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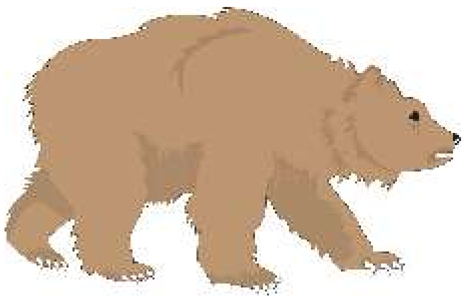
Ursus Philosophicus

Essays dedicated to Björn Haglund on his sixtieth birthday



Remarks on the alleged non-determinateness of certain mathematical concepts

Per Lindström

> "doubts more dubious than
> what is being doubted"

1. Introduction.

Most people know what the sequence ω:

0, 1, 2, ..., n, n+1, ...

of natural numbers looks like: (i) it has a first member 0; (ii) every member n of ω has an (immediate) successor n+1; (iii) different members of ω have different successors; (iv) every member of ω can be reached, starting from 0, by taking successors. In fact, these properties characterize ω up to isomorphism; and this is sufficient for the purposes of mathematics.

From a mathematical point of view, however, (iv) is unsatisfactory and so is usually replaced by the condition of (complete or mathematical) induction (v): Every set X (of natural numbers) which contains 0 and is such that for every member n of ω, if n is a member of X, so is n+1, every such set contains all natural numbers. (i), (ii), (iii), (v) are essentially what is known as the Peano axioms (due to Dedekind). The reason we accept (v), when it is pointed out to us or, like Fermat and Pascal, we discover it ourselves, is that we can see that, given (i), (ii), and (iii), (v) is implied by (in fact, equivalent to) (iv).

Thus, it would seem, we have a determinate conception of the sequence of natural numbers; there is, up to isomorphism, one and only one sequence satisfying this conception: we can see in our mind's eye what that sequence is like. But, although this seems quite clear, and is taken for granted as a matter of course by the vast majority of mathematicians, there are people, philosophers and others, who claim that it is not true, in other words, that we (they) do not have a determinate conception of the natural numbers. Hartry Field ([F]) is one example. His ideas will be discussed in §3.

But even if simple concepts such as *finite* and *natural number* are determinate, it may still be the case that other basic set-theoretic concepts are not. For example, the concepts *set*, *cardinal number*, and *ordinal number* are almost certainly non-determinate. This has been pointed out by Bernays ([Ber]), Putnam ([P]), and others: there is no inherent difference, as Cantor believed, between sets, on the one hand, and what he called "absolutely infinite or inconsistent multiplicities", that is, proper classes, on the other.

Another concept for which the question arises, if it is determinate, is the concept (*non-*)*denumerable*. Hilary Putnam ([P2]) and others have argued that it isn't. Putnam's ideas will be discussed in §2.

In discussing the ideas of Putnam and Field I have, with a few exceptions, restricted myself to objections, that are quite simple and sufficient to show that their arguments are wrong and why; I have not mentioned all possible objections to their ideas, by no means.

2. The concept *denumerable*.

As is well known Skolem, Putnam ([P2]), and others have argued that there is no (we have no) determinate concept (*non-*)*denumerable*. This, Putnam claims, follows from the (downward) Löwenheim-Skolem (LS) theorem applied to set theory (or to some more extensive first-order theory obtained from set theory by including the empirical constraints of natural science and our (true) everyday beliefs) in combination with any "naturalistic" overall philosophy. According to Putnam, in view of the LS theorem, "*the total use of the language*" [...] does not 'fix' a unique 'intended interpretation' [of set theory]", in fact, it does not even "fix" the interpretation of "(non-)denumerable". And neither does "understanding", since "what can our 'understanding' come to [...] which is more than *the way we use our language*?".

This implies, or is supposed to imply, that if M and M' are (standard) models of set theory and the same sentences are true in these models, there can be no sense in which one of them is an "intended" model of set theory whereas the other is an "unintended" model of set theory: all that matters is which sentences are true in M and M': "What we want to know as mathematicians is what sentences of set theory are true; we don't want to have the sets themselves in our hands." ([P2]) (Let us call this argument the *equivalence argument*.) In fact, Putnam goes on to argue, either there is a unique "intended" model of set theory or no (standard) model of set theory is "unintended". And so, since, as we have already mentioned, there is no unique “intended” model of set theory, there can be no (respectable) sense in which a (any) standard model of set theory is “unintended”.

This is certainly not a convincing argument. But we need not trouble ourselves with the details of the argument, since it is easy to see that Putnam's conclusion is false. Let M be a denumerable (standard) model of set theory. There is then a function f on ω onto the domain of M. (This is the definition of “denumerable”.) But it is provable in set theory, and therefore true in M, that there is no such function. It follows that f is not in M. (This is the standard explanation of the “Skolem paradox”.) Moreover, by Cantor's diagonal argument, the subset $\{n \in \omega: n \notin f(n)\}$ of ω is not in M. And, although “unintended” is not a precisely defined term, it would seem that, if M' is a standard model and we can show that there is a set of natural numbers not in M', and even give an example of such a set, we may conclude that M' is “unintended”. It follows that M is “unintended”. (Note that in this argument we do not beg the question by assuming that the set of subsets of ω is a well-defined (non-denumerable) set.) But, of course, one may feel that it is enough to observe that the function f is not in M.

What happens in Putnam's argument is this. First he assumes that you are allowed to describe the model M as denumerable, which presupposes that M can be looked at from “outside”; the function f mapping ω onto D is not a member of M. He then argues as if M is the “real world” and, therefore, can only be viewed from “inside”, in which case it may, of course, be impossible to show that M is “unintended”. But then the first part of the argument won't belong to our “total use of language”.

Thus, Putnam “proves” too much. But even though the arguments of Putnam, and others, are not correct, it doesn't follow that there is (we have) a determinate concept *non-denumerable*: this concept is certainly more difficult to grasp than, for example, the concept *finite*. (It doesn't even follow that not all models of set theory are “unintended”.) Whether or not there is a determinate concept *non-denumerable* has to be decided in some other way, for example, by contemplating the (full) binary tree with branches of length ω; this tree has non-denumerably many branches.

3. The concept *natural number*.

In [P2] there is also the quite different argument that sentences that are undecidable in set theory, such as the continuum hypothesis, are neither true nor false (on our "current" concept of set). (Let us call this the *incompleteness argument*.) Field feels that in [P2] "Putnam has raised a challenge that seems prima facie very difficult in principle to meet: explain in virtue of what we 'intend' a standard as opposed to a 'non-standard' interpretation of 'set' " ([F]). And Field now considers the possibility that the incompleteness argument can be used to show that we have no determinate concept *finite* or, equivalently, *natural number*. This idea he calls "radical indeterminism".

The basic thesis of radical indeterminism is that "at any time, a person's concept of [natural number] is exhausted by the maximal mathematical theory of [natural number] that he or she implicitly accepts at that time: more specifically, any mathematical claim about [natural numbers] that has different truth-values in different models of someone's maximal theory has no determinate truth-value on that person's current conception of [natural number]" ([F]).[1] On the basis of this thesis the radical indeterminist then argues that we don't have a determinate conception of the natural numbers. This, it is claimed, follows from the Gödel incompleteness theorem: the theory of natural number you "implicitly accept" is (axiomatizable and therefore necessarily) incomplete.

But it is quite clear that the question, if we have a determinate conception of the natural numbers, has nothing at all to do with Gödel's theorem. What is put in question is if we have a definite conception of the sequence ω of natural numbers. But the complete first-order theory of ω, whether formulated in terms of the successor function or the usual ordering of ω, is axiomatizable, in the latter case even finitely axiomatizable. Thus, Gödel's theorem does not apply and is therefore irrelevant. Indeed, all attempts to show that we

[1] You "*implicitly accept* a sentence if [you] could fairly easily be brought to explicitly accept it without feeling that [you were] learning anything essentially new; though this would require qualifications to rule out things [you were] brought to accept by fallacious arguments that [you] could be brought to see were fallacious" ([F]) (by another fallacious argument?). (Your "maximal theory" need not be closed under logical consequence.) But "accept" in what sense? Hold to be true? But then, true of what?

have no determinate conception of the natural numbers based on Gödel's theorem will fail for this (almost trivial) reason.[2]

Moreover, addition and multiplication can be uniquely defined, in terms of 0 and the successor function, by the usual inductive conditions. And so, if we have a determinate conception of ω, our concept of a standard model of arithmetic is surely fully determinate.

But, as in the case of non-denumerability, from the fact that one argument (in this case based on Gödel's theorem) fails to show that the concept *natural number* is non-determinate, it does not follow that this concept is determinate. And had there been at least some independent evidence that there is, or may be, something wrong with our concept of natural number, that we can't trust our intuitions, it would have been natural to listen to theories explaining exactly how and why.[3] But there is no such independent evidence – none – in the history of mathematics from antiquity to the present day. (You would expect, or at least hope, this would make Field, and others, stop and think – but NO.) All there is are a number of people, philosophers and others, who, for some reason, claim that we (they) have no determinate conception of the natural numbers. Here I agree with (a younger) Putnam that "[w]hen one is defending a commonsense position very often [...] one has to keep throwing the burden back to the other side, by asking to be told *precisely* what is 'unclear' about the notions being attacked" ([P]). For example, to conclude that "arithmetic totters", as Frege did, when faced with the Russell paradox, is simply poor judgement[4]; it is not arithmetic but (Frege's) philosophy that "totters".

Field's ideas (and Frege's) may be compared with Bernays' view shared, at least in practice, by almost all mathematicians, that "for number theory the use of the intuitive concept of a number is the most natural. In fact, one can thus establish the theory of numbers without introducing an axiom such as that of complete induction, or axioms of infinity like those of Dedekind and Russell" ([Ber]); in other words, (first-order) arithmetic

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[2] For another such attempt, see [D].

[3] This is what happened to, for example, such concepts as *real number*, *continuous function*, etc. They were found not to be sufficiently well-defined, by mathematicians not philosophers, and were subsequently clarified, by mathematicians not philosophers.

[4] Benacerraf ([Ben]): "If ["anything can be counted"], then Frege's analysis of number was correct, and he was right to remark, when presented with the Russell paradox, that 'Arithmetic totters'."

can, and perhaps even should, be based directly on the intuition of ω.[5] Indeed, in spite of the efforts of Field, and others, there is every reason to believe that we do have a perfectly determinate conception of the natural numbers and it is a mystery (to me) why anyone should, on such flimsy grounds, seriously doubt that we do.

4. Concluding comments.

"There is a reluctance in recent philosophy to envisage a perfectly definite concept which is incapable of a complete description but is somehow grasped by our intuition and serves as a guide to much of our pursuit in a given area. One is left wondering whether too exact a standard of clarity, while undoubtedly useful in excluding obscure and irresponsible speculations, may not also prevent us from doing full justice to our common heritage of a rich and inexhaustible intuition in our mathematical thinking." ([W], X.6.5) I completely agree – except that it is questionable (to say the least) if the "standard of clarity" of the views of, for example, Field and Putnam is more "exact" and their views more convincing than the mathematical concepts and ideas they dismiss as being "non-determinate". But exactly which (mathematical) concepts or conceptions are (fully) determinate, and in what sense, is not at all clear.

Much of the philosophy of mathematics today can, it seems to me, only be characterized as consisting of a number of rather frustrating cases of straining gnats (the concept *natural number*, truth in first-order arithmetic, anything "smelling of" the analytic/synthetic distinction, etc.) and swallowing camels (naturalism, holism, (radical) indeterminism, etc.).[6] Indeed, "one is inclined to ask for a deeper understanding and a better taste which, with most people, seem to come only after an actual experience of doing a subject" ([W], Introduction), if at all. And, surely, there are more interesting tasks for the philosophy of mathematics.

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[5] As I suggest in [L]. It wasn't until after [L] had appeared that I realized that my (very simple) idea is almost the same as Bernays' and had also appeared elsewhere in the literature (e.g. in [McN]) but, as far as I know, without attracting any attention at all. In the introduction to [B&P] Bernays' paper is mentioned just once and its content is not discussed.

[6] "Instead of [...] examining the courses of the misuses of the [analytic/synthetic] distinction, familiar arguments are employed to derive, without any conclusiveness, general theses as there being no distinction to be drawn, and to jump to mushy irresponsible slogans of wholism [sic!] and gradualism." ([W], VIII.1)

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Per Lindström

Gatan 3, 448 50 Tollered

Sweden

per.lindstrom @ mbox334.swipnet.se