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MATHEMATICAL EXISTENCE

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Despite some discomfort with this grandly philosophical topic, I do in fact hope to address a venerable pair of philosophical chestnuts: mathematical truth and existence. My plan is to set out three possible stands on these issues, for an exercise in compare and contrast.[1] A word of warning, though, to philosophical purists (and perhaps of comfort to more mathematical readers): I will explore these philosophical positions with an eye to their interconnections with some concrete issues of set theoretic method.

Let me begin with a brief look at what to count as 'philosophy'. To some extent, this is a matter of usage, and mathematicians sometimes classify as 'philosophical' any considerations other than outright proofs.[2] So, for example, discussions of the propriety of particular mathematical methods would fall under this heading: should we prefer analytic or synthetic approaches in geometry?[3] Should elliptic functions be treated in terms of explicit representations (as in Weierstrass) or geometrically (as in Riemann)?[4] Should we allow impredicative definitions?[5] Should we restrict ourselves to a logic without bivalence or the law of the excluded middle?[6] Also included in this category would be the trains of thought that shaped our central concepts:

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[1]I do not mean to suggest these are the only possibilities; my goal is to identify some landmarks.

[2]There is a hint of this in Gödel [1964], when he describes the undecidability of the continuum hypothesis as 'a precise formulation of the ... conjecture ... that the difficulties of the problem are probably not purely mathematical' (p. 259). Of course, this was no off-hand remark; Gödel had highly developed philosophical views (see, e.g., van Atten and Kennedy [2003]).

[3]See, for example, the 19th century debate in which supporters of synthetic methods hoped 'to free geometry from the hieroglyphics of analysis' (Carnot) and the 'clatter of the coordinate mill' (Study) (Kline [1972], p. 835).

[4]See Tappenden [200?] for discussion.

[5]As predicativists do not (e.g., see Feferman [1988]).

[6]As various constructivists propose (e.g., see Bridges [2003]).

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should a function always be defined by a formula?[7] Should a group be required to have an inverse for every element?[8] Should ideal divisors be defined contextually or explicitly, treated computationally or abstractly?[9] In addition, there are more general questions concerning mathematical values, aims and goals: Should we strive for powerful theories or low-risk theories?[10] How much stress should be placed on the fact or promise of physical applications?[11] How important are interconnections between the various branches of mathematics?[12] These philosophical questions of method naturally include several peculiar to set theory: should set theorists focus their efforts on drawing consequences for areas of interest to mathematicians outside mathematical logic?[13] Should exploration of the standard axioms of ZFC be preferred to the exploration and exploitation of new axioms?[14] How should axioms for set theory be chosen? What would a solution to the Continuum Problem look like?

Philosophical questions like these might be called 'methodological', in contrast with questions more properly characterized as 'metaphysical': for example, are our mathematical theories true?, do mathematical objects exist?[15] These ramify: what is the nature of mathematical truth? (Is it logical? Analytic? Is it like the truth of scientific statements?); do mathematical things exist subjectively or objectively, necessarily or contingently?, and so on. Considerations of this metaphysical variety often turn up in the course of methodological discussions: for example, when the Axiom of Choice is defended on the grounds that sets exist objectively, or when impredicative definitions are criticized on the grounds that sets are created by our definitions.

With this rough distinction between methodological and metaphysical philosophy in mind, let us now turn to our main topic. Serious questions of

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[7]For an overview of this debate, with references, see [1997], pp. 116–128.

[8]Stillwell ([2002], p. 367) observes that early definitions only required cancellation laws (e.g., Cayley in 1854). With the move to include infinite groups, the postulation of inverses became explicit (e.g., Dyke in 1883).

[9]See Avigad [200?] for an overview of the controversy and extensive references. Dedekind's set theoretic ideals have carried the day, though Edwards (e.g., in [1992]) argues that there are still advantages to defining 'what it means for a divisor to divide something, without defining what the divisor is' (p. 18), as in Kummer's and later Kronecker's treatments.

[10]The debate between the detractors and defenders of infinitary mathematics might be viewed in this light, e.g., the conflicts between Kronecker and Cantor (see Dauben [1979]).

[11]See, e.g., Kline [1968] or [1980], chapter XIII.

[12]See, e.g., Friedman [2000], Steel [2000], p. 433, Maddy [2000], pp. 419–420.

[13]See references in footnote 12.

[14]See Shelah [1993].

[15]In [1997], I reserved the term 'philosophy' for what I here call metaphysical questions; the methodological questions of the previous paragraph I there classified as part of mathematics proper, leaving no room for questions that are both mathematical and philosophical. This non-standard choice of terminology led to a range of avoidable misunderstandings.

mathematical existence first arose in mathematics itself with the introduction of negative and complex numbers: Kline reports that 'opposition ... was expressed throughout the [18th] century', and the controversy continued into the first half of the 19th ([1972], pp. 592, 596).[16] Opinion began to shift with Gauss's introduction of geometric interpretations in 1831. By identifying a complex number with a point in the plane, Gauss claims the

> intuitive meaning of complex numbers [is] completely established and more is not needed to admit these quantities into the domain of arithmetic. (Quoted in Kline [1972], pp. 631–2)

Kline continues:

> He also says that if the units 1, -1 and $\sqrt{-1}$ had not been given the names positive, negative, and imaginary units but were called direct, inverse, and lateral, people would not have gotten the impression that there was some dark mystery in these numbers. (Kline [1972], pp. 632)

Unfortunately, the dependability of geometry as arbiter of existence was soon undercut by increasing awareness of the logical shortcomings of Euclid's arguments[17] and the rise of non-Euclidean geometries.[18] Against this backdrop, efforts to found analysis turned to arithmetization instead.[19] This line of development led to the set theoretic notion of function, to real numbers understood as sets, and ultimately to our contemporary orthodoxy: to show that there are so-and-sos is to prove 'So-and-sos exist' from the axioms of set theory.[20] So existence in set theory seems a reasonable place to begin an inquiry into mathematical existence in general.

[16]E.g., in 1759, Masère writes that negative roots 'serve only, as far as I am able to judge, to puzzle the whole doctrine of equations, and to render obscure and mysterious things that are in their own nature exceedingly plain and simple'. As late as 1831, DeMorgan wrote 'the imaginary expression $\sqrt{-a}$ and the negative expression $-b$ have this resemblance, that either of them occurring as the solution of a problem indicates some inconsistency or absurdity. As far as real meaning is concerned, both are equally imaginary, since $0 - a$ is as inconceivable as $\sqrt{-a}$'. Both are quoted, with references, in (Kline [1972], pp. 592–3).

[17]See Kline [1972], pp. 1005–7.

[18]As early as 1799, Gauss believed that the parallel postulate could not be proved, that alternative geometries were logically consistent, and by 1817, he was no longer convinced that space itself is Euclidean: 'We must place geometry not in the same class with arithmetic, which is purely a priori, but with mechanics' (quoted in Kline [1972], p. 872). By 1854, Gauss's student, Riemann, was among the first to suggest that the geometry of space might depend on the distribution of mass (Kline [1972], pp. 893–4).

[19]See Kline [1972], pp. 947–8.

[20]Of course, this is only the 'contemporary orthodoxy' for mainstream classical mathematics, my subject in this paper; various schools of constructivism, for example, would disagree. Furthermore, set theoretic reduction does not confer certainty, nor does it show that the mathematical items provided with set theoretic surrogates 'were really sets all along'. (For a more complete discussion of the senses in which set theory is and is not a foundation for classical

Set theory is often regarded as an essentially 'realistic' or 'platonistic' theory, as if a certain metaphysics is straightforwardly presupposed in its axioms and theorems. So, for example, Bernays ([1934]) sees a brand of platonism as implicit in the combinatorial notion of set,[21] the axioms of infinity and choice, and the use of the law of the excluded middle and impredicative definitions. More recently, Feferman writes that in set theory

> Sets are conceived to be objects having an existence independent of human thoughts and constructions. Though abstract, they are supposed to be part of an external, objective reality. (Feferman [1987], p. 44)

This underlying platonism 'reveals itself most obviously in such principles as the Axiom of Extensionality and the Axiom of Choice' (op. cit.). As noted above, defenses of Choice and impredicative definitions do often appeal to metaphysical pictures of this kind.

The most famous position of this type is the one familiar from Gödel:

> For someone who considers mathematical objects to exist independently of our constructions and of our having an intuition of them individually ... there exists, I believe, a satisfactory foundation of Cantor's set theory ... (Gödel [1964], p. 258)

He goes on to describe what we now call the iterative hierarchy of sets. If this conception of set is

> accepted as sound, it follows that the set-theoretical concepts and theorems describe some well-determined reality ... (Gödel [1964], p. 260)

From this platonistic perspective, Gödel gains his objective, that is, a defense of the continuum problem: in this reality

> Cantor's conjecture must be either true or false. Hence its undecidability from the axioms being assumed today can only mean that these axioms do not contain a complete description of that reality. (Gödel [1964], p. 260)

The prescription, then, is a search for new axioms.

As is well-known, the metaphysical portion of such platonistic views has come under serious philosophical attack: in particular, it seems that these abstract, eternal, objective things fall outside the range of our human cognitive powers. Some have hoped to improve the prospects for a viable theory

mathematics, see [1997], pp. 22–35.) Those sympathetic to the idea that mathmematical objects like numbers and functions have an existence (or not!) quite independent of their set theoretic surrogates should regard this paper as a discussion of truth and existence in set theory.

[21] On the combinatorial picture, the existence, e.g., of a set of integers does not depend on there being a definition or rule picking out its elements; rather, it is viewed as the result of 'an infinity of independent determinations' (Bernays [1934], p. 260) of membership or non-membership for each integer.

of set theoretic knowledge by insisting that the relevant subject matter is not a far-away world of mathematical things, but the concept of set. The axioms of set theory, on this view, are not descriptions of a set theoretic reality, but explications of this concept; set theoretic statements are true if they are part of concept (or follow from it). Obviously, views of this sort vary dramatically depending on the notion of concept involved, but if the resulting set theory is to be objective, if the truth or falsity of its statements is to be independent of our thought, of our ways of knowing, then it seems the concept of set must itself be objective.[22] So, though eliminating abstract objects also eliminates the problem of how we can know about them, this realistic version of conceptualism faces the equally baffling problem of how we know about objective concepts. Let me lump these positions together as Robust versions of Realism.[23]

Advocates of Robust Realism are often drawn to it by the promise of a determinate truth value for independent statements like the Continuum Hypothesis (CH), and familiar forms of object realism, like Gödel's, do take the universe of sets to be entirely precise. Still, we should note the possibility of a robust reality that isn't fully determinate. So, for example, a concept realist's concept of set might exist objectively, independently of our inquiries, and so on, without deciding every particular; it might be vague or indefinite in places. Likewise, there are forms of object realism that allow abstract objects, like sets, to be indeterminate in various respects.[24] The defining feature of Robust Realism is that it sees set theory as the study of some objective, independent reality; that reality might dictate that CH is true, that CH is false, or that there is no fact of the matter about CH.

Now there may be ways around the fundamental epistemological problem for Robust Realism—I once tried to find one myself[25]—but here I

[22]It seems that both Frege and Gödel thought of concepts in this way. (See Frege [1884], section 47, Gödel [1944], p. 128.) Martin [2005] examines Gödel's conceptual realism in some detail.

[23]Another version of Robust Realism is the view that set theory is the study of an objectively existing set theoretic structure, e.g., Shapiro's 'ante rem structuralism' (Shapiro [1997]).

[24]Akiba [2000] defends such a view; see also Field [1998], p. 398. As an example, Field suggests versions of structuralism that take a mathematical object to be a position in a structure, with no properties other than those conferred by its being that position; Akiba also compares his position with this variety of structuralism. Both writers focus on numbers, which may be neither identical to nor distinct from, e.g., the von Neumann ordinals, but Parsons deals directly with the possibility of sets as 'incomplete objects' (see his [1990], [1995], and [2004]). He allows that structures can have non-isomorphic instances ([2004], p. 69), so perhaps sets (as positions in the set theoretic structure) are not determinate enough to settle CH. For a hint of this, see Parsons [1995], p. 79: 'looking at the structuralist view itself, one might ask whether it concedes to set theory the degree of objectivity that many set theorists are themselves inclined to claim, following the example of Gödel in "What is Cantor's Continuum Problem?"'.

[25]In [1990]. Shapiro makes analogous efforts on behalf of his Robust structuralism in [1997]. For discussion of the differences and similarities, see [1990], pp. 170–177.

want to focus instead on a challenge to the position that arises on the methodological end of our philosophical spectrum. Consider, for illustration, one of the central maxims of set theoretic practice, the admonition to maximize: the idea is that if set theory is to serve as arbiter of mathematical existence, it should be as generous as possible, so as not to constrain the free pursuit of pure mathematics. To see how this plays out, suppose we consider a natural extension T of ZFC; suppose, in addition, that there is another natural extension T' in which T can be naturally interpreted (and not vice versa); and finally, suppose that T' gives us interesting new structures not available from T; then we should regard T as restrictive and avoid it. So, for example, ZFC + Gödel's Axiom of Constructibility ($V = L$) is restrictive because it can be naturally interpreted in ZFC + Large Cardinals (LCs)[26] and not vice versa, and ZFC + LCs gives us $0^{\#}$ (which can't exist in L). In contrast, though set theory with the anti-foundation axiom AFA (that is, (ZFC-Foundation) + AFA) can naturally interpret ZFC,[27] it can also show that any structure realized in non-well-founded sets is isomorphic to some structure already present among the well-founded sets. So AFA, unlike LCs, delivers no new structures, and ZFC (with Foundation) need not be regarded as restrictive (at least not on these grounds).[28]

We have here an argument against adding $V = L$ to the list of standard axioms.[29] In the terminology adopted here, it is not a purely mathematical argument, because it is not a proof; rather, it is a philosophical argument of the methodological variety. A Robust Realist adds some further philosophizing, of the metaphysical variety: he holds that there is an objective world of sets (or an objective concept of set or an objective set theoretic structure or ...) and that our set theoretic statements aim to assert truths about this world; in particular, the axioms of our theory of sets should be true in this objective world, and (given that logic is truth-preserving) our theorems will be, too. So, from a Robust Realist's point of view, an argument against adding $V = L$ to the list of standard axioms must be an argument that $V = L$ is false in the world of sets.

That is the trouble. The *maximize*-based argument shows why adding $V = L$ leads to a theory of the sort we dislike, a restrictive theory, but how does its being a theory we dislike provide compelling evidence that it is false? To put the problem more generally: granting that *maximize* generates theories we like, what reason do we have to think it likely to generate theories

[26]Via the relativization '$x \in L$'.

[27]Via the relativization 'x is well-founded'.

[28]See [1997], pp. 206–234, and Steel [2000], p. 423, [2004], p. 3, for discussions of *maximize*. The differences between my version and Steel's are largely due to my not entirely successful effort to spell out what counts as a 'natural' extension of ZFC and a 'natural' interpretation.

[29]I am concerned here not with the details or even the ultimate viability of this particular argument, but with the style of argument that it exemplifies.

that are true?[30] As any casual observer of the scientific process knows, the physical world has all too often failed to conform to scientists' preferences;[31] it would seem that this is one of the characteristic hazards of any attempt to describe an objective, independent reality. So it seems the metaphysics of a Robust Realism undercut a methodological philosophy central to the practice of contemporary set theory:[32] until he has reason to believe that maximize tends to produce true theories—and it is devilishly hard to see how that claim could be defended—the Robust Realist must look elsewhere for evidence against $V = L$.[33]

Some, no doubt, will feel that the *maximize*-based argument against $V = L$ is stronger than the case for Robust Realism. The argument from maximize is refreshingly concrete for a piece of philosophy: it begins from a simple, reasonable goal of set theoretic practice[34]—that of providing an ontological basis for classical mathematics—, works out what kinds of features a theory of sets should have in order to serve this goal, and concludes that theories of a certain type should be rejected. The various defenses of versions of Robust Realism are another matter, involving more distant metaphysical flights into philosophy proper.[35] For those inclined to trace the fault in this conflict to the metaphysics of Robust Realism rather than the methodology of *maximize*, I suggest that a weaker brand of realism is available.

To get a sense of how this might go, recall the structure of debate and resolution in a well-known historical case that involved an explicit commingling of metaphysical and methodological considerations, namely, the case of the Axiom of Choice.[36] On the one hand, metaphysical elements tinge the famous debate between Hadamard and the French School of Baire, Borel, and Lebesgue—do sets exist objectively, independently of us, or do they exist only insofar as they have been defined or constructed by us?—in other words, there was a conflict between something like Robust Realism and a version

[30]I heard this objection to *maximize* raised explicitly by a prominent set theorist I suspect of being a concept realist.

[31]Tracing the history of the wave theory of light, Einstein and Infeld ([1938], p. 117) ask whether light waves are longitudinal (like sound waves) or transverse (like water waves): 'Before solving this problem let us try to decide which answer should be preferred. Obviously, we should be fortunate if light waves were longitudinal. The difficulties in designing a mechanical ether would be much simpler in this case. ... But nature cares very little for our limitations. Was nature, in this case, merciful to the physicists attempting to understand all events from a mechanical point of view?' Of course the answer was no. 'This is very sad!', Einstein and Infeld conclude (p. 119).

[32]Cf. [1997], pp. 131–2.

[33]E.g., to confirmed predictions of hypotheses inconsistent with it. See Martin [1998], pp. 223–5.

[34]One among many, of course, but the one operative here.

[35]As in Gödel [1964] or my [1990].

[36]Obviously, I cannot do justice to this episode in a paragraph. For more, see [1997], pp. 54–57, 123–126, or the definitive source, Moore [1982].

of Idealism. On the other hand, there were widespread appeals, beginning with Zermelo, to the growing body of important mathematics that required the Axiom, in analysis, topology, abstract algebra and mathematical logic, as well as set theory proper. We all know how the story ends: the Axiom of Choice is now among the standard assumptions of mathematics. Given that the general metaphysical conflict between Realism and Idealism remains unresolved,[37] it seems reasonable to conclude that the debate over Choice was decided by a simple bit of methodological philosophy: adopting this axiom gives us theories that help us meet our mathematical goals.

Now we might react to this historical reality as the Robust Realist did to *maximize*, that is, we might resist popular opinion and insist that the metaphysical issues must be settled before the methodological decisions can be justified. But there is room here for a different point of view. Perhaps the moral to be drawn from these stories is that decisions about mathematical method are properly settled on mathematical terms, by what we have called methodological philosophy—perhaps metaphysical philosophy is irrelevant.[38] On this view, the characteristic methods of set theory are properly justified by its characteristic methodological philosophies, understood in light of its characteristic mathematical values and goals: if one of the aims of set theory is to provide an ontological arena for classical mathematics, then it is perfectly rational to pursue theories that *maximize*; if adopting the Axiom of Choice is an effective means towards many set theoretic and mathematical goals, then we are perfectly justified in doing so.

The alternative realism I have in mind starts from this idea: the choice of methods for set theory is properly adjudicated within set theory itself, in light of their effectiveness as means to its various goals; the axioms and theorems of set theory, as generated by those methods, are taken to be true, including the existential claims, so sets exist. The metaphysically-minded will ask: what are these sets like? The answer, on this view, is that the methods of set theory tell us what sets are like, and indeed, set theory tells us an impressive story of what sets there are, what properties they have, how they are related, and so on. Still, our metaphysician wants information of a different sort about these sets: do they exist objectively or are they mental constructions or fictions? Are they part of the spatiotemporal, causal universe? Is their existence contingent on something or other, or do they exist necessarily? And so on. In other words, he wants a metaphysical account as well, not just a methodological one.

[37] New forms of definabilism (e.g., Chihara [1990]) and constructivism (e.g., Kitcher [1983]) allow for the Axiom of Choice, as do recent fictionalist accounts of set theory (e.g., Balaguer [1998]). Notice, by the way, that this undercuts Feferman's contention that set theory requires Robust Realism. I come back to this point below.

[38] This is a central theme of [1997].

Here our new realist must tread lightly, because set theory itself is silent on these topics, and set theory, on this account, is supposed to tell us all there is to know. There may be some temptation to dismiss the metaphysician's questions as unscientific pseudo-questions—there is precedent for this move, after all[39]—but whatever its pros and cons in general, I think this is not an option here. Even if they are irrelevant to the justification of methodological decisions in the practice of mathematics, there remain perfectly legitimate and challenging scientific questions about the nature of human mathematical activity: how does mathematical language function?, does it relate to the world in the same ways as the language of natural science?, what happens when human beings come to understand mathematical theories?, how does mathematics work in various kinds of applications?, and so on. In the course of addressing these questions, we will be forced to face many of the metaphysician's concerns: do mathematical entities exist, and if so, what is the nature of that existence? Are mathematical claims true, and if so, how do humans come to know this? These are not detached, extra-scientific psuedo-questions, but straightforward components of our scientific study of human mathematical activity, itself part of our scientific investigation of the world around us, to be approached using the methods of linguistics, psychology, physics, and any other application of scientific method that turns out to be relevant.[40]

To see how our new realist's answers to these metaphysical questions might go, let me turn to the comments of two contemporary thinkers whose positions seem to me to exemplify the weaker style of realism I am after here. Consider first the set theorist John Steel, who writes:

> To my mind, Realism in set theory is simply the doctrine that there are sets... Virtually everything mathematicians say professionally implies there are sets.... As a philosophical framework, Realism is right but not all that interesting. (FOM posting 1/15/98, quoted with permission)
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> 'there are sets' ... is not very intriguing. 'There are sets' is, by itself, a pretty weak assertion! Realism asserts that there are sets, and hence... that 'there are sets' is true. (FOM posting 1/30/98, quoted with permission)

Here the existence of sets, a version of realism, is being deduced from what mathematicians say 'professionally', that is, using the methods and principles of mathematics in general and set theory in particular. Steel admits that:

[39]E.g., see Carnap [1950].

[40]It may seem odd to suggest that metaphysical questions be addressed scientifically, but this is a defining feature of naturalistic philosophy. See [200?] for a survey of Quine's original version and two post-Quinean variants (John Burgess's and my own).

> Both proponents and opponents sometimes try to present it as something more intriguing than it is, say by speaking of an 'objective world of sets'. (FOM posting 1/15/98, quoted with permission)

Steel remarks that 'such rhetoric adds more heat than light', and he seems uncertain that his 'not very interesting' Realism is Robust:

> whether this is Gödelian naïve realism I don't know. (FOM posting 1/30/98, quoted with permission)

I suggest that it is not, and it may be that Steel himself has more recently come to agree:

> The 'official' Cabal philosophy has been dubbed *consciously naïve realism*. This was an appropriate attitude when the founding fathers were first laying down the new large cardinals/determinacy theory ... It may be useful now to attempt a more sophisticated realism, one accompanied by some self-conscious, metamathematical considerations related to meaning and evidence in mathematics. (Steel [2004], p. 2)

Perhaps the defense of *maximize* counts as such a self-conscious metamathematical consideration: we talk about what sorts of theories are to be preferred and why.

Now what about the metaphysician's questions? Steel addresses only one of them directly:

> ... sets do not depend causally on us (or anything else, for that matter). Virtually everything mathematicians say professionally implies there are sets, and *none of it is about their causal relations to anything*. (FOM posting 1/15/98, italics added, quoted with permission)

Set theory, again, tells us all there is to know about sets, and it says nothing about their being causally related to anything, so they are not. Addressing mass rather than causation, John Burgess (our second exemplar) extends this way of thinking to include what natural scientists say and do not say about sets:

> I think the fact that ... sets ... don't have mass can be inferred from the fact that when seeking to solve the 'missing mass' problem in cosmology, physicists may speculate that neutrinos have mass, but never make such speculations about ... sets. (personal communication 4/24/02, quoted with permission)

Similarly, set theory does not tell us that sets are located in space, or that they have beginnings or endings in time, or that they are involved in causal interactions, and natural scientists do not seek them out or appeal to them as causal agents in explanations, so we should conclude that they are not spatiotemporal or causally active or passive. Summing up, Burgess writes:

> One can justify classifying mathematical objects as having all the negative properties that philosophers describe in a mis-leadingly positive-sounding way when they say that they are abstract. ... But beyond this negative fact, and the positive things asserted by set theory, I don't think there is anything more that can be or needs to be said about 'what sets are like'. (personal communication, 4/14/02, quoted with permission)

In sum, sets are taken to have the properties ascribed to them by set theory and to lack the properties set theory and natural science ignore as irrelevant. There is nothing more to be said about them. In philosophical circles, such posits are sometimes called 'thin', so let us call this Thin Realism.[41]

Given that the mathematical things of Thin Realism are non-spatiotemporal and acausal, it might seem to share the Achilles heel of Robust Realism, that is, it might seem to impede a reasonable account of how human beings come to know mathematical facts. This is where 'thinness' does its work: sets just are the sort of things that can be known about by careful application of the methods of set theory. To see how we come to know what we do about sets—assuming we are not concerned about the logical connections at the moment—we need only examine how various mathematical and set theoretic considerations led us to accept the axioms, a study, Burgess notes, that is 'given in the standard histories'.[42] So, for example, the arguments touched on above in favor of *maximize* and the Axiom of Choice are just the right sort of thing for defending methods and axioms of set theory.[43] The Robust Realist's conviction that some further justification is needed must strike the Thin Realist as akin to the radical skeptic's challenge to the natural scientist: yes, your claims meet the most stringent of scientific standards, but are they *really* justified? The Thin Realist is unmoved.[44]

Though it escapes the familiar difficulties, Thin Realism also fails to deliver the metaphysical pay-offs that put many Robust Realists on this path in the first place: for example, it cannot play the role of Robust Realism in the debate over independent statements like CH, in particular, in assessing whether or not CH has what the Robust Realist calls 'a determinate truth

[41]The term 'Thin Realism' is reminiscent of Azzouni's 'thin posits' ([1994]), but certain peculiarities of his position (e.g., that 'there are x's' can be true without there being x's, see his [2004]) make it difficult to compare it with those under consideration here.

[42]Burgess is writing here with Rosen, in their [1997], pp. 45–6.

[43]It is sometimes suggested that the Thin Realist's success at turning away the difficulties of Robust Realism depends on his embrace of a disquotational theory of truth. (Indeed, Burgess (with Rosen in their [1997], p. 33) takes naturalism to include a disquotational theory; I disagree, see [2001], §III.) It seems to me that this isn't correct—that a correspondence theorist can be a Thin Realist and that a Robust Realist can be a disquotationalist—but it would take us too far afield to go into this here.

[44]This is not to say that the Thin Realist's reasons for being unmoved are precisely the same as those of the natural scientist. I come back to this point below.

value'. The Thin Realist is perfectly willing to assert 'CH or not-CH', or 'CH is either true or false', or 'Either CH is true or not-CH is true'—as these are all straightforward assertions of set theory—but as we have seen, the Robust Realist wants something more than an appeal to classical logic. Thus he posits an objective reality that the ordinary methods of set theory, including classical logic, do (or do not) allow us to track.

In stark contrast, the Thin Realist sees no such gap between the methods of set theory and sets: sets just are the sort of thing that can be known about in these ways; the Robust Realist is wrong to interpose some mysterious extra layer of justification between the two. Various considerations lead the Robust Realist to his concern over determinacy: the independence of CH, the existence of assorted models of set theory, the inability of large cardinal axioms to settle CH, the lack of intuitive force behind the strong axiom candidates currently available, and so on. These same considerations may lead the Thin Realist to fear that we will never come up with an acceptable theory that decides the question, but this possibility does not change the fact that CH is either true or false.[45]

The possibility of this Thin Realism suggests that set theory is not, in and of itself, committed to Robust Realism. Contrary to the claims of Feferman and others,[46] the methods and axioms of set theory—like *maximize* and the Axiom of Choice—can be justified internally, in terms of set theory's goals and values; they do not require appeal to

> an external, objective reality ... [of] independently existing entities [in which] statements such as the Continuum Hypothesis have a definite truth value (Feferman [1987], pp. 44–5)

The Thin Realist will hold, in Burgess's negative way, that sets are not created by our thoughts or definitions, that they are acausal and non-spatiotemporal, but he will regard the Robust Realist's further worry over whether or not CH has a determinate truth value as misguided. CH is either true or false because our best theory of sets includes 'CH or not-CH'; there is no more to it than that. Our coming to know which will depend on whether or not there will one day be a mathematically well-motivated way of settling it—perhaps an extension of ZFC, perhaps something else, along the lines of Woodin's

[45]Speaking of apparent indeterminacy in vague contexts, Hartry Field accuses epistemic theorists—those who hold that Borderline Joe is either bald or not-bald, we just cannot tell which—of failing to distinguish between 'factual but unknown' and 'non-factual' (see Field [2000], p. 284). For the epistemicist, everything is factual. The same might be said of the Thin Realist with regard to set theory.

[46]These 'others' seem not to include Bernays, who distinguishes between 'platonistically inspired' methods of set theory, which he calls a 'restricted platonism', and an 'absolute' or 'extreme platonism', which interprets the methods of set theory 'in the sense of conceptual realism, postulating the existence of a world of ideal objects containing all the objects and relations of mathematics' ([1934], pp. 259, 261).

recent efforts[47]—and that question remains open. But set theory tells us what there is to know about sets, and it reveals no further problem.

With the Robust Realist and the Thin Realist on stage, let me now introduce the third and final player in our little drama. This figure begins as a hard-nosed natural scientist, confident that the methods of science are the best ways she has of finding out about world, about what there is and how we know it. When she turns her scientific investigation on the practice of science itself, to understand and improve those methods as she goes, she quickly notices that mathematics plays a central role and that its methods differ from those of her natural science. She sets out to examine and evaluate mathematical methods on their own terms, to investigate how and why the resulting theories are so useful in her study of the world, and to understand the nature of mathematics as a human practice.

The question is: what will the results of these studies tell her about the existence of mathematical objects? She, like the Thin Realist, sees that existence assertions appear in the theory of sets and that they are appropriate there, in light of the relevant values and goals, but she also holds that natural science, not mathematics, is the final arbiter of what there is. Some have argued that the role of mathematical entities in our best scientific theories is enough to establish their existence: the idea is that the confirmation of a scientific theory accrues to all the entities it embraces, abstract and concrete alike.[48] Upon careful examination of the practice of science, our inquirer disagrees; it seems to her that this is not how scientific theorizing does or should work. For example, despite the explanatory and predictive successes of atomic theory, despite its providing the best account of a wide range of chemical and physical phenomena, the existence of atoms was still open to legitimate doubt until it was subjected to more specific tests by Jean Perrin and others in the early 20th century. In fact, the attitudes of scientists toward their best theories are complex and nuanced: some posits are regarded as useful fictions, some aspects are suspected to be artifacts of the modeling, and explicit idealizations are freely employed.

Suppose our inquirer concludes, on careful examination, that the mathematical entities present in our best scientific theories are not among the posits of those theories whose existence has been established. Let us also suppose that her best linguistic account of the semantics of mathematical language, her best psychological account of human mathematical experience, her best explanation of the so-called 'miracle' of applied mathematics—that none

[47]Woodin's case against CH takes the following form: an extension of ZFC like this would be mathematically desirable, it is possible to get an extension of ZFC like this, and furthermore, every such extension of ZFC makes CH false. For discussion, see Woodin [2001].

[48]The key figure here is (of course) Quine. See [200?] for a discussion of Quine's argument, the reply sketched in the text, and references for both.

of these studies or any other studies of the nature of the human practice of mathematics turns out to involve the existence of mathematical things (outside of explicit idealizations and modeling, that is). If natural science is the final arbiter of what there is, it seems she must conclude that mathematical things do not exist, that pure mathematics is not in the business of discovering truths. Let us call this position Arealism.[49]

So what is set theory doing, according to our Arealist, if it is not advancing our store of truths, not telling us what sets there are and what they are like? Here philosophers seem occupationally predisposed to analogies: mathematics is like a game,[50] or mathematics is like fictional story-telling,[51] or mathematical language is like metaphorical language.[52] (Even the Robust Realist often appeals to an analogy between mathematics and natural science.[53]) Let us imagine that our Arealist instead undertakes to characterize mathematics directly, as itself; instead of trying to understand mathematics by analogy with something more familiar, she tries to make mathematics itself more familiar. The pursuit of set theory, then, is a process of devising and elaborating a theory of sets, prompted by certain problems (recall its beginnings in Cantor's analysis of sets of singularities), guided by certain values (power, consistency, depth), in pursuit of certain goals (a foundation for classical mathematics, a complete theory of reals and sets of reals, and so on).[54] This particular mathematical process of theory formation is directed by these internal mathematical problems, norms and goals just as other processes of mathematical concept formation are directed by their own constellation of considerations, for example, the development of the concept of function or group or topological space.[55] With care, these processes can be described and analyzed, and their underlying rationality assessed.

This Arealism recalls some versions of Formalism, in that both deny that mathematics is in the business of seeking truths about certain abstracta. But the dissimilarities are at least as dramatic; let us note just a few. Arealism does not limit its attention to formalized theories: the natural language discussions of goals and methods that surround ZFC are fully part of set theoretic practice, and even proofs from ZFC and its extensions are rarely formalized. In addition, the Arealist is obviously far from judging, as some Formalists do, that any consistent axiom system is as good as any other: one

[49] I avoid the more usual term 'Anti-realism' because it is most often used for positions that rest on principled, often a priori, objections to Realism. My Arealist is not against mathematical entities any more than she is against unicorns; she just has no evidence for the existence of either.

[50] See Shapiro [2000], pp. 144–8, for discussion.

[51] See Burgess [2004] for criticism of the fictionalist analogy.

[52] See Burgess and Rosen [200?] for criticism and references.

[53] E.g., in Gödel [1944], [1964], or my [1990].

[54] For more, see [1997].

[55] See [2001a].

of the central aims of her methodological study of set theory is to understand and assess arguments for and against axiom candidates. Finally, the Arealist may well agree that some strictly finitary statements—like $2 + 2 = 4$—are different in kind from those of infinitary mathematics, perhaps indeed that they are true,[56] but she does not view its ability to generate true statements of this 'contentful mathematics' as grounding the full weight and worth of 'ideal mathematics'.[57] Pure mathematics has a wide range of uses, from facilitating various internal mathematical goals to providing methods and structures for applications outside mathematics—uses which the Arealistic methodologist is keen to investigate and understand.

A moment ago, we tried to illuminate Thin Realism from the right by contrasting it with Robust Realism: the 'thinness' of its mathematical entities serves to undermine any challenge to the effectiveness of set theoretic methods for securing knowledge of sets; the price paid is the loss of any Robust claims about 'determinate truth values' for independent statements. Let us now examine Thin Realism from the left, in contrast this time with Arealism. At first blush, the difference here is more stark than with Robust Realism – the Thin Realist takes set theoretic theorems to be true and sets to exist; the Arealist does not—but in fact it seems to me that the actual differences between Thin Realism and Arealism are considerably less significant than those separating Thin Realism from Robust Realism.

To see this, let us start with a simple case: the justification of the Axiom of Replacement. History and mathematical considerations suggest that it was adopted as a fundamental axiom for a number of reasons:[58] it is needed to prove the existence of cardinal numbers like $\aleph_\omega$ that were present in informal set theory; it squares with limitation-of-size, one of the traditional guidelines for avoiding paradox;[59] it implies a number of important theorems that make set theory more tractable (every set is equinumerous with an ordinal,[60] transfinite recursion, Borel determinacy); and so on.[61] We saw that the Robust Realist must explain why a principle with all these welcome features is also likely to be true; the Thin Realist dismisses this worry by characterizing sets as entities we can learn about by the exercise of set theoretic methods like these. But how will the Thin Realist differ from the Arealist? Each will tell the same story on Replacement, citing the same

[56]E.g., $2 + 2 = 4$ might be understood along the realistic lines of rudimentary logic in [2002].

[57]See Detlefsen [1986], especially, pp. 3–24, for this instrumentalist reading of Hilbert's Formalism.

[58]Replacement is no different from the other standard axioms in this respect; similar lists can be given for each of them. See [1997], pp. 36–62.

[59]See, e.g., Fraenkel, Bar-Hillel and Levy [1973], pp. 32, 50.

[60]Assuming the Axiom of Choice, of course.

[61]See Mathias [2001] for more on the poverty of set theory without Replacement. (I am grateful to Kanamori for the reference.)

turns of history, the same mathematical consequences, the same set theoretic problems, norms and goals. Only at this point, once all relevant theorems and facts of methodology are in place, does the question of truth arise: the Thin Realist counts the facts on the table as justifying a belief in the truth of Replacement, while the Arealist views them simply as good reasons to include Replacement among the axioms of our theory of sets.

What exactly is added in moving from 'this is a good axiom for reasons x, y, z' to 'this is a good axiom for reasons x, y, z, and therefore likely to be true'? Setting aside formal truth predicates, which are equally available to the Thin Realist and the Arealist, the word 'true' turns up in various contexts in the practice of set theory: 'Cantor's theorem is true'; 'So-and-so's conjecture turned out to be true, much to my surprise'; 'I think the axiom of measurable cardinals is true'; 'CH does (or does not) have a truth value'. Different set theorists might attach different cognitive content to these claims, depending on their metaphysical leanings (platonistic, formalistic, or whatever), but I submit that for all such statements involving truth—by inspecting the role they play in practice, what underwrites their assertion, what is taken to follow from them, and so on—we can easily isolate what they come to for the practice of set theory, a sort of methodological core or 'cash value' if you will, that is available to the Arealist as well as the Thin Realist. I have in mind such simple readings as: 'such-and-such is provable from appropriate axioms'; 'such-and-such is a good axiom for reasons x, y, z'; 'I think we will (or will not) find compelling mathematical reasons for adopting a theory in which such-and-such turns out to be provable or disprovable'. On this much the Arealist and the Thin Realist will completely agree; the difference only comes in the way the word 'true' is then applied: the Thin Realist will find in all this good evidence for truth, while the Arealist takes talk of 'truth' as inessential and sticks to the methodological facts unadorned. As far as the practice of set theory is concerned, it is hard to see what is lost on the Arealist's approach.

It is possible, of course, that reasons to prefer the Thin Realist's position, with truth and existence, to the Arealist's austerity will turn up in our scientist's extra-mathematical study of the human practice of mathematics—perhaps in the role of mathematics in science, perhaps in our explanation of how mathematics is applied, perhaps in our semantic account of mathematical language, perhaps in the psychology of mathematical experience—but, as hinted a moment ago, this seems to me unlikely. These topics present difficult questions, but I do not see how our stance on whether the objects of pure mathematics exist in the Thin Realist's sense or are just part of a good theory in the Arealist's sense, or whether the theorems of mathematics are true in the Thin Realist's sense or merely good mathematics in the Arealist's sense, will help or hinder our efforts to meet the challenges they present.

Supposing this is right, what then does the disagreement over truth and existence come to? Do the notions of existence and truth, clearly at home in the context of natural science,[62] also belong in other areas of human activity, and is pure mathematics one of these? We feel confident that there is a right and a wrong answer here, but the problem bears a disquieting resemblance to an old example of Mark Wilson's.[63] He tells the story of a tribe of natives inhabiting an isolated island in the South Pacific. These natives have never seen an airplane until one crashes in their jungle one day. Wilson entertains two scenarios. In the first, the natives see the plane as it descends and crashes and declare that a great silver bird has fallen from the sky. In the second, they come upon the crashed plane with the flight crew living out of the fuselage and declare that a great silver house has appeared in the jungle. So, is the plane a bird or a house? Each group of natives will firmly defend its usage, but we can see that there is no pre-determined fact of the matter to which the decision is answerable, that the natives' strong convictions, one way or the other, are due to mere historical contingency.

Now imagine our Arealist beginning with her fundamental use of 'true' and 'exists' in science.[64] She recognizes an array of extra-scientific human activities, from pure mathematics to astrology, and she also realizes that pure mathematics, because of its role in science, must be investigated in different terms from those used for astrology. (For example, she must explain why mathematics works as it does in science, which will require more than an account in terms of human history and psychology that might be enough for astrology.) But, we are supposing, her investigations do not lead her to think that an extension of the terms 'true' and 'exists' is appropriate. Now imagine that the Thin Realist begins from a somewhat different perspective, this time from a contemplation of the full range of human endeavor with the terms 'true' and 'exists' liberally sprinkled throughout. She quickly sees good grounds on which to remove these terms from many places, like astrology, but they seem unobjectionable in their mathematical occurrences. So there they remain.

To give these imaginings a bit more substance, let us return to the Arealist's starting point: the perspective of the hard-nosed scientist. As I described her, she was a natural scientist: mathematics is not itself a science, though its role as a handmaiden to science means it requires a more complex treatment than astrology. But what if this handmaiden role instead prompted us to assume that mathematics *is* a science, alongside the various natural sciences? Then the methods of science—in particular, the methods of mathematical science—would lead us to assert the existence of sets and the truth of set theoretic claims. Unless we had reason to doubt these mathematical methods, we would be led to a version of Realism, and the Thin Realist, as we saw, denies there is room for such doubts, maintaining that sets just are the

[62] I do not mean to suggest that truth in science is without its complexities and p zles

[63] See Wilson [1982].

[64] See footnote 62.

sort of thing that can be known about in these ways. So an inquirer like our Arealist, but who begins with a more generous notion of 'science', might well end up a Thin Realist.

Is there anything that compels us to choose one or the other of these two starting points? Again, there are obvious considerations on both sides: the Arealist is struck by the stark differences between the methods of natural science and the methods of mathematics; for the Thin Realist, the gulf between these two subjects and the range of other human undertakings, like astrology, is more salient. The Arealist might point to another divide between natural science and the Thin Realist's mathematics: whatever the natural scientist may say to the radical skeptic, it is not that his challenge is incoherent, but given the Thin Realist's denial of any gap at all between set theoretic methods and sets, the demand for further justification in this case is actually ill-formed. In other words, that sets can be known about in these ways is part of what sets are; the reliability of these methods is, as we might put it, conceptual. The Arealist will see this as yet another reason to deny that mathematics is a science—science is not conceptual, it is empirical!—but the Thin Realist replies with complete composure that some sciences are and some sciences are not.

If this is a fair description of the situation, then it seems there is no fact of the matter here about which we can be right or wrong, just as nothing in the natives' usage before they saw the airplane pre-determined what they should call it. The decision between Thin Realism and Arealism comes down to matters of convenience, taste, and preference in the bestowing of these honorific terms (true, exists, science).[65] Each course has its advantages and disadvantages. The awkwardness of Arealism is easy to spot: though the existence claims of set theory are rational and appropriate moves in the development of set theory, sets do not in fact exist; the theorems of set theory are not true. Burgess would disparage this as 'taking ... back ... in one's philosophical moments what one says in one's scientific moments' ([2004], p. 19), but it seems to me that a better description would be taking back in one's scientific moments what one says in one's mathematical moments. It would be more comfortable, admittedly, never to take anything back, but doing so for scientific reasons is perhaps less disconcerting than doing so for philosophical ones.[66]

The awkwardness of Thin Realism, on the other hand, is more diffuse: in various areas, its ontology of abstracta allows uncomfortable questions to

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[65] Perhaps this helps explain why mathematicians are often reluctant to take the question seriously.

[66] 'Science' here includes naturalistic philosophy that is conducted by natural scientific methods; 'philosophy' means extra-scientific or 'first philosophy'. As far as I can tell, 'philosophy' in Burgess's remark also means first philosophy, but his 'science' includes mathematics, despite the methodological divide between natural science and mathematics. See [200?] for discussion.

be asked. In fact, I think these questions can probably all be answered by calling on the thinness of the entities involved. We already discussed the most prominent—how do we come to know about sets?—which was answered by a declaration of thinness: sets just are the kind of thing that can be known about by set theoretic methods. 'How do we come to refer to sets?' can be treated analogously: sets just are the kind of thing that can be referred to by customary use of set theoretic language. Questions about the nature of set theoretic existence were to be answered negatively—not causal, not spatiotemporal—but here we might worry that it is not always obvious which of a pair of opposing properties is the negative one. For example, do sets exist necessarily or contingently? Presumably the answer should be that they exist necessarily, that existence in all possible worlds, despite appearances, is a negative feature because it comes to a denial that the existence in question is contingent on anything—but one might rather avoid this topic altogether. Or, to take a new example, are we referring to the same sets as Zermelo did? As Cantor did? Our methods of establishing their existence are different, as are some of the problems we are trying to solve, some of our goals, and so on. Does this matter? Again, there are probably ways of answering these questions, but a point of view from which they do not arise in the first place has its obvious attractions.

This delicate balance between the pros and cons of Thin Realism and Arealism, combined with the sense that there is no fact of the matter to which the choice must conform, seems to leave room for legitimate indecision at this point in the development of set theory. Still, John Steel has meditated repeatedly on a speculative scenario that he fears might threaten the very tenability of Thin Realism if set theory were to develop as he imagines it. How does this story go?

Notice first that the smooth working of the Thin Realist's link between existence and provability in set theory depends on the fact that there is, so far, a single best theory—ZFC plus large cardinals—that set theorists are out to explore and extend. If set theory were to fragment somehow into a range of theories, the Thin Realist's treatment of 'existence' and 'truth' would be considerably more problematic. Of course, the set theoretic aspiration to serve as arbiter of mathematical existence motivates not only *maximize*, but also the admonition to *unify*, that is, to provide a single all-inclusive theory.[67] So far, in cases that might seem to suggest a bifurcation—say between $V = L$ and Large Cardinals—there has been a clear choice, because one can have one's large cardinals while preserving all the benefits of $V = L$ (as the theory of L). If this were not possible, if two appealing set theories could not be combined in this way, with one subsuming the other, the natural reaction would be to search for a third, stronger theory that could subsume them both. Steel's worry involves a scenario that might foil these familiar

'For discussion, see 99 pp. 208

moves, a scenario that grows out of his *maximize*-based hope for settling CH.[68]

The story begins with the success of the theory 'ZFC + a proper class of Woodin cardinals' as an account of second order arithmetic. The merits of this theory are well-known, so I will not rehearse them,[69] except to note evidence that it settles all natural questions about projective sets: it implies that the $L(R)$'s in any two set-generic extensions of the universe are elementary equivalent, so no sentence of second order arithmetic can be shown to be independent of the theory by forcing. As Steel writes:

> it is natural to ask whether this kind of completeness extends to the language of third order arithmetic ... (Steel [2000], p. 430)

the level where CH turns up. Large cardinals cannot do this by themselves, but

> one can hope for an axiom/hypothesis A such that (1) A is compatible with all large cardinal axioms [and] (2) granted sufficiently large cardinals in V, any two set-generic extensions of V satisfying A have elementarily equivalent $L(P(R))$'s. (FOM posting 1/30/98, quoted with permission)

This would be the analogous completeness result for third order arithmetic, but conditional on A. Steel continues:

> To my mind, the best hope for a solution to CH in my lifetime is the discovery of conditional generic absoluteness theorems extending ... to all levels of the Σ^n_m hierarchy and beyond. (FOM posting 5/18/03, quoted with permission)

that is to say, through n-th order arithmetic, for all n.

The trouble is:

> There may be another axiom B with all the properties of A, but incompatible—maybe even deciding CH differently. (FOM posting 1/30/98, quoted with permission)

There might even be a series of conditional generic absoluteness theorems starting from B, as from A, extending to all orders of arithmetic. These theories would be 'generically intertranslatable', that is,

> The A-believer can think of B as the theory of a certain sort of generic extension of his universe, and the B-believer can think of A similarly. (FOM posting 1/30/98, quoted with permission)

[68]Steel discusses these issues in several places: Steel [2000], [2004], and FOM postings on 1/15/98, 1/30/98, 1/25/00, and 5/18/03. For the record: Steel's program depends on the falsity of the so-called Omega Conjecture; Woodin's program (see footnote 47 above) depends on its truth. Obviously, I am in no position to endorse one or the other here! My aim is only to consider the consequences of Steel's scenario for Thin Realism.

[69]See Steel [2000], pp. 425–430, for a summary.

Here the natural impulse to look for a more extensive theory that fairly interprets both A and B is ineffective, because we already have two: both the A theory and the B theory are universal in this sense. What becomes of the Thin Realist's 'truth' and 'existence' on this scenario?

Steel suggests in passing that this question might be addressed by appeal to the notion of meaning.[70,71] Without going into details, let me say that it seems to me unlikely that any scientific notion of meaning, arising from linguistics and related disciplines, would ratify the sorts of synonymy claims this approach would need. But let me leave that aside, because I suspect that what Steel really wants is something simpler, with considerably less theoretical superstructure:

> Developing one sequence of theories would be the same as developing the other, so there would be no need to choose between them. (FOM posting 1/25/00, quoted with permission)

> Developing one would be the same activity as developing the other, so that there would be no significant behavioral difference between adopting one and adopting the other. (Steel [2004], p. 21)

Think of it this way. If I am pursuing A-theory, anything the B-theorist does is fine with me; I just regard it as pursuing the theory of some set-generic extension, $V[G]$. This runs parallel to the large cardinal theorist's attitude toward the $V = L$-theorist's work: it is quite welcome as an investigation of L. The difference in this case is that the B-theorist, unlike the $V = L$-theorist, can turn the tables: he can regard my work as an important and

[70]One possibility is that 'there is at some deep level an ambiguity in the language of set theory' (FOM posting 5/18/03): 'It may be that, in the end, our solution to the Continuum Problem is best seen as resolving some ambiguity' (Steel [2000], p. 432). Presumably, the idea is that the word 'set' in the A-theorist's mouth means something different than it does in the B-theorist's mouth, and that settling CH would come to deciding between these. If the A-meaning and the B-meaning are both consistent extensions of our current meaning, it is hard to see how choosing between two new meanings is any easier than, or indeed any different from simply choosing between the A-theory and the B-theory in the first place, and it seems Steel might agree: 'It is hard to distinguish sharpening the meaning of our language from discovering new truths' (Steel [2000], p. 432).

[71]Another possibility traces back to Steel's thought that the 'natural' interpretations of *maximize* 'preserve meaning' (Steel [2000], p. 423). B-theory is naturally interpretable in A-theory, as the theory of some $V[G]$, and vice versa; if these interpretations are meaning preserving: 'It might then be appropriate to regard the decision to adopt one of these theories as analogous to the decision to speak one of some family of intertranslatable languages' (Steel [2000], p. 432). In other words, being an A-theorist rather than a B-theorist would be no more significant that speaking English rather than French; the two theories say the same thing in different languages, so despite appearances, they in fact endorse the same truths and make the same existence claims. But it is hard to see how the A-theorist's '$V[G]$ thinks S' could be synonymous with the B-theorist's S, and beyond that, translation back and forth would make B-theorist's S synonymous with his '$V[H]$ thinks "$V[G]$ thinks S"'! Not to mention that the global translation map from one theory to the other doesn't seem to be onto, as one might expect if one theory as a whole is to be synonymous with the other.

penetrating analysis of some $V[H]$. Anything the B-theorist can do, the A-theorist can do in $V[G]$; anything the A theorist can do, the B-theorist can do in $V[H]$.

This means that any practicing set theorist working in A-theory can think of himself as an A-theorist or as a B-theorist working in $V[H]$. In Steel's terms, there is no behavioral difference between these options; in the terms used here, there is no methodological difference. And, of course, the situation is analogous for the set theorist working in B-theory. The Arealist would elaborate along these lines: there is no methodological cash value to the difference between A-theory and B-theory; we could think of ourselves as working in one or the other, but this would make no difference in how we actually go about our mathematizing; for official purposes, we would pick one or the other, perhaps more or less conventionally, so as to satisfy *unify*; in practice, we would allow ourselves to move back and forth at will.

From here, we can get a glimpse of why Steel claims that having even one such theory would constitute a solution to CH:

> Developing any one such theory would be the same as developing them all, so in a practical sense finding any such theory would solve the Continuum Problem. (FOM posting 1/15/98, quoted with permission)

> Finding any such sequence of theories would solve the Continuum Problem. (Steel [2004], p. 21)

The idea is that if we had one such theory, it would include all the others as theories of various set-generic extensions of V, so it would encompass them all in natural interpretations. The size of the continuum would be unimportant in the sense that it could have a range of values, each in the context of a set theory with all the *maximizing* virtues. We could pursue set theory in the context of the one theory we have without compromising any norms, goals, or values of set theory.

Or so the Arealist might say. The worry is that it is hard to see how the Thin Realist could imitate this line of thought: there would still be the matters of truth and existence to settle; if the word 'true' is to behave in any of its usual ways, we cannot say that A-theory and B-theory are both true. This, I suggest, is what pushes Steel to the thought that they are synonymous: if the apparent alternatives are really the same, if they all say precisely the same thing in different ways, then they are all true and they all make the same existence assertions. This type of move is familiar: the logical positivists once faced the problem of deciding between two physical theories with all the same empirical consequences; they attempted to solve it by adopting a verificationist theory of meaning that makes the two synonymous. This account of meaning did not hold up in the long run and I suspect Steel's Thin Realist attempts to dissolve his problem with a theory of meaning would fare no better.[72] It is one thing to say that each of this batch of theories is perfectly acceptable for set theoretic purposes, that they are methodologically indistinguishable; it is quite another to try to claim that they are all really saying the same thing.

In fact, I think Steel's concern is less troublesome than he imagines, because I retain the conviction that the difference between Thin Realism and Arealism is essentially cosmetic. If this is right, the Thin Realist should be able to imitate in his way of thinking any move available to the Arealist, and vice versa. Let us look more closely, then, at the Arealist's reaction when confronted with *A*-theory and *B*-theory. Motivated by his desire to *unify*, he will first search for reasons to prefer one theory to the other, perhaps in differences arising beyond *n*-th order number theory, perhaps in other sorts of methodological considerations, perhaps in their degree of conformity with various intuitive ideas about 'the concept of set'. In practice, there would almost certainly be some consideration, however small, that would serve to tip the balance one way or the other, even if it ultimately came down to such matters as familiarity or historical contingency. Once the tie was broken, the strong and well-justified injunction to *unify* would kick in, producing a single official theory of sets.

The methods employed here by the Arealist are easily understood as rational, given the relevant set theoretic goals, and Thin Realism, remember, takes set theoretic methods to be authoritative, that is, he takes sets to be the sort of thing that can be known about by set theoretic methods. It would seem, then, that the Thin Realist could proceed exactly as the Arealist does, and then unblushingly add that the results of these proceeding are reliable, that either *A*-theory or *B*-theory, whichever wins out, is true. By the Thin Realist's lights, this is simply how knowledge of set theoretic truth is achieved.

Let me sum up. I have described three possible positions on the existence of sets and the truth of set theoretic claims: Robust Realism, Thin Realism and Arealism. I have suggested that Robust Realism raises questions for set theoretic knowledge that seem inappropriate from a point of view internal to set theory: a theory or method could have all the mathematical virtues we look for and more, all the methodological evidence might be in and favorable, but the question of truth remain open, to be settled, if at all, by some other means. Thin Realism and Arealism avoid this problem by investing the decisive authority with set theory itself, and the choice between them, I suggest, is not constrained by any facts, mathematical or scientific. Perhaps considerations from the theory of truth might favor either Thin Realism or Arealism, but that is a topic for another day.

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ee previous footnote.

RE RENCE

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