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. V=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 V=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 V=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 V=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 V=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 V=L by adopting ZFC + V=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.[1] Thus

$$\mathrm{Inst}(T, \Pi^0_1) \equiv \mathrm{Con}(T),$$

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

[1] 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 "$(F \vdash \varphi) \rightarrow \varphi$", 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

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

(This is close to the theory ZFC + V=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 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.[2]

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,

[2] 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.

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.[3] If our educated guesses 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.

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

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 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.[4]

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

Here are some examples of relative consistency results obtained by these methods. The first four are equiconsistencies.[5]

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

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, 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

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

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.[6]

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,

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

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

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

[6]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[7] 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.[8]

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

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

[8] 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.

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 which uses only ZFC was found.[9] 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.[10]

It is sometimes claimed that there is an alternate, equally good, theory of projective sets which we get from V=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 V=L implies there is a Δ^1_2 wellorder of the reals, under V=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 pro-

[9] 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.

[10] 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.

jective sets based on V=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 V=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 "V=L" while acting as if you believe something else.) Adopting V=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 order arithmetic can be shown independent of existence of arbitrarily large Woodin cardinals by forcing:[11]

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

[11]The hypothesis of 2.2 follows from the existence of arbitrarily large Woodin cardinals. I have stated 2.2 this way in order to point out its converse. The converse is closely related to work of the author.

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 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 $n < \omega$.

3 Replies

3.1 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.[12]

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 ZFC + LCA, where LCA is the applicable large cardinal axioms, and accepting 1-Con(ZFC + 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-Con(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 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.

[12] 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.

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.[13]

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

[13] 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.

the foundations of mathematics, that makes these problems more interesting 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.

3.2 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.

3.3 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.

References

[1] Kurt F. Gödel, What is Cantor's Continuum Problem, American Mathematical Monthly 54 (1947), 515-525.

[2] Solomon Feferman, Is Cantor necessary?, in *In the Light of Logic*, Oxford Univ. Press, N.Y., 1998.

[3] Matthew Foreman, Menachem Magidor, and Saharon Shelah; Martin's Maximum, saturated ideals, and non-regular ultrafilters, *Annals of Mathematics*, v. 127 (1988), 1-47.

[4] Penelope Maddy, *Naturalism in Mathematics*, Oxford University Press, Oxford, 1997.

[5] Donald A. Martin, Mathematical evidence, in *Truth in Mathematics*, H.G. Dales and G. Oliveri (eds.), Oxford: Clarendon Press (1998), 215-231.

[6] Donald A. Martin and John R. Steel, A Proof of Projective Determinacy, *J. Amer. Math. Soc.* v. 2 (1989), p. 71-125.

[7] Donald A. Martin and John R. Steel, Iteration Trees, *J. Amer. Math. Soc.* v. 7 (1994), p. 1-73

[8] Saharon Shelah and W. Hugh Woodin, Large cardinals imply that every reasonably definable set of reals is Lebesgue measurable, *Israel Journal of Mathematics*, v. 70 (1990), 381-394.