Truth and the Absence of Fact
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Mathematical Objectivity and Mathematical Objects

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Abstract and Keywords

Focuses on an issue about the objectivity of mathematics—the extent to which undecidable sentences have determinate truth-value—and argues that this issue is more important than the issue of the existence of mathematical objects. It argues that certain familiar problems for those who postulate mathematical objects, such as Benacerraf's access argument, are serious for those with highly 'objectivist' pictures of mathematics, but dissolve for those who allow for sufficient indeterminacy about undecidable sentences. The nominalist view that does without mathematical entities is simply one among several ways of accomplishing the important task of doing without excess objectivity. There is also a discussion arguing for one kind of structuralism but against another.

Keywords: Paul Benacerraf, continuum hypothesis, finiteness, indeterminacy, mathematical objects, mathematics, nominalism, objectivity, Platonism, structuralism, undecidability

Mathematics appears to be a highly objective discipline: there seem to be clear standards of rightness and wrongness in mathematics. One argument for the existence of mathematical objects, and for their having a nature that is independent of human opinions about mathematics, is that this is required for mathematics to have that kind of objectivity.

Somewhat less vaguely, the argument might go like this. First, if mathematics is to be objective, then when we try to answer a mathematical question, we must be trying to figure out which answer to it is objectively correct, that is, objectively true: anything less than this would be a sacrifice of objectivity. So for instance, if we are trying to figure out the order of the Galois group of some polynomial (over the rational numbers), then of the possible answers

A_0: G has order 0,

A_1: G has order 1,

A_2: G has order 2,

etc.,

one of them must be objectively true and the others objectively false. But second, if there are no mathematical objects, then the term ‘G’ (that is, ‘the Galois group of p(x)’) simply doesn't refer (and neither do ‘0’, ‘1’, ‘2’, etc.); so there can be no more sense to the question of which of these answers is objectively true than there is to the question of exactly how many hemoglobin cells there are in Santa Claus's body today. So there must be mathematical objects, if mathematics is to be an objective discipline.

For a variety of reasons, this isn't a very good argument, and I mention it now just to indicate a way that the two topics of this survey, mathematical objectivity and mathematical objects, have often been linked. My principal focus in the early part of the survey will be with mathematical objectivity, not with mathematical objects. But since I have mentioned the argument, I should indicate at the start one possible response to it. (Not in the end the best response, I think.) The response I have in mind is that the argument overlooks the possibility of understanding mathematical claims at other than face value. A widespread view in the philosophical literature is that a mathematical sentence which seems to make a claim about mathematical objects (groups, (p.316) polynomials, numbers, etc.) really does no such thing. How then is it to be understood? That differs from author to author, and maybe from one part of mathematics to another as well; but one possibility (see Putnam 1967, Hellman 1989) is that any such mathematical sentence is to be understood as a complicated kind of possibility statement, whose details I will not try to give. This might suggest that we can have mathematical objectivity without mathematical objects: even if there are no mathematical objects, why couldn't it be the case that there is exactly one value of n for which A_n modally interpreted is objectively true?

I'll come back to this idea, but first I want to look a bit more closely at the idea of mathematical objectivity.

1. Logical Objectivity and Specifically Mathematical Objectivity

The idea that mathematics is an objective discipline is an idea with several facets.

A

One way in which mathematics seems on its face to be completely objective is that it seems on its face that there are completely objective standards of mathematical proof. Perhaps standards of mathematical proof weren't always objective—that seems to be the moral of Lakatos 1976—but since about the time of Frege it has been required that proofs be formalizable. On current standards, to mathematically prove something one must state all nonlogical assumptions explicitly as axioms, and one must argue from one's axioms to the claim to be proved in a way that could be turned into a formal derivation given sufficient effort.

Of course, there are sometimes disputes as to whether an informal derivation could be turned into a formal derivation: but this is no serious qualification on the objectivity of mathematics, since such disputes are settlable. Is there any possibility of a more serious challenge to the view that standards of mathematical proof are completely objective? Yes, at least two challenges are possible. First, even if the above makes it an objective question what is a genuine derivation *in a given derivation procedure*, one might hold that there is no objective fact as to whether a genuine derivation in procedure P should count as a proof, by holding that there is no objective fact as to whether the logical inferences licensed by P are logically correct. More radically, one might hold with Wittgenstein 1956 (see also Kripke 1982) that there is something unobjective even about the drawing of consequences in a formal derivation procedure. Both of these positions are challenges to the objectivity of logic, and hence to the objectivity of mathematical proof.

(p.317) I mention such challenges only to put them aside. From now on I will be assuming that logic, hence mathematical *proof*, is fully objective. And because proof is so important in mathematics, this concedes most of what we may have had in mind in calling mathematics objective. It ought to be obvious that if mathematics is objective only in this sense, then the link between mathematical objectivity and mathematical objects, contemplated at the start of the chapter, is wholly illusory: you don't need to make mathematics actually be about anything for it to be possible to objectively assess the logical relations between mathematical premises and mathematical conclusions.

B

But there are further respects in which it might be asked whether mathematics is an objective discipline. These further issues concern the objectivity of mathematics *per se*, as opposed to logic: we can ask not about the objectivity of proof from given axioms, but rather about the objectivity of the choice of axioms. One possible position is that the ‘correctness’ of a mathematical statement is simply a matter of its being derivable from explicitly or implicitly accepted axioms (and of its negation *not* being derivable from those axioms—a qualification we can omit if we assume that the accepted axioms are consistent). We might call this ‘extreme anti-objectivism’ (though it really isn't *that* extreme, in that it casts no doubt on the objectivity of proof). More fully, the view is

> (i) that even when a mathematical statement is ‘correct’, this will only be as a result of our having explicitly or implicitly adopted axioms from which it is derivable; and consequently,
> (ii) that for sentences that our mathematical theories (including implicit axioms) can't prove or refute, neither they nor their negations are objectively correct.

In the rest of this section and the next, my focus will be on evaluating aspect (ii) of the view.

A popular example to illustrate aspect (ii) of anti-objectivism concerns the size of the continuum (i.e., the set of real numbers). In standard set theory (with the axiom of choice) one can prove that there are infinitely many sizes that infinite sets can have, and that these sizes fall into a simple order $\aleph_0$, $\aleph_1$, $\aleph_2$, and so on. (Here, ‘and so on’ goes beyond the finite ordinals into the transfinite.) And there is a famous proof that the size of the continuum is bigger than $\aleph_0$. But how much bigger? It turns out that virtually any answer you want to give to this question is consistent with standard set theory. (And indeed, with any expansion of standard set theory to include other axioms that are typically regarded as at all ‘evident’.) That is, for all finite values of α other than 0, and for all but a few isolated transfinite values, the claim that the size of the continuum is $\aleph_\alpha$ is consistent with everything we accept; one can't even put an upper bound on all the possible values. (This follows from celebrated (p.318) results of Gödel, Cohen, and Solovay.) And given these facts, it is very natural to wonder if it makes any sense to suppose that there is any such thing as an ‘objectively correct’ answer to the size of the continuum. The anti-objectivist says ‘no’: any answer to the question (aside from the few answers that are inconsistent with axioms we already accept) is equally good, and could be added on as a new axiom. (It wouldn't be ‘evident’, but being ‘evident’ is presumably only a requirement on axioms if your goal is to get axioms that are objectively correct.)

There are other cases where the extreme anti-objectivist position seems much less plausible. For instance, Gödel showed not only that our mathematics (if it is consistent) leaves some sentences unsettled, he showed that it leaves unsettled some sentences of the simple form

(*) For all natural numbers x, B(x)

in which B(x) is a decidable predicate, hence a predicate such that for any numeral **n** we can either prove B(**n**) or prove ¬B(**n**) (and by very uncontroversial proofs). But it is plausible to argue that any undecidable sentence of this form must be objectively correct. For the undecidability of (*) shows that there is no numeral **n** for which ¬B(**n**) is provable. (A proof of ¬B(**n**) would yield a proof of the negation of (*), contrary to its undecidability.) So by the supposition about B(x), it must be that for each numeral **n**, B(**n**) is provable (by a very uncontroversial proof), hence presumably objectively correct. And that seems to show that the generalization (*) is also objectively correct. This argument is not beyond controversy—for instance, it assumes at the last step that it is objectively the case that there are no natural numbers other than those denoted by numerals, which could conceivably be questioned—but it is surely very compelling. If so, an anti-objectivist should probably moderate his position to accommodate it.

Indeed, nearly everyone believes that the choice between an undecidable sentence and its negation is objective not only for the simple sorts of number-theoretic statements just discussed, but for elementary number-theoretic statements more generally. This belief would be a very hard one to give up, since many claims about provability and consistency are in effect undecidable number-theoretic claims, so that an anti-objectivist about elementary number theory would need to hold that even claims about provability and consistency often lack objectivity. That is a position that few will want to swallow. Still, it isn't obvious that if one grants specifically mathematical objectivity to elementary number theory one must grant it to higher reaches of mathematics.

2. Mathematical Objects Without Specifically Mathematical Objectivity

One might suppose that the issue of specifically mathematical objectivity (issue **B** above) is intimately connected to the issue of the existence of mathematical (p.319) objects (numbers, functions, sets, etc.) that mathematical theories are about. (a) In one direction, one might argue that if there are entities such as sets or numbers for mathematical theories to be about, then there is an objective answer to the question of which theory about those entities is true. (b) In the other direction, one might argue that this is the only way that we could get specifically mathematical objectivity. The argument for (b) might run something along the lines of the argument that opened the chapter. The argument would be slightly better now, in not suggesting that mathematical objects were required merely for the objectivity of mathematical *proof*; but it would still not be very good I think. But let's put (b) aside for now, and ask whether (a) is correct.

Hilary Putnam has given a powerful argument (1980) that makes it very hard to see how the question of the size of the continuum could have an ‘objectively correct’ answer, *even if there is a single fixed universe of mathematical objects*. The argument, in sketchy outline, is that there are lots of properties and relations that the mathematical objects in this universe can stand in; and there isn't a whole lot to determine which such properties and relations we should take our mathematical predicates to stand for, beyond that they make the mathematical sentences we accept true. (Mathematical predicates, after all, are not causally tied down to their extensions in any direct way, in the way that observational predicates are; and unlike theoretical predicates in empirical science, they don't even seem to be very strongly tied down to their extensions in an *indirect* way.) So it looks like whatever choice $\aleph_\alpha$ we care to make for the size of the continuum (as long as it's consistent with the rest of our set theory), we can find properties and relations for our set-theoretic vocabulary to stand for that will make that choice true and the other choices false; and there is nothing in our use of set-theoretic predicates that could make such an interpretation of the set theoretic vocabulary ‘bizarre’ or ‘unintended’. That is Putnam's argument, in broad sketch; and if correct (as I believe it is), it makes it hard to get much objectivity as to the choice of set-theoretical axioms *even assuming the existence of mathematical objects*. (With minor variation, Putnam's argument can also be brought to bear against the objectivity of undecidable sentences involving higher-order quantification.)[1]

Of course, there is a worry as to how far Putnam's argument can be pushed: does it equally show that there is no objectivity even for undecidable sentences in number theory? If so, that is reason to suspect his argument, for I noted before that it is hard to take seriously the supposition that there is no objectivity there. Fortunately, Putnam's argument does not extend to number theory: see Field 1994 or Chapter 12 of this volume for a more careful account of Putnam's argument that shows why this is so. Basically the moral of those (p.320) discussions is that Putnam's argument doesn't preclude that we have a determinate notion of finiteness[2] that defies formalization; and indeed, there is a natural account, consistent with Putnam's argument, of what makes the notion determinate. Given this, a moderate anti-objectivist position is that the 'correctness' of a mathematical statement is simply a matter of its being a *consequence* of accepted axioms (and of its negation *not* being a consequence of those axioms), in an objective but not-fully-formalizable sense of consequence that goes a bit beyond first order consequence in including the logic of the quantifier 'only finitely many'. This is enough to give rise to complete objectivity in number theory, because number theory is completely axiomatizable in the logic of finiteness (using the axiom that every natural number has only finitely many predecessors). I think that it is anti-objectivism of this moderate sort, rather than extreme anti-objectivism, that Putnam's argument really suggests.

Let's use the notion of 'consequence' in the slightly broad sense just indicated, and use 'consistent' correspondingly: a set of sentences is consistent if not every sentence is a consequence of it in that broad sense of consequence.[3] Then according to moderate anti-objectivism, mathematicians are free to search out interesting axioms, explore their consistency and their consequences, find more beauty in some than in others, choose certain sets of axioms for certain purposes and other conflicting sets for other purposes, and so forth; and they can dismiss questions about which axiom sets are *true* as bad philosophy.

I suspect that many mathematicians would find this position highly congenial. If you like you can summarize the position by saying that there is no objectivity in mathematics beyond the objectivity in logic. But this summary requires two qualifications: first, the 'objective logic' here is not formalizable, it includes the logic of finiteness (though not higher-order logic); second, the summary is inaccurate if it is taken to imply that in choosing one system of axioms over another we can't take into account such factors as interestingness, utility, beauty, and concordance with one's concepts. The point of this 'anti-objectivism' is merely that truth adds nothing as a further constraint: it is too easy to achieve. When mathematicians decided to accept the axiom of choice (assuming for the sake of argument that they hadn't implicitly accepted it all along), they refined their pre-existing conception of set so that the axiom became true of it; if we were to give up the axiom of choice in favor of some alternative such as the axiom of determinacy, we would be revising our conception of set in such a way that the axiom of choice is false of it. Once you have consistency (in the expanded sense I've indicated), your advocacy of the axioms will be enough to make them true as you intend them. That in effect is what Putnam's argument suggests.

(p.321) 3. The Prospects for Mathematical Objectivity Without Mathematical Objects

If the view of mathematics just sketched is correct, then the argument that opened the paper—the argument that purported to derive the existence of mathematical objects from the objectivity of mathematics—went wrong prior to where the advocate of non-face-value interpretations challenged it.

The argument from objectivity to objects had two steps. The first step was the claim that, if mathematics is to be objective, then when we try to answer a mathematical question, we must be trying to figure out which answer to it is objectively true. The second step was the claim that mathematical truth can only be made sense of in terms of mathematical objects. It is the second step that advocates of non-face-value interpretations of mathematics challenge. But whether or not they are correct about the second step, I have argued that the first step is already highly questionable: we can account for whatever mathematical objectivity there is quite independent of any assumptions we make about mathematical truth, using just the objectivity of logic in the slightly expanded sense given above. And not only *can* we do so, we *must*: for Putnam's argument shows that at least on the standard view of mathematical truth, where it is explained in terms of mathematical objects, truth is too easy to achieve to constrain our choice of mathematical axioms.[4]

Could the idea of specifically mathematical objectivity be saved by shifting to a non-face-value interpretation of mathematics? At first sight this seems plausible. Consider for instance the view that mathematics should be understood modally (Putnam 1967, Hellman 1989). Even if there are no mathematical objects, why couldn't it be the case that there is exactly one value of α for which C_α ('The size of the continuum is $\aleph_\alpha$') modally interpreted is objectively true?

There are two reasons why it is doubtful that any objectivity is achieved in this way. The first is that there may be more than one scheme for modally translating set theoretic claims. Why not suppose that under one scheme for modally translating set theory, C_{23} comes out true and the others false, whereas on another scheme it is C_{817} that is the true one? The second and probably more important difficulty is that modal translations, and non-face-value translations of mathematics more generally, employ powerful logical notions whose objective status is itself questionable. For instance, they certainly employ modal operators; and usually (as in both the cases of Hellman and Putnam) they also employ higher-order quantification, for which questions of interpretation arise that are very closely analogous to questions about the interpretation of quantification over sets. Indeed, the argument of Putnam (1980) that if there are mathematical objects there is no determinate answer (p.322) as to just which ones we are picking out when we speak of ‘all sets’ carries over completely to the case of second-order quantification, with the result that the size of the continuum is no more objective on the ‘mathematics as modal logic’ picture than on the ‘mathematical object’ picture (or than on the fictionalist picture that will be discussed in section 4).

We seem then to have an argument that on *any* view of mathematical truth, whether in terms of mathematical objects or not, mathematical truth does not give rise to *specifically mathematical* objectivity, and plays no role in accounting for the kind of mathematical objectivity that there is. But there are two places where some may feel that mathematical truth is playing a role behind the scenes, even on the picture of (non-specifically) mathematical objectivity I have enunciated.

The first place mathematical truth might be argued to be playing an unannounced role is in the notion of logical correctness: it may be proposed that the idea of *correct* logical inference must somehow be based on a prior notion of mathematical truth. This proposal would be extremely hard to defend if applied to the inferences of first-order logic: how on earth could one develop mathematics prior to such logic? But it might seem better as applied to inferences involving the quantifier ‘only finitely many’: one might think that the correctness or incorrectness of such inferences must be based on mathematical truths about the natural numbers or about cardinality (hence on facts about natural numbers or sets, on the mathematical object picture). But even if this view were accepted (which I don't think it should be), it would not undercut the main point that I have argued. What I have argued is that in choosing among typical competing mathematical claims $\{A_1, \ldots, A_n\}$ (say about groups or topological spaces or whatever), the objectivity of the choice is unaffected by considerations about the truth value of the A_is. The view now under consideration would imply at most that issues about the truth value of certain *other* mathematical claims may be involved in the decision.

The other place that mathematical truth might be argued to be playing an unannounced role is in my mention of utility. I have allowed (as surely anyone must) that considerations of utility play a role in our selecting some mathematical axioms over others. But it might be argued that if utility plays a role in the decision, truth is indirectly playing a role: for the utility of a mathematical theory can only be explained (or anyway, is best explained) in terms of its truth. But this strikes me as an unpromising view. One problem with it is that utility is relative to the purposes at hand: a given set theory could perfectly well be useful in one context while a 'competing' one (say, one that attributes a different size to the continuum) was useful in another. Of course, when we find use for prima-facie competing theories, we typically say that the theories don't really compete after all: an arithmetic in which 68+57 = 5 needn't count as false, it can be taken as a correct theory of, say, the numbers modulo 120. But this move is available, whatever the prima-facie competing theories (as long as they are consistent); since it is always available, lack of truth can never be used to rule out a mathematical theory, which is just (p.323) another way of saying that calling a consistent mathematical theory *true* seems completely unexplanatory of any utility it may have.[5]

4. The Existence and Nature of Mathematical Objects

Those who have recognized the limitations on the objectivity of mathematics have tended to draw as a moral some ‘anti-platonist’ view about the nature of mathematical objects. For instance, one view often associated with a denial of objectivity is that mathematical objects are mind-dependent, or dependent on the beliefs of the mathematical community: that view of mathematical objects certainly suggests that any mathematical claim that we accept will be true providing it is consistent with the other mathematical claims we accept, and that is just what the limitations on objectivity we have seen also suggest.[6] Another view that seems to have the same anti-objectivist consequence is Dummett's suggestion (1959) that mathematical objects and mathematical facts pop into existence as we probe. There is another view with this anti-objectivist consequence which is less happily characterized as anti-platonist: it is the ‘full blooded platonism’ of Balaguer 1995 and 1998, according to which there isn't just a single universe of sets, but many different ones existing side-by-side: some in which the continuum has size $\aleph_{23}$, some in which it has size $\aleph_{817}$, and so on. Also, as we've seen, Putnam's argument has it that even a view on which there is a single universe of sets, independent of the mind and existing prior to our probing, ultimately must yield the same anti-objectivist consequence as do these other views: for even if there is a single universe of pre-existing sets, there are multiple relations on it that are candidates for what we mean by membership, so that the effect of many universes is achievable in a single universe. ‘Multiple universe’ views and ‘mind-dependent objects’ views merely have the virtue of making the anti-objectivist consequence manifest.

One more view with the same anti-objectivist consequence is the fictionalist view, on which there are no mathematical objects at all. Again, this gives rise to the same limitation on objectivity: without mathematical objects, anything goes, as long as it meets the requirements of consistency (in our broad sense); though of course some fictions are more useful, beautiful, etc. than others.

The fictionalist view sounds at first more radical than the other forms of anti-platonism, in that on the most straightforward view of mathematical truth, there can't be mathematical truth in any interesting sense if there are no mathematical objects. A fictionalist may if he likes choose to avoid this (p.324) consequence by adopting a non-face-value view of mathematical language, according to which ‘there are prime numbers bigger than a billion’ really doesn't assert the existence of anything, but simply makes (say) a modal claim. Assuming that an acceptable non-face-value interpretation of mathematical language can be worked out, would this form of fictionalism be better than the form that simply says that mathematics isn't true? I think that the issue between these two forms of fictionalism would be wholly uninteresting: both agree on the metaphysics, they disagree simply on the semantics of ordinary language. (The dispute seems uninteresting even if we assume that ordinary language has a semantics clear enough to settle the issue; and that assumption is dubious.)

It might indeed be doubted that there is a significant difference between fictionalist views and some or all of the other anti-objectivist views I've mentioned. The doubt is clearest for the anti-platonist views: can there really be an important issue between, say, the view that there are only mind-dependent mathematical objects and the view that there are no mathematical objects at all? There are various ways in which one might try to hold these views only verbally different—differing, say, only in the meaning they assign to ‘exist’. An analogous doubt can be raised about the difference between anti-platonist views and Balaguer's ‘full-blooded platonism’; Balaguer himself raises such a doubt near the end of Balaguer 1998. Indeed, it might even be argued that the difference between the anti-platonist views and the standard platonist view of a single universe of mind-independent mathematical objects collapses, *if* the standard platonist accepts Putnam's argument and recognizes the futility of thinking that those mathematical objects will supply a kind of objectivity that is unavailable on the anti-platonist views. So in the rest of the chapter when I discuss platonism, I will primarily have in mind the kind of platonism that does *not* accept Putnam's point: the kind of platonism according to which there is a uniquely correct answer to the size of the continuum, difficult as it may be to know what that answer is.

5. The ‘Access’ Argument

Probably the most influential argument against platonism has been that it is hard to see how we could have epistemological access to mathematical objects as the platonist conceives them. This argument received its most influential articulation in Benacerraf 1973. The Benacerraf articulation of the argument was in terms of a causal theory of knowledge, and defenders of platonism have sometimes seized on the doubtfulness of any such theory of knowledge as their response to Benacerraf. But such a response is completely superficial: the point Benacerraf was making surely goes deeper.

One way to make Benacerraf's point is in terms of the principle that a theory tends to be undermined if it needs to postulate massive coincidence. Consider the following two claims: (p.325)

(1) John and Judy have run into each other every Sunday afternoon for the last year, in highly varied locales: in opera houses, at hockey games, in coffee houses, in sleazy bars, and so forth.
(2) John and Judy have no interest in each other and would never plan to meet; nor are they both in a club that has met in these varied locales, nor is there any other such hypothesis that could explain the perfect correlation between their locations on these 52 consecutive Sundays.

(Of course, if the universe is deterministic there must be *some* sort of explanation of any correlation that might exist between John's and Judy's location: for each of their locations is separately explainable from the laws of physics and the initial conditions of the universe. What (2) is intended to rule out is the possibility of explaining the correlation in any more ‘unified’ way.) It seems clear that (1) and (2), though not jointly inconsistent, stand in strong tension: a system of beliefs that contained both (1) and (2) would be highly suspect. Put another way: if you believe the correlation in (1), you better believe that there is some unified explanation of it. But if this is so, won't platonism also be highly suspect, unless it postulates some explanation of the correlation between our mathematical beliefs and the mathematical facts (that is, some explanation of why it is that we tend to believe that p only when p, for mathematical p)? And it is hard to see how to explain such a correlation without postulating something extremely mysterious: a causal influence of mathematical objects on our belief states, a god who predisposes us to recognize the basic truths of the mathematical realm, or whatever.

A platonist can partially answer this challenge by pointing out that there are logical interconnections between our mathematical beliefs. Indeed, if one considers only mathematics in the modern era when it has become highly axiomatized, one could argue that the task of explaining the correlation between our mathematical beliefs and the mathematical facts reduces to (a) explaining why we tend to infer reliably, and (b) explaining why we tend to accept p *as a mathematical axiom* only if p. This ameliorates the problem slightly, but certainly doesn't eliminate it, for the question arises as to how the reliability in (b) is to be explained if not by some non-natural mental powers or some beneficent god.

One common response to the Benacerrafian argument as I have outlined it is that it ‘proves too much’: the claim is that if the argument were valid it would undermine a priori knowledge generally, and logical knowledge in particular; the unacceptability of the latter consequence would then show that there has to be something wrong with the argument. I have addressed this elsewhere (Field 1996, sect. V), arguing that there is a fundamental difference between the logical and the mathematical cases. I also argue there against the idea that the 'metaphysical necessity' of mathematics can be used to block the Benacerrafian argument.

To my mind, the Benacerrafian argument is thoroughly convincing against any form of platonism that pretends to much 'specifically mathematical objectivity'. But it seems to me not to have a great deal of power against Balaguer's (p.326) 'full blooded platonism',[7] or against the Putnamian view that there is a single mind-independent mathematical universe but that the mathematical sentences we accept so directly determine their content that they are bound to come out true as long as they are consistent. Once again it is not the mathematical objects *per se*, but the claims about mathematical objectivity implicit in most standard platonism, that seem to give rise to the most serious problems.

6. The Structuralist Insight

Another influential argument against some forms of platonism was presented in another classic paper of Benacerraf's (1965). The article begins by considering the significance of alternative reductions of elementary number theory to set theory: for instance, there is Zermelo's reduction, according to which 0 is the empty set and each natural number $n>0$ is the set that contains as its sole member that set that is $n-1$; and von Neumann's reduction, according to which each number n is the set that has as its members the sets that are the predecessors of n. It seems clear that there is no fact of the matter as to which of Zermelo's and von Neumann's identifications and any of the other identifications one might come up with 'gets things right': there isn't anything to get right.

Just what broader implications to draw are controversial. One possibility, of course, is that numbers are simply *sui generis* entities, distinct from sets, so that all of the alleged identities are just false. It seems clear that Benacerraf didn't want to draw this conclusion, but he didn't want to draw the conclusion that numbers are definitely sets either: rather, his conclusion was that the *sui generis* option was just another option, on par with the Zermelo and von Neumann identifications: there is no fact of the matter as to whether numbers are sets, as well as there being no fact of the matter which sets they are if they are sets. The core idea—which I'll call *the structuralist insight*—is that it makes no difference what the objects of a given mathematical theory are, as long as they stand in the right relations to one another.[8]

If one focuses just on Benacerraf's example, it might seem attractive to deny the structuralist insight: given that we had the idea of natural number before the idea of set, the proposal that the ordinary idea of natural number is about (p.327) *sui generis* objects is certainly much more plausible than that it is about sets, and so the possibility broached at the start of the preceding paragraph is not unattractive here. But even if we were to accept this and consequently reject a full-blown structuralism, it is important to realize that the Benacerraf example is just the tip of the iceberg: throughout mathematics one is constantly defining kinds of objects, and almost every time one does so there is considerable arbitrariness in just how one does so. (Should one, for instance, define a 2-place function as a set of ordered triples, or as a function from objects to 1-place functions, or in some third way? Should one define a lattice as an ordered pair of a set and a certain kind of partial ordering on it, or as an ordered triple of a set and two operations of meet and join?) It doesn't seem at all attractive to regard all of these objects as *sui generis*, but here too it seems completely arbitrary which identification one makes. And an adequate philosophy of mathematics needs to account for this.

One possible account of the Benacerraf phenomenon is the fictionalist one (see Wagner 1982): numbers are fictitious objects anyway, and while the fiction in which they standardly figure tells us that 0 and 1 each precede 2, it doesn't tell us which if any objects are members of 2; so asking what the members of 2 are is like asking what Little Red Riding Hood had for lunch the day before she visited her grandmother.

An alternative account—the one that Benacerraf himself proposed, and is also proposed in Hellman 1989—is that arithmetic should be construed at other than face-value. In Benacerraf's version, it doesn't really treat of the numbers 0,1,2,. . . , but instead treats of arbitrary (actual or possible) progressions (ω-sequences) of distinct objects. As Kitcher (1978) points out, this really doesn't help as it stands, since the Benacerraf phenomenon arises for ω-sequences as much as for numbers. Hellman's response to this problem is to restate the ω-sequence idea in second-order logic, without use of special objects. It would take us too far afield to try to evaluate this here, or to investigate the prospects for an analogous treatment of other examples where the Benacerraf problem arises (lattices, topological spaces, tensor products, and so forth).

Another alternative, similar in spirit to the Benacerraf—Hellman line but not requiring a non-face-value interpretation of mathematics (or second-order logic), is to treat mathematics as referentially indeterminate: our singular terms ‘0’, ‘1’, ‘2’, etc. purport to single out unique objects, but fail to do so; similarly, our general terms like ‘natural number’ and ‘<’ and ‘is the sum of’ fail to single out unique classes of or relations among objects. It isn't hard to develop this line in a way that allows the standard number-theoretic truths to come out true, but that makes there be no fact of the matter as to whether ‘2 is a set’ is true.[9] (It is this ‘ontological platonism coupled with substantial (p.328) indeterminacy line’ that the Putnamian argument of Section 2 extends: see footnote 8.)

Still a fourth alternative is that numbers are objects that are somehow ‘incomplete’: 2 has properties like preceding 3 and being prime, but simply has no property that determines whether it is a set. (This view is sometimes put by saying that 2 is simply a position in a structure, and that it has no properties other than its structural ones: see Resnik 1981, Shapiro 1989.) This view is rather like the view that there is vagueness in the world rather than in language. But I am skeptical that the view can live up to its motivations. I assume that the prima facie attraction of the view is that it avoids indeterminacy in language: the symbol ‘2’ determinately refers to an incomplete object with only number-theoretic properties. This seems to work fine not only for ‘2’ but for the terms we use to describe structures in which there are no symmetries; but symmetries create a problem. Consider an example from Brandom 1996.[10] In the theory of complex numbers, −1 (like every other nonzero complex number) has two square roots; the term ‘i’ is standardly introduced for one of them (−i, of course, being the other), and is standardly used in many calculations. But even assuming that we have somehow fixed which objects are the complex numbers, which subset of them are the real numbers, and which functions on them are addition and multiplication, our usage must leave completely undetermined which of the two roots of −1 our term ‘i’ refers to: for there is no way to distinguish i and −i in the theory of complex numbers, no predicate A(x) not itself containing ‘i’ that is true of one of them but not the other.[11] So even if one says that ‘i’ is just a position in the system of complex numbers, there is indeterminacy, for the complex plane contains two structurally identical positions for roots of −1, with no distinguishing features. I doubt, then, that ‘structuralism’ in the sense of the fourth view is the best way to capture the structuralist insight.

7. Mathematical Objects and the Utility of Mathematics

Finally a few words on Putnam 1971, an article with a rather different tenor from Putnam 1980. Putnam 1971 is the *locus classicus* for the view that we need to regard mathematics as true because only by doing so can we explain the utility of mathematics in other areas: for instance, its utility in science (e.g. for stating fundamental scientific laws) and in metalogic (e.g. for theorizing about logical consequence). And although Putnam earlier held that we can use modality instead of mathematical objects to explain mathematical truth, it is (p.329) not at all clear that we can explain the applications of mathematics to contingent disciplines such as physics in ordinary modal terms. (That we can't do this in any reasonable way was the conclusion of Field 1989/91: 252–69. Hellman 1989 has a proposal for how to do it, but using a much more controversial kind of modality than the kinds needed elsewhere in his theory.) So this may provide an argument for mathematical objects as well as for mathematical truth.

The general form of this Putnamian argument is as follows:

> (i) We need to speak in terms of mathematical entities in doing science, metalogic, etc.;
> (ii) If we need to speak in terms of a kind of entity for such important purposes, we have excellent reason for supposing that that kind of entity exists (or at least, that claims that on their face state the existence of such entities are true).

There are two strategies for disputing the argument.

The bold (some would say foolhardy) strategy involves substantially qualifying premise (i). The idea is to try to show that in principle we don't need any assumptions that seem on their face to postulate mathematical entities in formulating theories in science, metalogic, or elsewhere: we can in principle do these disciplines ‘nominalistically’. Even if this ‘nominalization project’ can be carried out, we still need mathematical entities to do science, metalogic, etc., in a practical way: even if they are ‘theoretically dispensable’ they are practically indispensable. And so we need to explain the practical indispensability of mathematical objects. But the advocate of the bold strategy says that to explain that, given their theoretical dispensability, we need only show how mathematical entities serve to facilitate inferences among nominalistic premises. And (he continues) if facilitating inferences is the only role of mathematical entities then (ii) fails: something much weaker than truth (‘conservativeness’) suffices to explain this limited sort of utility. That's the idea of the strategy. Unfortunately, the nominalization project is nontrivial. I did a certain amount of work trying to carry it out some time ago.[12] I won few converts, but I'm a stubborn kind of fellow who is unwilling to admit defeat.

The less bold (more likely to succeed?) strategy is to challenge premise (ii) of the argument in a more thorough-going way: to deny that we can get from the theoretical indispensability of existence assumptions to the rational belief in their truth. Putnam calls arguments based on (ii) ‘indispensability arguments’, and vigorously defends them, but there is a good bit of recent work in the philosophy of science arguing that they need some sorts of qualification, and many have argued that the sorts of qualifications needed rule out the application to the mathematical case.

The most frequent basis for arguing this latter claim has to do with the fact that mathematical entities don't seem to be causally involved in producing (p.330) physical effects. This response has considerable plausibility. One worry about it is that if mathematical entities are theoretically indispensable parts of causal explanations (as (i) contends), there seems to be an obvious sense in which they *are* causally involved in producing physical effects; the sense in which they are *not* causally involved would at least appear to need some explanation (preferably one that gives insight as to why it is reasonable to restrict (ii) to entities that are ‘casually involved’ in the posited sense). I suspect, though, that a close look at the reasons that make it hard to theoretically dispense with certain mathematical entities in certain contexts would enable one to sharpen the intuition that the role of these entities is not causal and does not support a very convincing indispensability argument.[13] (For another way to argue that indispensability arguments are less plausible in mathematics than elsewhere, see Hawthorne 1996.)

I will say no more here about the prospects for either the more bold or the less bold strategy for responding to Putnam 1971. But suppose both strategies fail: suppose that Putnam's argument forces us to believe in mathematical truth, and perhaps in mathematical objects. Would this force us to revise the conclusions earlier in the paper about mathematical objectivity? One might naturally suppose that it would: for if we need to regard mathematics as true in order to explain its utility for science or metalogic, then couldn't issues about the truth of disputed hypotheses such as the size of the continuum make a difference to science or metalogic? (For instance, since our theory of physical space has it that straight lines have basically the structure of the real numbers, mightn't the size of the continuum make a difference to the theory of physical space?) If so, that would presumably give an objective basis for deciding issues like the size of the continuum.

Despite its superficial plausibility, I think this argument can be shown to be thoroughly misguided. The reason is that the part of the role of mathematical entities in theorizing that is not easily shown dispensable is their role as exemplars of possibilities: mathematics provides rich structures that are not found in the physical world but that are nonetheless highly useful in describing the physical world, and in describing logical inference patterns also. (See Shapiro 1983.) But in its role as a source of rich structures, set theory with one choice of continuum size and set theory with another choice are equal: if mathematics with one choice for the size of the continuum were used in an application, one could use mathematics with another choice for the size as well (if need be, by constructing a model for the second mathematics within the first).[14] Even supposing that it is an objective matter whether physical lines (p.331) contain $\aleph_{23}$ points, it doesn't follow that the acceptability of a mathematics that says that the continuum has size $\aleph_{23}$ should turn on this: for physical lines are one thing and the mathematical continuum (the set of real numbers as defined via Dedekind cuts) another. In other words, even if talk of the cardinality of physical lines makes objective sense and that cardinality differs from the cardinality one accepts for the mathematical continuum, it doesn't follow that one's mathematical theory is unsuited to the description of physical space: it's just that the structure of physical lines may not be quite that of the real numbers, but might be that of some other ordered field that 'looks a lot like' the real numbers. And that, it seems to me, is a conclusion we ought to regard as possible on independent grounds.

The conclusion I have been trying to suggest, here as elsewhere in the chapter, is that what is of primary importance in the philosophy of mathematics isn't the issue of mathematical objects but the issue of mathematical objectivity. This is not an original view: it is one that Putnam attributes to Kreisel (Putnam 1975: 70). Kreisel, though, was presumably saying it in defense of a view on which mathematical objectivity far transcends logical objectivity; whereas I have been saying it in defense of the view that (barring the qualification about finiteness made in section 2) logical objectivity is all the objectivity there is.

Notes:

(1) Boolos 1984 gives an attractive account of monadic second order quantifiers according to which they don't range over special entities, but instead are ‘plural quantifiers’. This helps make (monadic) second order quantification acceptable to those with ontological qualms. But as far as I can see it does not help alleviate the force of Putnam's argument against the *determinacy* of second order quantification.

(2) More exactly, of the quantifier ‘only finitely many’.

(3) In the case of impure mathematics, one really needs to broaden the notion of consistency in a different direction as well, to what I've called conservativeness: see Field (1989/91: 55–8; n. on 96–7). This qualification doesn't affect anything of substance in the present paper, so I will ignore it in what follows.

(4) More fully, Putnam's argument shows that truth is too easy to achieve as long as there are (infinitely many) mathematical objects. I'd add that if there aren't, then truth is too *hard* to achieve to be a useful constraint.

(5) This last point also seems to me to cast doubt on the idea (contemplated in the previous paragraph) that the correctness of logical inference must be based on the truth of mathematical claims. For if *any* consistent mathematics can count as true, which mathematics is it that constrains our logic?

(6) Provided of course that consistency is understood in the strong sense discussed in section 2.

(7) Indeed, the desire to get around the Benacerraf argument was the explicit motivation of Balaguer 1995.

(8) More exactly, there is no choosing between two isomorphic models of a mathematical theory. Earlier in the paper I advocated Putnam's model-theoretic argument. This accepts the structuralist insight and extends it. First, it extends it from isomorphic models to certain cases of elementarily equivalent models, that is, certain cases of models that differ in structure in a subtle enough way that they give rise to the same truth value for every sentence in the mathematical language. (Indeed, I think it extends the conclusion to all models that are elementarily equivalent *in an expansion of first order logic that includes the finiteness quantifier standardly interpreted*.) Second, it extends it to certain cases of models that aren't elementarily equivalent but give rise to the same truth values for all of our mathematical beliefs.

(9) I did so in Ch. 7. Kitcher 1978 says that his objection to the previous alternative applies to this one too, on the ground that you need set theory to develop the account of truth for indeterminate mathematical sentences; but I think this extension incorrect, for I think the set-theoretic metalanguage can itself be regarded as indeterminate.

(10) Brandom uses the example for a different, though not unrelated, purpose: for arguing against Frege's logicism.

(11) Slightly more strongly, the function that takes x+iy into x−iy (for x, y real) is a real-algebra-automorphism of the complex numbers (i.e. a field-automorphism that leaves the reals fixed).

It is also pretty clear that the *applications* of the complex numbers don't serve to distinguish i from −i, though I won't take the trouble to make this claim precise.

(12) See for instance Field 1980 for the applications of mathematics to physics, and Field 1991 for the applications to metalogic.

(13) To test this out, suppose that the role of sets in physical theory was simply to allow us to assert the local compactness of physical space; or suppose that its role was simply to allow us to accept (C_p) rather than (C_s), where these are as in Field 1989/91: 136–7.

(14) ‘Construct’ must be understood as a bit looser than ‘define’, because of well-known limitations on ‘inner model’ proofs. The second model could for instance be the result of collapsing a Boolean valued model (which *is* explicitly definable in the first model, but is not a model in the usual sense) by an appropriate ultrafilter; appropriate ultrafilters provably exist in the first model, but none will be explicitly definable in it.