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SKOLEM'S PARADOX AND CONSTRUCTIVISM

INTRODUCTION

Skolem's paradox has not been shown to arise for constructivism. Indeed, considerations that we shall advance below indicate that the main result cited to produce the paradox could be obtained only by methods not in mainstream intuitionistic practice. It strikes us as an important fact for the philosophy of mathematics. The intuitionistic conception of the mathematical universe appears, as far as we know, to be free from Skolemite stress. If one could discover reasons for believing this to be no accident, it would be an important new consideration, in addition to the meaning-theoretic ones advanced by Dummett (1978, 'Concluding philosophical remarks'), that ought to be assessed when trying to reach a view on the question whether intuitionism is the correct philosophy of mathematics.

We give below the detailed reasons why we believe that intuitionistic mathematics is indeed free of the Skolem paradox. They culminate in a strong independence result: even a very powerful version of intuitionistic set theory does not yield any of the usual forms of a countable downward Löwenheim – Skolem theorem. The proof draws on the general equivalence described in McCarty (1984) between intuitionistic mathematics and classical recursive mathematics. But first we set the stage by explaining the history of the (classical) paradox, and the philosophical reflections on the foundations of set theory that it has provoked. The recent symposium between Paul Benacerraf and Crispin Wright provides a focus for these considerations. Then we inspect the known proofs of the Löwenheim – Skolem theorem, and reveal them all to be constructively unacceptable.

Finally we set out the independence results. They yield, we believe, the deep reasons for the localised constructive failures. Besides showing

Note: 'ε' is used both as the epsilon of set membership and for the existential quantifier. Context will always make clear which use is intended.

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that weak versions of the Löwenheim – Skolem Theorem cannot be proved in extensions of the intuitionistic set theory IZF, we prove that the Theorem entails principles which many constructivists would reject (e.g., Kripke's Schema) and is falsified outright by principles (Church's Thesis and Markov's Scheme) which a number of constructivists would accept. Let us also emphasize at the outset that the metamathematics which we adopt in giving our proofs is itself constructive.

I

Skolem's paradox was thought by Skolem, and has been thought by his skeptical successors since, to show that the notion of absolute non-denumerability is ineffable. In Skolem's own words (1922):

> . . . auf axiomatischer Grundlage sind höhere Unendlichkeiten nur in relativem Sinne vorhanden.
> (on axiomatic foundations higher infinities occur only in a relative sense.)

We say that the paradox has been thought to *show* this precisely because, unlike Russell's paradox, it does not *say* anything inconsistent in the viciously circular way we have come to associate with the logical and set-theoretical paradoxes.[1] Skolem's paradox, unlike Russell's, is not crisply expressed by any one sentence of the object language. Instead, it involves a traversing of levels between object and metalanguage. It arises because Cantor's theorem, provable in the object language of set theory, says that there is no one-one correlation of the set of natural numbers onto the set of all its subsets; while a model existence theorem, provable (using informal set theory) in the metalanguage, says that the axiomatic set theory of the object language has a countable model.

Two theorems therefore produce the paradoxical tension. Let $M[t]$ be the denotation, in model M, of the term t. Let '$P(\omega)$' be the term for the power set of ω, the set of natural numbers. Suppose M is a countable model of set theory. The tension is this: $M[P(\omega)]$ appears 'within' the countable model M (on pain of contradicting Cantor's theorem) not to be the domain of any one-one mapping, *within* M, onto the set ω; the model fails to contain an element in its own domain serving as the set of ordered pairs that would establish such a one – one correlation. But from 'outside' the model M the set $M[P(\omega)]$ does

appear to be the domain of a one-one mapping onto the set ω. Cantor's theorem, however, prevents this mapping from being in the model M. From 'inside' the countable model M, as it were, one fails to appreciate just how 'small' $M(P(\omega)]$ is.

That, in a nutshell, is the paradox. The Skolemite Skeptic invokes it to undermine our confidence that one can communicate a conception of the mathematical universe. If the mathematical universe is rich enough to contain the real numbers (in the form of the power set of ω), the paradox says it can be impoverished. Or, rather, the account of it can be so devalued by interpretation in a countable (sub-) universe that one cannot gain a purchase on its contents.

We shall argue that the devalued linguistic currency may be strictly classical. The *intuitionist* or *constructivist* mathematician appears not to be affected by the problem. For one of the two theorems involved in the classical case is not intuitionistically true. So, ironically, although being able to 'say' much more by virtue of his stronger logical methods, the classicist appears to speak to less pointed effect than the intuitionist.

Our method of exposition will be as follows. First we shall examine the recent exchange on the problem of Skolem and the Skeptic by Benacerraf and Wright. Then we shall look more closely at the method of proof of Cantor's theorem, and bring out its rich constructive import. Then we shall inspect the various methods of proof of the countable downward Löwenheim – Skolem theorem, this being the other result (available classically) that produces the paradox, as explained above. These methods include Skolem's original one, using Skolem normal forms, another using prenex normal forms, and the one by Henkin expansion. We shall show how each method fails in the intuitionistic case. Finally we shall give a general result behind all these failures: the independence, within a strong version of intuitionistic set theory, of forms of the arbitrary countable models theorem. This is an interesting and important phenomenon that deserves further investigation.

II

In his searching examination of the source of Skolem's paradox, Benacerraf (1985) reflects on the inadequacy of the formal axiomatic method as follows (p. 111):

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 may explicitly set down in any given *account* – or may ever be able to set down.

The account in question is, of course, an axiomatic one at first order, involving only countably many sentences. In his paper Benacerraf does attempt to locate more precisely where the trouble lies than merely the countable character of our sayings. He suggests that the problem lies not so much with the interpretation of 'ε' (the membership predicate), but rather with the interpretation of the universal quantifier:

. . . whether *T says* that a set is non-denumerable depends on *more* than whether the interpretation is over a domain of sets, 'ε' 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 (the set of natural numbers). (*loc. cit.* p. 103)

In his reply, Wright (1985) is sympathetic to this diagnosis of the classical set theorist's problem, even if not sympathetic to the classicism that generates it. He claims that the diagonal argument establishes a result about uncountability only on a nonconstructive interpretation. Otherwise, he thinks, all it can be taken to show is that there is no effective enumeration of all constructive (that is, decidable) sets of natural numbers. As Wright puts it (pp. 134–135):

. . . before 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 grasp the notion of a *non-effectively enumerable denumerably infinite subset of natural numbers*. This is what, if he is in the business of giving explanations, the Cantorian needs to explain.

We shall now argue that, in this fascinating encounter between a classicist and a constructivist, the real problem has been mislocated and the proper solution has been missed. The points to be made are historical, logical and conceptual. But first, in order to set the stage properly for discussion, let us survey the many potential sources of the difficulty, and see how far we can agree with Benacerraf and Wright in putting them aside. This will enable us to focus more sharply on the problem they neglect and on the shape of its solution.

III

The following is a digest of points worth considering in this regard.

1. What *is* the classical notion of set?
   a. Can it be communicated only informally? Does it elude axiomatic characterization
      i. at first order
      ii. at second or higher order
      iii. in finitary languages
      iv. in infinitary languages
      v. using only countably many sentences
      vi. using uncountably many sentences?[2]
   b. Does it have *built into it* the existence of:
      i. an infinite set
      ii. an uncountably infinite set?
2. How are the two theorems involved in Skolem's paradox proved? Constructively or strictly classically?
3. What is the significance of having a countable *sub*model theorem as opposed to one which merely guarantees the existence of some countable model (whether or not it be a submodel of an intended model presumed given)?

Having posed these questions, let us now sketch what we take to be common ground between Benacerraf and Wright. (a) Both take the classical notion of set, if communicable, to be communicable only informally. (b) Both see no point in considering higher order or infinitary languages, or uncountable theories in the axiomatic characterization of the notion. For with all these (it could be argued) the notions of set or of non-denumerability are being presupposed in the very project of communicating or imparting an understanding of them. (c) Both consider worthwhile and admissible only countable axiomatic characterizations in finitary first order languages.

With this much we are in sympathy. Skolem himself remarked on the circularity of the presupposition just mentioned (1922, p. 144). But Skolem would also have accorded no particular significance to the distinction mentioned in (3) above (as historical considerations will in due course show); and in this regard we too would be unwilling

to move on to the common ground between Benacerraf and Wright. For both of them regard the countable *sub*model theorem as contributing an important part of the discomfiture produced by Skolem's paradox. Their thought is that a countable submodel extracted from the intended model arguably preserves the interpretation of 'ε', and thereby shifts the locus of the difficulty to the interpretation of the universal quantifier (at least as it applies to sets). Benacerraf himself states as one version of the Löwenheim – Skolem theorem the following:

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.) (p. 101).

And Wright endorses his preference for this version of the countable model theorem as follows:

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 . . . (p. 118; our emphasis).

In his eagerness to concentrate on the countable *sub*model version of the theorem, Benacerraf even goes so far (p. 94) as mistakenly to claim that it was this version that Skolem proved in his 1922 paper. In the next section we shall return to the distinction between the two versions of the model existence theorem, and argue that it is philosophically irrelevant. We shall argue also that both Benacerraf and Wright have followed a red herring, despite clear contextual clues in Skolem's own writings that the distinction *is* irrelevant, and that the problem cannot be shifted away from the interpretation of 'ε' and onto the interpretation of 'all sets' or 'all subsets of . . . '. But first let us complete our summary responses to the lists of questions above.

As to the method of proof of the theorems involved in the paradox – Cantor's theorem and the theorem on the existence of countable models of countable theories – Benacerraf says nothing, apart from an inaccurate historical remark (p. 91, n. 3) that Löwenheim employed

the axiom of choice in his original proof. In fact, it was Skolem who introduced the use of choice in order to simplify Löwenheim's proof. More to the point would be the question whether the countable models theorem is constructively provable. Wright enters constructivist considerations, and sees the embarrassment of Skolem's paradox as affecting, first and foremost, the Cantorian. He makes the Skeptic sound thoroughly constructivist; he suggestively asserts (p. 124) that

> the Skeptic will urge (that) 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.

And later, on p. 126 he writes:

> . . . if the ZF-axioms, with 'ε' 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.

It would have been natural, in registering constructivist doubts about the *existence* of (uncountably) infinite sets as completed totalities, to have mentioned also the relevance of the more narrowly restricted *logical* methods of which the constructivist may avail himself compared with the classicist. It would have been appropriate then to ask whether indeed the countable models theorem is *constructively* provable; whether, that is, the Cantorian embarrassment arises from paradisial *logical* manoeuvres as much as it does from ontological excess. Now Wright did not himself pursue this question; for he was allowing the classicist the full use of classical methods in order to bring out the inadequacies internal to realism. But it is only natural to ask whether, once the problem has been posed by the proofs of the two main theorems – Cantor's theorem, and the countable models theorem – the same problem will continue to bedevil the constructivist who cuts back on permissible methods of proof.

IV

Let us suppose, with all parties to the debate, that what is sought and what is being assessed for explicatory adequacy, is some formal characterization of the notion of set using a finitary first order language

and a countable formulation using rules and or axioms. Let us for the moment register, but set aside, the problem of the correct choice of *logical* rules of inference. Note that there are two aspects to the 'notion of set'. One may be called *structural-theoretical*, the other *ontological*. The *structural-theoretical* aspect is addressed by such principles as the principle of extensionality and Church's conversion schema. To wit, sets with the same members are identifiable; and the members of the set of F's are precisely the F's.

The *ontological* aspect is addressed by such questions as:

is there a null set?
is there an infinite set?
is there an uncountably infinite set?
is there a universal set?

and it is not at all clear that (the first three at least of) *these* questions should be answerable by anyone who has mastered the *concept* of set as governed by the principles mentioned earlier. At least, it is not at all clear without further argument that this is indeed so. Take, for example, a mastery of the concept 'tiger'. Does that mastery entail ability to decide *a priori* whether there are only finitely, or infinitely many, tigers? We should think not. But, it may be objected, this is simply because the concept in question applies to what the set-theorist calls *urelements*. Were we to take instead any sensible concept applying only to *pure sets*, then (so this reply goes) answers to such questions would be entailed *a priori* merely by adequate grasp of the concept involved.

But *would* they? Certainly the recent history of mathematics tells against such an assertion. Many a writer has denied the existence of infinite sets as completed totalities, even though not denying that there are infinitely many things of different kinds, such as natural numbers. And even if one concedes *a priori* that there be infinitely – indeed, even uncountably – many pure sets, that *still* falls short of securing a pure set with infinitely (or uncountably) many members. Are constructivists with reservations such as these to be accused of deficient grasp of the concept of set, even though they are perfectly well acquainted with the agreed principles governing the interrelationships among predication, set formation and membership?

The point we are making may best be put as follows. There is what may be called the *logic of sets*: a collection of rules governing these interconnections between set formation and membership, and predication and existence; and it is this logic alone which underlies proper grasp of the *notion* or *concept* of set. There is then what may be called the *theory of sets*, formulated according to one's ontological convictions. One may or may not postulate the existence of ω, the set of natural numbers. But whether we do or do not, we are all (classicists and intuitionists alike) agreed that, *if* we do, we shall be able to show that ω has strictly more subsets than it has members. This is because Cantor's proof, using separation most importantly among the ZF axioms, is thoroughly constructive (cf. Greenleaf, 1981). It is not even necessary to have the power set of ω in the picture itself as a completed set.

To see this, let us look more closely at how Cantor's theorem is proved. Cantor's reasoning shows that it absurd to assume that one can correlate subsets of ω one-one with members of ω, in such a way that every subset is dealt with. Given merely that ω exists, the diagonal argument requires only that one be able to 'cull' from ω a 'diagonal subset' which, on pain on contradiction, cannot be dealt with by the method of correlation presumed given. If R is the method in question, so that xRy means 'the subset x of ω is correlated with the member y of ω, then the diagonal subset is simply defined as the set of all z in ω such that z is not a member of the y such that yRz.

Since ω is assumed to exist, and since R is assumed to serve up, for each z in ω, the unique subset y of ω such that yRz, it follows, by separation, that this diagonal set will exist. Call it d. Consider now the member e of ω such that dRe. Is e a member of D? It is if and only if it isn't. No appeal is made to the existence of $P(\omega)$, the power set of ω. Given this *reductio* of the assumption that we could have any such method R, the constructivist is able to assert that *there are strictly more* subsets of ω than there are members of ω. He cannot be denied this cardinality reading of his result, since he agrees with the Cantorian analysis of equinumerosity in terms of one-one correlations. It is not a justifiable move even from the *classicist's* vantage point to re-interpret the Cantor proof (as Wright does on pp. 133 – 134) as establishing, not a result about uncountability (of the subsets of ω),

but rather one to the effect that there can be no effective enumeration of all decidable subsets of ω. And certainly the constructivist would refuse to be read that way. For the constructivist takes himself to be talking about correlations and subsets *tout court*. Wright himself does not do justice to the implicit power, from the constructive standpoint, of Cantor's result. Not only is Cantor's proof constructively acceptable, but, given the constructive interpretations available for the term 'countable', its conclusion can be made even stronger. Cantor's argument shows not only that $P(\omega)$ is uncountable, but that it is *not subcountable*, and that is *ω-productive*.

A *subcountable* set is one which is the range of some function whose domain is a *sub*set of ω. An *ω-productive* set is one in which, should any of its subsets be the range of a partial or total function defined on the natural numbers, one can find an element that lies outside that range. The notions *countable*, *subcountable* and *non-ω-productive* coincide classically. Intuitionistically, however, they come apart. Countable implies subcountable; subcountable implies non-ω-productive. To each converse, however, there is a constructive counterexample. It is ω-productivity which yields the strongest intuitionistic reading of the conclusion of Cantor's proof; and the method of proof directly justifies that reading. Detailed analyses of the constructive content of Cantor's proof with reference to the notions 'countable', 'subcountable' and 'ω-productive' have appeared in Grayson (1978) and Greenleaf (1981).

Note that in proving Cantor's theorem, the constructivist does not have to appeal to the power set axiom. Depending therefore on one's view of separation – is it a 'logical' axiom governing sets, or a 'mathematical' one? – one might regard Cantor's result as embedded in the very concept of set. One might even go so far as to question whether Wright is entitled to say (pp. 123–124)

> 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.

But to pursue this point here would be to digress, since what we have to say below is independent of any decision one might reach concerning the precise status of separation.

The logic of sets is formulated in Tennant (1978).[3] It consists of rules for the introduction and elimination of the set term-forming operator in contexts of identity. The introduction rule codes extentionality; the elimation rules code the conversion schema. The logic is a free logic, so that, for example, the reasoning behind Russell's paradox furnishes a proof that the Russell set does not exist. The logic is proved sound and complete with respect to the obvious semantics. Properly *mathematical* assumptions may then be made about the existence of sets: in particular, the null set and the set of natural numbers. But this, on our account, is to go strictly *beyond* what is involved in the correct analysis of the notion of set, as enshrined in the logical rules alone.

The null set axiom and the infinity axiom both make *outright* claims about existence. In so doing they are the clearest possible cases of what we take to be strictly *mathematical* claims about sets. But there is a penumbral family of axioms falling between the outright existence claims and the introduction and elimination rules mentioned earlier. These are the axioms of *conditional existence*: power set, pairs, unions, replacement, and choice. They all say that *if* such-and-such sets exist, *then* so too does one of a certain kind where the latter kind gives the axiom its special character. Now the interesting thing about separation is that it too makes a conditional existence claim, yet makes it so generally that it is difficult to regard it as doing anything more than merely contributing to the explication of the notion of set itself. Separation is an axiom schema, with instances obtained by choosing a particular formula $F(x)$. An instance will say 'for all sets y' there exists a set whose members are exactly those members x of y such that $F(x)$'. If we take separation as part of the logic of sets, then we have also as a purely 'logical' result that the null set exists if *any* set does: simply apply separation with '$\neg x = x$' for $F(x)$. But there will be no similarly quick way to the set of natural numbers. The existence of *that* set remains, on this analysis, a strictly mathematical postulate.

Having more or less clearly separated the ontological from the conceptual aspects of 'set', one can then go on to exercise further choice as to the logic appropriate for developing the consequences of whatever existential theoretical commitments one might care to make. For

the so-called logic of sets concerns itself thus far only with the curly brackets and epsilon, and identity. Nothing has yet been laid down for the logical connectives and quantifiers. A range of positions thus becomes available, each founded upon but properly extending the common core of analytical agreement over the conceptual aspect of 'set':

(i) postulate the existence of the null set
    and
    work with intuitionistic logic in the object language
(ii) postulate the existence of the null set
    and
    work with classical logic in the object language
(iii) postulate the existence of the set of natural numbers
    and
    work with intuitionistic logic in the object language
(iv) postulate the existence of the set of natural numbers
    and
    work with classical logic in the object language.

(iv) is the position of classical ZF. (ii) gives the classical theory of the hereditarily finite sets. (i) represents an extreme Ockhamite constructivism. (iii) is a natural and inviting alternative, for the intuitionist, to classical ZF. What we want to investigate is whether, had Wright but made such a clear and explicit choice as (iii), and had he been prepared to concede that (iii) was all that was available *informally in the metalanguage* as well, he might have seen the Skolemite problem in a different light, and offered a different constructive resolution of the paradox.

V

Skolem gave two proofs of his theorem. The first was in the paper of 1920. There he used the axiom of choice to construct a countable *sub*-model of any given model of a first order theory. The method was as follows: first one replaces each sentence of the theory, without loss of generality, by its Skolem normal form. This would be a sentence

beginning with universal quantifiers, followed by existentials, all appended to a matrix of a certain form. Next one chooses an element from the domain of the given model, and uses it multiply to instantiate the universals of the first chosen sentence. One then chooses (using the axiom of choice) at most finitely many existential satisfiers of the following existential quantifiers, and puts these alongside the original chosen element. Then one extends one's attention to the second sentence of the theory as well. (The sentences of the theory are assumed given in some countable enumeration.) One tries every possible way, using the finitely many elements so far in the picture, of instantiating the universal quantifier strings of both the first and the second sentence. For each way at most finitely many new existential satisfiers for the following existential quantifiers have to be chosen (again using the axiom of choice). One proceeds in this way, progressively taking into account more and more sentences of the theory, and recruiting new satisfiers for the existentials in each sentence with respect to each of the increasingly numerous ways of instantiating their universal prefixes. The countable model being extracted is in an obvious sense the product of this process in the limit. It arises, one might say, by *interated existential closure*, via the axiom of choice, from the Skolemised surrogates of the original sentences of the theory.

There is another proof using choice, which Skolem could have used, given that Zermelo had established in 1904 that choice is equivalent to the well-ordering principle. This proof applies choice globally at the outset, by taking the domain to be well-ordered. Iterated existential closure then simply trawls the ordering for its countable catch.

In his second proof of the theorem, in his paper of 1922, Skolem drops the appeal to the axiom of choice and thereby proves a slightly different result. No longer is it a *sub*model of a given model that is being constructed; rather, it is a model erected on the natural numbers. The assumption that Skolem normal forms are available is, as before, an absolutely crucial feature of his method of proof for the classically understood object language.

But there is the possibility also of using *prenex* normal forms, as is done in the classical case by Quine (1959) and Grandy (1977). We

note this as an alternative to Skolem's method, and return to it below.

Now what is remarkable, in the light of Benacerraf's discussion, is that it is only in the 1922 paper that Skolem formulates the set theoretic paradox that now bears his name. He saw it as quite sufficient simply to produce *some* countable model of the theory, rather than a countable submodel of some given intended model. And his philosophical conclusion about higher infinities cited at the beginning of this paper came but one paragraph after a much more general conclusion that he drew concerning the relativity of the (classical) *notion of set* itself:

Die axiomatische Begründung der Mengenlehre führt zu einer Relativität *der Mengenbegriffe*, und diese ist mit jeder konsequenten Axiomatik untrennbar verknüpft. (Our emphasis; in the original the whole sentence is italicised.)

One might even speculate that Skolem himself would have been aware of the significance, if any, of the difference between a countable model extracted from an intended model of set theory and a countable model erected directly upon the natural numbers, insofar as philosophical conclusions about conceptual relativity were in the offing. For he, after all, was the author of both kinds of theorem. The first used choice to rummage within a given model and pare it down. The second eschewed choice by starting with a (set theoretically) phoney line-up.

It might be maintained in response to this, and on Benacerraf's behalf, that Skolem could well have been blind to the difference, since he had not bothered to reflect further on the possibility of apportioning blame between our interpretation of the universal quantifier and grasp of the notion of the uncountable. But what precisely is the extra significance accorded by Benacerraf to the version of the countable model theorem which has one *extract* it from an intended model?

Benacerraf sees the extraction as somehow preserving the interpretation (assumed correctly given in the intended model) of the membership relation. This is because, according to him, the countable model of set theory extracted from the intended model will, like its parent model, be *transitive*. Transitivity, an important global feature of the membership relation, must not be lost if the constructed model is to have any

claim at all to be a model of *set* theory. But what is transitivity, exactly? Benacerraf defines it as follows:

> A model is *transitive* if and only if each element of each set in the model belongs to the domain of the model. (p. 101)

Let b be a set in the model. That is, let b be a member of the domain of the model. What is it for a to be an element of b? There are two answers to this question. First, the *internal* one: a is an element of b just in case a is, like b, in the domain of the model, and the ordered pair (a, b) is in the extension of 'ε' within the model. That is, a is, *according to the model*, an element of b. Secondly, the *external* answer: both a and b might have genuine properties, or genuine internal structure, not registered within the model. They are recruited as members of the domain of the model and assigned, within the model, various skeletal relations to each other (perhaps) and to other members of the domain. The assignment can ignore their genuine properties and internal structure. The web of relations within the model can fail to unpack the metaphysical richness which they intrinsically bring with them 'from outside' the model, so to speak. It is a little like treating a Royal procession as a model for a strict linear discrete ordering with first and last elements. The kinship relations and the character traits go unheeded. So too with sets – according to Benacerraf. We may put the genuine power set of ω into the domain of a countable model of set theory, but this model will be a transitive model only if every one of that set's members – that is, every set of natural numbers – is also in the model. The external reading has it that what is really the case with membership must be properly reported within the model itself – otherwise the model won't be transitive.

There are problems with both the internal reading and the external reading of transitivity. First, on the internal reading every model will be transitive; so the requirement of transitivity is trivial. For what the model *says* bears epsilon to b will obviously have to be in the domain of the model! Secondly, on the external reading no countable model of ZF that contains the genuine power set of the naturals can possibly be transitive! For, if the model contains $P(\omega)$ then, in order to be transitive, it would also, as just observed, have to contain every member of $P(\omega)$. But there are uncountably many such members – so

some of them would have to be missing from the domain of the model in order for it to be countable, as supposed.

Thus, for the model to be transitive, the genuine $P(\omega)$ itself would have to be missing. One might put the familiar response as follows: the set term '$P(\omega)$' is not *rigid* – it does not denote the *same* set as one passes from one transitive model of set theory to another. And the failure of *sameness* is a radical one, involving (from an external perspective) not just substitution of an isomorph, but also collapse of cardinality at times. Any countable *transitive* submodel of the intended model therefore cannot contain the genuine $P(\omega)$ as its own denotation of the set term '$P(\omega)$'. With this symptom of Skolemite non-standardness, it is difficult to see how in a countable model the further requirement of transitivity could yield assurance that the model be any better behaved on epsilon than any other model would be. The intension of 'ε', in allowing an extension to be determined for 'ε' in any countable domain for ZF, is irretrievably parableptic.

There is a further, and in our view clinching, reason not to be persuaded of the alleged philosophical relevance of Benacerraf's distinction, accepted by Wright, between 'set models' delivered by SMT and the 'numerical models' delivered by Skolem's 1922 proof of the countable models existence theorem. Benacerraf's version SMT of the Löwenheim – Skolem theorem is no more telling in posing Skolem's paradox than would be any version merely guaranteeing the existence of a countable model of set theory. For his own philosophical purposes in his paper, Benacerraf could just as well have stated his version of the theory as follows (a version which follows from Mostowski's contraction lemma):

Any *standard* model for ZF has a countable submodel ε-isomorphic to the *minimal model*; where the latter is the sole model which is a countable standard transitive model of ZF + V = L and also a submodel of every standard transitive model of ZF.

A *standard* model is one in which 'ε' is interpreted as set-membership. The *minimal model* helps, as it were, to uniformize the Skolemite's ministrations.[5] Here is the best behaved countable fragment of the *real* epsilon relation that one can get. But the *real* power set of the naturals must, by our foregoing considerations, *not* be caught up in

this fragment. Something else in the fragment is playing the role of that power set. $P(\omega)$ is therefore quite unlike the Benacerrafian 3 (cf. Benacerraf, 1965). For while Benacerraf was able to conclude that the number 3 was no particular set on any set-theoretic construal of numbers, but rather a structural locus – the role played by whatever 'is' 3 in any standard recursive progression – he is robbed of a similar thought concerning $P(\omega)$. He cannot say that $P(\omega)$ is the role played by whatever 'is' $P(\omega)$ (that is, whatever is denoted by '$P(\omega)$') in any standard transitive model of set theory. For the most crucial feature of $P(\omega)$ is that it has (by Cantor's reasoning) uncountably many members. Yet here, in the countable model called the minimal model (which is both standard and transitive), whatever it is that stands as the denotation of '$P(\omega)$' does *not* have uncountably many members! – neither within itself, 'genuinely' (for the model is both standard and transitive) nor by model-relative alliance via 'ε' (for the model is countable). By contrast, the Benacerrafian 3 always has *three* predecessors (0, 1 and 2) in any progression.

VI

Thus far we have made the classicist's predicament more pointed. And we take the challenge to be not so much how to get out of that predicament, but rather how to avoid getting into it in the first place. We have not yet investigated the consequences of position (iii) above: the one that postulates the existence of the null set and the set of natural numbers, but restricts one to intuitionistic logic (both in the object language and, let us also assume, in the metalanguage). Restriction to intuitionistic metalogic is important. We shall now see how all the familiar proofs of the countable models theorem are intuitionistically objectionable. These include Skolem's proof discussed above; the proof using prenex normal forms, which we mentioned in passing, and the proof by Henkin's method. Once we have seen how all these methods of proof fail, we shall advance perfectly general reasons for their doing so: we shall show that a general form of the countable models theorem is independent of a strong version of intuitionistic Zermelo Fraenkel set theory (IZF).

First, the use of the axiom of choice in Skolem's proof of his theorem is intuitionistically unacceptable. Choice is constructively correct

for certain sets, such as the natural numbers, but not for arbitrary sets. Yet it is in the more general setting that Skolem needs choice. It is simply mistaken to think that on an intuitionistic construal of operators in the object language, the truth of a sentence of the form

$$\forall x_1 \ldots \forall x_n \mathrm{E} y_1 \ldots \mathrm{E} y_m P(s_1, \ldots, x_n, y_1, \ldots, y_m)$$

requires that there be a uniform effective method for choosing, for each $a_1, \ldots, a_n$, appropriate members $b_1, \ldots, b_m$ of the domain so that $P(a_1, \ldots, a_n, b_1, \ldots b_m)$. A sentence of that form could be intuitionistically provable for reals without there being any *uniform* effective method as described; although over the naturals it is always possible to uniformize the respective methods for each instantiation of the universal prefix (cf. Diaconescu, 1975). (Thus Dummett is in error when he claims (1977, p. 64) "As *always*, a form of the axiom of choice holds good. . . ." (our emphasis).)

Intuitionistically incorrect also is the over-swift assumption that each sentence of the theory to be Skolemized can be replaced by an *intuitionistically* equivalent sentence of an appropriate syntactic form in order for the construction of the model to go through, even with liberal use of choice (which, however, as we have noted, the intuitionist cannot permit in the general case). So if we are in the business of looking for an intuitionistic analogue of the well known proof, due to Skolem, of the existence of countable submodels, we have to enquire more closely about the first step in his proof for the projected intuitionistic version of his result.

What results are there concerning Skolem normal forms for sentences of first order languages? The answer is that, for the would-be constructivist Skolemite, they are distressingly meagre. First, there appears to be little prospect of furnishing a countable model using choice in the light of the limited extent to which sentences could be replaced by intuitionistically equivalent Skolem normal forms, as revealed in a proof-theoretical study by Minc (1972). Smorynski (1978) has since established the same negative result by simpler model-theoretic methods: Skolemization cannot in general be obtained within the bounds set by constructive logic. (It is worth remarking here that the axiom schema of separation in IZF will have arbitrarily complex instances. Thus it would be futile to look for "Skolemisability

within limits" on logical complexity of one's set of axioms.) Skolem himself started, it is now clear, from an unregenerately classical vantage point. At this point one could ask (as Michael Resnik did in correspondence) whether the intuitionist might not be able to mimic the prenex method of proof, even if not the Skolem method. But here too (with the most obvious mimicking) he would be frustrated. The cut elimination theorem for intuitionistic logic (cf. Dummett, 1977, p. 150) has as a corollary that theoremhood of prenex forms is decidable. But then this would contradict the undecidability of theoremhood in the full language (which holds even in the monadic case for intuitionistic logic), *if* the prenex normal form theorem held for intuitionistic logic. For, given a sentence A, one could find an equivalent prenex form A' simply by enumerating proofs; and then apply to A' the decision method for theoremhood provided by the cut-elimination theorem. This would yield a decision as to the theoremhood of A.

To re-inforce this point, note what can happen on the intuitionistic front if one tries to apply the standard prenex normal form algorithm from the classical camp. Consistent theories can then be converted into inconsistent ones. For example, $\neg\forall x(FX \vee \neg Fx)$ is intuitionistically *consistent*. But the standard algorithm for prenexing converts this to $\mathrm{E}x\neg(Fx \vee \neg Fx)$, which is intuitionistically *inconsistent*.

Another way of proving the classical Löwenheim–Skolem theorem is by Henkin's method. Could *this* way possibly be adapted so as to meet constructivist requirements? On this approach, one starts with a consistent set of sentences and expands it to a maximal consistent set with witnesses. Then one defines a canonical model autonomously on the language of the expanded set and shows that its theory is precisely the expanded set of sentences. And since the model, by construction, is countable, we have the desired result. This method, however, is non-constructive at the point where one expands the original set of sentences. One does so by contemplating both sentences and formulae in one free variable drawn from two assumed denumerable lists. One adds a sentence when it is consistent to do so, and one adds a fresh instance of a formula should it be consistent to assume its existential quantification. This requires an infinite sequence of choices, each made after deciding a question of consistency. But we know by Church's theorem that there is no general recursive method for

making such decisions; hence the Henkin method is not available. Therefore, the intuitionist, as long as his mathematics is consistent with Church's Thesis, cannot use Henkin's method to establish a general countable model theorem and so to produce a Skolem paradox. (This criticism applies just as forcefully to the classical proof of the completeness of intuitionistic logic with respect to Kripke models, as given in Tennant (1978).)

But what about *intuitionistic* proofs of the completeness of intuitionistic logic? There are two of these in the literature, by Veldman (1976) and de Swart (1976). Veldman's proof uses a single 'universal' model, whereas de Swart's uses a certain class (more precisely: *fan*) of (countable) models. The models under discussion here are neither the familiar structures of classical model theory nor standard topological models but generalized Beth and Kripke structures. By 'generalized', we mean that allowance is made for the possibility that both a sentence and its negation might be forced at some node in the underlying frame.

It suffices to consider de Swart's method more closely in order to see that, even here, there is no *constructive* countable models theorem in the offing. Let the fan be B. We define 'the set X of sentences B-implies the sentence P' in the obvious way: for every structure M in B, if every member of X is valid (i.e. intuitionistically true) in M, then so is P. De Swart's completeness proof provides a constructive method for producing an intuitionistic proof of P from some finite subset of X, on the assumption that X B-implies P. Unlike Henkin, he does *not* provide a method (let alone a constructive one) for producing, for any consistent set Y of sentences (that is, a set Y which cannot be proved inconsistent using intuitionistic logic), a countable structure in B making every member of Y true. Nor is this a construction in their completeness proofs.

A result of Gödel, as reported by Kreisel (in Kreisel, 1962) shows that there can be no direct analogue of Henkin's method in the intuitionistic case. Let $T(A, M)$ be the sentence in set theoretic notation that expresses "A is true in the model M"; let Con(A) and Prov(A) be as usual (with reference to intuitionistic logic). Let CM (for "consistency implies model existence") be the claim

$$\text{for all } A, \text{ if Con}(A) \text{ then, for some } M, T(A, M)$$

and let VP (for "validity implies provability") be the claim

for all A, if for all M, $T(A, M)$, then Prov(A).

Each of CM and VP constructively implies (in IZF) arithmetic Markov's principle. But at least infinitely many instances of Markov's principle are independent of IZF. Therefore neither CM nor VP is provable in IZF.

There is a fairly extensive literature on the completeness problem for intuitionistic predicate logic, a representative sample of which would include Kreisel (1962), Leivant (1972) and van Dalen (1973). Surveys of results on completeness appear in Dummett (1977) and Troelstra (1977). Relatively little of this material bears directly on the constructivity of the general Löwenheim – Skolem theorem. Those results that do apply, e.g., theorems of the form

if A is not a theorem, then there is a subcountable model for not-A

hold at most for restricted classes of formulae such as the class of negative formulae. It would not be possible, therefore, to apply these results, without further ado, to the axioms of set theory and of arithmetic. (See the final section for more detailed argument on this last point.)

Note that the results of Gödel and Kreisel, as well as those to be obtained below, do not require the creation of any arcane version of "intuitionistic model theory". The model theory that we shall do in (informal) IZF and its extensions involves simple duplications of the existing definitions from classical model theory for such notions as "sentence A holds in model M". All that differs in our treatment is the underlying (meta)logic, which of course is intuitionistic. In particular, we can help ourselves to the normal Tarskian clauses in the definition of model relative satisfaction.

VII

We have found no evidence so far that the intuitionist can visit upon himself, on the mere assumption that his set theory is consistent, that Skolemite embarrassment that now may be the peculiar and dubious

privilege of the classicist. Indeed, this is no mere appearance. We can advance rather general considerations in support of the claim that no countable models theorem of any of the usual straightforward forms is intuitionistically provable. These general negative results consolidate and extend all the frustrations so far of attempts to devise constructive analogues of the countable downward Löwenheim–Skolem theorem. Let us now explain how the results are obtained. Note first that one obviously cannot *refute* the downward claim in IZF, since it holds in a classical, consistent extension of IZF. So what we have so show is that we cannot *prove* the downward claim in IZF.

A corollary to a general theorem in McCarty (1984) is the well known more specific fact that;

a set of natural numbers is recursively enumerable in the classical sense if and only if that set is countable in the Kleene realizability model V(Kl) for IZF+.

IZF+ is intuitionistic Zermelo–Fraenkel set theory and other strong principles. These include Church's Thesis; Markov's Principle; Brouwer's theorem; various forms of choice (strong natural forms of which are relativised dependent choice and the Blass–Aczel presentation axiom) and a panoply of other axioms including the uniform reflection principle.

Moreover, is it a simple (constructively provable) recursion-theoretic fact that:

immune sets exist.

These are sets that have no infinite recursively enumerable subsets (cf. Rogers, 1967, p. 106). Putting these two results together, it is easy to see that the countable downward Lowenheim–Skolem claim is independent of IZF plus the other principles that hold in V(Kl). One version of this claim which we shall now show cannot be proved in IZF+ is the following:

(C_1) for every set X of sentences, for every model M of X, there is a countable submodel of M satisfying X.

In what follows the turnstile represents intuitionistic deducibility.

THEOREM 1. *IZF+* $\nvdash C_1$.

Proof. To see why C_1 cannot be proved in IZF+, take any immune set I (such sets exist). Consider I^*, its analogue in V(Kl). In V(Kl), I^*

is a subset of ω and has no infinite *countable* subset. (This is because countability, as noted above, is the realizability analogue of recursive enumerability.) It follows that there is no countable submodel of $(I^*, =)$ for the theory of identity over I^*. For this theory contains the sentences E_n (n any integer) saying "There are not not at least n individuals". If there *were* a *countable* submodel of the theory, it would accordingly have to be infinite, contradicting what we know about I^*. ■

Note that this proof actually establishes a stronger independence result than the one already stated. For it shows the independence of the following weaker claim:

(C_2) For every *recursively enumerable* (r.e.) theory X, for every model M of X, there is a countable submodel of M satisfying X.

THEOREM 2. $IZF+ \nvdash C_2$.
Proof. See above. ■

This observation deals with the possible complaint from the Skolemite that he is concerned to visit the paradox on set theory, which is axiomatisable.

The reader should be reminded that the realizability methods used in the proofs do not adversely affect the generality of the independence claims just made. Granted, the proofs themselves rely upon phenomena which are at present of very restricted mathematical application: immune sets and their theories of identity. The statements shown to be independent of IZF do not partake of any correlative restriction; on the contrary, they remain perfectly general versions of the downward Löwenheim – Skolem Theorem. Assertions such as C_2 are not to be understood as restricted to those models whose domains are immune sets or to theories which are theories of immune sets. The statements shown to be independent do not refer to or suffer restriction from immune sets in any way. (It might be well to compare this situation with the more familiar one of Cohen forcing. The Continuum Hypothesis neither refers to nor suffers restriction from forcing

conditions and generic sets, even though these constructs enter into the proof of its independence.)

We should also point out that an examination of the details of the proof of Theorem 1 will license two further strengthenings of Theorem 2. Consider the statement:

($C_{2.5}$) For every r.e. theory X, for every model M of X, it is *not not* the case that there is a countable submodel of M satisfying X.

THEOREM 2.5. *($C_{2.5}$) is not provable in IZF+*

Proof. this is immediate from the proof of Theorem 1. ∎

Next, recall that a sentence is negative whenever it is equivalent, within a constructive theory, to a sentence devoid of disjunctions and existential quantifications and in which every atomic sentence appears doubly negated. Our proof of Theorem 1 also shows that we can take X to be a set of negative sentences such as "There are not not at least n individuals" for arbitrary n.

What $C_{2.5}$ and our remark on negative sentences show is that no superficial 'negativization' or application of a Gödel–Gentzen negative translation suffices to bring back the strong Löwenheim–Skolem Theorem in its standard form.

Our proofs of Theorems 1 and 2 do not, in themselves, require that the existence of immune sets be provable constructively – for example, within IZF or IZF plus MP. We can suppose that the metatheory in which we define V(Kl) and work with it is classical ZF. So in the metatheory we can avail ourselves of all the benefits of classical mathematics, including the ready assurance that immune sets exist. On the other hand, we can prove, even constructively, that immune sets exist – Post's original argument (Rogers, p. 106) is readily constructivized. And we can define the realizability structure and prove the fundamental results about it in a constructive metamathematics.

So the prospects of an intuitionistic analogue of the full countable downward Löwenheim–Skolem theorem are bleak indeed. In fact, it can already be seen that it would even be a slight understatement of our result to point out that *the assumption of a strong counterexample*

(using immune sets) *to the countable downward claim is consistent with all of Bishop's constructive mathematics.* This understatement is true because all the latter can be done in IZF plus relativised dependent choice.

So far the Skolemite appears to be intuitionistically empty-handed as far as countable *sub*models are concerned. But might he clutch at a surviving straw: the existence of countable models *überhaupt*, be they submodels or not of the original model? Once again, the answer is no. For the following claim is independent of IZF+:

(C_3) for every set X of sentences, for every model M of X, there is some countable model M' (not necessarily a submodel of M) satisfying X.

Bear in mind that on a constructive interpretation, if we have X and M then the countable model M' whose existence is guaranteed by the claim depends parametrically on X and M, and so too does the counting function (with domain ω) that makes it countable. Once again we concentrate only on the need to show that the claim (C_3) does not *follow* from IZF+.

THEOREM 3. *IZF+* $\nvdash C_3$.

Proof. Here first is a summary of the argument:

Consider once more the realizability model V(Kl). Let $[i]$ be the i-th partial recursive function under the standard enumeration. Were (C_3) to be provable, and (by the soundness of the realizability semantics) true in V(Kl), the predicate "$[i]$ is a total recursive function" would be recursively enumerable. But the predicate in question is known not to be recursively enumerable (cf. Rogers, 1967, p. 264). Hence (C_3) is not provable.

Let us now expand this summary of the result with more argumentative detail.

Assume for *reductio* that (C_3) is true in V(Kl). For each natural number i, consider the set

$$\mathbf{i} =_{\text{df}} \{0\} \cup \{1 : [i] \text{ is total}\}$$

Let M be the model with $\mathbf{i}$ as its domain, and identity as its only relation. We consider only the language of identity. Let X be the

theory in this language for M. It follows from the assumed truth of (C_3) in V(Kl) that X has a countable model M', with enumerating function f_i. (Here we are following our recent advice, by bearing in mind that this counting function depends parametrically on i.) This is because the recursion-theoretic properties of i help at least in part to determine the original model M of the claim (C_3) above, which is the focus of our *reductio*.

Since the domain of M is a subset of the integers, M satisfies the claim that identity is decidable:

$$\forall x \forall y (x = y \vee \neg x = y)$$

Thus this sentence is in X. Hence, by assumption, it holds in M' as well. Now (c.f. Minio, 1974) countable sets with decidable equality are isomorphic (with respect to $=$) to subsets of ω. Thus the domain of M' can without loss of generality be taken to be a subset of ω. Now, since f_i maps ω into ω, we may assume that f_i is total recursive, with index e_i depending *effectively* on i. (That is, f_i is $[e_i]$.) This is because V(Kl) satisfies *Church's Thesis*: that every number-theoretic function is total recursive.

Now the statement that says that the recursive function $[i]$ is total has the form

$$\forall n \mathrm{E} m P(i, m, n)$$

where the three-place predicate P is primitive recursive.

We shall now prove the equivalence

$$\forall n \mathrm{E} m P(i, n, m) \text{ if and only if } \mathrm{E} n \mathrm{E} m \neg f_i(n) = f_i(m)$$

which contradicts well-known results of ordinary recursion theory. For the right hand side is a recursively enumerable predicate of i. But the left hand side, expressing the claim "$[i]$ is total", is not recursively enumerable (cf. Rogers, p. 264). This will complete the *reductio* of the assumption that (C_3) is true in V(Kl). Hence (C_3) cannot be proved in IZF+.

We establish the equivalence above by arguing first in the direction from left to right:

$$\begin{array}{cl}
\forall n \mathrm{E}m P(i, n, m) \text{ (that is, } [i] \text{ is total)} & \\
\cline{1-1}
\mathbf{i} = \{0, 1\} & \text{by definition of } \mathbf{i} \\
\cline{1-1}
M \models \mathrm{E}x\mathrm{E}y \neg x = y & \text{by Tarski clauses} \\
\cline{1-1}
M' \models \mathrm{E}x\mathrm{E}y \neg x = y & \text{by choice of } M' \\
\cline{1-1}
\mathrm{E}x \text{ in } M' \quad \mathrm{E}y \text{ in } M' \quad \neg x = y & \text{by Tarski clauses} \\
\cline{1-1}
\mathrm{E}m\mathrm{E}n \neg f_i m = f_i n & \text{since } f \text{ is onto}
\end{array}$$

and then from right to left:

the proof schema

$$\dfrac{\dfrac{t \text{ in } i}{t = 0 \vee (t = 1 \,\&\, [i] \text{ is total})} \quad \dfrac{(1)}{t = 0} \quad \dfrac{\dfrac{\dfrac{\dfrac{\dfrac{}{t = 1 \,\&\, [i] \text{ is total}}\,(1)}{\begin{array}{c}[i] \text{ is total} \\ \text{i.e. } \forall n \mathrm{E}m P(i, n, m)\end{array}}}{\mathrm{E}m P(i, a, m)} \quad \neg \mathrm{E}m P(i, a, m)}{\wedge}}{t = 0}}{t = 0}\,(1)$$

establishes the inference $\dfrac{t \text{ in } i \quad \neg \mathrm{E}m P(i, a, m)}{t = 0}$ for arbitrary t.

This is now applied twice over in the following proof, along with Markov's principle (for primitive recursive F) in its inferential form

$$\dfrac{\begin{array}{c}\dfrac{}{\neg \mathrm{E}m Fm}\,(1) \\ \vdots \\ \wedge\end{array}}{\mathrm{E}m Fm}\,(1), \quad \text{and the axiom scheme} \quad \overline{f_i t \text{ in } M'}.$$

$$\dfrac{\dfrac{\dfrac{\mathrm{E}m\mathrm{E}n\neg f_i m = f_i n \qquad \dfrac{\dfrac{\dfrac{\overline{f_i c \text{ in } M'} \quad \dfrac{(2)}{\neg \mathrm{E}mP(i, a, m)}}{f_i c = 0} \qquad \dfrac{\overline{f_i d \text{ in } M'} \quad \dfrac{(2)}{\neg \mathrm{E}mP(i, a, m)}}{f_i d = 0}}{f_i c = f_i d} \qquad \dfrac{(1)}{\neg f_i c = f_i d}}{\wedge}}{\wedge}\,(1)}{\mathrm{E}mP(i, a, m)}\,(2)\ \text{Markov}}{\forall n \mathrm{E}mP(i, n, m)}$$

i.e. $[i]$ is total

This completes our proof of the equivalence, and also the proof of Theorem 3. Again, the reader is cautioned not to confuse the method by which we prove the theorems with the statements whose independence the theorems establish. We have shown that a general countable Löwenheim–Skolem theorem is not constructively provable by giving a formalization of the theorem its realizability interpretation. On the interpretation, the statement of the general Löwenheim–Skolem theorem comes to imply that a certain uniform effective method exists. By marshalling other considerations, one shows that the required method cannot exist and, hence, that the Löwenheim–Skolem theorem is not constructively provable. It is essential to note that neither the statement of the Löwenheim–Skolem theorem, nor its formalization nor its standard constructive interpretation asserts, or even implies, that there is such an effective method. It is not the case that our proof shows no more than that an "effectivization' of the Löwenheim–Skolem theorem is independent of IZF+. Rather, it shows just what it purports to show – that the general theorem itself is not constructively provable.

There now arises the following possible objection:

"You have proved the independence of (C_3) only from IZF+. But IZF+ does not contain the Fan Theorem or Bar Induction. So how do you know that there is not proof, using these stronger principles, of a form of the downward Löwenheim–Skolem theorem?"

The answer to this objection is as follows. One can show (as we shall below) that the strong downward countable Löwenheim–Skolem

theorem is constructively inconsistent with Markov's Principle for arbitrary natural number functions. In the terminology of Brouwerian intuitionism, the conjunction of the two claims has a "weak counter-example". That is, *modulo* IZF (or even second-order Heyting arithmetic), they imply the law of excluded middle for arbitrary sentences. For the formal statement of the result, let us introduce some abbreviations:

(C_4) is the claim: for every model M there is a countable model M' elementarily equivalent to M.

MPF is Markov's Principle for arbitrary natural number functions:

$$\text{if } \neg\forall x\forall y(fx = fy) \quad \text{then} \quad \mathrm{E}x\mathrm{E}y\neg fx = fy.$$

THEOREM 4. *Let A be an arbitrary formula. Then IZF, MPF, $C_4 \vdash A \vee \neg A$.*

COROLLARY 1. *Since V(K1) satisfies MPF but not all formulae of the form $A \vee \neg A$, it follows that C_4 is independent of IZF+.*

COROLLARY 2. *Since there is a sheaf model (cf. Fourman and Hyland 1979) for the fan theorem, bar induction and Markov's Principle that does not satisfy all formulae of the form $A \vee \neg A$, it follows that C_4 is independent of IZF plus the fan theorem plus bar induction.*

Proof. Since $\neg\neg(A \vee \neg A)$ is an intuitionistic theorem, it suffices to show, for any sentence B, that IZF, MPF, C_4, $\neg\neg B \vdash B$. So let $\mathbf{B} =_{\mathrm{df}} \{0\} \cup \{1 : B\}$. In other words, x is in $\mathbf{B}$ if and only if $(x = 0 \vee (x = 1 \; \& \; B))$. We shall identify $\mathbf{B}$ with the model whose domain is $\mathbf{B}$ and whose only relation is the identity relation.

Assume B holds, Then $\mathbf{B}$ by definition contains 0 and 1. Thus were we to assume C: $\mathbf{B} \vDash \forall x\forall y(x = y)$, a contradiction would ensue by virtue of the Tarskian clause for the universal quantifier, and the standard interpretation of $\vDash$.

By the intuitionistic proof schema

$$\dfrac{\dfrac{\begin{matrix}\underbrace{{}^{(2)}\overline{C}\qquad\overline{B}^{(1)}}\\ \vdots\\ \wedge\end{matrix}}{\neg B}\,(1)\qquad\qquad \neg\neg B}{\dfrac{\wedge}{\neg C}\,(2)}$$

We have a proof of $\neg C$ from the assumption $\neg\neg B$. We shall now continue with a proof of B from $\neg C$. This will establish overall that B follows from $\neg\neg B$.

So assume $\neg C$, that is, $\neg(\mathbf{B} \models \forall x \forall y(x = y))$. By the Tarskian clause for negation,

$$\mathbf{B} \models \neg \forall x \forall y(x = y).$$

Now, by C_4, let N be a countable model elementarily equivalent to $\mathbf{B}$ with respect to the language of identity. Let f be the function from ω to (the domain of) N that enumerates it. Since $\mathbf{B}$ is a subset of ω, identity is decidable on $\mathbf{B}$; that is, $\mathbf{B} \models \forall x \forall y(x = y \vee \neg x = y)$. By elementary equivalence,

$$N \models \neg \forall x \forall y(x = y).$$

By elementary equivalence, we also have that identity is decidable on N. By Minio (1974) as before, we can assume without loss of generality that N is a subset of ω and, hence, that f is a number-theoretic function. Since

$$N \models \neg \forall x \forall y(x = y),$$

we have

$$\neg \forall m \forall n(fm = fn)$$

By MPF we obtain

$$\mathrm{E}m\mathrm{E}n \neg fm = fn$$

Thus $N \models \mathrm{E}x\mathrm{E}y \neg x = y$. Hence by elementary equivalence, $\mathbf{B} \models \mathrm{E}x\mathrm{E}y \neg x = y$. It follows by the Tarskian clauses for the

universal quantifier and negation that

$$\mathrm{E}x \text{ in } \mathbf{B}\ \mathrm{E}y \text{ in } \mathbf{B} \neg x = y$$

Now note that by the definition of **B**, the following schematic inference is valid:

$$\frac{t \text{ in } B}{t = 0 \vee (t = 1 \,\&\, B)}$$

We proceed intuitionistically as follows:

$$
\begin{prooftree}
\AxiomC{$\mathrm{E}x \text{ in } \mathbf{B}\ \mathrm{E}y \text{ in } \mathbf{B} \neg x = y$}
\AxiomC{(3)}
\UnaryInfC{$b \text{ in } B$}
\UnaryInfC{$b = 0 \vee (b = 1 \,\&\, B)$}
\AxiomC{(3)}
\UnaryInfC{$a \text{ in } B$}
\UnaryInfC{$a = 0 \vee (a = 1 \,\&\, B)$}
\AxiomC{(1)}
\UnaryInfC{$a = 0$}
\AxiomC{(2)}
\UnaryInfC{$b = 0$}
\BinaryInfC{$a = b$}
\AxiomC{(3)}
\UnaryInfC{$-a = b$}
\BinaryInfC{$\wedge$}
\UnaryInfC{B}
\AxiomC{(1)}
\UnaryInfC{$a = 1 \,\&\, B$}
\UnaryInfC{B}
\RightLabel{(1)}
\TrinaryInfC{B}
\AxiomC{(2)}
\UnaryInfC{$b = 1 \,\&\, B$}
\UnaryInfC{B}
\RightLabel{(2)}
\TrinaryInfC{B}
\RightLabel{(3)}
\BinaryInfC{B}
\end{prooftree}
$$

This completes our proof of B from $\neg\neg B$, and also the proof of Theorem 4.

Insofar as *countable* models are concerned, then, our four theorems appear to block the most obvious routes to the result, in any of its usual straightforward forms, that the Skolemite needs.[6]

VIII

But what about *subcountable* models – whether or not they are *sub*-models of the original model? So far we have shown the independence of the downward claim involving the cosntructively strong notion of countability, thereby apparently weakening the independence result. It remains to be seen whether independence survives upon substitution of 'subcountable' for 'countable'. But whether it does or not, we may have already drawn the Skolemite's sting. For it is consistent with IZF+ to assume that extremely capacious sets are subcountable. For example, in V(K1) every metric space – including the reals – is

subcountable! The intuition of the capaciousness of the reals holds firm, backed by Cantor's argument. Indeed, it would be one of the standards against which one would judge the appropriateness or adequacy of any attempted explication, in mathematical terms, of the notion of cardinality.

It is clear from McCarty (1984) and Grayson (1978) that subcountability cannot serve as a fully satisfactory constructive measure of the size of a set. For subcountability is incapable of sustaining distinctions of cardinality which the constructivist wishes to make. Insofar as there is a constructive theory of cardinality, it uses the notion of countability. This is the notion based on *total* counting functions, which is already so familiar to the classicist.

Finally, what about negative translations? On the basis of these translations, it may appear, at first sight, as though the paradox could be re-instated. The line of thought would run as follows: As is well known, there are negative translations (Friedman, 1973; Powell, 1975; Beeson, 1985; Leivant, 1985) of classical set theory into intuitionistic 'correlates'. Cannot now the 'negative' version of LST induce Skolemite stress? Our reasons for disagreeing are as follows. The general pattern is this: a translation f is defined so that the following holds:

$$\text{If ZF} \vdash_C \phi \text{ then IZF} \vdash_I f\phi.$$

But the translation f has to be defined with some care, in order to overcome certain difficulties presented by set theory, Friedman, in his choice of f, went beyond the usual double-negation treatment of atomic formulae, disjunctions and existential quantification by also replacing any atomic formulae $a \in b$ with an extremely complicated formula in a and b. And Powell, in his choice of f, went beyond the Gödel–Gentzen dualization of $\vee$ and $\exists$ in terms of $\neg$, & and $\forall$ by further restricting all quantifiers to range over *stable sets* (sets in which not not being a member implies being a member).

Now, in the light of these remarks, we have to consider the putative objection based on the observation that (for some such choice of f)

$$\text{If ZF} \vdash_C \text{LST then IZF} \vdash_I f(\text{LST}).$$

Is this an adequate objection to our pessimism over the prospects for a constructively acceptable analogue of the downward Löwenheim–

Skolem theorem? We think not. For the statement f(LST) is not a statement of constructive model theory. The tampering with $\in$ or with the range of the quantifiers give the lie to the objector's reading of f(LST) as a version of LST in any serious sense. But quite apart from those features of f, the logical rewriting via the negative part of the translation totally obstructs such a construal. For even f(LST) would not deal with theories (sets of sentences closed under derivability) but with collections X of sentences such that, if it is not not the case that B is derivable from X, then B is not not a member of X. In the same way, the "double negative translation" of the predicate "is a model" is not "is a model".

One would want to say very much the same thing about the negative form of the classical mean value theorem (MVT), which is also not constructively provable. The negative translation of the MVT does not afford a counterexample to the claim that the MVT is independent of constructive set theory because the negative form of the theorem is, quite simply, not a statement of real analysis. It deals neither with real-valued functions nor with real values.

Secondly, the negative translation of the Löwenheim–Skolem theorem is not likely to give rise to worries of the Skolemite sort because it does not assert the existence of countable non-standard models. All it would assert (even if it *did* concern theories, models and the like) is that there *not not* exists a countable model (or rather: a countable^f model^f), a claim which is much weaker than the claim that gives rise to the Skolem paradox. When a constructivist claims that there *not not* exists a certain structure, he is claiming that one can rule out on mathematical grounds the assumption that no such structure exists. But this is less than what is needed to get the paradox off the ground: that countable^f nonstandard model^{f}s fail to be prohibited is not tantamount to *countable* nonstandard *model*s receiving, as they do in classical mathematics, a general licence.

There remains one further possible worry about whether we have been successful in blocking the application of a downward Löwenheim–Skolem theorem to set theory. The worry takes this form: might we not have left open the possibility that one could (constructively) derive *specific* completeness or countable models theorems for formal set theories or more 'ordinary' theories such as arithmetic? We believe

the following results go some way toward warding off such worries. We will limit ourselves to statements of results; complete proofs will appear in McCarty (forthcoming). On the basis of the first two theorems below, one sees that there is no hope of arriving at a countable downward Löwenheim – Skolem Theorem by way of a model existence theorem for simple extensions of arithmetic and set theory.

THEOREM 5. *If IZF is consistent, then one cannot prove, in IZF + A, the statement*

> *If T is consistent, then there not not exists a model of T where T ranges over extensions (even finite or r.e.) of Heyting (intuitionistic) arithmetic.*

A can be Church's Thesis, Markov's Principle or the Uniform Reflection Principle.

THEOREM 6. *Let S be any formal set theory (e.g., a suitable subtheory of IZF) which contains arithmetic separation and which is at least as strong as Heyting arithmetic. Then, if IZF is consistent, one cannot prove in IZF + A the statement*

> *for all sentences B, if S + B is consistent then it is not not the case that there is a model of S + B.*

A should be such that IZF + A proves that A is true under realizability and such that ZF + A does not prove that ZF is inconsistent.

Consequently, one cannot provide various weakened forms of model existence theorems for theories representing a reasonable amount of constructive mathematics.

Next, one can prove outright, using Church's Thesis in IZF, that strong set theories have absolutely no models which are either very small in cardinality or have "well structured sets" as their carriers.

THEOREM 7. *In IZF plus Church's Thesis, there is a proof that, if a set theory T has arithmetic separation and is at least as strong as Heyting arithmetic, then T has no models of the same cardinality as some subset of the natural numbers. In fact, T will have no models which support stable equality.*

The equality relation on a set will be stable when it is invariant with respect to double negation. Theorem 7 implies that set theory will have no true interpretations of the same cardinality as a metric space; this will include the reals, Baire space and Cantor space.

Finally it is consistent with IZF to assume that there are theories in the language of first-order arithmetic for which none of the standard problems of "ontological relativity" can arise. It will follow from Church's Thesis plus Markov's Principle that Heyting arithmetic determines its models up to isomorphism.

THEOREM 8. *In IZF, Church's Thesis plus Markov's Principle proves that Heyting arithmetic is categorical.*

Obviously, these results, even taken together with those given earlier, do not absolutely rule out the possibility that a version of the Skolem Paradox might be applicable to some strong constructive theory. Nothing that one could do in the way of independence results would show definitively that all statements which could conceivably be thought 'versions' of the Löwenheim – Skolem Theorem are independent of all theories which could conceivably be thought 'constructive'. Besides, it would be foolish to attempt to draw a formal circle around just those mathematical claims which might pose metaphysical problems. But this is not to say that nothing has been accomplished; the theorems of the paper do suffice to show that, if 'Skolemism' can arise for some extension of constructive set theory, then it must be a relatively 'local' phenomenon. As we have seen, the countable models theorem is not, at least in constructive mathematics, an ineliminable feature of the study of any countable consistent first-order theory. If the theorem is constructively available, it will only be so in virtue of the fine details of the theory under consideration and of the assumptions in the attendant metamathematics.

What we have shown is that even very weak forms of the Löwenheim – Skolem theorem are independent of the strongest intuitionistic set theories commonly considered. It follows that none of the theorems of Bishop-style constructivism, none of the work of the members of the Markov – Sanin 'School' and none of the axioms of standard Brouwerian constructivism will prove the

Löwenheim–Skolem theorem in anything approaching its ordinary general form. Consequently, the possibility of a Skolem-style paradox is definitely not the concommitant of any attempt to formalize a sufficiently large part of constructive mathematics. Or, if some refined form of the paradox were to be resurrected, it could only be on the basis of axioms (such as those of the creative subject) which lie outside the 'core' area of constructive mathematics or by employing metamathematical methods which are truly novel. If there is some way of infecting constructive mathematics with 'Skolemism', we have yet to see what it is and from whence it could come.

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NOTES

[1] For a proof-theoretic analysis of this vicious circularity, see Tennant (1982).
[2] We have not raised here the question of characterising the uncountable directly by means of the quantifier 'There exist at least uncountably many x such that . . . '. As Timothy Smiley has observed, Keisler's completeness proof for a (classical) logic based on a simple set of axioms and rules for this quantifier gives an intriguingly quick answer to the question whether there is *any* way at all of characterizing the uncountable. (Vaught had earlier established that the logical truths in this language were recursively enumerable; the interest of Keisler's result is that he shows four simple schemata using the new quantifier to be sufficient for its axiomatisation.) This victory is made to look somewhat Pyrrhic, however, by the impossibility of recursively axiomiatising the logic of 'there exist at least infinitely many x such that . . . '. (This impossibility follows from Vaught's test: see Bell and Slomson, 1971, p. 266). Thus, direct expression by means of quantifiers has a freakish pattern of success and failure. The interest in how *set theory* fares in characterizing both the infinite and the uncountable derives, we believe, from the thought that both these notions either reduce to, or can *some*how be conveyed by, the use of some more basic notion, such as that of set. And there, says the Skolemite, lies the rub.
[3] The interested reader should compare this with Quine's 'virtual set theory'.
[4] Mostowski (1949).
[5] We are indebted to Kit Fine for this point.

[6] Those familiar with constructive mathematics will see that our proof of theorem 4 actually supports a much stronger (and more interesting) conclusion. We have shown that C_4, even restricted to models of the pure theory of identity having at most two elements, implies Kripke's Scheme. The latter is commonly taken to axiomatise Brouwer's theory of the creative subject.

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