Hamkins on the Multiverse

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> Universal skepticism, though logically irrefutable, is practically barren; it can only, therefore, give a certain hesitancy to our beliefs, and cannot be used to substitute other beliefs for them.
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> *Our Knowledge of the External World* (1914)
> BERTRAND RUSSELL

Ever since the rise of non-standard models and the proliferation of the independence results there have been two conflicting positions in the foundations of mathematics. The first position—which we shall call *pluralism*—maintains that certain statements of mathematics do not have determinate truth-values. On this view, although it is admitted that there are are *practical* reasons that one might give in favour of one set of axioms over another—say, that the one set of axioms is more useful than the other, with respect to a given task—, there are no *theoretical* reasons that can be given since, on this view, we are dealing with statements that lack determinate truth-values. The second position—which we shall call *non-pluralism*—maintains that statements of mathematics do have determinate truth-values. On this view, the matter of selecting one set of axioms over another, incompatible set of axioms is more than one of mere practical expedience—it is a substantive matter, one where the there is hope of giving theoretical reasons for one over the other since, on this view, the statements in question do have determinate truth-values.[1]

[1] I say that there is a *hope of* finding theoretical reasons and not that there *are* theoretical reasons since I want to leave it open—at least as far as the *characterization* of the position is concerned—for the non-pluralist to admit that there are *absolutely undecidable* statements, that is, statements for which one cannot give any (convincing) theoretical reasons.

This description of the pluralist/non-pluralist divide is really just a first-approximation. For it makes it look as though one must either be a pluralist with regard to *all* of mathematics or a non-pluralist with regard to all of mathematics. But, in fact, many people are non-pluralists with regard to certain branches of mathematics and pluralists with regard to others. For example, a very popular view in the foundations of mathematics embraces non-pluralism for first-order number theory while defending pluralism for the higher reaches of set theory. Most people would, for example, maintain that the Riemann Hypothesis (which is equivalent to a Π^0_1-statement of arithmetic) has a determinate truth-value and yet there are many who maintain that CH (a statement of set theory, indeed, of third-order arithmetic) does not have a determinate truth-value. Feferman is an example of someone who holds this position. So instead of two positions—pluralism versus non-pluralism—we really have a hierarchy of positions.

This hierarchy of positions is usefully described by organizing the space of all possible mathematical theories under the relation of interpretability. The resulting hierarchy is known at the *interpretability hierarchy.* At the bottom of this hierarchy one has week systems such as Q and as one moves upward one passes through stronger and stronger systems like PRA, PA, ATR_0, PA_2, PA_3, ZFC and ZFC supplemented with large cardinal axioms. For any given level of the interpretability hierarchy one can maintain non-pluralism below that level and pluralism beyond. For example, the *strict finitist* draws the line at Q, the *finitist* draws the line at PRA, and the *predicativist* draws the line at ATR_0.

The two extremes of this hierarchy of positions are *radical pluralism*—which upholds pluralism across the board, except, of course, for logic (which, for the purposes of this discussion, is assumed to be classical)—and *radical non-pluralism*, which maintains that all of the statements of mathematics—including higher set theory—have determinate truth-values. The first extreme is embraced by *if-thenists* and the second extreme was embraced by Gödel.

Hamkins embraces the first extreme—he's a radical pluralist.[2] His view can be briefly characterized as *if-thenism driven by Skolemism.* Skolemism starts with the mathematical fact that every consistent theory has a count-

[2]Once we have set up the machinery to articulate his position I will qualify this: In the terminology to be explained below, it is unclear whether Hamkins is a *radical pluralist* or a *relative pluralist* (where these are the forms of pluralism associated with the radical broad multiverse and the relative broad multiverse).

able model and argues for the philosophical conclusion that all such models are on a par from the point of view of mathematical truth. Arguments of this form have convinced some that certain statements of mathematics do not have determinate truth-values. For example, Skolem (1923) was led to the conclusion that statements attributing uncountablity do not have determinate truth-values and von Neumann (1925) was led to suspect that statements attributing finiteness did not have determinate truth-values. Once the independence phenomenon enters the picture the way is open for a more radical conclusion to be drawn. For it provides us with a radical plurality of theories. Hamkins is driven by such considerations—especially those arising from the model-theoretic constructions in set theory—into embracing a radical form of pluralism, one that is a form of if-thenism. If-thenism maintains that non-pluralism holds for logic alone. In place of straightforward mathematical assertions of the form φ, where φ is a theorem, the if-thenist—thinking that such statements do not have determinate truth-values when standing on their own—falls back on statements of the form "if A then φ", where A is a finite conjunction of axioms that logically implies φ. In short, a refined version of Skolemism leads Hamkins into embracing if-thenism.

Hamkins does not formulate his view proof-theoretically as an if-thenist would; instead he formulates it model-theoretically. He does this by developing a *multiverse* conception of set theory. On this view there is not a single *universe* of set theory but rather a *multiverse* of legitimate candidates, some of which may be preferable to others for certain practical purposes but none of which can be said to be the "true" universe. The *multiverse conception of truth* is the view that a statement of set theory can only be said to have determinate truth-values if it has the same truth-value in all universes of the multiverse; if it is evaluated as true in all universes of the multiverse then it is true simpliciter; if it evaluated as false in all universes of the multiverse it is false simpliciter; and if it is evaluate as true in some but false in other universes of the multiverse then it is indeterminate according to the multiverse conception of truth. How radical such a view is depends on the breadth of the conception of the multiverse.

There is, of course, a close correspondence between the pluralism/non-pluralism divide and multiverse conceptions—for each way of drawing the non-pluralist/pluralist line in set theory there is a corresponding multiverse conception of set theory and conversely. At one extreme there is the *broad multiverse* consisting of all models of consistent formal systems in the language of set theory. This corresponds to radical pluralism. At the other extreme there is the *narrow multiverse* consisting of a single universe, the universe of sets V. This corresponds to radical non-pluralism. In between these two extremes there are many intermediate conceptions: One can narrow the broad multiverse along one dimension by placing model-theoretic demands on the universes of the multiverse; for example, one can restrict to ω-models or β-models or models with stronger closure conditions. One can also narrow the broad multiverse along another dimension by restricting to models that satisfy a certain theory, like ZFC, or ZFC along with certain large cardinal axioms, and so on.

It would probably be good for me to state my own position at the outset. Given our current state of knowledge I think that there are some tenable brands of pluralism. For example, I think that given the current state of our knowledge a case can be made for being a non-pluralist about ZFC and large cardinal axioms while being a pluralist about CH. This is the position that Woodin's *generic multiverse* conception aims to articulate and it's a position held by Steel.[3] There are also tenable multiverse conceptions tied to my work with Woodin on incompatible Ω-complete theories. It is not that I think that such conceptions are unproblematic. Indeed I think there are mathematical conjectures and scenarios that weight against them and, in general, I am inclined to be a non-pluralist about V and place the limitations on the side of epistemology by embracing (if the search for new axioms does reach a limit, for reasons that we can see) absolutely undecidable sentences.

But although I think that the above pluralist conceptions are tenable (while thinking there are some reasons against them) I do not think that the broad multiverse conception and the associated view of the radical pluralism is tenable. That is what I shall argue in this paper.

Here is an overview of the paper. Section 1 is devoted to a preliminary discussion of multiverse conceptions of set theory, focusing on the two extremes—the *broad* multiverse conception and the *narrow* multiverse (or the *universe* conception). I'll consider the two standard arguments for each conception and argue that each is too quick and has little force. Section 2 is devoted to a discussion of Hamkins' arguments for the broad multiverse conception. I will go through his case step by step, arguing that each step is problematic, and that when the dust settles we are left with no reasons to

[3]It was in work on this conception that the term "multiverse" first arose. In comments on an early draft of the paper I suggested the term "multiple universes" and then, drawing on the parallel with physics, Woodin settled on the term "multiverse".

embrace the broad multiverse conception of set theory.

1 The Broad Multiverse and the Narrow Universe

Let us consider the two extremes—the *broad multiverse* consisting of all models of all consistent formal systems in the language of set theory and the *narrow multiverse* consisting of a single universe, the universe of sets V.

There is a standard argument for each. The standard argument for the broad multiverse conception rests on the completeness theorem and the independence phenomenon, while the standard argument for the narrow multiverse conception rests on the categoricity results. In each case the mathematical facts are uncontested. The question is whether the mathematical facts warrent the philosophical conclusion. In each case, I will argue that they do not—in each case the argument has a circular aspect; one gets out as much as one puts in. To approach the question of pluralism in a productive manner a more subtle approach is needed.

1.1 The Broad Multiverse

1.1.1 The Radical versus the Relative Broad Multiverse

The reader will have noticed that the very formulation of the broad multiverse requires—at least if it is to yield a conception that is non-trivial—a certain amount of background mathematics. For, to escape non-triviality, one must be in a position to prove that the theories in question are consistent. For this reason we should really be speaking of the *broad multiverse conception relative to* T, where T is a given background theory. For example, if T is ZFC + Con(ZFC) then one can formulate a version of the broad multiverse that contains universes that satisfy ZFC.

This observation leads to a distinction between what I will call the *radical* broad multiverse conception and the *relative* broad multiverse conception. According to the *radical* broad multiverse conception there is no background theory T that is determinate and so we must apply the multiverse conception to any provisional background theory. We might use a theory T to provisionally set up the broad multiverse conception but the intention is that the multiverse conception applies to T as well. In the words of Russell (used in a different context)

> It is exactly of the same nature as the Hindu's view, that the world rested upon an elephant and the elephant rested upon a tortoise; and when they said, "How about the tortoise?" the Indian said, "Suppose we change the subject."

The advocate of the radical broad multiverse conception tells us instead that its turtles all the way down.

According to the *relative* broad multiverse conception there *are* determinate background theories T which we can hold fixed when articulating the broad multiverse conception. For example, one might maintain that ZFC and its open-ended sequence of extensions by iterated consistency statements are determinate; the advocate of the relative broad multiverse conception relative to these theories can then use any such element of the sequence, say ZFC + Con(ZFC), as a fixed, determinate, background theory in which to articulate the broad multiverse conception for a weaker theory, say ZFC.

1.1.2 The Problem of Articulation

Let us assess these views, starting with the latter. There is a *problem of articulation* that appears to arise for the relative broad multiverse conception. To fix ideas, suppose that the advocate of this conception holds that ZFC and its open-ended sequence of extensions by iterated consistency statements is determinate. Suppose we wish to articulate the broad multiverse conception for one of the theories in this sequence, say ZFC. Then we can pick a stronger theory, like ZFC + Con(ZFC), and, relative to that, articulate the broad multiverse conception where all of the models satisfy ZFC. The goal in doing this is to capture the idea that ZFC is determinate but statements like CH are not. And this approach works for ZFC and CH since ZFC is held fixed across the broad multiverse in question and CH is true in some of the universes of the multiverse and false in others. But it renders *too many* statements indeterminate. In addition to articlating the idea that CH is indeterminate it also articulates the idea that the Rosser sentence for ZFC is indeterminate. But the Rosser sentence for ZFC is also taken to be determinate since it follows from the determinate stock of theories used as background theories! So there is an inconsistency between the external claim that the stock of background theories is determinate and the internal claims of determinateness made when one formulates the broad multiverse relative to a theory from that stock.

One might try to work with the entire stock of background theories but this will not work since either (being open-ended) it is not recursively enumerable or it is recursively enumerable. In the first case, one cannot even get started; in the second case, one is lacking a sufficiently strong background theory to establish non-triviality. The relative broad multiverse conception is, therefore, highly problematic.

The radical broad multiverse conception also faces a problem of articulation but one that is more subtle. It certainly *looks* like it runs directly into the above problem since at any level the background theory will have the resources to see that the multiverse relative to that background theory misses its mark since it will prove a statement that the multiverse conception (relative to that background theory) deems indeterminate. But to this the advocate of the radical conception will respond that the background theory is really just a "provisional prop" that one uses to "partially articulate" the conception—since at bottom (or really, because there is no bottom) the multiverse conception applies all the way down. In other words, to partially convey his conception the advocate of the radical multiverse conception will fix a background theory, provisionally take it to be fixed and determinate, and then articulate the multiverse conception relative to a weaker theory. He will do the same for the background theory, relative to another, stronger background theory. And so on, up through stronger and stronger theories. But notice that at each stage there is only a partial articulation—it applies only to a given theory T—and at each stage one provisionally takes a non-pluralist stance concerning a *stronger* theory. Also, although these stronger theories are given a radical broad multiverse reading—against the backdrop of non-pluralism with regard to even stronger theories—at no stage are we given an account of the radical broad multiverse view for its intended range of application. It is unclear that the advocate of the radical broad multiverse can articulate the view without provisional props of a non-pluralist nature. This is what would be required for a clear articulation of the view. In other words, set theory would have to be replaced, wholesale by "multiverse theory", where it is clear of all background theories *at once* that one is upholding the broad multiverse view regarding them.[4]

[4]This problem applies because of the model-theoretic nature of the view. It does not apply to an "if-thenism" that accepts only first-order logical statements of the form "if T

1.1.3 The Quick Argument

Let us set aside the problem of articulation, assuming that it can somehow be overcome. What are the arguments for the broad multiverse conception?

There is a quick argument based on the completeness theorem and the independence phenomenon. As we saw above, if we select a given background theory, say ZFC + Con(ZFC) we can use the completeness theorem and the independence results to consider a host of models of ZFC which differ in the statements that they satisfy, for example, CH. The quick argument takes this alone to establish that such statements do not have a determinate truth-value.

Here are some examples:

(1) Assuming that ZFC is consistent we can construct ill-founded models of ZFC. These models satisfy "all sets are well-founded". Does that fact alone undermine the idea that statements asserting well-foundeness have determinate truth-values.

(2) Assuming that ZFC is consistent we can construct countable models of ZFC. These models satisfy "there are uncountable sets" and yet these models are countable. Does that fact alone undermine the idea that statements asserting uncountablity of a certain (described) object—say ω_1—have determiante truth-values?

(3) Assuming that PA is consistent we can construct models of PA + ¬(Con(PA). Does that fact alone undermine the idea that statements like Con(PA) have a determinate truth-value.

(4) Assuming that Q is consistent we can construct models of Q + ¬Exp. Does that fact alone undermine the idea that the statement Exp—asserting that 2^n exists for all n—has a determinate truth-value?

Some have gone down this path for a certain stretch; for example, Skolem and von Neuman took the first two steps, taking the results in (1) and (2) to show that there statements attributing uncountability and well-foundedness do not have determiante truth-values. Few have gone further. What then is the distinguishing feature? If the mere existence of the models is sufficient, then there is no reason not to go all of the way, that is, if the quick argument

then φ". One might try to regard the radical broad multiverse conception as the "model-theoretic" trace of this proof-theoretic view.

establishes anything then it establishes the radical broad multiverse conception. If one draws the line—say between (2) and (3)—then one must point to some feature *other* than the mere existence of such models, that is, one must supplement the quick argument.

It is clear that one must supplement the quick argument if one is to make a case for the relative broad multiverse conception—one must say what it is that allows one to draw the line between the cases where the existence of incompatible models is and the cases where it is not sufficient to establish indeterminateness.

But the quick argument must be supplemented even in the case of the radical broad multiverse conception. For even the staunchest realist can consistently grant that there are models of $\mathrm{Q}+\neg\mathrm{Con}(Q)$ while admiting $\mathrm{Con}(Q)$. (Indeed one cannot even be in a position to know that such models exist unless one knows $\mathrm{Con}(Q)$.) The quick argument is in need of supplementation. Moreover, it is hard to see how one might supplement it since it is hard to see from what priveledge vantage point (apart from an outright proof of inconsistency, which will not happen) one can doubt a simple statement like Exp.

The radical view is certainly consistent. The problem with if-thenism is not that it is inconsistent. The problem with it is that there is no more reason to believe it than there is to believe in universal skepticism and, as Russell says, universal skepticism, though logically irrefutable, is practically barren. Skepticism of a non-universal sort has its value—it adds a certain hesitancy to our beliefs. But in the end the right approach—I think—is to start in medias res, applying skepticism locally in a productive fashion, scrutinizing our beliefs not from some mythical Archimedean vantage point, but in the only way we can, from within. This is our default position and the advocate of the radical broad universe conception—just like the skeptic—has said nothing that will dislodge us from it.

1.2 Narrow Multiverse

Let me now turn to the quick argument for the narrow multiverse conception, the conception where the multiverse consists of one element, namely V. The argument traces back to Zermelo's quasi-categoricity result which shows that if one fixes the height of the universe by adding an anti-large cardinal axiom—such as "there are no inaccessible cardinals"—then the full second-order system consisting of ZFC and the anti-large cadinal axiom provides us with a categorical characterization of the universe of sets up to that level.

This argument has been nicely refined by Martin (2001).[5] On Martin's approach one starts with the informal notion of "the concept of set". This notion has two components, one concerning width—"absolute powerset"—and one concerning height—"absolute infinity". These are best brought out by looking at extensional structures that "meet" the concept of set. In order for an extensional structure to meet the concept of set it must be such that for every set X in that structure the powerset of X contains "absolutely all subsets of X" and it must be such that it contains "absolutely all ordinals". Suppose then that two structures M and N meet the concept of set. One can then build an isomorphism between M and N in step-wise fashion: Supposing that the isomorphism $\pi \restriction V_\alpha^M$ has been constructed we can extend it to the next level as follows: For $A \subseteq V_\alpha^M$ such that $A \in M$ we let $\pi(A) = \pi``A$. Since N meets absolute powerset this element is in N and since M meets absolute powerset this map is onto $V_{\alpha+1}^N$. We have thus extended the isomorphism to the next level. At limit levels we simply take the union. Finally, it is easy to see that M and N must have the same height since if one structure outran the other then the shorter one would fail to meet the concept of set because it would fail to meet the part concerning absolute infinity. In short, no multiverse consisting of a universe other than V is coherent since if the universes are to meet the concept of set we can amalgamate them to show that there must have only been one to start with.

This argument has a certain force. For it shows just how hard it is to provide a foundational framework in which one can articulate a non-trivial multiverse conception. Suppose that our conception takes the natural numbers as determinate and so all of the universes in the multiverse agree on V_ω. But suppose that they differ on $V_{\omega+1}$. In this situation there are universes M and N such that M has a subset of V_ω that N does not. Let A be such a set. We have the isomorphism up to V_ω—it's just the identity. So N is missing a set. Put otherwise: Upon reflecting on the multiverse and recalling that the intention in taking the powerset of V_ω was to include *all* subsets of V_ω one is led to the following response: "When I said that one takes the powerset of V_ω I intended to take all subsets of V_ω. But a subset of V_ω I just meant a well-founded, extensional object all of whose members are among V_ω. Now you are telling me that there are some objects that I missed. You are also

[5] For a related view concerning the categoricity results in arithmetic, see Parsons (1990) and Parsons (2008).

telling me that they have all the earmarks of what I meant by subsets of V_ω—they are well-founded, extensional, objects. So they are in my $V_{\omega+1}$. It is not as though there are two kinds of well-founded, extensional objects all of whose members are in V_ω—say the "blue" ones and the "red" ones—which by some sort of Pauli-exlusion principle cannot co-exist in the same universe. You are drawing a distinction without a difference."[6]

But this doesn't get any traction with the advocate of the multiverse since it presupposes absolute conceptions of powerset and infinity and it presupposes that there is a single, univocal conception of set. The advocate of the multiverse will argue that the above argument is circular. "True if one presupposes that there is a univocal conception of set, one which has absolute notions of powerset and infinity, then one can run the categoricity argument. But that just presupposes in the meta-language what one set out to establish. One gets out what one puts in."

1.3 Summary

It thus appears that our two parties are simply talking past one another. The above arguments are too quick. We need a deeper analysis if we are to make progress on the question of pluralism.

2 Hamkins on the Broad Multiverse

The question is whether Hamkins has make an improvement on the above quick argument for the broad multiverse. He certainly thinks that he has. For example, regarding the continuum hypotheses he writes:

> I shall argue in section 7 that the question of the continuum hypothesis is settled on the multiverse view by our extensive, detailed knowledge of how it behaves in the multiverse. As a result, I argue, the continuum hypothesis can no longer be settled in the manner formerly hoped for, namely, by the introduction of a new natural axiom candidate that decides it. Such a dream

[6] I am not endorsing this response. I am just pointing out that from a metaphysical point of view it is hard to wrap one's head around a multiverse conception. That doesn't mean it can't be done. It just means that there is some work that needs to be done to deflate the above response.

> solution template, I argue, is impossible because of our extensive experience in the CH and ¬CH worlds.

I will argue that his arguments do not go very far beyond the quick argument discussed above. His arguments are also too quick. They have no traction with the opponent. We still need to find a more subtle approach, one that proceeds on more neutral ground.

2.1 Model Theory of Set Theory

The core of Hamkins' case for the broad multiverse is based on the independence phenomena and the associated model theoretic results. This is a supplement to the original arguments of Skolem and von Neumann since, first, the models need not be elementarilly equivalent to the original models and, second, there are many more model construction techniques available today than when Skolem and von Neumann were writing. We now have inner model theory, ultrapowers, iterated ultrapowers, and the vast variety of forcing constructions. The question is whether these additional features drive us down the road to embracing the multiverse and whether they drive us as far as embracing the broad multiverse.

Hamkins claims that as a result of the independence phenomena and the associated model-theoretic constructions, set theory has become of a piece with group theory, ring theory, and topology:

> As a result, the fundamental objects of study in set theory have become the models of set theory, and set theorists move with agility from one model to another. ... While group theorists study groups, ring theorists study rings and topologists study topological spaces, set theorists study the models of set theory. (Hamkins (2012), p. 3)

To this one is bound to protest that the fundamental objects are still what they always were, namely, *sets*. Some of those sets happen to be models of certain theories. At certain times set theorists focus on sets that are models—for example, when they are asking whether a given problem that they are trying to solve is independent of the axioms that they are employing. At other times set theorists focus on sets in general—for example, when they are proving results in ZFC about singular cardinal combinatorics and, more controversally, when they are trying to find axioms that settle CH. But in all cases they are studying sets. This, at least, is the default view. The mere existence of model theory is not sufficient to undermine the default view.

On the default view there is a major difference between so-called *formal* mathematics—branches like group theory, ring theory, and topology—and *concrete* mathematics—brances like arithmetic, analysis, and set theory. In the case of formal mathematics one discerns certain high-level structural features that appear in many different structures. These structural features are of interest in their own right. So one writes down axioms that abstract these structural features—like the group axioms—and then one takes the axioms to *characterise* the class of structures one is interested in—in this case, the class of groups. No one, throughout the history of formal mathematics, has ever been under the illusion that there is a single such structure. No one has ever thought that they were investigating *the* group or *the* ring or *the* topological space. Such talk doesn't even make sense, given the nature of formal mathematics. There has never, for example, been a program to settle the axiom of commutativity.

In the case of concrete mathematics the situation is quite different. Here, the original aim of the axioms was not to characterise a class of structures but rather to write down princples that pertain to a *fixed* structure (up to isomorphism).[7]

I want to stress that I am not saying that since there is this *appearance* of a difference between concrete and formal mathematics on the default reading that we can conclude that there really *is* a difference. In fact, I think that very little can be concluded from such appearances. I'm not putting forth a positive argument; I am only against the argument that Hamkins puts forward. He is arguing that since there is (now) the appearance of a similarity between set theory (concrete mathematics) and formal mathematics we should conclude that there really is a similarity; indeed, so much so that set theory *is* a branch of formal mathematics. Even if he were right about the appearances I don't think that the conclusion would be warrented. But my point is more direct: His argument does not even get off the ground since the appearances point in the *other* direction—they point toward a difference, not

[7]Henceforth I will drop the qualification 'up to isomorphism' taking it to be obvious that that is what I intend. I use the word 'pertain' since at this stage I want to remain neutral on the issue of whether the axioms of conrete mathematics are 'true of' a fixed structure or rather 'implicit definitions' of that structure. This issue is independent of the one I am discussing. The key point for my purposes is that (on either reading) we are here dealing with a fixed structure.

toward similarity. This, at least, is the case on the default reading; it his how things proceeded historically and it would take some serious argumentation to override those fundamental difference and force us to conclude that set theory is really of a piece with formal mathematics.

Have the developments driven us to this conclusion? Let's first continue with some differences, differences that further support the default reading, even in the light of subsequent developments.

(1) In the case of concrete mathematics we have categoricity results (for arithmetic, analysis, and systems of set theory (with anti-large cardinal axioms)). This helps articulate the idea that in each case we are dealing with a fixed structure. In the case of formal mathematics there are no such results. I am not, of course, saying that the existence of categoricity results *secures* a unique structure and suffices on its own to undermine the multiverse conceptions. I have denied as much above. Rather I am saying that in concrete mathematics we *have* such theorems (while remaining neutral on the philosophical significance of such theorems) and in formal mathematics we do not. Regardless of the philosophical significance one attaches to categoricity results, the fact that we have them in one realm and not in another certainly tells us *something* about the difference between those two realms.

(2) In the case of concrete mathematics when we are faced with a problem that is independent of a given system we behave quite differently than in the corresponding case in formal mathematics. Consider some examples: (1) Over the base theory EFA one considers the Finite Ramsey Theorm. (2) Over the base theroy RCA_0 one considers the Hilbert Basis Theorem. (3) Over the base theory ACA' one considers Goodstein's Theorem. (4) Over the base theory ZFC one considers Friedman's Exotic Case in Boolean Relation Theory. (5) Over the base theory ZFC + "There is a measurable cardinal" one considers the statmenent Projective Uniformization. In each case the statement in question is independent of the base theory (under the relevant consistency assumptions, of course). That means that there are models in which the base theory holds and the statement holds and there are models in which the base theory holds and the statement fails. Still, this fact alone does not undermine our interest in knowing whether these statement hold *independently of that fact about independence*. In the early cases, most would agree that as far as the question of whether the statement holds are not, the independence result is neither here nore there. This is particularly clear in the earlier cases. Most would agree that the Finite Ramsey Theorem is a statement of arithmetic that has a determinate truth-value. We wish to

know that truth-value. The fact that the statement is independent of EFA is neither here nor there. In fact, the statement is true. Why? There are many reasons, one being that it follows from the totality of superexponentiation. Why doesn't the same kind of response apply in the other cases? Certainly something more needs to be said if we are to be moved from the default position.

In the case of formal mathematics our attitude is entirely different. It is hard to even find what one would regard as *independent statements*. Take the statement that a group is commutative (abelian). In a formal way one can make sense of the idea that this statement is independent of the group axioms—this is established by the existence of abelian groups and the existence of non-abelian groups. But this is a strange way to put it. The right way to put it—and the way that it is normally put—is by saying that some groups are abelian and some are not. We have an adjective—'abelian'—that expresses a property that applies to some groups and not others. In the case of concrete mathematics the case is quite different. It is not as though we have an adjective—'Finite-Ramseyian'—that expresses a property that applies to some natural number structures and not to others.

Again, I am not saying that this difference alone suffices to secure a real difference and so secure the universe view. The point is that there is an antecedent difference, it is quite well entrenched in our practice, and it is something that is going to require more than the mere existence of independence and model theory to dislodge.

We have here a case where the advocate of the universe view and the advocate of the broad multiverse view talk past one another. The advocate of the universe view agrees on the mathematical facts—the independence phenomena is real and the model theory of set theory is a rich and interesting subject. All of the models so constructed, along with their inter-relations, exist within the universe of sets. It is of interest to focus on these models since it shows us what rests on what and it guards us against being led down paths that lead to dead ends. It may even serve to help us determine certain truths about *the* universe of sets. Here it is helpful to compare the case of arithemtic. The study of non-standard models of arithmetic is a rich and fascinating study. But alongside of that one is still interested in the statements that hold within *the* intended structure of the natural numbers—statements such as the Finite Ramey Theorem, the Hilbert Basis Theorem, and so on. In fact, the parallel goes further, for as Woodin has pointed out, it may even be the case that the study of non-standard models illuminates our understanding of the truths about the natural numbers; for example, the answer to question of whether $P = NP$ may very well be illuminated through the study of non-standard models of arithmetic.

It is unclear whether Hamkins thinks that there is a substantive difference between the case of arithemtic and the case of set theory. At times he appears to apply the multiverse conception to both; for example, when he advocates the radical broad multiverse. At others he appears to treat them differently; for example, when he responded to the point about Goodstein's Theorem by saying that it was different because it held in all the universes of his multiverse (which was conditioned on ZFC, something he appeared to take as determinate). So far we can say that there are serious problems facing the radical broad multiverse conception; certainly we have not been given an argument for it. And we have not been given an argument for a relative broad multiverse conception. But perhaps there is such an argument. Perhaps the case of independence in arithetic and set theory really does force us into treating the latter as a branch of formal mathematics. To determine this we will have to dig more deeply into the independence techniques in set theory. But first I want to add a digression on geometry.

2.2 The Case of Geometry

In addition to saying that set theory is now really a branch of formal mathematics—like group theory, ring theory, and topology—Hamkins also says that it is like geometry; trying to solve CH is, on this view, like trying to solve the parallel postulate. The case of geometry is something of an outlier since it is not easily classified as either concrete or formal mathematics. It will be helpful to examine its status and determine whether it provides us with a true analogue of contemporary set theory.

Geometry has undergone an interesting evolution. In its early history, before the distinction between concrete and formal mathematics, geometry was conceived in a manner that had elements of both. Indeed it was through careful reflection on the case of geometry that we were led to the distinction between formal and concrete mathematics. On this early conception, geometry had elements of the concrete conception in that the intention was to investigate a fixed structure. But what that structure was was not exactly clear. It had something to do with physical space. But it was physical space in an *idealized* sense. And it is through this idealization that one might say that it had something in common with formal mathematics.

After a long history of failed attempts to prove the parallel postulate from the other axioms, its independence from the other axioms was established by the construction of non-Euclidean geometries. Shortly thereafter—especially in the hands of Riemann—formal geometry became a subject in its own right. The distinction between concrete and formal mathematics was clearly drawn.

What then of geometry as originally construed? Was it formal or was it concrete? Perhaps there is no clear answer to this question. It had elements of both but it seems a mistake to classify it as either. What is clear is that after Riemann the parallel postulate clearly lost its significance as an independent question *in the context of formal geometry.* But still, there was a question that remained; perhaps not an question that is equivalent to the old question, but one which nontheless remained—was physical space Euclidean.[8] The relationship between formal geometry and physical geometry became much clearer through the work of Russell (1897) and Einstein (1916) and subseqent elaborations of Carnap and Reichenbach. On this view formal geometry provides us with a collection of purely mathematical structures (Euclidean geometry, Hyperbolic Geometry, Riemanian Manifolds of non-constant curvature) and one sets up brige equations between these and the physical world (sending, say, geodesics to (idealized and interrupted) beams of light) and this gives sense to the questions of physical geometry. It makes no more sense to ask whether the parallel posulate holds of formal geometry (say, as characterized, by Minimal Geometry) than it does to as whether the axiom of commutativity holds of "group". But it does make sense to ask whether it holds of physical geometry, once we have fixed the bridge equations; this is just the question of whether, given the bridge equations, the geometry selected from the class of formal geometries, satisfies the parallel postulate. The answer for our physical universe (given the bridge equations used in general relativity—sending geodesic to (idealized and uninterrupted) beam of light, etc.—is 'no'.

In summary: Today we distinguish between physical and formal geometry. Set theory is quite unlike physical geometry. The question of whether it is like formal geometry is just like the question of whether it is like any other branch of formal mathematics—like group theory, ring theory, and topology. So our considerations above apply here as well, the conclusion being that we

[8]There is a story (probably apocraphal) that Gauss (one of the discoverers of non-Euclidean geometry) proposed answering this question by measuring the angles between the lines connecting three mountain peaks.

have not yet been given reason to think that it is.

2.3 Ontology of Forcing

Let us now dig a little deeper into the independence results and model-theoretic constructions in set theory, focusing on the case of forcing.

There are two standard model-theoretic accounts of forcing. The first starts with a countable transitive model M of ZFC (or whatever base theory over which one wishes to establish independence), selects an appropriate partial order $\mathbb{P} \in M$ and then uses the Baire Category Theorem to construct a generic object G, the result being a countable transtive model $M[G]$ which satisfies ZFC + $\neg\varphi$, where φ is the statement that one wished to establish was not provable in ZFC (or the relevant base theory). Let us call this the *countable transitive model approach.* It produces a set-size (indeed countable) object $M[G]$. As such $M[G]$ is (by construction) a non-standard model of set theory.

The second approach starts with a complete Boolean algebra $\mathbb{B}$ (which is very closely related to $\mathbb{P}$ above) and produces a Boolean-valued, class-size model $V^{\mathbb{B}}$. One then shows that with Boolean-value 1, $V^{\mathbb{B}}$ satisifes ZFC+$\neg\varphi$. Let us call this the *Boolean-valued approach.* It produces a class-size object $V^{\mathbb{B}}$ but one which is not two-valued. As such $V^{\mathbb{B}}$ is (by construction) a non-standard model of set theory.

Thus far we have, from the model-theroretic vantage point, a complete parallel with the case of arithmetic: In the case of artithemtic when one establishes independence model-theoretically the construction provides one with a model that is (by construction) non-standard.[9] The case in set theory with forcing (under either of the two standard approaches) is exactly the same.

Were we to end our discussion here the conclusion could only be this: The advocate of the universe view is unmoved by these considerations since the models produced are not candidates for the universe of sets, the first because it is an object *within* the universe of sets, the second because it is a description of a class-size structure which is not even of the relevant type. The mere existence of the model-theory of forcing (something that

[9]It is perhaps worthwhile to note that one *can* do the model-theory of arithmetic *within* arithmetic by using the arithmetized completeness theorem due to Hilbert and Bernays. Here again (of course) one sees that the model constructed is non-standard.

is uncontroversially accepted by both parties) is not sufficient to secure the multiverse conception. Something more needs to be said.

But Hamkins goes further. He introduces a third approach to forcing—the *naturalist approach.* Let us see if it fares any better.

Hamkins' aim is to "legitimize the actual practice of forcing, as it is used by set theorists" (p. 8). The background is this: Set theorists often use 'V' instead of 'M' and so write '$V[G]$'. But if V is the entire universe of sets then $V[G]$ is an "illusion". What are we to make of this? Most set theorists would say that it is just an abuse of notation. When one is proving an independence result and one invokes a transitive model M of ZFC to form $M[G]$ one wants to underscore the fact that M could have been *any* transitive model of ZFC and to signal that it is convenient to express the universality using a special symbol. The special symbol chosen is 'V'. This symbol thus has a dual use in set theory—it is used to denote *the* universe of sets and (in a given context) it is used as a free-variable to denote *any* countable transitive model (of the relevant background theory). The former use hardly ever gets invoked (except in certain philosophical moments) since when doing set theory if one wants to say something that holds of all the sets one utters an unrelativized statement. The latter gets invoked all of the time. But it is important to stress that it is—and this is understood by set theorists—on the most natural reading just an abuse of notation. Gerald Sacks once quipped: "Abuse of notation is the most powerful tool in mathematics." This is a case of that.

There is no need to "legitimize the actual practice of forcing"—it is already well-understood and legitimized. What Hamkins' is doing is trying to provide an account in which one takes this abuse of notation not as an abuse but as a description of multiple universes. The naturalist account of forcing is based on an interesting theorem: The theorem asserts that for any partial order $\mathbb{P}$ there is an elementary embedding of the universe V into a class model $\bar{V}$ for which there is a $\bar{V}$ generic filter $G \subseteq \mathbb{P}$. Moreover, the embedding and the forcing extension $\bar{V}[G]$ of $\bar{V}$ are definable classes in V and $G \in V$. In short, V can see (as definable classes) an elementarily equivalent copy of itself ($\bar{V}$) and a forcing extension of *that* ($\bar{V}[G]$). All of this lives inside V, as definable classes.

There are three important things to note about $\bar{V}[G]$—it need not be transitive, it need not be well-founded, it is a definable class in V. For all three reasons it is as non-standard a model of set theory as those produced in the first two approaches to forcing. Again, one sees by construction that the model produced is not of the appropriate type to count as the universe of sets.

But Hamkins wants to apply the theorem in a way which *does* produce a new candidate for the universe of sets. The idea is this:

> In such an application, one exists inside a universe V , currently thought of as the universe of all sets, and then, invoking [the theorem] via the assertion
>
> "Let $G \subseteq B$ be V-generic. Argue in $V[G] \ldots$"
>
> one *adopts* the new theory of [the theorem]. The theory explicitly stated in [the theorem] allows one to keep all the previous knowledge about V, relativized to a predicate for V, but adopt the new (now current) universe $V[G]$, a forcing extension of V . Thus, although the proof did not provide an *actual* V-generic filter, the effect of the new theory is entirely *as if it had*. This method of application, therefore, implements in effect the content of the multiverse view. That is, whether or not the forcing extensions of V *actually* exist, we are able to behave via the naturalist account of forcing entirely *as if* they do. In any set-theoretic context, whatever the current set-theoretic background universe V, one may at any time use forcing to jump to a universe $V[G]$ having a V-generic filter G, and this jump corresponds to an invocation of [the theorem].

The key step—the first one—is that of "adopting the new theory". It is admitted that the proof does not provide an *actual* V-generic filter—one does not construct an *actual* outer model $V[G]$ of V (after all $\bar{V}[G]$ is an inner model of V)—but one can act *as if* it does and, guided by the theory that one has adopted in the first step, *regard* as the new universe of sets and regard the old universe (again guided by the theory) as an inner model of the new universe. In short, one starts with a situation where we have an inner model (of the relative outer model) V and, guided by the theory of that model, one "flips" the two, now regarding V as the inner model of what was formerly the inner model V.

The point I wish to make is that from a model-theoretic view $\bar{V}[G]$ is *by construction* an inner model (of a non-standard sort) of V. This business of acting *as if* $\bar{V}[G]$ is an outer model of V is guided by the theory of $\bar{V}[G]$ alone and if *that* works in this case it also works in the case of the other two approaches to forcing. For when we construct $M[G]$ within V or when we construct the Boolean-valued model $V^{\mathbb{B}}$ we are just as free (again, guided by the theory of these models) to act *as if* these models are outer models of V.[10] So from the philosophical point of view the detour through the naturalist account of forcing is something of a red herring—we are back in the situation we were in before.

Hamkins, in fact, rasises something like this objection:

> Of course, one might on the universe view simply use the naturalist account of forcing as the means of explaining the illusion: the forcing extensions don't really exist, but the naturalist account merely makes it seem as though they do. The multiverse view, however, takes this use of forcing at face value, as evidence that there actually are V-generic filters and the corresponding universes $V[G]$ to which they give rise, existing outside the universe.

How is this supposed to be *evidence* that these models *really* exist outside of V?

The closest that Hamkins comes to an answer is on the basis of an analogy with the complext numbers. The idea is this: We start with the *real* numbers and we consider the complex numbers. For definiteness consider $\sqrt{-1}$. We have a copy of the complex numbers inside the real numbers. We are then free—appealing to the free, creative nature of mathematics—to treat the domain of complex numbers as a domain in its own right. This domain extends the real numbers and it contains, in addition, to the real numbers, *imaginary* numbers like $\sqrt{-1}$. We are supposed to treat $V[G]$ in the same manner.

But this analogy breaks down completely. In the case of the real numbers what we introduce is not a new *real* number but rather a *complex number* number which is not a real number. In the case of the universe of sets what we introduce is a new *set*. In the first case no one would protest: "Wait. You can't do that. My intention was to capture all of the real numbers and you have introduce a real number. So it was among the one's I considered in the first place." In the second case the advocate of set theory on the default

[10]One might protest that there really is a difference in this case, namely, in the Hamkins setting $\bar{V}[G]$ has access to the theory of V. But this must ultimately trace back to the Laver-Woodin result that the ground model is always definable in the generic extension. In any case, we can apply that fact in the previous settings as well if we wish to have a complete parallel.

reading will protest: “Wait. You can’t do that. My intention was to capture all of the sets and you have introduced a set. So it was among the one’s I considered in the first place.” To make the comparison even more transparent consider the case where the forcing adds a “new” real number, say a Cohen real. One protests: “Wait. You can’t do that. My intention was to capture all of the real numbers and you have introduced a real number. So it was among the one’s I considered in the first place. This fact is evident when we implement forcing via countable transitive models of the form $M[G]$.”

In summary, on the face of it, all three methods provide us with models that are either sets in V or inner models (possibly non-standard) of V or class models that are not two-valued. In each case one sees by construction that (just as in the case of arithmetic) the model is non-standard. One can by an act of the imagination treat that new model as the “real” universe. The broad multiverse position is a *consistent* position. But we have been given no reason for taking that imaginative leap. We have been given no reason for embracing the broad multiverse.

2.4 The Dream Solution

That completes our discussion of Hamkins’ positive arguments for the broad multiverse conception. Before summarizing our results I want to discuss his negative arguments against people like Woodin who are trying to resolve the undecided statements, like CH. Hamkins thinks that he can show that such an endeavour will never succeed.

He first points out that CH can be forced over any model of set theory and ¬CH can be forced over any model of set theory. Thus, one can start with any model of set theory and flip CH on and off like a switch by passing to successive forcing extensions. What is the significance of this fact? Hamkins takes it to substantiate the multiverse, at least with regard to CH and sentences which bear this “switching” feature.

But the switching feature is simply due to the fact that CH is an Orey sentence and that this is established by the main independence technique of set theory. It is straightforward to construct an Orey sentence for *any* minimally strong system—this includes Q, PA, ZFC, etc. In fact, for each such system one can construct a (provably) Δ^0_2 Orey sentence for the system. Any such sentence φ for a theory T has the feature that over any model of T one can end-extend the model to get a model of φ and one can end-extend to get a model of $\neg\varphi$. Thus, one can start with any model of T and flip φ

on and off like a switch by passing to successive end-extensions.

If Hamkins reasoning for concluding that CH is indeterminate is correct then it is hard to see how the parallel reasoning for concluding that φ is indeterminate is not also correct. And this is a difficult conclusion to endorse. For example, few would allow that a (provably) Δ^0_2 sentence for Q is indeterminate.

I'm not sure how Hamkins would respond to this. There are points (see below) where he seems to advocate the relative broad multiverse (relative to ZFC) and others when he seems to advocate the radical broad multiverse. If he takes the second tack then he has a response—he simply embraces the conclusion. But as we concluded, that poisition through logically irrefutable is philosophically barren and no argument has been offered for it. Like all forms of universal skepticism we are passing over it in silence. If he takes the first tack he might respond as follows: "Our background theory ZFC settles the Orey sentence and so we know that it is determinate; in fact, we know its truth-value". But one can construct Orey sentences for which this response is not available. Hamkins' background theory appears to include ZFC and a bit more (some small large cardinal axioms). But whatever it is it certainly does not include PD and the corresponding large cardinal axioms (those asserting the existence of Woodin cardinals). Now one can construct an Orey sentence which is just like PU in the following sense: It is an Orey sentence for ZFC and remains an Orey sentence for ZFC when one supplements it will large cardinal axioms *falling short of those asserting the existence of Woodin cardinals* but when one supplements if with the large cardinal axiom asserting that there are ω-many Woodin cardinals then the Orey sentence becomes provable. This sentence is as out of the reach of Hamkins' background theory as PD is. Yet it is a simple arithmetical sentence. So he is going to either have to take the same stance regarding it that he takes toward PU and CH or he is going to have to point to a difference between this case and the case of CH.

Perhaps Hamkins will respond by saying that in the case of CH the models produced are very "natural"—they "look" like they are good candidates for the true universe of sets—, while in the case of the Orey sentence φ the models produced are non-standard and so do not look like good candidates for the true universe of natural numbers. But the models in the second case are only seen as unatural and non-standard if we help ourselves from the start to the idea of the standard model of arithmetic. And exactly the same situation prevails in the case of set theory. From the default view in arithmetic one can "see" that each model produced is non-standard since (helping oneself to the idea of the true natural numbers) one can select the standard part of any such model. Correspondingly from the default view in set theory one can "see" that each model produced is non-standard since either it is countable or it is Boolean-valued or it is a class-size inner model that need be neither transitive nor well-founded.

Hamkins tries to further butress his claim by arguing against any possible solution to CH. He characterizes the "dream solution" as follows:

> Set theorists traditionally hoped to settle CH according to the following template, which I shall now refer to as the dream solution template for CH:
>
> > Step 1. Produce a set-theoretic assertion Φ expressing a natural 'obviously true' set-theoretic principle.
> >
> > Step 2. Prove that Φ determines CH. That is, prove that $\Phi \to \text{CH}$ or prove that $\Phi \to \neg\text{CH}$.
>
> In step 1, the assertion Φ should be obviously true in the same sense that many set-theorists find the axiom of choice and other set-theoretic axioms, such as the axiom of replacement, to be obviously true, namely, the statement should express a set-theoretic principle that we agree should be true in the intended intended interpretation, the pre-reflective set theory of our imagination. Succeeding in the dream solution would settle the CH, of course, because everyone would accept Φ and its consequences, and these consequences would include either CH or $\neg$CH. (Hamkins (2012), p. 16)

The first thing to point out about this is dream that no one has ever had. First, it is misleading to say that the axiom of choice and the axiom of replacement are 'obviously true'. It is doubtful that even very basic principles of arithemtic (like the totality of exponentiation) are obviously true. But presumably Hamkins means something weaker than obviousness and something more like what Parsons has called 'intrinsic plausibility'. But even if we make that modification the dream solution is still a dream that no one has ever had. No one has thought that the case for axioms settling CH would be as clean as the case for axioms like AC or the axiom of replacement. It would, at most, be more like the case for PD, a case that has gained wide acceptance by the set-theorists (in particular, inner model theorists and descriptive set theorists) who know the details of the constructions and theorems involved in the case that has been made for PD.

Suppose then that the real deam solution—that of finding a "PD-like" case for axioms settling CH—has panned out and has led to an axiom Φ that purports to meet Steps 1 and 2. Hamkins points out that over any model of Φ we can force CH and we can force $\neg$CH. (Notice that the forcing does not preserve Φ!) But how is that supposed to be a problem? The advocate of Φ will point out Φ has not been preserved. Hamkins will respond: "But these CH models and these $\neg$CH models are equally good candiates for the universe of sets". The advocate of Φ responds: "No. They don't both satisfy Φ!". Hamkins responds: "But you have to explain away the illusion that these are genuine candidates for the universe of sets. You have to explain away our experience with these models." The advocate of Φ responds: "But they are non-standard models—by construction, they are countable, Boolean-valued, or (possibly non-transitive or ill-founded) inner models. By construction they are not candiates for V. All you have to go on in saying that they are candidates for V is the theory involved (not the model-theoretic construction) and as far as the theory involved goes they are not candidates for V since they don't satisfy Φ. If you want to engage on the level of theory then you can't simply appeal to a model-theoretic construction—you have to engage with the case I have presented for Φ."

It's instructive to compare the case of arithmetic. Consider first the base theory PA and the statement of Goodstein's theorem. We can construct models of PA in which Goodstein's theorem holds and we can construct models of PA in which it fails. But this alone doesn't mean that Goodstein's theorem is indeterminate. In fact, we have reasons for accepting it—for one it follows from the fact that ϵ_0 is well-founded. If one wants a case that provides a closer parallel to that of CH one can simply take a (provably) Δ^0_2 Orey sentence for PA. Such a sentence has the feature that we can always end-extend them to flip the truth-value of the Orey sentence. But this alone does not suffice to establish that the Orey sentence is indeterminate. For example, we could have started with an Orey sentence which through independent of PA follows from the well-foundeness of ϵ_0.

When I pointed out such examples to Hamkins during the question period of his talk he responded by saying that in the arithemtical cases the parallel did not hold since the Orey sentences in question were settled by ZFC and so were all true in the universes of his multiverse. But, of course, I could have used an Orey sentence that is not settled by ZFC and so this response fails. But Hamkins' response also reveals something—it reveals that in accepting ZFC (and hence that Goodstein's theorem is indeed determinate and true) he is really advocating the *relative* broad multiverse, a multiverse that is conditioned on ZFC. But what is his reason for accepting ZFC? Were he to be advocating the broad multiverse in 1908 would his multiverse be conditioned on Z? Is it only because the community has been won over to AC and the axiom of replacement? If so, what is to say that in 100 years the same will not be true of PD and a new axioms settling CH? It appears that Hamkins is not standing on principled ground, but rather making a projection about the future on the basis of the current state of play, while ignoring the path by which we got to where we are now. It is as if one were to argue against the hope of a grand unified theory in physics on the basis of the fact that we do not yet have one. If history continues in its current vein then we can expect that progress will be made on both fronts. Whether this will lead to a resolution of CH only time will tell. But what seems clear is that it is going to take more than quick arguments of the kind we have been discussing. One is going to have to actually engage with the serious cases that have been put forward for axioms that settle CH.[11]

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[11]Hamkins discusses Frieling's axiom and the Powset Size Axiom. But these are not serious candiates for new axioms and set theorists have quickly dismissed them. The serious proposals involve forcing axioms, $(\star)$, inner model theory, and axioms concerning the structure theory of $L(V_{\lambda+1})$.

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