Notes on “Skolem’s Paradox” and its Philosophical Implications

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1 Hunter on the “Paradox” and Its Implications

1.1 Hunter on the Upward LST and Number-Theoretic Concepts

I begin with some of the material from pages 205–208 of Hunter. Here, Hunter provides some clarification of the technical results surrounding “Skolem’s Paradox,” and he offers his own resolution of the “paradox.” First, he gives some results pertaining to the upward LST and number-theoretic concepts (two kinds).

49.1. No first-order theory can have as its *only* model one whose domain is the set of natural numbers $\mathbb{N}$.

Proof. This “follows” from the key lemma [45.15] of the completeness theorem of QS. Every consistent first-order theory T has a Henkin model M_T' (in addition to its *intended* model M_T, the domain of which is, say, $\mathbb{N}$), the domain of which is the set of closed terms of T. Closed terms are symbols, not numbers. So, the domain of M_T' is not $\mathbb{N}$. [At least, we don’t *intend* to be talking about numbers when we talk about Henkin models! Structuralists (like Quine) need not worry about this weak kind of ambiguity, so long as there is a structure-preserving 1–1 mapping – an *isomorphism* – between the two domains. Our proof of lemma 45.15 did not *construct* such an isomorphism. But, it seems that one should exist here, since both sets are denumerable, and the models are *equivalent*. We’ll return to these issues in depth, below.] □

49.2. Let T be any first-order theory. Fix the meanings of the logical connectives and quantifiers [the *logical constants* — on which, see the *Stanford Encyclopedia of Philosophy* entry by our own John MacFarlane] in the usual way. And, let the axioms of T fix the meanings of the non-logical symbols (the prdicates, functions, *etc.*) of T — to the extent that they *can* fix these meanings. Even if T has denumerably many axioms, the axioms of T cannot force us to interpret any predicate symbol in T as meaning “is a natural number”, and they cannot force us to interpret any expression in T as the name of a natural number.

Proof. Again, this “follows” from 45.15, which says that T will have an *unintended* model $M_T' \neq M_T$. As such, any expression of T that is a name of a natural number (or means “is a natural number”) on the intended interpretation of T will be the name of some closed term of T (or will be a property whose extension is not any subset of $\mathbb{N}$ but some subset of the set of closed terms of T) on M_T. So, we are not forced to interpret any expression of T as the name of a natural number (or as meaning “is a natural number”). The moral here is supposed to be that we cannot unambiguously define *numeral nouns* using just the narrowly logical structure of any first-order theory. But, again, this weak sense of ambiguity (the “intuitive” lack of “*identity isomorphism*” between M_T' and M_T) is one that needn’t bother a structuralist, since 45.15 is consistent with there being an *isomorphism* between M_T and M_T' (more on this below). □

49.3. As for 49.2, with, in addition, $F^{**\prime}$ (=) interpreted as meaning *identity* in the domain of the model.

Proof. As explained on Kenny’s handout, if T has any model at all, it will have a *normal* model M_T [a model in which $F^{**\prime}$ (=) means *identity*] the domain of which is the set of closed terms of T (*not* $\mathbb{N}$). □

49.4. Any first order theory that is intended to be an axiomatization of number theory (if it has a model at all) has a model that is not even isomorphic to its intended model.

Proof. This follows from the *upward* Löwenheim-Skolem-Tarski theorem (discussed in Kenny’s handout on the LST), which implies that if a first order theory T has a denumerable model (a model whose domain is of cardinality $\aleph_0$), then it also has a model of whose domain has cardinality $\mathfrak{c}$ (the cardinality of the continuum $\mathbb{R}$). Cantor’s diagonalization argument (part I of the course) shows that there is no isomorphism between $\mathbb{N}$

and $\mathbb{R}$. This stronger kind of ambiguity should (at least on it face) be more worrisome for the structuralist, since there can be no isomorphism between M_T and M'_T. Some have responded by moving to second-order logic to characterize $\mathbb{N}$ (see below for an extended discussion). □

49.5. Provided that $F^{**\prime}$ (=) is interpreted as identity, it *is* possible to unambiguously define *numeral adjectives* such as "there are exactly n objects that have the property F".

Proof. This is also explained on Kenny's handout. Basically, we can define "there are exactly n objects that have the property F" unambiguously (for any natural number n), using the theory $QS^=$, as follows:

$$\bigvee x_1 \ldots \bigvee x_n \Big[Fx_1 \wedge \ldots \wedge Fx_n \wedge x_1 \neq x_2 \wedge \ldots \wedge x_1 \neq x_n \wedge x_2 \neq x_3 \wedge \ldots \wedge x_2 \neq x_n \wedge \ldots \wedge x_{n-1} \neq x_n \wedge \\ \bigwedge x_{n+1} (Fx_{n+1} \supset (x_{n+1} = x_1 \vee x_{n+1} = x_2 \vee \ldots \vee x_{n+1} = x_n)) \Big]$$

Similarly, as Kenny explains, if we have *only* the predicate = (interpreted as identity) in T, then we can ensure that the domain of any model of T will have exactly n elements, for any natural number n. □

49.6. Using just the narrowly logical structure of a first-order theory with the predicate = interpreted as identity, we can unambiguously define numeral adjectives, but we cannot do the same for numeral nouns.

Proof. "Follows" from 49.2 & 49.4. There are two senses in which first-order theories of $\mathbb{N}$ are *ambiguous*:

- *Weak Ambiguity*: there will (by 45.15, DLST) be models of T that are (intuitively) *non-identical* to the intended interpretation of T (because their domain contains *terms*, *not numbers*).

- *Strong Ambiguity*: there will (by ULST) be models of T that are not even *isomorphic* to the intended interpretation of T. Presumably, our intentions are doing some work here in pinning down M_T! □

Remarks. These results indicate that there are limitations on what can and cannot be unambiguously defined (or what concepts can be given a precise explication) using *only* the narrowly logical (axiomatic) structure of any first order theory T. At this point, you should ask yourself some questions.

- What are the *aims* of formal logic? Is one of these aims to unambiguously define numeral nouns?

- What are the aims of mathematics (in particular, *number theory*)? Is one of these aims to unambiguously define numeral nouns? And, even so, why should mathematics be restricted (in this endeavor) to *using just the narrowly logical (axiomatic) structure of some first order theory T*?

Some mathematicians (and even logicians, like Quine) have had this sort of strong aim. Quine was suspicious of second-order logic (see below for elaboration). But, others, like Frege, have thought that higher-order theories would be required to provide such unambiguous explications of numeral nouns. Frege used second-order logic for this purpose. And, *even then*, Frege was *still* worried about ambiguity! In his *Foundations of Arithmetic*, he worries that we may never be able to

> decide by the means of our definitions whether any concept has the number Julius Caesar belonging to it, or whether that conqueror of Gaul is a number or not.

Frege's worries cannot have been based on any Skolem-type theorem, since second-order theories do not admit of such theorems (and, besides, Frege was unaware of such theorems anyway — he was writing years before Skolem). This illustrates that *ambiguity* (the possibility of non-standard or unintended interpretations or models of formal theories) is always a potential problem for formal systems (and formal theorizing). Many modern mathematicians and logicians would want to enforce *categoricity* (isomorphism of all models of a theory) as a standard of non-ambiguity. In this sense, it can be shown that second-order logic *is* sufficient to unambiguously define numeral nouns, since all models of (adequate) second-order theories of the natural numbers are isomorphic. Frege, it seems, had a more stringent standard of non-ambiguity in mind (since it seems he was *still* worried!). I'll return to this more general kind of worry about ambiguity, below (and when I discuss Myhill's paper on "Skolem's Paradox").

At this point, it would be useful for us to reflect on the aims of logic *vs* mathematics. To borrow a pair of locutions of C.S. Peirce, I think of (formal) logic as *the (formal) science of deduction*, and (formal) mathematics as a family of *(formal) deductive sciences*. Let's take logic first. If formal logic is the formal science of deduction, then what it aims to do is provide a formal framework for systematizing as many intuitively valid arguments as it can. Propositional logic P is able to systematize some intuitively valid arguments. Monadic predicate logic Q^M is able to capture even more valid arguments, and full first-order logic with identity $QS^=$ is able to capture a great many more. But, there are intuitively valid arguments that $QS^=$ can't capture. Moreover, first order axiomatizations of number theory are inherently *incomplete* (they fail to capture all the intuitively valid arguments in number theory). Similarly, even formal second-order theories will fail to capture some intuitively valid second-order arguments (for they too are incomplete). Now *these* are *logical* shortcomings! It seems to me that a formal theory is *logically* defective to the extent that it fails to allow us to systematize some body of intuitively valid arguments that we wish to capture. So, it seems to me that the fact that we cannot unambiguously define any numeral nouns using the logical structure of any first-order theory is rather beside the point, from a *logical* point of view. But, it is *not* beside the point from a *mathematical* point of view. So, what about mathematics?

In the case of number theory, one can think of this as the deductive science of natural numbers. To be sure, such a science is concerned with precise explications of number concepts, including those expressed by numeral nouns. But, we now have two crucial questions about mathematics so construed:

(1) Why assume that the only tools it can use for such explications are first order theories?

(2) Why assume it can use *only the narrowly logical (axiomatic) structure* of *any* formal theory (even a higher-order one as in Frege's case) for such purposes?

Concerning (1), first-order theories are often thought to be "elementary" and "clear" in a way that second-order theories are not. And, this explains why many modern mathematicians and logicians prefer first-order logic (and theories) to second-order ones. For one thing, first-order logic is consistent and complete, and it is semi-decidable, whereas second order logic is incomplete (if consistent), and not even semi-decidable. Moreover, second-order logic does not admit of as powerful techniques of mechanical (*i.e.*, computerized) reasoning. These all seem to be *logical* virtues of first-order logic over second-order logic. But, there are certainly *expressive* virtues of second-order logic. One can use second-order logic to characterize the natural numbers (up to isomorphism, *i.e.*, *categorically*), and that is something that you can't do with any first-order theory. But, to gain these expressive powers, one must give-up the logical virtues of first-order logic. This trade-off between "logical virtue" and "mathematical virtue" of a formal framework is something important to keep in mind as you read about debates over "Skolem's Paradox". I discuss this further, below, in my discussion of Gödel, Benacerraf, and Quine.

So, ultimately, I think it's best to think of "Skolem's Paradox" (and related issues) as issues in the philosophy of *mathematics* (as opposed to logic proper). Many mathematicians want *more* than mere systematization of valid argument forms that crop up in mathematics. Those who are mathematical realists, for instance, believe that there are objective facts of the matter about (say) natural numbers. Like physicists, such mathematicians want theories that allow us to capture and explain as many *truths* about natural numbers as possible; and, presumably, *this* will involve (*inter alia*) being able to unambiguously explicate concepts expressed by numeral nouns. The analogy between (pure) mathematics and physics is useful here, I think. Surely, no physicist would say that their goal is to precisely explicate the concept of (say) "electron" *using only the narrowly logical structure of the mathematical theories of modern physics*! But, there is an important difference here. Presumably, there is something *external* to the logical (or mathematical) structure of our physical theories that allows us to pin down the domain of those theories *unambiguously*. We can *see* tracks in cloud chambers that are *caused* by electrons. *This* is what allows us to say that (some of) the statements in our physical theories are *about electrons*. Note: the kind of non-ambiguity we have in our formal theories about the physical world is of a much stronger kind than mere "isomorphism of models". I take it that we think we can uniquely – up to *identity*, and not merely up to isomorphism – pin down the referent "the electron in that cloud chamber". What allows us to do this is, presumably, some sort of causal relationship that obtains between our minds and physical objects. The question is: how could we do anything like this for mathematical entities like natural numbers? This brings us back to Frege's worry about Julius Caesar.

One of the reasons it would be nice [(2)!] to be able to unambiguously define various number concepts using just the narrowly logical structure of our mathematical theories is that it's difficult to imagine how *else* we could do it. It is for this reason that most contemporary philosophers of mathematics (and logic) have looser standards for non-ambiguity than Frege did. Typically, if we can pin things down *up to isomorphism*, then this is taken to be *good enough* for the purposes of formally explicating mathematical concepts like "the number 5". In second-order logic we can achieve *this kind* of unambiguous characterization, but not in first-order theories. This is the significance of the upward LST for number theory. Analogous things can be said about the downward LST and set-theory, which brings us to Hunter's remarks on set theory, the downward LST and "Skolem's Paradox". First, a digression…

Digression #1: Gödel's Realism and Benacerraf's Anti-Platonism/Structuralism About Mathematics

Gödel articulated some interesting views about mathematical entities (*e.g.*, numbers and sets). Some of these metaphysical views were *Platonistic* (*i.e.*, *Realistic*) in nature. Here are some quotes:

> The truth, I believe, is that these concepts form an objective reality of their own, which we cannot create or change, but only perceive and describe … I mean … that mathematics describes a non-sensual reality, which exists independently both of the acts and the dispositions of the human mind and is only perceived, and probably perceived very incompletely, by the human mind.
>
> It seems to me that the assumption of such objects [classes and concepts] is quite as legitimate as the assumption of physical bodies and there is quite as much reason to believe in their existence. They are in the same sense necessary to obtain a satisfactory system of mathematics as physical bodies are necessary for a satisfactory theory of our sense perceptions.
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> … the question of the objective existence of the objects of mathematical intuition … is an exact replica of the question of the objective existence of the outer world…

He was also sensitive to the fact that there needs to be some story about mathematical *epistemology* — *i.e.*, about how we (can) *know* that there are numbers and sets, and how we can know about their properties. And, he realized that his metaphysical Platonism about mathematical entities did not make this trivial. He had a suggestion:

> … the objects of transfinite set theory … clearly do not belong to the physical world and even their indirect connection with physical experience is very loose … But, despite their remoteness from sense experience, we do have something like a perception also of the objects of set theory, as is seen from the fact that the axioms force themselves on us as being true. I don't see any reason why we should have less confidence in this kind of perception, i.e., in mathematical intuition, than in sense perception, which induces us to build up physical theories and to expect that future sense perceptions will agree with them, and, moreover, to believe that a question not decidable now has meaning and may be decided in the future.

Most contemporary philosophers of mathematics are *not* Gödelian realists. [Although, interestingly many mathematicians seem to be!] This is (mainly) because there is skepticism about Gödel's "mathematical intuition" as a source of mathematical knowledge. Most contemporary philosophers are *naturalists* (and not Platonists). They think that the only way we can know about the world is by *causally* (*physically*!) interacting with it. Beneacerraf is one of the most articulate writers concerning this skepticism about mathematical knowledge in Gödel's sense:

> I find this picture [of Gödel's] both encouraging and troubling. What troubles me is that without an account of how the axioms "force themselves upon us as being true," the analogy with sense perception and physical science is without much content. For what is missing is precisely ... an account of the link between our cognitive faculties and the objects known. In physical science we have at least a start on such an account, and it is causal. We accept as knowledge only those beliefs which we can appropriately relate to our cognitive faculties. Quite appropriately, our conception of knowledge goes hand in hand with our conception of ourselves as knowers. ... So much for the troubling aspects. More important perhaps and what I find encouraging is the evident basic agreement which motivates Godel's attempt to draw a parallel between mathematics and empirical science. He sees, I think, that something must be said to bridge the chasm, created by his realistic and platonistic interpretation of mathematical propositions, between the entities that form the subject matter of mathematics and the human knower. Insead of tinkering with the logical form of mathematical propositions or with the nature of the objects known, he postulates a special faculty through which we "interact" with these objects. We seem to agree on the analysis of the fundamental problem, but clearly disagree about the epistemological issue — about what avenues are open to us through which we may come to know things.

A popular contemporary version of anti-Platonism about mathematics is called *Structuralism*. Structuralism in the philosophy of mathematics is the view that the proper subject matter of mathematics is the logico-structural relationships between various kinds of mathematical entities, rather than the entities themselves. So, for instance, number theory is the study of ω-sequences. The nature of the entities that constitute the ω-sequences is irrelevant — what is important is the *structure – up to isomorphism* – that is common to all ω-sequences. Structuralism provides a response to Frege's Julius Caesar problem. According to structuralists, Julius Caesar *can* play the role of the number two, so long as Caesar has the appropriate relationships with other entities, and together they make up an ω-sequence (with Caesar being the successor of the successor of the entity playing the zero role, of course). According to Structuralists, the number two is no more and no less than a position in a structure (individuated up to isomorphism — not up to "intuitive identity"). Benacerraf and Quine end-up defending structuralist views.

Structuralism goes hand in hand with (1) making the goal of mathematics to explicate mathematical concepts *using only the narrowly logical structure of formal theories*. Why? Because the formal, axiomatic structure is (presumably) something we can know about *by logical analysis alone* — we don't need any mysterious "intuitive connection to mathematical entities" for *that*. In other words, Structuralism seems to give us a more "naturalized epistemology friendly" philosophy of mathematics. It is motivated by the desire to have both a metaphysics and an epistemology for mathematics that fit together in a broadly naturalistic picture of the world. This trend toward structuralism and away from Platonism also explains why the *strong* kind of ambiguity arising from the lack of isomorphism of the models of a formal theory (as is guaranteed for first order theories by the LST) is considered the salient kind of ambiguity in modern philosophy of mathematics, and why Fregean *weak* ambiguity (the "Julius Caesar" kind of ambiguity) is no longer a very serious source of worry for contemporary philosophers of mathematics. But, this still doesn't explain why there is (2) a preference among contemporary philosophers like Quine for *first-order* theories — especially, since we are *unable* to characterize "the natural numbers" *even up to isomorphism* using first-order theories, but we *can* achieve this goal if we allow ourselves second-order logic. This is where set-theory and the DLST becomes crucial. We'll return to that soon. But, first, another digression:

Digression #2: "Realism *vs* Anti-Realism" in the Philosophy of Science/Math

There are various things that might be meant by "realism" in philosophy of science and math:

- **Metaphysical Realism**. This is the view that there objective, mind-independent (*i.e.*, independent of human conceptualizations and theories) facts of the matter concerning some domain. For instance, a metaphysical realist about the physical world thinks there is an objective fact of the matter about claims made by physical theories. This would include statements like "there are electrons". According to a metaphysical realist, whether such statements are true or false does not depend on what anyone thinks (or on the nature of the human mind, etc.). A metaphysical realist about mathematics would say something similar about "there are numbers" or "there are sets". NOTE: *being a metaphysical realist does not commit you to the existence of any specific entities.* For instance, you can be a metaphysical realist about physics, but believe that there are no electrons. Or, you can be a metaphysical realist about mathematics, but believe there are no numbers. The important thing is that you think that these are objective, mind-independent statements and issues. And, I will assume that metaphysical realists interpret theories *literally* (according to their intended interpretation).

- **Platonism**. This is a family of views (all of which fall under the umbrella of metaphysical realism) concerning the mind-independent existence of certain kinds of abstract objects. Typically, Platonists believe that there are universals (forms). And, in mathematics, Platonism involves a commitment to the existence of numbers and sets. A **Gödelian** Platonist believes that there are numbers and sets and that they are not fully characterized by logical or structural properties alone (since, for Frege and Gödel, 2 ≠ Julius Caesar is a "Platonic Truth"). **Structuralist** Platonists believe that there are numbers (and sets), and that they are fully characterized by their logico-structural properties (J.C. *could be* "the number 2", if it had the right structural properties and relations). As I will use the term, structuralist Platonism is not (necessarily) committed to any Platonic Heaven that is outside the physical realm (more on this below, in my detailed discussion of Quine's view).

- **Scientific Realism**. This is a family of views (all of which fall under the umbrella of metaphysical realism) concerning (1) the mind-independent existence of certain kinds of unobservable/theoretical entities, and (2) the aims of science. Scientific realists are committed (1) to the *literal truth* of our best scientific theories – including the parts of our best theories that involve unobservable/theoretical entities, and (2) to the claim that the aim of science is to obtain literally true theories about the world – including any unobservable entities in the world. So, a scientific realist believes that there are quarks (and perhaps even sets – see below), and that our aim is to discover the truth about the world – including its "deep, unobservable" structure. Typically, this aim is associated with *explaining* phenomena – often by the postulation of and the appeal to (in principle) unobservable entities. If our "best explanation" of a phenomenon makes use of unobservable entities, then we should be committed to their existence. This is something a scientific realist believes, but a scientific empiricist does not. Or, at least, this seems to be central to the disagreement (see below).

- **Scientific Empiricism (Anti-Realism)**. This is a family of views (all of which fall under the umbrella of metaphysical realism!) concerning (1) the mind-independent existence of certain kinds of unobservable/theoretical entities, and (2) the aims of science. A scientific empiricist is *agnostic* on the existence of (in principle) unobservable entities (entities whose existence cannot be confirmed by any experiment, say, which may include electrons and even sets). And, a scientific empiricist is an *instrumentalist* about the aims of science. They believe that science aims only at constructing *empirically adequate* theories (theories that make accurate predictions about observables, or about the outcomes of possible experiments). NOTE: this is mainly a disagreement about the *aims* of science, and *not* about metaphysical realism. Both the scientific realist and the scientific empiricist are metaphysical realists. And, the empiricist does not believe that unobservables do not exist – they just think that beliefs about such things are *non*-scientific (not part of science *qua* science), which is ultimately about observables. For the empiricist, science is about *prediction of obervable phenomena*, *not explanation of observable phenomena*. And, the empiricist believes that predictions about observables can be made without any commitment to the existence of (in principle) unobservable entities. As van Fraassen (our canonical contemporary empiricist) explains in his book *The Scientific Image*:

  > Scientific realism is the position that scientific theory construction aims to give us a literally true story of what the world is like, and that acceptance of a scientific theory involves the belief that it

> is true. Accordingly, anti-realism is a position according to which the aim of science can well be served without giving such a literally true story, and acceptance of a theory may properly involve something less (or other) than belief that it is true. ... What does a scientist do then, according to these different positions? According to the realist, when someone proposes a theory he is asserting it to be true. But according to the anti-realist, the proposer does not assert the theory to be true; he displays it, and claims certain virtues for it. These virtues may fall short of truth: empirical adequacy, perhaps; comprehensiveness, acceptability for various purposes. This will have to be spelt out, for the details here are not determined by the denial of realism.

I want to push the analogy between philosophy of science and philosophy of mathematics a bit farther today. In philosophy of science, the debate between the realist and the anti-realist is, mainly, not about metaphysics but about the aims of science and the bounds of (scientific) epistemological commitment. I'd like to suggest this as a model for thinking about realism and anti-realism in philosophy of mathematics as well. In this way, the anti-Platonist need not *deny* that there are numbers or sets. They can remain agnostic on that question, and shift the focus of the debate to the question of the aims of mathematics (*e.g.*, number theory and/or set theory). For the Gödelian Platonist, the aim of mathematics is to provide *literally true accounts* of various sorts of mathematical entities (*e.g.*, numbers, sets, *etc.*). This makes the Gödelian Platonist rather like the scientific realist. Next, I will talk about Quine, who had an interesting realist[1] view about mathematics that was parasitic on a kind of scientific realism. And, I will compare Quine with van Fraassen, our contemporary scientific anti-realist. Their views are (anachronistically — see *fn.* 1, below) interestingly related. And, they both explicitly talk about models of theories.

Quine was a scientific realist, and a **confirmational holist**. He thought that the totality of our theories about the world get confirmed (or tested) *together* "as a corporate body" – including the mathematical and logical parts of the theories. For this reason, Quine thought that the empirical, theoretical, mathematical, and logical parts of our theories are all revisable in light of our experience. It is perhaps less likely that we will revise logic or mathematics in light of experience rather than revising some less theoretical beliefs. But, for Quine, if this is true, then it is merely a difference in degree and not a difference in kind (in this case, it would just be that math and logic are further away from the "experiential periphery" of our theories, and thus more "insulated from experience"). Moreover, according to Quine, if your best (confirmed) theory quantifies over certain entities, then you should be committed to the existence of such entities. This includes numbers, sets, *etc.*, as well as electrons, forces, and the like. Presumably, Quine has this view about commitment, because he would want to interpret theories *literally*, and he thinks commitment to a scientific theory is commitment to its *truth*. So, since Quine thought that our best theories quantify over sets (as well as electrons, *etc.*), he thought we should be committed to the existence of both sets and electrons. So, for Quine (who was a structuralist Platonist about sets), the truth of set theory gets confirmed by experience (albeit more indirectly) in much the same way that the truth of "snow is white" gets confirmed by experience. On the other hand, someone like van Frassen would agree that – interpreted literally – our (currently) best scientific theories quantify over electrons (and perhaps even sets). Moreover, since van Fraassen is a metaphysical realist, he wants to interpret theories literally. So, he would concede that our scientific theories (literally) imply the existence of electrons (and perhaps even sets). But, van Fraassen would deny that commitment to a scientific theory is commitment to its truth. Rather, he would say that this involves commitment to the theory's *empirical adequacy*. Much of the disagreement between the positions of Quine and van Fraassen involves what they imply about the aims of science, and (hence) about what features of scientific theories determine how well-confirmed (or acceptable) they are. van Fraassen sees science as aiming at providing theories that are *predictively accurate* (where prediction involves observables), whereas Quine sees science as aiming at providing *literally true* theories. They both agree that theoretical concepts from mathematics and logic (as well as other theoretical concepts like "electron" and "force") are instrumental toward "the aims of science." But, since they disagree about these aims, they end-up disagreeing about what the instrumentality of such theoretical concepts implies about our epistemological commitments (*qua* scientists) concerning them. That said, all that seems to follow for

[1] Quine was not *really* a realist (at least, it's not at all clear how we should classify him, in the end). But, I will assume that he was, to make the comparison with Gödel and van Fraassen more interesting.

Quine (so far) is that sets (and whatever else our best theories literally quantify over) *exist*. But, what, precisely, does this *mean* for Quine? Does it mean that there exist structures that are in some sort of Platonic Heaven (and not part of the physical world, as Gödel thought they were)? What are sets and numbers *like* for Quine? Quine thinks his structuralism + confirmational holism + naturalism provides an answer to this question:

> ... the structuralist treatment of number ... is just a way of eliminating an idle question—"What is number?"—and a gratuitous decision among indifferent alternatives ... there can be no evidence for one ontology as over against another, so long anyway as we can express a one-to-one correlation between them. Save the structure and you save all. Certainly we are dependent on a familiar ontology of middle-sized bodies ... but once we have an ontology, we can change it with impunity. For abstract objects this is unsurprising ... For familiar bodies it is less intuitive. But we must bear in mind that an observation sentence, say, 'That's a rabbit', retains all its original visual associations; it is only the purported object that is gratuitously displaced. The very notion of object, or of one and many, is indeed as parochially human as the parts of speech; to ask what reality is really like, however, apart from human categories, is self-stultifying. It is like asking how long the Nile really is, apart from parochial matters of miles or meters. Positivists were right in branding such metaphysics as meaningless. But early positivists were wrong if and when they concluded that the world is not really composed of atoms or whatever. The world is as natural science says it is, insofar as natural science is right; and our judgment as to whether it is right, tentative always, is answerable to the experimental testing of predictions. ... reference can be wildly reinterpreted without violence to evidence ... [this] is just part of a wider picture. Presumably yet more extravagant departures, resistant even to sentence-by-sentence interpretation into our own science, could conform equally well to all possible observations. If we were to encounter such a case ... we might even proceed to master it and then switch back and forth for richer perspectives on reality. But naturalism would still counsel us that reality is to be grasped only through a man-made conceptual scheme, albeit any of various. My global structuralism should not, therefore, be seen as a structuralist ontology. To see it thus would be to rise above naturalism and revert to the sin of transcendental metaphysics. My tentative ontology continues to consist of quarks and their compounds, also classes of such things, classes of such classes, and so on, pending evidence to the contrary. My global structuralism is a naturalistic thesis about the mundane human activity, within our world of quarks, of devising theories of quarks and the like in the light of physical impacts on our physical surfaces.

So, Quine's position seems to be that numbers or quarks or sets are *whatever they must be – up to isomorphism – in order to make our best scientific theories come out literally true.* There may be various observationally equivalent interpretations of a theory in which the "purported objects" of the theory are "gratuitously displaced" in some of these interpretations. But, this indeterminacy (or weak ambiguity) is no problem, so long as there is an *isomorphism* between the models in question. The slogan is: "Save the structure and you save all." In other words, any interpretation of a theory that is isomorphic to the intended interpretation is "ontologically equivalent" to it. Intuitively, we could have *non*-isomorphic models of a theory that are observationally equivalent. For instance, take an (intended) interpretation of a space-time theory that "says" the domain of the theory is continuous, and another model that "says" the domain is denumerable. These models are non-isomorphic, but (plausibly) they may be observationally equivalent. Thus, for Quine, the ontological commitments of a theory may outstrip its observational consequences. Since Quine wants to interpret theories literally, and because he is committed to the truth of the theory (so interpreted), he would (one supposes) be committed to the continuity of space-time (given that our best theory – literally interpreted – implies this). But, then, he would have to deny (as many scientific realists in fact do!) that observationally equivalent theories are always equally acceptable. That is to say, for Quine, there must be other features of a theory that can be relevant to how acceptable it is, such as "simplicity" and "explanatory power". *Pace* Quine, van Fraassen would say "save the *observational* (sub)*structure*" of a theory, and you save all. As an empiricist, van Fraassen thinks observationally equivalent theories are always equally acceptable. NOTE: This is a disagreement about epistemology (and the aims of science), and *not* about metaphysics. The empiricist makes observation and experimental evidence the sole criteria for theory acceptance, whereas the realist thinks there are other salient factors. The empiricist thinks it odd to say "no evidence could favor T_1 over T_2, but I reject T_1 and I accept T_2." Nonetheless, this is the position the scientific realist (*e.g.*, Quine) is in. For Quine, there is no distinction between "pragmatic" and "epistemic" reasons to accept a theory. For Quine, "evidence" is something more akin to "reason to believe", taken in a very broad sense so as to include pragmatic and epistemic factors.

Here is a related puzzle about Quine's view. For Quine, the confirmation of mathematical claims seems to be parasitic on the confirmation of the scientific theories in which they occur. But, *the very same mathematical claims have appeared in both well-confirmed and strongly **dis**confirmed scientific theories*. Why aren't they *disconfirmed* by association with the inadequate scientific theories in which they occur? His view seems to be that (*e.g.*) set theory is *indispensable* for good science. It will crop-up in *any* well-confirmed scientific theory, but not in any inadequate theory. Since it will inevitably appear in any two (seriously) competing theories, no observations will favor (*e.g.*) the denial of set theory over set theory. Also, notice that *even if* there *were* observational evidence that weighed against set theory, its simplicity or some other "super-empirical virtue" might trump it. van Frassen would be puzzled by this. For him, observations are all that counts. And, for vF, evidence can confirm one part of a theory without confirming the entire theory (*e.g.*, the observational part could be confirmed by experimental evidence, while the mathematical part, and also possibly the part about unobservable entities like electrons, is not). See the bottom of the last page of this handout for a "conceptual map" of the views of Gödel, Quine, and vF.

1.2 Hunter on the Downward LST and Set-Theoretic Concepts

There are various first-order formal systems for set theory. That is, there are first order formal systems, which consist of $QS^{=}$, plus proper axioms involving the binary relations $\in$, $\cup$, $\cap$, and $\subseteq$ (and the constant $\varnothing$) whose intended interpretation involves *sets*. Such theories allow us to capture as derivations *many* (intuitively) valid set-theoretic arguments. One of the most widely used (and powerful) first order set-theories is called ZFC. It is unknown whether ZFC is consistent. And, ZFC (indeed, any interesting formal system for set theory) will fail to capture *some* intuitively valid set-theoretic arguments (like theories of the natural numbers, set theories are *incomplete if they are consistent*). But, ZFC does allow us to prove many important theorems of set theory (*e.g.*, Cantor's theorem that there are more sets than integers). As such, there will be theorems of ZFC which, on their standard interpretation, entail the existence of *uncountably many* sets. Let U be such a statement. The *downward* LST ensures that there will be a model M'_{ZFC}, whose domain is the set of closed terms of ZFC, in which U will be true. Thus, U will be true in some *countable* model. But, intuitively, U asserts the existence of *uncountably many* sets. *This* is usually what is called "Skolem's Paradox". Hunter does a nice job of clarifying the "paradox":

> . . . if ZFC has a model, there is a model of ZFC in which U does *not* say that there are uncountable sets or even that there are uncountably many things of any sort . . . if ZFC has a model, then one normal model of ZFC will make U speak not of sets at all, but of closed terms of ZFC. Since in that model U will be true, and there are only countably many closed terms of ZFC, U will *not* imply *in that model* "There are uncountably many closed terms": rather it will express some truth about the closed terms of ZFC. And if there is a model of ZFC, with a countable domain, in which U *does* speak about sets, then *still in that model* U will *not* imply "There are uncountably many sets of such and such a sort". . .

Hunter's remarks sound a bit like some remarks Skolem himself made about the "paradox". This brings us to Alexander George's paper, which is about Skolem's response to his "paradox". But, first, a few remarks.

Remarks. If our goal is to provide a formal explication of some set-theoretic concept like "the power set of the set of natural numbers", which unambiguously captures all of its intuitive properties (including its intuitive uncountability), then it seems that we're going to need more than the "narrowly logical structure" (in Quine's words) of any first-order theory (including ZFC). We could always move to a higher-order logical framework in order to try to more fully explicate such concepts. But, there are potential costs to such a move. As before, there are "logical" costs, in that second-order logic is incomplete, (fully) undecidable, *etc.*

Quine thought there were other costs as well. He thought that we don't have any way of understanding second-order logic that doesn't already presuppose an understanding of set theory itself. Indeed, Quine characterized second-order logic as "set theory in sheep's clothing." He thought that the only way to understand quantifying over predicates is as quantifying over *sets of individuals*. And, this presupposes an understanding of the concept of "set of individuals", which (on this proposal) is one of the things that we were supposed to be using second-order logic to explicate in the first place. As such, he thought it would be circular to use second-order logic to provide a foundation for set theory (and, hence for mathematics). On the other hand, Quine thought that we have a clear understanding of first-order logic that is independent of set theory, which is why (in addition to its various logical virtues) he preferred first-order logic to second-order logic as a foundational framework for mathematics. Not everyone agrees with Quine. Boolos, Resnik, Shapiro, and other "structuralist" philosophers of mathematics have tried to find a way to use second-order logic as a non-circular, adequate foundation for mathematics. Quine rejects this, and in doing so he gives up various (apparent) advantages of second-order logic, like *categoricity* of the theory of the natural numbers.

This still leaves Quine with an apparent problem. The LST seems to establish an ambiguity (lack of categoricity) that sounds like it should be unwelcome to a structuralist. It seems to show that we can "collapse" or "reduce" the domain of set theory down to a denumerable domain (one which might as well be $\mathbb{N}$), and hence that we have somehow lost our grip on the ontology of set theory. Quine argues that the LST is not strong enough to undergird such a collapse or reduction, since it does not provide an explicit construction of a (1–1) *proxy function* from the intended ontology to the *ersatz* ontology. Proxy functions have to tell us, for each object d in the intended domain (the intended ontology), which object d' in the *ersatz* domain is to "go proxy for" for d. And, the LST cannot provide any such thing — it only guarantees the existence of an *equivalent* model, *not* an *isomorphic* one. And, even when the *ersatz* domain has the same cardinality as the intended domain, the proof of the LST (or Henkin's theorem) does not allow us to *explicitly construct* a proxy function from the domain of M_T into the domain of M'_T. So, for Quine, *isomorphism with a proxy function construction* is required for such ontological reductions, and this is not provided by the LST. Various authors have argued that this is an *ad hoc* maneuver for Quine. And, it has also been shown that we often *can* provide such proxy functions by means other than the LST. But, Quine does have another line of attack here. *Even if* there is a proxy function construction in a particular case, the *ersatz* model may be more unwieldy than the intended model, and so "ideological" considerations of "simplicity", "explanatory power", and the like might still serve to (pragmatically) favor one ontological scheme/interpretation over another.

In the end, however, Quine became more and more skeptical about ontology and reference. Eventually, Quine came to think that reference and ontology were (ultimately) *inscrutable*. He ended-up concluding that there was a deep kind of (idealogical?) *relativity* in our ontological commitments. That is, that (in a sense) there is no "ideology independent" fact of the matter about what the terms in our theories refer to (*e.g.*, for all we know, we *only* ever refer to sets!). This is clearly a move away from scientific realism, toward some kind of anti-realism. Here's one last telling quote from Quine's later writings, which makes the comparison with van Fraassen even more interesting:

> Reference and ontology recede thus to the status of mere auxiliaries. True sentences, observational and theoretical, are the alpha and omega of the scientific enterprise. They are related by structure, and objects figure as mere nodes of the structure. What particular objects there may be is indifferent to the truth of observation sentences, indifferent to the support they lend to the theoretical sentences, indifferent to the success of the theory in its predictions.

Hilary Putnam makes explicit use of model-theoretic arguments to try to establish an anti-realist view of his own. We won't have time to discuss Putnam's argument, but we have discussed many of the issues it raises. What's interesting for our purposes is that Quine and Putnam both seem to have been led to various negative conclusions about ontology and reference by considerations that arise in the metatheory for first-order logic (this is especially clear from Quine's "ontological reduction" and "ontological relativity" papers). That's what makes this a good example of results from metalogic having some impact or implications in 20th century analytic philosophy. Quine's (and Putnam's) later "ontological relativity" views are somewhat similar to the relativistic views of Skolem (and Tarski). That brings us back to the George paper, which is our last topic. But, first, a remark about *infinitude*.

It is worth noting how crucial *infinitude* is for the phenomena surrounding "Skolem's Paradox". Quine:

> Once the size [of the universe of discourse] is both finite and specified... ontological considerations lose all force; for we can then reduce all quantifications to conjunctions and alternations and so retain no recognizably referential apparatus

Maybe it is the fact that the mathematical (and metalogical!) theories in question here imply the existence of infinitely many things that is generating the "paradoxes". Perhaps it is *infinity* and not reference that is inscrutable. Many paradoxes involve infinity (in some way or other). So, this is not a crazy idea.

2 George, Jané, and Skolem's *Relativistic* Response to the Paradox

Alexander George's and Ignacio Jané's papers (both available on the course website) are historical studies of Skolem's own thinking about the "paradox". Skolem didn't think of it as a paradox at all. Rather, he thought it indicated a *relativity* of certain mathematical concepts. For instance, Skolem thought that various set-theoretic concepts such as "the intersection of all sets having some property ϕ", may be different in different models of ZFC. Here are three quotes illustrating Skolem's relativism:

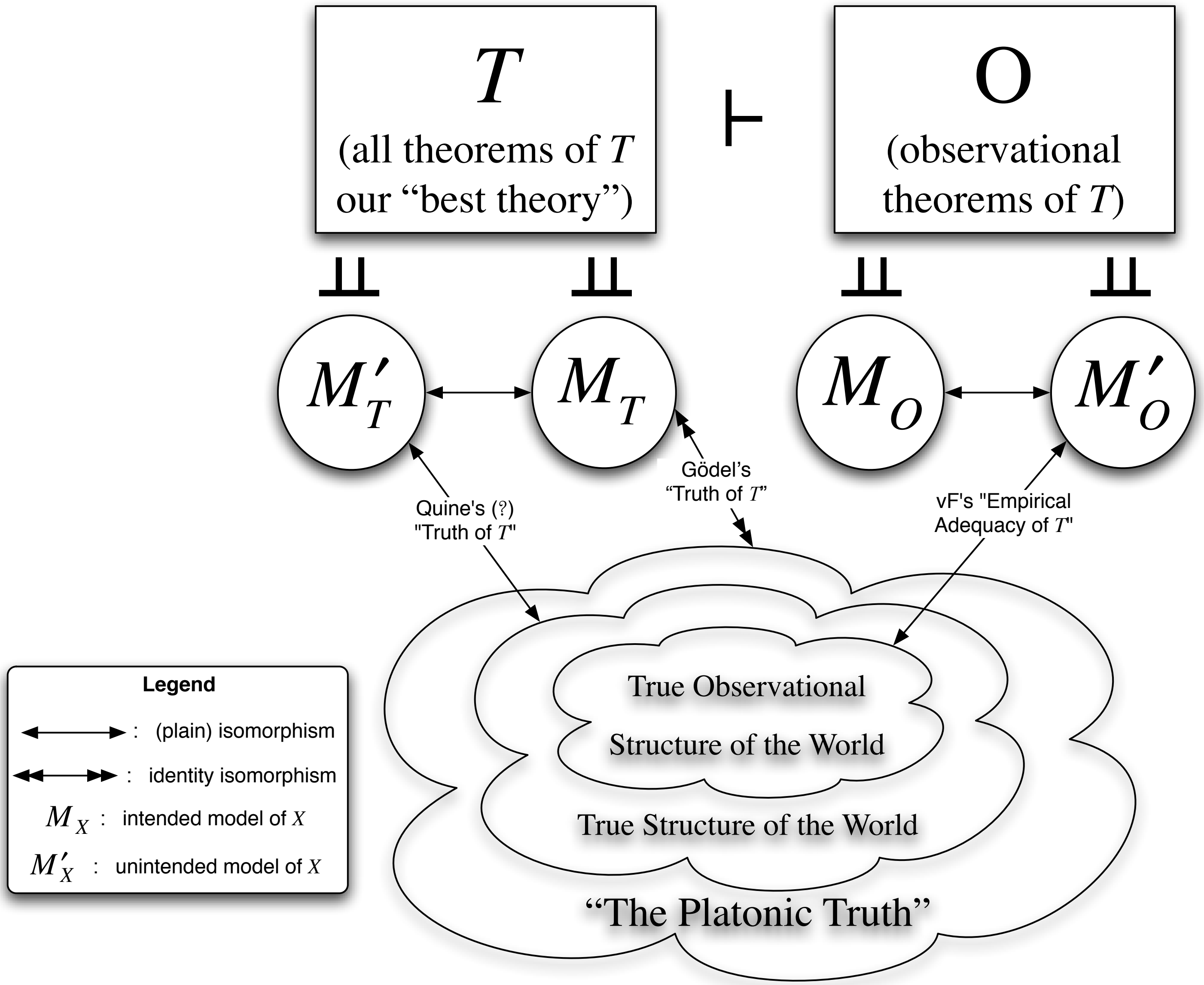
> ...[such a] definition [of $\subseteq$] cannot ... be conceived as having an absolute meaning, because the notion subset in the case of infinite sets can only be asserted to exist in a relative sense.
>
> A very probable consequence of relativism is again that it may not be possible to characterize completely the mathematical concepts; this already holds for the concept of integer. Thus the question arises whether the usual idea of the unicity or categoricity of mathematics is not an illusion.
>
> The fact that axiomatization leads to relativism is sometimes considered the weak point of the axiomatic method. But without reason. Analysis of mathematical thought, determination of fundamental hypotheses, and modes of reasoning, are nothing but an advantage for the science in question. It is not a weakness of a scientific method that it cannot do the impossible.

Pushing this relativism of set-theoretic concepts a bit farther – to *entire logical frameworks* – Tarski says

> The Löwenheim–Skolem theorem itself is not true but for a certain particular interpretation of the symbols. In particular, if we interpret the symbol '$\in$' of a formalized theory of sets as a dyadic predicate analogue to any other predicate, then the Löwenheim–Skolem Theorem can be applied. But if instead we treat '$\in$' like the logical symbols (quantifiers, etc.), and we interpret it as meaning membership, we will not have, in general, a denumerable model.

Finally, coming back to the role of infinity in this context, we should note that all responses to "Skolem's paradox" presuppose that uncountable sets (sets larger than the integers) exist. But, the existence of such sets can only be established *in set theory* (or, as Skolem might have said, *relative to* the standard axiomatizations of set theory). But, *why* should we accept the existence of uncountable sets in the first place? Skolem's response to the *upward* LST arguments of Tarski (and Benacerraf) is telling in this connection:

> I may mention that some authors in connection with the Löwenheim theorem [here, he has in mind Tarski, but Benacerraf makes an explicit argument along these lines in his paper "Skolem and the Skeptic", references on next page] also set forth the inverse theorem that if a logical formula can be satisfied in a denumerable domain, it can also be satisfied in a non-denumerable one, even with an arbitrary cardinal number. What is meant by such a statement? What kind of set theory is used? Is Cantor's set theory still going strong in spite of the antinomies? Or are the text books in ordinary analysis the source of the knowledge that non-denumerable sets exist? Or is the word non-denumerable only meant in the relative sense, namely relative to a certain axiom system of set theory?



Recommended Readings (most of these are on the course website)

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