F. A. MULLER

DEFLATING SKOLEM

ABSTRACT. Remarkably, despite the tremendous success of axiomatic set-theory in mathematics, logic and meta-mathematics, e.g., model-theory, two *philosophical* worries about axiomatic set-theory as the adequate catch of the set-concept keep haunting it. Having dealt with one worry in a previous paper in this journal, we now fulfil a promise made there, namely to deal with the second worry. The second worry is the Skolem Paradox and its ensuing 'Skolemite skepticism'. We present a comparatively novel and simple analysis of the argument of the Skolemite skeptic, which will reveal a general assumption concerning the meaning of the set-concept (we call it 'Connexion M'). We argue that the Skolemite skeptic's argument is a *petitio principii* and that consequently we find ourselves in a dialectical situation of stalemate.

Few (if any) working set-theoreticians feel a tension – let alone see a paradox – between, on the one hand, what the Löwenheim–Skolem theorems and related results seem to be telling us about the set-concept, and, on the other hand, their uncompromising and successful use of the set-concept and their continuing enthusiasm about it, in other words: their lack of skepticism about the set-concept. Further, most (if not all) working set-theoreticians have a relaxed attitude towards the ubiquitous undecidability phenomenon in set-theory, rather than a worrying one. We argue these are genuine *philosophical problems about the practice of set-theory*. We propound solutions, which crucially involve a renunciation of Connexion M. This breaks the dialectical situation of stalemate *against* the Skolemite skeptic.

1. SKOLEMITE SKEPTICISM

Famously, the Norwegian logician and mathematician Thoralf Skolem (1922, 237) became skeptical about axiomatic set-theory as a foundation for mathematics:

> (...) secondly, I believed that it was so clear that axiomatisation in terms of sets was not a satisfactory ultimate foundation of mathematics that the majority of mathematicians would not be much concerned with it. But in recent times I have seen, to my own surprise, that very many mathematicians think that these axioms of set-theory provide the ideal foundation for mathematics; therefore it seems to me that the time has come to publish a critique.

The reason that set-theory flounders as a foundation for mathematics is that the axiomatised theory makes its fundamental concepts, e.g., denumerability, subset and cardinality, 'relative'. In Skolem (1929, 42) it is said of the

Synthese (2005) 143: 223–253

set-concept that “eine *vollständige* Charakteristik is wohl nicht möglich”; in Skolem (1922, 224) one reads about denumerable set B in a denumerable model “dass die Dinge in B ein andere und weit *eingeschränktere Bedeutung* Haben”; in Skolem (1958, 13) one reads “le *caractére vague* de la notion d’ensemble”; and in Skolem (1962, 218) one reads that “there is no reason to assume the notion of ‘subset’ to have an *absolute meaning*”, as in Skolem (1941, 468) that “les *sens* de ces concepts n’est pas *absolu*”. So the set-concept cannot be ‘completely characterised’, has a ‘shrunken meaning’ in a denumerable model, is ‘vague’, is ‘relative’, has no ‘absolute meaning’ and no ‘absolute sense’. Skolem is here using words from colloquial language to express the same meta-mathematical phenomenon that his (and other’s) meta-theorems have discovered. For a philosopher, however, these expressions are different and have different implications. One of our tasks will be to seek out what exactly these *meta-mathematical* discoveries do and do not imply *philosophically*. But let us first state the crucial meta-theorems, if only for the sake of future reference.

We denote the standard 1st-order formal language of set-theory by $\mathfrak{L}_{\in}$.[1] ZFC is standard, Zermelo–Fraenkel set-theory, ZF is ZFC but without the axiom of Choice (C), and Z is ZF without the axiom of Replacement (F) but with the axiom of pair-sets and the axiom schema of separation; see Fraenkel et al. (1973, 22). We position ourselves in a suitable meta-theory, such as ZFC^{+}, which is ZFC plus the existence of the first non-denumerable strongly inaccessible cardinal number, denoted by i. Let $\mathbf{V}$ be the cumulative hierarchy of all pure sets of ordinal rank smaller than i. This yields the *standard model* of ZFC and its deductive children (ZF, ZC, Z):

(1) $\langle \mathbf{V}, \in \rangle \vDash \mathrm{ZFC}$.

The phrase ‘Löwenheim–Skolem Theorem’ refers to a family of closely connected metamathematical theorems. The following version suffices for our purposes.

(2) LÖWENHEIM–SKOLEM META-THEOREM. Every consistent set of 1st-order sentences, hence every consistent 1st-order theory, has a numerical model; and it has a denumerable model as well as models of every larger cardinality such that the smaller model always is a submodel of every larger one.[2]

If ZFC is consistent, then according to meta-theorem (2) there is some relation E between the natural numbers ($E \subset \mathbb{N} \times \mathbb{N}$) hat behaves like the membership-relation so as to have a numerical model:

(3) $\langle \mathbb{N}, E \rangle \models$ ZFC.

From meta-theorems (1) and (3) it follows that ZFC is not a categorical theory, a conclusion which generalises to every consistent set of 1st-order sentences. This was first observed by John von Neumann (1925):

(4) VON NEUMANN'S COROLLARY. No 1st-order set-theory that has a model can be categorical, which is to say that not all models that make a consistent 1st-order set-theory true are isomorphic.

So ZFC and all its deductive children are not categorical. In fact, as we know after the Gödel–Cohen purifications, models of ZF are not just non-isomorphic; they can differ tremendously.

Finally, let Th be any set-theory formulated in $\mathfrak{L}_\in$. Call $\varphi(\cdot, Y)$, which is an arbitrary *set-theoretical predicate* (an open sentence of $\mathfrak{L}_\in$, possibly with set-parameters abbreviated by Y) *referentially indeterminate in* Th iff there is a set Z (i) that exists and meets $\varphi(\cdot, Y)$ according to Th, i.e., Th $\vdash \varphi(Z, Y)$; and Th has two models $\langle M_1, E_1 \rangle$ and $\langle M_2, E_2 \rangle$ such that (ii) the first model is a submodel of the second one ($M_1 \subseteq M_2$ and $E_1 \subseteq E_2$), and (iii) set-term 'Z' has a common referent in both models, $A \in M_1 \cap M_2 = M_1$ say, such that $\langle M_1, E_1 \rangle$ makes $\varphi(A, Y)$ *true* and $\langle M_2, E_2 \rangle$ makes $\varphi(A, Y)$ *false* (McIntosh 1979, 322). We point out that part (i) of this definition is a non-triviality requirement; part (ii) ensures that in so far as the models are comparable (on $M_1 \cap M_2$), they referentially-interpret $\mathfrak{L}_\in$ identically; and part (iii) makes the models assert, precisely where they are comparable, conflicting things about the same referent-set (A) of the same set-term ('Z'). Let us also call set-theory Th *referentially indeterminate* iff $\mathfrak{L}_\in$ has some predicate that is referentially indeterminate in Th. McIntosh (1979) deduced from meta-theorem (2) the following.

(5) MCINTOSH'S COROLLARY. ZF has two models, one being a submodel of the other, such that both models assign the same set, A say, to the set-term '$\wp\mathbb{N}$' (the power-set of $\mathbb{N}$), and such that one model makes true some assertion about A, namely that it is denumerable, whereas the other model makes the same assertion about A false; hence ZF is referentially indeterminate.

Of course every 1st-order set-theory is referentially indeterminate due to Von Neumann's Corollary (4), not only ZF.

These meta-mathematical results give rise to: (a) the Skolem Paradox and (b) Skolemite Skepticism. We next briefly explicate what this means, following McIntosh (1979) and Benacerraf (1985), but simplifying things considerably and going directly to the heart of the matter by bringing to the surface a premise of a general character which so far has never been formulated explicitly ('Connexion M'), but is needed to have a valid philosophical argument. Then we are in a position to announce what the present paper attempts to contribute to the extant literature on the Skolemite subject-matter.[3]

(a) *The Skolem Paradox.* Consider, with Skolem, *Cantor's Theorem*: there exist non-denumerable sets, such as the set of real numbers ($\mathbb{R}$) and the power-set of the set of natural numbers ($\wp\mathbb{N}$):

(6) $Z \vdash \neg\text{Denum}(\mathbb{R}) \wedge \neg\text{Denum}(\wp\mathbb{N})$,

wherein predicate $\text{Denum}(X)$ stands for 'X is *denumerable*', i.e., 'X is equinumerous to $\mathbb{N}$', i.e., there is a bijection, or one-one correspondence, from set X to $\mathbb{N}$ (everyone thus unhesitatingly accepts $\mathbb{N}$ as the standard of denumerability). All models of a theory make all its theorems true, so for numerical model (3) and Cantor's Theorem (6) we have:

(7) $\langle\mathbb{N}, E\rangle \vDash \neg\text{Denum}(\mathbb{R}) \wedge \neg\text{Denum}(\wp\mathbb{N})$.

Skolem (1922, 223) wondered: "How can it be that the entire range $\mathbb{R}$ can be enumerated by the positive integers?" Indeed, how on Earth can the demonstrably *non-denumerable* sets called '$\wp\mathbb{N}$' and '$\mathbb{R}$' have referents that can have at most a *denumerable* number of members? How can the sets $\mathbb{R}$ and $\wp\mathbb{N}$ – let alone *larger* sets such as $\wp\wp\mathbb{R}$, $\wp\wp\wp\mathbb{N}$, $\wp\aleph_w$, etc. – survive in model $\langle\mathbb{N}, E\rangle$ (3) that seems far too sparsely populated in order to supply set $\wp\mathbb{N}$, $\mathbb{R}$ and the larger sets with the members they need? Whereas the standard model $\langle\mathbf{V}, \in\rangle$ (1) fills all sets with exactly the number of members they must have according to ZFC, the numerical model $\langle\mathbb{N}, E\rangle$ lacks the resources to do so yet makes true the existence of sets that are *larger* than $\mathbb{N}$ (7). So the *size* of sets seems up for grabs: we *say* there are sets which have non-denumerably many members ($\wp\mathbb{N}$ and $\mathbb{R}$), but these sets (or better: the set-terms '$\wp\mathbb{N}$' and '$\mathbb{R}$' in $\mathfrak{L}_\in$) can refer to sets which have no more members than there are natural numbers. This situation is known as the *Skolem Paradox*.

It is important to realise there is no 'Skolem Contradiction' here. The denumerable domain $\mathbb{N}$ of model $\langle\mathbb{N}, E\rangle$ (3) does have 'too few' members to fill the sets $\wp\mathbb{N}$ and $\mathbb{R}$ with the non-denumerably many members that these sets seem to require; nevertheless it makes true Cantor's Theorem that $\wp\mathbb{N}$ and $\mathbb{R}$ are non-denumerable sets (6), because $\langle \mathbb{N}, E \rangle$ is so sparsely populated that it does not contain a single bijection from the referent of '$\mathbb{N}$' in $\mathbb{N}$ to the referents of '$\wp\mathbb{N}$' and '$\mathbb{R}$' in $\mathbb{N}$ either. The non-existence of this bijection is exactly what makes Cantor's Theorem true. In the standard model $\langle \mathbf{V}, \in \rangle$ we have recourse to literally *all* functions from $\mathbb{N}$ to $\wp\mathbb{N}$ and from $\mathbb{N}$ to $\mathbb{R}$, but among them there is not a single *bijection*, simply because $\mathbb{N}$ has too few members when compared to $\wp\mathbb{N}$ and to $\mathbb{R}$. So in these two different models we have a different explanation of the same truth.

(b) *Skolemite Skepticism.* We consider two related kinds of 'Skolemite skepticism'. First we define a *Founding Theory of Mathematics* (**F**) to be a theory such that (F1) all concepts of mathematics can be translated in the (formalised) language of **F**, and (F2) all thus translated theorems of mathematics, accepted by the mathematical community as proved, are theorems in **F**. We say that **F** founds mathematics *adequately* iff **F** founds mathematics and captures the meanings of the primitive concepts of **F** *adequately* (read on for the meaning of 'adequate'). The two skeptical theses read as follows.

(8) FOUNDATIONAL SKEPTICISM. No 1st-order set-theory can found mathematics adequately.

This first thesis concerns the foundations of mathematics, in particular the foundational status of set-theory. The second thesis concerns 1st-order set-theory 'itself', independent from its putative status as an occupant of the foundational throne:

(9) SET-SKEPTICISM. No consistent 1st-order set-theory captures the meaning of the set-concept adequately; every 1st-order theory fails in capturing the meaning of the set-concept adequately.

Skolem subscribed to both skeptical theses (8) and (9) – as is evident from the quotations in the opening paragraph of this paper. Zermelo and Von Neumann followed suit.[4] Set-Skepticism (9) is *prima facie* the general message that the Löwenheim–Skolem theorems seem to be conveying. How exactly this comes about, we are going to spell out right now.

Set-theory ZC (Zermelo's original theory of 1908 extended with Von Neumann's axiom of regularity) still suffices *to found* about 99% of current mathematics; ZFC hauls in some of the missing 1% (mostly transfinite cardinal arithmetic); and ZFC^+ suffices to haul in category theory too, in particular the theory of large categories, which presents the only real threat to set-theoretical foundations ever since set-theory came to occupy the foundational throne.[5] So the following thesis is an established truth:

(10) SET-FOUNDATION THESIS. There is some 1st-order set-theory that founds mathematics, for example ZFC^{+}.

Yet these 1st-order set-theories are supposed to fail as *adequate* founding theories of mathematics. What is sufficient and necessary to reach *this* conclusion on the basis of Löwenheim-Skolem (2) is the following conditional (M of Meaning):

(11) CONNEXION M. If a theory captures the meaning of its primitive concepts adequately, then it makes all meaning-constitutive predicates of the set-concept referentially determinate.

So, to arrive in logically valid fashion at the two skeptical theses (9) and (8), the skeptic does not need to delve into the meaning of 'adequate' (as one may have feared), but only needs to accept referential determinacy as a necessary condition of it – substitute your own favourite adjective in stead of 'adequate' to get your own version of set-skepticism going: precise, complete, exact, absolute, sharp, indeterminate, etc.

We now need to say what the 'meaning-constitutive' predicates of the set-concept are (11). In Section 2 we shall address this subject more elaborately; for the moment it suffices to say that denumerability and finiteness count as such predicates. The logic of the skeptical arrangement is, then, crystal clear: we *prove* there are referentially indeterminate meaning-constitutive predicates in every 1st-order set-theory (5). Then, by Connexion M (11), set-theory does not capture the meaning of the set-concept adequately. This is Set-Skepticism (9). Now, according to the Set-Foundation Thesis (10), some 1st-order set-theory can found mathematics, but does it *inadequately* (9). This is Foundational Skepticism (8). To summarise:

(12) LöwSk Thm. (2) $\wedge$ Connexion M (11) $\rightarrow$ Set-Skepticism (9),
Set-Skept. (9) $\wedge$ Set-Found. (10) $\rightarrow$ Found. Skept. (8).

Notice that if one does not insist on a founding theory that it should perform its duties as a meaning-catcher *adequately*, then the inference from Set-Skepticism (9) to Foundational Skepticism (8) fails.

The point is that both the foundational skeptic and the set-skeptic need as an *assumption* Connexion M (11), which so far has drawn little attention in the literature on the Skolem Paradox (if mentioned at all: see footnote

3). Connexion M provides the bridge to go from *meta-mathematical* discourse, where only proof and disproof rule, to *philosophical* discourse, where (among other things) the meaning of concepts is analysed. Bringing Connexion M to the fore will enable us to attain a more general perspective on the Skolem trouble, namely a perspective in the philosophy of language. But most of all, this opens a novel route for escaping from the skeptic's conclusions as well as for making sense of the practice of set-theory.

Let us finally emphasise that our analysis is not (supposed to) prejudge on the realism-issue in mathematics. Both Realists and Nominalists can endorse the present analyses as well as the conclusions we shall arrive at in the present paper. In this paper we want to reach six aims (A–F).

Aim A. To introduce Aim A, we quote Putnam (1980, 481–482) from his first paper on his celebrated model-argument against 'metaphysical realism', where he pointed in a Wittgensteinian direction for dissolving Skolemite skepticism:

> 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*?
> (...)
> To adopt a theory of meaning according to which a language whose whole use is specified still lacks something, *viz.* its interpretation, is to accept a problem which can only have a crazy solution. To speak as if *this* were my problem, 'I know how to use my language, but how shall I single out an interpretation of it?' is to speak nonsense. Either the use already fixes the 'interpretation' or nothing can.

Observe the tacit acceptance of Putnam of Connexion M (11) to arrive at Set-Skepticism from Skolem's meta-theorems! (Observe also that Putnam's "does not capture the intuitive notion of a set" is our "does not capture the meaning of the set-concept adequately".) Putnam is here pointing in the right direction but leaves it at that. It is something of a scandal that everyone has left it at that. Surely philosophers of mathematics cannot leave matters at the stage of finger-pointing, can they? By means of Horwich's Wittgensteinian theory of meaning called *semantic deflationism* and an improved conception of 'implicit definition' we shall try provide a viable destination consistent with the direction of Putnam's finger-pointing. This is Aim A and we attempt to reach it in Section 2.

Aim B. We argue that the skeptic begs the question by surreptitiously assuming Connexion M; we analyse a few arguments in favour of Connexion M and find them no good; this turns the alleged skeptic's victory over the anti-skeptic into a dialectical position of stalemate (Section 3). So Aim B is to demonstrate that the skeptic's argument is unconvincing because it is a *petitio principii*. Then we argue *against* ConnexionM from the current practice of set-theory (Section 4).

Aim C. It is a fact that few (if any) working set-theorists feel a tension between, on the one hand, whatever the Löwenheim–Skolem theorem (2) seems to be telling us about the set-concept, and, on the other hand, their uncompromising and successful use of the set-concept and their continuing enthusiasm about this concept. Is this tension real and is the set-theoretician simply philosophically insensitive, or is this tension unreal and is the fact that no set-theoretician feels this tension an indication that we have here a typical example of a *Scheinproblem* on our hands? We claim the mentioned fact is a genuine *philosophical problem about the practice of set-theory to be solved by a philosopher*, rather than a mathematical problem to be solved by a set-theoretician. Aim C is to solve it; our solution will crucially involve a rejection of Connexion M (Section 5).

Aim D. A fact about the practice of set-theory similar to the one just mentioned is that quite a few set-theoreticians regard the meta-mathematical fact that there is a legion of undecidable statements, of which the continuum hypothesis is the most celebrated example, not as a defect of set-theory. They have developed a remarkably relaxed attitude towards the ubiquitous undecidability phenomenon. This, too, is a genuine *philosophical problem about the practice of set-theory*. Aim D is to solve it (Section 6).

Aims E and F. Finally we aim to explain why we are so easily seduced into taking Connexion M for granted; this is Aim E. (Section 7). As soon as we realise the grounds for taking Connexion M for granted, it begins to dissolve, or so we claim. Collecting the arguments against Connexion M from Sections 4, 5 and 7 we shall have reached out final Aim F, which is to show that Connexion M is not a reasonable premise for a philosophical argument. This will then break the dialectical situation of stalemate reached by Aim C and hopefully turns it into a defeat of Connexion M.

In a sense to be explained at the end of the next Section, we shall have reached, when our aims are fulfilled, a viable destination which, at best, has been pointed at from a distance by Putnam (1980) and by Wright (1985).

As far as our limited vision permits us to see, the two mentioned problems concerning the practice of set-theory have been ignored by philosophers of mathematics. This is something of a scandal too, we submit. Philosophers of mathematics should mobilise their analytic apparatus to hunt down philosophical problems concerning the practice of mathematics and then try to solve them – and not *only* be occupied by the tantalising question whether abstract objects *really* exist. We claim to have discovered two of such problems and we claim to have viable solutions of them. Perhaps we are wrong. But at least we do not go gently into that dark night. We shall prevent the dying of the light as long as we can.

Enough rhetorics! Let's go to work now. Before we start, we must mention that in this paper we presuppose the superiority of 1st-order over 2nd-order classical predicate logic as the provider of a sound, complete, topic-neutral, recursively enumerable deductive apparatus and of a concomitantly *viable* notion of proof – by which we mean that every proof of a theorem accepted by the mathematical community can be faithfully construed as a valid proof in 1st-order predicate logic (Hilbert–Ackermann thesis). So those who agree with us on this presupposition (for which we cannot argue *hic et nunc*) can accept our conclusions as soon as they also accept the theory of meaning we employ (semantic deflationism); and those who favour 2nd-order logic can no longer claim the *necessity* of adopting 2nd-order logic in order to avoid Skolemite skepticism. Thus for friends and foes of 1st-order logic there should be something to learn from the present paper.

2. THE MEANING OF THE SET-CONCEPT

Wittgenstein (1953, §43; 1979, 48) famously held the use of an expression to be the clue to its *meaning*. To know the meaning of an expression is to understand it; and to understand an expression is the capability to *use* it *correctly*, i.e., in agreement with the semantic rules that can be considered to govern the use in a particular *linguistic practice*, or *context*, or in Wittgensteinian slang: to govern the *language-game* we happen to play. These semantic rules fix the *semantic grammar* of words and expressions, just as syntactic rules fix the syntactic grammar of sentences in a language, their 'grammatical form'. Unlike syntactical rules, however, which are *transcontextual*, i.e., they generally hold in (almost) every context, semantic rules for a given expression can be contextual, i.e., for the same expression they can vary from one context to another. So, succinctly, an expression can acquire meaning iff it is possible to have some context and a language in which the expression occurs and can be used successfully by a community of language-users. In principle, then, every expression can acquire a meaning, but of course not every expression *has* (acquired) a meaning.

Although Wittgenstein avoided building a philosophical theory of meaning like the plague, very few doubt that Wittgenstein had some coherent conception of meaning in mind, on the basis of which he performed his analyses and from which he launched his criticisms. In his book *Meaning* (1998), Horwich expounds a theory of meaning which he has baptised *semantic deflationism*; it is intended to clarify Wittgenstein's conception of meaning (and truth), and to support and to apply it. We are going to employ semantic deflationism.

Semantic deflationism counts five postulates (some terminology and simplifications are ours).

I. *Concept Postulate*. Meanings are concepts and concepts are abstract objects.
II. *Use Postulate.* For every expression there is a 'small' submanual of its use manual that constitutes its meaning; this submanual we call the 'meaning-manual' of the expression.
III. *Synonymy Postulate.* Synonyms have the same meaning-manuals.
IV. *Truth Postulate (Convention T).* For any proposition P, 'P' is true iff P.
V. *Reference Postulate.* The propositional content of term τ_b (usually: 'b') designates object c iff b is identical to c. Term τ_b refers to object c iff there is some term, σ_a say, such that: σ_a and σ_b have the same propositional content, and σ_a designates c.

Since postulates III, IV and V will play no part in this paper, we can afford to ignore them (we merely have included them here for the sake of completeness). Next follow a few words to explain postulates I and II.

The assertion that meanings are abstract objects is merely to distinguish them from concrete objects – no transcendent realm of meanings is or needs to be posited. The *use* of some expression in a language, ξ say, is the sum-total of all contexts in which some expression is spoken or written that contains ξ (over some period of historical time). We can label the contexts, thus denoted by $\mathcal{C}_j$ ($j \in \mathbb{N}$), and the concomitant expressions so as to make ordered pairs of type $\langle \mathcal{C}_j, L_j[\xi] \rangle$, were $L_j[\xi]$ is a list of expressions $\sigma_0[\xi], \ldots, \sigma_k[\xi]$ that each contain expression ξ ($k \in \mathbb{N}$). So the idea is the following ($l = 0, 1, 2, \ldots, k_j$): in context $\mathcal{C}_j$, the usage of any expression $\sigma_l[\xi] \in L_j[\xi]$ is appropriate. We collect all the ordered pairs in $U(\xi)$, which we call the use-manual of expression ξ. List $L_j[\xi]$ will generically contain many expressions, because quite a number of expressions containing ξ will be appropriate in the given context $\mathcal{C}_j$. The same expression can be used on different occasions, e.g., 'This is good', 'That is big', so that it will occur in many lists of many contexts. The meaning of the word 'small' will depend heavily on the context (it means something different in subnuclear physics than in architecture); so 'small' has a contextual meaning. In contrast, the meaning of a word like 'and' and an expression like 'is larger than' will be the same in all contexts; they have a transcontextual meaning. For Horwich, the meaning of every linguistic expression, ξ say, is a *property of it* in context $\mathcal{C}_j$, which we abbreviate by $M(\xi, \mathcal{C}_j)$; this property is a concept, y say, which in turn is an abstract object, so that $M(x, \mathcal{C}_j) = y$.[6] Horwich (1998, *passim*) writes things like: M(cat) = cat, which is supposed to mean that the meaning of the word 'cat' is the abstract object cat (reassuring remark: 'cat' also refers to cats, the familiar hairy creatures we can interact with physically – but not mentally).

The notion of 'to constitute' in the Use Postulate is supposed to be the familiar one, as in: 'consisting of H_2SO_4-molecules' *constitutes* 'being vitriol', and 'air molecules vibrating longitudinally with frequencies between 20 and 20,000 Hz' *constitutes* 'being sound'. Hence 'to constitute' seems a relation between properties, which we describe by predicates. Let F and G be two predicates, which need not have the same logical form. Then Horwich (1998, 25) defines: F *constitutes* G iff (c1) F and G apply to the same things; and (c2) F and (c1) explain facts about G. The predicates 'being vitriol' (G) and 'consisting of H_2SO_4-molecules' (F) apply to concrete objects (c1); and facts about H_2SO_4-molecules and about what happens when you take lots of them together explain the facts of vitriol, e.g., its liquid state at room temperature, transparency, viscosity, adhesion, smell and capability to solve many chemical substances and others not (c2).

Call a *submanual* of $U(\xi)$ a collection of contexts $\mathcal{C}_j$ occurring in $U(\xi)$, each one paired with a sublist from the list, $L_j[\xi]$, occurring in $U(\xi)$. At last we can understand what is being asserted in the Use Postulate: every expression has a 'small' submanual that *constitutes* its meaning. This *meaning-manual*, denoted by $M(\xi)$, then, is supposed to explain the overall use of the expression (this follows from how 'constitutes' was defined above); it should be taken as Wittgenstein's 'semantic grammar'. The smaller it is, the more bite will the theory have and the more the theory will take a stand against the utterly implausible 'meaning holism', which takes *every* newly uttered expression that includes a given word to contribute to the meaning of that word. (See Horwich (1998, 45) for examples of words whose meaning-manual counts a few items only.) So much for semantic deflationism generally. We now turn to the idea of an implicit definition.

A *definition* of some expression in a language is *semantic*; it is another expression stated explicitly in the same language. The *meaning* of the *definiendum* is by convention asserted to be the same as the meaning of the *definiens*; a synonymous expression has been given. This is the standard notion of a definition, as in: a set is *denumerable* iff it is equinumerous with $\mathbb{N}$; etc. Unlike in the context of mathematics, in other contexts most expressions do not have explicit definitions. In mathematics too various concepts do not have definition, e.g., ‘set’ and ‘is a member of’; for them something between having and not having an explicit definition will be achieved.

In the penultimate letter of the exchange between Hilbert and Frege on definitions and axioms, dated 22 September 1900, Hilbert summarised his view on the status of axioms as follows (Frege (1980, 51)):

> In my opinion, a concept [primitive notion, FAM] can be fixed logically only by its relations to other concepts. These relations, formulated in certain statements, I call *axioms*, thus arriving at the view that axioms (perhaps together with propositions assigning names to concepts) are the *definitions* of the concepts.

Thus Hilbert, and likewise Poincaré, regarded axiomatisations as achieving something for the primitive concepts of the language between an explicit definition and having no definition, namely an *implicit definition*. In his axiomatisation of Cantorian set-theory, Von Neumann (1925, 36) explicitly adhered to the same view.

Now, when we restrict ourselves to mathematics, Wittgenstein’s ‘social’ conception of meaning harmonises with Hilbert’s ‘rational’ conception of implicit definability, because accepted axioms of some branch of mathematics, together with the logical deduction-rules, govern the rigourous *use* of the primitive notions as they are actually *used* by the community of mathematicians in this branch. Hilbert’s implicit definition and Wittgenstein’s semantic grammar seem pretty much the very same thing.

Let $\mathrm{Th}[\tau, P]$ be an axiomatised theory in a language $\mathfrak{L}(\tau, P)$, wherein term τ and predicate P form the primitive vocabulary; and let $\mathrm{AxTh}[\tau, P]$ be the (non-empty) collection of its axioms. (So Hilbert proposed to take $\mathrm{AxTh}[\tau, P]$ as an *implicit definition* of the term τ and the predicate P.) Suppose we have some mathematical practice that has recently established itself as a legitimate branch at the tree of mathematical knowledge; and suppose $\mathrm{Th}[\tau, P]$ has been accepted as the right theory. We make a List of sentences from the mathematical practice in which τ and P occur, called $L[\tau, P]$, that we intuitively consider to be constitutive for the meaning of τ and P. We call a predicate of $\mathfrak{L}(\tau, P)$ *meaning-constitutive* iff it occurs in the list $L[\tau, P]$. Somewhat in analogy with calling a scientific *theory empirically adequate* iff it saves the relevant perceptual phenomena, we call $\mathrm{Th}[\tau, P]$ *meaning-adequate* iff it saves the *relevant linguistic phenomena* (the meaning-constitutive sentences), that is to say, iff $\mathrm{Th}[\tau, P] \vdash L[\tau, P]$. All items in list $L[\tau, P]$ are facts about the meaning of the set-concept indirectly referred to in the Use Postulate of semantic deflationism via the meaning of ‘to constitute’.

Now we are able to formulate our

(13) IMPLICIT DEFINABILITY CRITERION. The axioms of a theory implicitly define its primitive vocabulary iff (Cr1) it is logically possible that the axioms are true and (Cr2) they save the relevant linguistic phenomena, which is to say that the axioms entail all the meaning-constitutive sentences.

Elsewhere we explained what is wrong with Hilbert's proposal to require only consistency – which is co-extensive with (Cr1) when truth is construed model-theoretically; we formulated four outstanding problems that any account of implicit definability must solve; and we argued that adopting Criterion (13) solves all four problems (see Muller (2004)). We therefore proceed here with Criterion (13).

In order to assert that the axioms of ZFC, denoted by AxZFC[set, $\in$], qualify as an implicit definition of the set-concept and the membership-relation, we must, according to Criterion (13), establish that (Cr1) ZFC is possibly true and that (Cr2) ZFC saves the relevant phenomena of the practice of set-theory, collected in a list L[set, $\in$], which includes all meaning-constitutive sentences up. Part of Muller (2004) was devoted to accomplish precisely this; we therefore will not repeat it here. We also provided there a pragmatic argument in favour of the possible truth of ZFC, in order to meet (Cr1). We want to add another argument here.

This additional argument starts by asserting that the axioms of ZFC are supposed to be *true of sets*, if anything. Whatever the meaning of the word 'set' is, the axioms of ZFC must be true of it. They are, in fact, *analytic truths* about the set-concept: the axioms of ZFC are true of the set-concept because they single out the meaning it must have in order to make them true. Next, ZFC is closed under 1st-order deduction, which is demonstrably sound. So all theorems of ZFC are analytically true. We certainly know at least one sentence in the language of ZFC that is *not* a theorem of ZFC. We now conclude that ZFC is a consistent collection of analytic truths about the set-concept – a conclusion that is actually stronger than we need in order to meet (Cr1). Of course, by meta-mathematical necessity we cannot formalise this argument (Gödel), yet as an informal argument it makes sense. So much for AxZFC[set, $\in$] meeting Criterion (13).

Let next $\mathcal{C}_\in$ stand for the mathematical context of doing Zermelo–Fraenkel set-theory; and let $\mathcal{A}^j_\in$ stand for the j-th context in which the set-concept is applied (there are finitely many of them, say $n \in \mathbb{N}$). We re-define the *meaning-manual* of the set-concept as follows:

(14) $M[\text{set}, \in] \equiv \{\langle \mathcal{C}_\in, \text{AxZFC[set,}\in]\rangle,$
$\langle \mathcal{A}^j_\in, \text{AxZFCU[set,}\in]\rangle \mid j = 1, 2, \ldots, n\},$

where ZFCU stands for ZFC plus 'primordial elements' (German: *Urelementen*), which is a conservative extension of ZFC.[7]

The Use Postulate now permits us to assert that the set-concept has a meaning which is constituted by meaning-manual $M[\text{set}, \in]$ (14), which, in turn, *implicitly defines* the set-concept *because* it meets Criterion (13). This implies that on the basis of $M[\text{set}, \in]$ we must be able to explain all relevant facts concerning the meaning of the set-concept, besides $L[\text{set}, \in]$ of course (Muller 2004). We end this Section by sketching how the explanations work for two other facts.

First Fact. In the practice of set-theory, results based on a proof in ZFC have by far the highest acceptance-value (Shelah 2002a); the lowest value have consistency proofs; proofs from large cardinals or proofs in Gödel's universe have intermediate value. This we take to be a fact about the practice of mathematics. When we assume that only the axioms of ZFC (and hence their deductive consequences) are the 'untouchable truths' of the set-concept (Shelah (2002a): ZFC exhausts our intuition), so that axioms leading to consistent extensions of ZFC are at best possible truths, then the very fact that ZFC exhausts the analytic truths about the set-concept explains the big difference in acceptance-value, because analytic truths generally have the highest acceptance-value.

Second Fact. When we teach pupils the set-concept, we mention a flock of sheep, a herd of cattle, a school of fish, a class of pupils, the population of a country, a swarm of bees, etc. When we explain what the union-set is, we say that the life-stock of a farmer is the union-set of his sets of cows, chickens and sheep, say. When we explain what the intersection-set is of the Dutch people and the Tunisian people, we say it is the set of people having a passport of each of these countries. When we explain what the choice-set is, we imagine a cupboard with various candy jars and choosing one candy from each of these. And so forth. How to explain the fact that we begin explaining the abstract set-concept and several set-theoretical notions by such concrete examples? More generally, how to explain that set-theory applies to the world?

This is explained by pointing out that in such mundane contexts, the axioms of ZFCU constitute the meaning of the set-concept. All mentioned objects are primordial elements when seen through set-theoretical glasses. With the Replacement Schema also applying to primordial elements, the meaning-manual $M[\text{set}, \in]$ (14) explains the applicability of set-theory to the world, because now we can make sets of any kind of objects. (Although this deserves further elaboration, it is plausible that such an elaboration can be given; for our present purposes this is enough.)

Two more facts concerning the set-concept and the practice of set-theory will be treated in Sections 5 and 6.

So far we have *not* talked in meta-mathematical tongue. But how about the model-theory of ZFC? How about truth and reference? How do they bear on the meaning of the set-concept? How about Connexion M? These are the questions we attempt to answer in the following Sections, armored with our meaning-manual $M[\text{set}, \in]$ (14); it will lead us, at last, to a direct confrontation with the Skolemite skeptic.

3. DIALECTICAL STALEMATE

In order to avoid lethal Cretean contradictions, we must make a choice between either *doing model-theory* and move to the meta-level (Meta), or *not doing model-theory* and remain at the object-level (Obj). *Tertium non datur.* If we choose (Obj), we cannot legitimately talk about the notions of reference and truth. These notions occur neither at the object-level nor in the meaning-manual of the set-concept (14), and therefore they have no bearing on the *meaning* of the set-concept. It is a simple as that. When we talk about 'the domain of discourse of all sets' or assert that set-theory 'is about sets', we must realise that this referential talk is formally illicit because not part of $\mathfrak{L}_\in$. When we speak about a specific set as 'existing (in the domain of discourse)', we do not mean to suggest we somehow enter this domain, suddenly see the relevant set showing its face, hunt it down and catch it with a predicate; rather, we mean by 'the mathematical existence of A' the provability of a sentence of $\mathfrak{L}_\in$ in which X occurs and begins with an existential quantifier $(\exists X : \varphi(X))$ – any set-theoretician transcending *this* by asserting things that cannot be translated into $\mathfrak{L}_\in$ proceeds at his own peril. So, confined to the object-level (Obj), the Skolemite skeptic will starve. The relevant meta-theorems *cannot even be expressed*.

Choice (Meta) elevates us to the meta-level into some (informal or formal) meta-theory, where we have the resources to define a tremendous variety of putative 'referents' of the set-terms in $\mathfrak{L}_\in$ and to define recursively 'truth-in-a-model' (Tarski's satisfaction-relation $\models$). The meta-language and the meta-theory are the Skolemite skeptic's bacchanal. (Of course the meta-theory can still be, and usually is, a set-theory, but the conceptual resources are enlarged, notably the possibility to *mention* items of $\mathfrak{L}_\in$.) Every model provides a context in which certain sentences of $\mathfrak{L}_\in$ are true and others are false. Due the soundness of 1st-order logic, all theorems of ZFC are true in all models; *their* truth is, model-theoretically construed, transcontextual. Sentences which are made true in some model and false in some other never can be meaning-constitutive. Two crucial questions now face us.

(15) **Q1.** When we engage in the model-theory of set-theory, must we then extend the meaning-manual $M[\text{set}, \in]$ of the set-concept?

Q2. If so, how to extend it?

First of all, the Skolemite skeptic *must* answer **Q1** in the affirmative. For if not, then no conclusions can be drawn from whatever happens at the meta-level (Skolem trouble) that will have any bearing on *the meaning* of the set-concept and hence on the meaning-adequacy of ZFC. As long as we do not touch the meaning-manual, the meaning of the set-concept trivially remains unaffected. Now, as soon as the skeptic considers Skolem trouble as a motivation for extending the meaning-manual of the set-concept into the meta-realm so as to be able to argue in favour of the meaning-*inadequacy* of ZFC (9), we can justifiably appeal to the phrase that one philosopher's *modus ponens* is another's *modus tollens*: if enlarging the meaning-manual into the meta-realm leads to skepticism, then this is an excellent reason not to enlarge it. We thus have arrived at a dialectical stalemate. Therefore, if the Skolemite skeptic wants to win, she must produce some convincing reason to answer **Q1** *in the affirmative that makes no appeal to Skolem trouble*. Since no commentator on the Skolem phenomenon has even posed question **Q1** (perhaps because all commentators take it too obvious to deserve mention), there is little else to do for us than try to think of a few reasons the skeptic may advance for an answer in the affirmative of **Q1** (15).

One reason to extend the meaning-manual into the meta-level might be that we must do it in order to know *how to use* the set-concept at the meta-level (Meta), because how things currently stand in the meaning-manual $M[\text{set}, \in]$ (14), we only know how to use 'set' at the object-level (Obj). Hence, on the very basis of semantic deflationism, $M[\text{set}, \in]$ is unacceptable because it cannot explain a single fact about the use of the set-concept at the meta-level (and therefore does not meet the Use Postulate)! So to answer question **Q2**, then, in a single sweep: we must enlarge $M[\text{set}, \in]$ with the definitions of the model-theoretic notions 'referent', 'referential interpretation' and 'truth-in-a-model', because they all involve the use of the set-concept at the meta-level. Not only the linguistic phenomena at the object-level (Obj) but also those at the meta-level (Meta) must be saved in order for $M[\text{set}, \in]$ to pass Criterion (13).

This reason is no good. Indeed, in order to use, at the meta-level (Meta), the notions of referent, truth-in-a-model, etc. legitimately and rigourously, we need to have the Tarskian definitions of them all right. *But these definitions are the meaning-manuals of these newly introduced model-theoretic notions.* No reason has been produced for why they also must be added to the meaning-manual of the set-concept. A reason has been produced for why we must adopt them, not for why we must in *addition* lump them into $M[\text{set}, \in]$.On the contrary, the newly added model-theoretic notions do not seem to make it necessary at all to tinker with $M[\text{set}, \in]$. Why must we strengthen it? Why can we not take the very same concept with us in our haver-sack when we take a walk in the realm of meta-mathematics and leave the meaning of 'set' unaltered? The use we make of the set-concept at the meta-level can be explained by $M[\text{set}, \in]$ as it stands in combination with the meaning-manuals of the novel model-theoretic notions (their definitions at the meta-level are their meaning-manuals); hence $M[\text{set}, \in]$ still constitutes the meaning of the set-concept. All the meta-mathematical phenomena can be saved without changing a word in $M[\text{set}, \in]$. The dialectical situation of stalemate persists.

Another line of attack of the Skolemite skeptic could be to assert that Criterion (13) is too weak, and that as an additional requirement (Cr3) one should add Connexion M (11) to it. (This is an answer to **Q2**.) Then she can conclude that AxZFC[set, $\in$] is not an implicit definition of the set-concept *because* the Skolemite meta-theorems entail that (Cr3) is violated. The putative fact that AxZFC[set, $\in$] does not implicitly define the set-concept is also ground for skepticism, for what else can fix its meaning? If she holds that without an implicit definition passing Criterion (13) the set-concept is up for grabs, Set-Skepticism (9) comes within reach.

But this dialectical move, again, results in stalemate, because the anti-skeptic will say that the Skolemite meta-theorems are an excellent reason not to add Connexion M as a third requirement (Cr3) in Criterion (13). The skeptic must produce a reason why to add Connexion M to Criterion (13) that *makes no appeal to Skolem trouble*. This is, however, the same problem we have been discussing above, as a moment's thought will reveal.

So the dialectical situation of stalemate still persists. In the next two Sections we make an attempt to break this situation in favour of the anti-skeptic.

4. THE ABSTRACTION-SCALE

What was already plausible from the practice of set-theory in the days of Zermelo, Fraenkel, Von Neumann, Skolem, Tarski, Ulam, Sierpinski and Hausdorff, has become obvious from the practice of set-theory today, the days of Shelah, Martin, Lévy,Woodin, Silver, Kanamori, Steel, Friedman, Moschovakis, Jensen, Devlin, Kunen, Drake, Enderton and Jech: the set-concept is somewhere between (α) the numbers (natural and real), say, and (ω) a group, a ring, a topological structure, a linearly ordered set, a functor-category, and many more. Please indulge us to envision a scale of mathematical structures, to be referred to as the *abstraction-scale*, with α and ω being the end-points, on which mathematical structures are linearly ordered by the relation 'is more abstract than'. Of course we do not possess a definition of this relation; but we submit the following remarks to elucidate it so as to accept it as sufficiently meaningful to be used it in philosophical arguments.

First Elucidation: Untouchable Truths. About mathematical structures at end α all of us (academics, say) somehow have come to possess 'untouchable truths'. Any number-theory that proves 1573 + 429 = 2003, that proves there are finitely many prime numbers, or that the relation 'is smaller than' is not a linear ordering, is unacceptable because it violates untouchable truths, full stop. In the case of the natural and of the real numbers (note the use of the definite article), we can easily draw up a list of untouchable truths, each of whose theoremhood is a *conditio sine qua non* for any theory about these concepts to be taken seriously. And yet even in these cases, which concern structures almost as old as mathematics itself, there are also numerous sophisticated statements where the intuition falters, even the highly developed intuition of specialists in the relevant fields. In these cases, *whatever* is proved by the canonical theory will be accepted. At the other end of the abstraction-scale (ω), we find the mirror situation: the intuition falls silent and only the specialist will be able to muster a few general untouchable truths about the structures at that end, e.g., a group or a functor-category, say, that any theory about it must save.

In general, for structures at end ω, a group say, it makes sense to say that sentence φ is a truth about Abelian groups iff it is a theorem about Abelian groups, but not at end α. Think of the Gödel-sentence: it is demonstrably not a theorem about natural numbers but it is also an untouchable truth about natural numbers.

Second Elucidation: Generality. At end α of the abstraction-scale the mathematical structures are somehow most specific, whereas at ω they are most general. By 'general' we mean that the practice of mathematics is populated with many different specimen of these structures. For mathematicians, the natural numbers are the natural numbers and that is the end of it: 0, 1, 2, 3, ...; they make up the 'Peano-structure' $\langle \mathbb{N}, S, 0\rangle$ (S is the successor-relation '+1'), and this specific structure is ubiquitous in mathematics. The other number-structures, $\mathbb{Z}$ (integers), $\mathbb{Q}$ (rationals),

$\mathbb{R}$ (reals), $\mathbb{C}$ (complex numbers) and $\mathbb{Q}$ (quaternions) are also at end α. Numbers seem to form the most specific structures of the whole of mathematics, perhaps together with the Euclidean plane. To the right of α we find a general Peano-structure $\langle N, S, a\rangle$, meeting the Peano-axioms, of which $\langle \mathbb{N}, S, 0\rangle$ is but one specimen – in ZFC one proves that all 'Peano-structures' $\langle N, S, a\rangle$ are isomorphic.[8] About here we also find Von Neumann's structure of a complex infinite-dimensional Hilbert-space $\mathcal{H}$ (also all isomorphic in ZFC); specimen are Hilbert's space of complex square-summable sequences $l^2(\mathbb{N})$, the space of complex square-integrable functions $L^2(\mathbb{R}^n)$ with $n \in \mathbb{N}$ real variables (both of which are very specific and one finds these specimen near end α), the dual $\mathcal{H}^*$ of continuous linear functionals of Hilbert-space $\mathcal{H}$, and the space of so-called Hilbert–Schmidt operators acting on any $\mathcal{H}$. Towards end ω we encounter the very general concept of a vector-space, encompassing all Hilbert-spaces, all Banach-spaces, all Von Neumann-algebras, all $\mathcal{C}^*$-algebras, and what have you. To the right of the concept of a vector-space one finds that of a topological space, which is even more general (all vector-spaces have a norm-topology and therefore are also topological spaces, but there are many more topological spaces which are not vector-spaces). We are now at end ω.

We emphasise that when moving from α to ω along the abstraction-scale, the mathematical structures *do not become imprecise or inexact or vague or deserve any other pejorative adjective*. The key word here is generality. To repeat, in mathematics generality means: having many instantiations, having many applications in various branches of mathematics. In meta-mathematics generality means: encompassing many non-isomorphic models. Generality is not vagueness. Generality is not a vice. Generality is not a basis for skepticism.

Enter the set-concept. What we want to argue next is that the set-concept, when we conceive every set X also as a 'structure' $\langle X, \in\rangle$, is located somewhere in the middle of α and ω (the fact that all structures can be seen as sets notwithstanding). This will motivate a rejection of Connexion M (11).

(a) The ambition of set-theory to mount the founding throne of mathematics began to emerge around the beginning of the XXth-century. In Section 1 we defined a *Founding Theory of Mathematics* (**F**) to be a mathematical theory such that (F1) all concepts of mathematics can be translated in the (formalised) language of **F**, and (F2) all thus translated theorems of mathematics, accepted by the mathematical community as proved, are theorems of **F**. To achieve (F1), **F** must have a quite flexible language, which accommodates translations of all known mathematical concepts; in other words, it must be very 'general', rather than very narrowly suited for a very specific mathematical subject-matter. To achieve (F2), the axioms of **F** must possess an Atlas-like strength, because they must carry the whole wide world of mathematical knowledge and all its abstract inhabitants. Now, *if* the axioms of **F** can serve as a meaning-manual of the primitive concepts of **F**, *and if* they can even be considered as an implicit definition of these concepts (so that Ax**F** is also meaning-adequate and hence founds mathematics *adequately*), *and if* AxF is deductively strong (so as to meet F2), *then* the meanings Ax**F** bestows on its primitive concepts will also be strong and 'specific'. The more axioms, or the 'stronger' the axioms, the more strictly the use of their concepts is regimented, the more 'specific' their meanings are. Hence requirement (F1) together with the requirement of meaning-adequacy, and requirement (F2) seem to pull in opposite directions of the abstraction-scale: (F1) pulls towards end ω and (F2) pulls towards end α. So the primitive concepts of any **F** will be found somewhere in the middle of end-points α and ω. The Set-Foundation Thesis (10), then, suggests that the 'structure' $\langle X, \in\rangle$, the 'primitive structure' of ZFC, actually resides somewhere in that middle.

(b) Further, it is a fact that we all have untouchable truths about the set-concept: we can make subsets of sets; the cardinality of a finite set is some natural number; the cardinality of the union-set of two disjoint sets is not smaller than the sum of the cardinalities of the disjoint sets; self-membered sets are nonsense; if we replace every member of a given set with some other thing, then we obtain another set with these other things as members; equinumerous sets have the same cardinality; $\mathbb{R}$ has a larger cardinality than $\mathbb{N}$; and so forth. Therefore it does not make sense to say that sentence φ is a truth about sets iff it is a theorem. Of course, ZFC performs so well in turning untouchable truths into theorems that we are prepared to accept theoremhood as necessary for truths about sets generally – theoremhood for untouchable truths is a necessary condition for accepting a set-theory. But not as sufficient? Remember also that 'untouchable truth' is not the same as model-theoretic truth (true in all models), although there is a connexion which follows from untouchable truths being theorems: they are therefore also model-theoretic truths. Well, there are many statements about sets, formulated in $\mathfrak{L}_{\in}$, where the intuition falls silent, even that of the experts in the field. Those statements we gladly promote to the status of truth or falsehood as soon as they are proved or disproved, respectively, in ZFC. But the point is that there are many statements about sets which are demonstrably unprovable in ZFC. This all serves to illustrate that the 'structure' $\langle X, \in\rangle$ is somewhere in between ends α and ω on the abstraction-scale.

(c) Anyone who is an intellectual tourist in the current practice of set-theory (like this author), rather than an active researcher in the fields of Descriptive Set-Theory (Moschovakis 1980), of Constructible Sets (Devlin 1984), of Classification Theory (Shelah 1985), of Model-Theory (Hodges 1993) or of the Higher Infinite (Kanamori 2003), will have concluded that the set-concept currently encompasses *much* more than its originator Cantor ever dreamt of. For instance, almost every sophisticated set-theoretical assertion can be shown to be consistent with ZFC plus (the existence of) some large cardinal (kinds that Cantor never dreamt of either), and vice versa. Now that we mention large cardinals: adding 84 inaccessibles to ZFC makes the theory inconsistent if it is consistent with 17 ones, but adding instead 49 can be done consistently, although the resulting theory has no well-founded models. In some model of ZFC, e.g., Göodel's constructible model $\langle \mathbf{L}, \in \rangle$, there are $\aleph_1$ real numbers (as the continuum hypothesis requires), whereas in one of Cohen's forced models there are $\aleph_{137}$ real numbers, which is 136 distinct levels of infinite cardinality higher up. A Report filled with results like these can be drawn up by any intellectual tourist from the massive available literature (see above). The Löwenheim–Skolem theorem is but one item in this Report. What is this Report telling us?

Answer: *this is what the set-concept is like.* The meaning of the set-concept *is* such that asserting every single result in the Report is appropriate. This is what we have found out about the set-concept. The answer is empathically not that we should turn skeptical about (the meaning of) the set-concept, we submit, it is emphatically not that ZFC is inadequate to capture its 'precise meaning'. No matter how set-theoreticians use the set-concept, they never turn *against* ZFC.[9] This is why $M[\text{set}, \in]$ (14) is exactly the right choice for the meaning-manual of the set-concept. The meaning which $M[\text{set}, \in]$ bestows on the set-concept is such that the Report is appropriate.

Although we suspect that in spirit Shelah (2002b, §2) is quite close to what we are defending here when he speaks about "the glory of proven ignorance: to show that we cannot know", we would prefer to speak of: *the glory of proved knowledge of the meaning of the set-concept.* For to understand a concept is to have knowledge of its meaning and it stands beyond reasonable doubt that the understanding of the set-concept has grown spectacularly over the last century. Kanamori (2003, xx–xxi) submits that the Report of results tells us the following (our italics):

From Skolem relativism to Cohen relativism the role of set-theory for mathematics became even more evidently one of an *open-ended framework* rather than an elucidating foundation. From this point of view, the fact that the axioms of ZFC do not determine the cardinality of the set of reals seems an entirely satisfactory state of affairs. With the *richness of possibility* for arbitrary reals and mappings, no axioms that do not directly impose structure from above should constrain a set as *open-ended* as the collection of reals or its various possibilities of well-ordering.[10]

We interpret this as saying that we have learned a lot about of the set-concept, namely that it is such that it gives rise to a wealth of possibilities rather than selecting a single possibility as 'the right' one. *This is what the set-concept is like.* In a recent overview of developments in set-theory, Steel (2000, 428) writes about ZFC: "By placing classical set-theory in this broader context, we have *understood it better*." This is almost an identical wording to how we prefer to put things: we have gained much knowledge about the meaning of the set-concept. *This is what the set-concept is like.*

We conclude from (a), (b) and (c) that the set-concept is not located at or nearby end α of the abstraction-scale; therefore imposing Connexion M (11) on set-theory is unreasonable. The set-concept occupies a position on the abstraction-scale which falls outside the jurisdiction of Connexion M, if it has one at all. Imposing Connexion M is like surreptitiously shoving $\langle X, \in \rangle$ to end α, where it does not belong. But then the Skolemite skeptic's argument (12) has an implausible premise and we therefore are no longer committed to whatever conclusions follow from it, notably Set-Skepticism (9) and Foundational Skepticism (8).

5. THE WORKING SET-THEORETICIAN

The aim of the current Section (Aim C) is to explain the fact that few if any working set-theorists feel a tension between, on the one hand, whatever the Löwenheim–Skolem theorems and related results seem to be telling us about the set-concept, and, on the other hand, their uncompromising and successful use of the set-concept and their continuing enthusiasm about this concept. We claimed earlier this is a genuine *philosophical problem about the practice of set-theory*.

From the previous Sections the solution ought to be obvious: set-theoreticians have rejected Connexion M (11) unconsciously. That they must reject it, follows from our succinct construal of the skeptic's argument, which now turns into a philosophical*reductio ad absurdum* argument (12). For, given the Skolemite meta-theorems, in particular McIntosh Corollary (5), and given the assertion that ZFC does capture the meaning of the set-concept adequately – which is the denial of Set-Skepticism (9) – it follows that Connexion M (11) is false. As soon as Connexion M is rejected, we can straightforwardly explain the absence of the mentioned tension among the working set-theoreticians: without Connexion M, one cannot argue validly from *meta-mathematical* discourse, where proof and disproof rule, to *philosophical* discourse, where analysis, argument and interpretation rule; more specifically, without Connexion M (the bridge between these two realms of discourse), to claim that the Skolemite meta-theorems entail skeptical theses (9) and (8) is to commit a *non sequitur*. Only those who are inclined to accept Connexion M feel the mentioned tension, because as soon as Connexion M is made explicit, the tension is validated by turning it into the logical incompatibility between the Skolemite meta-theorems and the anti-skeptical claim that the meaning of set-theory can be captured adequately. So much for Aim C.

Let us return to the situation of stalemate: given the Löwenheim–Skolem Meta-Theorem (2), the skeptic validly draws skeptical conclusions about set-theory *because* she antecedently accepts Connexion M (11), whereas the anti-skeptic validly rejects Connexion M because it leads to skeptical conclusions. Since renouncing Connexion M explains a brute fact about the practice of set-theory, whereas adumbrating it makes an explanation impossible, stalemate has been broken in favour of the anti-skeptic, we submit. Recall that Aim F was to argue that the skeptical argument (12) is unconvincing because Connexion M (11) is not plausible; we claim to have mounted an argument to this effect.

6. UBIQUITOUS UNDECIDABILITY

Famously, Cantor's Continuum Hypothesis (CH), asserting there are $\aleph_1$ real numbers – or equivalently, the cardinality of $\wp\mathbb{N}$ equals that of $\mathbb{R}$ –, can be consistently added to ZFC (proved by K. Gödel around 1940) as well as its denial (proved by P. J. Cohen in 1964); in other words, CH is demonstrably independent of ZFC. In the decades following these results, numerous sentences in $\mathfrak{L}_\in$ have been demonstrated to be undecidable: Whithead's conjecture of free groups, Suslin's hypothesis, Kurepka's conjecture, 'there is an inaccessible cardinal', etc (Kunen 1980; Hodges 1993, *passim*). Of course, to understand why these meta-theorems hold, one must study their proofs. But is it possible to understand this state of affairs without studying the proofs of all these undecidability-theorems and by only pondering the meaning of the set-concept? We believe the answer is in the affirmative! We think this will, then, also explain the relaxed attitude that most if not all set-theoreticians have developed towards it.

The meaning of the set-concept is constituted by its meaning-manual $M[\text{set}, \in]$ (14), which in the context of pure mathematics reduces to AxZFC[set, $\in$]. What do the axioms of ZFC say? Extensionality tells us that sets are fully fixed by their members and by nothing else: if two sets share their members, then this is sufficient to pronounce them identical (the converse is a theorem of logic, given the definition of = in $\mathfrak{L}_\in$). Regularity prevents the membership-relation from behaving pathologically; further, it simplifies proofs and makes the cumulative hierarchy of sets exhaust the domain of discourse of ZFC. These are the only axioms that tell us something about sets and $\in$ 'itself', about 'structure' $\langle X, \in \rangle$ if you like, and they do not tell us very much it seems. All the other axioms (Union, Power, Replacement, Choice, Pair and Separation) are of the following type: if set X exists, then *another* set related to X exists too. These axioms permit one to make many more sets *if* a set is given; they tell us next to nothing about sets 'themselves'. So we also must have an axiom positing a set unconditionally; this is Infinity, which posits a smallest infinite set. That is all. End of story. No 'structural' features are bestowed upon sets. Nothing is assumed about their *cardinalities* or how these relate to each other; and *mutatis mutandis* for ordinal numbers; nothing is assumed about whether their members can always be ordered in a particular fashion (surprisingly, in 1904, Zermelo proved, using Choice, that sets can always be well-ordered).

When we look at these axioms, is it, then, such a miracle that a legion of complex sentences in $\mathfrak{L}_\in$ cannot be decided on the basis of *these* axioms? No, it is not. We call A. Kanamori to the witness stand (see his testimony in the displayed quotation in the previous Section). What *we* find miraculous is the Set-Foundation Thesis (10), the fact that 99% of all of current mathematics can be seen as a body of theorems of ZFC, and that every single concept and structure in mathematics can be defined in the extremely simple 1st-order language $\mathfrak{L}_\in$. What *we* find miraculous is that already such a gigantic amount of deep and interesting theorems about sets have been proved in ZFC. What we find miraculous is that so much about ordinal- and cardinal-numbers can be proved whereas not a single axiom of ZFC even *mentions* these numbers, as if the theory is not about these numbers at all. And *mutatis mutandis* for $\mathfrak{R}$ (descriptive set-theory). So yes, given the meaning of the set-concept as constituted by AxZFC, we can understand the ubiquitous undecidability phenomenon in set-theory very well and *ipso facto* the relaxed attitude of the set-theoreticians towards it. Thus we have reached our fourth aim (Aim D).

7. CAPTIVATING PICTURES

In this final Section, we are going to argue that the Skolemite skeptic is to a certain extent 'in the grip of a picture', to use Wittgensteinian slang (Aim E). We claim this explains the skeptic's tacit adherence to Connexion M. Two such pictures come to mind in the present context: what we call since Wittgenstein (1953, 1–2) the ‘Augustinian picture of meaning’ (*AugPict*), and a ‘Platonist picture of reference and truth’ (*PlatoPict*). We hope to show that these pictures are misleading, if not false, *when taking them as illustrations of what is going on in model-theory*, which is where the Löwenheim–Skolem meta-theorems belong – in this capacity these pictures seduce us to accept Connexion M.

(*AugPict*) In his *Philosophical Investigations*, Wittgenstein (1953, §1, our emphasis) writes, after the long opening quotation from Augustine’s *Confessiones*:

> These words, it seems to me, give us a particular picture of the essence of human language. It is this: the individual words in language name objects – sentences are combinations of names. In this picture of language we find the roots of the following idea: *every word has a meaning. It is the object for which the word stands.*

This is usually called the *Augustinian picture of meaning*. Talk about model-theory as the provider of the *semantics* of a formal language, as traditional terminology in logic would have it, suggests that models bestow *meaning* on an otherwise ‘un-interpreted calculus of symbols’; it suggests that the meaning of a word is “the object for which the word stands”. The logical-positivists and the great Bourbaki (1968, 8), for example, implicitly or explicitly subscribed to this Augustinian picture:

> This faculty of being able to give different *meanings* to the words or prime concepts of a theory is indeed an important source of enrichment of the mathematician’s intuition, which is not necessarily spatial or sensory, as is sometimes believed, but is far more like a certain feeling for the behaviour of mathematical objects, aided often by images from very varied sources, but founded above all on everyday experience.

(From the model-theoretic context of this passage, it is evident that by “different meanings” Bourbaki means: different referents.) Zermelo, Skolem, Von Neumann and other sceptics were all to a considerable extent under the sway of the Augustinian picture, as were some Formalists. Skolem (1941, 455) speaks of predicates having the same referents in two models as “on conserve *la signification*” (conserving meaning). But also an anti-Formalist as the Platonist Gödel says (reported in Wang (1996, 180), our italics):

> Language is *nothing but* a one-to-one correspondence between abstract objects and concrete objects (namely the linguistic symbols).

To assert generally that model-theory tells us what the set-terms of $\mathfrak{L}_\in$ *can mean* amounts to embracing (some version of the) Augustinian picture of meaning applied to the set-concept. We then have new and mutually exclusive *meanings* of ‘set’ in every model!

Let us, for a moment, ponder what it generally means *to interpret* a linguistic expression. If we understand an expression, that is, if we know its meaning (in the context in which it occurs), then there is no need to interpret it. We feel the need to interpret an expression iff we do not understand it, iff we do not know what it means. Let us call such an expression *obscure* for us. There are three, mutually exclusive cases of obscurity. One is that we can think of more than one meaning of it but the context does not seem to single out one of them – perhaps we are mistaken and have to read the text more carefully, or have to inquire elsewhere to decide which meaning is the intended one. The second case is that we can think of one meaning only but we are far from sure that this is what is meant. The third case is that we cannot think of even a single meaning to attach to the expression. In this third case, and arguably also the second case, it is natural to use the word 'obscure'; but we use it for the first case too for the sake of contrast and emphasis with what is to come. We now take to *interpret* a linguistic expression to mean: to assign a meaning to it of which we are confident it is the right one. Let us call interpretation in this sense *Clarification*.

Obviously, Clarification has *nothing* to with what model-theoreticians and logicians call 'interpretation'. The task of model-theory is not to remove obscurity by assigning a clear meaning to meaningless terms – unless, again, the Augustinian picture of meaning is presupposed, for then to assign a referent will be to assign a meaning and then the meta-mathematical domain of discourse will consist of meanings.

This consideration again leads us, again, to conclude that model-theoretic talk of the *semantics* of an object-language, as if the activity of model-theoreticians has something to do with assigning clear meanings to otherwise obscure expressions, i.e., with Clarification, is highly misleading.

But if Clarification is not what model-theory is good for, then why do we engage in it at all? Well, for reasons that any student of model-theory can tell you: we engage in model-theory to prove the consistency-theorems about mathematical theories, to examine the independence of axioms, to investigate various other meta-mathematical aspects, such as soundness, completeness, the complexity of formal sentences and what have you. *That* is the *raison d'être* of model-theory. We surely do not engage in model-theory in order to liberate mathematics from a sorry state of meaninglessness and obscurity to restore Clarity. Arguably mathematics is the clearest project of inquiry in the entire history of thought.

The Augustinian picture of meaning, which is strongly suggested by the terminology in model-theory, is not just misleading in that it gives the wrong picture of what model-theory is about, but also because it is an untenable picture of *meaning* generally and of meaning in the language of mathematics in particular. The only facts available to investigate the meaning of mathematical concepts is not to study the model-theory of the theories of the concepts and ignore what mathematicians are doing, but to study the multifarious ways in which these concepts are used by mathematicians at the object-level. In the grip of the Augustinian picture, Connexion M, which asserts, roughly, that adequate meaning-capture implies referential determinacy, is then almost a tautology, and who does not accept tautologies?

(*PlatoPict*) Another picture suggested by the model-theoretic terminology of 'reference' and 'truth' is the *Platonic picture of reference and truth*. In this picture, Mod(ZFC) is seen as the whole of 'all possible set-worlds', which somehow (can be thought of to) exist independently of us; these set-worlds are populated by items to which our object-language $\mathfrak{L}_\in$ of ZFC can genuinely refer; this referring 'explains' why certain sentences in $\mathfrak{L}_\in$ are true and others are false of the given set-world. The referential indeterminacy of ZFC is, then, seen as a weakness of ZFC: we talk ZFC but we do not and cannot have a clue *which* set-world we are talking about, because all set-worlds in Mod(ZFC) make ZFC true. So a pluralist kind of Platonism, a variety of maximally inflationary metaphysics, is hovering above such considerations. (Besides this 'many-worlds-interpretation' of mathematics, the Platonist can single out one set-world, preferably **V** to deflate her inflationary metaphysics a bit.)

The inadequacy of this picture as a picture of what is going on in model-theory becomes clear when we focus on the several concepts involved in the comparison. One suggestive model-theoretic misnomer is the word 'referent', e.g., by calling the images of the mapping that sends 'elements of the meta-domain of discourse', the putative 'referents', to set-terms of $\mathfrak{L}_\in$. Whether inscrutable or not, it is a fact of linguistic practice that the reference-concept is employed in acts of ostension, almost always are directly observable, spatially extended chunks of matter, such as when we point to a cat on a mat when we say to a small child 'There is a cat on the mat', or point to a cherry in a tree when we say 'There is a cherry in the three'. (Possible confusion between the referents of the referring words in these sentences disappears as soon as we point our finger and if needed accompanied with a few more words.) Ostensive ceremonies that endow singular terms and descriptions with referents and referential instances, respectively, observable or not, *always* involve causal dealings with the observable world. Therefore the reference-concept, no matter how broadly construed, has little, if anything, to do with the 'reference-mapping' of model-theory. The meaning of the model-theoretic concept of reference is something completely different from what we usually consider to be doing when we are *referring*, the fact there are also similarities notwithstanding.

Exactly the same holds for the model-theoretic notions of *existence* and *truth*. The model-theoretic notion of existence of objects to which terms of a formal language 'refer' is simply construed as we construe existence at the object-level (Obj): either as the provability of existence-theorems (but now of the meta-theory rather than of the object-theory), or as the consistent conjoinability of existence-sentences to the meta-theory (Hilbert's notion of mathematical existence as consistency). In contradistinction to these two notions of mathematical existence, the existence of cats, cherries, and even of unobservables such as the cosmic background radiation, tau-neutrinos and black holes is something completely different.

The model-theoretic notion of *truth-in-a-language-by-a-model* is for most theories a recursively defined notion in terms of some 'reference-mapping' ('satisfaction'), so that truth-conditions also come under the rigourous sway of the meta-theory. The correspondence-notion of truth is something quite distinct – some similarities intended by Platonists notwithstanding. The truth of the matter of model-theory is that the models and everything they are equipped with are *just as much theoretical constructions at the meta-level as are the constructions at the object-level.* Putnam (1980, 482) considered this observation to be sufficient for the solution of the Skolem trouble:[11]

> Models are not lost noumenal waifs looking for someone to name them; they are Constructions within our theory itself and they have names from birth.

The moment we realise this, the Platonic picture of reference and truth begins to dissolve and Connexion M dissolves with it. We now have reached our fifth and sixth aim (Aims E and F).

ACKNOWLEDGMENTS

I am indebted to valuable comments of anonymous referees. These investigations were financially supported by the Dutch Organisation for Scientific Research (NWO), Section Philosophy and Theology.

NOTES

[1] It has only variables, called *set-variables* and one primitive, dyadic predicate: 'is a member of', denoted by Peano's $\in$.

[2] For proofs, see Skolem (1920, 1922), which includes a proof of the numerical version without using the Axiom of Choice; Skolem (1929); Tarski and Vaught (1957, 93) first proved the 'upwards' version in (2), which is equivalent to Choice, cf. Hodges (1993, 87–94).

[3] McIntosh (1979), Putnam (1980), George (1985), Benacerraf (1985), Wright (1985), Field (1994), Hallett (1994), Jané (2001).

[4] See Neumann (1925); for analyses of the skeptical views of Skolem and Zermelo, see Benacerraf (1985), George (1985), Fraenkel et al. (1973, 302–305), Dalen and Ebbinghaus (2000) and Jané (2001).

[5] See Muller (2001) for a 1st-order theory of sets and classes logically weaker than ZFC^+ yet still being able to found all of mathematics, including all of category theory; there we argue that ZFC^+ and sibling pure set-theories are all *conceptually flawed as a founding theory for category theory*.

[6] So rather than a property meaning looks more like a relation: between an expression and a context. Let that pass.

[7] In Muller (2004) we only considered pure mathematics.

[8] It would then be more appropriate to write $\langle N, \wp N, S, 0\rangle$. This is not the same as saying that *number theory*, e.g., Peano Arithmetic, is categorical, for that depends on whether a 1st- or a 2nd-order language is used.

[9] Save a few exceptions, such as P. Aczel's investigations into non-well-founded sets. Such entities arguably fall under a concept that is markedly different from the set-concept we are discussing.

[10] The 'open-endedness' is often connected to M. A. E. Dummett's notion of an 'indefinitely extensible' concept, the handling of which must proceed with non-classical logic and some *idem dito* semantics, or so Dummett argued. Paseau (2003) has confuted this thesis and has argued that classical logic and semantics will do just fine for set-theory.

[11] Cf. Hallett (1994), who elaborates this point too.

REFERENCES

Benacerraf, P.: 1985, 'Skolem and the Skeptic', *Proceedings of the Aristotelian Society. Supplementary Volume* **59**, 85–115.

Bourbaki, N.: 1968, *The Theory of Sets*, Hermann, Paris.

Dalen, D. van and H.-D. Ebbinghaus: 2000: 'Zermelo and the Skolem Paradox', *Bulletin of Symbolic Logic* **6**, 145–161.

Devlin, K. J.: 1984, *Constructible Sets*, Springer-Verlag, Berlin.

Field, H.: 1994, 'Are Our Logical and Mathematical Concepts Highly Indeterminate?', *Midwest Studies in Philosophy* **XIX**, 391–429.

Fraenkel, A. A., Bar-Hillel, Y., Lévy, A., and van Dalen, D.: 1973, *Foundations of Set-Theory*, North-Holland, Amsterdam.

Frege, G.: 1980, in G. Gabriel (ed.), *Philosophical and Mathematical Correspondence*, Basil Blackwell, Oxford.

George, A.: 1984, 'Skolem and the Löwenheim–Skolem Theorem: A Case Study of the Philosophical Significance of Mathematical Results', *History and Philosophy of Logic* **6**, 75–89.

Hallett, M.: 1994, 'Putnam and the Skolem Paradox', in P. A. Clark and B. Hale (eds.), *Reading Putnam*, Basil Blackwell, Oxford, pp. 66–97.

Hodges, W.: 1993, *Model Theory*, Cambridge University Press, Cambridge.

Horwich, P.: 1998, *Meaning*, Clarendon Press, Oxford.

Jané, L.: 2001, 'Reflections on Skolem's Relativity of Set-Theoretical Concepts', *Philosophia Mathematicae* **9**, 129–153.

Kanamori, A.: 2003, *The Higher Infinite. Large Cardinals in Set-Theory from Their Beginnings*, 2nd ed., Springer-Verlag, Berlin.

Kunen, K.: 1980, *Set Theory. An Introduction to Independence Proofs*, North-Holland, Amsterdam.

McIntosh, C.: 1979, 'Skolem's Criticisms of Set-Theory', *Noûs* **13**, 313–334.

Moschovakis, Y. N.: 1980, *Descriptive Set Theory*, North-Holland, Amsterdam.

Muller, F. A.: 2001, 'Sets, Classes and Categories', *British Journal for the Philosophy of Science* **52**, 539–573.

Muller, F. A.: 2004, 'The Implicit Definition of the Set-Concept', *Synthese* **136**.

Neumann, J.: 1925, 'Eine Axiomatisierung der Mengenlehre', *Journal für die Mathematik* **154**, 219–240; also in his *Collected Works I*, A. H. Taub (ed.), Pergamon Press, Oxford, 1961, pp. 35–47.

Paseau, A.: 2003, 'The Open-Endedness of the Set-Concept and the Semantics of Set-Theory', *Synthese* **135**, 379–399.

Putnam, H.: 1980, 'Models and Reality', *Journal of Symbolic Logic* **45**, 464–482.

Shelah, S.: 1985, *Classification Theory*, North-Holland, Amsterdam.

Shelah, S.: 2002a, 'The Future of Set-Theory', Los Alamos arXiv: math.LO/02.11.397

Shelah, S.: 2002b, 'Logical Dreams', Lecture held at the conference *Mathematical Challenges of the 21th Century*, Los Alamos arXiv: math.LO/02.11.398.

Skolem, T.: 1920, 'Logisch-kombinatorische Untersuchungen über die Erfüllbarkeit oder Beweisbarkeit mathematischer Sratze nebst einem Theoreme uber dichte Mengen', *Videnskapsselskapets Skrifter, I. Matematisc-naturvidenskabelig klasse*, no. 3.

Skolem, T.: 1922, 'Einige Bemerkungen zur axiomatischen Begrundung der Mengenlehre', *Matematikerkongressen i Helsingfors den 4–7 Juli 1922, Den femte skandinaviska matematikercongressen, Redogörelse*, Akademiska Bokhandeln, Helsinki, 1923: 217-237 [Skolem 1929] T., 'Über einige Grundlagenfrage der Mathematik', *Skrifter Vitenskapsakademiet i Oslo I*, **4**, 1–49.

Skolem, T.: 1941, 'Sur la Portée du Théorème de Lowenheim–Skolem', *Les Entretiens de Zurich*, 25–52.

Skolem, T.: 1959, 'Une relativisation des notions mathématiques fondamentales', *Colloques Internationaux des Centre National de la Recherche Scientifique*, Paris, pp. 13–18.

Skolem, T.: 1962, 'Interpretation of Mathematical Theories in the First-Order Predicate Calculus', in Y. Bar-Hillel (ed.), *Essays on the Foundations of Mathematics. Dedicated to A. A. Fraenkel on his Seventieth Anniversary*, North-Holland, Amsterdam, pp. 218–225.

Steel, : J. R.: 2000, 'Mathematics Needs New Axioms', *Bulletin of Symbolic Logic* **6**, 422–433.

Tarski, A. and Vaught, R. L.: 1957, 'Arithmetical Extensions of Relational Systems', *Compositio Mathematica* **13**, 81–102.

Wang, H.: 1996, *A Logical Journey. From Gödel to Philosophy*, The MIT Press, Cambridge, MA.

Wittgenstein, L.: 1953, *Philosophical Investigations* (translated by G. E. M. Anscombe), Basil Blackwell, Oxford.

Wittgenstein, L.: 1979, in A. Ambrose,(ed.), *Cambridge Lectures 1932-1935*, Basil Blackwell, Oxford.

Wright, C.: 1985, 'Skolem and the Skeptic', *Proceedings of the Aristotelian Society. Supplementary Volume* **59**, 117–137.

Institute for History and Philosophy of Science and Mathematics
Utrecht University
P.O. Box 80.000, 3508 TA Utrecht, The Netherlands
E-mail: f.a.muller@phys.uu.nl