SKOLEM AND THE SKEPTIC

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In 1922 Thoraf Skolem published 'Some Remarks on Axiomatized Set Theory'. This paper contains a powerful skeptical argument which, siren-like, has lured philosopher after philosopher at least to flirt with the disaster toward which it beckons. One of the argument's most recent and worthy naufragés is Hilary Putnam, who has deployed Skolem's argument as a major move in the realism/anti-realism wholly wars (Putnam [1977]).

The present paper is about Skolem, an imaginary being (The Skeptic), their respective views on set theory as a foundation for mathematics and on the foundations of set theory itself, and by way of commentary and afterthought, some uses to which Putnam has recently put these views and arguments.

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I

Introductory remarks

I presuppose some familiarity with the basic concepts of set theory, and, to some extent, with Zermelo [1908a]. For the reader without the latter, I include in an Appendix a list of Zermelo's axioms, as well as a very sketchy summary of the philosophical views he advances there, too sketchy to do them justice, but I hope sufficiently ample to make the present paper intelligible. The interested reader should consult Zermelo's
paper for a very straightforward presentation (of all except perhaps for the disputed aspects of Axiom III—see the discussion below).

My discussion will revolve around my reading of the extremely fecund Skolem [1922]:

It contains, among other things, (1) the ‘syntactic’ explication of Zermelo’s notion of ‘definite property’ as employed in Axiom III, the Aussonderungs axiom; (2) a suggested strengthening of Zermelo’s axioms by the addition of the axiom of substitution—whose customary attribution solely to Fraenkel is codified in the customary name ‘ZF’; (3) a simplified proof of Löwenheim’s theorem (amounting to an almost-proof of the Completeness Theorem for First Order Logic); (4) the renowned application of the Löwenheim-Skolem theorem in the argument that the concepts of axiomatic set theory are inexorably relative—most particularly, that there is no absolute concept of non-denumerability;¹ (5) that ZF (without the axiom of Foundation) has non-isomorphic models, specifically ones with infinite descending ε-chains; and more.

All this is well known.

But I read Skolem [1922] as a paper written to make a philosophical point. I will try to bring out what that point is and locate it in the development of Skolem’s own thought and in current debates on the foundations of set theory, mathematics, and philosophy.

II

The Skeptic

For ease of reference, I introduce ‘The Skeptic’. The Skeptic will turn out to be largely a skeptic, of sorts. But nothing will hang on that, and no effort will be expended in defense of the view that The Skeptic is a skeptic—i.e. that his views are in fact skeptical views. (That would require a deeper understanding of philosophical skepticism than I possess. And besides, as I said above, nothing will hang on that issue.)

¹ I will use ‘countable’, ‘enumerable’, and ‘denumerable’ as stylistic variants of one another (and similarly for their cognates). M is countable if and only if it is the range of a function (partial or total) whose domain is a subset of the natural numbers. (Thus, harmlessly, ‘countable’ includes ‘finite’. Where it matters, we will specify ‘countably infinite’.)
The Skeptic believes several things, most notoriously that the concepts ‘denumerable set’ and ‘non-denumerable set’ must be viewed as relative, not absolute, in a sense of ‘relative’ and ‘absolute’ that can be culled from an examination of his arguments. Indeed, the relativity in question is claimed for all set-theoretic concepts, but attention is usually focused on the cardinality concepts and the ones used to define them (finite, non-denumerable, denumerable, subset, etc.) largely, I think, because of their prominence in the development of Cantorian (and post-Cantorian) set theory. Set theory is a theory of cardinality (and ordinality), to put it contentiously.

The Skeptic also believes that set-theoretic concepts should be presented axiomatically, in first-order formalization. As we will see, expressing the view in terms of the notion of formalization may be a bit artificial and somewhat controversial. But ‘first-order’ is a necessary ingredient of the view; it is normally taken to be a syntactical concept which may itself require for its explanation at least a flirtation with the notion of formalization, if not a full employment of it. What ‘formalizability as a first-order theory’ amounts to—how it is to be explained—is a question of critical import, as the very applicability of the Skeptic’s principal argument depends on it.2

The Skeptic then does his familiar number:

2 It would be interesting, both historically and philosophically, to examine the role of formalization—representation in a logistic system—in the development of logic and its philosophy. We think of the Löwenheim-Skolem Theorems as ones that apply exclusively to formal languages, or at least to languages whose syntax is sufficiently well specified to be clearly first order languages—whatever that may amount to. But it’s not entirely clear that Skolem did.

It might well be (though this is hardly the place to examine the issue) that the concept of a logistic system and the notion of formalization on which it depends is an outgrowth of the Frege/Russell-Whitehead/Hilbert-Bernays/Carnap tradition in logic, whereas Löwenheim, Skolem, and others were bred from another, more algebraic strain, the Boole/Schröder tradition. There is philosophical substance here bearing directly on how the paper that is before us—Skolem [1922]—is to be interpreted: the intended thrust of Skolem’s arguments will vary with the background ‘premises’ he takes himself to be granted without argument. As will soon emerge, I claim Skolem [1922] to be arguing against The Skeptic that he is later to become. His paper will not make sense unless we suppose him to be granting himself premises (e.g. that some intuitive principles of model theory make sense) that The Skeptic would find unintelligible. But these principles (used in the proofs and subsequent interpretations of Löwenheim’s theorem and of Skolem’s generalization of it) are more intelligibly seen as in the Boole/Schröder algebraic tradition than in the strain to which Frege, Carnap, et al. belong.
The Skeptical Argument. Suppose that it has been 'proved' in some first-order set theory that there exist non-denumerable sets—perhaps by proving a theorem to the effect that some particular set, the power set of the integers (henceforth $P\mathbb{Z}_0$), say, is non-denumerable. If the theory is consistent, it has a countable model in which sets 'proved' to be uncountable within the theory must be denumerable, since the entire domain of the model can be enumerated. Hence 'uncountable' as determined (defined?) by the axioms applies to an object (a 'collection'), $P\mathbb{Z}_0$, which is provably countable from another standpoint. (We will, of course, return to this argument.)

Finally (one of his less publicized views): The Skeptic believes that set theory, thus construed, makes a suitable foundation for mathematics: mathematical axioms and concepts are to be reduced to those of set theory. Grafted onto such a reduction, the Skeptical argument banishes the (absolutely) non-denumerable from mathematics altogether.

Who ever held this view? Many, I suppose; including Skolem, of course. Several Skolems, actually. A typical one is the 1941 Skolem. In his paper 'Sur la portée du théorème de Löwenheim-Skolem' he argues that the Löwenheim-Skolem Theorem (henceforth 'LS', except where it matters which version is at issue) implies the relativity of cardinality and concludes:

Since all reasoning in axiomatic set theory or within a formal system is carried out in such a way that the absolutely non-denumerable does not exist, the claim that absolutely non-denumerable sets exist should be considered a mere pun; this absolute non-denumerability is therefore only a mere fiction. The true import of Löwenheim's theorem is precisely this critique of the absolutely non-denumerable. In short: this critique does not reduce the higher infinities of the simple theory of sets ad absurdum, it reduces them to non-objects. ([1941a], p. 468)

Two pages later:

... that axiomatization should lead to relativism is a fact sometimes considered to be a weak point of the axiomatic method. But without reason. The analysis of mathematical thought, the fixing of its fundamental hypotheses and
modes of reasoning can only be an advantage for science. It is not a weakness of a scientific method that it doesn’t yield the impossible. But it appears that most mathematicians are terrified that the absolute theory of sets should turn out to be an impossibility. ([1941a], p. 470)

A near cousin, the Skolem of 1958 says (in ‘Une relativisation des notions mathématiques fondamentales’):

My point of view is therefore that one should employ formal systems for the development of mathematical ideas. ([1958a], p. 634)

I do not understand why most mathematicians and logicians do not seem to be satisfied with this notion of set as defined by a formal system, but on the contrary speak of the inadequacy of the axiomatic method. Naturally this notion of set has a relative character: for it depends on the formal system chosen. But if this system is suitably chosen, one can, nevertheless, develop mathematics on its basis. ([1958a], p. 635)

To summarize, the Skeptic holds that:

(i) Set-theoretic concepts must be presented in axiomatic form, in first-order formalization, and that
(ii) Thus presented, set theory constitutes an adequate foundation for mathematics.

Applying LS, he then concludes that

(iii) set-theoretic notions [and the ensuing mathematical notions defined in terms of them] are relative, not absolute.

Notably, no ‘set’ is absolutely ‘non-denumerable’, because any ‘set’, ‘non-denumerable’ in some model of set theory, is ‘denumerable’ in some other.

So much for The Skeptic and the Skolems of [1941a] and [1958a]. They are a perfect match.

III

But what of the Skolem who first advanced the relativistic argument: Skolem [1922]? His intriguing paper is a blistering attack on Zermelo’s 1908 paper ‘Investigations in the
Foundations of Set Theory’, in which Zermelo makes three important claims:

(a) It is the task of set theory to serve as a foundational discipline for mathematics.
(b) Axiomatization is the way of salvation for set theory, its intuitive underpinnings having been cut away by the paradoxes.
(c) His axioms (now familiar to us) should be adopted as the appropriate foundation for this foundation.

Skolem takes exception to all three of these claims, with the conjunction of (a) and (b) bearing the brunt of his attack. Indeed, his principal point can be put:

Set theory, axiomatically presented, cannot serve as a foundation for mathematics.

Once more, by ‘axiomatically presented’ I take Skolem to mean something like ‘presented as a theory formalized (or formalizable) in the Lower Predicate Calculus (LPC)’, where that serves to guarantee that the meaning of the logical constants, and only the logical constants, is fixed. It is otherwise extremely unclear how LS (particularly the numerical version Skolem invokes) could be applied in the argument.

Skolem [1922]’s adversary—a near twin of Zermelo—occupies the following position:

(a) Set theory (and its fundamental concepts) must be presented axiomatically in first-order formalization.
(b) Thus presented, set theory (in particular, the system Z of Zermelo [1908a], suitably amended and dressed up as a first order theory) is an adequate foundation for mathematics.

Despite its striking resemblance to The Skeptic’s position (and to that adopted by the Skolem of [1941a] and [1958a]), this is the view Skolem [1922] attacks. To be perfectly explicit, what I am claiming is that, far from adopting The Skeptic’s position, Skolem in [1922] is arguing directly against it. I was long misled on this very issue because, seeing the relativity argument in [1922] I read back into that paper the view which that argument is normally used to support. But it is the burden of the first
portion of the present paper to argue that the relativity argument appears in [1922] as just one more nail in the coffin of Zermelo’s view—as a *reductio* of what happens when you maintain that the set-theoretic concepts are implicitly defined by the axioms: they end up relative. Skolem’s paper has eight sections and a conclusion. I will comment briefly on each.

§1. Presented axiomatically, Z (Zermelo’s Theory) can’t serve as a foundation for set theory because to present a theory axiomatically is to presuppose the transparently set-theoretic notion of a domain:

The entire content of this theory is, after all, as follows: for every domain in which the axioms hold, the further theorems of set theory also hold. But clearly it is somehow circular to reduce the notion of set to a general notion of domain. (p. 292)

He adds the further remark that it won’t do in reply to replace the general concept of domain with that of the particular universe B of sets; for if that were permissible, then if a question arose ‘about some unspecified set’ we could, *mutatis mutandis*, waive the general notion of set and say: ‘No general notion of set is needed, but only the idea of a single set that we assume to be given.’ (p. 292)

§2 contains a critique of Zermelo’s notion of ‘definite proposition’ in Axiom III, the Aussonderungs axiom (‘Probably no one will find Zermelo’s explanation of it satisfactory’ (p. 292)). Skolem proposes its replacement by the well-known syntactic definition, in which the troublesome notion of a ‘definite property’ is replaced by the *syntactic* concept of an open sentence with a single free variable. (For a little more on this, see fn. 4 below.)

For a brief moment, this will have the appearance of a *constructive* suggestion.

§3. Skolem offers a simplified proof\(^3\) of the following form of the Löwenheim-Skolem theorem:

\(^3\)The ‘simplified proof’ is one which avoids the axiom of choice (which Löwenheim employed in his original proof). Skolem motivates this avoidance by philosophical scruple: ‘... where we are concerned with the foundations of set theory, it will be desirable to avoid the principle of choice as well.’ (p. 293) That sounds unexceptionable. Still, it is worth noting in passing that such scruple is often excessive, and may be so in
If a denumerable set of First Order propositions is consistent (i.e., has a model), it has a model in the integers.

After presenting this proof, he notices that his 'clarification' in §2 of Zermelo's concept of a definite proposition has insured that the axioms of Z constitute 'a denumerable set of First Order propositions' (by reducing what was formerly an indefinite, or circular, or second order axiom to an infinite list of first order axioms). He concludes that Zermelo's axiom system, 'when made precise' (these are his words), must itself have a model in the integers, if it has any model at all.4

This sets the stage for the famous passage, the *locus classicus* of so-called 'Skolem Paradox' and the origin of the doctrine of the relativity of set-theoretic concepts:

So far as I know, no one has called attention to this peculiar and apparently paradoxical state of affairs. By virtue of the axioms we can prove the existence of higher cardinalities, of higher number classes, and so forth. How can it be, then, that the entire domain $B$ can already be enumerated by means of the finite positive integers? The explanation is not difficult to find. In the axiomatization, 'set' does not mean an arbitrarily defined collection; the 'sets' are nothing but objects that are connected with one another through certain relations expressed by the axioms. Hence there is no contradiction at all if a set $M$ of the domain $B$ is nondenumerable in the sense of the axiomatization; for this means merely that *within* $B$ there occurs no one-to-one mapping $\Phi$ of $M$ onto $\mathcal{Z}_0$ (Zermelo's number sequence). Nevertheless there exists the possibility of numbering all objects in $B$, and therefore also the elements of $M$, by

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this case. If the conclusion of the investigation is to be a skeptical one—if the form of the argument is a *reductio* of a set of propositions accepted by his adversary and that include AC among them, as in the present case, it is perfectly legitimate, *ad hominem*, to use propositions from the questionable set in the argument. However there do remain excellent reasons not to use AC in the present case. I mention two. (1) The relativistic conclusions are meant to apply to versions of set theory that don't include AC, and (2) the argument is more informative if it can be presented with weaker premises.

Although the use of AC in the proof of LS is largely (although not entirely) tangential to the matters that concern us here, I mention the passage to highlight the *philosophically contentious* character of Skolem's paper.

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4 *See Appendix 1.*
means of the positive integers; of course, such an enumeration too is a collection of certain parts, but this collection is not a 'set' (that is, it does not occur in the domain \( B \).) (p. 295)

Before returning (in section E below) to discuss this argument in some detail, I will complete my survey of Skolem's paper. We are still in Section 3 and Skolem has a few more shots to take. He suggests (somewhat obscurely) what one might take as the first hint (1) of the existence of non-standard (non-isomorphic) models for the Zermelo integers and (2) of a good reason for it.

Likewise, the notion 'simply infinite sequence' or that of the Dedekind 'chain' has only relative significance. If \( Z \) is a set having the property required by Axiom VII, Zermelo's number sequence \( Z_\circ \) is defined as the intersection of all subsets of \( Z \) that have the same (chain) property. But being a subset of \( Z \) is not merely to be in some way definable, and nothing can prevent a priori the possibility that there exist two different Zermelo domains \( B \) and \( B' \) for which different \( Z_\circ \) would result. (pp. 295-6)

He concludes §3: 'Thus axiomatizing set theory leads to a relativity of set-theoretic notions, and this relativity is inseparably bound up with every thoroughgoing axiomatization' (p. 296; his emphasis). Given Skolem's own work in model-theory and his earlier remarks about the set-theoretic character of model-theoretic reasoning, it is difficult to escape the conclusion that his target here is not set-theoretic concepts in general, but only axiomatically presented (i.e., implicitly defined) set-theoretic concepts. He ends the section with two remarks, both of which seem to me to support this interpretation. Again, he does best speaking for himself:

With a suitable axiomatic basis, therefore, the theorems of set theory can be made to hold in a merely verbal sense, on the assumption, of course, that the axiomatization is consistent; but this rests merely upon the fact that the use of the word 'set' has been regulated in a suitable way. We shall always be able to define collections that are not called sets; if we were to call them sets, however, the theorems of set theory would cease to hold. (p. 296)
In other words, implicitly define 'set' and, modulo consistency, you can’t go wrong. But the arguments of §3 show that any such fixing of the concept ‘set’ will leave out collections which, relative to that fixing, we don’t happen to be calling ‘sets’ (but which deserve the name nonetheless).

There is a gap in the argument here, even construed as ad hominem against Zermelo. At most what has been shown in §3 is that given any denumerable set of first order axioms A (presumably that are in some sense recognizably set-theoretic), and any model M for these axioms, there are collections (B₁, . . . , Bᵢ, . . .) definable on M, that don’t belong to M. It has not been shown that the Bᵢ aren’t sets in the sense defined by the axiomatization (whatever that may be). The Bᵢ depend not only on A, but on M as well, and no argument has been given that there is no model M’ to which these very Bᵢ belong, thus legitimizing their sethood. This opens the question of the very meaning of the claim of relativity: Is it relativity to an axiomatization? To a model, given an axiomatization? Both?

Without straining charity, we can fill the gap and interpret him as saying that any denumerable system A of first-order axioms has denumerable models if it has any at all; now, given A and a denumerable model M for A, there are collections not provided for by M. Hence defining ‘set’ by fixing A won’t adequately fix the meaning of ‘set’.

This, remember, is Skolem.

§4 is a constructive attack on Z, pointing out that although Z₀, PZ₀, PPZ₀, . . . all can be proved to exist, the union of all the members of the sequence cannot. He suggests what we now know as the Axiom of Substitution, in the form: The range of any function whose domain is a set is itself a set. This is the axiom that we think of as Fraenkel’s contribution (and which Fraenkel did indeed obtain independently).

§5 is a complaint against the impredicativity of Zermelo’s axioms considered as generating principles. And the complaint is a narrowly philosophical one: The impredicative nature of the Aussonderungs axiom, in the presence of the axiom of infinity (he notes that without it there is a model in the finite sets), renders proof of consistency by constructive methods unlikely (re-
member, consistency for Skolem is a semantical concept—it means: having a model). He appears to make two points. (1) It is unlikely that a constructive proof can be found, i.e., that a suitable model can be constructed and (2) even if one were found, it wouldn’t be available to the likes of Zermelo, Russell, Whitehead, etc., since it would depend on the concept of integer that they all require be defined in set theory—once more Skolem is arguing *ad hominem* against those who would reduce mathematics to some version of set or type theory.

Commenting on what appears to be this passage, Jon Barwise astutely notices Skolem’s anti-Skeptic bent and remarks:

Skolem’s suggestion . . . that we use ‘first order property’ for ‘definite property’ explicitly presupposes that one is willing to form the universe $V$ of all sets,

$$V = \bigcup \alpha R_{\alpha}$$

(the union taken over all ordinals) and consider it a meaningful mathematical totality. In fact this was one of Skolem’s arguments against using set theory as a foundation for mathematics.$^5$

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$^5$ K. J. Barwise ([1972], p. 593). It is perhaps worth noting that Barwise’s remark appears to be too strong in two ways. (1) Unquestionably, the natural reading of the resulting theory requires one to quantify over all sets. Whether this amounts to a willingness to form $\bigcup \alpha R_{\alpha}$ and consider it to be a meaningful mathematical totality depends upon whether the right interpretation of quantifiers involves such a willingness. I hope not. (2) Secondly, the impredicativity appears to infect numerical models as well, as Skolem was perhaps aware (hence the ‘difficulty’ of producing a Löwenheim model which, for Skolem, would have been a numerical one). It appears to depend on the first order structure of $\mathbb{Z}$ ($\mathbb{Z}^F$) not on its interpretation in terms of sets.

It is unclear what this comes to. Someone might counter, maybe Barwise himself: ‘The impredicativity argument is often directed at the $\mathbb{Z}$ axioms (and others) when considered as generating principles for the elements of the domain. Viewed as descriptive of the relations that obtain among the elements of the domain, they are unexceptionable (See Gödel [1944]). So the numerical models are immune because we have reasons independent of the $\mathbb{Z}$ axioms for believing in the existence of the numbers.’ But I would find such a reply unconvincing, because what’s at issue in an alleged numerical model for $\mathbb{Z}$ may well be the truth of the claims of the model about the integers. What reason could we have for thinking that the integers of the model are related to one another as the axioms specify? What are the properties attributed by the axioms? Any ‘construction’ of a numerical model would be a proof that the axioms held in some domain consisting of integers and would thus imply the consistency of $\mathbb{Z}$. It would therefore be a mathematical theory of considerable complexity.
§6 contains another pot shot: Your axioms don’t ‘uniquely determine’ your domain, pointing out the possibility of two kinds of models; those that are well-founded and those that are not. If ‘uniquely’ is to be interpreted as ‘up to isomorphism’, the cardinality arguments already establish that. However, the dialectical position is such that he may not have wanted to avail himself of notions he had argued were unavailable to his opponent—such as the cardinality concepts. In any event, although it is not clear that he had a proof of it, the result is of interest in its own right and independent of cardinality and holds within each cardinality.

§7 contains a discussion of the difficulties Zermelo’s view poses for foundational investigations of set theory (consistency and independence proofs): These can proceed set-theoretically, by constructing set models and counter-models, in which case they beg the question; or they can proceed syntactically, in which case they presuppose the concept of an ‘arbitrary finite number’ [of applications of the axioms], and thereby, as we have seen Zermelo [1930] was later also to argue, concepts destined to be reduced to set-theoretic terms. This would make them viciously circular. There follows a beautifully anti-Zermelo, anti-Skeptic passage:

> Set-theoreticians are usually of the opinion that the notion of integer should be defined and that the principle of mathematical induction should be proved. But it is clear that we cannot define or prove ad infinitum; sooner or later we come to something that is not further definable or provable. Our only concern, then, should be that the initial foundations be something immediately clear, natural, and not open to question. This condition is satisfied by the notion of integer and by inductive inferences, but it is decidedly not satisfied by set-theoretic axioms of the type of Zermelo’s or anything else of that kind; if we were to accept the reduction of the former notions to the latter, the set-theoretic notions would have to be simpler than mathematical induction, and reasoning with them less open to question, but this runs entirely counter to the actual state of affairs. (p. 299)

He ends the section with a similar diatribe against Hilbert.
§8 is a comment about the Axiom of Choice, that further reveals his own bias—evident throughout the paper—for an intuitive and probably predicative conception of set. It is brief and can be quoted in full.

8. So long as we are on purely axiomatic ground there is, of course, nothing special to be remarked concerning the principle of choice (though, as a matter of fact, new sets are not generated univocally by applications of this axiom); but if many mathematicians—indeed, I believe, most of them—do not want to accept the principle of choice, it is because they do not have an axiomatic conception of set theory at all. They think of sets as given by specification of arbitrary collections; but then they also demand that every set be definable. We can, after all, ask: What does it mean for a set to exist if it can perhaps never be defined? It seems clear that this existence can be only a manner of speaking, which can lead only to purely formal propositions—perhaps made up of very beautiful words—about objects called sets. But most mathematicians want mathematics to deal, ultimately, with performable computing operations and not to consist of formal propositions about objects called this or that. (p. 300)

In his Conclusion, which is certainly susceptible to misunderstanding if you read only the first sentence, ‘The most important result above is that set-theoretic notions are relative’, Skolem makes it plain that he is not The Skeptic: Although he had obtained these results six or seven years back, he hadn’t published them because he ‘believed that it was so clear that axiomatization in terms of sets was not a satisfactory foundation for mathematics that mathematicians would, for the most part, not be very much concerned with it’. But they were.

IV

Why the turnabout?
Somewhere between 1922 and 1941 Skolem drops his reliance on intuitive mathematics, and with it, the anti-reductionist arguments; but he continues to accept all the arguments for relativity and non-categoricity. Whereas in 1922 these latter arguments had functioned as reductio arguments of Zermelo's
view, they are now used to point to the inescapable consequences for the philosophy of mathematics of the only tenable foundational position: the very brand of reductionism he attacked in [1922].

I don’t know why he changed. I don’t even know of any passage in which he explicitly recants, although it appears that [1929b] may be a transitional paper in precisely this regard. Its first two sections are entitled ‘Proof of (the) set-theoretical relativity’ and ‘Proof of the Relativity Without Appeal to Löwenheim’s Theorems’ and contain arguments that are largely the same as those offered in [1922]. Unlike [1922], in which ‘axiomatic’ was present as a qualifier to the set theory whose concepts were being shown to be relative, here it is dropped: It is simply ‘set theory’ whose concepts are said to be relative; not ‘axiomatic’ set theory (recall: ‘. . . on an axiomatic basis higher infinities exist only in a relative sense ([1922], p. 296; his emphasis))). The argument remains the same, but greater generality is claimed for the conclusion—as if Skolem had, in the interim, accepted the proposition that the only viable way to do set theory was axiomatically (in Zermelo’s sense). The dropping of the qualifier appears at this point to have been simply a slip—almost as if he had been persuaded of the relativist conclusion and simply detached it whereas before he had contraposed: As someone very astute once pointed out: One person’s modus tollens is another’s modus ponens.

Although this may not help explain the change, it seems clear that Skolem became wedded to formalizing theories in LPC (note his work on reducing type-theoretical formalisms in LPC—in [1961a] he brandishes the result of Gilmore [1957], that the simple theory of types is ‘translatable’ into a first order theory, in an effort to ward off objections to his work based on second-order versions of ZF). When he noted [1933d, 1934b] that arithmetic, which until that point had played the role of an

6 Perhaps it’s not worth speculating about. Addressing this question, G. Kreisel remarked ‘You should not ask this; you see, Skolem didn’t have a philosophical bone in his body.’

7 In a way, he argues for this, but only in [1941], where he surveys only three kinds of set theory as if these were all there were: ‘simple’—i.e. simple-minded (= naive); axiomatic—i.e. à-la-Zermelo; and ‘logico-formal’—i.e. Russell-Whitehead Types. The intuitive ‘ordinary set theory’ of [1922] has dropped away, even as an option to be rejected.
intuitive foundational discipline, was similarly non-categorical, and that the deeper reason for it was what we might call the ‘unformalizability’ of the induction axiom because of its reference to all subsets of the integers, he accepted the obvious consequences. There was no longer a foundational discipline left which could rival axiomatic set theory by being free of its defects. Inductive inferences, which he had declared in 1922 to be ‘clear, natural, and not open to question’ were now infected with the same diseases as was set theory—perhaps because of their own ‘dependence’ on an unformalizable notion of set.8

This is a turning point. He could take this new evidence (the unformalizability of the concept of number) as a reason for rejecting as too demanding relativistic arguments based on the unformalizability of the key notions. Or he could retain the arguments and extend his ‘skepticism’ to whatever concepts were ever to be found to be similarly unformalizable, including those he had previously accepted as perfectly clear and unproblematic. He evidently chose the latter. So, we find him in 1958 unable to ‘understand why most mathematicians and logicians don’t seem to be satisfied by the notion of set as defined by a formal system’.

But that this is in fact how he reasoned is sheer speculation.

V

A. The Skeptic’s central argument

This section will contain an analysis of the passage from Skolem [1922] quoted above, and some general remarks concerning ways that Skolem’s argument (which I will find unsatisfactory) could be strengthened, as an argument against Zermelo, in an effort to discover both its appeal and its Achilles heel.

8 It is of some interest to speculate what should be made of the reductionistic arguments—more specifically those that reduce all mathematical concepts to those of some formalized axiomatic set theory—in a setting in which the very notion of formal system is drawn into question by the newly discovered instability in the concept of number. It is no longer just the alleged circularity that is in question, but the very definiteness of the formal systems to which the reduction is meant to be carried out. It can no longer be argued that the concepts of mathematics (including that of ‘finite iteration’) are relative to their particular embodiment in a given specified formal system: the very notion of a definitely specifiable formal system has now been undermined by the very relativistic and reductionistic arguments that led Skolem to use it as a grounding conception. Suppose, for example, that some provable sentence (‘0 = 1’ say) had a non-standard integer as the number of lines in its proof . . . I owe this example to Hilary Putnam.
Let us return then to Skolem's central argument for relativity, since that is perhaps the most distinctive feature of The Skeptic's position. In the classical passage in which it is presented Skolem is explaining how it can be that a set—PZ₀, say—can be provably 'non-denumerable' and yet, because of its embedding in a denumerable model, be denumerable as well. This is the core of the argument that cardinality concepts are relative: M is non-denumerable in the model B but denumerable from some external standpoint. As it stands, the argument is inconclusive, because there is an obvious equivocation on 'denumerable' and related notions. In the model B, something is denumerable if it is an integer that bears a certain relation—the '∈' of the model—to another integer—the designation in B of the term 'Z₀', the term that represents the 'set' of Zermelo integers within the theory. But what is it for a set to be non-denumerable? To see, we must look at the definition of denumerability:

\[
\text{Den}(x) = _{df} \exists f (f \text{ is a } 1-1 \text{ function with domain } Z_0 \text{ and range } = x)
\]

So, putting 'M' for 'x', M is non-denumerable iff there is no such f. So much for what happens within the theory, and therefore within the model B. The argument continues, however, by showing that, since the domain B is itself denumerable, and since M is a subset of it, M must also be denumerable, no matter what the theory says.

At this point, Zermelo should resist, since the latter part of Skolem's argument employs the outlawed intuitive set-theoretic reasoning, treating B as a real set, M as a subset of it, etc. . . M started out as a number, but all of a sudden it has sprouted 'elements', without which the 'therefore' of the last line on p. 92 above would be very hard to justify. As it stands, therefore, the argument is merely a complicated equivocation. To give it some semblance of plausibility Skolem must do one of two things:

(a) Run it entirely in terms of some intuitive notion of set—which would render the argument useless against an antagonist like Zermelo, who rejects such notions, or (b) move to eliminate the intuitive concept of set altogether and run the argument through entirely in terms of numerical models, continuing to base it on the numerical version of the Löwenheim-Skolem Theorem.
That too risks making it useless against Zermelo (or another reductionist), but for a different reason: it employs a concept of number available only through reduction to the theory itself. If, however, it could be run within ZF or a near cousin of it, in terms of that theory's own 'numbers,' the reductionist's objection may be blunted.

I will explore both possibilities, somewhat deviously, by 'trying' to generate a genuine paradox and seeing where the attempt fails (for there is none); I will then explore what went wrong and to what extent the arguments can be patched up, if not to produce paradox, at least to obtain some form of relativity. Perhaps we can help Skolem in his fight against Zermelo.

First, let me state two versions of the Löwenheim-Skolem Theorem which it will prove useful to have before us:

N (the numerical version); Any consistent countable set of First Order sentences has a model in the integers.

SMT (a transitive submodel version): Any transitive model for ZF has a transitive countable submodel. (A model is transitive if and only if each element of each set in the model belongs to the domain of the model.)

What follows is an (admittedly feeble) attempt at generating a paradox from the elements at our disposal.

A first try:

(i) If ZF is consistent, it has a model M whose elements are 1, 2, 3, . . . (by N, above).

(ii) But we can prove in ZF that non-denumerable sets exist, more particularly, that the power set of the integers is non-denumerable:

\[ ZF \vdash \text{Den}(\mathcal{P}\omega) \] [call this sentence 'T']

(a) T is therefore true in every model of ZF; and

(b) T says that \( \mathcal{P}\omega \) is non-denumerable, therefore

(1) T is true iff \( \mathcal{P}\omega \) is non-denumerable, and

(2) T is true in a given model M iff \( (\mathcal{P}\omega)_M \) is non-denumerable.
(iii) But no element of a denumerable model for ZF can be non-denumerable.

Therefore

(iv) If ZF is consistent, it has a model $M$ in which $(\mathbb{PZ}_0)_M$ is denumerable (by (iii)) and non-denumerable (by (ii b 2)).

This is unacceptable; but happily, glaring blunders abound. To begin with, (iii) is clearly false (although Skolem clearly assumes it). There is no result in model theory that implies that the set of real numbers in the unit interval cannot be an element of some countable model for ZF—so long as its members aren’t also elements of that model, i.e., so long as the model isn’t transitive. Furthermore, it is hard to maintain that there is any recognizable sense in which $T$ says that $(\mathbb{PZ}_0)_M$ is non-denumerable (ii above), if $M$ is a numerical model. So if we are to generate even an air of paradox, we should scrap (iii), moving to transitive models, and also leave the realm of numerical models for the more rarified one of set models. We will return to numerical models below.

A second try:

(i') Any transitive set model $M$ for ZF has a transitive denumerable submodel $M'$ in which $\varepsilon_M \subseteq \varepsilon_{M'}$.

(ii') $T$ is a theorem of ZF and says that $\mathbb{PZ}_0$ is non-denumerable in any model whose domain consists of sets and in which ‘$\varepsilon$’ denotes the membership relation. $T$ is therefore true in such a model $M'$ iff $(\mathbb{PZ}_0)_M'$ is non-denumerable.

(iii') No element of a transitive countable model for ZF is non-denumerable.

Therefore

(iv') $(\mathbb{PZ}_0)_M'$ is non-denumerable (by (ii')), and denumerable (by (iii')).

This looks more serious. (i') is essentially what we have called SMT, the transitive sub-model theorem. Further, since in a transitive set model every element of the domain is a subset of
the domain (that is the precise import of transitivity), \((iii')\) is true as well. This leaves \((ii')\) as the obvious culprit.

Now it pays to look at \(T\).

\[
T) \; \exists f [ f \text{ is } 1-1 \& \text{ Dom}(f) = \mathbb{Z}_0 \& \text{ Range}(f) = \mathbb{PZ}_0 ]
\]

So, the interpretation of \(T\) in a denumerable model—even in a denumerable transitive submodel of the standard model—is not sufficient to guarantee that \(T\), on that interpretation, \(\text{says that } \mathbb{PZ}_0 \text{ is non-denumerable—i.e. not every such interpretation of } T \text{ is one in which } T \text{ says of the referent of } \text{'PZ}_0\text{' in that interpretation that it is non-denumerable. That's a proposition that is not invariantly expressed by } T \text{ over all transitive submodels of a given model, even if that model starts out as somehow 'standard'—i.e. as the (an) 'intended' model. The conclusion is obvious; whether } T \text{ says that a set is non-denumerable depends on more than whether the interpretation is over a domain of sets, 'e' of the interpretation coincides with membership among those sets, and every element of any set in the model is also in the model. The universal quantifier has to mean all, or at least all sets—or at least it must range over a domain wide enough to include 'enough' of the subsets of } \mathbb{Z}_0.\]

But as we have already seen, this first reply is useless to Zermelo against Skolem since Zermelo must do without the intuitive concept of set in terms of which it is framed—just as the argument exhibited there is useless to Skolem against Zermelo. Does Skolem have any cards left to play, or must he rely upon an intuitive concept of set in forcing a relativistic conclusion on Zermelo?

The following might help. (Call this the Supermodel Theorem, as we start with a model \(N\) and build it up to a supermodel \(N'\):

If ZF is consistent it has

\((a)\) numerical models \(N\) and \(N'\) such that \(N \subseteq N'\), i.e. \([\text{Dom}(N) \subseteq \text{Dom}(N') \text{ and } \in_N \subseteq \in_{N'}\], and

\((b)\) an element \(P\) of \(N\) (and \(N'\)) which is \(\text{Den}_N\) and \(\text{Den}_{N'}\).\n
\(^9\)For a proof of the Supermodel theorem, see Clifton McIntosh's fine treatment of some of these problems in "Nous", 1979; although we do not have exactly the same view of Skolem, I am indebted to his discussion in a number of places.
So, 'denumerability' is relative to the model, since the very same object is 'non-denumerable' from the standpoint of one model (N) and yet 'denumerable' from the standpoint of a supermodel (N'). This avoids the submodel version of the Löwenheim-Skolem theorem, and is therefore usable against Zermelo. In both cases 'denumerable' and 'non-denumerable' are numerical concepts, hence Skolem's equivocation between a 'numerical' and a set-theoretic sense of the set-theoretic concepts doesn't obstruct the argument (as an *ad hominem* argument against Zermelo).

One final note: all of these arguments, including Skolem's, can be formalized in ZF+ (ZF augmented by a truth-predicate), which Zermelo would (presumably) accept. So there is a sense in which they are *all* acceptable to Zermelo. The problem then becomes, of course: How are we to understand the formulas of ZF+ which express the premises and conclusions? Set-theoretically? Or numerically? If they are consistent, they have a numerical model.

**B. General Remarks**

In this subsection I will offer a few remarks by way of commentary. They are meant to indicate the direction in which I feel we should grope for a resolution, not as that resolution itself.

One important reason for constructing a theory of sets is to represent the intuitive content of Cantor's Theorem. That content is not preserved in a restricted model of some First Order Theory whose existence is guaranteed by one of the Löwenheim-Skolem Theorems. Any interpretation I of ZF on which one can define a function from \((PZ_0)_1\) onto \(Z_0\) is *eo ipso* an inadequate interpretation. There *should be* no such functions.

Therefore, not every model of the axioms is an admissible interpretation—if one is doing set theory (although they might be for other purposes).

Some pictures will make this *seem* unduly restrictive. Once we have written down some axioms, they should be able somehow to stand on their own. Yet they don't wear their interpretation upon their sleeves. So, perhaps we should supply an interpretation, to forestall misunderstandings.

But an interpretation is just a further sentence correlating
predicates with extensions in some fixed domain. A kind of super axiom.

Generalized, this picture can prove fatal. But the apparent need for such a further sentence stems from confusing a language without an interpretation explicitly and visibly attached to it with an uninterpreted language—with one that is open to all models (and non-models) as admissible interpretations. That is a confusion—an understandable confusion but a confusion nevertheless. To point it out should suffice to block the arguments for relativity, because it takes the language out of the range of applicability of the Löwenheim argument. Only the axiomatic presentation of a theory meant to be understood model-theoretically is subject to Skolem’s relativistic claims. And even then.

Skolem [1922] argued against a formalist or conventionalist position in mathematics, defending [an unspecified portion of] intuitive mathematics as the core on which all of it is based. He saw the axiomatization and formalization of set theory as attempts to escape from intuitive mathematics and found mathematics on an empty shell. It makes no sense, he argues, because (1) it presupposes for its very formulation some intuitive model-theoretic notions that look suspiciously set-theoretical: A domain and relations on that domain—satisfaction of the theorems in every model of the axioms; (2) it presupposes for its investigation (the consistency problem) the acceptance at least of independently established principles, either of set theory, of arithmetic, or of (what we now call) syntax [indeed Skolem argues that it presupposes these for its formulation (see (4) below)]; (3) as a foundational enterprise, it is clearly less secure than the mathematics it attempts to found; (4) the particular axiomatization chosen suffers from a number of unclarities and seemed insufficient to do justice to ‘the usual theory of sets’. These unclarities concerned Zermelo’s notion of ‘definite property’ and one of the insufficiencies was the weakness to be remedied by the axiom of substitution. Both depend, in the formulation Skolem gives, on the concept of an arbitrary finite number (which is used to define the concept of formula needed to replace the notion of definite property or function). At the end (e.g. [1958a]) we see Skolem holding precisely the position he had criticized in 1922, hailing as virtues what he had
condemned as some of its most glaring faults; he nevertheless recognizes what he had not explicitly recognized in 1922—that the complete characterization of the number sequence is parasitic on the set-theoretic notions he claims are relative, thereby dragging the numbers down into the mire as well (if to lack a complete first-order characterization be to be bemired). It is hard to say how seriously he takes this, as he seldom speaks of the integers as 'shadowy non-entities' which, chameleon-like, change their plumage with each new interpretation of set theory. He says as late as 1955 in ‘Peano’s Axiom and Models of Arithmetic’:

It was then to be expected that if we try to characterize the number series by axioms, for example by Peano’s, using the reasoning with sets given axiomatically . . . we would not obtain a complete characterization . . . This fact can be expressed by saying that besides the usual number series other models exist of the number theory given by Peano’s axioms or any similar axiom system.” ([1955d] p. 587, my italics).

Given the philosophical view he held at that time, he has no right to put the matter in this way, because he can have no concept of ‘the usual number series’, any more than he has a concept of all subsets of the integers. Yet he is clearly right—that is the right way to put the point. Just as the correct reply to Zermelo was that allowing just any model of his axioms to count as a domain of sets won’t get you much set theory back (though ‘thin’ models are a wonderful tool for the foundational examination of those axioms). The meaning of ‘∈’ and the range of the quantifiers must constrain the class of permissible interpretations if the formalized version is to retain the connection with intuitive mathematics with which set theory began—if it is to be a formalization of set theory.

In 1958, Alfred Tarski attempted a reply to Skolem along similar lines, but one that, in my opinion, ultimately fails. Let me put Tarski’s point and the reply I feel Skolem should make (no reply was recorded) somewhat fancifully, in the form of a brief dialogue:

10 In the discussion following Skolem [1958a], p. 638. He also makes the ‘upwards’ LS reply discussed below. Skolem had complained about that reply (in [1955c], p. 583) that, given relativity, what could the replier mean by non-denumerable?
Tarski: It is hardly surprising that treating ‘∈’ as an uninterpreted symbol of the theory leaves you with the unintended denumerable models. If, however, you treated ‘∈’ as one of the logical constants, on a par with the quantifiers, negation and material implication, the denumerable models would no longer be possible. The meaning of ‘∈’ would have to be preserved in each interpretation.

Skolem: As you know, Löwenheim’s theorem (I have always resisted grafting my own name onto it) comes in a submodel version: To any infinite model M of a theory T there corresponds a denumerable submodel M’ of T, in which the domain of M’ ⊆ domain of M and the properties and relations of M’ are just those of M, cut down to the narrower domain. So, even models in which ‘∈’ means membership can be denumerable. Treating ‘∈’ as a logical constant isn’t enough—unless part of fixing its meaning involves stocking the quantifier domain with the right materials, in this case, enough sets. But this would set ‘∈’ sufficiently apart from the ‘other’ logical constants to make it questionable that it should be called one at all. Besides, you yourself were the first to point out in a very illuminating way the arbitrariness of the choice of logical constants, in the absence of a theory that would pick them out. We still lack such a theory, so your suggestion strikes me as very much ad hoc.

There are, of course, other replies to The Skeptic. Perhaps the most interesting come from the radical wing and invoke the ‘upwards’ Löwenheim-Skolem Theorem: If a theory has any infinite models at all, it has models of every infinite cardinality. Although there isn’t space to discuss them fully, it might be helpful to make a remark or two.

First, the use of this theorem serves well to bring out of the closet The Skeptic’s bias for the finite, or at least the at-most-denumerable: Less is better, or at least more intelligible. To heighten the contrast, the Upwards Skeptic chooses his

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11 Skolem explicitly takes the position that the meaning of the predicates is preserved in the submodel version—cf. [1941a], p. 455.
favorite infinite cardinal, say aleph_17, and argues, *mutatis mutandis*, that there is no absolute concept of non-aleph_17-infinite, since every attempt to specify such a cardinality by axioms must fail: If the theory in which the set in question is embedded is not one all of whose models are finite, then it has a model in which the set has aleph_17 members. Who is to say otherwise? Failing a convincing _foundational_ argument, we seem to be at a standstill. This, in itself, is a fairly good _indirect_ argument against the relativity that The Skeptic is urging on us. His argument is _too_ good: By keeping exactly the same things fixed (the meanings of the logical constants) we seem to be able to show that any infinite cardinality other than aleph_17 is relative.

But, of course, this is grist for the _clever_ Skeptic’s mill: If the Upwards Skeptic has a point at all, things are even worse than even The Skeptic had suspected; if he does not, since the entire argument is interpretable with the denumerable models The Skeptic prefers, it shows nothing at all. It is for this reason that I don’t find the Upwards Skeptic a fully convincing tool to wield against the convinced (downwards) Skeptic.

Interestingly enough, nowhere in Skolem have I found an argument that supports or even argues for the conclusion that only first-order structure matters, or makes mathematical sense. Nor, by the way, is there any argument to support the related underlying claim that first-order structure itself is _sacrosanct_: Why isn’t there a _further_ relativity due, say, to the possibility of interpreting quantifiers and negation intuitionistically? But to explore the implications of this line of reasoning would take us too far afield.

The reason we don’t have a Skolem Paradox, and the reason set-theoretic notions _aren’t_ relative in Skolem’s sense, is that there is no reason to treat set theory model-theoretically—which is _not_ to say that there is no reason to study its model theory. What a set-theoretical statement _says_ can simply not be identified with what is invariant under all classical models of the theory (i.e., models that allow ‘∈’ to take on whatever binary relation will satisfy the axioms). Tarski’s point, when divorced from the suggestion that ‘∈’ is a logical constant, is precisely the right one. Axiomatized set theory took its roots in informal mathematics and in order to retain its sense through axio-
matization and formalization it must retain these points of contact.

After all, Zermelo (and Skolem, as we saw in connection with the axiom of replacement) chose these particular axioms because they give us these theorems, which have an independent intuitive meaning of their own. Why turn our backs on this ancestry and pretend that the sole determinant of meaning of the axioms is their first order structure? We saw above that even when he is trying very hard to think about the problem in this formalistic way (in connection now with number theory) Skolem lapses into sense and describes the situation quite accurately—but in a way that would make no sense if his general position were correct.

The Skolem I presented to you is a man divided—seduced by a misunderstanding of the import of his own arguments. What his view needs to substantiate it is no less than the principle that the only mathematical concepts we have are those that are embodied by all models of our mathematical theories under first order formalization. But that is a principle whose very expression requires mathematical concepts (models, first order theories, etc.) which themselves have—and must be taken to have—intuitive content if it is itself to make sense. Clearly the Skolem of [1922]—the locus classicus of The Skeptic’s most seductive argument—did not hold that view.

Equally clearly, the Skolem of [1958a] did. Paradoxically, (and this is the real Skolem paradox) he adopted it after brilliantly unfolding the disastrous consequences of that position in its earlier Zermeloite incarnation.

VI

Putnam: Logocentricity

Putnam (or The Skeptic) will argue that the shoe belongs on the other foot—that in urging The Skeptic’s 'paradoxes' on us they are not claiming, at least not directly—that the only concepts that make sense are those imbedded in theories whose first order axioms determine their models up to isomorphism, or that what sense others make is limited to what structure is preserved by all models. The more moderate (and charitable) view of their position is one that sees their flaunting of this 'paradox in the philosophy of language', as Putnam calls it, as a means of posing a challenge: it is up to us 'platonists' or 'realists' to make
clear what we mean when we present theories whose model theories don’t have that form of pristine simplicity. Witness:

> The philosophical problem appears at just this point. If we are told, ‘axiomatic set theory does not capture the intuitive notion of a set’, then it is natural to think that something else—our ‘understanding’—does capture it. But what can our ‘understanding’ come to, at least for a naturalistically minded philosopher, which is more than the way we use our language? And the Skolem argument can be extended . . . to show that total use of the language (operational plus theoretical constraints) does not ‘fix’ a unique ‘intended interpretation’ any more than axiomatic set theory by itself does. (Putnam [1977], p. 424)

But this gives the show away. Of course no explanation can be satisfactory. It is obvious why. The reason resides in our logocentric predicament: Any explanation must consist of additional words. Words which themselves are going to be said to need interpretation. His strategy has a wondrous simplicity and directness. He will construe any account we offer as an uninterpreted extension of our already deinterpreted theory—by explaining we merely produce a new theory which, if consistent, will be as subject to a plethora of (true) interpretations as was the old. (‘Any interpretation is itself susceptible to further interpretation.’)

To a point, the challenge is well taken. We do need a metaphysically and epistemologically satisfactory account of the way mathematical practice determines or embodies the meaning of mathematical language. (We could also use a satisfactory account for areas other than mathematics.) We may even need to devise new concepts of meaning to forge such an account.

No one, to my knowledge, has made satisfactory, comprehensive, sense of both the constructivist and classical traditions in the philosophy of mathematics. They pull in opposite directions. Each picture has its attractions, but neither seem fully to meet the challenge of the other. I see Putnam’s Skeptical arguments as a challenge laid down to the Platonist. That is their strength. But they fall short of establishing a contrary position. They aren’t even strong enough to refute the Platonist, since they
depend so completely on this question-begging Skolemization of our ‘theory of the world’. Even if we agree with Putnam that the determinants of mathematical meaning must lie somewhere in our use of mathematical language, why think use to be captured (or capturable) by axioms (we do argue about new axioms)? Or that the proper way to treat the explanation is to de-interpret it and add it to our axioms to form a new first order theory of the world, an extension of our old theory, itself de-interpreted and a candidate for Skolemization? His view builds in the systematic undercutting of any ground there could be for explaining not only what we mean, but what it is to mean anything at all. That is the effect of adding the explanation to the theory and ignoring the meaning of the terms in the extended theory in order to treat it model-theoretically.

I wish I had a positive account to offer that had any hope of being objectively satisfactory (clearly none can satisfy Putnam). For now, I fear we must be content with the insight Sweeney expressed when, struck by his own logocentric predicament, he said,

... I gotta use words when I talk to you
But if you understand or if you don’t
That’s nothing to me and nothing to you
We gotta do what we gotta do ...

Despite the imagined possible misunderstandings, mathematical practice reflects our intentions and controls our use of mathematical language in ways of which we may not be aware at any given moment, but which transcend what we have explicitly set down in any given account—or may ever be able to set down.

With Gödel, I incline toward this view. But I am sufficiently aware of its vagueness and inadequacy not to be tempted into thinking it constitutes a view. It is merely a direction.12

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12 Ancestors of this paper were delivered at New York University, Stanford University, the University of Colorado (Boulder), and a meeting of the Association for Symbolic Logic. It was written in part as the author was (simultaneously) supported by leave from Princeton University and as Fellow of the Center for Advanced Study in the Behavioral Sciences, Sloan Foundation Fellow, Fellow of the National Endowment of the Humanities. This generous support is gratefully acknowledged. Although a more specific acknowledgement would be difficult to make, I have also profited immensely from Hao Wang’s lucid and sensitive Introduction [1970] to a partial edition of the works of Skolem.
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Note: References to Skolem [1922] are to the translation appearing in van Heijenoort [1967]; references to his other works are to their appearance in Skolem [1970]. Where the articles do not appear in English, quotations are my own translations. With the exception of [1922] and [1970], a date followed by a letter in Skolem’s works is as they appear in the Bibliography in Skolem [1970].


SKOLEM AND THE SKEPTIC


APPENDIX I

It is uncertain whether Zermelo actually held a view to which Skolem’s arguments will apply. In particular, although Zermelo unquestionably held that set theory should serve as a foundation for mathematics, it is prima facie questionable whether a first-order axiomatization was the way he had in mind: Either: (a) Aussonderungs is meant as a second-order sentence; or, what is more likely, (b) no first-order/second-order distinction was intended, or (c) even if a first-order axiomatization is acceptable Zermelo would not have agreed that formalization within LPC is the proper means of presenting those axioms.

Interestingly enough, Zermelo [1929] helped settle the first issue when he rejected Fraenkel’s [1925] syntactic first-order formulation of replacement (apparently he still hadn’t seen Skolem [1922], which first appeared in Norwegian) in favor of an attempted axiomatization of the notion of ‘definite property’, a kind of extension of the axioms of Z. This proposal is mute on the matter of the first-order/second-order distinction, but addresses itself instead to the rejection of a syntactic, i.e. formal, rendering of the notion of ‘definite property’. Helpful to a fault, Skolem seized upon Zermelo’s account, pointed out its obscurity and suggested a ‘natural’ clarification which, as luck would have it, rendered the patched up proposal equivalent with the Skolem-Fraenkel version and therefore equally subject to the deployment of the Löwenheim-Skolem argument. If it applied before (to Z), it still applies (to the newly-created ZF). Zermelo objected to formalization, arguing that formalization requires recursive definitions (of the syntactic concepts), and thus presupposes syntax, which itself presupposes the concept of ‘finite iteration’. Since in his view this concept was destined in turn to be reduced to set theory, he felt that the presentation of set theory could not presuppose it. See Hao Wang’s Introduction to Skolem [1970], esp. pp. 35–37.

Of course, this bears importantly on whether Skolem’s arguments reach the
real Zermelo, but somewhat more circuituously, since Skolem [1922] does not himself speak in terms of formalization. His early views are innocent of most of these concepts of logistic systems. (Skolem [1929b] and, esp. [1941a] and [1958a] are more explicit on the matter.) My reconstruction of his 1922 paper has Skolem attributing to Zermelo a formulation of Zermelo's view that he (Skolem) will find easy to attack with the model-theoretic tools near at hand. As we just saw, Zermelo can (and does) defend himself against this attack by repudiating such a view. But at what price? Set theory is a foundation for mathematics; all mathematical concepts must be defined in set-theoretic terms. Set-theoretic concepts are characterized (implicitly defined) by the axioms—axioms whose very consistency cannot be discussed except in terms of the concept of finite iteration, a concept which he requires us to reduce to the membership relation being 'defined' by those very axioms. With such a defense, Zermelo might fare better taking his chances against Skolem.

If my rendering of Skolem's construal of Zermelo doesn't at least come close to the way Skolem actually thought of it in [1922], the actual situation had to be far more complicated still. My rendering of Skolem's construal of Zermelo may be the only hope we have of lending Skolem's case any initial degree of plausibility.

APPENDIX II

ZERMELO 1908a:

*The Philosophical View*

A. Set theory must serve as a foundational discipline for mathematics, in the strongest sense: All mathematical concepts and proofs must be reduced to set-theoretical ones.

B. Intuition having proved bankrupt, set theory must itself be presented axiomatically.¹³

C. The following axioms are the appropriate foundation for this foundation:

*The system Z*

I. **Extensionality.** A set is determined by its members.

II. **Axiom of elementary sets:** Null set, unit sets, pairs.

III. **Aussonderungs:** Given a set M and a propositional function P that is definite* for all members of M, there is a set M(P) of all members of M satisfying P.

*‘An assertion is definite if the fundamental relations of the domain, by means of the axioms and the universally valid laws of logic, determine without arbitrariness whether it holds or not.’

IV. **Power set:** The power set (PM) of M is the set of all subsets of M.
V. Union, or Sum: the set of all members of members of a set M.
VI. Choice: Given any set M of disjoint non-empty sets, there exists a set with exactly one member from each member of M.
VII. Infinity: There exists a set A which contains the null set and the unit set of each member of A.

13 I am grateful to Zlatan Dannjanović for pointing out to me that some of Zermelo's other writings in this period suggest that he clearly had an intuitive conception of set that he took himself to be recording in the axioms listed below. He did not see the specification of these axioms as something like giving an implicit definition of "e". But Skolem's arguments make it equally clear that this is how he interpreted Zermelo. Rather than try to unravel these further intricacies here, I will simply acknowledge that B, as I have interpreted it in the text, appears to have been a figment of Skolem's imagination. A and C remain uncontroversially Zermelo's.
SKOLEM AND THE SKEPTIC

Paul Benacerraf and Crispin Wright

II—Crispin Wright

I shall not try to add to Professor Benacerraf’s illuminating presentation and diagnosis of the evolution in the views on these difficult matters of the actual historical Skolem. Such (largely inexpert and simple-minded) remarks as I have to make will mainly concern the ‘direction’ with whose commendation Benacerraf concludes his paper.

I

Like Benacerraf, I have found it difficult to see how a cogent version of the argument for ‘set-theoretic relativity’ might seem to run. But it is worth separating what I think are two quite different lines of thought.

Moral relativism, for instance, is (or could be) the view that there are no absolute moral standards, and (hence?) that moral judgements take on a content which is conditioned, in part, by the moral standards of the culture(s) to which their makers belong. Someone who took this view might hold, e.g. that there is no genuine conflict between enlightened English-speaking opinion on capital punishment and the views of the followers of the Ayatollah Khomeini, since there is sufficient cultural diversity between the respective sources of these judgements to ensure that the moral concepts involved are not the same. Deeply confused as this thinking may be, it at least serves to remind us that relativism is a classic form of escape from contradiction: the relativist postulates an ambiguity in order to avoid a collision.

The first line of thought belongs in this sort of relativistic tradition. The Löwenheim-Skolem theorem (LST) about, say, ZF set theory is presented as contradicting Cantor’s theorem in ZF. Cantor’s theorem entails that the power set of the integers cannot be put into 1–1 correspondence with the integers; whereas LST entails—(at this point we begin to struggle)—that ZF deals in at most countably many objects, and hence that the power set of the integers, as an object dealt with in the theory, is
itself at most countably infinite. As Benacerraf says, this is, so far, a tissue of confusion. Bluntly: LST entails neither that ZF deals only in a countable infinity of objects nor that it deals only in objects which are at most countably infinite. It says that ZF, if it has a model at all, has a countable model. There is not even the appearance of a contradiction. All we have—or so the Cantorian is so far at liberty to contend—is a proof that ZF, if consistent, has non-standard models.

In order to get a line worth considering we must I think, go for something like the more sophisticated reconstruction of the argument which Benacerraf builds on the transitive countable sub-model version of LST (=SMT). The intended interpretation for ZF involves, I take it, a transitive model: that is, every member of every set which the intended interpretation would include in the subject matter of set theory is likewise part of that subject matter. According to SMT, then, if ZF can sustain its intended interpretation at all, it may be interpreted in a countable sub-domain of the sets involved in the intended interpretation, in such a way that ‘ε’ continues to mean set membership and every set in the domain of the new interpretation is itself at most countably infinite. Even now it is apt to seem pretty unclear how exactly there is supposed to be an appearance of contradiction. The thought, presumably, will be something like the following. Consider the Power Set Axiom:

\[(x) (\exists u) (t) [t \in u \equiv (y) (y \in t \supset y \in x)]\]

And let x be \(\mathbb{Z}_0\), the set of positive integers. (It could, in fact, be any denumerably infinite set which features both in the domain of the intended interpretation and in that of the countable sub-model—there has, presumably, to be some such set.) Let D be the domain of the intended interpretation and D′ that of the sub-model. Since—the argument is supposing—\(\mathbb{Z}_0\) is an element both of D and D′, and since the Power Set Axiom holds under its intended interpretation with respect both to D and D′—(for, remember, ‘ε’ is not re-interpreted in the sub-model and is the sole non-logical constant in the Power Set Axiom)—the power set of \(\mathbb{Z}_0\), \(P\mathbb{Z}_0\),—the u whose existence the Power Set Axiom stipulates—must be an element both of D and D′. And now we do appear to have shown contradictory results about it. Cantor’s theorem, proved within ZF, entails that there is no 1-1
correspondence between $\mathbb{Z}_v$ and $\mathcal{P}\mathbb{Z}_v$; but if $\mathcal{P}\mathbb{Z}_v$ is an element of a transitive, countable sub-model of the ZF-axioms, it must itself be countable, i.e. there must indeed be such a 1-1 correspondence.

One response would be to regard the situation as demonstrating that ZF, like any set theory adequate for the demonstration of Cantor's theorem, is, if not actually inconsistent, at least unsound: a misdescription of what is true of sets. The relativist response, in contrast, would be to introduce an ambiguity, so that the two results are not really in conflict. Cantor's theorem, proved within the system, should—the relativist would contend—be seen as a result about the mapping-defining power of the system: no 1-1 correspondence between $\mathbb{Z}_v$ and $\mathcal{P}\mathbb{Z}_v$ can be defined within the system. And this claim is quite consistent with the reflection that such a 1-1 correspondence can be defined outside the system, using methods, presumably, for whose formalization ZF is inadequate.

Well, that this could not be a satisfactory response is evident, it seems to me, from the mundane reflection that it is so far utterly unclear why the purported $\mathbb{Z}_v:\mathcal{P}\mathbb{Z}_v$ mapping, mysteriously unformalizable in ZF set theory, is not confounded by Cantor's reasoning when presented informally. For Cantor's reasoning seems entirely general: it invites us, presented with any purported 1-1 correspondence, $R$, between the integers and their subsets, to form a set of exactly those which are not members of their correlates under $R$. Intuitively, there ought to be such a set when the relation in question is the one which establishes—allegedly—the extra-systematic countability of $\mathcal{P}\mathbb{Z}_v$; and, on pain of contradiction, this set cannot itself be correlated with an integer under $R$. So something is wrong here: if the relativist argument were correct up to the point where there is an apparent contradiction and a relativistic response might seem to be called for, that response could not possibly be intuitively adequate.

Benacerraf seems to me to bring out perfectly what has gone wrong. The trouble is in the supposition that if $\mathbb{Z}_v$ is an element both of $D$ and $D'$, then the fact that the Power Set Axiom holds for both domains, without reinterpretation of its sole non-logical constant, is a guarantee that $\mathcal{P}\mathbb{Z}_v$ is an element of both domains as well. This is simply incorrect. What the Power Set Axiom
guarantees is that each domain will contain, for \( Z_\Omega \), a set \( u \) which contains every \( t \) in that domain which satisfies the condition on the right-hand-side of the biconditional \( \text{vis à vis} \ Z_\Omega \). But whether these should intuitively be regarded as the same set depends, of course, on the respective ranges of \( t \) in the two domains. If those ranges are not the same, it cannot be assumed that the \('PZ_\Omega'\) which is an element of \( D' \) is indeed \( PZ_\Omega \), the full-blown power set of the integers. And without that assumption, the transitive countability of \( D' \) is in no sort of tension with the uncountability of \( PZ_\Omega \). So we simply do not get to the point when we appear to confront contradictory claims about one and the same object—the point where relativisation of the content of those claims might seem to be an appropriate strategy for dissolving the problem. We don’t get to that point because the supposition is so far totally unjustified that it is one and the same object with which the claims are concerned.

This seems to me a decisive and helpful point, and I think that Benacerraf’s conclusion, in effect that there simply is no coherent relativistic argument, of the classic sort, to be gleaned from SMT (or LST), is justified. However, I think the situation with the second train of thought is different.

II

Here is a passage from a justly influential article:

. . . numbers are not objects at all, because in giving the properties . . . of numbers you merely characterise an abstract structure—and the distinction lies in the fact that the ‘elements’ of the structure have no properties other than those relating them to other ‘elements’ of the same structure. . . .

. . . That a system of objects exhibits the structure of the integers implies that the elements of that system have some properties not dependent on structure. It must be possible to individuate those objects independently of the role they play in that structure. But this is precisely what cannot be done with the numbers. To be the number 3 is no more and no less than to be preceded by 2, 1, and possibly 0, and to be followed by 4, 5, and so forth. And to be the number 4 is no more and no less than to be
preceded by 3, 2, 1, and possibly 0, and to be followed by...

... Any object can play the role of 3; that is, any object can be the third element in some progression. What is peculiar to 3 is that it defines that role—not by being a paradigm of any object which plays it, but by representing the relation that any third member of a progression bears to the rest of the progression.

Arithmetic is therefore the science that elaborates the abstract structure that all progressions have in common in virtue of being progressions. It is not a science concerned with particular objects—the numbers. The search for which independently identifiable particular objects the numbers really are (sets? Julius Caesars?) is a misguided one.¹

The argument which precedes these conclusions is well known. Suppose that someone has received satisfactory definitions of '1' (or '0'), 'number', 'successor', '+', and '×' on the basis of which the laws of arithmetic can be derived. Let him also have confronted, if you like, an explicit statement of Peano's axioms. Suppose further that he has been given a full explanation of the 'extra-mathematical' uses of numbers—principally, counting—and has thereby been introduced to the concepts of cardinality and of cardinal number. Then it is possible to claim that, conceptually at least, the subject's arithmetical education is complete: he may be ignorant of all sorts of aspects of advanced (and less advanced) number theory, but his deficiencies, if any, are not in his understanding—at least not if he has followed his training properly. Yet the striking fact is—the argument runs—that someone who in this way perfectly understands the concept of (finite cardinal) number has no basis for (non-arbitrary) identification of the numbers which any objects given in some other way. Benacerraf makes the point vivid by comparing two hypothetical logicist-educated children, each of whom takes zero to be A but one of whom identifies successor...


(following Zermelo) with the unit set operation while the other (following Von Neumann-Berneys-Gödel) identifies the successor of a number with the set consisting of that number and all its elements. Each of the set-theoretic frameworks is perfectly adequate for the explanation of the arithmetical primitives, and the derivation of the Peano axioms, and supplies the background against which the applications of arithmetic can be satisfactorily explained. Yet a dispute between the two children as to the true identity of the numbers is intractable.

The moral is that the concept of number has no content sufficing to resolve such disputes, has indeed no content sufficing genuinely to individuate the numbers at all. When the explanations, formal and informal, are in, a good deal will have been said to characterise the structure which the numbers collectively exemplify, and which, in Benacerraf's view, is the real object of pure number-theoretic investigation; but nothing will have been said to enable a subject to know which, if any, sets the numbers are—or which, if any, objects of any sort they are. However if the numbers really were objects of some kind, surely someone who perfectly understood the concept of number should be able, at least in principle, to identify them. Since we do not have the slightest idea how such an identification might be defended, we ought to contrapose. Whence Benacerraf's anti-platonist conclusion.

I have tried to give reason elsewhere\(^3\) for thinking that the force of this argument is qualified both by certain internal weaknesses—the concept of finite cardinal number is determinate in ways the argument overlooks—and by the company it keeps—for instance Frege's 'permutation' argument and the various Quinean arguments for inscrutability of reference. But what is the point of reminding you of this argument here? Simply that it may be contended to furnish somewhat strange company for the direction Benacerraf would have us take in response to his 'Skeptic'. He writes

> The meaning of 'e' and the range of the quantifiers must constrain the class of permissible interpretations if the

\(^3\)C. Wright, *Frege's Conception of Numbers as Objects*, Aberdeen University Press 1983, especially Section xv, pp. 117-29.
formalised version is to retain the connection with intuitive mathematics with which set theory began—if it is to be a formalization of set theory. (this volume, p. 106)

And later

The reason we don’t have a Skolem paradox, and the reason set-theoretic notions aren’t relative in Skolem’s sense, is that there is no reason to treat set theory model-theoretically—which is not to say that there is no reason to study its model theory. What a set-theoretical statement says can simply not be identified with what is invariant under all classical models of the theory (i.e. models that allow ‘e’ to take on whatever binary relation will satisfy the axioms). . . . Axiomatized set theory took its roots in informal mathematics and in order to retain its sense through axiomatization and formalisation it must retain these points of contact.

After all, Zermelo . . . chose these particular axioms because they give us these theorems, which have an independent intuitive meaning of their own. Why turn our backs on this ancestry and pretend that the sole determinant of meaning of the axioms is their first order structure? (ibid, p. 108–9)

In common, I imagine, with many, I find the general train of thought here attractive; at least, if there is a coherent line of resistance to ‘Skolemism’ in general, it must surely involve taking issue with the assumption that the meaning of a sentence, or class of sentences, can aspire to no greater determinacy than is somehow reflected in the common ground between its, or their, defensible (by some criterion or other) interpretations. But I am not clear why, on the specific issue concerning uncountability, the Skolemite—Benacerraf’s Skeptic—needs to take this line on determinacy of sense. His argument is better put, it seems to me, in a form which, superficially at least, closely resembles Benacerraf’s own argument against arithmetical platonism. Let somebody have as rich an informal set-theoretic education as you like—which, however, is to stop short of a demonstration of Cantor’s theorem, or any comparable result, since these findings
are, after all, supposed to be available by way of discovery to someone who has mastered the intuitive concept of set. And let him recognize the axioms of ZF as a correct formal digest of his informal notion. Then SMT entails that a commitment to these axioms, plus possession of the intended grasp of ‘∈’—the sole non-logical primitive—need involve no commitment to the uncountable; someone who rejects it need not say or do anything at variance with acceptance of all the standard set-theoretic axioms interpreted as set-theoretic axioms. In other words: just as—according to Benacerraf’s argument—a full and complete explanation of arithmetical concepts is neutral with respect to the identification of the integers with any particular objects, so—the Skeptic will urge—a full and complete explanation of the concept of set is neutral with respect to the existence of uncountable sets. But if there really were uncountable sets, their existence would surely have to flow from the concept of set, as intuitively satisfactorily explained.

Here, there is, as it seems to me, no assumption that the content of the ZF-axioms cannot exceed what is invariant under all their classical models. It is granted that they are to have their ‘intended interpretation’: ‘∈’ is to mean set-membership. Even so, and conceived as encoding the intuitive concept of set, they fail to entail the existence of uncountable sets. So how can it be true that there are such sets?

Benacerraf’s reply is that the ZF-axioms are indeed faithful to the relevant informal notions only if, in addition to ensuring that ‘∈’ means set-membership, we interpret them so as to observe the constraint that ‘the universal quantifier has to mean all or at least all sets’ (p. 103). It follows, of course, that if the concept of set does determine a background against which Cantor’s theorem, under its intended interpretation, is sound, there is more to the concept of set that can be explained by communication of the intended sense of ‘∈’ and the stipulation that the ZF-axioms are to hold. And the residue is contained, presumably, in the informal explanations to which, Benacerraf reminds us, Zermelo intended his formalization to answer. At least, this must be so if the ‘intuitive concept of set’ is capable of being explained at all. Yet it is notable that Benacerraf nowhere ventures to supply the missing informal explanation—the story which will pack enough into the extension of ‘all sets’ to yield
Cantor's theorem, under its intended interpretation, as a highly non-trivial corollary.

The dialectical position then—according to this second 'Skeptical' line of thought—is not that we have been given what is, by ordinary criteria, a perfectly satisfactory explanation of the intuitive concept of set—but an explanation whose very expression some uncooperative individual now persists in 'Skolemising'. Rather, it is unclear whether the 'intended interpretation' has been satisfactorily explained, formally or informally, at all. At any rate, if the 'intuitive conception of set' is satisfactorily explained by informal characterization of the meaning of '∈' and stipulation of the ZF-axioms, then the Cantorian—Benacerraf?—owes an explanation, I believe, of why the Skeptic about uncountability does not have an argument strategically identical to that which Benacerraf brought against arithmetical platonism. And if, on the other hand, such is not a satisfactory explanation of the intuitive concept, and more, in particular, needs to be packed into the interpretation of the quantifiers, then an appropriate informal explanation is owing of the residue. (I shall return to this.)

A successful skeptical argument has somehow to provide a bridge from the non-categoricity of the ZF-axioms to the realisation that all is not well with the classical concept of set in general or indenumerability in particular. Benacerraf represents the Skeptic as seeking to effect the bridge via some such assumption as that 'the only mathematical concepts we have are those that are embodied by all models of our mathematical theories under first order formalisation' (p. 109) and hence that the content of the ZF-axioms cannot have a greater determinacy than that reflected by whatever properties are invariant through all their models. He complains, rightly as it seems to me, that such a contention not merely seems quite arbitrary but has in addition a self-defeating character: the very formulation of the contention requires recourse to certain concepts—'model', 'first-order theory', etc.—which it has to take to have intuitive content. The last thing which the Skeptic intends, presumably, by such a contention is something dilute enough to be shared by all admissible interpretations of it! If the Skeptic is happy to rely on specific intuitive concepts in the formulation of his claim, why does he refuse his Cantorian opponent the same right?
This is surely a fair point *ad hominem*. But, whatever Skolem or any other actual relativist may have said, the second line of thought I have described would not attempt to give the bridge this shape. The non-categoricity of the ZF-axioms must concern anyone who, while granting Benacerraf that there is a genuinely intuitive, informal concept of set to be had, inclines to view the standard axiomatization as something akin to an *explication* in Carnap’s sense. The essence of explication, of course, is that the *explicans* preserves everything essential to the *explicandum*; unlike strict analysis, or definition, however, it is permissible that elements of determinacy, or precision, be introduced. At any rate, a good explication cannot be *weaker* than the intuitive concept it supplants, cannot be neutral on points on which the latter is committed. Accordingly if the ZF-axioms, with ‘∈’ interpreted as set membership, did constitute a satisfactory explication of the extension of the intuitive concept of set, the fact that they do not, so interpreted, entail the existence of uncountable sets would force the conclusion that there is no such entailment from the intuitive concept of set either. So I do not think it is enough for Benacerraf to urge, on behalf of the Cantorian, that we recognize that set theory is answerable to intuitive concepts. So much is recognised by one who looks to the ZF-axioms for an explication, and the difficulty still arises. The Cantorian claim has to be something stronger: that the intended range of the quantifiers in the ZF-axioms is precisely *not* susceptible to explication by axiomatic stipulation at all. It is not so much a matter of paying due heed to the informal roots of the discipline as recognising, in the Cantorian view, that no satisfactory formal explication of the concept of set can be given, even when we hold fixed the interpretation of the only set-theoretic primitive—‘∈’—which need feature in such an explication. It does not seem to me that only a theoretical position which drew on the assumption which Benacerraf complains about could inspire misgivings about this.

III

One reason why it can be hard to hear the modest criticisms which, in my view, SMT can properly encourage, is because of the clamour raised by the much less modest generalisation of
‘Skolemism’ developed by writers such as Putnam. The last third of a century has, indeed, been a bad time for the notion of determinacy of meaning in general. Quine, Putnam, and Kripke (on behalf of Wittgenstein) have all developed arguments which, notwithstanding important differences, would each, if sustained, have the effect that the traditional notion of meaning simply could not survive. Quine, Kripke’s Wittgenstein, and Putnam all seem to believe that we can, and should, pay this price: whether by a more resolute adherence to the models of explanation displayed in theoretical physics, or by the adoption of some form of ‘skeptical solution’, or by a shift to a more constructivistic conception of meaning, intellectual life can flourish without the traditional notion of meaning. Like many, I am skeptical whether this is so. To mention but one cause of anxiety: the notion of meaning connects, in platitudinous ways, with that of truth—whether a particular utterance expresses a truth is a function of what, in context, it succeeds in saying, which is in turn a function of the meaning of the type-utterance of which it is an instance. If we really decided that there is no such thing as determinate meaning, what would be the determinants of the truth of an utterance? Nothing is more likely to make us sympathise with the kind of response which Benacerraf wants to make to ‘Skolemism’ about the classical notion of set than the belief that the argument involved is simply a restricted precursor of the grand-skeptical arguments which have commanded so much recent philosophical attention. We are not, most of us, in the market for purchase of the conclusion of these arguments; they can at best have a status of paradoxes, rather than findings, even if there is no general agreement about how to disinfect them. So, for as long as it seems that Skolemistic doubts about the determinacy of the classical concept of set can be sustained only if we are prepared to admit their wholesale generalisation, we are likely to want to sympathise with Benacerraf’s point of view: Cantorianism may carry its own intellectual discomforts but it’s a good deal more comfortable than what appears to be the alternative.

It seems to me that the dichotomy should be false. I have great sympathy for Benacerraf’s ‘direction’ as a response to generalised Skolemism, and I shall try in a moment to indicate why. But this sympathy is quite consistent with the sort of doubt about the
good standing of the classical concept of set which, I believe, SMT may contribute towards motivating.

I have no space here to attempt to review the detail of Quine’s, Kripke’s, and Putnam’s arguments. But someone who is familiar with them will recognise, I think, that they involve a common assumption. The assumption is a version of the later Wittgenstein’s idea that meaning cannot transcend use. One of Quine’s claims is exactly that whatever the extent of his data about our use of an expression, a radical interpreter would still be confronted with indefinitely many mutually incompatible hypotheses about the meaning of that expression each of which would serve adequately to rationalise the data. Kripke’s skeptic persuades his victim to accept that his previous use of an expression cannot rationally constrain its interpretation to within uniqueness, and hence that the fact—assuming, at this stage of the dialectic, that there is such a fact—in which the determinacy of what he meant by the expression consists must be sought elsewhere. And Putnam writes—if I may repeat the quotation cited by Professor Benacerraf—that

If we are told, ‘axiomatic set theory does not capture the intuitive notion of a set’, then it is natural to think that something else—our ‘understanding’—does capture it. But what can our ‘understanding’ come to, at least for a naturalistically minded philosopher, which is more than the way we use our language? And the Skolem argument can be extended, as we have just seen, to show that the total use of the language (operational plus theoretical constraints) does not ‘fix’ a unique ‘intended interpretation’ any more than axiomatic set theory by itself does.⁴

The dilemma is this. If we hold that meaning cannot transcend use, we seem to be committed to the contention that there cannot be more, or more specificity, to the meaning of an expression than would be apparent to someone who engaged in purely rational reflection upon a sufficient sample, or an otherwise adequate characterisation, of its use. The various skeptical arguments then all take the form of contending—with a good degree of plausibility, which is what gives them their

⁴See above, p. 110.
interest—that no amount of data about the use of an expression, and no (axiomatic) characterisation of its use, can rationally constrain its interpretation to within uniqueness. But if we lurch to the opposite wing and allow that meaning can somehow transcend use, the price we pay for securing determinacy of meaning that way is that we are beggared for a satisfactory epistemology of understanding (=knowledge of meaning). Meaning, it seems, will have to be the object of some sort of direct intellection, since inaccessible to mere rationalisation of perceived use. So, if the sceptical arguments are sound, the choice would appear to be between dropping the idea that there is such a thing as determinate meaning or retaining it at the cost of an awkward silence when charged to explain how meanings can be public, how they can be known, and so on.\(^5\) In these circumstances, since the response of allowing meaning to transcend use seems so utterly futile, one feels there has to be something amiss with the sceptical arguments. But what?

Consider Benacerraf’s response to Putnam’s generalisation of Skolemism:

> Even if we agree with Putnam that the determinants of mathematical meaning must lie somewhere in our use of mathematical language, why think use to be captured (or capturable) by axioms. . ? Or that the proper way to treat the explanation is to de-interpret it and add it to our axioms to form a new first order theory of the world, an extension of our old theory, itself de-interpreted and a candidate for Skolemisation? [Putnam’s] view builds in the systematic undercutting of any ground there could be for explaining not only what we mean, but what it is to mean anything at all. That is the effect of adding the explanation to the theory and ignoring the meaning of the terms in the extended theory in order to treat it model-theoretically. (p. 111)

Now, Wittgenstein wrote, in response to the skeptical paradox which Kripke interprets, that the solution consists in seeing that ‘there is a way of grasping a rule which is not an interpretation’

\(^5\) I have not been able to understand how Putnam’s espousal of verificationism gets to grips with the dilemma.
What Benacerraf is urging, comparably, is that there is a way of receiving an explanation which is not an interpretation. So if someone e.g. lays down the ZF-axioms and then adds some informal remarks by way of explanation of the intended concept of set, these remarks, Benacerraf is urging, can have a determinacy of content, and so an explanatory force, which far exceeds the constraints which they impose on any model of their first order union with the original axioms.

But what is the way of receiving an explanation which is not an interpretation? Has not Benacerraf in effect simply begged the question against the skeptic, assuming that there is such a thing as determinacy of meaning when the least the skeptical arguments teach us is that the right to suppose so is bought only at extortionate epistemological cost? I do not think so, since I believe we can glimpse the possibility of an alternative to the platonism which, pending disclosure of some internal flaw in the skeptical arguments, was all that seemed available as an alternative to the skeptical conclusion. Wittgenstein is very inexplicit about his ‘way of grasping a rule which is not an interpretation’ but he does at least say that it is something ‘which is exhibited in what we call “obeying the rule” and “going against it” in actual cases’ (ibid). What we need to win through to, I suggest, is a perspective from which we may both repudiate any suggestion of the platonic transcendence of meaning over use and recognise that meaning cannot be determined to within uniqueness if the sole determinants are rational methodology and an as-large-as-you-like pool of data about use. Wittgenstein wanted to suggest that the missing parameter, the source of determinacy, is human nature. Coming to understand an expression is not and cannot be a matter of arriving at a uniquely rational solution to the problem of interpreting witnessed use of it—a ‘best explanation’ of the data. Still less is it a matter of getting into some form of direct intellectual contact with a platonic concept, or whatever. It is a matter of acquiring the capacity to participate in a practice, or set of practices, in which the use of that expression is a component. And the capacity to acquire this capacity is something with which we are endowed not just by our rational faculties but to which elements in our sub-rational natures also contribute: certain natural propensities we have to uphold.
particular patterns of judgement and response. Crudely, then, what makes it possible to derive something more specific from an explanation than whatever is invariant through the totality of 'models' of the 'theory' containing that explanation is that one's response to the explanation will be conditioned by other factors than the attempt rationally to interpret that 'theory'.

These remarks too constitute 'merely a direction'. But it is a hopeful direction: it holds out the promise of an explanation of how, without platonising meaning, we can uphold the right to maintain that some particular interpretation—of set theory or of anything you like—is what is intended, and what is communally understood to be intended, without suffering the probably hopeless commitment to defending the claim that only that interpretation can rationalise our use of the relevant concepts.\(^6\)

IV

To come back to earth, however—if to resume consideration of the uncountable can be to do that—let me conclude by trying to explain why I still think that the classical concept of set, that concept of set which requires the existence of uncountable sets, is indeed something to stumble over. The Cantorian may, of course, disclaim the need for explanation altogether, though if he does he can scarcely hope for much respect for his views. What we have seen is that if the intuitive concept of set is indeed satisfactorily explicable—and how else could it be communicable?—the explanation has to be, at least in large part, informal; and it will not suffice informally to explain the set membership relation and then to stipulate e.g. that the ZF-axioms are a correct digest of the principles of set existence. If the Cantorian wishes it to follow from his explanation that there are

\(^6\) There is, admittedly, little cause to be sanguine that a development of this response could be effective against the strong form of Quinean indeterminacy: the version which holds that there are, \textit{ab initio} as it were, rival interpretations which will each serve to explain any amount of a subject's linguistic behaviour (rather than the weaker claim that for any amount of data about linguistic behaviour, there are rival interpretations . . . etc.). Wittgenstein's idea is addressed to the question how the patterns of linguistic practice constitutive of a particular concept can be apparent in a finite sample. But if the strong claim is right, shared patterns of linguistic practice cannot justifiably be taken to constitute shared concepts. The crucial question, of course, is whether Quine succeeds in presenting cogent reason for this claim.
all the sets which, intuitively, he believes that there are, he has to do something more. What?

He has to say something which entails that there are uncountably many sets. And that is not the same as stipulating an axiomatic framework in which Cantor’s theorem may be proved, since the difficulty is exactly that if his preferred set theory can take its intended interpretation at all, it can take a set-theoretic interpretation under which Cantor’s theorem cannot be interpreted as a result about uncountability. So how does the Cantorian get across to a trainee that interpretation (of the range of the individual variables of the theory) which will constrain conception of Cantor’s theorem as a cardinality result? Now, the reason why it can seem as if there is no very great difficulty here is the point noted earlier, that Cantor’s theorem, and indeed his Diagonal Argument, is apt to impress us as having unrestricted applicability as a piece of informal mathematics. That is why the original relativistic position on the uncountability of e.g. the reals seems so unconvincing: the extra-systematic enumerability of the ‘reals’ in a countable model for ZF ought, intuitively, to be disruptible by diagonalisation—there ought to be something with, intuitively, perfect credentials to be a real number which is nevertheless not, on pain of contradiction, in the range of the enumerating function. Is it not this that makes us feel entitled to regard any countable set model for the ZF-axioms as an unintended model, something that only imperfectly embodies our intuitive set-theoretic concepts? For the fact is that there is an informal set-theoretic result which we can achieve—the beautifully simple result which Cantor taught us—which we can prove about this model, which is not to be identified with the corresponding result within the system when the latter is interpreted in terms of this model, and which shows that the model does not include everything which the intended interpretation of the system embraces.

These remarks are intended to be diagnostic. I am suggesting that Cantor’s reasoning—it doesn’t matter at present whether we concentrate on the power set theorem or the Diagonal Argument—plays a role in the formation of our conception of what the intended interpretation of set theory is. Its role is less to articulate a surprising consequence of concepts fixed in some other way than to lead the determination of an inchoate concept
of set in a particular direction. At any rate, we have absolutely no right to the idea that a countable set model of the ZF-axioms has to be 'non-standard', a misrepresentation of our intuitive intentions, unless we buy Cantor's reasoning as a piece of informal mathematics, something that defeats any candidate for a 1-1 correlation of the integers and reals, however characterised in whatever sort of system.

The question, accordingly, is whether Cantor's reasoning is cogent as (contributing to) an introduction to the intended conception of the range of the individual variables in set theory; whether it does indeed lead us to a concept of set of which no countable model can be an adequate realisation. Well, the answer, I believe, for reasons which you can probably all too easily anticipate, is that it does not. Let us informally rehearse the Diagonal Argument that the reals are uncountable. We know that the rationals are countable, and that the union of any pair of countable disjoint sets is itself countable. So the reals are uncountable if and only if the irrationals are. Each irrational can be represented as an infinite non-recurring decimal; to suppose that they are countable is therefore to suppose that there is some array

\[
\begin{align*}
    a & . a_1 a_2 a_3 a_4 a_5 \ldots \\
    b & . b_1 b_2 b_3 b_4 b_5 \ldots \\
    c & . c_1 c_2 c_3 c_4 c_5 \ldots \\
    d & . d_1 d_2 d_3 d_4 d_5 \ldots \\
    e & . e_1 e_2 e_3 e_4 e_5 \ldots
\end{align*}
\]

(where each letter to the left of the decimal point represents some numeral, while those to the right represent any of 0–9) in which every non-recurring decimal occurs somewhere. But then, Cantor observes, it is easy to see that no such array can be complete; we have only to reflect that there is a decimal which differs at the \(i\)th place from each \(i\)th decimal in the array. However, in order to imbue Cantor's observation with its intended significance, we need to make a substantial assumption. Suppose we restricted our attention to effectively computable infinite decimals—those whose every \(i\)th place can (at least in principle) be calculated; and suppose we stipulated, in addition,
that the whole array must itself be effective, i.e. that for each i, it should be effectively determinable what the ith decimal in the array should be. Under these restrictions, the Diagonal Argument takes on a wholly constructive character; we can now effectively compute a decimal which, on pain of contradiction, is no member of the array. But just for that reason we no longer have a result about uncountability. At least, we have no such result if we suppose that Church’s Thesis holds. For in that case all the decimals in the array, and each successive diagonal decimal which we care to define, will correspond to recursively enumerable sequences of numerals. And we know independently that the totality of recursive functions is only countably infinite, since they are all definable in a finitely based vocabulary. Under these restrictions, then, Cantor’s argument takes on a quite different significance: rather than showing something about uncountability, it shows that there is no recursive enumeration of all recursively enumerable infinite decimals. (The reader will easily see how similar considerations may be applied to the informal proof of the power set theorem.)

What goes for real numbers goes for subsets of natural numbers, since every infinite decimal has a unique binary equivalent, and an infinite binary expansion may be regarded as determining a unique subset of natural numbers in accordance with the principle that the ith natural number will be a member of the subset in question just in case the ith element in the binary expansion is 1. So, under the restrictions, the result is, again, that there is no effective/recursive enumeration of all effectively/recursively enumerable subsets of natural numbers. And, again, we know that the finite and recursively enumerable infinite subsets of natural numbers form a countable set. So the moral is simple: before the Diagonal Argument, or the informal proof of the power set theorem, can lead us to a conception of the intended range of the individual variables in set theory which will allow us to regard any countable set model as a non-standard truncation, we need to waive the restrictions. And in order to understand the waiver, we need to grasp the notion of a non-effectively enumerable denumerably infinite subset of natural numbers.7

7 We could hardly attain the conception of a non-effectively enumerable denumerably infinite array of infinite decimals without grasping this notion first.
This is what, if he is in the business of giving explanations, the Cantorian needs to explain. Now there are, I suppose, two routes into the informal notion of a subset of a given set. One is formally reflected in the Aussonderungsaxiom: a subset of a given set is determined by any bona fide \textit{property}. The other route proceeds via the notion of a \textit{selection}: a subset of a given set corresponds to every way, rule-governed or arbitrary, of selecting some of its members. Neither of these routes, it seems to me, holds out any very plausible promise of meeting the Cantorian's needs. Certainly, whichever he chooses, there are extremely awkward challenges to face. For instance, it is evident that the requisite properties cannot all be identified with the content of a possible predication (open sentence) in something we could recognise as a single language. Intuitively, there are indeed non-effectively enumerable infinite subsets of natural numbers which can be characterised in a finite and perfectly definite way; an example would be the set of Gödel-numbers of non-theorems of first order logic. But the Cantorian cannot give us the slightest reason to think that everything which he wishes to regard as a subset of natural numbers would allow of an intelligible, finite characterisation in this manner. Let him be granted that such a characterisation need not be constructive, i.e. that there need be no effective way of determining whether a particular natural number qualifies for membership. Still, a property is something which it should be possible to \textit{claim} to be exemplified by a particular object, even if the claim cannot be assessed. If the Cantorian believes that a notion of a subset (of natural numbers) can be gleaned by this route which is sufficiently fertile for his purposes, he commits himself to the belief—if he respects the connection between \textit{property} and \textit{possible predication}—that there are uncountably many \textit{finitely expressible properties}. This is an enormously problematic idea. Naturally, no single language—at least no finitely based language—can be adequate to express all. Since any intelligible language is, presumably, finitely based, and hence can express at most countably many distinct properties, the Cantorian who takes this line is committed to a potentially uncountable infinity of languages no two of which are completely intertranslatable. Perhaps there is no contradiction in such a conception but it is, at best, a highly unfortunate consequence of something which
was supposed to be an intuitive explanation. And it is, besides, a very nice question—where inter-translatability is missing—what would make a pair of the relevant properties, each expressed in one of these languages in a manner not translatable into the other, distinct. (It would of course be circular in the present context to attribute their distinction to that of the subsets which they respectively define.) If, on the other hand, the Cantorian severs the connection between property and possible predication he is open to the charge that he is obscurantising the notion of a property and thus can use it to explain nothing.

Matters are no better if we focus instead on the notion of a selection. The selections in question—if they are to generate enough subsets for the Cantorian’s purposes—have to be thought of as uninformed by any sort of recursive procedure. But they also have to be thought of as complete, since it is only when infinitely many selections have actually been made—in a non-recursive way—that we actually have a non-recursively enumerable infinite subset of natural numbers. So long as only finitely many selections have been made, the constitution of the subset so far determined will be consistent with supposing it to be recursively enumerable. So in order to arrive at the intended notion of subset, we have to understand what it would be actually to complete an infinite but arbitrary selection from the set of natural numbers. Of course some philosophers, notably Russell, have found no difficulty in crediting us with such a conception. But I do not think that anyone who follows up the literature on ‘Supertasks’ is likely to feel at ease with the idea. Someone who is inclined to think there is no problem should ask himself what it would be like to have reason to think that he had, magically as it were, acquired the capacity to complete such a selection. Benacerraf himself writes

... there is probably no set of conditions that we can (nontrivially) state ... whose satisfaction would lead us to conclude that a supertask had been performed ... no circumstance that we could imagine and describe in which we would be justified in saying that an infinite sequence of tasks had been completed ... there is nothing we can describe that it would be reasonable to call a completed infinite sequence of tasks.8
Can we claim to understand what a certain capacity would be if we do not know what it would be like to have exercised it? And what can the idea of an arbitrary actually infinite selection come to if we do not know what the capacity to perform one would be?

I am, of course, aware that what counts as adequate clarity in an explanation is, inevitably, to some extent a subjective business. What I claim is that the analogies which underpin the further development of the notion of subset—for that is what it is—with which the Cantorian wishes to work are very, very stretched. If someone is pleased to think that they are nevertheless good enough, I have no decisive counter-argument. But I hope to have reminded you that there are certain not very heavily theoretical—'anti-realist' or whatever—reasons for dissatisfaction with 'the intuitive concept of set', and 'the intended interpretation' of classical set theory. And, more importantly, to have made it plausible that we need not, in sympathising with these reasons, be receiving the thin end of a meaning-skeptical wedge.

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