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ORIGINAL ARTICLE

The Role of Axioms in Mathematics

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Abstract To answer the question of whether mathematics needs new axioms, it seems necessary to say what role axioms actually play in mathematics. A first guess is that they are inherently obvious statements that are used to guarantee the truth of theorems proved from them. However, this may neither be possible nor necessary, and it doesn't seem to fit the historical facts. Instead, I argue that the role of axioms is to systematize uncontroversial facts that mathematicians can accept from a wide variety of philosophical positions. Once the axioms are generally accepted, mathematicians can expend their energies on proving theorems instead of arguing philosophy. Given this account of the role of axioms, I give four criteria that axioms must meet in order to be accepted. Penelope Maddy has proposed a similar view in *Naturalism in Mathematics*, but she suggests that the philosophical questions bracketed by adopting the axioms can in fact be ignored forever. I contend that these philosophical arguments are in fact important, and should ideally be resolved at some point, but I concede that their resolution is unlikely to affect the ordinary practice of mathematics. However, they may have effects in the margins of mathematics, including with regards to the controversial "large cardinal axioms" Maddy would like to support.

1 Introduction

An important contemporary debate (going back to (Gödel 1964)) in the philosophy of mathematics is whether or not mathematics needs new axioms. This paper is an attempt to show how one might go about answering this question. I argue that the role of axioms is to allow mathematicians to stay away from philosophical debates,

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and thus focus on their primary goal of proving interesting theorems. I find four tests that a new axiom candidate must pass in order to be accepted. However, seeing whether an axiom passes some of these tests will involve some serious philosophical work—axioms only delay the philosophical questions, they don't dissolve them.

Solomon Feferman opens his contribution (Feferman 1999) to this debate with the observation:

> The question, "Does mathematics need new axioms?", is ambiguous in practically every respect.

- What do we mean by 'mathematics'?
- What do we mean by 'need'?
- What do we mean by 'axioms'?

The sense of 'need' I aim for is the instrumental, pragmatic one, rather than a purely epistemically normative one (or a deontic one). If there were purely epistemic norms for the axioms, then it seems that they would be a part of logic. While there are those who would support a logicist picture of mathematics, it seems difficult to maintain that the axioms of replacement and foundation are purely logical truths. There are also problems with claiming that *any* existentially quantified statement could be a logical truth. But regardless of these difficulties, it seems that the instrumental sense of 'need' is the one on which we can hope to some day give a negative answer to the question. If we can find out what ends we want our axioms to serve, and can show that these axioms suffice, then we can say that mathematics does not in fact need new axioms. Thus, to say whether mathematics needs new axioms, I will first try to see what use it makes of the axioms it already has, and see if these axioms are sufficient, or if new ones will be necessary.

I will also need to devote at least a few moments to discussion of what I mean by "axioms". As Feferman points out, there are (at least) two distinct types of axioms. Some axioms—like those stating that a partial ordering is reflexive, transitive, and antisymmetric—seem to constitute a definition of the relevant terms. Some other axioms—like the Peano axioms for arithmetic, stating that every number has a distinct successor, that zero is not a successor, and that induction holds—are not generally taken to be definitive. We don't just mean *any* such set when we talk about "natural numbers"—we are instead trying to characterize some of the properties of the natural numbers, which we take ourselves to understand independently of the axioms. I will follow Feferman in calling the former type "structural axioms" and the latter type "foundational axioms".[1]

[1] This way of putting the distinction is slightly tendentious, but something like it should make sense to most mathematicians. After all, we recognize a distinction between the debate about whether all rings have an identity element, and the ultrafinitist debate about whether every natural number has a successor. On some structuralist views of mathematics, this distinction turns out to be illusory. All axioms are assimilated to the structural sort. "Mathematics is the study of those structures which arise in different uses but with the same formal properties—and mathematicians aim to carry out that study by using proofs. This view, unlike platonism, also accounts for the ways in which mathematics is used in other sciences." (Mac Lane 1997, p. 151) This view does exist, but it is not popular, even among structuralists. If this view really is tenable, then it may undermine the entire debate about axioms. But I suspect that it isn't, because it classes the ultrafinitist debates with merely terminological ones.

The debate about new axioms is about foundational axioms. Virtually everyone accepts that we will constantly need new structural axioms. "Groups" and "topological spaces" were defined almost a century ago, "categories" and "schemes" in the middle of the century, "braids" and "stacks" much more recently. As mathematicians continue their work, they will keep axiomatizing new types of structures that help them systematize the knowledge they already have, and suggest new results. However, many mathematicians believe that with the addition of the Axiom of Choice to the Zermelo-Fraenkel axioms for set theory (ZFC), no new foundational axioms will ever be needed. The debate is over whether this is true. So I will primarily discuss the role of foundational axioms rather than structural axioms, though I will at various points consider both sorts and compare them.

2 Axioms as Epistemic Foundations

One initially plausible story about the role of foundational axioms is that they are intuitively obvious statements that we can use to establish our theorems with epistemic certainty. Feferman quotes the Oxford English Dictionary defining an axiom in mathematics as "A self-evident proposition requiring no formal demonstration to prove its truth, but received and assented to as soon as mentioned." This story is supported by the fact that the first foundational axioms didn't arise until 1879, when Frege axiomatized first-order logic in his (Frege 1879).[2] The others followed within the next few decades—Peano's axioms for the natural numbers came in 1889, Zermelo's axioms for set theory in 1908, Russell and Whitehead's foundations in 1910, and Skolem and Fraenkel's near-simultaneous completion of the modern system of ZFC in 1922.

This particular time period was a time of much uncertainty in mathematics. Non-Euclidean geometry had called into question just how much could be said to be obvious, and many so-called "theorems" in real analysis had turned out to contradict one another. For instance, Cauchy claimed to have proven that the sum of any convergent series of continuous functions was continuous, and at about the same time Fourier proved that *every* periodic function, continuous or not, was representable as the sum of a convergent series of continuous harmonic functions. The hope seemed to be that we could find a small set of axioms that could be known to be correct, and then base all our theorems on these so that we could avoid false "theorems" like Cauchy's.

Certain axioms, like the axiom that every natural number has a successor, or that for any two objects there is a set containing just those two objects, seem to have the right sort of obviousness and certainty to play this role. Gödel suggested that this might be the case for all the axioms, saying,

[2] I will generally ignore Euclid's axioms for geometry. It's unclear whether they were taken to be structural or foundational. And as Hilbert eventually showed in his 1899 *Foundations of Geometry*, Euclid's axioms weren't actually sufficient to prove all the results he claimed. Thus, the Euclidean use of axioms differed substantially from the modern practice. "Hilbert's axiomatization of geometry furthered this process [making *all* assumptions explicit] by using a *formal* axiomatic method as distinct from that of Euclid." (Moore 1982, p. 309)

> despite their remoteness from sense experience, we do have something like a perception also of the objects of set theory, as is seen from the fact that the axioms force themselves upon us as being true. I don't see any reason why we should have less confidence in this kind of perception, i.e., in mathematical intuition, than in sense perception. (Gödel 1964, pp. 483–484)

2.1 Worries about this Role

However, Hartry Field has argued (Field 1980) that we don't need to guarantee that our theorems are true. He suggests that mathematics is useful primarily for its applications, and thus that all we really need is conservativeness. That is, as long as every statement about the world that we can derive with the help of mathematics is also derivable (perhaps in a more roundabout way) without mathematics, then our math has served its purpose. We don't need the purely mathematical statements to be *true* on top of this. So if the role of the axioms is to guarantee that our theorems are true, then it's not clear that we need the axioms at all. Instead, all we need is something that guarantees that our theorems are conservative. Truth (and thus, obvious truth) seems almost irrelevant.[3]

Whether or not Field is right that guaranteeing truth is *unnecessary*, there is an older worry about whether guaranteeing truth is *impossible*. In (Benacerraf 1973), Paul Benacerraf raised the worry that if mathematical truth is to be taken literally in the sense that Gödel seems to suggest, then there is no plausible mechanism by which we could come to know it. Since his argument was based on a causal theory of knowledge, John Burgess and Gideon Rosen tried, in (Burgess and Rosen 1997), to defuse Benacerraf's worry by pointing out that the causal theory of knowledge is no longer widely accepted by epistemologists. However, Field, Øystein Linnebo, and others have pointed out that the worry still arises in the question of whether our mathematical beliefs can be reliable (Field 1991; Linnebo 2006). Linnebo tries to assuage this worry, but at the moment the state of play is unclear.

At any rate, ignoring these concerns, there is plenty of evidence that axioms *don't* in fact play this role in general. While it might be useful for us to have a set of axioms that guarantee the literal truth of our theorems, and it might even be possible to get such axioms, the axioms that we actually have don't seem to be playing this role of epistemically supporting the theorems, and the matter at hand will be to find out what role they *actually* play, to see if *it* will generalize and require more axioms.

Although the foundational view is right that proofs from axioms generally do strengthen our belief in the theorems, it also often seems that the direction of epistemic support flows in the other direction. For instance, the Intermediate Value Theorem (that if a continuous function is positive at some point and negative at another, then it has a zero somewhere in between those points) and the Jordan Curve Theorem (that any non-intersecting closed curve in the plane cuts the plane into exactly two parts) are much more obvious than some of the axioms used in their

[3] Field tried in addition to show that mathematics is in fact *false*, but his program is widely considered to be unsuccessful so far. See (MacBride 1999) for a discussion of the development of this program.

proofs. It seems that the fact that we can prove both of these theorems is at least part of the reason that we accept the axiomatic characterization of the reals as Dedekind cuts (or Cauchy sequences) of rationals.

Gödel and Russell both recognized that this epistemic justification can flow the other way, and thus did not actually subscribe to the naive foundationalist view of axioms.

> [Russell] compares the axioms of logic and mathematics with the laws of nature, and logical evidence with sense perception, so that the axioms need not necessarily be evident in themselves, but rather their justification lies (exactly as in physics) in the fact that they make it possible for these "sense perceptions" to be deduced; ... This view has been largely justified by subsequent developments. ... Of course, under these circumstances mathematics may lose a good deal of its "absolute certainty;" but, under the influence of the modern criticism of the foundations, this has already happened to a large extent. (Gödel 1944, p. 449)

Thus, it seems that even seeming supporters of the foundationalist view admitted that the axioms didn't have to be obvious in themselves.

Maddy points out in (Maddy 1988) that Zermelo (the first to axiomatize set theory in a consistent way) didn't introduce his axioms as soon as he saw they were obvious. Instead, he introduced them in order to prove Cantor's controversial Well-Ordering principle. He codified principles Cantor (and others) had already been using for several decades without comment. The axioms used may not have been taken to be obvious (because he included the Axiom of Choice), but they were taken as uncontroversial among set theorists of the day. While there was relatively little controversy about his results, it was fine for Cantor to proceed informally. But when controversy emerged, it was necessary for Zermelo to axiomatize the relevant system, which ended up bringing controversy to the Axiom of Choice. As Maddy puts it, "the first axioms for set theory were motivated by a pragmatic desire to prove a particular theorem, not a foundational desire to avoid the paradoxes." particular theorem, not a foundational desire to avoid the paradoxes.“ (Maddy 1988, p. 483)

Thus, it seems that axioms are not chosen because they are inherently certain and let us make an uncertain result certain—they can certainly play this role, but that is not how or why they are chosen. Rather, they are uncontroversial and we use them to make a controversial result uncontroversial. I would like to suggest that this is the real role of axioms in mathematics—to stop arguing about our disagreements, and just work together on proving theorems.

3 Axioms as a Social Practice

It seems clear that the primary product of mathematics is theorems (though of course conjectures, hypotheses, and other results have great importance as well). In other sciences, theorems exist as well, but these are all tied to some theoretical framework or other. One can talk about Hawking's theorem in Brans-Dicke gravity, Coase's theorem in neo-classical economics, or Fisher's theorem in Mendelian population genetics. However, it's clear that theorems play a far smaller role in just about any science other than mathematics.

It seems that the social practice of establishing a set of axioms to which anyone can appeal has contributed greatly to this distinction between math and the other sciences. Once these axioms are established (and publicized), mathematicians can focus on proving theorems and know that these theorems will be accepted by all. However, scientists in other areas need to constantly argue about their basic principles and assumptions about the world. As a result, efforts are divided between proving and refuting, people repeat the same result in different frameworks, and much potentially productive research time is instead spent on amateur philosophy. But since mathematicians have been able to agree on the axioms, they have been able to concentrate on their real goal of proving theorems instead of arguing with one another.

I suggest that mathematicians need axioms so that they can bracket the important foundational philosophical questions. As a result of this communal (rather than individual and rationalistic) process of adopting axioms, it might seem that they are accepted on no philosophical basis whatsoever, which would seem not to be confidence-inspiring. However, since they are in fact (nearly) unanimously accepted, this means that each individual mathematician takes herself to be justified in accepting them. Since there is a wide variety of philosophical viewpoints under consideration at any time, as long as at least one of them actually has good arguments in favor of the axioms, then they will be good. By requiring agreement across philosophical positions, we limit ourselves to axioms that are quite likely to be good ones.

If there were no philosophical disputes then there would be no need for axioms. However, if the disputes were too large then it might not be possible to agree on a set of axioms. So both the need for axioms and their very possibility are a product of the very particular set of philosophical worries that mathematicians and philosophers of mathematics currently have.

To play a role like this, the axioms need to be both uncontroversial (accepted by almost everyone working in the field) and fruitful (being able to settle questions that mathematicians care about). Further, to justify their place on a canonical list, there should be good evidence that the axioms are independent of one another, and they should be covering up real philosophical disagreements. In the standard metaphor, the axioms provide a foundation on which to construct our mathematical edifice. I would like to suggest that instead the axioms provide a ground floor on which to build, and that the foundations may remain unclear.

While the Axiom of Choice was still controversial, results depending on it were often published in a conditional form. So rather than publishing X, mathematicians would publish "If AC then X", which was in fact a consequence of the rest of the axioms. But nowadays, dependence on the axiom of choice is rarely explicitly mentioned in non-set-theoretic contexts. If my view of axioms is correct, then a nice explanation for this fact is that since the rise of this modern axiomatic method for avoiding controversy, mathematicians have always wanted to prove their theorems from the limited set of axioms that are accepted by all mathematicians. While the

Axiom of Choice was not part of this set, it had to be included in the antecedent of a conditional so that the stated theorem could be proven from the uncontroversial axioms. Now that Choice is uncontroversial, we don't need to explicitly mention it. We see a similar phenomenon currently with so-called "large cardinal axioms", which go beyond ZFC. Most mathematicians haven't yet accepted these axioms, so when set theorists show that some result follows from one of them, they publish it as "LCA implies X". If I'm right about axioms, then in contexts where large cardinal axioms are more widely accepted, this explicit marking will be less common.

The 20th century has seen a remarkable flourishing in all areas of mathematics, and I suggest that at least some of this was due to the fact that the modern axiomatic method allows mathematicians to concentrate on proving theorems rather than having philosophical arguments.

4 When Does Mathematics Need New Axioms?

On my account, adopting an axiom requires at least four things. It should be able to be widely accepted. It should be useful in establishing interesting results that mathematicians accept. There should be real philosophical problems that can be avoided through the use of the axioms. And it should not be provable from the existing axioms.

The latter condition is important to make sure that new axioms aren't proposed unnecessarily. Since the axioms are supposed to be accepted from every philosophical position, we want there to be as few axioms as possible. In addition, if a result can be proven, then this is something that mathematicians want to know, to help understand the interrelation of all the facts they know. It is exactly this condition that prevents people from adopting useful and widely believed statements like the Riemann Hypothesis as axioms. At this point there is no widely convincing evidence that it is independent of ZFC, so it has not been accepted as an axiom. However, many important results have been shown to follow from it, and as was once the case with the Axiom of Choice, the dependence on the Riemann Hypothesis is explicitly flagged. Thus, I suggest that a statement meeting all conditions but the last should be taken as a conjecture, rather than an axiom.

The number theorist and algebraic geometer Barry Mazur points out in (Mazur 1997) that conjecture is an important part of contemporary mathematics. A series of conjectures is made that outlines a major research program. Work on the project gradually convinces most mathematicians that the conjectures must be correct, and that they lead to many interesting and useful results. Finally, it remains to be shown that the conjectures themselves follow from previously established work. Mazur stresses the analogy between the use of conjecture and the axiomatic method while keeping them distinct. "Perhaps the growth of conjecture just mirrors the growth of modern axiomatic method. This is only reasonable in that the two developments are surely close cousins, or perhaps, twins." (Mazur 1997, p. 200)

As for the condition that there be real philosophical disagreements avoided by the use of axioms, I think this is best illustrated by the fact that axioms were not seen to be necessary for most of mathematics through most of its history. The modern axiomatic method emerged from Frege's arguments over Kantian ideas about the analyticity of mathematical knowledge, and crystallized amid the epistemological and ontological debates about the Axiom of Choice. The existence of philosophical disagreement clarifies the distinction between foundational and structural axioms. The distinction, recall, is that foundational axioms are ones that are taken to be correct about some antecedently characterized class of entities, the way Peano's axioms are taken to be correct about the natural numbers and ZFC about sets. Structural axioms, by contrast, do the characterization themselves. So the axioms of group theory, or of linear algebra or topology, are themselves neither correct or incorrect, but serve as the definition of what is to count as a group, vector space, or topological space.

Any set of axioms holds of a wide range of structures, as a result of the Löwenheim-Skolem theorem and related results. For an axiom to count as foundational, rather than structural, there must be one "correct" structure in this class. In such cases, there is always room for philosophical disagreement among mathematicians who agree on the axiom, because they think that only one of these models actually is the "real" natural numbers, or universe of sets. If there is no room for philosophical disagreement, then the axioms must be treated structurally rather than foundationally.

Structural axioms, like those defining a topological space, can play a parallel role. With foundational axioms, realists about abstract numbers can share the theorems of Peano arithmetic with fictionalists, while with structural axioms, group theorists can share theorems about topological spaces with set theorists. The foundational axioms bracket philosophical disputes, while the structural ones bracket concerns about different (but related) structures.[4]

As for the requirement that new axioms be useful for proving results that mathematicians care about, this seems straightforward enough. A new axiom that has no interesting consequences will likely never be adopted. Harvey Friedman (see his contribution to (Feferman et al. 2000)) and Stephen Simpson have engaged in a research program to find undecidable statements that have interesting consequences, in order to convince mathematicians not working in foundations that there is in fact a need for new axioms. I agree with them that this is an important step in validating the need for new axioms. As has been suggested to me by Branden Fitelson, if the point of the axioms is to help mathematicians progress instead of arguing about philosophy, then the axioms should prove things that mathematicians care about, or else they are engaging in unnecessary philosophy.

Finally, in order to be adopted, it is clear that an axiom must be widely acceptable. However, there are several ways for this to occur. One can either argue from every popular philosophical perspective that the axiom is acceptable, or one can convince people to abandon the perspectives that reject the axiom, or one can engage in a combination of these approaches. For Peano's axioms, and the more

[4] The case of probability seems to be interestingly complicated. Some philosophers argue that subjectivist, frequentist, and propensity interpretations of probability are all useful, making Kolmogorov's axioms structural. But others argue that only one of these interpretations is correct, so that the Kolmogorov axioms just represent a foundational agreement among probabilists to work together to prove theorems, while bracketing philosophical questions for later. See (Hájek 2003).

basic axioms of ZFC, it seems that the former has been done. (Though some philosophical argument against ultrafinitists and the like must have occurred.) But for the Axiom of Choice, it seems more likely that the latter occurred, explaining the only gradual acceptance of the axiom. In the early 20th century, intuitionism was a popular philosophy of mathematics, which recommended rejecting AC. However, in more recent decades, intuitionism has largely died out among mathematicians, and at least some intuitionists may have adopted a version of the position that accepts the Axiom of choice.[5] Thus, in order to expand our repertoire of axioms, we have had to resolve certain philosophical issues. As we resolve more of these issues, we may adopt more axioms; conversely, as more interesting and independent axioms are discovered, we may have means and motivation to resolve more philosophical issues. However, it seems likely that such progress can always be made in a piecemeal way—we will never have to resolve all the philosophical issues in order to decide on the adoption of a particular new axiom.

I should clarify that although mathematicians may only need to have a few philosophical debates, philosophers will continue to have many. Logicists may contest my argument against the foundational role of axioms—however, they will have to contest my claims about the impossibility and non-usefulness of such a role, rather than claiming that their role is the one that axioms *currently* serve. And philosophers may continue debates about types of ultrafinitism and constructivism that are largely considered dead in the mathematical community. However, all of these debates can be helped by the emergence of new axiom candidates, which may draw mathematical attention to new issues, or new aspects of old issues.

5 Maddy's Naturalism

A recent position reminiscent of this view of axioms is that proposed in (Maddy 1997). However, she takes what she calls a naturalist position and suggests that questions about mathematics must be resolved using purely mathematical (and not philosophical) methods.

To support this claim, she makes two arguments. First, she argues that some historical debates about which axioms to use were settled before all the relevant philosophical questions were, so the philosophical methods must be irrelevant.

> The methodological debates have been settled, but the philosophical debates have not, from which it follows that the methodological debates have not been settled on the basis of the philosophical considerations. ... These debates were decided on straightforwardly mathematical grounds. (Maddy 1997, p. 191)

Second, she tries to show that this question can in fact be turned into a purely mathematical question, by developing some rules she calls MAXIMIZE and UNIFY, and trying to say mathematically when an axiom is unacceptable.

[5] Jules Richard, responding to Hadamard and Poincaré, defended AC by appealing to an alphabetization of the definitions of elements of the sets, as in his paradox of definability. (Moore 1982, pp. 104–105) “From the constructivist viewpoint espoused by Richard, the Axiom [of Choice] was true precisely because the notion of set was restricted to that of containing a definable element.” (Moore 1982, p. 309)

However, mathematizing this question is remarkably difficult. Despite some suggestive results, she never definitively achieves her goal of justifying the axiom "V $\neq$ L" (which most set theorists accept), which goes beyond ZFC. Thus, there is still room for the claim that these mathematical questions require at least some philosophical resources to answer. Her first, historical argument, seems to presuppose that philosophical progress can only help when it is completed. However, I have given a picture on which methodological questions can in fact be answered by solving *some* philosophical questions. As long as there can in fact be some piecemeal progress in philosophy, I suggest that this can contribute to mathematics.

Maddy suggests that mathematical and philosophical concerns can't be separated, so philosophers of mathematics should adopt the methods of mathematics to resolve their questions. However, another response to this naturalistic unification would be that mathematicians can adopt philosophical methods to answer some of their questions. My disagreement with Maddy is that while I say that the axiomatic method allows mathematicians to ignore philosophical questions for the time being, she suggests that it dispels the questions entirely. If I'm right, then some traditionally philosophical concerns are part of the mathematical practice (though they have been hidden over the years because of the axioms), and therefore the two should be unified in a way more favorable to philosophy than she believes. I have certainly not definitively established that philosophical work has been historically relevant to the development of mathematics, but it seems that the burden of proof is on Maddy to refute its potential relevance.

I advocate a sort of humility about the foundational questions—we haven't solved them yet, or even dissolved them. I (like John Steel, in his contribution to (Feferman et al. 2000)) think the decision of which (if any) further axioms to adopt will depend on the resolution of some of these remaining philosophical questions. But I am optimistic that we don't need to resolve all these questions. Maddy is right that traditional philosophical questions can normally be ignored by mathematicians. But this is only insofar as the topics of their interest can be resolved using existing axioms. When their research goes beyond the capacities of these axioms (as the work of Gödel and Friedman suggests that it will), mathematical work must be done to justify the interest of these results, but philosophical work must also be done to support some principle that will lead to the results. This philosophical work can either involve refuting existing philosophical positions or showing that these positions support the new axiom, but I suggest that this philosophical work cannot be avoided forever.

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