Brief Remarks on Putnam and Realism in Mathematics[*]

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Hilary Putnam has through much of his philosophical life meditated on the notion of realism, what it is, what form of realism is a sensible and natural philosophical view, and what forms might be excessive and to be rejected. Much of this reflection is quite general, and it would be absurd for me to pursue the matter on that level here. Even with respect to mathematics, Putnam's reflections over the years have formed a subtle and complicated thread. I have tried to navigate it in my paper for the LLP volume on Putnam's philosophy.[1] What I will try to do today is to try to deal more briefly with this theme in Putnam and to confront it with some interesting remarks on the subject made by others. This talk is little more than a footnote to the LLP paper. But I hope it will prompt Hilary to express his current views on some questions.

A fact about Putnam's realism, which surfaces from time to time in his remarks about mathematics, is that his most fundamental commitment to realism is to *scientific* realism. I think I may not have appreciated that to a sufficient degree in the LLP paper. And in these remarks it will come up only at the end.

I will begin with two characterizations of realism in mathematics, not due to Putnam, which have had a considerable influence on me. The first is William Tait's: One is a realist if one takes the language of mathematics at face value, and

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[*] Edited and corrected version of remarks to the conference Philosophy in the Age of Science, honoring Hilary Putnam in anticipation of his 85th birthday, presented June 1, 2011.

[1] "Putnam on realism and 'empiricism' in mathematics," in Randall E. Auxier and Lewis Edwin Hahn (eds.), *The Philosophy of Hilary Putnam* (Chicago and La Salle, Ill.: Open Court, forthcoming).

accepts what has been proved in mathematics as true, without demanding that it be translated into some alternative language in order to make its commitments acceptable from some philosophical point of view. He says that realism in this sense is the default position about mathematics, that it ought to be treated as a truism. I will call this default realism. It follows from remarks of Tait that it is natural to understand it in a relativized way, because one might have different views as to what counts as acceptable mathematics. As regards the mathematics that is acceptable from an intuitionist point of view, the intuitionist is by this standard a realist.[2]

This view might well be characterized as the minimal stance that has any claim to be regarded as realist. One of the great difficulties and sources of confusion about realism about mathematics is whether we should stop there or seek to make out some more robust form of realism. Although Putnam hints at such a minimalist attitude in a remark at the end of "Models and reality," in general he belongs among those who have sought a more substantive version of realism.

One natural next step up is what was expressed by Paul Bernays in his classic "On platonism in mathematics."[3] The further step is admitting quantification and the law of excluded middle in the form '$\forall x Fx \vee \exists x \neg Fx$. One might distinguish admitting LEM from bivalence, but let's ignore that. Bernays

[2] Cf. Tait, *The Provenance of Pure Reason* (New York and Oxford: Oxford University Press, 2005), p. 291. For the general idea of default realism, see the Introduction and Essays 3 and 4 of that book.

[3] "Sur le platonisme dans les mathématiques," *L'enseignement mathématique* 34 (1935), 52-69, translation in Paul Benacerraf and Putnam (eds.), *Philosophy of Mathematics: Selected Readings* (2d ed. Cambridge University Press, 1983).

was quite explicit about relativization: what he calls "platonism" is a stance that one can take in some domains of mathematics but not others. This was an important move, because it was a first attempt to give a philosophical rationale to a hierarchy of degrees of strength of the mathematical theories one might admit. But I will leave it aside for now.

The minimal idea of realism leaves quite open the question whether mathematical statements are true or false whether or not we know or can know which they are, while the "platonism" of Bernays does make room for the possibility of unknown or unknowable truths. Even though he himself seems to have represented it as principally a methodological stance, that is a reason why it should have a stronger claim to be "realism" than the minimal view.

We're all familiar with the denial of the existence of unknowable mathematical truths by Brouwer and those who have followed him. I am more interested in another such denial. In his famous lecture "On the infinite," David Hilbert expressed eloquently his conviction that every mathematical problem has a solution:

> We are all convinced of that. After all, one of the things that attract us most when we apply ourselves to a mathematical problem is precisely that within us we always hear the call: here is the problem, search for the solution; you can find it by pure thought, for in mathematics there is no *ignorabimus*.[4]

In another lecture several years later, he expressed the same conviction and closed with the words, "Wir müssen wissen; wir werden wissen." These words

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[4] "Über das Unendliche," *Mathematische Annalen* 95 (1926), 161-190, p. 189, translation from van Heijenoort, p. 384.

were engraved on his tombstone. Hilbert's expression of this conviction was connected with views that were refuted by Gödel's incompleteness theorem. But that the conviction itself was not refuted is indicated by the fact that Gödel himself expressed a similar conviction many years later.[5]

However, for both Hilbert and Gödel this was primarily an expression of rational faith, which may not have been as widely shared as Hilbert intimates. However, it points to a question about recognition-transcendent truth. That is the difficulty of presenting a convincing example of a mathematical statement that we are convinced is either true or false, where there are concrete reasons for thinking that it may be impossible to determine which.

Gödel, the prototypical realist about set theory, famously wrote that the axioms of set theory describe a "well-determined reality, in which the continuum hypothesis is either true or false." But what would convince us of that? In particular, what tells against the argument of skeptics who maintain that the concepts underlying set theory are either vague or have fundamental ambiguities? (In the past, skeptics about set theory appealed to paradoxes and argued that we do not have sufficient reason to think that set theories are consistent. For reasons not worth going into here, such arguments have not had much appeal in at all recent years.)

Surely the main thing that tells against the skeptical view is the discovery of unambiguous solutions to problems that are posed in the course of research in set theory. It follows that the continuum problem, first posed by Cantor, is a

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[5] See Hao Wang, *From Mathematics to Philosophy* (London: Routledge and Kegan Paul, 1974), pp. 324-25.

difficult case for a view like Gödel's, while many other, more successful cases reinforce a view like his. However, the success of a program like Hugh Woodin's for solving the continuum problem would be powerful argument against the skeptical view.

Even the successful cases in set theory differ from more or less intractable cases in other areas of mathematics. We have seen in recent years solutions to some long-outstanding problems, such as Fermat's "last theorem" and the Poincaré conjecture. Neither has required the discovery of new *axioms*, or even the exploitation of the strongest axioms that are applied, in particular by set theorists. Of famous problems in, say, number theory that remain unsolved, we have no particular reason to think them different in this respect, even if we can't quite rule out the possibility that they are.

In a paper published ten years ago, Putnam pointed out that a theory might give rise to questions that have definite answers, but where we have no reason to expect that we will ever find it, and there may be physical limits on the possiblity of doing so. The example was physical. Can the same be done in mathematics?

Let's try the following: We can probably describe (e.g. in terms of superexponentiation) a number n such that no physically possible computer could compute all of the first n digits of the decimal expansion of π. Strengthening Brouwer's early intuitionisitic counterexamples, consider the question whether there is a $k > n$ such that the 100 digits of the expansion beginning with the kth consist of ten sequences of the form 0123456789. I think no one who accepts classical mathematics would doubt that the assertion of the existence of such an k is either true or false, and it is hard to take seriously the idea that there is some hidden ambiguity or vagueness by virtue of which it would not be "determinately" true of false that there is such a k.

One solution to this problem, that such a number k might just turn up in the course of computing the expansion, is ruled out by hypothesis. No doubt Brouwer didn't rule out the simpler hypothesis that a sequence 0123456789 might just turn up. (I have heard that it has, but have not seen this in writing. In any case supercomputers would be required.) He probably thought it unlikely, and the force of his examples was probably that he didn't think anyone would have any idea of how to prove that such a sequence could never turn up. I would be surprised if anyone has an idea of how to prove that the k I described cannot turn up. The same difficulty would attend the project of proving that there is such a k, without carrying out the computation so as to discover it. But if I am wrong about this, someone more knowledgeable about number theory could probably produce a more convincing example.

But my example has an inconvenient feature. That is that at the beginning it depends on physics, through the assumption that we can describe a number n such that no physically possible computer could compute the decimal expansion of π that far. This appeal to physical limits probably takes us far beyond the actual limits of the human mind. If we appeal to "reason" in a quite abstract way, it is not clear that we encounter any limits at all. Have mathematicians even considered a question whose available formulations involve, let's say, 100 quantifiers? But if not, that is no doubt because it is beyond the limit of complexity that humans can handle. But that again is a fact about human beings that we know by experience.

Other kinds of complexity are less fanciful. The four-color problem was solved by a proof that was so complex that it could only be generated by a computer. Although it has been simplified since, what is available still has that feature. In the last thirty years logicians have stated propositions that can be proved but only by assuming hypotheses of great logical strength. It is likely that there is some level in the hierarchy of such strength (say in terms of interpretability) such that, assuming the history of research in set theory to be finite, no mathematician will state or understand a hypothesis of that degree of strength. (So the statement that there is such a level is without any description of it.) There are certainly, by Gödel's theorem, propositions that even such a hypothesis would not decide, and there may be mathematically natural ones.

The continuum hypothesis is more challenging philosophically than these fanciful examples, because we know many propositions that imply it, and many that imply its negation. We know that the large cardinal axioms that have been formulated, which are the most persuasive extensions of standard set theory, do not decide it either way. The program for settling it that Hugh Woodin presented in lectures and publications from 2000 on seems now not to be embraced by its author. There is another program that seems largely to be his doing, but which I'm not competent to say much of anything about, to produce an "ultimate inner model" that would be preserved even in the face of future large cardinals that might be discovered. Such a model would satisfy CH. Would that show CH to be true? I have to leave it to the experts to say.

I believe Putnam interprets scientific realism as requiring that we take the real numbers as a definite totality that could not be "indefinitely extensible" in the way it is on constructions inspired by the rejection of impredicativity. This intuition is bolstered by a purely mathematical development, that large cardinal axioms (that there is a proper class of Woodin cardinals) are sufficient to make the theory of the reals and of definable sets of reals immune from being upset by set forcing.[6] CH can be formulated as a statement of *third*-order arithmetic, i.e. in a language admitting quantification over reals and sets of reals. Here's a question I would like to put to Hilary: Does science similarly require that the totality of sets or functions on real numbers be a determinate totality?[7]

Paul Bernays remarked in a number of places that the fundamental intuitive idea of the continuum is geometric, and the set-theoretic construction of the real numbers that we all know and love is a successful attempt to capture that intuitive idea and make it amenable to rigorous mathematics, with some but not fatal intuitive sacrifice. It is not clear that the same motivation is there for even the next iteration of the power set operation, in spite of the naturalness that that axiom has acquired through a century of axiomatic set theory.

Of course large cardinals arise only through iterations of the power set operation that go wildly beyond what is involved in formulating the continuum problem. My question to Hilary probably only makes sense on the supposition that we can do set theory while admitting that there is something either vague or

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[6] Thanks to Peter Koellner for correcting an earlier, inaccurate statement.

[7] In his reply to the papers presented, Putnam said that in his view "set theory is a part of science." I think he meant to imply that sets of reals, as well as higher power sets, are determinate.

ambiguous about the power set operation. The freedom with which set theorists talk about different models gives some encouragement to that idea. But it is very limited, since we don't regard all models of (let's say) ZFC as equally good representations of the universe of sets. And any claim that higher power sets are indeterminate would have to find some way of explaining away the categoricity argument originally proposed by Zermelo.

I don't mean to *assert* that the power set operation *is* vague or ambiguous. But it is an idea that I don't think has been decisively refuted, and I can't quite get myself to dismiss it. Hence my question.