WHITHER MATHEMATICS?

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Abstract

We describe three successive crises faced by mathematicians during the twentieth century, and their implications for the nature of mathematics.

1 Introduction

During most of the twentieth century there was remarkable agreement about the right way to present results in (pure) mathematics. The subject consisted of a list of theorems, each of which was proved from an underlying set of axioms using what were called rigorous arguments. In a few cases, such as Peano arithmetic, the truth of the axioms seemed self-evident, but in many cases they simply defined the domain of discourse. For mathematicians, talking as mathematicians rather than as amateur philosophers, philosophical distinctions between the invention and discovery of new concepts did not affect the way they practised their subject.

In this paper we will argue that developments of the classical Greek view of mathematics do not adequately represent current trends in the subject. It proved remarkably successful for many centuries, but three crises in the twentieth century force us to reconsider the status of an increasing amount of current mathematical research.

The apparent consensus among mathematicians *as mathematicians* stands in stark contrast to the disagreements between those studying the philosophy of mathematics. This subject has been dominated by a single issue. This concerns the peculiar status of mathematical objects: if one maintains that they exist in some Platonic realm, it seems impossible to give any account of how we, as creatures embedded in space and time, can come to know about them. The argument that we may have no access to these objects but can nevertheless work out what they are like by the use of our reasoning powers is unconvincing for the following reason: we could apparently follow exactly the same lines of reasoning about the properties of and relationships between mathematical entities even if the Platonic realm did not exist. Whole books have been devoted to the discussion of the relationship between ontology and epistemology in mathematics, but it is fair to say that agreement about its solution is not imminent, [5, 6, 25, 26].

Mathematicians as amateur philosophers are no more agreed about the status of their subject than are philosophers. As representatives of many others we cite Roger Penrose as a committed realist (i.e. Platonist), [20, 21], and Paul Cohen as an anti-realist, [12, 13]. Einstein was clear that mathematics was a product of human thought and that, as far as the propositions of mathematics are certain, they do not refer to reality, [16]. The author of the present article has always been critical of Platonism, [14]; he now fully accepts the existence of mathematical entities, but only in the Carnapian sense, [15]. This allows mathematical theories to be products of the human imagination, but nevertheless to have definite properties just as chess and Roman law do; it also allows numbers to exist in the same sense as the black king does in chess. Fortunately mathematicians *as mathematicians* do not need to refer to their philosophical beliefs, and hence can achieve a large degree of agreement amongst themselves. This agreement is, however, not total: constructivists adopt a strict, algorithmic notion of existence that is more acceptable to applied mathematicians, numerical analysts and logicians than it is to most pure mathematicians, [7, 8, 9, 15].

Kurt Gödel's astonishing insights in the 1930s created the first of the three crises to which we refer. He demonstrated that within any sufficiently rich axiomatic system there must exist certain statements that can not be proved or disproved. He also established that the consistency of arithmetic was not provable. There have been many discussions of his work, but these frequently involve implicit philosophical assumptions on the part of the writer. For example, the belief of Gödel himself that the continuum hypothesis must be either true of false independently of whether we can prove this fact reveal his wholehearted commitment to Platonism in mathematics. Gödel's theorems are technical in nature and do not establish that there is a fundamental distinction between truth and provability in mathematics without the insertion of extra philosophical assumptions.

It might be thought that Gödel's attitude towards his own results must be of great significance, but he was a somewhat eccentric figure; his argument that one can have the same confidence in mathematical intuition as in sense perception does not sit happily with the consensus of psychologists that sense perception is heavily dependent on constructions within the human mind, [11], [14, p 38]. Other giants in the field have taken quite different attitudes. For example Paul Cohen, the person who eventually proved the independence of the continuum hypothesis, did not share Gödel's views, believing that set theory was no more than an axiomatic structure: it was not the partial description of an external entity, [12, 13].

In spite of the enormous literature emphasizing the importance of Gödels' work for the foundations and philosophy of mathematics, it had very little effect within mathematics itself for several decades, excepting logic, regarded as one among many fields of mathematics. Its relevance within mainstream mathematics only emerged when it was discovered that the word problem and the isomorphism problem for finitely presented groups were algorithmically insoluble and, as a consequence, the homeomorphism problem for 4-manifolds was also insoluble. Gradually more and more such issues have been revealed, but, in spite of this, most mathematicians ply their trade exactly as they would have done if Gödel had never existed.

Since 1970 two other crises have arisen in mathematics, neither of which was anticipated, just as Gödel's work had not been. Both involve the issue of complexity: proofs that are too long and complex for anyone to be able to assert with total confidence that the theorems claimed are certainly true. These crises have not been discussed much in the philosophical literature, even though both are starting to have more impact on the way that mathematicians think about their subject than Gödel's work ever has. In October 2004 the Royal Society held a two day discussion meeting in London on 'The Nature of Mathematical Proof' to discuss possible ways of responding to them; see [10]. The meeting provided a variety of insights into the issues involved but no solutions. There was evidence of a serious communication problem between the mathematicians and computer scientists present.

At first sight it seems obvious that the 'crises of complexity' that we will describe are epistemological in character, and say nothing about the ontology of mathematics. On the other hand some mathematicians prefer to think of mathematics as involving a process of creation rather than discovery, just as in architecture. One is free to pursue many different ideas as long as one follows certain basic rules, and need not accept that distinctions between ontology and epistemology are relevant. The crises may simply be the analogy of realizing that human beings will never be able to construct buildings a thousand kilometres high, and that imagining what such buildings might 'really' be like is simply indulging in fantasies.

2 Computer-Assisted Proofs

The first example of a major mathematical theorem that depended on computer assistance was the four colour theorem, proved by Appel and Haken in 1976, [1, 2]. It caused great uneasiness among some mathematicians for two reasons. One was that it was considered that one could not be *certain* that a machine had performed a calculation correctly if one could not check every line of the proof by hand. At that time 'proper' theorems had proofs that were agreed to be unassailable. Mistakes might occasionally occur, but they could and would be rectified with the passage of time. The other issue was that some mathematicians considered that they were not interested in *whether* theorems were true but *why* they were true. A proof that did not generate understanding was of no interest to them.

The four colour theorem did not have any very important applications, and for a considerable time it was possible to regard it as an aberration. Perhaps it was not really very interesting after all, and had only acquired fame because it was easily stated. However, as time has passed, and computers have become more available, the number of computer-assisted proofs has slowly grown. It would serve no useful purpose to enumerate all such cases, so we turn to the most recent example.

The Kepler problem is to determine the best way of packing identical solid spheres in three-dimensional space, so as to maximize their average density. The expected solution has been known for many years, and involves packing the spheres exactly as oranges are displayed in every grocer's shop. In 1998 Tom Hales announced the rigorous solution of this problem using a combination of geometrical analysis and heavy computer calculations. Annals of Mathematics solicited his paper, and set up a team of twenty of the top experts in the field to referee the work. They started by holding a conference in Princeton to decide their strategy. As the years passed referees gradually left the team, and early in 2004, the effort of refereeing the paper had to be discontinued. The Annals editors decided to publish the 'theoretical part' of the paper and send the computer-based part to a more appropriate journal for publication. One of the Annals editors, Robert MacPherson, admitted that the (unpublished) policy of the Annals editors for such papers had failed; see [18].

At the Royal Society meeting there were lively discussions about whether formal proofs of the correctness of programs could have made a contribution to the refereeing process. According to Macpherson the panel did not have any member who understood the technology of program correctness proofs, so this way of increasing confidence in the computer-assisted part of the proof was not considered. The programs had not been written with the possibility of formal verification in mind, and it is generally recognized that this greatly impeded any attempt to apply such methods.

Another possibility would be to write a totally new program that implemented the ideas in the theoretical part of the proof. This was dismissed as being too much to demand of any group of referees, a statement that shows how little mathematicians appreciate the labour involved carrying through projects to completion in other areas of science, for example the Cassini space probe to Saturn. Also relevant is the fact that as the refereeing process continued it became apparent that the computations were so specific to the particular problem that they provided few insights that could be applied to other similar problems.

The Kepler problem is closely related to finding the ground state energy of a large assembly of bodies, which may have a variety of shapes and ways of interacting with each other. There is a huge number of similar minimization problems, and that it is infeasible to understand the field by solving them one at a time by highly specific computations. If there is no other way perhaps most of these problems are not so interesting after all. However, the Kepler problem itself has connections with several other issues of known importance, including the theory of error-correcting codes.

On the positive side I must mention the steadily increasing use of computers, which are transforming the work of pure mathematicians. Here are a few randomly chosen examples, which fall into several different categories. Computer algebra can transform hopelessly lengthy calculations and has been used extensively in various fields. Benoit Mandelbrot was responsible for producing the beautiful colour pictures of the set now named after him (but known long before he was born), and for stimulating widespread interest in the subject. The investigation of chaotic dynamical systems could not have progressed without the possibility of numerical experimentation; it is true that the existence of chaotic phenomena was discovered by Henri Poincaré at the end of the nineteenth century, but progress in understanding the subject had to wait for the development of computers. The enormous differences between the spectral behaviour of self-adjoint and non-self-adjoint matrices came to light as a result of numerical experiments, and has spawned the new field of pseudospectra, which is now being studied as an area of rigorous mathematics in its own right, [28].

Controlled numerical calculations are also playing an essential role as intrinsic parts of papers in various areas of pure mathematics. In some areas of nonlinear PDE rigorous computer-assisted proofs of the existence of solutions have been provided; [22] and [23] provide typical examples. These use interval arithmetic to control the rounding errors in calculations that are conceptually quite straightforward. The key is to provide a rigorous proof of an inequality that is then used as a vital ingredient in the proof of the theorem. In principle the calculations could be done by hand, but in practice this would be quite impossible.

3 Formal Verification of Proofs

Anyone who has written even short computer programs knows that they are much less forgiving than mathematics. Tiny errors of syntax are caught by the compiler and stop the program completely. The multiple use of variable labels do not stop the program running, but they are usually easily detected by the fact that the output is rubbish. Mathematical errors are often detected by running the program on a very simple problem of the same type, to which the solution is already known. Varying the parameters of the problem allows one to check that the effects are as expected. Possible errors or inaccuracies in standard routines built into a software package are more difficult to detect, since the effects are likely to be small or infrequent. Nevertheless programs of length only a few hundred lines can be extremely powerful aids to mathematicians, and experience shows that they can be made to function as expected after some debugging. The real problems occur with *much* bigger programs and are a major problem: as I write the British Civil Service is trying to resolve a flawed software upgrade that has stopped the work of an entire department for almost a week.

The formal verification of software packages is simultaneously an area of applied logic and a business. The increased reliability of Windows XP has been achieved with the aid of powerful program analysis tools, which are themselves based on the mathematics of program correctness which was originally explored with the goal of formal verification. However, in some respects the problem faced by computer scientists is quite unlike that faced by mathematicians. The specification of some software, such as Java, may run to more than a hundred pages, far longer than would be acceptable for the statement of a theorem. It is not clear in some cases whether unexpected behaviour of a software package should be called a bug or a feature. Crashes, often caused by buffer overflows, are clearly the consequences of design faults, but one cannot say the same of the refusal of LaTeX2e to allow the user to do something that the designers never thought of. Inadequate specifications of large software projects are a much more common cause of commercial disasters than incorrect implementations of the specifications.

The proven value of formal proofs of correctness in the software context has encouraged some computer scientists to try to apply the same methods to mathematics, but this is, at present, an immature field. The following comments indicate that there are likely to be serious difficulties in implementing formal proofs of correctness in my area of analysis. They may well not be so relevant to other fields, such as logic or algebra, but I leave such judgements to others. I give some details in order to provide some feeling for the issues, but these are not essential. Almost every proof of a theorem in analysis alludes to external facts, that are frequently not spelled out, because they are assumed to be a part of the background of the reader. A paper might well start by stating that it intends to study the spectral theory of the Laplacian on a bounded Euclidean region subject to Dirichlet boundary conditions. There are hundreds, possibly thousands, of papers even on this tiny subject, and the writer will assume a familiarity with a substantial part of the literature. On some occasions he will refer to papers containing recent results that he considers the reader might not know about, but in many cases he will use older results without reference, confident that almost everyone who is well enough educated to want to read the paper will already know these.

There are real traps into which one can fall, and people sometimes do fall into them. When using a particular result it is possible to forget that there are often many versions of a theorem in analysis, with similar conclusions, but depending on different technical hypotheses. Monographs often make standing hypotheses, which are mentioned at the start of some section or chapter, but not anywhere near the statement of the theorem being quoted.

It is commonplace to justify a step in a proof by reference to some classical result for which no reference is given. I was challenged recently by one of my students in relation to Mercer's theorem. Mercer's original version referred to kernels on a one-dimension interval, but I was using a more general version of the theorem without explanation. When he asked me to justify my comment I was unable to find a statement of the theorem in the literature that was sufficiently general to cover the application that I was making. After looking through half a dozen books I eventually decided to write out the proof. It was obvious to me, and would have been to anyone who had read the original proof in sufficient detail, that the classical restriction to an interval was unnecessary, but it nevertheless took me four pages to describe and prove a sufficiently general form of the result. I did not regard this as a serious gap, in the sense that I was confident throughout that the result needed was correct, and that it would be obtained by extracting the core of Mercer's argument. The student ended up satisfied.

It seems that mathematics is carried in people's heads, and that it is malleable in the sense that experts 'know' almost instinctively whether it is possible to modify standard theorems to fit the context being discussed; perhaps this is the definition of an expert. Every now and again someone summons up the energy to write out a fairly comprehensive account of a field as a monograph. This provides a huge service, by giving a systematic account of a field to which one can then refer. Very frequently it also misrepresents the literature somewhat, because an author is almost bound to adopt a particular, uniform context in his monograph, and many of the theorems that he proves will be true under weaker conditions.

4 Finite Simple Groups

The third crisis that we discuss is also one concerning complexity, but it is in some ways more serious. Since it does not involve computers, we cannot dismiss it simply by declaring computer-assisted proofs illegitimate, i.e. not a part of what we call pure mathematics. In addition the example that I will describe involves one of the most central concepts in mathematics: symmetry, or, more technically, group theory.

During the 1970s more than a hundred group theorists came together in a consortium devoted to classifying all finite simple groups. The task was a massive one, and provided what is still the only example of industrial scale pure mathematics. Under the leadership of David Gorenstein the problem was broken up into smaller packages that were entrusted to various groups around the world. Intensive work over ten years led to a complete list of all finite simple groups: three infinite families, together with 26 sporadic (i.e. exceptional) groups. The existence of the largest of these, the so-called Monster, was only proved with the aid of a computer. Fortunately we can discuss the crisis surrounding this problem without knowing what the classification is, and without even knowing what a finite simple group is.

What happened after 1980 has been as interesting as the classification itself. One positive development in this period was the discovery of a method of avoiding the use of computers in the proof of the existence of the Monster. It was appreciated that the work of the different groups needed to be integrated into a single coherent account, but attempts to do this led to the discovery of many gaps in the proofs.

Many of these were patched up, but one seemed very serious, and in 1990 claims that the classification was complete had to be reconsidered. Eventually this gap was also filled by Aschbacher and Smith and, once again, it seems likely that the proof is sound, [3]. However only about five out of the twelve volumes of the final proof have been published, almost 25 years after the theorem was 'proved'; see [3, 27] for details. Michael Aschbacher, one of the people most heavily involved in the project, admits the possibility that a new finite simple group might one day be discovered. If that group has characteristics sufficiently similar to the others, this might not be too disturbing, but he accepts that the discovery of a new finite simple group quite different from the others would throw the problem wide open again; see [4]. Note that Jean-Pierre Serre is also very cautious about accepting the proof, [24].

Aschbacher has noted that the proof seems to be robust. By this he means that every gap so far discovered can be plugged with only a moderate amount of extra work, leaving the main lines of the proof unaffected. Unfortunately, this does not imply that the result is correct. A chain is as strong as its weakest link, and the fact that every faulty link has so far been replaced by a sound one provides no guarantee that it will remain so. If one thinks that the proof is more like a web, in which flaws in many threads would not jeopardize the integrity of the whole, then it is possible that the web contains a large enough hole for a fly to escape through it. Most flies might be caught by the web, but not necessarily all.

The idea of comparing mathematical knowledge to a web of interrelated facts de-emphasizes the role of linear logic in favour of the confidence associated with a highly redundant structure. This is not a new idea, but it has not been emphasized by mathematicians much until recently. Aschbacher uses a related analogy in [4], invoking the paradigm of biology as an information rich subject in which there is an overabundance of different ways of organizing the data, and contrasting this with 'classical mathematics'.

The completion of the classification project (in the sense of the publication of a connected account of the entire calculation) is threatened by the attrition of the leading players by death and retirement. Within ten years most of them may have stopped working, and there may well be too few left with the necessary deep understanding of the subject to complete the task. Even if the project is brought to a conclusion, it is likely that fewer than a dozen mathematicians will be able to claim a reasonably comprehensive understanding of the main lines of the proof.

We have thus arrived at the following situation. A problem that can be formulated in a few sentences has a solution more than ten thousand pages long. The proof has never been written down in its entirety, may never be written down, and as presently envisaged would not be comprehensible to any single individual. The result is important, and has been used in a wide variety of other problems in group theory, but it might not be correct.

It is of course possible that a much simpler approach to this particular classification problem will one day be discovered, but it is equally possible that it will not. Aschbacher is pessimistic about the existence of a moderately simple proof, observing that the estimated overall length of the (still unwritten) proof has not decreased much over the last quarter century. It follows from Turing's work that there are theorems whose proofs are far longer than their statements: indeed the ratio of the two lengths can be arbitrarily large. According to Cohen 'the vast majority of even elementary questions in number theory, of reasonable complexity, are beyond the reach of any reasoning', [13]. So we have to anticipate that more and more such results will be discovered as time passes.

5 The Consistency of Arithmetic

In this section we argue that the existence of simple statements that have extraordinarily long proofs may be of great importance. Gödel taught us that it is not possible to prove that Peano arithmetic is consistent, but everyone has taken it for granted that *in fact* it is indeed consistent.

Platonistically inclined mathematicians would deny the possibility that Peano arithmetic could be flawed. From Kronecker onwards many consider that they have a direct insight into the natural numbers, which guarantees their existence. If the natural numbers exist and Peano's axioms describe properties that they possess then, since the axioms can be instantiated, they must be consistent. Often this is dressed up with references to the expected or intended model of Peano's axioms, but expectations or intentions do not by themselves settle anything.

When we delve into history we see many reasons for doubting claims for certainty, even in mathematics. For many centuries it was thought self-evident that Euclidean geometry necessarily provided the correct description of space, but eventually Riemann and then Einstein proved this wrong. The status of the axiom of choice is usually regarded as unproblematical nowadays, but there was a vigorous debate early in the twentieth century about its acceptability. Even its inventor, Zermelo, eventually agreed that the most compelling reason to accept it was the fact that without it mathematicians could not prove large numbers of results that they needed; see Maddy [19, p. 56]. These doubts have not been resolved, but merely forgotten, by most of the community. We finally mention that Hilbert's confidence about the possibility of resolving all mathematical problems was shared by most of his contemporaries, until Gödel showed that it was unfounded.

It is, in fact, logically possible that Peano arithmetic is internally inconsistent. There is no evidence for this, and we do not claim that it is likely to be inconsistent, only that it is possible. To investigate this idea further we consider an example from group theory. Consider the following list of axioms.

(1) G is the set of elements considered, and it is supposed that the elements obey the group axioms.

(2) G is supposed to be finite but not isomorphic to any of the known list of finite simple groups.

(3) G is supposed to be simple. In other words if N is a subset that has a certain list of properties (those of a normal subgroup other than the trivial subgroup) then $N = G$.

These axioms can be compared to those of Peano arithmetic. The last is similar in form to the induction axiom (or axiom schema in first order logic) in that it refers to an unspecified set with certain properties, and concludes that it is equal to G (we assume that one can switch back and forth between subsets and predicates). Although G is assumed to be finite its size is not specified, so one cannot simply enumerate all objects of the above type, however long the time given: the only way of understanding the axiom system is via proofs.

The fact that an axiom scheme so similar to Peano arithmetic might require such a long proof of its inconsistency (if indeed it is inconsistent, as most group-theorists believe) provides a reason why we cannot be absolutely sure of the consistency of Peano arithmetic itself. Perhaps the shortest proof of an inconsistency in Peano arithmetic is one hundred million pages long, and we will never discover it. If we were never led into a contradiction, would the inconsistency matter? We could continue to prove theorems and derive interesting interconnections between ideas without ever suspecting the awful truth.

Such a situation need not imply that our efforts were worthless. There are many examples in the past in which contradictions in axiom systems, or counterexamples to theorems, once pointed out, have been rectified. A famous book of Imre Lakatos is a celebration of the ability of mathematicians to respond to counterexamples to a sequence of flawed statements of Euler's theorem, [17]. The most famous inconsistency was in Frege's foundations of mathematics, to which Bertrand Russell found a paradox. Within twenty years the ZFC set theory removed these particular problems, although at some cost in terms of elegance. Interesting mathematics (certainly in the field of analysis) is remarkably tolerant of changes in the axiomatic framework, and can often be rescued from technical errors, possibly after changing or increasing the number of assumptions.

6 Discussion

It seems to the author that the prospects for a complete proof of the Kepler problem are better than they are for the classification of finite simple groups. One day the programs may be rewritten in a form that permits a formal proof of the correctness of Hales' theorem. In the Royal Society meeting some mathematicians repeated the well-known argument that this would still not be satisfactory, because computer programs are fallible, computer hardware is fallible, and anyway the computer might be hit by a cosmic ray during the computation. These statements are obviously correct, but it would be absurd to think that similar criticisms do not apply to human-generated proofs, particularly in the light of the finite simple group experience. All one can ask of the formal computer verification of proofs is that they perform better than human beings, in the sense that they find mistakes in proofs that humans have missed and that humans recognize once they are pointed out. In the field of software and chip design verification this has already happened, and it is to be expected that it will become more common in mathematics itself.

A number of mathematicians are very concerned about where this revolution is leading us. If the goal of mathematics is understanding, then one cannot deny that computer-assisted proofs do not supply it in full measure. But nor does the proof of the classification of finite simple groups. In both cases the proofs are only locally checkable, and this provides no guarantee of global correctness. Many mathematicians find the prospect of losing this understanding abhorrent, and their best remedy is to stick to fields in which such methods are not yet needed. Fortunately there are vast swathes of the subject that remain ripe for development by traditional methods, so they need not worry too much that their contribution will become unnecessary within the foreseeable future.

Taking an historical perspective, we can see that once the number of mathematicians became large enough, they were almost bound to start producing a quantity of mathematics that could only be validated at a collective level. Combine this with the development of ever more sophisticated computer software, and the possibility of individuals being able to understand all aspects of a complex proof was certain to vanish. The twentieth century provided both of these conditions for the decisive and irreversible change in the nature of mathematical research. Pure mathematics will remain more reliable than most other forms of knowledge, but its claim to a unique status will no longer be sustainable. It will be seen as the creation of finite human beings, liable to error in the same way as all other activities in which we indulge. Just as in engineering, mathematicians will have to declare their degree of confidence that certain results are reliable, rather than being able to declare flatly that the proofs are correct. Hilbert's goal of achieving perfect certainty by the laying of firm foundations died with Gödel's work, but the problem of complexity would have killed his dreams with equal finality fifty years later.

We finally ask if there are further crises still to be faced. One possibility is the discovery of a contradiction in a mathematical argument whose complexity is beyond any yet contemplated. One might imagine that the contradiction is the result of a mistake that is too deep for us to be able to locate it, even with the aid of computers. This may seem far fetched, but a somewhat similar problem has already arisen in computer chess programs, which occasionally make moves for which the best chess grandmasters can find no rationale. The computer can, of course, only declare that the said move yielded the highest score out of billions of combinations that it had considered. This does not imply that the move is indeed the best in the given position, because the method of scoring positions is derived from human advice. If such a scenario materializes, we may finally have to admit to limits on what our species can aspire to in the mental realm, as well as in other types of activity.

Whether or not these prognostications prove correct, the future of pure mathematics is certain to be very different from its past. In 1875 every sufficiently able mathematician could fully absorb the proof of every theorem that existed within a few weeks. By 1975, a year before the four colour theorem was proved, this was not even close to being true, but it was still the case that some mathematician fully understood the proof of any known theorem. By 2075 many fields of pure mathematics will depend upon theorems that no mathematician could fully understand, whether individually or collectively. Many mathematicians will still prove theorems by traditional methods, but these will stand out as landmarks in a much broader subject. Formal verifications of complex proofs will be commonplace, but there will also be many results whose acceptance will owe as much to social consensus as to rigorous proof. Perhaps by then the differences between mathematics and other disciplines will be so much reduced that philosophical discussions of the unique status of mathematical entities will no longer seem relevant.

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