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THE CRISIS IN CONTEMPORARY MATHEMATICS

BY ERRETT BISHOP
UNIVERSITY OF CALIFORNIA AT SAN DIEGO

There is a crisis in contemporary mathematics, and anybody who has not noticed it is being willfully blind. *The crisis is due to our neglect of philosophical issues*. The courses in the foundations of mathematics as taught in our universities emphasize the mathematical analysis of formal systems, at the expense of philosophical substance. Thus it is that the mathematical profession tends to equate philosophy with the study of formal systems, which require knowledge of technical theorems for comprehension. They do not want to learn yet another branch of mathematics and therefore leave the philosophy to the experts. As a consequence, we prove these theorems and we do not know what they mean. The job of proving theorems is not impeded by inconvenient inquiries into their meaning or purpose. In order to resolve one aspect of this crisis, emphasis will have to be transferred from the mechanics of the assembly line which keeps grinding out the theorems, to an examination of what is being produced.

The product (i.e., the concepts, theorems and techniques) of this assembly line can be evaluated from at least three distinct standpoints: pure, applied (physical sciences), and applied (data processing). Today, I wish to concentrate mainly on pure mathematics, although the crisis certainly extends further.

As pure mathematicians, we must decide whether we are playing a game, or whether our theorems describe an external reality. Assuming that it is no game, we must be as clear as possible about what objects we are describing, and what it is that we are saying about those objects. The basic point here is already made in full force by considering the question "What do we mean by an integer?". It is clear that the integer 3 differs in quality from the integer

$$99^{99^{99}}$$

which in turn differs in quality from the integer that is defined to be 1 if the four color theorem is true and is 0 otherwise. So then, there are at least three possibilities: (1) an integer may mean one that we can actually compute, (2) one that we can compute in principle only, or (3) one that is not computable by known techniques, even in principle.

To my mind, it is a major defect of our profession that we refuse to distinguish, in a systematic way, between integers that are computable in principle and those that are not. We even refuse to do mathematics in such a way so as to permit one to make the distinction. Many mathematicians do not even find the distinction interesting. Of course, the distinction between computable and non-computable, or constructive and non-constructive is the source of the most famous disputes in the philosophy of mathematics, and will continue to be the central issue for many years to come.

Now, the little that I have read in the history of the philosophy of mathematics has left me with an overwhelming impression: that the history of the philosophy of mathematics is very dangerous. I am surprised that this point has only been made in passing at this meeting. I think that is should be a fundamental concern to the historians that what they are doing is potentially dangerous. The superficial danger is that it will be and in fact has been systematically distorted in order to support the status quo. And there is a deeper danger: it is so easy to accept the problems that have historically been regarded as significant as actually *being* significant.

For example, there is a problem of the *truth* of the statement that every bounded monotone increasing sequence of real numbers converges. People sometimes ask me whether I believe that this or some similar statement is untrue. My answer is that it is not possible to answer the question until they tell me what interpretation they wish to attach to the statement. The statement is true when interpreted *classically* and false when interpreted *constructively*. Thus what are historically regarded as problems about truth are actually problems about meaning. I believe that if we agree on the meaning of such statements, then we can settle the question of their truth relatively easily.

There is only one basic criterion to justify the philosophy of mathematics, and that is, does it contribute to making mathematics more meaningful. It is not true that this criterion is commonly accepted. In fact, the philosophical criterion that most mathematicians prefer is that it enables them to prove more theorems and to be more secure about the theorems that they have already proved.

A very brief review of the central historical controversy about the nature of *mathematics* will be sufficient for me to discuss with you what I believe to be the important philosophical issues in the philosophy of mathematics today.

The controversy to which I refer is the grand dispute between Kronecker, Brouwer, H. Weyl and perhaps a few others on the one hand, who gave us techniques for deepening the meaning of mathematics, and Hilbert and others, who to a great extent rejected their discoveries. Hilbert feared that his cherished theorems, his paradise, would be taken away. In fact, the threat was never real. The only real threat was to label Hilbert's theorems for what they were, and to try to make some of them more meaningful; but perhaps Hilbert did not realize this. This controversy raged during the late nineteenth and early twentieth centuries, finally having been resolved, in the opinion of many, in favor of Bourbaki.

I shall contend that had the disputants been less dogmatic and more thoughtful, then we might all be more thoughtful mathematicians today, and in fact perhaps be doing mathematics which is in some respects quite different from the mathematics that we are doing.

Perhaps, the most logical place to begin looking at this dispute is with Cantor, because the paradoxes that arose in Cantorian set theory are what I think really provoked the crisis and the re-examination of the foundations of mathematics that took place. They gave the problem an urgency.

People reacted to these contradictions in basically two ways: (1) Cantor himself, Hilbert, Russell and a host of others, seemed to believe that the Cantorian ideas were essentially correct, and that the task of philosophy was to secure them for posterity by analyzing the source of the contradictions and in some way insuring that the same thing would not happen again; (2) Brouwer, Weyl, Borel and possibly Poincaré, took the contradictions as indicating that something was fundamentally wrong with Cantor's ideas and possibly even with pre-Cantorian mathematics. Of course, Kronecker is a special case, because even before this particular set of contradictions, Kronecker had said that something was wrong with the classical theory of the real numbers, as it had been developed during his lifetime.

It seems to me that the disputants in this controversy missed the point. The point is not whether a particular statement is true, but what do we *mean* by the statement. They should have been asking the question "What is a set?" and "What do we mean by the set of all sets?" instead of asking whether or not the set of all sets really existed.

So, the wrong question was asked. It is fascinating to speculate what would have happened if they had asked the right question, that is, what do these things mean, not whether these things are true or false. I am going to reconstruct history, and tell you what might have happened and what I wish had happened, if the disputants had been more concerned with communicating to one another rather than justifying themselves and putting each other down. It is important to remember that Brouwer and Hilbert understood the propositions of Cantorian set theory in different ways. They attached different meanings to Cantorian set theory, so it was necessary that one of them should reject it and the other accept it.

A similar situation undoubtedly held in the dispute between Kronecker and Weierstrass, about the validity of the real number system as it existed in those days. So there was a violation of the general philosophical principle not to discuss questions of truth until one settles questions of meaning. I think that Brouwer made a valiant attempt to say explicitly what meaning he attached to every mathematical theorem. For example, an integer to Brouwer (in my interpretation of Brouwer's philosophy) is either an integer in decimal notation or a method that in principle will lead after a *finite* number of steps to an integer in decimal notation. Again there is this notion of computability: if the integer is not given directly to be sure that it is finitely computable. This is as far as Brouwer could possibly have gone in expresssing himself on this subject. What was an integer to Hilbert? As far as I know he never discussed the point.

Perhaps Brouwer should not have denounced the mathematics that Hilbert wished to do as meaningless, even though Hilbert did not go to the pains that Brouwer did in saying what he meant by his mathematics. Perhaps he should have said to Hilbert, "I have told you what I mean by these things to the best of my ability; now you tell me what these things mean to you!" This would have been the first step in my reconstruction: for Brouwer to have taken this approach in his dealings with Hilbert on the philosophical question.

Then it is fascinating to try to anticipate what Hilbert's response would have been if Brouwer had approached him in this way. I can think of three possibilities:

(1) He could have said, "I cannot discuss that. The most I can do is to tell you the rules for doing mathematics and the meaning is then to be found in the rules plus whatever additional personal meaning you wish to read into it." If this had been the answer, then the designation of formalist that has been attached to Hilbert is indeed justified.

(2) Possibly Hilbert would have responded by a description of the inductive construction of the Cantorian universe, in as much detail and with as much care as it was possible for him to give. I doubt if he would have done so.

(3) Possibly Hilbert would have responded to Brouwer as follows: "I understand your explanation and the meaning that you attach to the objects and statements of mathematics, with the exception perhaps of your theory of choice sequences, which however can be omitted without significantly affecting the mathematics that you would be doing. In my opinion, you have a valid and consistent point of view, but there are other points of view, and I do not think that you should reject them as being meaningless. In your system of mathematics,everything ultimately reduces to finite computation within the set of integers. Let us extend your mathematics by allowing infinite computations. For most purposes, one particular kind of infinite computation will suffice: the examination of a sequence of integers to determine whether or not they all vanish. With this extended mathematics, I shall rest content." It would not have been possible for Hilbert to have preserved the Cantorian paradise within this extended mathematics, which allows in addition this one infinite computation consisting of the examination of a sequence of integers. But I suspect that what Hilbert really wanted to preserve was his own mathematics and other mathematics of the same sort. This certainly would have been possible under the system that I proposed that

Hilbert propose. If he really wished to preserve the full Cantorian paradise, he would have been compelled to introduce other infinite computations.

Let us go back to Brouwer. Assuming that Hilbert had responded as I told you that he should have done, Brouwer might then have replied as follows: "I appreciate your motivation, but I will not permit you to introduce any new computations into mathematics. The object constructed by an infinite computation is inherently different from an object constructed by a finite computation, as I have already told you many times. Your proposal would make it impossible to distinguish between them in a systematic way. However, there is a course which I believe will satisfy both of us. Let LPO (limited principle of omniscience) denote the statement that *it is possible to make an infinite computation of the type we described, that is, searching a sequence of integers to see whether they all vanish.* Then if we need LPO to prove a theorem, simply develop your mathematics in my system as the implication LPO implies whatever the theorem happens to be. You will be able to do your mathematics in my system without any loss of meaning and without any essential change in the method you have already been using. For me to do my mathematics in your system would entail a significant loss of meaning. Since we could both work in my system and pursue what we want to do and I cannot work in your system, please defer to me and accept my system." Hilbert would then have accepted Brouwer's proposal and mathematics would not be where it is today.

Where would mathematics be today if all this had come about? We would accept the meanings of "or", "there exists" and all the other connectives and quantifiers, as defined by Brouwer, not as defined classically. In particular, negation, disjunction, and existence would have their meanings changed. We would improve on Brouwer's definition of set in a way that I do not want to go into here. Classical mathematics would go on entirely as before except that every theorem would be written as an implication, either LPO $\rightarrow$ A or some extended version of an infinite computation implying A. So Hilbert's Cantorian paradise would remain intact within Brouwer's system. Those mathematicians who still believe in the Cantorian paradise as representing ultimate truth, would not be forced to taste forbidden fruit. On the other hand, when they saw that other mathematicians were tasting the fruit and thriving on the diet, they might decide that there was no reason to hold out. Of course, new vistas would be opened up, and it might transpire that Hilbert's paradise was not so perfect after all.

This is all very abstract. I want to take a concrete instance and illustrate what we might be doing. Unfortunately, I am about the only one who is doing it now and so I must apologize for choosing a concrete instance from my own mathematics. This theorem is not crucial, but it is an efficient demonstration of the sort of thing I am talking about, namely, the classical theorem that a function of bounded variation defined on the unit interval has a derivative almost everywhere. Now you might say that it does have a derivative almost everywhere and I would not disagree with you. But, if I wanted to talk to you in your own language, I would say compute the derivative. I suspect that many of you would answer, "I do not know how to compute the derivative". Some of you might say "I do not care" and others might really care and simply not know. Or, you might reply: "If you care whether you can compute the derivative, go ahead and consider the question, but do not try to change the whole system of mathematics just because you want to consider questions of whether you can compute things".

My point is that you *cannot* consider questions of whether you can compute things systematically and do a good job of it, unless you *do* change the whole system of mathematics. Now this does not seem to be true in things like number theory. One can do *ad hoc* constructivism in number theory and I do not think that it has posed any problems to do it that way. One simply cannot do *ad hoc* constructivism in analysis and develop good general theorems which correspond to the theorems of classical mathematics. In fact, there have been fewer analysts interested in constructive questions than there have been number theorists, and I suggest that it is because the classical system has tied their hands.

So let us see what we are going to do with this theorem, call it "A", that a function of bounded variation has a derivative almost everywhere. The classical "proof" actually proves A', which is the theorem LPO $\rightarrow$ A.

The harder problem is to prove A without using LPO. You simply cannot. Brouwer could have easily shown that there is no hope of actually computing the derivative of a function of bounded variation, essentially because the derivative does not necessarily get approximated when the function gets approximated.

Since there is no hope of proving Theorem A, you might think that the constructivist mathematicians should then rest content. He knows that he cannot get Theorem A in his system, and the classical mathematicians have already given him LPO $\rightarrow$ A. However, constructivism is not that trivial. This is what I mean by saying that accepting Brouwer's system would deepen the meaning of mathematics. Even though we cannot prove A, we still think that the implication LPO $\rightarrow$ A is ugly. So what can we do if we do not like the implication and we cannot prove the theorem? We can get an implication which is natural and reflects the nature of the problem. LPO is a general hypothesis *not* related at all to the structure of this particular theorem in any special way. Let us replace the left hand side of this implication by some statement which is naturally and, after we give it to you, obviously involved with the conclusion on the right hand side. Let f be the function of bounded variation whose derivative we wish to compute. Let

B(f) be the statement that we can *compute the total variation of f*. (I put it in this way for the benefit of the non-constructivists in the audience.) The theorem that I want to state is that *if we can compute the total variation then we can compute the derivative:* $B(f) \to A(f)$. This is a much stronger, much more natural and much more useful result than LPO → A. It is about the best that we can hope to do if you think about it; you cannot hope to get anything better than that.

[At this point Garrett Birkhoff gave another variant, the Jordan decomposition of a function of bounded variation. You can break such a function down into decreasing and increasing functions, so that your theorem would say that you can constructively prove that an increasing function is everywhere differentiable, because you then know the variation. Bishop agreed, pointing out that just because you can compute the derivative almost everywhere, does not guarantee that you can decompose it.]

In addition, we have a very nice corollary, generalizing it in an essential way, not trivially. We get the fact that an indefinite integral of an integrable function has a derivative equal to that function almost everywhere. This is because you can compute the total variation of the indefinite integral which is, of course, equal to the integral of the absolute value of the function.

This is the kind of mathematics that we might be doing. We might be taking many classical theorems and doing exactly this sort of thing to them if history had taken the course that I have discussed.

Actually, the development of this particular example should not stop here, because whenever you have a theorem: B → A, then you suspect that you have a theorem: B is approximately true → A is approximately true. So there should be an even further development of this theory, namely to say what we mean for B to be approximately true, and then we have conjectured an implication, which we should try to prove. I have not done this, but it occured to me while preparing this talk that the conjecture is clear enough. I shall not take the time to present it here.

In a way, the imaginary dialogue that I presented here might be regarded as a historical investigation if you believe as I do that it shows how two titanic figures such as these might have reached an accommodation that would have changed the course of mathematics in a profound way, had they spoken to each other with less emotion and more concern for understanding each other.

Instead, Hilbert tried to show that it was all right to neglect computational meaning, because it could ultimately be recovered by an elaborate formal analysis of the techniques of proof. This artificial program failed.

A more recent attempt at mathematics by formal finesse is non-standard analysis. I gather that it has met with some degree of success, whether at the expense of giving significantly less meaningful proofs I do not know. My interest in non-standard analysis is that attempts are being made to introduce it into calculus courses. It is difficult to believe that debasement of meaning could be carried so far.

Many mathematicians regard the theory of computation as a branch of recursive function theory. It is true that many constructivists, for instance the school of Markov in Russia, are recursivists. Brouwer, of course was not. The recursive constructivists seem to be motivated by the desire to avoid such vague terms as "rule" and "set". Their mathematics is forbiddingly involved and laborious, a great price to pay for the precision they hope to attain. My personal opinion is that they have not attained any additional precision. Perhaps any attempt to make the notion of "rule" more precise is futile. It is clear that the concept of a set, in its full generality, can be avoided to a very great extent, again however, at the price of awkward complications. More research is needed on this point.

In my opinion, the positive contributions of recursive function theory to both constructive mathematics and the more concrete aspects of the theory of computation are the construction of counterexamples, but here again inpressions are somewhat misleading. The methods of Brouwer, now largely neglected, are more suitable for providing counterexamples in most cases of interest than are the methods of recursive function theory.

That is all I want to say about pure mathematics. I would like to consider next another very interesting question that has occupied many people: what does the constructivist point of view entail for the applications of mathematics to physics? My own feeling is that the only reason mathematics is applicable is because of its inherent constructive content. By making that constructive content explicit, you can only make mathematics more applicable, Hermann Weyl seems to have had an opposite opinion. For him, the utility of mathematics extended even to that part of mathematics that was not inherently computational. I hesitate to disagree with Weyl, but I do. It is a very serious subject for investigation; it would be interesting and worthwhile to settle this point.

I have one final concern to express today. Perhaps the most critical problem in applied mathematics is what to do about the over-mathematization of our society. The scientists who developed the atom bomb would like to feel that they were not responsible for its use. Those of us who teach calculus etc. would like to feel that we are not responsible for the inappropriate uses to which our instruction is put and I am not talking about the construction of bombs. In these days, mathematics is being applied to psychology, to economics, etc. in a very thoughtless way. We need a philosophy, if that is the right word, of when mathematics is applicable and when it is not. In the meantime, I tell my students that I doubt the validity of many of the applications of mathematics to the non-physical sciences that are presented in the text books, and that more important than being able to do mathematics is to be sure the applications are meaningful.

I want to discuss today one fundamental reason why mathematics is so often applied so thoughtlessly: the arrogance of mathematicians. I have experienced this arrogance ever since I began work in the philosophy of mathematics and I am sure that you historians have experienced it too. People tell me in so many words that when I was proving theorems, I was doing something original and worthwhile; but when I started to think about philosophical questions, I could not possibly be doing anything deep. This prejudice, that all good work must be technical in the mathematical sense, has made economists, sociologists, etc, feel inferior, as if they should mathematicize, very often to the detriment of the real *meaning* of their work.

DISCUSSION

Aspray and Moore asked Bishop to comment of the work of Fitting, Troelstra and Kreisel, who have also worked on Brouwer's ideas. The following discussion ensued.

Bishop: Intuitionism was transmuted by Heyting from something which was anti-formal to something which is formal. When one speaks today of intuitionism, one is talking of all sorts of formal systems (studied by the logicians). That's not what Brouwer had in mind.

Moore: So you see yourself more the follower of Brouwer than Heyting or Kreisel are? (Bishop concurred.)

Kline: You did not indicate where one should stand on LPO (the limited principle of omniscience) described in your paper. Should it be accepted? Should one opt for a more limited assumption? Or should one not accept it and follow the intuitionists? Or is this a personal question? After all, one can prove more with LPO than without it.

Bishop: It is personal, because it is not going to affect our mathematics. Write the theorems that need LPO in their proofs as implications, and be careful not to use LPO for results that can be achieved without the use of it.

Mackey: Would you please justify your use of the word "crisis"? What terrible things are going to happen if we ignore what your're telling us? To put the question differently, let us compare this with the relationship of mathematics to physics. Consider the foundational question in physics: what is the real mathematics that the physicists are doing? The physicists don't care; they go ahead and say that they can get the kind of results they want -- and do. But we don't tell the physicists that they are having a big crisis. How do you compare these situations?

Bishop: Meaning in physics is different from meaning in mathematics. I am not a physicist; but physicists have told me that the sort of meaning that is appropriate to physics is *not* to ask whether the mathematics in question is rigorous. Rather, it involves the relations of the results to the real world.

Mackey: Brouwer had a point, but my reaction is that I don't want to think about these questions. I have faith that what I am doing will have some kind of meaning -- no matter what the status of these questions is.

Bishop: You can keep your attitude; but why can't you give me the kind of cooperation that Brouwer was willing to give Hilbert in my imaginary dialogue? Such cooperation will not harm your attitude. Mathematicians have cut themselves off from a large portion of mathematics which many, including myself, have thought to be meaningful because of their refusal to adopt a system that would cost them nothing.

Birkhoff: I think I have an answer to both Kline's and Mackey's queries. If mathematicians would admit that they don't know the answer to these fundamental questions, e.g. whether LPO or the Axion of Choice are true under all circumstances, and would keep an open mind about them, the situation would be better. I think this is what Bishop is urging. We should keep track of our assumptions, and keep an open mind.

Freudenthal: Bishop's thesis, that there is a crisis in mathematics, is not new. There has always been a crisis in mathematics. The present is not any different from other times in mathematics. For example, before Cauchy and Gauss complex numbers were considered a crisis in mathematics.

Dieudonné: There is no crisis in mathematics. Mathematics has never been as prosperous as it has been in the last ten years. Never before had we proved so many new and powerful theorems. I just want to work in the way Gauss, Riemann, and Poincaré worked; I want nothing else.

Abhyankar: My paper is in complete sympathy with Bishop's position.

Kahane: I agree partly with Bishop, partly with Dieudonné. I have to respect Bishop's work; but I find it boring. Perhaps it is boring to me because the constructivists do not have a unified consistent language.

Bishop: Most mathematicians feel that mathematics has meaning, but it bores them to try to find out what it is. You are typical of most mathematicians.

Kahane: I feel that Bishop's appreciation has more significance than my lack of appreciation.

Dreben: It has often been said that the main reason for the development of mathematical logic has been the paradoxes of Cantorian set theory. That is historically false. Frege, the greatest logician since Aristotle and the creator of the foundations of mathematical logic, that is, quantification theory and a totally formalized language for mathematics, had nothing to do with Cantorian paradoxes. The main reason for the development of pure mathematical logic, first by Frege and then by Russell (and Whitehead), was philosophical. Both Frege and Russell were motivated in their early work primarily by a desire to refute Kant. What is historically and philosophically interesting is that each of them took essentially the same technical path in order to refute Kant's thesis about the nature of pure arithmetic and its relation to logic; yet they came up with different conclusions. This might be taken as evidence for Wittgenstein's position that no technical result will ever really resolve any technical philosophical problem. Kant held that logic is analytic but arithmetic is synthetic *a priori*. Frege thought that in his *Grundlagen der Arithmetik* [C8], and later in his *Grundgesätze der Arithmetik* [C9], he had shown (by "reducing" it to logic) that pure arithmetic is analytic *a priori*, contrariwise. Russell in his classical period up to 1912 believed that the "logical reduction" had shown Kant to be right about arithmetic but wrong about logic; that is, since arithmetic was "derivable" from logic, logic had to be synthetic *a priori*. Of course, both Frege and Russell held that Kant had too narrow a conception of logic and was wrong in thinking that arithmetic and hence mathematics rested on extralogical modes of reasoning. The *epistemic* nature of logic and pure mathematics were what seemed important to them.