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DOES MATHEMATICS NEED NEW AXIOMS?

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Does mathematics need new axioms? was the second of three plenary panel discussions held at the ASL annual meeting, ASL 2000, in Urbana-Champaign, in June, 2000. Each panelist in turn presented brief opening remarks, followed by a second round for responding to what the others had said; the session concluded with a lively discussion from the floor. The four articles collected here represent reworked and expanded versions of the first two parts of those proceedings, presented in the same order as the speakers appeared at the original panel discussion: Solomon Feferman (pp. 401–413), Penelope Maddy (pp. 413–422), John Steel (pp. 422–433), and Harvey Friedman (pp. 434–446). The work of each author is printed separately, with separate references, but the portions consisting of comments on and replies to others are clearly marked.

PENELOPE MADDY

WHY THE PROGRAMS FOR NEW AXIOMS NEED TO BE QUESTIONED

SOLOMON FEFERMAN

The point of departure for this discussion is a somewhat controversial paper that I published in the ***American Mathematical Monthly*** under the title *Does mathematics need new axioms*? [4]. The paper itself was based on a lecture that I gave in 1997 to a joint session of the American Mathematical Society and the Mathematical Association of America, and it was thus written for a general mathematical audience. Basically, it was intended as an assessment of Gödel's program for new axioms that he had advanced most prominently in his 1947 paper for the ***Monthly***, entitled *What is Cantor's continuum problem*? [7]. My paper aimed to be an assessment of that program in the light of research in mathematical logic in the intervening years, beginning in the 1960s, but especially in more recent years.

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In my presentation here I shall be following [4] in its main points, though enlarging on some of them. Some passages are even taken almost verbatim from that paper where convenient, though of course all expository background material that was necessary there for a general audience is omitted.[1] For a logical audience I have written before about various aspects of the questions dealt with here, most particularly in the article *Gödel's program for new axioms*: *Why, where, how and what*? [2] and prior to that in *Infinity in mathematics. Is Cantor necessary*? (reprinted as Chs. 2 and 12 in [3]).

My paper [4] opened as follows:

> The question, "Does mathematics need new axioms?," is ambiguous in practically every respect.
> - What do we mean by "mathematics"?
> - What do we mean by "need"?
> - What do we mean by "axioms"? You might even ask, What do we mean by "does"?

Amusingly, this was picked up for comment by ***The New Yorker*** in its issue of May 10, 1999, in one of its little end fillers (op. cit., p. 50), as follows:

> "New" apparently speaks for itself.

I had to admit, they had me there.

Part of the multiple ambiguities that we see in the leading question here lies in the various points of view from which it might be considered. The crudest differences are between the point of view of the working mathematician not in logic related fields (under which are counted, roughly, 99% of all mathematicians), then that of the mathematical logician, and, finally, that of the philosopher of mathematics. Even within each of these perspectives there are obviously divergent positions. My own view is that the question is an essentially philosophical one: *Of course mathematics needs new axioms*—we know that from Gödel's incompleteness theorems—but then the questions must be: *Which ones*? and *Why those*?

Let's start by making some preliminary distinctions as to the meaning of 'axiom'. The ***Oxford English Dictionary*** defines 'axiom' as used in logic and mathematics by: "A self-evident proposition requiring no formal demonstration to prove its truth, but received and assented to as soon as mentioned." I think it's fair to say that something like this definition is the first thing we have in mind when we speak of axioms for mathematics: this is the *ideal* sense of the word. It's surprising how far the meaning of axiom has become stretched from the ideal sense in practice, both by mathematicians and logicians. Some have even taken it to mean *an arbitrary assumption* and so refuse to take seriously what status axioms are to hold.

[1]The parts taken from [4] are reprinted here with the kind permission of the ***American Mathematical Monthly***.

When the working mathematician speaks of axioms, he or she usually means those for some particular part of mathematics such as groups, rings, vector spaces, topological spaces, Hilbert spaces, and so on. These kinds of axioms have nothing to do with self-evident propositions, nor are they arbitrary starting points. They are simply definitions of kinds of structures which have been recognized to recur in various mathematical situations. I take it that the value of these kinds of *structural axioms* for the organization of mathematical work is now indisputable.

In contrast to the working mathematician's structural axioms, when the logician speaks of axioms, he or she means, first of all, laws of valid reasoning that are supposed to apply to *all* parts of mathematics, and, secondly, axioms for such fundamental concepts as number, set and function that underlie *all* mathematical concepts; these are properly called *foundational axioms*.

The foundational axioms correspond to such basic parts of our subject that they hardly need any mention at all in daily practice, and many mathematicians can carry on without calling on them even once. Some mathematicians even question whether mathematics needs any axioms *at all* of this type: for them, so to speak, mathematics is as mathematics does. According to this view, mathematics is self-justifying, and any foundational issues are local and resolved according to mathematical need, rather than global and resolved according to possibly dubious logical or philosophical doctrines.

One reason the working mathematician can ignore the question of need of foundational axioms—and I think that we [members of the panel] are all agreed on this—is that the mathematics of the 99% group I indicated earlier can easily be formalized in ZFC and, in fact, in much weaker systems. Indeed, research in recent years in predicative mathematics and in the Reverse Mathematics program shows that the bulk of it can be formalized in subsystems of analysis hardly stronger than Π^1_1-CA,[2] and moreover the scientifically applicable part can be formalized in systems conservative over PA and even much weaker systems.[3] So, foundationally, everyday mathematics rests in principle on unexceptionable grounds.

Before going on to the perspectives of the mathematical logician and the philosopher of mathematics on our leading question, let's return to Gödel's program for new axioms to settle undecided arithmetical and set-theoretical problems. Of course, the part of Gödel's program concerning arithmetical problems goes back to his fundamental incompleteness results, as first indicated in ftn. 48a of his famous 1931 paper [6]. It was there that Gödel asserted the true reason for incompleteness to be that "the formation

[2]For predicative mathematics, cf. [3], Chs. 13 and 14; for Reverse Mathematics, cf. Simpson [19].

[3]Cf. [3], Chs. 13 and 14 for the claim about PA. Feng Ye has shown in his dissertation [20] that substantial portions of scientifically applicable functional analysis can be carried out constructively in a conservative extension of PRA.

of ever higher types can be continued into the transfinite"; he repeated this reason periodically since then, but did not formulate in print the exact nature of such further axioms. An explicit formulation of the program in pursuit primarily of settling CH only appeared in the 1947 article on Cantor's Continuum Problem (and its 1964 revision in the light of subsequent events). It was also there that Gödel made the distinction between new axioms based on *intrinsic* reasons and those based on extrinsic reasons. Concerning the former he pointed to axioms of Mahlo type, of which he said that "these axioms show clearly, not only that the axiomatic system of set theory as known today is incomplete, but also that it can be supplemented without arbitrariness by new axioms which are only the natural continuation of those set up so far." ([7], p. 520) Since Gödel thought CH is false and recognized that Mahlo-type axioms would be consistent with $V = L$, he proposed other reasons for choosing new axioms; hopefully, these would be "based on hitherto unknown principles ... which a more profound understanding of the concepts underlying logic and mathematics would enable us to recognize as implied by these concepts", and if not that, then one should look for axioms which are "so abundant in their verifiable consequences ... that quite irrespective of their intrinsic necessity they would have to be assumed in the same sense as any well-established physical theory." ([7], p. 521)

My co-panelists are better equipped than I to report on the subsequent progress on Gödel's program in the case of set theory.[4] Briefly, the research in this direction has concentrated primarily on higher axioms of infinity, also known as large cardinal axioms (LCAs). These are divided roughly between the so-called "small" large cardinals such as those in the Mahlo hierarchies of inaccessible cardinals, and the "large" large cardinals, a division that corresponds roughly to existence axioms accepted on intrinsic grounds (or consistent with $V = L$) and those accepted on extrinsic grounds. The division is not a sharp one but falls somewhere below the first measurable cardinal.[5] By all accounts from the specialists, the high point in the development of "large" large cardinal theory is the technically very impressive work extending "nice" properties of Borel and analytic sets, such as Lebesgue measurability, the Baire property, and the perfect subset property—via the determinateness of associated infinitary games—to arbitrary sets in the projective hierarchy, all under the assumption of the existence of infinitely many Woodin cardinals.[6]

But the striking thing, despite all such progress, is that—contrary to Gödel's hopes—the Continuum Hypothesis is *still* completely undecided,

[4]That was indeed stressed in the presentations of Maddy and Steel.

[5]Cf. Kanamori [11], p. 471.

[6]This is due, cumulatively, to the work of many leading workers in higher set theory; cf. Martin and Steel [15] and Steel's presentation to this panel discussion, for the history of contributions.

in the sense that it is independent of all remotely plausible axioms of infinity, including all "large" large cardinal axioms which have been considered so far.[7] In fact, it is consistent with all those axioms—if they *are* consistent—that the cardinal number of the continuum is anything it "ought" to be, i.e., anything which is not excluded by König's Theorem.[8] That may lead one to raise doubts not only about Gödel's program but its very presumptions. Is the Continuum Hypothesis a definite problem as Gödel and many current set-theorists believe?

Here's a kind of test of one's views of that: as has been widely publicized, a Clay Mathematics Institute that has recently been established in Cambridge, Massachusetts is offering what it calls Millennium Prizes of \$1,000,000 each for the solution of seven outstanding open mathematical problems, including P = NP, the Riemann Hypothesis, the Poincaré conjecture, and so on. But the Continuum Problem is not on that list. Why not? It's one of the few in Hilbert's list from one hundred years ago that's still open. Would you feel confident in going to the scientific board of that institute and arguing that the Continuum Problem has unaccountably been left off, and that its solution, too, should be worth a cool million?

My own view—as is widely known—is that the Continuum Hypothesis is what I have called an "inherently vague" statement, and that the continuum itself, or equivalently the power set of the natural numbers, is *not* a definite mathematical object. Rather, it's a conception we have of the totality of "arbitrary" subsets of the set of natural numbers, a conception that is clear enough for us to ascribe many evident properties to that supposed object (such as the impredicative comprehension axiom scheme) but which cannot be sharpened in any way to determine or fix that object itself. On my view, it follows that the conception of the whole of the cumulative hierarchy, i.e., the transfinitely cumulatively iterated power set operation, is even more so inherently vague, and that one cannot in general speak of what is a fact of the matter under that conception. For example, I deny that it is a fact of the matter whether all projective sets are Lebesgue measurable or have the Baire property, and so on.

What then—on this view—explains the common feeling that set theory is such a coherent and robust subject, that our ordinary set-theoretical intuitions are a reliable guide through it (as in any well accepted part of mathematics), and that thousands of interesting and *prima facie* important results about sets which we have no reason to doubt have already been established?

[7]Interestingly, the only detailed approach we know of to settle CH that Gödel himself tried—first negatively and then positively—was not via axioms for large cardinals but rather via proposed axioms on scales of functions between alephs of finite index. Whatever the merits of those axioms *qua* axioms, his attempted proofs (c. 1970) using them proved to be defective; cf. [8], pp. 405–425.

[8]Cf. Martin [14]. The situation reported there in 1976 is unchanged to date.

Well, I think that only shows that in set theory as throughout mathematics, a little bit goes a long way—in other words, that only the crudest features of our conception of the cumulative hierarchy are needed to build a coherent and elaborate body of results. Moreover, one can expect to make steady progress in expanding this body of results, but even so there will always lie beyond this a permanently grey area in which such problems as that of the continuum fall.

While Gödel's program to find new axioms to settle the Continuum Hypothesis has not been—and will likely never be—realized, what about the origins of his program in the incompleteness results for consistent formal systems extending number theory? Throughout his life Gödel said we would need new, ever-stronger set-theoretical axioms to settle open arithmetical problems of even the simplest, purely universal form, problems that he frequently referred to as of "Goldbach type". But the incompleteness theorem by itself gives no evidence that any open arithmetical problems—or equivalently, finite combinatorial problems—of *mathematical interest* will require new such axioms.

We're all familiar with the fact that the Π^0_1 statement shown undecidable by the first incompleteness theorem for a given formal system S (containing arithmetic) is cooked up by a diagonal construction, while the consistency statement $\mathrm{Con}(S)$ shown independent by the second incompleteness theorem is of definite *metamathematical interest*, but not of mathematical interest in the usual sense. Also familiar is the work of Paris and Harrington proving the independence from PA of a special finite version of Ramsey's Theorem, and, beyond that, the work of Harvey Friedman proving the independence of a finite version of Kruskal's Theorem from a moderately impredicative system and of an Extended Kruskal Theorem from the system of Π^1_1-Comprehension.[9] Each of these is a Π^0_2 statement shown true by ordinary mathematical means (i.e., in a way understandable to mathematicians without invoking any mention of what axioms they depend on, or of any metamathematical notions) and is established to be independent of the respective S by showing that it implies (or is even equivalent to) the 1-consistency of S, $1\text{-}\mathrm{Con}(S)$.[10]

For a number of years, Friedman has been trying to go much farther, by producing mathematically perspicuous finite combinatorial statements φ whose proof requires the existence of many Mahlo cardinals and even of stronger axioms of infinity (like those for the so-called subtle cardinals), and

[9]Cf. Paris and Harrington [16] and, for results related to Kruskal's Theorem, Simpson [19], p. 408.

[10]1-Con(S) is the statement of ω-consistency of S restricted to Σ^0_1 sentences; in other words, it says that each such sentence provable in S is true.

he has come up with various candidates for such φ.[11] From the point of view of metamathematics, this kind of result is of the same character as the earlier work just mentioned; that is, for certain very strong systems S of set theory, the φ produced is equivalent to (or is slightly stronger than) $1\text{-}\mathrm{Con}(S)$. But the conclusion to be drawn is not nearly as clear as for the earlier work, since the *truth* of φ is now *not* a result of ordinary mathematical reasoning, but depends essentially on accepting $1\text{-}\mathrm{Con}(S)$. In my view, it is begging the question to claim this shows we need axioms of large cardinals in order to demonstrate the truth of such φ, since this only shows that we "need" their 1-consistency. However plausible we might find the latter for one reason or another, it doesn't follow that we should accept *those axioms themselves* as first-class mathematical principles.

My point here is simply that there is a basic difference between accepting systems such as ZFC + LCA, where LCA is the applicable large cardinal axioms, and accepting $1\text{-}\mathrm{Con}(\mathrm{ZFC}+\mathrm{LCA})$. As to the question of the need of large cardinal assumptions to settle finite combinatorial problems of the sort produced by Friedman, there is thus, in my view, an equivocation between needing a given axiom and needing its 1-consistency; it is only the latter that is demonstrated by his work. But if one does not grant that there is a fact of the matter whether statements LCA of various large cardinal axioms are true, is there a principled reason for accepting $1\text{-Con}(\mathrm{ZFC} + \mathrm{LCA})$ without accepting ZFC + LCA itself? Of course, if one does think that there *is* a fact of the matter as to whether such statements LCA are true, then the equivocation is a non-issue. But then, what is it that leads one to recognize LCA rather than its negation to be true?

Returning to the question of mathematical interest, there is not a shred of evidence so far that we will need anything beyond ZFC—or even much weaker systems—to settle outstanding combinatorial problems of interest to the working mathematician, such as those on the Millennium Prize list, nor is there any evidence that the kind of *metamathematical* work we've seen from Paris-Harrington to Friedman will bear any relevance to the solutions of these problems, if they are ever solved at all.

Thus, as I said at the outset, I think we are left to regard the question: Does mathematics need new axioms?, as primarily a philosophical one. And if you agree with me on that, then we have the discouraging conclusion that we can expect as many answers to the question as there are varieties of the philosophy of mathematics; among those that have been seriously supported in one quarter or another, we have the platonic-realist, structuralist, naturalist,

[11] In [4] I referred to Friedman [5] for his then most recent work in that direction. More recently, Friedman has been promoting rather different statements derived from Boolean relation theory; cf. my comments on Friedman's Urbana presentation below.

predicativist, constructivist, and formalist philosophies.[12] In other words, if the problem is indeed a philosophical one, we can hardly expect an answer that will command anywhere near general assent.

But as a mathematical logician, if not as a working mathematician or philosopher of mathematics, I can end with a bit more positive conclusion. Even if mathematics doesn't convincingly need new axioms, it may need for instrumental and heuristic reasons the *work* that has been done and continues to be done in higher set theory. For example, in my own subject—proof theory—analogues of large cardinal notions have proved to be very important in the construction of recursive ordinal notation systems for the "ordinal analysis" of various subsystems of analysis and admissible set theory.[13] So far, these just employ symbols that act in the notation systems like "small" large cardinals, and do not depend on the assumption that such cardinals actually exist. The widespread appearance of analogue large cardinal notions (and, more generally, large set notions) also in admissible set theory, constructive set theory, constructive type theory and my own systems of explicit mathematics[14] suggests that there should be a general theory of such notions which includes all these as special cases. So far, these analogues correspond mostly to "small" large cardinals. At any rate, without the considerable work in higher set theory that led to such notions, these other areas of mathematical logic might still be back where they were in the early 1960s. It remains to be seen whether the bulk of that work, which is on "large" large cardinals, can have similar applications, and if not—why not.

COMMENTS AND RESPONSES

Response to Maddy. Maddy argues from a position that she calls the *naturalistic* point of view as to the philosophy of mathematics.[15] According to this, mathematical practice, and set-theoretical practice in particular, is not in need of philosophical justification. "Justification ... comes from within ... in ... terms of what means are most effective for meeting the relevant mathematical ends. Philosophy follows afterwards, as an attempt to understand the practice, not to justify or to criticize it." From that point of view, the original panel question is "a bit off target". Rather, "it would be more appropriate to ask whether or not some particular axiom ... would or would not help this particular practice ... meet one or more of its particular

[12]Perhaps one should even add the philosophy that is implicit in the view that category theory provides the proper foundation of mathematics. What to call it?

[13]Cf., e.g., Pohlers [17] and Rathjen [18].

[14]For these, cf. Aczel and Richter [1], Griffor and Rathjen [9], and Jäger and Studer [10].

[15]Maddy has elaborated this position in [13]. That is incidentally a retreat from her attempt in [12] to formulate a compromise between Gödelian platonic realism and Quinean scientific realism (one form of naturalism) that would justify current higher set-theoretical practice.

goals." The example given, from contemporary set theory, is the assumption of many Woodin cardinals.

The naturalistic point of view in philosophy, as usually described, is that the entities to be admitted are just those posited by and studied in the natural sciences, and that the methods of justification and explanation are somehow continuous with those of the natural sciences.[16] One of the foremost exponents of the naturalist position in this sense is Quine and, according to his view, only so much of mathematics is justified as is indispensable to scientific practice. Thus, Maddy's use of 'naturalism' to describe her point of view is strikingly contrary to that, since her aim, above all, is to account for and in some sense give approbation to that part of current set-theoretical practice which accepts various large cardinal axioms (that happen to be inconsistent with $V = L$, among other things). This she does by taking mathematics in general and set theory in particular as a "science" to be studied in its own right, independently of its relationships to the natural sciences.

While Maddy keeps invoking mathematical practice in general in the scope of her naturalism, she does not reflect on the many instances in its history in which the question of what entities are to be admitted to mathematics and what methods are legitimate had to be faced, leading to substantial revisions from what's OK to what's not OK and vice versa. In binding itself to mathematical practice, this kind of naturalism is in danger of being unduly transitory. Even if one takes the proposed naturalistic point of view and mathematical practice as exemplified in set theory for granted, there is a crucial question as to what determines the "mathematical ends" for which the "most effective" means are to be sought. And having chosen the ends, in what sense does effectiveness justify the means? Why is it to be presumed that the "good" properties of Borel and analytic sets should generalize to all projective sets, given that they don't hold for all sets?

Maddy says her naturalist needn't concern herself with "whether the CH has a determinate truth value in some Platonic world of sets" or "confront the question of whether or not it is 'inherently vague'." Why then is it assumed that there has to be a determinate answer to whether all projective sets have the perfect subset property or the property of Baire, etc.? Is there something essentially different about the character of these set-theoretical problems that makes the latter determinate but not (necessarily) the former? At any rate, admitting the possibility of some kind of indeterminateness for CH seems to me to be a slippery slope for the naturalist.

As a final point, Maddy suggests that I'm in favor of limiting mathematics, though "the essence of pure mathematics is its freedom." Surely she does not think that *anything* goes in mathematics. Old-style infinitesimals, Dirac delta-functions, unrestricted comprehension? If not, what justifies what is to be admitted to mathematics? Once it is agreed that there has to be *some* sort

[16]Cf. (R. Audi, ed.) ***The Cambridge Dictionary of Philosophy***, 2nd ed. (1999), p. 596.

of justification, intrinsic or extrinsic, then one is in the game of potentially limiting mathematics in some way or other. I don't hew to any sort of absolute principle in favor of limiting mathematics.

Response to Steel. In his discussion of my contention that the continuum problem is inherently vague, Steel says that "if the language of 3rd order arithmetic [in which it is couched] permits vague or ambiguous sentences, then it is important to trim or sharpen it so as to eliminate these ... it may be that, in the end, our solution to the Continuum Problem is best seen as resolving some ambiguity."

It is useful, in response, to elaborate my ideas about vagueness more generally. These can be illustrated, to begin with, in the context of very familiar, set-theoretically low down mathematics. The conception of the structure **N** of the natural numbers is not a vague one (at least in my view); statements about **N** have a definite truth value, and the axioms of PA are among those and (on reflection) are evident for it. By comparison, the notion of *feasible* (or *feasibly computable*) *natural number* is a vague one, and inherently so; there is no reasonable way to make it definite. Though we might well admit certain statements about feasible numbers as being evident, e.g., if n and m are feasible, so is $n + m$, we cannot speak of truth or falsity of statements about feasible numbers in general. Nevertheless, the notion of feasibly computable number is sufficiently suggestive to act as a heuristic for a reasonable mathematical theory. Similarly, the notion of *random number between* 1 *and* 10 is vague, but the conception of it makes it evident that the *probability* of such a number being less than 6 is $1/2$. It is from such vague beginnings that substantial, coherent, and even robust mathematical theories can be developed—without committing oneself to a notion of truth as to the notions involved.

In the case of set theory, it is at the next level (over **N**) that issues of evidence, vagueness, and truth arise. Once the conception of the structure of *arbitrary sets of natural numbers* is presented to us and we reflect on it, the axioms of second-order arithmetic ("analysis") become evident for it. Nevertheless, in my view, the meaning of 'arbitrary subset of **N**' is vague, and so I would strongly resist talking about truth or falsity of analytic statements. In opposition to my view it might be argued that the structure of the continuum, when conceived geometrically, is not vague, and hence that analytic statements have a definite truth value via the interpretation of analysis in the real numbers. Probably if a poll were taken, few mathematicians would agree with me that the notion of *arbitrary real number* is vague, and so I would not want to make an issue of it. But I believe I would garner substantially greater support of my consequent view that the notion of *arbitrary subset of the real numbers* (existing independently of any human definitions or constructions) is vague, since we no longer have the anchor of geometric intuition there. Moreover, I would argue that it is *inherently vague*, in the sense that there is no reasonable way the notion can be sharpened without violating what the notion is supposed to be about. For example, the assumption that all subsets of the reals are in L or even $L(\mathbf{R})$ would be such a sharpening, since that violates the idea of "arbitrariness". In the other direction, it is hard to see how there could be any non-circular sharpening of the form that there as many such sets as possible. It is from such considerations that I have been led to the view that the statement CH is inherently vague and that it is meaningless to speak of its truth value; the fact that no remotely plausible axioms of higher set theory serve to settle CH only bolsters my conviction. From the quote above, Steel is apparently willing to countenance an ambiguity in the notions involved in CH. If, as he puts it, the best thing then to do would be to resolve the ambiguity, it would show CH to be vague but not inherently so; that is the nub of our disagreement.

Relatedly, Steel characterizes my views as being instrumentalistic, which he takes to be a "dodge", but he oversimplifies my position in that respect. One kind of instrumentalism that I have espoused, to the extent that I have done so in one place or another, is very much a Hilbertian one (in the relativized sense): given a system S that one understands and accepts, if another system T is reduced to S, *conservatively* in the language of S, then that justifies the use of T, even if one does not grant definite meaning to the language of T beyond that of S. As an example, the overwhelming part (if not all) of scientifically applicable mathematics can be formalized in certain higher order systems T which are conservative over PA, and in fact much of it is already conservative over PRA; that thereby justifies the use and applications of such T (cf. [3], Chs. 13, 14). Similar results hold for the bulk of everyday mathematics (whether pure or applied) conservatively (for certain analytic statements) over constructively justified systems (cf. [3], pp. 201 ff). This kind of instrumentalism is thus philosophically satisfactory.

I have also argued (e.g., in [3], p. 73), that one's picture of the cumulative hierarchy is clear enough as a whole to justify confidence in the use of ZFC (and like theories) for deriving number-theoretical results. This is a *pragmatic* instrumentalism which is *not* philosophically satisfactory since there is thus far no philosophically satisfactory justification for ZFC, at least none in my view. But the result of the case studies cited above shows that though this kind of instrumentalism admits much more in principle than the preceding, there is no real difference in practice (i.e., with respect to the mathematics of the "99% of all mathematicians".

Response to Friedman. The core of Friedman's presentation consists of two daring predictions about the effect of his new work on Boolean relation theory, which it is claimed will eventually force the mathematical community to accept fully (perhaps after a period of controversy) new large cardinal axioms. Which those are is not specified, and in particular it is not said whether these will just be "small" large cardinals (presumably palatable to mathematicians with a certain amount of encouragement) or also "large" large cardinals. It is also not predicted how long this will take. If he is right, time will tell. If not ...? The criteria a–g he proposes for the adoption of new axioms set a very high bar (Olympic sized), but in my view appropriately so.

Finally, Friedman addresses my point that there is an equivocation between needing a large cardinal axiom and needing the statement of its 1-consistency (over ZFC). He says that the choices are essentially equivalent for the purposes of proving Π^0_2 statements. Of course. He also says that it is more natural to develop such consequences of, say, Boolean relation theory under the assumption of the axiom rather than the statement of its 1-consistency. I also agree to that. But I do not agree with his conclusion that this will show we "need" large cardinal axioms. It is neither here nor there that he means by "φ needs large cardinals to prove" that "any reasonable formal system that proves φ must interpret large cardinals in the sense of Tarski." If φ is equivalent to the statement of 1-consistency of a large cardinal axiom LCA and PA $\subseteq$ T and T proves φ then of course LCA is interpreted in T by the formalized completeness theorem. But that doesn't show that we need either LCA or its consequence φ in the normal sense of the word.

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DOES MATHEMATICS NEED NEW AXIOMS?

PENELOPE MADDY

As Feferman has made clear,[17] we can't begin to address our title question—does mathematics need new axioms?—without first asking—need for what purpose? If we begin with the purpose of providing tools for current science, I think we must agree with him that ZFC is surely enough, and indeed that weaker systems would probably do (p. 109). Granted, there are some lingering worries about whether science could get by in practice on these weaker systems; in the context of discovery, the more free-wheeling impredicative and infinitary methods might be needed. But Feferman himself grants this (p. 109). So, we agree that mathematics doesn't need new axioms for its practical uses in current science, and that it probably needs far fewer axioms than it already has for its strictly formal uses in current science.

Still, there's an odd bias implicit in the shape of this discussion, and in the formulation of our title question. Why ask 'does mathematics *need* such-and such?' When they were first developed, did mathematics *need* n-dimensional spaces, non-Euclidean geometry, or abstract algebra? It surely didn't need them for the science of the time, though I've chosen examples that did later

[17]In his [2], from which his remarks in this panel discussion begin. Unreferenced page numbers in the text refer to the printed paper.

become essential to science. I think the moral of the story of the development of mathematics in the 19th and 20th centuries is that attempts to limit mathematics are a bad idea, that the essence of pure mathematics is its freedom, that mathematicians should be allowed to follow their mathematical noses wherever they lead—and that this would be true even if all we cared about were the usefulness of mathematics in science.[18] Perhaps we'd do better to replace the original question—does mathematics need new axioms?—with the more open-ended question—would mathematics benefit from new axioms?

I'll come back to this in a moment, but for now let's skip directly to the broadest version of our title question: what is needed for all of modern mathematics? Here, of course, we know that new axioms are needed to settle basic questions of descriptive set theory, questions about the properties of simple sets of real numbers, questions raised nearly a century ago by the early analysts Luzin and Suslin. Though these new axioms don't yet appear on official lists at the beginnings of our textbooks, they are largely adopted, sometimes without comment, by those working in the field. Thus it is common, in these discussions, to assume the existence of an array of large large cardinals, most often a generous store of Woodin cardinals.

Now anyone who works in the generous spirit of modern mathematics touched on a moment ago, anyone moved by his faith that 'the more mathematically interesting structures the better', any such person would applaud this development in set theory, but it seems that Feferman does not. Though he insists that his 'main concern' is simply to determine 'what, fundamentally, is needed for what?' (p. 109), we can hardly ignore his disapproval of efforts to find new axioms to settle the CH. Though the problem of the CH is more complex than the earlier problems in descriptive set theory and the outcome of efforts to settle it is still doubtful, it is unclear why the effort to settle CH should be fundamentally misguided in a way that the (successful) efforts to settle the others were not. In fact, I suspect that Feferman is as uncomfortable with the large cardinals needed to settle the old questions of descriptive set theory as he is with current efforts to settle the CH. What I want to do here is investigate two interrelated reasons that seem to me to lie behind this distaste.

The first of these is his view that Platonism—which he rejects as 'thoroughly unsatisfactory' (p. 110)—provides the fundamental justification for contemporary set theory (pp. 109–110). I take it Feferman has in mind the use of Platonism to argue that statements like CH, though independent, nevertheless have objective, determinate truth values.[19] Feferman emphatically rejects this point of view, insisting that 'the Continuum Hypothesis is

[18]For more discussion of the role of applied mathematics, see [5].

[19]As in [3], p.260.

an inherently vague problem', and thus that '*no* new axiom will settle [it] in a convincingly definite way' (p. 109).

Now perhaps I don't find Platonism as repugnant as Feferman does, but I do agree with him on the operative claim: that Platonism cannot justify the practice of set theory, in particular, the practice of seeking new axioms to settle the CH. Where I disagree with Feferman is in the implicit assumption that Platonism is the only possible justification for this set theoretic practice, and thus, that any failure of Platonism leaves the practice unjustified.

What I propose is that philosophical considerations like those surrounding Platonism are largely irrelevant to the very real methodological questions of set theoretic practice: is CH a legitimate mathematical question, despite its independence?; what reasons could there be for accepting or rejecting any given candidate for a new axiom of set theory? What matters for these questions, I would argue ([4]), is a wide range of specifically mathematical considerations, considerations directly linked to the goals of the particular mathematical practice in question. To see how this played out in one historical case, recall the debate over the Axiom of Choice. Much heat was generated in metaphysical battles between Platonism and various versions of Definabilism or Constructivism—a debate unresolved to this day—but in the end, as Greg Moore demonstrates in his delightful case study ([8]), the axiom was adopted because it is essential to achieving the mathematical goals of a stunningly diverse array of practices. To take just one such goal, close to home, if you want a well-behaved theory of transfinite numbers, you will want your set theory to include the axiom of choice. This is one among many mathematically sound reasons for preferring that our fundamental theory of sets include the axiom.

The idea, then, is that set theoretic practice in particular, and mathematical practice in general, are not in need of justification from philosophical quarters. Justification, on this view, comes from within, couched in simple terms of what means are most effective for meeting the relevant mathematical ends. Philosophy follows afterwards, as an attempt to understand the practice, not to justify or to criticize it. From this naturalistic point of view, our original question—does mathematics need new axioms?—is a bit off target. It would be more appropriate to ask whether or not some particular axiom—say one asserting the existence of many Woodin cardinals—would or would not help this particular practice—contemporary set theory—meet one or more of its particular goals. In this case, the answer is yes: the assumption of many Woodins provides a complete theory of the projective sets of reals, something set theorists had sought for decades, and it does so in a manner consistent with various other set theoretic goals. I won't try to spell out the details here; I just want to indicate the type of justification I have in mind for this naturalistic approach to set theoretic method.

What, then, does naturalism suggest for the case of the CH? First, that we needn't concern ourselves with whether or not the CH has a determinate truth value in some Platonic world of sets; we needn't confront the question of whether or not it is 'inherently vague', assuming this involves some extra-mathematical theory of the meaning of set theoretic vocabulary; we needn't even argue that the answer to the CH is somehow pre-determined, that there is a pre-existing right answer out there for us to discover. Instead, we need to assess the prospects of finding a new axiom that is well-suited to the goals of set theory and also settles CH. As Feferman points out, we don't currently have such an axiom. Perhaps it will turn out that there is no such axiom, perhaps for some reason of principle, but especially in light of recent work, it seems to me premature to declare the case hopeless.

So, I suggest that Feferman's first reason for discomfort over the search for new set theoretic axioms is his mistaken belief that the only possible justification for this practice must lie in philosophical Platonism. We can get at the second reason if we ask ourselves why he should be so pessimistic so soon.

To see this, notice that the mathematical reasons for adding one or another axiom to ZFC are often divided into two varieties: intrinsic and extrinsic. Extrinsic reasons are easy to recognize; they involve the consequences of a given axiom candidate, its fruits, if you will. Intrinsic reasons are described in various ways by various writers, using terms like 'self-evident', 'intuitive', and 'part of the very concept of set'. The justifications I've gestured towards so far have all been extrinsic.

Now Feferman says nothing directly against the notion of an extrinsic justification, but I think it's apparent that he's less than impressed by them. To begin with, he gives considerable attention to the OED's definition of an axiom as 'a self-evident proposition' (p. 100), which clearly excludes extrinsic justifications for axioms, and he praises the Peano axioms for coming 'as close as anything we have to meeting the ideal dictionary sense of the word' (p. 101). He continues

> If [the axioms of ZFC] are to be considered axioms in the ideal, dictionary sense, they should be evident for some pre-axiomatic concept that we have in mind. (p. 102)

One candidate for this pre-axiomatic concept is Frege's naïve notion of the extension of a property, but this falls prey to the paradoxes. The only other option is Zermelo's iterative conception, and this, according to Feferman, involves us in the Platonism he finds so unpalatable (pp. 102–103). Notice that the possibility of extrinsic supports for the axioms of ZFC isn't even raised.

Now, obviously, if Feferman is unmoved by extrinsic justifications, it's no surprise that he would disapprove the contemporary search for new set theoretic axioms, which relies on them so heavily. So we must ask for the ground of this preference: what's wrong with extrinsic justifications, and what's so good about intrinsic ones?

As a warm-up for this question notice that a Platonist might balk at extrinsic justifications of the broad variety under consideration here. These take the form: we want this particular mathematical theory to do such-and-such; using method so-and-so is an effective way of achieving such-and-such; therefore, it's reasonable for us to use method so-and-so. A true Platonist might well object that it isn't enough that our mathematical theory do all the things we want it to do, it must also be true—that is, true in the objective world of mathematical objects that the theory purports to describe. My naturalist doesn't care about these philosophical niceties; what matters are the intra-mathematical goals and the effectiveness of various means of achieving them.

So a Platonist might object to our extrinsic justifications, but of course, Feferman is no Platonist; he puts no stock in mathematical objects. Philosophically, he takes the central question to be—What is the nature of mathematical concepts?—and these are

> conceptions of certain kinds of ideal worlds, ... presented more or less directly to the imagination, from which basic principles are derived by examination ([1], p. 124).

Now if these concepts are taken to be objectively real, we could imagine an objection to extrinsic reasons that runs parallel to the Platonist's: it isn't enough that a theory be effective, it must also be true of those concepts! But here again, Feferman rejects the realist line. Still, he attaches great importance to teasing out 'what is implicit in the concepts and principles' ([1], p. 122), so there does seem to be some objective content about which we might go wrong if we trusted to merely justifications.

The question the naturalist can hardly help asking at this point is why it matters so much what is and what isn't part of our existing concept? Imagine ourselves, for example, around 1870, working with Felix Klein or Sophus Lie to isolate the notion of 'group'. We would have various mathematical goals in mind, and we would mold the axioms of group theory to meet those goals as best we could. [20] What we would not do is stop and ask ourselves if the existence of inverses is or is not part of our concept of 'group'. Why should set theory be any different? Why should a contemporary set theorist, appreciative of the power of large cardinals, stop to ask himself whether or not they are contained in the concept of set?

Here Feferman has a clear answer. The axioms of group theory are 'structural axioms'; they aim to 'provide a framework' and their value 'for the organization of mathematical work is now indisputable', but they 'have nothing to do with self-evident propositions' (p. 100). More could be said

[20]For more discussion of this case, and its analogies and disanalogies with set theory, see [5].

here, for example, contrasting the way one set of structural axioms—the definition of group—is used to bring out similarities between mathematical structures that otherwise seem quite dissimilar, while another—the definition of topological space—is used to isolate invariant properties, and so on. Each case in which the mathematical community successfully settles on the 'right' definition of some concept shows once again how a concept was molded to meet certain mathematical needs. Following Feferman's line of thought, we can fully agree that intrinsic considerations are largely irrelevant in such cases. But, Feferman continues, the axioms of set theory are different; they aren't structural, but foundational, and he suggests, they are needed 'to justify' mathematical practice 'in the end' (p. 100). He then proceeds with the analysis sketched above, in which ZFC comes up short in the matter of intrinsic justifications.

All this strongly suggests that Feferman regards intrinsic justifications as essential and extrinsic justifications as inappropriate in the case of foundational axioms like those of set theory. Now I fully agree that the set theoretic axioms are (at least partly) aimed at satisfying a foundational goal; as a naturalist I would argue that an appreciation of this foundational goal helps us see why certain widespread set theoretic methods are rational. The irony is that I see the effectiveness of an axiom candidate at helping set theoretic practice reach its foundational goal as a sound *extrinsic* reason to adopt it as a new axiom! Given that Feferman sees the foundational goal as requiring intrinsic justification, there must be some stark difference in our understandings of the foundational goal itself.

In fact, I think that difference lies fairly close to the surface. My own understanding is that set theory seeks to provide a unified arena in which set theoretic surrogates for all classical mathematical objects can be found and the classical theorems about these objects can be proved. This sort of foundation brings the various structures of mathematics onto one stage, where they can be contrasted and compared; it provides a uniform answer to questions of mathematical existence and proof. I trust there is no disagreement that these services are mathematically valuable. But set theoretic foundations in this sense do not do two of the things that earlier thinkers had hoped for: they do not reveal what mathematical entities *really* are, in some deep metaphysical sense; and, more to the point for our present purposes, they do not provide an epistemic foundation; they do not show us how to derive the various truths of mathematics by transparent steps from absolutely certain truths. This goal, it seems to me, is one that the development of mathematics has forced us, however reluctantly, to abandon.

But suppose you have not surrendered this goal. Then it might make perfect sense to insist that foundational axioms be conceptual truths of some sort or other, and thus self-evident and absolutely certain. If you insist on beginning from indubitable premises, as a true epistemic foundationalist must, you simply can't allow extrinsic justifications of any kind.

My suggestion, then, is that the fundamental difference between Feferman and my naturalist, the difference that leads to their disagreement over the legitimacy of extrinsic justifications in set theory, and from there to a disagreement over the viability of the search for new set theoretic axioms, is simply this: Feferman cleaves to a foundational goal that the naturalist regards as outmoded. On this point, I leave you to your consciences.

COMMENTS AND RESPONSES

Comments on Friedman and Steel. Hoping it might clarify the issues at stake, I'd like to take a moment to lay out what I take to be the disagreement between Steel and Friedman on our title question. It begins:

STEEL: Of course mathematics needs new axioms. It needs them to settle the outstanding questions of descriptive set theory, and it needs them if there's to be any hope of settling the CH.

FRIEDMAN: Given the goals of contemporary set theory, this is a perfectly rational answer.

The naturalist might wonder why this isn't the end of the story, but Friedman has more to say:

FRIEDMAN: But the goals of set theory are very different from those of core mathematics.

STEEL: So what? The algebraist's goals are different from those of the topologist. Both differ from the goals of the number theorist or the geometer. We should expect that the set theorist would have his own goals.

FRIEDMAN: In the cases you cite, the various parties respect the others' goals, even if they don't share them. But the core mathematician thinks the set theorist's goals are outright wrong, not just different. In particular, the core mathematician thinks the set theorist is too concerned with generality, with pathologies, etc.

STEEL: But surely these are irrational prejudices on the part of the core mathematician. We should try to educate him.

At this point, I'm not sure which of two tacks best represents Friedman's response. Perhaps it is some amalgam of the two.

FRIEDMAN NUMBER ONE: No, the core mathematician's reasons for rejecting the set theorist's goals are perfectly rational. They can only be countered by some development like my Boolean relation theory, which will show the core mathematician that he needs set theory to attain his own goals.

FRIEDMAN NUMBER TWO: Even if the core mathematician is not rational in his rejection of set theoretic goals, even if his objections are mere prejudices, the set theoretic community needs to counter them with something like Boolean relation theory. If it doesn't, then as a matter of sociological fact, set theory will go unsupported and eventually die out.

Notice that on either interpretation, Friedman is not arguing that the set theorist is irrational to carry on as he does, given the goals of his practice. Rather, Friedman is arguing that the set theorist should modify his goals, either for rational or for practical reasons.

Comments on Steel. Steel suggests that philosophy may 'have a more active role to play' than the naturalist allows, in particular, that philosophy may be needed to spell out what counts as a solution to the Continuum Problem. Perhaps. But it seems to me that Steel has masterfully outlined, as only a discerning expert can, the purely mathematical reasons why adding many Woodins (and hence PD) to ZFC counts as a solution to the old problems of descriptive set theory, [21] and that he's given us a tantalizing vision of purely mathematical developments that might one day qualify as a solution to the continuum problem. It's hard to see what persuasive force philosophy could add to this appealing (and purely naturalistic) picture. I fear Steel is too modest!

Comments on Feferman. Feferman has raised several concerns about my naturalism,[22] perhaps the most troublesome of which are the suggestions that it is 'unduly transitory' or 'relativistic' or 'parochial'.[23] Of course it is true that our rational judgment of which mathematical principles and methods are best will change as we learn more: we have less reason now to embrace infinitesimals than we did before Cauchy and Weierstrass; we have more reason now to embrace large cardinals than we did, for example, when Ulam first defined measurables in 1930. I don't see why this is any more alarming than the fact that scientists have more reason now to believe in atoms than they did before Einstein and Perrin; we learn new things, acquire new evidence, modify our theories, in both mathematics and science. So naturalistic justifications will shift as our understanding increases, but I

[21]The words 'true' and 'believe' crop up occasionally in Steel's discussion, but I can't see that they do any serious philosophical work. In practice, he gives deflationary readings of such terms, e.g., 'to believe that there are measurable cardinals is to seek to naturally interpret all mathematical theories of sets, to the extent that they have natural interpretations, in extensions of $ZFC+$ "there is a measurable cardinal"'. (The discussion referenced in footnote 26 concerns another such deflationary reading.)

[22]Feferman is right to note that my naturalism differs in some striking ways from Quine's. Those differences, and my reasons for continuing to use the word 'naturalism', are detailed in [4]. (Those with further interest in this version of naturalism might also see [5], [6], and [7].

[23]The last two descriptions come from an earlier version of Feferman's remarks, which was subsequently shortened for purposes of publication.

don't think this makes those justifications any more 'unduly transitory' than our scientific theories.

Naturalism is 'relativistic', in the sense that the justification for a given mathematical method is given in terms of the goals of the practice in which it is embedded. Certainly it is true, as Friedman has emphasized, that different groups of mathematicians will share different goals: a topologist hopes to isolate invariants; an algebraist hopes to uncover hidden similarities between widely varying structures; a set theorist hopes to provide a broad, rigorous foundation for classical mathematics. And it is, as Feferman notes, a delicate task to understand the implicit goals of a given practice.[24] But this doesn't mean that anything goes (e.g., early infinitesimals offended against the overarching mathematical goal of *consistency*)[25] or that there is no such thing as justifying a method in such terms (e.g., the rejection of V=L might be justified in terms of the *foundational* goal of set theory).

Obviously, the constructivist has different mathematical goals from those of the set theorist, goals in terms of which he adjudicates between various methods for his practice (e.g., rejecting proof by reductio ad absurdum). Analysis of these goals and justifications falls squarely within the naturalist's field of study; naturalism is not parochial to set theory, though set theory has been my own focus. What falls outside naturalistic scruples is philosophical incursions into mathematics (e.g., rejection of classical methods for reasons of a priori semantic theorizing, as in Dummett).

Finally, one point of clarification: the naturalistic methodologist takes no stand on whether or not either the CH or the questions of descriptive set theory 'have determinate answers' despite their independence.[26] The difference between the two is simply that we have attractive axiom candidates—attractive when viewed in terms of the goals of the practice—that settle the questions of description set theory, and we don't have such candidates in the case of CH. If there is a slippery slope here, as Feferman fears, the naturalist doesn't climb it in the first place.

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[25]Part of an extended naturalistic analysis of this episode would most likely reveal how the general goal of *consistency* was not enough to swamp the goal of providing mathematical tools for a wide range of scientific applications. But *consistency* remained a goal of the mathematical practice, and was eventually reached, without sacrificing the others.

[26]For discussion, see [4], pp. 194-196, 211-212.

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MATHEMATICS NEEDS NEW AXIOMS

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§1. Definitions. Let me begin by clarifying the question.

By *new* I shall simply mean: not a consequence of ZFC. This is of course somewhat arbitrary. If one takes a long view, the full strength of ZFC appears to be quite new; on the other hand, to a modern set theorist, large cardinal hypotheses going well beyond ZFC are rather old hat. Some of the weaker large cardinal hypotheses were actually discovered and studied before ZFC itself was fully isolated.

By *axiom* I shall mean: assumption to be adopted by all, as part of a broadest point of view. The "broadest point of view" proviso is meant to exclude from attention the temporary adoption of restrictive assumptions as a convenient device for avoiding irrelevant structure. $\mathsf{V} = \mathsf{L}$ is often assumed temporarily for such reasons by set theorists who do not believe it, just as "all functions are C^∞" is sometimes assumed by differential geometers who do not believe it.

The old self-evidence requirement on axioms is too subjective, and more importantly, too limiting. In the future, what "forces itself upon us as true" is more likely to be a theory as a whole, and the process is more likely to be gradual. We may very well never reach the level of confidence in the new theory that we have in, say, Peano Arithmetic. Nevertheless, new axioms may emerge, and be rationally justified. The self-evidence requirement would block this kind of progress toward a stronger foundation.

What *is* important is just that our axioms be true, and as strong as possible. Let me expand on the strength demand, as it is a fundamental motivation in the search for new axioms.

It is a familiar but remarkable fact that all mathematical language can be translated into the language of set theory, and all theorems of “ordinary” mathematics can be proved in ZFC. In extending ZFC, we are attempting to strengthen this foundation. Surely strength is better than weakness! Professor Maddy has labelled the “stronger is better” rule of thumb *maximize*, and discussed it at some length in her recent book [4]. I would say that what we are attempting to maximize here is the *interpretative power* of our set theory. In this view, to believe that there are measurable cardinals is to seek to naturally interpret all mathematical theories of sets, to the extent that they have natural interpretations, in extensions of ZFC + “there is a measurable cardinal”.

Maximizing interpretative power entails maximizing formal consistency strength, but the converse is not true, as we want our interpretations to preserve meaning.

In this light we can see why most set theorists reject $\mathsf{V} = \mathsf{L}$ as restrictive: adopting it restricts the interpretative power of the language of set theory. The language of set theory as used by the believer in $\mathsf{V} = \mathsf{L}$ can certainly be translated into the language of set theory as used by the believer in measurable cardinals, via the translation $\varphi \mapsto \varphi^L$. There is no translation in the other direction. While it is true that adopting $\mathsf{V} = \mathsf{L}$ enables one to settle new formal sentences, this is in fact a completely sterile move, because one settles φ by giving it the same interpretation as φ^L, which can be settled in anyone’s theory. If by “question” we mean interpreted formal sentence, adopting $\mathsf{V} = \mathsf{L}$ *settles no new questions*. It simply prevents us from asking as many questions, since we are then forbidden to ask about the world outside L.

Of course, the Axiom of Foundation also has the appearance of being restrictive. However, so far as we know, it is not in reality. We know of no mathematical structure outside the class of wellfounded sets. On the other hand, we know of plenty of interesting structure outside L.

It is sometimes maintained that one could obtain all the logical strength of measurable cardinals in an extension of $\mathsf{V} = \mathsf{L}$ by adopting $\mathsf{ZFC} + \mathsf{V} = \mathsf{L}$ + “there is a transitive model of ZFC + “there are measurable cardinals”. This is an example of what I would call the *instrumentalist dodge*. For any theory T and class of sentences Γ, the instrumentalist version of T over Γ, or $\mathrm{Inst}(T, \Gamma)$, is the theory: All theorems of T in Γ are true.[27] Thus

$$\mathrm{Inst}(T, \Pi^0_1) \equiv \mathrm{Con}(T),$$
$$\mathrm{Inst}(T, \Pi^0_2) \equiv 1\text{-}\mathrm{Con}(T).$$

[27] I shall generally use “theory” to mean “axiomatizable theory in the language of set theory extending ZFC”. It might be slightly more natural to let $\mathrm{Inst}(T, \Gamma)$ be axiomatized by the schema “$(F \vdash \varphi) \to \varphi$”, for $\varphi \in \Gamma$ and finite $F \subseteq T$, since the latter theory has precisely the same Γ-consequences as T.

One could obtain all the Σ^1_2 consequences of measurables in

$$\mathsf{ZFC} + \mathsf{V} = \mathsf{L} + \mathrm{Inst}(\text{There are measurables}, \Sigma^1_2).$$

(This is close to the theory $\mathsf{ZFC} + \mathsf{V} = \mathsf{L}$ + "there is a transitive model with a measurable cardinal", although the latter is slightly stronger.) One could go even further, eliminating not just all the non-constructible sets, but also all the infinite sets, and nevertheless obtain all the Π^0_1 consequences of measurables in

$$\mathrm{PA} + \mathrm{Inst}(\text{There are measurables}, \Pi^0_1).$$

There are endless variations here. The theory $\mathrm{Inst}(T, \Gamma)$, used simply as a device to avoid directly asserting T while retaining all its Γ-predictions, is a mathematical parallel of the physical theory: "There are no electrons, but mid-size objects behave as if there were." That is, it is a parallel of this theory as it might be used by a philosopher of today, not as it might have been used by a physicist in 1900. Perhaps a cleaner parallel would be the theory that the world popped into existence 5 minutes ago, looking exactly as it would if there had been a past like the one we believe in. Mis-used in this way, $\mathrm{Inst}(T, \Gamma)$ is no more than an odd way of asserting T; it only becomes more than that if one has a program for finding a tool for making Γ predictions which is better than T, and incompatible with T in the realm of non-Γ sentences. In evaluating a retreat from T to $\mathrm{Inst}(T, \Gamma)$, one should ask whether its proponent has any such competing tool in mind.[28]

Finally, let me make a few remarks regarding *mathematics* and *need*. First, it is true that most, if not all, of the mathematics applied in Physics, Chemistry, and the other sciences requires much less than ZFC. On the other hand, Gödel's incompleteness theorems guarantee this will never be more than a report of the current state of affairs, for Con(ZFC) does lead to predictions about the physical world (such as that no contradiction will be proved from ZFC in the next 20 years) which we do not know how to derive using only ZFC. Second, it is true that most, if not all, of the most active areas in pure mathematics require much less than ZFC. Once again, Gödel's theorems guarantee this will never be more than a description of the current state of affairs.

Professor Feferman has stated his belief that the famous open problems of number theory, such as Goldbach's conjecture or the Riemann Hypothesis, can be settled in ZFC. I agree that this is probably the case. However, we should recognize that until the problems are actually settled this will almost certainly never be more than an educated guess.[29] If our educated guesses

[28] I have included this brief discussion of instrumentalism because Professor Feferman sometimes makes such a move. His class Γ of meaningful sentences does not seem to go much further than the language of Peano Arithmetic. See for example [2], page 73.

[29] There is the very remote possibility that one could show ZFC settles the questions without actually exhibiting the relevant ZFC-proofs. Goldbach's conjecture and the Riemann

as to what techniques will be useful in solving a given problem were highly reliable, mathematics would be less interesting than it is.

Of course, the permanent possibility that new axioms will be needed is not the same as the reality of such a need. In this connection, we do know that many natural, well known problems in the more theoretical parts of pure mathematics, such as the Continuum Problem or Suslin's Problem, demonstrably require new axioms for their solution. The most concrete of these are the problems about projective sets of reals from classical descriptive set theory. These arose in the early 1900's, in the work of Analysts like Lebesgue and Luzin who were interested in the foundations of their subject. In what follows I shall sketch some reasons for believing that we do have the new axioms we need to settle all the classical questions about projective sets. It is worth noting that Lebesgue and Luzin would probably have been just as surprised to find that large cardinal axioms are useful in the theory of projective sets as modern mathematicians would be to find that they are useful in number theory.

Does mathematics really need this theory of projective sets we get from large cardinal axioms? Does it need a decision on the Continuum Problem? Let me just say that I prefer a different formulation of our question, namely:

> *Is the search for, and study of, new axioms worthwhile? Should people be working in this direction?*

It seems to me that this gets more directly at the practical question, and puts it in a more positive way. It doesn't matter so much whether we can make do without this line of research; the more important question is whether we are better off with it. And it doesn't matter so much whether we call the subject which is better off (ordinary, core, normal) mathematics, or something else. It is this reformulated question which I shall now discuss.

§**2. Some results in Gödel's program.** One of the chief inspirations of the line of research we are discussing is Gödel's paper [1], and so it is often called *Gödel's program*. How should we evaluate this program? As with any line of research, we should look at what has been done, and what's left to do. Unfortunately, we are discussing at least 35 years of continuous work by a reasonably large number of people. I shall settle, then, for a brief enumeration of some of the main themes.

2.1. We have found natural new axioms, the large cardinal axioms. These are strengthenings of the axiom of infinity of ZFC. They are expressions of an intrinsically plausible informal reflection principle.

2.2. These axioms have proved crucial to organizing and understanding the family of possible extensions of ZFC. Of course, there is nothing like a

Hypothesis are Π^0_1 statements, so one cannot prove them independent of ZFC without also proving them.

systematic classification of all the possible extensions of ZFC, but there is more order here than one might suspect:

(1) Many natural extensions T of ZFC have been shown to be consistent relative to some large cardinal hypothesis H, via the method of forcing. This method is so powerful that, at the moment, we know of no interesting T extending ZFC which seems unlikely to be provably consistent relative to some large cardinal hypothesis via forcing. Thus, at least if we allow "Boolean-valued" interpretations, the extensions of ZFC via large cardinal hypotheses seem to be cofinal in the part of the interpretability order on all natural extensions of ZFC which we know about.

(2) Often, it has been shown that the consistency of the large cardinal hypothesis H must be assumed, in that $\mathrm{Con}(T)$ implies $\mathrm{Con}(H)$. This involves constructing a canonical inner model for H. These canonical inner models admit a systematic, detailed, "fine structure theory" much like Jensen's theory of L. Such a thorough and detailed description of what a universe satisfying H might look like also provides good evidence that H is indeed consistent.[30]

Here are some examples of relative consistency results obtained by these methods. The first four are equiconsistencies.[31]

$$\mathrm{Con}(\mathsf{ZF} + \text{All sets Lebesgue measurable})$$
$$\leftrightarrow \mathrm{Con}(\mathsf{ZFC} + \text{There is an inaccessible}),$$
$$\mathrm{Con}(\mathsf{ZFC} + \text{There is a total extension of Lebesgue measure}$$
$$\leftrightarrow \mathrm{Con}(\mathsf{ZFC} + \text{There is a measurable}),$$
$$\mathrm{Con}(\mathsf{ZFC} + \mathsf{GCH} \text{ first fails at } \aleph_\omega)$$
$$\leftrightarrow \mathrm{Con}(\mathsf{ZFC} + \text{There is a measurable } \kappa \text{ of order } \kappa^{++}),$$
$$\mathrm{Con}(\mathsf{ZFC} + \text{All games in } L(\mathbb{R}) \text{ are determined})$$
$$\leftrightarrow \mathrm{Con}(\mathsf{ZFC} + \text{There are infinitely many Woodin cardinals}),$$
$$\mathrm{Con}(\mathsf{ZFC} + \text{There is a supercompact cardinal})$$
$$\rightarrow \mathrm{Con}(\mathsf{ZFC} + \text{Proper forcing axiom})$$
$$\rightarrow \forall n < \omega \ \ \mathrm{Con}(\mathsf{ZFC} + \text{There are } n \text{ Woodin cardinals}).$$

Of course, all the implications displayed are provable in Peano Arithmetic. Concerning the last set of results, it is generally believed that the Proper Forcing Axiom (PFA) is equiconsistent with the existence of supercompact cardinals. We do not have a consistency strength lower bound on PFA better than the one displayed because our tool for producing such lower bounds,

[30]The introduction to [7] contains an essay on the inner model program.

[31]These results are due to Baumgartner, Gitik, Mitchell, Schimmerling, Shelah, Solovay, Steel, Todorcevic, and Woodin, building on earlier work of Dodd, Jensen, Kunen, Magidor, Martin, Prikry, Silver, and others.

the construction of canonical inner models for large cardinal hypotheses, does not yet produce models with more than small numbers of Woodin cardinals. Extending inner model theory so that it can produce inner models with supercompact cardinals is one of the most important open problems in Gödel's program.

2.3. The pattern above extends to many more examples. It seems that every natural extension of ZFC is equiconsistent with an extension axiomatized by something like large cardinal axioms! If S and T are extensions of ZFC by large cardinal axioms, then it is generally easy to compare the consistency strengths of S and T; moreover, the consistency strengths of large cardinal extensions of ZFC fall into a wellordered hierarchy. Thus it seems that the consistency strengths of all natural extensions of ZFC are wellordered, and the large cardinal hierarchy provides a sort of yardstick which enables us to compare these consistency strengths.[32]

The ordering of consistency strengths corresponds to the inclusion order on the sets of Π^0_1 (or in fact arithmetical, or even Σ^1_2) consequences of the theories in question. That is, for any class Γ of sentences and any theory T, let

$$(\Gamma)_T = \{\, \varphi \in \Gamma \mid T \vdash \varphi \,\}.$$

The linearity of the ordering of natural consistency strengths means that for natural S and T extending ZFC,

$$(\Pi^0_1)_S \subseteq (\Pi^0_1)_T \quad \text{or} \quad (\Pi^0_1)_T \subseteq (\Pi^0_1)_S.$$

The fact that our relative consistency proofs actually produce reasonable interpretations, and in particular, wellfounded models, means that for natural S and T

$$(\Pi^0_1)_S \subseteq (\Pi^0_1)_T \leftrightarrow (\Sigma^1_2)_S \subseteq (\Sigma^1_2)_T.$$

Thus, at the level of Σ^1_2 sentences, we know of only one road upward, and large cardinal hypotheses are its central markers. Does this road lead to Σ^1_2 truth? It is certainly our best guess at the moment; moreover, there is no hint of a competing guess.

2.4. The phenomenon described in the last item extends to all sentences in the language of second order arithmetic, provided we restrict ourselves to consistency strengths which are sufficiently great. More precisely, for S and T natural theories of consistency strength at least that of "There are n Woodin cardinals with a measurable above them all", we have

$$(\Pi^0_1)_S \subseteq (\Pi^0_1)_T \leftrightarrow (\Sigma^1_{n+2})_S \subseteq (\Sigma^1_{n+2})_T.$$

[32] We know of no way to compare the consistency strengths of PFA and the existence of a total extension of Lebesgue measure except to relate each to the large cardinal hierarchy. The same is true for most other comparisons: the large cardinal hierarchy is essential.

Closely related to this is the phenomenon that any natural theory of consistency strength at least that of PD[33] actually implies PD. For example, the Proper Forcing Axiom implies PD. So does the existence of a homogeneous saturated ideal on ω_1. (Neither of these propositions has anything to do with PD on its surface.)

Thus, at the level of sentences in the language of second order arithmetic, we know of only one road upward. Large cardinal hypotheses are its central markers; moreover, it goes through PD. We are led to this road in many different ways. Along it lies our best guess at the truth for sentences in the language of second order arithmetic, and we have no hint of a reasonable competing guess.

2.5. The relationship between large cardinal hypotheses and PD is explained by the following theorem.[34]

THEOREM 2.1 (Martin, Steel, Woodin). *The following are equivalent*:

(1) PD,
(2) *For all* $n < \omega$, *every* Σ^1_n *consequence of* ZFC + *"there are n Woodin cardinals" is true.*

According to this theorem, PD is just the "instrumentalist's trace" of Woodin cardinals in the language of second order arithmetic.

2.6. The theory in the language of second order arithmetic based on large cardinal axioms contains answers to all the questions about projective sets from classical descriptive set theory. The theory of projective sets one gets this way extends in a natural way the theory of low-level projective sets developed by the classical descriptive set theorists using only ZFC; indeed, in retrospect, much of the classical theory can be seen as based on open determinacy, which is provable in ZFC. Virtually nothing about sets in the projective hierarchy beyond the first few levels can be decided in ZFC alone, but large cardinal hypotheses, via PD, yield a deep and powerful extension of the classical theory to the full projective hierarchy. (Kechris, Martin, Moschovakis, and Solovay are among the principal architects of this theory.) By placing the classical theory in this broader context, we have understood it better.

In this realm of evidence through consequences, I would like to mention a class of examples pointed out by D. A. Martin in [5]. Namely, some theorems are first proved under a large cardinal hypothesis, and then later the proof is refined or a new proof given in such a way as to use only ZFC. Borel determinacy is a prime example of this: originally it was proved assuming there are measurable cardinals, and then later a more complicated proof

[33] PD is the assertion that all projective games are determined.

[34] The breakthrough results of Foreman, Magidor, and Shelah [3], and then Shelah and Woodin [8], were a crucial step toward this theorem. See the introduction to [6] for a historical essay.

which uses only ZFC was found.[35] Martin points out other instances of this phenomenon, for example some cases of his cone theorem for the Turing degrees which can be proved by direct constructions. Another nice example down lower is Borel Wadge determinacy. This is an immediate consequence of Borel determinacy, but by work of Louveau and Saint Raymond, it is much weaker—in fact, provable in second order arithmetic. There are interesting potential instances of this phenomenon in which we have a proof from a large cardinal hypothesis, and at least some people suspect there is a proof in ZFC, but that proof is yet to be discovered. The point here is that the use of large cardinals to provide proofs of statements which can then be proved true more laboriously by elementary means constitutes evidence for large cardinals.[36]

It is sometimes claimed that there is an alternate, equally good, theory of projective sets which we get from $\mathsf{V} = \mathsf{L}$. There are two replies here.

First, the central idea of descriptive set theory is that definable sets of reals are free from the pathologies one gets from a wellorder of the reals. Since $\mathsf{V} = \mathsf{L}$ implies there is a Δ^1_2 wellorder of the reals, under $\mathsf{V} = \mathsf{L}$ this central idea collapses low in the projective hierarchy, and after that there is, in an important sense, *no* descriptive set theory. One has instead infinitary combinatorics on $\aleph_1$. This is certainly not the sort of theory that looks useful to Analysts.

More importantly, even if there were a wonderful, useful theory of projective sets based on $\mathsf{V} = \mathsf{L}$, adopting large cardinal axioms would in no way eliminate or devalue it. For the believer in large cardinals, this theory would make perfect sense as a wonderful, useful part of the first-order theory of L. On the other hand, the believer in $\mathsf{V} = \mathsf{L}$ cannot give the appropriate sense to the theory of projective sets we get from PD. (He may resort to some version of the instrumentalist dodge, but that amounts to uttering the words "$\mathsf{V} = \mathsf{L}$" while acting as if you believe something else.) Adopting $\mathsf{V} = \mathsf{L}$ brings with it a loss in this situation. Adopting measurable cardinals gives us $0^\sharp$, and with that, a much clearer view of L than we get if we are only allowed to look at it "from inside".

2.7. Large cardinal axioms seem to decide all natural questions in the language of second order arithmetic. There is metamathematical evidence of this completeness in the fact that no sentence in the language of second

[35] By Martin. This proof uses the "small cardinal hypothesis" that the power set operation can be iterated $\aleph_1$ times. H. Friedman showed that such a hypothesis is needed for the proof. Borel determinacy is a natural statement in the language of second order arithmetic which cannot be proved without appealing to arbitrary sets of reals, sets of sets of reals, etc.

[36] Gödel's result on speeding up proofs shows that some form of this phenomenon occurs whenever one increases consistency strength, but it does not guarantee that any mathematically interesting theorems have shorter proofs in the stronger system.

order arithmetic can be shown independent of existence of arbitrarily large Woodin cardinals by forcing:[37]

THEOREM 2.2 (Woodin). *Suppose there is an iterable inner model satisfying "there are ω Woodin cardinals"; then if M and N are set-generic extensions of V, we have*

$$L(\mathbb{R})^M \equiv L(\mathbb{R})^N.$$

There is only one theory with this kind of "generic completeness":

THEOREM 2.3 (Woodin). *Suppose that whenever M and N are set generic extensions of V, we have $L(\mathbb{R})^M \equiv L(\mathbb{R})^N$; then there is an iterable inner model satisfying "there are ω Woodin cardinals".*

2.8. If we regard Theorem 2.2 as a key indicator of the completeness of large cardinal axioms for sentences in the language of second order arithmetic, then it is natural to ask whether this kind of completeness extends to the language of third order arithmetic. There we immediately encounter CH, which is a Σ^2_1 statement. None of our current large cardinal axioms decide CH, and consequently they do not yield a theory which is generically absolute at the Σ^2_1 level:

THEOREM 2.4 (Levy, Solovay).

(1) *None of the current large cardinal axioms decides* CH.
(2) *Let A be one of the current large cardinal axioms, and suppose $V \models A$. Then there are set generic extensions M and N of V which satisfy A, but are not Σ^2_1-equivalent.*

Nevertheless, there may be some natural extrapolation of our current large cardinal axioms, some natural markers of still higher consistency strengths, which yield a "complete" theory in the language of third order arithmetic, with an accompanying generic absoluteness theorem. A second, better delineated alternative is an extension of our current large cardinal axioms which does not increase consistency strength, with an accompanying *conditional* generic absoluteness theorem for sentences in the language of third order arithmetic. Indeed, Woodin has shown that simply adding CH gives us such a theory at the Σ^2_1 level:

THEOREM 2.5 (Woodin). *Suppose $V \models$ "There are arbitrarily large measurable Woodin cardinals". Let M and N be set-generic extensions of V satisfying* CH; *then M and N are Σ^2_1-equivalent.*

It is open whether if $V \models$ "There are arbitrarily large supercompact cardinals", and M and N are set-generic extensions of V satisfying $\diamondsuit$, then M and N are Σ^2_2-equivalent. If so, this would indicate that in the presence

[37]The hypothesis of Theorem 2.2 follows from the existence of arbitrarily large Woodin cardinals. Woodin and the author noticed independently that the weaker hypothesis used here suffices. I have stated 2.2 this way in order to point out its converse. The converse is closely related to work of the author.

of large cardinal hypotheses, $\diamondsuit$ yields a complete theory at the Σ^2_2 level, just as CH does at the Σ^2_1 level.

2.9. There are many interesting open problems bearing on Gödel's program. Here are two which seem particularly important to me.

(1) Develop a theory of canonical inner models satisfying "There is a supercompact cardinal".
(2) Find conditional generic absoluteness theorems at the Σ^2_n level, for all $n < \omega$.

COMMENTS AND RESPONSES

Reply to Feferman. Feferman states his belief that CH "does not express a definite proposition", that it is "inherently vague". The trouble is supposed to be that the set of all reals is not a "definite mathematical object". But if that is the trouble, it would seem that "there is a set of all real numbers" does not express a definite proposition; indeed, mathematics would seem to be shot through with vague statements. Worse than that, with *inherently* vague statements, so that there is no hope of rectifying the situation, except, I guess, by chucking the whole mess. Taken seriously, this analysis leads us into a retreat to some much weaker constructivist language, a retreat which would toss out good mathematics in order to save inherently vague philosophy. Feferman himself does not seem to take that route; instead, he adopts an instrumentalist stance, according to which higher set theory has no definite meaning, but somehow has value nevertheless.[38]

Feferman's instrumentalism comes to the fore in the following passage:

> My point here is simply that there is a basic difference between accepting systems such as $\mathrm{ZFC} + \mathrm{LCA}$, where LCA is the applicable large cardinal axioms, and accepting $1\text{-Con}(\mathrm{ZFC} + \mathrm{LCA})$. As to the question of the need of large cardinal assumptions to settle finite combinatorial problems of the sort produced by Friedman, there is thus, in my view, an equivocation between needing a given axiom and needing its 1-consistency; it is only the latter that is demonstrated by his work.

To this I would reply: there certainly *could* be a basic difference between accepting LCA and accepting $1\text{-Con}(\mathrm{LCA})$, but except for the fact that he witholds the honorific "first-class mathematical principle" in the case of LCA, Feferman hasn't told us what the practical, behavioral content of the difference would be in his case. He proposes no tools for generating Π^0_2 truths which are not subsumed under some natural interpretation by axioms of the form LCA, and indeed, as I have emphasized, there is presently no

[38] I refer here to what Feferman calls his pragmatic instrumentalism. Hilbert-style reductions to constructivist systems are not a dodge, they are a retreat which tosses out too much good mathematics.

hint of such a tool. The suggestion that we might retreat from LCA to Inst(LCA, Π^0_2), which is at least implicit in this passage, is an empty one, as far as I can tell. No one's mathematical behavior would change.

Feferman goes on to say:

> But if one does not grant that there is a fact of the matter whether statements LCA of various large cardinal axioms are true, is there a principled reason for accepting 1-Con(ZFC + LCA) without accepting ZFC + LCA itself?

Feferman's answer seems to be "no", although he has suggested ([2], p. 73) that he believes ZFC itself is 1-consistent, presumably for unprincipled reasons. But if 1-Con(ZFC + Mahlos) is simply an open problem, how does Feferman propose we go about deciding it? Does the theory I have described count as positive evidence? Should we look in some other direction for a solution? Feferman does not answer these questions, except to say that they are not important to most mathematicians.

There may be something to the idea that the language of third order arithmetic is vague, but the suggestion that it is inherently so is a gratuitous counsel of despair. If the language of 3rd order arithmetic permits vague or ambiguous sentences, then it is important to trim or sharpen it so as to eliminate these. This will likely involve mathematical and metamathematical investigations like those indicated above. It is hard to distinguish sharpening the meaning of our language from discovering new truths, but it may be that, in the end, our solution to the Continuum Problem is best seen as resolving some ambiguity. That might be the case, for example, if we were to find conditional generic absoluteness theorems for mutually incompatible theories, each of them consistent with all the large cardinal axioms; it might then be appropriate to regard the decision to adopt one of these theories as analogous to the decision to speak one of some family of intertranslatable languages. At the moment, however, these are simply speculations, and the most important point is just that further investigation is needed.[39]

Feferman argues that the fact that the Continuum Problem doesn't belong on the Millenium Prize list indicates that CH does not express a definite proposition. But of course, Con(supercompacts) doesn't belong there either, although only a hard-core formalist would deny that it expresses a definite proposition. The true reason neither problem belongs on the list is that neither is likely to be solved in one controversy-free swoop. Both involve basic conceptual issues related to mathematical evidence. To one interested in the foundations of mathematics, that makes these problems more interesting

[39] In his argument that the concept of an arbitrary set of reals is inherently vague, Feferman likens it to the "concept" of a feasible number. This analogy is far-fetched at best. The concept of an arbitrary set of reals is the foundation for a great deal of mathematics, and has never led into contradiction. The first two things of a general nature one is inclined to say about feasible numbers will contradict each other.

than many of the Millenium problems. It would be a shame if logicians, who are specially equipped to deal with conceptual, foundational issues, were to shy away from the Continuum Problem because it involves such issues.

Reply to Maddy. I share the Naturalist's reluctance to trim mathematics in order to make it fit some theory of mathematical knowledge. Nevertheless, a solution to the Continuum Problem may need some accompanying analysis of what it is to be a solution to the Continuum Problem, and in this way, Philosophy may have a more active role to play at the foundations of mathematics than Maddy envisions.

Reply to Friedman. Applications are important; the more widespread and concrete, the better. However, while there are basic issues left in pure large cardinal theory, we should continue to develop it, regardless of applications. This can only make eventual applications more likely. Friedman's own applications would never have been discovered by a combinatorist who knew nothing about large cardinals.

Evidence for the *truth* of the large cardinal axioms being applied comes from the theory I have sketched, not from their role in finitary combinatorics.

Concerning Friedman's suggestion that absolute applications are especially important, one should note that "absolute" is a relative term. We now have analogues of the Shoenfield Absoluteness Theorem at all levels of the projective hierarchy, and beyond.

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NORMAL MATHEMATICS WILL NEED NEW AXIOMS

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I begin with the text of my presentation to the ASL meeting in Urbana, June, 2000, as a member of the panel entitled "Does Mathematics Need New Axioms?" I have added footnotes which point to extended remarks that follow.

I include a brief account of the new Boolean relation theory (BRT), as well as the even newer reduced form of BRT called disjoint cover theory (DCT). These new theories have radically changed my perspective and outlook on the topic at hand. Much of the discussion is predicated on the expectation that the results are correct. Draft manuscripts exist containing proofs of key initial results in BRT and DCT, but I caution the reader that none of the proofs have yet been gone over by experts.

Text of presentation. The point of view of the set theory community is well represented here [on this panel]. I want to concentrate on the perspective of mathematicians outside set theory.

§1. Mathematicians' viewpoint. New axioms are needed in order to settle various mathematically natural questions. Yet no well known mathematicians outside set theory are even considering adopting any new axioms for mathematics, even though they are aware of at least the existence of the independence results.[40]

The difference in perspective, of set theorists versus mathematicians who are not set theorists, is enormous. Recall that mathematics goes back, say, 2,500 years—whereas set theory in the relevant sense dates back to the turn of the 20th century.

For 2,500 years, mathematicians have been concerned with matters of counting and geometry and physical notions. These main themes gave rise to arithmetic, algebra, geometry, and analysis.

The interest in and value of mathematics is judged by mathematicians in terms of its relevance to and impact on the main themes of mathematics.[41]

It is generally recognized by most mathematicians that set theory is the most convenient vehicle for achieving rigor in mathematics. For this purpose,

[40]A large segment of the algebraic geometry community, following Grothendieck, has already accepted and freely uses the relatively modest new axiom "there are arbitrarily large strongly inaccessible cardinals". They are fully aware that ZFC has been sufficient for their concrete purposes, but nontheless find it very convenient.

[41]The relevance and impact of a range of topics in discrete mathematics on the main themes of mathematics, both realized and potential, is now generally recognized.

there has evolved a more or less standard set theoretic interpretation of mathematics, with ZFC generally accepted as the current gold standard for rigor.[42]

It is simply false that number theorists are interested in and respect set theory just as they are interested in and respect group theory, topology, differential geometry, real and complex analysis, operators on Hilbert space, et cetera.

The reason for this attitude is quite fundamental and extremely important. A number theorist is of course interested in complex analysis because he uses it so much. But not so with operators on Hilbert space. Yet there is still a distant respect for this because of a web of substantive and varied interconnections that chain back to number theory. Set theory does not have comparable interconnections.

For the skeptical, the degree of extreme isolation can be subjected to various tests including citation references—broken down even into their nature and quality. Using a critical notion from statistics, set theory is an extreme outlier.

Nor is set theory regarded as intrinsically interesting to mathematicians, independent of its lack of impressive interconnections.[43] Why?

For the mathematician, set theory is regarded as a convenient way to provide an interpretation of mathematics that supports rigor. A natural number is obviously not a set, an ordered pair is obviously not a set, a function is obviously not a set of ordered pairs, and a real number is obviously not a set of rationals.

For the mathematician, mathematics is emphatically not a branch of set theory. The clean interpretation of mathematics into set theory does not commit the mathematician to viewing problems in set theory as problems in mathematics.

The mathematician therefore evaluates set theory in terms of how well it serves its purpose—providing a clean, simple, coherent, workable way to formalize mathematics.[44]

[42]Category theory is commonly used as a general framework for the presentation of a great deal of mathematics, where the categorical formulations are incomparably more attractive than direct set theoretic formulations. However, category theory does not (at this time) serve as a satisfactory autonomous foundation for mathematics as does set theory. Mathematicians prefer to think of category theory in set theoretic terms, where categories are set theoretic constructions.

[43]I leave open the possibility of mathematical areas which are regarded as intrinsically interesting to mathematicians, without impressive interconnections with other areas. We seem to be in a period where this is quite unusual, and certainly "advanced" set theory is not such an example.

[44]I mean here that there has now been an abandonment of interest in new results in the theory of sets, yet an acknowledgment of the power of set theory as a formalization of mathematics. This is in contrast with how, for instance, new results in number theory are evaluated.

This point of view hardened as many mathematicians experimented for several decades with what has come to be known as set theoretic problems which turned out to be independent of ZFC.[45]

There was a growing realization that the cause of these difficulties was excessive generality in the formulations of the problems which allowed for pathological cases which were radically different in character from normal mathematical examples. That if the problems were formulated in more concrete ways that still covered all known interesting cases, then the difficulties completely disappeared.[46]

Furthermore, distinctions between these set theoretic problems causing difficulties and the most celebrated theorems and open problems in mathematics can be given *formally*. This is in terms of quantifier complexity and the closely related matter of absoluteness. Thus set theory comes out as an extreme outlier which can be documented *formally*.[47]

§2. The maligned axiom of constructibility—more is less and less is more. The set theorist is looking for deep set theoretic phenomena, and so $V = L$ is anathema since it restricts the set theoretic universe so drastically that all sorts of phenomena are demonstrably not present. Furthermore, for the set theorist, any advantage that $V = L$ has in terms of power can be obtained with more powerful axioms of the same rough type that accommodate measurable cardinals and the like—e.g., $V = L(\mu)$, or the universe is a canonical inner model of a large cardinal.

However, for the normal mathematician, since set theory is merely a vehicle for interpreting mathematics so as to establish rigor, and not mathematically interesting in its own right, the less set theoretic difficulties and phenomena the better.[48]

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[45]There is an alternative cynical point of view which asserts that this viewpoint hardened against set theoretic problems or problems with a distinctly set theoretic flavor simply because of the presence of the independence phenomena, rather than the excessive generality issues as discussed in the next paragraph. I disagree.

[46]This is a major distinction between distinctly set theoretic problems and normal mathematics. Today, when a problem in normal mathematics is viewed as being difficult because its generality goes beyond all known interesting cases, the community loses interest, and attention is shifted towards less general formulations. However, an arbitrary integer or real number is not considered to be excessive generality.

[47]The absoluteness mentioned here is a byproduct of the low quantifier complexity. My impression is that the problems in, say, [1], [2] are easily seen to be provably equivalent to two quantifier sentences in the analytic hierarchy, and mostly one, or even arithmetical. A detailed survey and analysis along these lines is long overdue. Moreover, I suspect that even when a significant open problem is formulated in higher terms, its solution can be naturally separated into standard facts living at that higher level, together with new facts readily expressed at a lower level—often with at most three arithmetic quantifiers, and frequently one.

[48]For the set theorist, "the more set theoretic difficulties and phenomena the better."

I.e., less is more and more is less. So if mathematicians were concerned with the set theoretic independence results—and they generally are not—then $V = L$ is by far the most attractive solution for them.

This is because it appears to solve all set theoretic problems (except for those asserting the existence of sets of unrestricted cardinality), and is also demonstrably relatively consistent.

Set theorists also say that $V = L$ has implausible consequences—e.g., there is a PCA well ordering of the reals, or there are nonmeasurable PCA sets.

The set theorists claim to have a direct intuition which allows them to view these as so implausible that this provides "evidence" against $V = L$.[49]

However, mathematicians disclaim such direct intuition about complicated sets of reals. Some say they have no direct intuition about all multivariate functions from $\mathbb{N}$ into $\mathbb{N}$!

§3. Question answered by classical descriptive set theory? The classical descriptive set theory coming from large cardinals is most often cited by set theorists as the reason why mathematics needs large cardinal axioms. I have several objections to this claim.

(a) Part of the argument is that large cardinals are needed to establish these results. But large cardinals are not needed to establish an alternative series of such results. E.g., $V = L$ provides another, entirely different, set of answers to these questions. The set theorists answer saying $V = L$ gives the wrong answers and large cardinals give the right answers, citing their direct intuition about projective sets of reals. I am very dubious about this direct intuition. I don't have it, and mathematicians in general disclaim it.

(b) Another part of the argument is that, in light of (a),

set theory needs large cardinals

and therefore

mathematics needs large cardinals.

But this inference depends on a reading of our question that makes this tautological.

Reading the question this way simply avoids the really interesting questions, replacing them by much less interesting questions. For instance, it avoids questions of how and under what circumstances the general mathematical community or individual mathematicians will adopt new axioms, should adopt new axioms, and if so, how this will be manifested.

Here is the closest I can come to the set theorists' point of view on our question.

[49]Some set theorists have recognized the need to take a more subtle line and are experimenting with formal criteria such as generic absoluteness.

There is an interesting notion of "general set theory in its maximal conceivable form" and that $V = L$ has no basis in this context. However, the notion is at present virtually completely unexplained, and no work that I have seen provides any serious insight into what this really means. I simply do not know how to explicate any relevant notion of maximality.[50]

I agree that

> "general set theory in its maximal conceivable form" needs large cardinals axioms

is very likely to be true. But I can't conclude even that

> set theory needs large cardinal axioms

let alone

> mathematics needs large cardinal axioms.

§4. General predictions. The picture is going to change radically with the new Boolean relation theory (BRT) and related developments, joining the issue of new axioms and the relevance of large cardinals in a totally new and unexpectedly convincing way.

Because of the thematic nature of these developments, and the interaction with nearly all areas of mathematics, large cardinal axioms will begin to be accepted as new axioms for mathematics—with controversy. Use of them will still be noted, at least in passing, for quite some time, before full acceptance.[51]

§5. Circumstances surrounding actual adoption of new axioms. The circumstances that I envision are a coherent body of consequences of large cardinals of a new kind.

(a) They should be entirely mathematically natural. This standard is very high for a logician trying to uncover such consequences, yet is routinely met in mathematics (set theory included) by professionals at all levels of achievement.[52]

[50]The generic absoluteness approach in Theorems 2.2 and 2.3 of Steel's contribution is an interesting attempt in this direction. However, it has a way to go before being convincing as a formal analysis of "general set theory in its maximal conceivable form". In particular, it needs to be appropriately extended beyond $L(\mathbb{R})$. Also the use of generic extensions needs justification.

[51]BRT provides compelling uses for Mahlo cardinals of finite order. These are generally referred to as "small" large cardinals, which are compatible with $V = L$. I doubt whether BRT in its present form will tie up with larger cardinals. However, preliminary work indicates that a more expressive form of BRT does tie up with larger cardinals, although it has yet to be couched in the same kind of utterly elementary, natural, and familiar mathematical terms. I conjecture that these more expressive forms will tie up with the entire large cardinal hierarchy, and that it will ultimately be put into a form that satisfies criteria (a)–(g).

[52]The general form of BRT is very simple. It is based on multivariate functions, which should be set up with some care. They are taken to be pairs (f, k), where f is a function

(b) They should be concrete. At least within infinitary discrete mathematics.[53] Most ideally, involving polynomials with integer coefficients, or even finite functions on finite sets of integers.[54]

(c) They should be thematic. If they are isolated, they will surely be stamped as curiosities, and the math community will find a way to attack them through an ad hoc raising of the standards for being entirely natural. However, if they are truly thematic, then the theme itself must be attacked, which may be difficult to do. For instance, the same theme may already be inherent in well known basic, familiar, and useful facts.[55]

(as a set of ordered pairs), $k > 0$, and all elements of the domain of f are k-tuples. For $f = (f, k)$ and A, we define fA to be the set of all values of f at k-tuples from A. BRT is based on pairs (V, K), where V is a set of multivariate functions and K is a set of sets. In the critical case of two functions and three sets, BRT on (V, K) seeks to determine the truth or falsity of all statements of the form

for all $f, g \in V$ there exist $A, B, C \in K$ such that a given "Boolean relation" holds in $A, B, C, fA, fB, fC, gA, gB, gC$.

There are a number of relevant notions of "Boolean relation" among sets. Here I concentrate on equational BRT, where I use Boolean equations among sets. These are equations between Boolean terms, where the universal set U is taken to be the union of K.

[53]We have investigated equational BRT on (V, K) where V is the set of multivariate functions from Z into Z of expansive linear growth and K is the set of all infinite subsets of Z. Here f has expansive linear growth if and only if there exist rational constants $a, b > 1$ such that $a|x| \leq |f(x)| \leq b|x|$ holds for sufficiently large $|x|$, where $|x|$ is the sup norm of x. The main result is that for two functions and three sets, one of the 2^{512} instances is provably equivalent to 1-Con(MAH) over ACA_0. Here MAH $=$ ZFC $+$ (there exists an n-Mahlo cardinal)$_n$. Furthermore, an instance is presented in a particularly intelligible form. With $K' =$ the set of bi-infinite subsets of$Z$, the particular instance is yet simpler. The main conjecture is that all of the 2^{512} instances are provable in ACA_0, refutable in ACA_0, or provably equivalent to 1-Con(MAH) over ACA_0. In addition, I conjecture that whenever we can find nonempty finite $A, B, C \subseteq Z$, we can find $A, B, C \in K$,or even $A, B, C \in K'$. Significant partial results on these conjectures have been obtained. The latter conjecture implies 1-Con(MAH), and I conjecture that it is equivalent to 1-Con(MAH) over ACA_0.

[54]Preliminary indications are that I can instead use the integral piecewise linear functions (finitely many pieces) subject to the inequality

$$\text{for all } x \in \mathbb{Z}^k, |F(x)| \geq 2|x|,$$

where F is k-ary, and K or K' as before, and make the same claims and conjectures. Preliminary indications are that when such piecewise linear functions are used, the sets in K or K' can be represented as integral piecewise linear images of geometric progressions in N, where a triple exponential bound can be placed on this representation. This leads to a purely universal arithmetic sentence provably equivalent to Con(MAH) over exponential function arithmetic.

[55]Preliminary indications are as follows. For any interesting set V of multivariate functions—even consisting of just unary functions—and interesting set K of sets substantially related to V, BRT is interesting and delicate even for a single function and a single set, let alone two functions and three sets. Note that many areas of mathematics are closely associated with particular sets of multivariate functions and sets of related sets. As an indication of the power of the formalism, take V to be the set of all bounded linear operators

(d) They should have points of contact with a great variety of mathematics.

(e) They should be open ended. I.e., the pain will never end until the adoption of large cardinals.[56]

(f) They should be elementary. E.g., at the level of early undergraduate or gifted high school. That way, even scientists and engineers can relate to it, so it is harder for the math community to simply bury them.[57]

(g) Their derivations should be accessible, with identifiable general techniques. This way, the math community can readily immerse itself in hands on crystal clear uses of large cardinals that beg to be removed—but cannot.[58]

I have omitted an additional circumstance:

(h) They should be used in normal mathematics as pursued before such thematic results.

For some mathematicians, h will be required before they consider the issue really joined. I already know that for some well known core mathematicians,

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on Hilbert space (a set of unary functions). Take K to be the set of all nontrivial closed subspaces of Hilbert space. Then the particularly trivial looking instance of equational BRT in one function and one set

$$\text{for all } f \in V \text{ there exists } A \in K \text{ such that } fA \subseteq A$$

expresses the famous open problem known as the invariant subspace problem for Hilbert space.

[56]I conjecture that as BRT is developed for a wide variety of natural V, K, we will see a variety of examples illustrating the power of large cardinals, even for just two functions and three sets. There will also emerge elegant transfer principles that are provably equivalent to the 1-consistency of large cardinals.

[57]There is an important reduction of BRT called disjoint cover theory (DCT), which is easier to work with and is definitely more elementary. A disjoint cover relation between sets is a triple of those sets indicating that the second and third sets are disjoint and their union includes the first set. In the critical case of two functions and three sets, DCT on (V, K) seeks to determine the truth value of all statements of the form

> for all $f, g \in V$ there exist $A \subseteq B \subseteq C$ from K such that a given set of disjoint cover relations hold in A, B, C, fA, fB, fC, gA, gB, gC.

Any set of disjoint cover relations is equivalent to a single Boolean equation and provides particularly intelligible presentations of Boolean equations. All claims, preliminary indications, and conjectures made for BRT are made for DCT. In fact, for V, K of footnote 53, three disjoint cover relations suffice, and for V, K', two disjoint cover relations suffice. In the latter case of V, K', I have determined the truth values of all such statements with two disjoint cover relations using Mahlo cardinals of finite order (necessarily).

[58]The derivations of the simple instances of BRT and DCT from Mahlo cardinals of finite order are entirely accessible and familiar looking to mathematicians, where the large cardinals are packaged in terms of a suitable Ramsey like property of some well ordered set. A good account of these derivations can be given in a one hour talk to a general mathematical audience.

(h) is definitely not required—that the issue is already sufficiently joined for them by Boolean relation theory.[59]

Implicit in criteria (a)–(g) is that the body of examples and the theme launch a new area, with an eventual AMS classification number. This new area will be accepted as part of the general unremovable furniture of contemporary mathematics whose intrinsic interest is comparable to other established areas in mathematics. In this way, the issue of large cardinal axioms will be joined for a critical number of important core mathematicians.[60]

§6. Large cardinals or their 1-consistency? The statements coming out of Boolean relation theory are provably equivalent to the 1-consistency of large cardinals. So instead of adopting the large cardinal axioms themselves, one can instead adopt their 1-consistency.

When put into proper perspective, this is more of a criticism of form than substance. Adopting large cardinals amounts to asserting

"every consequence of large cardinals is true."

Adopting the 1-consistency of large cardinals amounts to asserting

"every Π^0_2 consequence of large cardinals is true."

The obviously more natural choice is to accept large cardinals, since the latter is syntactic and not an attractive axiom candidate.

However, for the purposes of proving Π^0_2 sentences, these two choices are essentially equivalent.

Another consideration is more practical. When the working mathematician wants to develop Boolean relation theory, the proofs are incomparably more direct and mathematically elegant when done under the assumption of the large cardinal axioms themselves than under the 1-consistency.

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[59]It is reasonable to expect that all open problems in discrete mathematics explicitly formulated in the literature are provable or refutable in ZFC. Nevertheless, there is the question of just how close an independent statement in discrete mathematics can be to the existing literature. I look upon BRT and DCT as new classification subjects, which I suspect are special cases of a wide family of yet to be uncovered classification subjects. In these classification subjects, one analyzes the logical form of standard mathematical theorems with the eye towards varying certain chosen parameters. In the case of BRT I fixed on Boolean relations, and varied them as parameters. But there are many other ways of looking at mathematical theorems and identifying parameters that are to be varied. The key feature one is looking for is spaces of mathematical statements with an appropriate finite number of elements. I conjecture that such classification subjects can be attached to virtually any simply stated mathematical theorem, resulting in a deep and interesting study which, with some frequency, can and can only be carried out by going far beyond ZFC.

[60]I am testing the acceptance of BRT and DCT out in the field, both with prominent mathematicians and in lectures for general mathematical audiences. Preliminary indications are favorable.

When I assert that "j needs large cardinals to prove" I formalize this as "any reasonable formal system that proves j must interpret large cardinals in the sense of Tarski." This gives a precise sense to "needs."[61]

There is an interesting point of some relevance here. Statements in Boolean relation theory are also consequences of the existence of a real valued measurable cardinal—a related kind of large cardinal axiom.

Let me put it somewhat differently. There is a substantial and coherent list of nonsyntactic axiom candidates, including large cardinal axioms and other axioms. In this list, only certain axiom candidates settle questions in Boolean relation theory. The most appropriate ones from various points of view are in fact the small large cardinal axioms. That is the obvious move to make from the point of view of a working scientist. If they later prove to be inconsistent, then we can undergo theory revision. The key advance is that the issue of new axioms finally promises to get joined in a serious way for the mathematics community.

§7. **Appendix.** Two open questions in set theory. The following are relevant to the panel discussion.

(a) Prove that large cardinals provide a complete theory of the projective hierarchy.

Here a major challenge is to come up with an appropriate definition of "complete."[62]

(b) Prove that there are no "simple" axioms that settle the continuum hypothesis.

Here I mean "simple" in the same sense that the axioms of ZFC are "simple." For example, very short in primitive notation.[63]

[61] The objection has been raised that this precise sense of "needs" is not in accordance with the usual sense of "needs". Nevertheless our sense of "needs" is still appropriate as long as large cardinals remain the accepted unified benchmark as they are now. Of course, there are other kinds of statements that have the required consequences. E.g., projective determinacy or the existence of an atomless probability measure on all subsets of $[0, 1]$. After BRT, DCT, and the like are fully integrated into the normal mathematical environment, it then makes sense to concentrate on just which of various mutually interpretable axiom candidates are preferred by mathematicians. Footnote 40 gives favorable indications for the ultimate acceptance of the large cardinal approach.

[62] One approach to this problem is given by Theorem 2.2 in Steel's contribution. However, there an arbitrary sentence in the language of second order arithmetic is regarded as a statement about the projective hierarchy, and so there cannot be the kind of completeness envisioned. In particular, we envision the set of true statements about the projective hierarchy to be recursive.

[63] Since most people have given up on trying to find evident or obvious new axioms that settle the continuum hypothesis, it seems important to develop theorems that demonstrate or at least suggest senses in which this is impossible. I am optimistic about this program. In particular, I conjecture that every statement of set theory that is as "simple" as the axioms of ZFC is provable or refutable from ZFC.

COMMENTS AND RESPONSES

I close with some specific comments on the contributions of Feferman, Maddy, and Steel.

Comments on Feferman. Feferman cites "the work of Harvey Friedman proving the independence of a finite version of Kruskal's Theorem from a moderately impredicative system and of an Extended Kruskal Theorem from Π^1_1-Comprehension" as examples of independence results for statements of mathematical interest. It is worth mentioning that my work on Extended Kruskal's theorem turned out to be a stepping stone to the subsequent work on the Robertson Seymour graph minor theorem, which, along with its corresponding finite forms, was also shown to be independent from Π^1_1-CA_0 (see [4]).

Near the beginning of Feferman's essay, he writes

> My own view is that the question [does mathematics need new axioms?] is an essentially philosophical one: Of course mathematics needs new axioms—we know that from Gödel's incompleteness theorems—but then the question must be: Which ones? and Why those?

I find this paragraph puzzling. First of all, I disagree that the question is an essentially philosophical one, at least in the sense that it is going to be decided by primarily philosophical considerations. More about this below.

But primarily, I do not see how Gödel's incompleteness theorems by themselves show that mathematics needs new axioms in the usual sense of the word. After all, those theorems have been around and understood for nearly 70 years, and mathematicians do not yet feel the remotest need to add new axioms because of them. (Contrast this with footnote 40.) It appears that Feferman is using the word "need" in a sense that requires discussion.

The question is going to be decided by the mathematical community firstly by just how useful it is to be using axiom candidates outside ZFC as opposed to staying within ZFC. But even if their use greatly simplifies proofs of interesting results, if proofs can instead be given within ZFC, then they will be regarded as an important heuristic rather than as accepted new axioms.

Granting that there is a sufficiently striking body of results in normal mathematics that can and can only be obtained by using axiom candidates outside ZFC, the issue will then be decided on whether particular axiom candidates can be identified that are particularly useful, intelligible, coherent, and manageable, as judged by the mathematicians that use them. There also must be some sort of confidence in their consistency.

The process of acceptance of axiom candidates could fail if there are alternative axiom candidates from which opposite answers to interesting mathematical questions can be derived. This might well be the case with the continuum hypothesis and related problems of a set theoretic nature.

However, experience indicates that this is not going to happen with normal mathematical statements, which are characteristically much lower down in the logical hierarchies. One expects to find that such statements, when independent of ZFC or even weak fragments of ZFC, are provably equivalent to some sort of extended consistency of a standard system that is part of the far reaching and coherent hierarchy of systems going from weak fragments of arithmetic up through the large cardinal hierarchy.

I believe that within 20 years, the rich and varied development of Boolean relation theory, disjoint cover theory, and offshoots, with points of contact with virtually every area of mathematics, will provide the required sufficiently striking body of results, with the hierarchy of small large cardinals as the preferred new axioms. Mathematicians will still note their use and look for proofs within ZFC. But when the large cardinals are used, and it is shown that the result implies their consistency, they will consider the result as having been settled positively, and full credit will be awarded for obtaining the result.

This process will be greatly bolstered by the emergence of other classification programs with similar properties, but not involving Boolean relations among sets. I expect these developments as well.

But what of the larger large cardinals such as measurable cardinals and beyond? I have preliminary indications of an extended form of BRT that corresponds to these much higher cardinals as well. The apppropriate framework for this with the same compelling nature has not yet been identified, but I am confident that it will.

Comments on Maddy. Maddy and I agree that the question "does mathematics need new axioms?" is not going to be decided on primarily philosophical grounds. However, let me extend my predictions a bit into a realm where philosophical considerations are expected to play a highly significant role.

Even though it appears that large cardinal axioms will be adopted as new axioms by the mathematical community, two issues will remain of vital concern.

Firstly, mathematicians will want forms of the axioms that are the most normal looking and directly useful mathematically. For small large cardinals, this may require relatively minor changes in presentation, but for the larger cardinals, this requires more. I have already done things like this in the restatement of cardinals around the level of elementary embeddings from a rank into itself and higher in terms of infinitary combinatorics of a straightforward kind.

More importantly, they will want good indications that they are consistent. Here is my prediction.

While the mathematics community is accepting that they really do substantially benefit from adopting large cardinal axioms, they will be uncharacteristically open to the development of new kinds of axioms based on general philosophical principles that can secure the consistency of large cardinals.

They wouldn't want to use such new kinds of philosophically based axioms, but would greatly appreciate their use by logicians in proving the consistency of large cardinals. Along these lines, I have in mind work I have done with regard to axiomatizations of set theory (with and without large cardinals) via two universes. See [5] and its references.

Finally, Maddy clearly recognizes that different communities may have different goals, and therefore may appropriately come to different conclusions about axiom candidates. In particular, the general mathematical community might well appropriately come to different conclusions than the set theory or mathematical logic community.

But so far, Maddy and her associates have concentrated their naturalistic investigations on the set theory community. I think it would be interesting and timely for them to expand their investigations into the wider mathematical community. That community will be considerably more difficult to make sense of from the naturalistic perspective, but this is well worth the extra effort involved.

Comments on Steel. Steel's section 3.3 could create the impression that Boolean relation theory might be viewed as the applied side of large cardinal research. That would be misleading.

Conceptually speaking, long before one gets to even the smallest of large cardinals, one meets cardinals such as $\beth_2, \beth_3, \ldots, \aleph_2, \aleph_3, \ldots$. For the normal mathematician, these are enormous objects indeed. And so enormous and far removed from their everyday concerns—especially in discrete and continuous mathematics—that conventional wisdom has long set in to the effect that there can be no substantial use of such objects for anything like their purposes as a matter of principle.

If we go higher up into Borel mathematics, then Borel determinacy and related statements provide necessary uses of such cardinals. In [3] it is shown that it is necessary and sufficient to use uncountably iterations of the power set operation (in effect, uncountably many uncountable cardinals) to prove

> every symmetric Borel subset of the unit square contains or is disjoint from the graph of a Borel function.

The crucial issue is whether even the tiniest of the infinite cardinals have any significant role to play in discrete and continuous mathematics.

According to BRT and DCT, even larger cardinals have a significant role to play in discrete mathematics. However, I would like much greater control so that there would be results in suitably adjusted BRT and DCT that can be proved in, say, Zermelo set theory, but not in bounded Zermelo set theory. I am expecting to achieve such control with adjusted forms of BRT and DCT.

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