely by virtue of their own internal structure and of their relations to one another. That is the way in which logical truths are true: Hence the logicist thesis that mathematics is merely logic. In practice, of course, logicists have tended to use the term "logic" rather loosely, sometimes including all of set theory under that name. But the basic idea is always to deny that mathematical assertions have factual content—that is, to deny that their truth rests on anything outside of the structure of the mathematical statements themselves. That is presumably also what Kline means to deny by the words quoted at the beginning of this essay. (For an explicit statement of Kline's views, see [13, pp. 424–431]. For more details, see Kline [14, pp. 1028–1039].)
Logicism motivated much of the early work in mathematical logic. I think that logicism has made greater contributions than any other philosophy of mathematics to our understanding, not so much of the practice of mathematics, but of its foundations. The desire to reduce all of mathematics to “logic”—that is, to merely conceptual reasoning—has provided a strong impetus to simplify and unify the basic mathematical notions and to find and make explicit the fundamental principles upon which mathematics is based. Moreover, logicism is still making such contributions today. Much of what is now called proof theory can be seen as an effort to view larger and larger parts of mathematics as consisting of logical truths by extending the concept of logic in various directions. To mention only one example, the past twenty-five years have seen the development of a theory of infinitely long formulas and proofs so as to give a “logical” analysis of arithmetic and of increasingly extensive fragments of mathematical analysis.

Unlike formalism or intuitionism, logicism does provide an adequate account of a significant part of actual mathematical practice. Much of mathematics really is just logic. We reason from clearly formulated premises, trying to find an argument that will settle some previously formulated question. I doubt, however, that any work a mathematician would consider deep can be accounted for in terms the logicist would accept. Every mathematician knows that his best work is based not on mere reasoning but on the characteristic kind of insight he calls “intuition.” In this sense, the word “intuition” refers to a faculty by which the mathematician is able to perceive properties of a structure which, at the time, he is not in a position to deduce. This perception can be trained, and it is often quite reliable. Sometimes, when trying to work deductively, one feels like a man trying to find his way around an unfamiliar room in the dark. The mind is full of details that fail to cohere into a pattern. But then, either gradually or suddenly, one’s eyes adjust to the dark, one sees dimly how the room is arranged, one knows about chairs one has not yet bumped into, and one is able to get about comfortably. It is an everyday occurrence that a mathematician “knows intuitively” that thus and so must be the case but does not have the vaguest idea how to go about proving it. Often, of course, he is wrong. But far more often than not he is right. Certainly, if I respect a particular mathematician and if he has had extensive experience with a particular structure, I will be willing to rely on his intuitions about the structure even in the absence of a proof—not absolutely, but to a very large extent.

Let me say at once that I am not urging the existence of an occult faculty whereby we have direct knowledge of platonic objects. Rather, I think that the mathematician’s intuition is a special case of the general human ability to recognize patterns or, more specifically, to synthesize complex structures from scattered cues. Thus I think the mathematician’s intuition about a particular structure is simply the result of long experience with that structure. It is not different in kind from a carpenter’s “feel” for his wood. The fact is that mathematicians are able to arrive at more or less reliable conclusions about mathematical objects without having to deduce those conclusions. Indeed, mathematical creativity is much more a matter of intuition than it is of logic. (For essentially the same view, see Wilder [19] or Resnik [16].) It follows that a logicist account of mathematics cannot be adequate.

But what is missing? The logicist holds that mathematics is a body of truths that are not about anything. They are true just by virtue of their internal logical structure, not by virtue of any external objects to which they refer. But if that were true, then the phenomenon of mathematical intuition would be incomprehensible. For if the logicist is right, then there are no structures for the mathematician to become familiar with or to have insight into.

An interesting special case of this difficulty is the problem, from a logicist point of view, of the status of axioms. A principle which is neither a logical truth nor deduced from antecedently accepted principles is not being accepted merely by virtue of reasoning. Logicists, therefore, often deny that such principles are being accepted at all. Thus they tend to think of geometry, for example, as a hypothetical discipline. If physical space satisfies the axioms, then it satisfies the theorems. (For this opinion see the references to Kline above, or see Hempel [10].) But, as a matter of fact, we have a clear intuition of Euclidean space, and the theorems of Euclidean
geometry are outright true about that structure. It is generally held that the earliest geometrical knowledge was arrived at empirically. If so, then that knowledge does not have a hypothetical character. The non-Euclidean geometries only show the logical consistency of denying the parallel postulate. They do not show that the parallel postulate is false. The general theory of relativity shows that certain esoteric observations are well described by treating space-time as a four-dimensional manifold of non-constant curvature. It may follow from this, though I am not sure that it does, that the space of our intuition does not correspond perfectly to physical space. It certainly does not follow that we do not have a clear spatial intuition. Moreover, Euclidean geometry remains an excellent description of the space we actually live in and actually experience. It is not as though the use of figures in geometrical demonstrations were derivative from purely logical proofs based on the axioms. On the contrary, some of the axioms, such as the axioms of order, are so evident to the intuition that the need for them was not noticed until the nineteenth century. It seems implausible that all the geometers before Moritz Pasch were guilty of the same systematic logical errors. It seems much more likely that they were engaged in some activity other than deducing the logical consequences of a set of axioms. I think they were studying space.

Let me summarize the argument. Mathematical intuition is practically real. It is only comprehensible as a non-deductive insight into structures external to the mathematics itself. Hence such external mathematical structures are practically real. But it is essential to logicism that it denies the objective reality of any such structure. Therefore, by the principle of objectivity, logicism cannot be an adequate philosophy of mathematics.

6. Platonism. Logicism, in other words, must be rejected as an incomplete philosophy of mathematics because it omits the objects that mathematics is about. Thus we may make a fourth attempt at a philosophy of mathematics as follows: Mathematics consists of truths about abstract structures existing independently of us, of the logical arguments that establish those truths, of the constructions underlying those arguments, of the formal manipulation of symbols that expresses those arguments and truths, and of nothing else. This is the philosophy of mathematics that I think ought properly to be called platonism. Its most distinguished contemporary proponent was Kurt Gödel. (For example in his [8].)

A platonist would interpret the fundamental theorem of arithmetic literally. For the platonist there are such things as natural numbers existing independently of us, and it is a matter of fact true that they are all uniquely decomposable into prime factors.

The most characteristic expression of platonism within mathematical logic is model theory. This discipline is the study of the semantic content of mathematical theories. Of course formalism, intuitionism, and logicism all deny that mathematical theories have semantic content. The central problem of model theory is the question of what properties of structures can be expressed in particular languages. This question only arises if structures are assumed to exist and to have properties independently of their description.

Let me try to summarize quickly the picture of mathematical activity that platonism offers. The mathematician, on this view, is confronted by a wide variety of abstract structures which themselves precede his mathematical activity. He does not create these structures; he finds them. In the course of his training, and then as he develops his powers, he forms and refines an intuition about these structures. Typically, of course, he will have much more insight into some of them than into others. His intuition is formed by the truths about the mathematical world that have been discovered by his predecessors and by his colleagues, and then his intuition, in turn, enables him to find new structures and to make new conjectures about the old structures. In order to verify these conjectures, to answer the questions that occur to him, he performs constructions, makes arguments, defines new concepts. These constructions, in turn, get expressed in mathematical English, are bolstered by computations, are made rigorous and formal. Thereby they are made publicly accessible and verifiable and become part of the larger social dialectic through which mathematics develops.
This seems to me a fairly satisfactory account of what the pure mathematician is doing. Indeed, I think that most contemporary mathematicians, even if they have not bothered to articulate it for themselves, would accept some variant of this view. So satisfactory is platonism that very few recent mathematicians or philosophers of mathematics have felt any need to go beyond it. Just in the past few years, however, there have been signs of discontent. To indicate their source, let me pause for some brief historical remarks.

In the eighteenth century, mathematics was considered a science distinguished from the other sciences only in being more certain and more fundamental. Its special province was the laws governing space and quantity. In the course of the nineteenth century, this conception of the nature of mathematics was strongly undermined. First the non-Euclidean geometries were used to deny the existence of a unique spatial structure for our intuitions to be about. Then analytic geometry was used to undercut the view that there was an intuition of space at all apart from our intuition of the numerical continuum. The end product of this development is the contemporary mathematician who tells his undergraduate students that by three-dimensional Euclidean space he means the set of all ordered triples of real numbers. Obviously, that is not what Euclid meant. Toward the end of the nineteenth century, even the intuitive conception of quantity or magnitude was replaced, at least officially, by the purely conceptual structures introduced by Weierstrass, Dedekind, and Cantor. Again, a contemporary mathematician is likely to tell his students that by a real number he means a Dedekind cut. Obviously, that is not what Euler meant.

One effect of these changes was to produce what might be called a foundational vacuum—a situation in which mathematicians were without any systematic account of the nature of the structures they were dealing with. Axiomatic set theory rushed in to fill this void. The set-theoretic view of foundations, however, is platonism in its most narrowly reductionistic form. All the objects of the set-theorist's world are abstract. Even if individuals are allowed, and they are usually excluded, these individuals are taken to have neither internal structure nor intensional relationships. They are mere abstract points. Thus the reduction of all of mathematics to set theory entails a narrowing of the subject matter of mathematics so as to exclude all of concrete reality.

For about two generations axiomatic set theory was a great success. I think there can be little doubt that set theory provides an elegant and convenient framework within which to do pure mathematics. It is wonderfully simple in conception, almost never gets in the way of mathematical practice, gives smoothly reassuring answers to questions like "But what are numbers, really?" and provides a wealth of interesting structures of which no one before Cantor could have dreamed.

In the past decade, however, set theory has been undermined roughly in the same way that geometry was undermined about a hundred years earlier. The independence results, the proliferation of large cardinal axioms, and the construction of increasingly bizarre models for set theory have made mathematicians realize how weak their set-theoretic intuition actually is. In the absence of new insight, the views of set-theorists begin to diverge. Some still follow Cantor in thinking the continuum hypothesis plausible, but others follow Gödel in believing more and more strongly that it must be false. It is becoming truistic that we need a new concept, one more fundamental than that of a set. Unfortunately, no one can imagine where to look for such a concept.

None of this is incompatible with a sufficiently liberal platonism. Increasingly one hears the suggestion that there is not just one set-theoretic universe, but many. You work in a world in which the continuum hypothesis holds, and I will work in one in which Martin's axiom holds but the continuum hypothesis fails. He will work in a universe containing a measurable cardinal, and she will work in one in which, since all sets are constructible, a measurable cardinal is impossible. These are all just different structures, all equally entitled to be considered interesting and worthy of study. Where is the problem?

The problem, of course, is the same as it was in 1890. How do these different structures
interact? What are they? What are the laws that govern the mathematical universe as a whole, if none of these set-theoretic "universes" can any longer be regarded as including all of the structures mathematicians concern themselves with? None of these questions have generally accepted answers. I think it is out of despair at this situation that some mathematicians retreat to formalism, intuitionism, or logicism—positions from which such questions cannot arise.

Let me put the problem differently. It seems to me that mathematics can only flourish if there is a common conception of what we are about, if there is an agreement that the different structures we study are aspects of one reality. Without a foundational consensus, it seems to me, mathematics will tend to break apart into schools.

Actually, not only is set theory tending to split into pieces, but mathematical platonism itself is the result of a split in the larger structure of science. The traditional view of the nature of science, for example in the time of Newton, was that there is only one reality and therefore only one science. On this view the several special sciences—mathematics, physics, chemistry, biology—share a common reality but ask different questions about it and use different methods to study it. Of course, each special science will reveal its own particular aspect of the world; it remains a fundamental assumption of science as traditionally conceived that these various aspects are complementary, mutually illuminating aspects of one world. As a matter of fact, most branches of mathematics cast light fairly directly on some part of nature. Geometry concerns space. Probability theory teaches us about random processes. Group theory illuminates symmetry. Logic describes rational inference. Many parts of analysis were created to study particular physical processes and are still indispensable for the study of those processes. The list could be extended almost indefinitely. From the point of view of the platonist, however, only pure mathematics is really mathematics. For, according to platonism, the objects which mathematics studies are necessarily abstract. How can the theory of finite groups tell us about the structure of crystals if the only groups we consider are built up out of sets of sets of sets?

When the foundations of mathematics became completely abstract and ceased to have anything to do with the world of the senses, the connection between mathematics and the other sciences became obscure. Recently, as economic circumstances have forced mathematicians to look around for new means of support, this divorce of mathematics from the other sciences has ceased to be a matter for pride and become a matter of concern. Set theory, however, provides no clue as to how a reconciliation with the rest of science is to be effected.

Thus I think that mathematical platonism is again a form of surfacism. It is a practical reality that our best theorems give information about the concrete world. It is a practical reality that there is no clear boundary between pure and applied mathematics. There is only one science. It follows from the principle of objectivity that an adequate philosophy of mathematics would identify the objective content of these facts. Such a philosophy of mathematics would be only one chapter in a larger philosophy of science. That philosophy would make it clear in what sense there is only one objective world and how it is that the objects studied by the mathematician, many of which are not realized in physical reality, can nevertheless be seen as part of that world. Unfortunately, that philosophy has yet to be formulated.

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References

WHAT IS MATHEMATICS?

ERNST SNAPPER

1. Introduction. Mathematics is sometimes defined in a strictly mathematical way by saying, "Mathematics consists of everything which can be formulated and proved by means of the language and axioms of ZF." Here, ZF stands for Zermelo and Fraenkel; any other axiom system of set theory would of course have done just as well.

How good is this definition? In the sense that all of classical mathematics, except perhaps for some material on the fringes of category theory, can be developed in terms of ZF, the definition is pretty good. But how useful is it? We would like, from any definition of mathematics, to obtain at least a hint of how one might answer the question, "Why is mathematics free of contradictions?" The above definition turns this question into: "Why is ZF free of contradictions?" Since no one knows how to prove the consistency of ZF, the above definition of mathematics is not very useful. But worse, this definition suffers from a serious defect. Namely, it flatly denies that intuitionism is part of mathematics. The author does not accept the intuitionistic thesis that intuitionism is all of mathematics and that anything that is not intuitionism is meaningless (see Section 6). On the other hand, he feels equally strongly that intuitionism is part of mathematics and that any definition that denies this is wrong.

From the above discussion emerge the following three necessary criteria a useful definition of mathematics must satisfy. (1) It must recognize all of classical mathematics as mathematics. (2) It must give some hint as to why mathematics is free of contradictions. (3) It must recognize intuitionism as a branch of mathematics. Is such a definition possible?

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