# IS SET THEORY INDISPENSABLE?

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Abstract. Although Zermelo-Fraenkel set theory (ZFC) is generally accepted as the appropriate foundation for modern mathematics, proof theorists have known for decades that virtually all mainstream mathematics can actually be formalized in much weaker systems which are essentially number-theoretic in nature. Feferman has observed that this severely undercuts a famous argument of Quine and Putnam according to which set theoretic platonism is validated by the fact that mathematics is "indispensable" for some successful scientific theories (since in fact ZFC is not needed for the mathematics that is currently used in science).

I extend this critique in three ways: (1) not only is it possible to formalize core mathematics in these weaker systems, they are in important ways *better* suited to the task than ZFC; (2) an improved analysis of the proof-theoretic strength of predicative theories shows that most if not all of the already rare examples of mainstream theorems whose proofs are currently thought to require metaphysically substantial set-theoretic principles actually do not; and (3) set theory itself, as it is actually practiced, is best understood in formalist, not platonic, terms, so that in a real sense *set theory is not even indispensable for set theory*. I also make the point that even if ZFC is consistent, there are good reasons to suspect that some number-theoretic assertions provable in ZFC may be false. This suggests that set theory should not be considered central to mathematics.

Probably most mathematicians are more willing to be platonists about number theory than about set theory, in the "truth platonism" sense that they firmly believe every sentence of first order number theory has a definite truth value, but are less certain this is the case for set theory. Those mathematicians who are unwilling to affirm that the twin primes conjecture, for example, is objectively true or false are undoubtedly in a small minority; in contrast, suspicion that questions like the continuum hypothesis or the existence of measurable cardinals may have no genuine truth value seems fairly widespread.

Some possible reasons for this difference in attitudes towards number theory and set theory are (1) a sense that natural numbers are evident and accessible in a way that arbitrary sets are not; (2) suspicion that sets are philosophically dubious in a way that numbers are not; (3) the existence of truly basic set-theoretic questions such as the continuum hypothesis which are known to be undecidable on the basis of the standard axioms of set theory, and the absence of comparable cases in number theory; and (4) the fact that naive set theory is inconsistent. The classical paradoxes of naive set theory particularly cast doubt on the idea of a well-defined canonical universe of sets in which all set-theoretic questions have definite answers.

One philosophically important way in which numbers and sets, as they are naively understood, differ is that numbers are *physically instantiated* in a way that sets are not. Five apples are an instance of the number 5 and a pair of shoes is

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an instance of the number 2, but there is nothing obvious that we can analogously point to as an *instance of*, say, the set $\{\{\emptyset\}\}$. This is significant because it suggests that we can be truth platonists about number theory without making any extravagent metaphysical assumptions about the literal existence of abstract numbers ("objects platonism"), by interpreting number-theoretic assertions in terms of these kinds of concrete instances, whereas it is much harder to see any natural way to meaningfully interpret set language unless one literally believes in sets as real entities.

Unfortunately, the philosophical difficulties with set-theoretic objects platonism are extremely severe. First, there is the ontological problem of saying just what sets are. Second, there is the epistemological problem of explaining how we, as physical beings, could know anything about them. And third, there is the fact that naive set-theoretic platonism is inconsistent and the remedies for this which have been proposed are not really cogent. (I develop this point in Section 1 below.) Combined with the observation of the preceding paragraph, this leaves abstract set theory with no clear philosophical basis.

Perhaps the most influential philosophical defense of set theory is the Quine-Putnam indispensability argument. According to this argument, mathematics is *indispensable* for various established scientific theories, and therefore any evidence that confirms these theories also confirms the received foundation for mathematics, namely set theory. But as a result of work of many people going back to Hermann Weyl, we now know that the kind of mathematics that is used in scientific applications is not inherently set-theoretic, and indeed can be developed along purely number-theoretic lines. This point has been especially emphasized by Feferman. Consequently, contrary to Quine and Putnam, the confirmation of present-day scientific theories provides no special support for set theory. Indeed, the implications of the indispensability argument are reversed: if we find that none of the philosophically questionable parts of mathematics have any clear scientific value, this only tends to strengthen doubts about their actual validity.

Going beyond the indispensability argument, I see three further questions which can be raised in defense of the view that axiomatic set theory is the proper foundation for mathematics. First, even if the bulk of mainstream mathematics (including all scientifically applicable mathematics) can be formalized in essentially number-theoretic systems, is the formalization in Zermelo-Fraenkel set theory (ZFC) not more elegant and aesthetically appealing? Second, is set theory not needed for *some* important (even if not at present scientifically applicable) mainstream mathematics? And third, is set theory not of sufficient intrinsic interest on its own to dissuade us from abandoning it? I deal with these questions in Sections 4, 5, and 6. Briefly, I claim that formalizing mainstream mathematics in essentially number-theoretic systems actually carries substantial advantages over formalization in ZFC; that in fact, essentially *all* mainstream mathematics, not just the scientifically applicable part, is formalizable in metaphysically unexceptionable systems; and that set theory itself need not be abandoned because it is already largely practiced in a formalistic manner which does not actually require any kind of platonism for its justification.

This raises the possibility that the use of set theory as a foundation for mathematics may be an historical aberration. We may ultimately find that ZFC really has no compelling justification and is *completely irrelevant* to ordinary mathematical practice.

## 1. Platonist defenses of ZFC

Before I treat the question of indispensability I want to make a case that there is no clear philosophical basis for ZFC, on either platonist or anti-platonist grounds. This section will deal with platonism and the next with anti-platonism.

According to the platonist view, sets are in some sense real objects and the Zermelo-Fraenkel axioms are true statements about these objects. Generally sets are held to be "abstract" objects, so that they are not supposed to exist in space and time. Whatever this even means, it immediately leads to the *epistemological problem* of how we could know anything about them, a problem given wide currency by Benecerraf [1].

We must also ask what sort of entities sets are supposed to be (the *ontological problem*). This is related to the epistemological problem; presumably both questions would have to be answered together. However, whereas the set theory literature is largely silent on epistemology, every introductory text has something to say about ontology. Sometimes this is nothing more than some version of the non-definition "A set is a collection of objects" ([14], p. 4), but more often the nature of sets is explained in terms of spurious examples such as flocks or herds. For instance, Halmos says that a pack of wolves, a bunch of grapes, and a flock of pigeons are examples of sets ([10], p. 1), a statement satirized by Black with the comment that "It ought then to make sense, at least sometimes, to speak of being pursued by a set, or eating a set, or putting a set to flight" ([3], p. 615).

As Black's comment illustrates, there is little connection between the mathematical concept of a set and everyday expressions of the kind cited by Halmos. This was established in a decisive fashion by Slater, who analyzed in detail the various types of collective expressions used in ordinary language and showed that none of them has anything to do with sets in the mathematical sense ([24], Section II). His conclusion is that "the 'sets' of 'Set Theory' cannot be interpreted either in terms of groups of physical things, or in terms of numbers of things, or by translation into plural expressions. Certainly there is the set-theoretic symbolism, and the rules for its manipulation, and maybe it all has some interpretation. But it does not have any of the traditional interpretations, on the basis of which it was developed" ([24], p. 63). (See also Section 1.1 of [34] for more on this point.)

This raises the question of why, if naive set theory really is so ill-conceived, it is so easy to learn and feels so natural. As I noted in the introduction, sets are not physically instantiated in any obvious way. However, they may be at least to some extent linguistically instantiated, as natural language exhibits the same hierarchical nested behavior that is seen in set theory. This suggests that naive set theory appeals to our lingistic intuitions and that the classical paradoxes of naive set theory merely reflect limitations that would be encountered in any attempt to develop an ideal self-referential language.

Interpreting the paradoxes from a platonist perspective is not so easy. Indeed, it could be argued that they should block any solution to the ontological problem, on the grounds that understanding exactly what sets are is just what would be needed to render the construction "the set of all sets" legitimate.

The standard platonist answer to the paradoxes is that sets should be understood in terms of the *iterative conception* enunciated by Gödel: "a set is something obtainable from the integers (or some other well-defined objects) by iterated application of the operation 'set of' " ([9], p. 474). One interpretation of this idea is that it is meant to single sets out as a special kind of collection. Some authors clearly display this interpretation by writing, in the context of discussing the iterative conception, about collections which do not lie in the cumulative hierarchy and hence are not sets (e.g. [6], p. 2 or [17], p. 40). But this merely amounts to a change in terminology and evidently leaves the paradoxes in place as regards collections. Since "collection" is the more primitive concept, this is ultimately unhelpful.

The iterative conception can only help resolve the paradoxes if we view it as clarifying the intuitive concept of a collection, not as introducing a new, distinct "set" concept. But this clarification is elusive, as can be seen by looking at some of the versions of the conception which have appeared in the literature:

> sets are 'formed', 'constructed', or 'collected' from their elements in a succession of stages ... ([18], p. 506)

> According to the iterative conception, sets are created stage-by-stage, using as their elements only those which have been created at earlier stages. ([19], p. 183)

> In the metaphor of the iterative conception, the steps that build up sets are "operations" of "gathering together" sets to form "new" sets. ([21], p. 637)

> Thus a set is formed by selecting certain objects ... we want to consider a set as an object and thus to allow it to be a member of another set ... When we are forming a set $z$ by choosing its members, we do not yet have the object $z$, and hence cannot use it as a member of $z$. ([22], p. 322-323)

It should be apparent from this selection that the nature of the set-forming operation is extremely unclear. There seems to be no general agreement even as to whether this is an actual operation which could in any sense be carried out, or instead some kind of impenetrable metaphor. The problem is apparent in Boolos's remark that "a rough statement of the idea ... contains such expressions as 'stage', 'is formed at', 'earlier than', 'keep on going', which must be exorcised from any formal theory of sets. From the rough description it sounds as if sets were continually being created, which is not the case" ([4], p. 491). Yet Boolos does not follow his rough statement with a more informative informal description that avoids the objectionable phrases, and it seems doubtful that he could. Without these expressions there *is* no informal description.

This difficulty is connected to the ontological problem, about which none of the authors cited above has anything meaningful to say: if we have no idea what sets are supposed to be, obviously there is little we can say about how they are supposed to be formed. Yet the idea that sets are in some sense "formed" from elements which enjoy some kind of "prior" existence is crucial to the iterative conception's ability to evade the classical paradoxes. The point is supposed to be that the set of all sets, or the set of all ordinals, or Russell's set, are illegitimate on the iterative account *precisely because they cannot be "formed"*. So it seems fair to say that the iterative conception successfully deals with the paradoxes only to the extent that it presents a clear picture of the operation of set formation, which is to say, not at all.

This problem is especially acute because it is also part of the iterative conception that the power set operation is the basic step to be used in the "construction" of sets. Jané explains why this causes trouble (a similar point is made by Lear [13]):

> We must also have recourse to some form of the power set operation before setting up the iterative conception. This is an important point that is often obscured and whose neglect might lure us into believing that the power set axiom of ZF simply follows from the idea of iteration. The reason given for the validity of this axiom is that if a set lies on a layer, so do all its subsets, and therefore the set of all of them lies on the next layer. One question about this way of presenting the matter is what is meant by "all subsets" of a set $a$. Perhaps from the standpoint that the iterative conception only describes how the world of sets is actually structured there is really no question to be asked (for if we can resort to the universe of sets, there is no difficulty in saying what are all subsets of $a$; they are just those sets all of whose members are members of $a$). But if we want to account for the set-theoretic universe as built by iterated application of the power set operation, such an explanation is of no use whatever. Since we cannot turn to the result of the iteration to tell what to do at each step, the notion of *all subsets* of a given set cannot be taken for granted, but must be clarified at the outset. ([12], p. 374)

This actually understates the problem, because however the notion of *all subsets* is clarified, if the force of the iterative conception against the paradoxes is to be maintained this must be done in a manner that allows us to retain some sense of "construction" or "formation" of power sets. But the essentially circular nature of power sets renders this prospect quite hopeless. I refer here to the predicativist criticism of the power set of the natural numbers as an inherently circular object. The sort of circularity that is involved can be seen in an example I introduced in [31]. Let $(A_n)$ be a standard enumeration of the sentences of second order arithmetic. These are formal expressions which include first order variables $x, y, z, \ldots$ ranging over the natural numbers and second order variables $X, Y, Z, \ldots$ ranging over sets of natural numbers, and which allow quantification of both types of variables. Then consider the set $S = \{n : A_n \text{ is true}\}$. This is a well-defined set of numbers, provided each sentence $A_n$ has a definite truth value. However, it is the existence of the power set of the natural numbers as a well-defined totality which guarantees that these sentences do have definite truth values; without assuming this we cannot give a definite meaning to second order quantification. Thus, if the power set of the natural numbers is not already available then we cannot expect to be able to determine the set $S$, or to put it differently, *$S$ is a set of natural numbers which only becomes available after we have all sets of natural numbers.* Because of this kind of circularity, there is no sense in which we can imagine the power set of the natural numbers as being built up in a piecemeal fashion, if it was not already available to begin with.

The "medieval metaphysics" ([7], p. 248) of platonism must be attacked directly on epistemological and ontological grounds. But even leaving these questions aside, the iterative conception is internally incoherent. If language about "formation" and "construction" is to be taken as having any content whatever — which it must if the paradoxes are to be defused — then the idea that the formation of power sets is to count as a legitimate set-building operation is meretricious. There is no meaningful sense in which power sets can be thought of as being formed or constructed.

## 2. Anti-platonist defenses of ZFC

The goal of this section is to argue that there is no good anti-platonist justification of ZFC. But first something needs to be said about what would count as a "justification" in this context. Since anti-platonists do not believe in a well-defined universe of sets, they obviously cannot be expected to affirm that the Zermelo-Fraenkel axioms are really true in any strong sense.

It is tempting to suppose that the only issue for anti-platonists is whether ZFC is consistent. However, this ignores the fact that *some* theorems of ZFC are genuinely meaningful even from a very strong anti-platonist perspective. For example, certain sentences in the language of set theory are directly interpretable as statements of first order arithmetic. Anyone who recognizes such statements as meaningful ought to be interested in knowing whether all theorems of ZFC of this form are actually true.

At a minimum, I think that any attempt to justify ZFC on anti-platonist grounds ought to say something about whether ZFC is $\Sigma_1$-*valid*, i.e., whether every $\Sigma_1$ sentence of first order arithmetic that is provable in ZFC is actually true. These are, in effect, assertions of the form "Turing machine $x$ halts on input $y$" for particular values of $x$ and $y$. I take it that few anti-platonists would deny the meaningfulness of such assertions. Therefore the question of the $\Sigma_1$-validity of ZFC is legitimate. It is also pressing: if ZFC gives us bad information about which Turing machines halt, we surely would not want to use it as a basis for mathematical theories used in scientific applications, nor should we be enthusiastic about using it as a basis for mathematical theories generally.

Mere $\Sigma_1$-validity would seem to be a very minimal requirement, but there are a number of purported anti-platonist justifications of ZFC which in fact do not address this issue in any way. For example, a variety of reasons have been given by various authors as to why anti-platonists ought to feel confident that ZFC is consistent. These arguments may or may not be persuasive, but even if we could be certain that ZFC is consistent, that in itself would not entail its $\Sigma_1$-validity. For instance, if ZFC is consistent then the theory $\mathrm{ZFC} + \neg\,\mathrm{Con}(\mathrm{ZFC})$ is also consistent, but it proves the false $\Sigma_1$ sentence $\neg\,\mathrm{Con}(\mathrm{ZFC})$.

(According to ([25], pp. 1-2), Gödel criticized the formalist goal of proving the consistency of infinitary mathematics in precisely this way: "... it would be, e.g., entirely possible that one could prove with the transfinite methods of classical mathematics a sentence of the form $\exists x F(x)$ where $F$ is a finite property of natural numbers (e.g. the negation of the Goldbach conjecture has this form) and on the other hand recognise through conceptual considerations that all numbers have the property *not-$F$*; and what I want to indicate is that this remains possible even if one had verified the consistency of the formal system of classical mathematics.")

"Naturalistic" philosophies which rationalize ZFC in terms of the fact that set theorists like its properties also fail the $\Sigma_1$-validity test. For example, Maddy approves of arguing from "this theory has properties we like" to "this theory is true" ([15], p. 163); but we may like a theory for reasons which have no bearing on its $\Sigma_1$-validity, and the sorts of reasons Maddy admires generally do fall in this category. In particular, she rejects demands that mathematical theories be true in any substantive sense as "philosophical niceties" and states that "what matters are the intra-mathematical goals and the effectiveness of various means of achieving them" ([16], p. 417). Probably when she says this she is thinking of questions like the continuum hypothesis which could plausibly be supposed to have no definite content, and not about the fact that axioms which settle such questions might also have elementary number-theoretic consequences.

On the other hand, there are more ambitious philosophical programs, such as those in [5] and [8], which attempt to justify ZFC (or a large fragment of ZFC) in a very strong way on anti-platonist grounds. If some program of this sort were to succeed, we could then be confident that ZFC is $\Sigma_1$-valid. However, these attempts are, I believe, generally regarded as actually involving substantial platonist assumptions, and this is only to be expected given the proof-theoretic strength of ZFC. As I suggested in the introduction, a straightforward case can be made for Peano arithmetic on anti-platonist grounds, but when we turn to set theory the prima facie case goes against anti-platonist acceptability. Indeed, Peano arithmetic (PA) lies more or less at the limit of what can be regarded as obviously legitimate by an anti-platonist. There are accepted methods of working up to stronger systems, for example by adding an assertion of the consistency of PA and then iterating (cf. Section 5), but it seems extremely unlikely that one could get all the way up to ZFC using such techniques. The point is that there is a *huge* gap in consistency strength between PA and ZFC, so that the prospect of incrementally working up to ZFC appears quite hopeless. The other possibility, that there is some anti-platonist principle that would allow us to bridge the gap in a single step, also seems highly unlikely. So the prospect of anti-platonistically justifying ZFC in a manner sufficient to establish its $\Sigma_1$-validity is not realistic.

I have been taking it as prima facie plausible that Peano arithmetic is anti-platonistically legitimate; I defend this claim in more detail in Section 1.3 of [35]. Various objections could be raised. For example, some set-theoretic platonists would say that even number theory requires objects platonism. Their response to the argument that numbers can be understood in terms of concrete instances might be that this only works for small numbers since the universe could be finite. To the contrary, I find it perfectly reasonable to suppose that one can understand basic arithmetic without having to believe in an abstract world of numbers. The question whether our universe is finite or infinite does not seem relevant since the point is not that we have to actually physically observe and manipulate $n$ objects in order to have access to the number $n$. All we need is to have an idea of what it would mean to observe and manipulate $n$ objects. I think this is a perfectly sensible way to interpret number theory and I think the idea that there is something deeply problematic with it is disingenuous. Note that set theory apparently cannot be interpreted in any similar way because there are no natural concrete proxies for sets.

An objection can also be made in the other direction, from the point of view of intuitionism. Whereas the set theorist accepts PA himself, but argues that one must be an objects platonist to do this, the intuitionist is genuinely unable to accept PA. This position has integrity but I think it runs against common sense. Even if we reject the idea of a platonic world of numbers and interpret number-theoretic assertions in concrete terms, questions like the twin primes conjecture still seem to me completely definite. I argue this point further in Section 1.3 of [35].

It is important to recognize that the legitimacy of ZFC as a foundational system involves more than its mere alleged consistency. If it is to be taken as the rightful foundation of mathematics we should at the very least have good reasons for believing it is $\Sigma_1$-valid. Yet "soft" justifications of ZFC which merely aim to show that it is consistent or pleasing in some way do not speak to this question. There remains the possibility of a "hard" justification which does accomplish something substantive on this score, but this does not seem a realistic hope.

I will go further and say that it is more likely than not that some false statements of first order arithmetic are theorems of ZFC; see Section 7 below.

## 3. Applicable mathematics without set theory

In the preceding two sections I have indicated that Zermelo-Fraenkel set theory does not have a clear philosophical basis in either platonist or anti-platonist terms. It therefore becomes reasonable to ask what the consequences would be of rejecting ZFC as a foundation for mathematics.

Some philosophers may naturally be reluctant to pursue this question because it could entail having to tell mathematicians that they are practicing their subject incorrectly. The situation is not quite as bad as that, since, after all, most mathematicians have little interest in foundations and may have no particular commitment to ZFC. (Maddy [15] paints a very different picture, but her "mathematicians" really seem to be set theorists.) Still, for example, one commentator, who clearly recognized the difficulty in justifying the power set axiom, was driven to simply postulate the existence of power sets based on "external requirements", presumably meaning the fact that mathematicians use them ([12], p. 388).

However, it is now a settled fact that power sets of infinite sets are not actually needed for the vast bulk of mainstream mathematics. The philosophical stance which admits the natural numbers but not its power set is called *predicativism*; it was originally put forward by Bertrand Russell and Henri Poincaré, and there is a long line of research stretching back to Hermann Weyl which establishes in detail its ability to encompass ordinary mathematics. (See the introduction to [32] for references, and see [31] and Section 3 of [34] for more on the philosophical basis of this view.) The basic idea is that we accept the natural numbers and individual real numbers (or equivalently, individual sets of natural numbers, which can still be pictured in terms of physical instantiation) but do not assume the existence of a well-defined set of all real numbers (which cannot be meaningfully understood in terms of physical instantiation). In effect we treat the real line as a proper class. We can then accomodate all of the structures that appear in normal mathematics by various encoding tricks; for example, using an injection from $\mathbf{N} \times \mathbf{N}$ into $\mathbf{N}$ we can encode a sequence of reals as a single real number, and so on. Formalizing this approach yields the system $\mathrm{ACA}_0$, and the fact that the bulk of normal mathematics can actually be carried out in this system is established at length in [23]. $\mathrm{ACA}_0$ is conservative over PA, i.e., any theorem of first order arithmetic provable in $\mathrm{ACA}_0$ is already provable in PA.

There are a number of possible variations on the above. For instance, in [35] I present a system which allows a limited additional ability to reason about arbitrary properties of real numbers. This has to be done carefully in order to maintain the philosophical integrity of the system, but it has the advantage of reducing the coding machinery needed in [23]. Another approach is to explicitly construct a countable structure which can play the role of a miniature set-theoretic universe, and then formalize core mathematics within this structure in the standard way. This was done in [32].

Feferman has made the point that the fact that normal mathematics can be formalized in systems like $\mathrm{ACA}_0$ neutralizes the Quine-Putnam indispensability argument mentioned in the introduction ([7], Chapter 14). The latter is possibly the most influential philosophical defense of set-theoretic platonism, but in light of the above facts it completely loses its force. If all scientifically applicable mathematics can be straightforwardly formalized in systems which are conservative over PA, then the fact that some scientific theories which use this mathematics are well confirmed lends no particular support to ZFC.

Set theory apologists have responded rather grudgingly to Feferman's point. Quine himself, writing in 1991, acknowledged its validity in a magnanimous but curiously subjunctive way:

> Sanguine spirits there have been, and Solomon Feferman and Hao Wang are two of them today, who hope to show that enough mathematics can be derived for purposes of natural science without going beyond predicative set theory. This would be a momentous result. It would make a clean sweep of the indenumerable infinites and unspecifiable sets ... ([20], p. 229)

The "hope to show" and "would" language is a little strange, given that Wang outlined how to do this in 1954 [28] and gave details in 1963 ([29], Chapter XXIV), and several others had done similar work between 1955 and 1975.

Tellingly, Maddy finds it "striking" ([14], p. 4) and "remarkable" ([15], p. 23) that so much mathematics can be formalized in ZFC, and concludes "that set theory plays this role is central to modern mathematics" ([15], p. 35), yet as far as predicative systems are concerned she makes only the vague admission that "for the purposes of providing tools for current science ... weaker systems would probably do" ([16], p. 413) and sees no special significance in this fact. Steel also considers the formalizability of mathematics in ZFC "remarkable" ([26], p. 423) but thinks the fact that mainstream mathematics can be done in (unspecified) weaker systems "will never be more than a description of the current state of affairs" ([26], p. 424). In both cases formalizability in the favored system, ZFC, is accorded great significance but the stronger phenomenon of formalizability in essentially number-theoretic systems is dismissed as irrelevant.

Hellman actually claims that at present "*some* physically applicable mathematics appears to transcend the bounds of predicativism, especially the use of nonseparable Banach or Hilbert spaces, e.g. in quantum field theory (see e.g. Emch (1972))" ([11], p. 218; italics in original). It is unfortunate that this reference is not more specific, as I cannot find any use of nonseparable Hilbert spaces in Emch's book. They certainly play no significant role in modern quantum field theory, or in any area of modern physics for that matter.

Nonseparable Banach spaces, on the other hand, are routinely used in the mathematics of quantum field theory: every infinite dimensional von Neumann algebra is nonseparable. But already in ordinary quantum mechanics, the space $B(H)$ with

$H$ separable is itself non-separable, and even in engineering applications nonseparable $L^\infty$ and $H^\infty$ spaces are commonly used, so one hardly has to go all the way to quantum field theory to see nonseparable Banach spaces in applications. However, every such example of which I am aware (including local nets of von Neumann algebras in quantum field theory, presumably the point of the reference to Emch) can be straightforwardly handled in, e.g., the systems of [32] or [35] or, with minor indirection, in $\mathrm{ACA}_0$. (The Banach spaces cited above are all weak* separable, local nets of von Neumann algebras are countable unions of such spaces, and so on.) So Hellman's assertion is not well-taken.

Hellman, Maddy, and Steel are all impressed by the possibility that set-theoretically substantial mathematics might one day be needed in scientific applications. But of course the mere possibility of future applications provides no support whatever for the indispensability argument. Indeed, one could say of virtually any formal system that future applications are *possible*. We would only find this possibility noteworthy if we had separate reasons for being interested in the particular system in question. This is really the opposite of an indispensability argument, because ZFC is not gaining credibility from its scientific applications — at present it has none — but rather is seen as a good candidate for future applications because evidently it is already felt to be credible on some *other* grounds.

I must add, however, that given our current understanding of basic physics, the prospect of set-theoretically substantial mathematics ever becoming essential to meaningful scientific applications appears *extremely* unlikely. This should be obvious to anyone with a basic knowledge of mathematical physics and an understanding of the scope of predicative mathematics. An essential incorporation of impredicative mathematics in basic physics would involve a revolutionary shift in our understanding of physical reality of a magnitude which would dwarf the passage from classical to quantum mechanics (after all, both of these theories are completely predicative). I would rate the likelihood of ZFC turning out to be inconsistent as much higher than the likelihood of it turning out to be essential to basic physics.

The assumption that set-theoretically substantial mathematics is of any use in current science is simply false. One can hold out hope that some radically different future physical theory would require such mathematics, but there is no rational basis for this hope. It certainly finds no encouragement in the character of current physics. The argument that ZFC should be retained as the standard foundation for mathematics because it might conceivably be indispensible to some future scientific application only makes sense if we have an independent reason for favoring ZFC over other foundational systems, and is not itself a reason for favoring ZFC. Consequently, given the scope of predicative mathematics, the Quine-Putnam indispensability argument no longer has any force whatever.

## 4. ZFC is foundationally unsuitable

The idea that Zermelo-Fraenkel set theory is to be justified by its scientific applications is not the only potentially persuasive argument in favor of taking it to be the appropriate foundational system for modern mathematics. A case could also be made that predicative systems, while in principle adequate, are too awkward in practice and should yield to ZFC on aesthetic grounds. Or one could argue that in order to be acceptable a foundational stance must encompass all mainstream mathematics, not just the scientifically applicable parts. Or one could argue directly in defense of ZFC as a mathematical system of intrinsic interest.

I will address these points in this and the following two sections. First, is ZFC really the most attractive foundational system available? It is certainly aesthetically preferable to some predicative systems, particularly some of the older ones, but that does not settle the issue. A variety of predicative systems have been proposed as settings in which to formalize core mathematics; the question is whether any of them competes with ZFC in terms of elegance and ease of use.

For instance, several predicative systems based on type-theoretic formalisms have been put forward by various authors. Personally, I tend to dislike this sort of approach. Partly this is because extra work seems to be involved in keeping track of the different types, and partly it is because I find some of these systems unintuitive, but probably my main disagreement with type-theoretic approaches generally is that they seem stylistically too far removed from mainstream mathematical practice. Probably this is simply a matter of taste.

A different complaint can be raised against the formalization of core mathematics in $\mathrm{ACA}_0$ as described in [23], namely that it involves fairly heavy coding machinery. However, that criticism is unfair because the goal in [23] is to determine the *weakest possible* systems in which various theorems can be proven. Getting by with absolutely minimal assumptions may require some extra coding, but that is not the point. As I show in [35], passing to a third order language substantially reduces the need for coding.

We must remember that every formalization of mathematics involves some sort of coding. In ZFC natural numbers are coded as von Neumann ordinals, integers are coded as equivalence classes of ordered pairs of natural numbers (and ordered pairs are coded set-theoretically), rationals are coded as equivalence classes of ordered pairs of integers, and reals are coded as (say) Dedekind cuts. The predicative system CM of [35] requires roughly the same degree of coding. Here natural numbers are taken as primitive, so we do not have to code them as von Neumann ordinals. We use an injection from $\mathbf{N} \times \mathbf{N}$ into $\mathbf{N}$ to code ordered pairs of natural numbers as natural numbers; I do not think this is terribly awkward compared to coding ordered pairs set-theoretically. The constructions of $\mathbf{Z}$, $\mathbf{Q}$, and $\mathbf{R}$ in CM are then similar to their constructions in ZFC.

With one additional use of an injection from $\mathbf{N} \times \mathbf{N}$ into $\mathbf{N}$ the preceding constructions yield sequences of integers, sequences of rationals, and sequences of reals. Now virtually every standard mathematical space can be more or less straightforwardly realized inside $\mathbf{R}^\omega$, so the amount of coding needed in CM to construct these standard spaces is roughly comparable to that needed in ZFC.

One of the less attractive coding aspects of $\mathrm{ACA}_0$ involves continuous maps between complete separable metric spaces. In this setting a complete separable metric space $X$ is coded by a countable dense subset of $X$, and functions between such spaces are slightly complicated to handle because we cannot assume that the given dense subset of the domain is mapped into the given dense subset of the range. In CM this difficulty disappears because the third order language allows us to represent separable spaces (and even some nonseparable spaces) directly.

In short, I think the formalization of core mathematics in CM is quite comparable with its formalization in ZFC in terms of simplicity, elegance, and ease of use. Of course real mathematics is not actually formally executed in ZFC; it is presented informally in a manner which, ideally, would render formalization in ZFC tedious but not difficult. Instead taking CM as the foundational standard would in most subjects alter this informal presentation of mathematics in everyday practice *not at all*. In some more set-theoretically oriented fields like functional analysis there would be noticeable differences, but still, mainstream practice would not have to change in any really substantive way. The idea that predicative mathematics has to be horribly complicated is just not true.

The formalizations of core mathematics in ZFC and CM are roughly comparable in terms of elegance. However, this does not mean that the two systems should be thought equally suitable for this task. ZFC has one major shortcoming that predicative systems do not share, namely, that *the Zermelo-Fraenkel universe is grossly discordant with the realm of ordinary mathematics*. Following Gödel's iterative conception of sets (see Section 1), we can prove in ZFC the existence of sets $S_\alpha$, with $\alpha$ ranging over all ordinals, such that $S_0 = \mathbf{N}$, $S_{\alpha+1} = \mathcal{P}(S_\alpha)$ (the power set of $S_\alpha$), and $S_\alpha = \bigcup_{\beta<\alpha} S_\beta$ when $\alpha$ is a limit ordinal. This means that $S_\alpha$ is defined for $\alpha = \omega$, $\omega^2$, $\omega^\omega$, $\epsilon_0$, $\aleph_1$, $\aleph_\omega$, $\aleph_{\aleph_\omega}$, and so on. Yet *virtually none of this sequence beyond $S_1 = \mathcal{P}(\mathbf{N})$ is needed in mainstream mathematics*. Almost no objects, arguably no objects at all, in mainstream mathematics have cardinality greater than that of the continuum, and consequently virtually every ordinary mainstream object can be more or less straightforwardly encoded as either a set of natural numbers or a set of reals. This dichotomy between sets of numbers and sets of reals is just the dichotomy between *discrete* and *continuous* mathematics. We have no analogous word for mathematics at the level of $\mathcal{P}(\mathcal{P}(\mathbf{N}))$ or at any higher level because there is no mainstream mathematics there. Maybe the right word is *pathological*.

In contrast, the predicative universe exhibits a strikingly exact fit with the universe of ordinary mathematics. This is particularly well illustrated by the dual Banach space construction in functional analysis. Classically, every Banach space $V$ has a dual Banach space $V'$, but predicatively this construction is only possible when $V$ is separable. Remarkably, it seems to be a general phenomenon that for any "standard" Banach space $V$, its dual is also "standard" if and only if $V$ is separable. In other words, start with any well-known Banach space $V$ that commonly appears in the functional analysis literature, and iteratively take duals to create a sequence $V, V', V'', \ldots$. It will generally be the case that if $V$ is in common use then $V'$, $V''$, etc., will also be in common use — up to the first nonseparable space in the sequence. All spaces after that point will be highly obscure.

For example, take $V = L^1(\mathbf{R})$ (separable). Then $V' = L^\infty(\mathbf{R})$ (nonseparable), and $V''$ is an obscure space that has no standard notation. Or take $V = C[0,1]$ (separable). Then $V' = M[0,1]$ (nonseparable), and $V''$ is an obscure space that has no standard notation. Take $V = K(\mathcal{H})$ (the compact operators on a separable Hilbert space $\mathcal{H}$; separable). Then $V' = TC(\mathcal{H})$ (the trace class operators on $\mathcal{H}$; also separable), $V'' = B(\mathcal{H})$ (the bounded operators on $\mathcal{H}$; nonseparable), and $V'''$ is an obscure space that has no standard notation. If $V = c_0$ (separable) then $V' = l^1$ (separable), $V'' = l^\infty$ (nonseparable), and $V'''$ arguably has a standard notation — it is the space of Borel measures on the Stone-Čech compactification of the natural numbers — but it is certainly an obscure space that appears in the literature with extreme rarity. Examples of this type could be multiplied endlessly.

The explanation of this phenomenon is simple: generally speaking, duals of nonseparable spaces are highly pathological objects about which little of value can be said. This is characteristic of impredicative mathematics generally. The extra generality of ZFC is spurious, involving structures which are highly pathological from the point of view of mainstream mathematics. Generally speaking, "nice" spaces are predicative. This may sound like a purely subjective judgement, but it can also be seen more objectively in the fact that basic properties of impredicative spaces tend to be undetermined in ZFC. For instance, the most familiar example of a pathological, impredicative space is $\beta\mathbf{N}$, the Stone-Čech compactification of the natural numbers. Does $\beta\mathbf{N} - \mathbf{N}$ have any nontrivial self-homeomorphisms? The answer is independent of ZFC, assuming ZFC is consistent [27]. The existence of basic questions which cannot be answered in ZFC is typical of impredicative spaces.

Of course, the existence of undecidable statements is a feature of any sufficiently strong consistent formal system. The issue is not whether such statements exist, but how common and how basic they are. The impredicative portion of the universe of ZFC is rife with undecidability, and many known undecidable statements are not contrived, but appear quite fundamental — the prototype of such a statement being the continuum hypothesis. Despite strenuous efforts by logicians to identify similarly fundamental number-theoretic statements which are undecidable in predicative systems, the best available examples are still rather complicated and unnatural. Extravagent claims along these lines are sometimes made, but this seems to be wishful thinking similar to the expectation that impredicative mathematics is likely to become scientifically relevant (see the end of Section 3). Even if some really good examples were found, this still would not compare with the ubiquity of undecidability in impredicative mathematics.

The pathological quality of ZFC has consequences. One effect is that working mathematicians in certain fields have to expend effort learning to avoid set-theoretically pathological lines of investigation. This is illustrated by Hellman's suggestion mentioned in Section 3 that nonseparable Hilbert spaces could be important in mathematical physics. They are not, but this is something mathematical physicists have to learn. Similarly, functional analysts have to learn that duals of nonseparable spaces are not fruitful. Thus, in some fields the greater generality of ZFC merely opens non-productive avenues that working mathematicians must actively avoid.

I do not want to overemphasize this point, as it is not difficult to recognize set-theoretic pathology or to learn that it is to be avoided. However, the standard use of ZFC as a foundation for mathematics does entail some wasted effort in this way. A more significant effect is the loss of top-flight mathematicians to impredicative set theory. This is not to say that the study of ZFC should be abandoned wholesale (see Section 6), but on the other hand it is obvious that the intellectual power that has gone into the development of set theory is far out of proportion to its importance to mathematics as a whole. The erroneous assessment of ZFC as being central to mathematics has attracted many first-rate mathematicians to study it, drawing them away from other genuinely central subjects.

ZFC is not an appropriate foundation for mathematics. While the formalization of core mathematics in ZFC is reasonably straightforward, it is no more elegant than formalization in predicative systems like CM. But ZFC, unlike CM, is very poorly fitted to mainstream mathematics in that it embeds the well-behaved realm of ordinary mathematics in a vast arena of set-theoretic pathology. This can be a distraction for ordinary mathematicians because it opens up fruitless lines of investigation. More significantly, the widespread ascription of fundamental status to ZFC has the effect of channeling intellectual resources away from truly central subjects. Taking ZFC as the foundational standard is, in important ways, pernicious.

## 5. The limits of predicativity

As I discussed in Section 3, current science makes no essential use of impredicative mathematics. One reaction that some people have to this fact is that it is just an accident and future scientific theories surely will (or at least, there is a reasonable expectation that they will) use impredicative mathematics. I think this could seem plausible if one thinks of mathematics as a whole as being highly interconnected in an intellectually and aesthetically compelling way, and one also sees predicative mathematics as a small, artifically restricted part of mathematics as a whole. Then one might see no significance in the fact that all currently applied mathematics happens to be predicative and no reason to expect this to remain true as science develops further.

The clearest error in this way of thinking is the idea that predicative mathematics is a *small* part of mathematics as a whole. On the contrary, most mainstream subjects — differential geometry, algebraic topology, complex analysis, PDEs, etc. — as they are currently practiced, lie virtually entirely within the bounds of predicativity. Some other mainstream areas like abstract algebra or functional analysis in principle include impredicative material, but the role impredicativity plays in current research in these fields is still quite minimal. It is only in set theory itself that significant impredicativity is routinely seen.

This calls into question the "organic unity" picture of mathematics as regards set theory. The latter is *not* interconnected with the other fields listed above in anything like the same way that those fields are interconnected with each other. This should be obvious to any working mathematician; it can also be seen, for example, in the fact that virtually all mainstream mathematics can be straightforwardly construed as taking place in (at worst) $\mathcal{P}(\mathcal{P}(\mathbf{N}))$.

Thus, Feferman's emphasis on the fact that scientifically applicable mathematics is predicative (see Section 3) substantially understates the scope of predicativity, as it can leave the impression that there may be a large body of scientifically unapplied mainstream mathematics which is not predicative. Certainly, framing the debate in terms of scientifically applicable mathematics is the best way to make a case specifically against the Quine-Putnam indispensability argument because it is only this type of mathematics that is relevant to that argument. However, it may not be persuasive as a general defense of predicativism because of course one wants to preserve all mainstream mathematics, not just those bits that are currently being used in science.

Now it is more difficult to decisively show that all or virtually all mainstream mathematics is predicative because "mainstream" is not as sharp a concept as "scientifically applicable". But there are still ways of making this case. For example, one can observe that the material that appears on qualifying exams in typical mathematics Ph.D. programs is entirely or almost entirely predicative. By making observations like this one can build up the idea that impredicative mathematics is, by mainstream standards, highly pathological, and this should lead to the expectation that it is unlikely to be significantly used in successful scientific theories.

Although it is imprecise, the question of just how much mainstream mathematics is impredicative deserves to be considered further. Indeed, are there *any* compelling examples of clearly mainstream results that are fundamentally impredicative? And if there are, what are the philosophical consequences?

First of all, I do not accept the idea that the existence of even a single impredicative mainstream theorem would decisively discredit predicativism. If there were only a handful of examples of impredicative mainstream results and none of them seemed centrally important, then anyone who found predicativism to be philosophically persuasive might be willing to simply give up those results. (Actually, as I will explain in Section 6, this would be an overreaction: impredicative mathematics does not have to be given up, but only to be reinterpreted in formalist terms.) But I have already made the point in Section 3 that the vast bulk of mainstream mathematics is uncontroversially agreed to be predicative. So the case for predicativism does not hinge on whether there are *any* good examples of clearly mainstream results that are essentially impredicative.

Having said this, I will now add that I believe there are in fact no such examples. This runs against claims routinely made in the foundations literature that certain mainstream results (e.g., Kruskal's theorem) are known to be impredicative. The basis for assertions like these is an analysis of the proof-theoretic strength of predicative systems that was carried out by Feferman and Schütte, based on an idea of Kreisel. I have thoroughly criticized this analysis, including a series of subsequent papers of Feferman, in [33]; here I will outline my critique of the original analysis involving autonomous systems.

Without going into great detail, the general idea of the autonomous systems is the following. We construct a recursive well-ordering $\prec$ of $\omega$ with least element 0 and a corresponding family of formal systems $S_a$ ($a \in \omega$) such that we can predicatively infer, for any $a$, that if all $S_b$ with $b \prec a$ are valid then so is $S_a$. In particular, the base system $S_0$ is predicatively acceptable; the successor system to $S_a$ is something like $S_a + \mathrm{Con}(S_a)$ (actually, it is a bit stronger than this). Also let "$a$ is an ordinal notation" mean that $\{b \in \omega : b \prec a\}$ is well-ordered.

The question is which systems are predicatively acceptable. Kreisel's answer was that if $S_0$ proves that $a_1$ is an ordinal notation, then $S_{a_1}$ can be accepted, and if $S_{a_1}$ proves that $a_2$ is an ordinal notation, then $S_{a_2}$ can be accepted, and so on. The Feferman/Schütte analysis identifies a countable ordinal $\Gamma_0$ with the property that it is the smallest ordinal such that there is no finite sequence of ordinal notations $a_1, \ldots, a_n$ with $a_1 = 0$, $a_n$ a notation for $\Gamma_0$, and such that $S_{a_i}$ proves that $a_{i+1}$ is an ordinal notation. Thus, $\Gamma_0$ is the smallest predicatively non-provable ordinal. One can show that Kruskal's theorem implies that a notation for $\Gamma_0$ is well-ordered, so we conclude that it cannot be predicatively proven.

There are two problems with this analysis. First, the plausibility of Kreisel's proposal hinges on our conflating two versions of the concept "well-ordered" — supports transfinite induction for arbitrary sets versus supports transfinite induction for arbitrary properties — which are not predicatively equivalent.

When we prove $a_i$ is an "ordinal notation" in the Feferman/Schütte analysis, we are *only* showing that transfinite induction up to $a_i$ holds for statements of the form "$b$ is in $X$". That is, we know that if the assertion "for all $b$, everything less than $b$ is in $X$ implies $b$ is in $X$" holds for some set $X \subseteq \omega$, then everything less than $a_i$ is in $X$. *But to infer soundness of $S_{a_i}$ from this fact, we would have to be able to form the set*

$$X = \{b : S_b \textit{ is sound}\},$$

*which predicatively we cannot do.* The problem is that the systems $S_a$ are formulated in the language of second order arithmetic and hence involve assertions about all sets of natural numbers. So in order to diagnose whether a given $S_a$ is sound we need to quantify over $\mathcal{P}(\mathbf{N})$. This means that the set $X$ displayed above is a set of natural numbers which is defined by means of a condition that quantifies over all sets of natural numbers. This is circular in exactly the same way as the example I described near the end of Section 1. Indeed, it is a fundamental feature of predicativism that such constructions are considered illegitimate. The set $X$ is not a predicatively legitimate set, so we cannot use the fact that transfinite induction up to $a_i$ holds for sets to predicatively infer that $S_{a_i}$ is sound, and hence the Feferman/Schütte analysis collapses. This point is made in greater detail in [33]. I show there that essentially the same problem pervades all of the analyses of predicativity which link it to $\Gamma_0$.

I made this criticism public in 2005 and I have yet to see any substantive counterargument, or even any indication that it has been clearly understood.

There is a second basic problem. Let $I(a)$ be the assertion that $a$ is an ordinal notation. In order for $\Gamma_0$ to be the exact bound for predicativity on the Feferman/Schütte analysis, it must be the case that for any formula $A$ and any numbers $a$ and $n$ the predicativist has some way to make the deduction

$$\text{from} \quad I(a) \quad \text{and} \quad S_a \vdash A(n) \quad \text{infer} \quad A(n). \tag{$*$}$$

($S_a \vdash A(n)$ means: $A(n)$ is a theorem of $S_a$.) We have just seen that this inference is predicatively illegitimate on its face. But suppose the predicativist had some way to draw this inference. In order for the Feferman/Schütte analysis to succeed, it would also have to be the case that he *cannot* accept the single assertion

$$(\forall a)(\forall n)(I(a) \wedge S_a \vdash A(n) \quad \rightarrow \quad A(n)) \tag{$**$}$$

for arbitrary $A$, as this would allow him to go beyond $\Gamma_0$. Not only is $(*)$ predicatively invalid, it is hard to imagine what could lead anyone, predicativist or not, to accept every instance of $(*)$ but not accept $(**)$. In [33] I discuss three separate remarks of Kreisel on this question; the first is a brief comment that has no substance, the second is clearly fallacious, and the third is a highly implausible speculation. As far as I know, no other writer has even attempted to address this objection, or perhaps even recognized its existence.

An extra difficulty about this point that has apparently never been recognized is that whatever reason is given for the predicative acceptability of each instance of $(*)$ would itself have to be impredicative. For if a predicativist could see that he would accept each instance of $(*)$, then he would grasp the validity of the entire Feferman/Schütte construction and thus get beyond $\Gamma_0$. Expositions of the Feferman/Schütte analysis typically presume that the predicative acceptability of each instance of $(*)$ is obvious, which raises the question of why this is not obvious to the predicativist himself (as well as the question of why the predicative acceptability of $(**)$ is not equally obvious).

I believe this criticism of the Feferman/Schütte analysis is decisive. Later attempts to verify the same conclusion about $\Gamma_0$ in other ways involve so many errors (see [33]) that its uncritical acceptance by the foundations community for forty years raises serious sociological questions. There is no connection between predicativism and $\Gamma_0$, nor does there appear to be *any* coherent foundational stance which would lead one to accept all ordinals less than $\Gamma_0$ and not $\Gamma_0$ itself.

In the second part of [33] I show how hierarchies of Tarskian truth predicates can be used to access ordinals well beyond $\Gamma_0$, sufficient to prove Kruskal's theorem. I also make a case that these theories are predicatively legitimate. The basic idea here is to create a hierarchy of formal systems $(S_a)$ in which the successor theory to $S_a$ has a truth predicate $T_a$ with which one can reason about the truth of statements in the language of $S_a$. This is similar to the autonomous progressions of Kreisel. But we then jump one level up and consider a formal system $S_0^1$ in which we can reason about the entire sequence $(S_a)$, use this as the basis of a new sequence $(S_a^1)$, and iterate this process. The system $S_0^\omega$ proves that a notation for $\Gamma_0$ is well-ordered, but there is no reason to stop here. Indeed, we can reason about the sequence $(S_0^a)$, and this leads to a system which proves that a notation for the Ackermann ordinal $\phi_{\Omega^2}(0)$ is well-ordered. This technique can be pushed further to access even larger ordinals.

It is important to be clear that there are two aspects to [33], the first being a criticism of the various analyses which have been put forward as showing that $\Gamma_0$ is a limit to predicativism, and the second being a new analysis involving hierarchies of truth predicates which goes beyond $\Gamma_0$. If the second part is right, then examples of impredicative mainstream results vanish almost entirely. The best example of which I am aware that is not covered by the results of [33] is the graph minor theorem; however, it seems likely that further work along similar lines will succeed in establishing this result predicatively as well.

Such a large fraction of mainstream mathematics is already uncontroversially recognized to be predicative that failure to cohere with mathematical practice is no longer a meaningful criticism of predicativism. If anything, it is a greater problem for ZFC; see Section 4. Moreover, if the truth theories approach of [33] is predicatively valid then meaningful examples of impredicative mainstream mathematics may disappear altogether.

## 6. Set theory and formalism

ZFC is not a good choice to be the standard foundation for mathematics. It is unsuitable in two ways. Philosophically, it makes sense only in terms of a vague belief in some sort of mystical universe of sets which is supposed to exist aphysically and atemporally (yet, in order to avoid the classical paradoxes, is somehow "not there all at once"). Pragmatically, ZFC fits very badly with actual mathematical practice insofar as it postulates a vast realm of set-theoretic pathology which has no relevance to mainstream mathematics. We might say that it is both theoretically and practically unsuited to the foundational role in which it is currently cast.

As we have seen, these two defects are linked: reducing to the philosophically more sensible attitude of considering structures which could conceivably be physically instantiated has the effect of neatly eliminating the set-theoretic pathology which characterizes ZFC, while retaining all of the structures that are essential to mainstream mathematics.

However, this is not to say that the study of ZFC must be abandoned. It can still be understood as an interesting formal system which some mathematicians may find quite appealing. The same could be said for ZFC plus various large cardinal axioms, or ZF plus the axiom of determinacy, or ZFC plus V = L, or ZFC plus Martin's axiom, and so on.

How are we to interpret theorems proven in such systems? First of all, the fact that some statement is a theorem of ZFC is a combinatorial fact which is just as valid predicatively as it is classically. We can also regard such a theorem as a true statement in any model of ZFC, which predicatively exist provided ZFC is consistent. (And even if we are not certain that ZFC is in fact consistent, we can still reason under this hypothesis.) What we cannot do is *to regard theorems of ZFC as being true statements in some canonical universal model of ZFC*, because we have no reason to believe that such a model exists.

It is worth noting that whatever set theorists privately believe, their professional behavior is quite consistent with a formalist interpretation of ZFC. Indeed, a large part of modern set theory explicitly concerns itself with the study of ZFC and various extensions of ZFC *as formal systems*. For example, the seminal result of modern set theory is the relative independence of the continuum hypothesis from ZFC. This is the fact, provable in PA (or even weaker systems), that if ZFC is consistent then so are ZFC + CH and ZFC + $\neg$CH. One finds in the set theory literature very little discussion of whether questions like CH are *really true*, but a great deal of work — work which can be formalized in PA — about which axioms imply or fail to imply questions like CH. Thus, should predicative foundations be universally adopted, the actual practice of set theory would have to change very little.

A parallel can be drawn between set theory and nonstandard analysis. In both cases we formally legitimize a vague intuitive idealization — actual infinitesimals in the case of nonstandard analysis, and concretely existing uncountable structures in the case of set theory. Both are very elegant from a formal standpoint, but not from the point of view of models: the nonstandard reals do not have the simple, canonical quality that the standard reals possess, and even if one believes that ZFC has a canonical model this is only an abstract fact; we do not have a clear picture of it in the same way that we have a picture of $\mathbf{N}$ (the canonical model of PA) or even $J_2$ (which models a predicative set-theoretic system [32]).

Adopting a predicative foundational outlook does not entail abandoning ZFC altogether. Rather, it means that we must interpret ZFC in formalist terms, something that, operationally, set theorists already do.

## 7. ZFC and number theory

I just made the point that we have no a priori reason to believe that ZFC has a canonical model. One consequence of this is that we should be skeptical of the actual truth of number-theoretic results proven in ZFC that are not provable in predicative systems. These could only be trusted if we had some reason to believe that ZFC has models in which $\omega$ is standard.

Now we may believe that ZFC is probably consistent because (1) no inconsistency has been found yet and (2) we have built up some sort of intuition for ZFC which tells us that it is consistent. I personally find these arguments persuasive but not compelling. They suggest that ZFC probably does have a model. *However, they tell us nothing about whether it has a model with a standard* $\omega$. This seems to me more likely to be false than true. Given the recursive compexity of ZFC (as measured by its proof-theoretic ordinal, and already suggested by the circularity of the power set of $\omega$ mentioned in Section 1) we should not expect that there is such a model absent some special reason to do so. The presumption should be that ZFC has no such model and hence that there are probably some false statements of first order arithmetic that are provable in ZFC.

Antiplatonistic belief in the arithmetical validity of ZFC seems to be mainly a matter of faith. One could argue that the hierarchy of large cardinal axioms exhibits a compelling structure which is evidence for the truth of the arithmetical consequences of these axioms. Maybe so, but this is at best very indirect evidence and hardly seems very convincing. At present I think a rational assessment of the evidence would have to conclude that ZFC very likely proves false number-theoretic assertions.

I hasten to add that this is not an indictment of mainstream number theory, since mainstream number theory can be formalized in predicative systems. Rather, we should be suspicious of any number theoretic result whose proof requires set-theoretically substantial mathematics. (Harvey Friedman has given examples of such results.)

## 8. Conclusion

In brief, my position is as follows:

Reifying collections as "abstract objects" is an elementary philosophical mistake and is directly responsible for the paradoxes of naive set theory.

The "iterative conception of sets" does not succeed in legitimizing abstract set theory. It crucially involves an idea of *set formation* as if sets were physical objects that could be manipulated, which they are simultaneously denied to be (yet no characterization of the sort of entities they are is given). It also takes the ability to form power sets as basic, yet there is no meaningful sense in which one can imagine *forming* power sets of infinite sets.

We cannot evade the problem of justifying set theory by settling for the fact that "mathematicians (meaning set theorists) like ZFC" or the probability that ZFC is consistent. At a minimum, if ZFC is to be taken as the standard foundation of mathematics then we should at least demand some conviction that it is $\Sigma_1$-valid, which anti-platonism is unable to provide. Thus ZFC has no clear philosophical basis. We should not ignore the real possibility that some number-theoretic assertions provable in ZFC might be false.

Sets are universally understood in quasi-physical terms and their properties are justified in terms of imagined quasi-physical manipulations. (E.g., the axiom of choice seems true because we could run through any family of nonempty sets and choose one element from each, etc.) This is only legitimate for *in principle physically possible* structures, which is precisely the world of predicativism. The idea that uncountable structures are in any meaningful sense physically possible does not withstand scrutiny; indeed, by the Löwenheim-Skolem theorem we know that we have no way (that does not presuppose set theory) of describing a possible universe which contains uncountable structures.

All of the mathematics that is currently applied in science is predicative, and the idea that this will some day change is not credible. Virtually all mainstream mathematics is predicative [23], and it is probably the case that absolutely all mainstream mathematics is predicative [33]. The predicative system CM of [35] is at least comparable to ZFC in terms of elegance and ease of use.

I am calling for the abandonment of ZFC *as a foundational standard*. I believe its erroneous identification as the correct framework for mathematics as a whole has led it to receive a disproportionate amount of attention. However, I am not calling for the study of ZFC to cease altogether. I believe it and various related systems (including its augmentation by large cardinals, and variants not including the axiom of choice) are interesting and worthy of study, but they are also peripheral to the concerns of mainstream mathematics.

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