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NOMINALISM IN MATHEMATICS: MODALITY AND NATURALISM

by

JAMES S.J. SCHWARTZ

DISSERTATION

Submitted to the Graduate School

of Wayne State University,

Detroit, Michigan

in partial fulfillment of the requirements

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2013

MAJOR: PHILOSOPHY

Approved by:

Advisor

Date

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DEDICATION

For my mother

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Chapter three has improved greatly from extensive comments from Eric Hiddleston and from Michael McKinsey. The chapters on naturalism (four and five) are the result of many rounds of comments from Susan Vineberg. Their influence on these sections of the dissertation is warmly acknowledged; I fear they do not receive enough credit in the footnotes—especially for helping me to think more clearly about which ideas were worth developing and which were not.

Finally, I would to thank my mother, Cynthia Johnson, whose encouragement and sup-

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Introduction

I Thesis

This dissertation is a defense of certain *modal* nominalist strategies in the philosophy of mathematics against two varieties of criticism. My primary thesis, which is defended in Part I (chapters one, two, and three), is that modal nominalists do not encounter uniquely challenging difficulties in regards to justifying the modal assertions that figure in modal nominalist theories of mathematics. My secondary thesis, which is defended in Part II (chapters four and five), is that modal nominalism does not engender any serious conflict with the practice of mathematics, and thus is consistent with the naturalistic impulse to respect science and mathematics. In defending these theses I hope to establish modal nominalism as a viable and attractive philosophy of mathematics, one that can provide satisfactory answers to many of philosophy of mathematics' longstanding questions.

My goal in this introduction is to explain the provenance of these theses by examining the following questions: What is the philosophy of mathematics? What is nominalism? What is *modal* nominalism? What has modality got to do with nominalism in mathematics, and what, if anything, should a modal nominalist find troubling about this? What is naturalism, and why should anyone suppose that naturalism and modal nominalism are incompatible?

II The Basics

II.1 What is the Philosophy of Mathematics?

The philosophy of mathematics is a broad-ranging subdiscipline of analytic philosophy tasked with answering questions such as: “What is mathematics about?” “What is the best characterization of what the practicing mathematician *does*?” “What is the methodology of mathematics, be it pure or applied?” “How do mathematicians decide whether to adopt new theories or axioms?” “Do there exist mathematical objects?” “What kind of a logical foundation is necessary for doing mathematics?” “Why is mathematical reasoning so successful in scientific applications?”... and the list could go on. Nevertheless, the picture that emerges is one according to which philosophers of mathematics are interested in the nature of mathematics as well as the methodology of its practice.

With few exceptions, mathematicians have rather little to say about the nature of their discipline; they would appear to be interested more in activities like seeking out fruitful mathematical concepts and axioms, constructing proofs, and investigating the structural properties of formal systems, than in the activity of determining whether the axioms utilized in their proofs are made true by any particular objects. Questions such as “What can be proven from a particular set of mathematical axioms?” and “What properties are shared by all dense linear orderings?” do not strike me as overtly philosophical questions, at least when they are asked from the standpoint of someone who is trying to understand mathematics *as it is practiced*. Related to the case of proof construction, perhaps there are good philosophical reasons—although I doubt this—for rejecting classical logic. But to argue on philosophical grounds that mathematicians have no reason to believe in the conclusions of nonconstructive proofs seems not so much an exercise in attempting to *understand* mathematics as it is a case of attempting to tell mathematicians what they can and cannot do.

On the other hand, questions as to whether a set of mathematical axioms holds *true* of

anything, and moreover, whether there is any uniquely *mathematical* ontology, do strike me as philosophical questions. And provided that the philosopher does not say anything to sully the practice of mathematics, she should not be impeded in her attempts to understand what exactly the *truth* of a mathematical theory comes to, if indeed mathematical theories are the kinds of things she ought to regard as true.

In this dissertation I will principally be concerned with the question about whether mathematical objects exist. I aim to defend several theories, *viz.*, Charles Chihara's Constructibility Theory, Geoffrey Hellman's Modal Structuralism, and Hartry Field's fictionalism, each of which purports to offer accounts of, e.g., mathematical knowledge, and the content of mathematical claims, in ways that do not presuppose or otherwise require the existence of mathematical objects. Views such as these are generally described as *nominalistic* accounts of mathematics. But what does it mean for an account of mathematics to be nominalistic? And are Constructibility Theory, Modal Structuralism, and fictionalism genuinely nominalistic?

II.2 What is Nominalism?

Nominalism is a philosophical thesis according to which abstract objects, such as universals, do not exist. It is often contrasted with a thesis known sometimes as 'realism' and other times as 'platonism',¹ which holds that there *do* exist abstract objects of various kinds. Nominalism *in mathematics* is the philosophical thesis that there do not exist any abstract *mathematical* objects, such as numbers, functions, sets, and so forth. It is often contrasted with platonism in mathematics, which holds that there *do* exist abstract mathematical objects. With the exception of the remainder of this section, I will drop the lengthy phrases 'nominalism in mathematics' and 'platonism in mathematics,' using just the terms 'nominalism' and 'platonism' to refer to their respective positions in the philosophy of mathematics. On the (rare!) occasions when I discuss the general views, I will use terms

¹Note the lower-case 'p' to distance contemporary platonism from genuine Platonic doctrines. I shall use the terms 'realism' and 'platonism' interchangeably throughout this dissertation.

such as ‘general nominalism/platonism’ and ‘nominalism/platonism in general.’

The distinction between nominalism/realism *in general* and nominalism/realism *in mathematics* is important. A defender of nominalism (or realism) *in mathematics* is not, on pain of contradiction, obliged to provide concomitant defenses of nominalism (or realism) in other areas. There is conceptual space available for a person interested in denying the existence of abstract *mathematical* objects, while accepting the existence of universals or other kinds of abstracta. Similarly, one might be a realist about mathematical objects, while denying that universals and other kinds of abstracta exist. Perhaps there is some peculiar kind of inconsistency at play in such views, but whatever inconsistency may be present, I can make no sense of saying that it is a *logical* inconsistency.

The nominalist faces a number of challenges within the philosophy of mathematics. Following W.V. Quine and Hilary Putnam, many have argued on naturalistic grounds that abstract mathematical objects are somehow *indispensable* for describing the world in a scientifically perspicuous way. The best scientific theories include mathematics; one is compelled to believe the best scientific theories—but how could one do this without believing the constituent mathematical assertions of the best scientific theories? Or, to put the point more strongly, it seems rational to regard the best scientific theories as being *true*, but how could such theories be regarded as true unless their constituent mathematical assertions are similarly regarded as true? *Prima facie*, mathematical assertions are about things like numbers, functions, vectors, and spaces—how could such assertions be *true* without there existing numbers, functions, vectors, and spaces?

Much of the work done by nominalists in the 1980s consisted of attempts to rebut the indispensability arguments. Various strategies were proposed, ranging from denying that the best scientific theories require mathematics, to denying that mathematical assertions are best understood as being about abstract mathematical objects. A great deal of *this* work, *viz.*, the work stemming from Field’s fictionalist program, has been motivated by epistemological concerns. In the previous decade the causal theory of knowledge had

won over many philosophers. If one must be causally connected to an object in order to have knowledge about it, and if abstract mathematical objects are acausal, then no one is causally connected to any mathematical objects, from which it follows that no one can know that they exist, making it a great mystery as to how mathematicians (and ordinary folks) apparently know so many things about them. Although the causal theory of knowledge is currently out of favor, many believe there are lingering problems about how best to explain why it is that mathematicians come to possess their very reliable mathematical beliefs. What mechanisms ensure that mathematical reasoning, which is done largely without reference to metaphysical hypotheses about the nature of mathematical truth, accesses truths about mathematical objects, platonistically construed? What explanation can be given for the correlation between statements such as “complex numbers exist” and “mathematicians believe that complex numbers exist?” For, according to the traditional platonist picture, mathematical objects in no way interact with human beings. This suggests that mathematicians would believe that complex numbers existed even if the facts about the mathematical realm were entirely different, and serves to undermine any claim that the mathematician thereby *knows* that complex numbers exist.

Nominalism in mathematics, at least in the primary sense, requires the outright denial that mathematical objects exist. That is the positive component of nominalism in mathematics, at any rate. But notice that the epistemological concerns just mentioned function primarily as *objections* to platonism in mathematics, i.e., as criticisms of the claim that anyone has good reasons for supposing that mathematical objects exist. This identifies a further, negative component of nominalism in mathematics—to undercut platonist arguments that, if sound, would establish the existence of mathematical objects. As it turns out, most of the theories of mathematics that have been described as “nominalist” theories of mathematics, including those defended in this dissertation, do not seek to decisively establish the non-existence of mathematical objects, but are instead constructed for the purpose of undermining platonism in mathematics. Thus, most “nominalist” theories

of mathematics are *not* nominalist in the primary sense but are instead nominalist in a weaker, secondary sense, in which their truth is compatible with (but does not require) the non-existence of mathematical objects.

Though I welcome efforts to establish the truth of nominalism (in mathematics and in general) in the primary sense, this dissertation is, strictly speaking, a defense of several theories of mathematics that are “nominalist” in the secondary sense. That is, this dissertation is a defense of several theories—Chihara’s Constructibility Theory, Hellman’s Modal Structuralism, and Field’s fictionalism—that were constructed for the purpose of showing that it is *not necessary* to assume the existence of mathematical objects in order to provide an account of the content of mathematical claims. In particular, I aim to defend the specific metaphysical claims that these “nominalists” make in the construction and advancement of their theories of mathematics. In this dissertation I will often use the term ‘modal nominalism’ to refer to these views as a group (which are loosely related in that each invokes modality in important ways). Thus, when I discuss, defend, and offer arguments in support of modal nominalism, I am not trying to establish the truth of nominalism in mathematics in the primary sense; I am only trying to establish nominalism in mathematics in the weaker, secondary sense.

III Modality

What distinguishes modal nominalist theories in philosophy of mathematics is their use of modality in eschewing commitment to mathematical objects. But why do modal nominalists opt to embrace modality in order to eschew commitment to mathematical objects? And how can modality be used to facilitate this task?

III.1 Modality in Philosophy of Mathematics

As a first example, consider the Constructibility Theory of (Chihara 1990). Chihara avoids postulating the existence of mathematical objects by replacing mathematical existence assertions with assertions about the constructibility of open-sentence tokens, through

which he hopes to capture a simple type-theoretic framework. These constructibility assertions are taken to express statements about the metaphysical possibility of constructing open-sentence tokens. A second example is Hellman's Modal Structuralism (1989). On Hellman's view, one can construe mathematical assertions as assertions about what holds within various *possible* mathematical structures, avoiding the need to quantify over any *actually* existing mathematical objects. Hellman takes such assertions to be statements about what is primitively logically possible. A final example is Field's fictionalism (1980). Field's account of the *scientific applications* of mathematics holds that mathematics is *conservative* in the sense that any nominalistic consequence of a nominalized physical theory that includes mathematics is a consequence of the nominalized physical theory alone. In articulating this view he develops an account of mathematical *practice* which holds that mathematical knowledge is just logical knowledge. The mathematician can be described simply as person who investigates what follows from mathematical assumptions. According to Field, accounting for this kind of logical exercise requires premises no stronger than those which assert the consistency of the relevant mathematical assumptions, which, for Field, comes to the assertion of the primitive logical possibility of their conjunction.

More details on these three projects are given in the first chapter, but it should now be clear that the use of modality performs important work in modal nominalist philosophies of mathematics. Platonists have seen fit to criticize them on this score. If modality must be invoked in order to eschew commitment to mathematical objects, this would appear to improve this situation only if modality does not raise epistemological and metaphysical difficulties that are just as serious as those surrounding the mathematical objects under exile. For instance, it might be that the best story about the truth-conditions for modal assertions involves some postliminary increase in ontology. Proposed reductive bases for modality have included full-blooded *possible worlds*, maximally consistent *sets of propositions*, and maximal combinations of *states of affairs*, each of which have at least a tinge of platonism to them. If the modal nominalist opts for such reductions, then two

apparently undesirable consequences follow. First is that each proposed reduction requires an increase in ontology (a move that is clearly problematic for those interested in pursuing a general nominalism). Second is that modal nominalists must further secure a means for justifying assertions about the proposed reductive bases. The modal nominalist could hardly claim to have improved on the platonist's account of mathematical knowledge if she proposes only to exchange one set of difficulties (justifying existence and knowledge claims about abstract mathematical objects) for another set of difficulties (justifying existence and knowledge claims about possible worlds, sets of propositions, etc.). Modal nominalists believe that they can avoid any such increase in ontology by maintaining that possibility and necessity are primitive and unanalyzable notions, but doing so only serves to raise questions about how it is possible to justify assertions about what is primitively necessary and possible (e.g., to explain why it is the case that the axioms of Zermelo-Fraenkel set theory are jointly possible and to justify such an assertion on nominalistically acceptable grounds).

III.2 Shapiro's Challenge to Nominalism

The criticism that modal nominalism raises serious questions concerning the modal nominalist's ability to justify modal assertions is voiced most forcefully by Stewart Shapiro (1997). Any interpretation of mathematics that is powerful enough to capture ordinary mathematical reasoning must provide a scheme for translating classical mathematical assertions into nominalized assertions.² Shapiro's insight is in recognizing that these translations can also be *undone*. That is, the modal nominalist's translations can be translated *back into* their original platonistic counterparts. This is a significant result for Shapiro, who, as a mathematical structuralist, views mathematics as the study of the *relations* that hold between objects (e.g., number-theoretic relations) rather than an investigation of the *objects* themselves (e.g., numbers). The intertranslatability between ordinary mathematical

²E.g., in Hellman's case, the assertion that 'Peano Arithmetic implies A ' can be "nominalized" by reconstruing it as 'Necessarily, if X satisfies the Peano axioms then X implies A .'

assertions and the modal nominalist's reconstructions suffices to demonstrate that both accounts capture the *same mathematical structure*. According to Shapiro, mathematical structures are freestanding objects, akin to *ante rem* universals, and any view that characterizes a mathematical structure is thereby committed to that structure's existence. Thus, modal nominalist views are ultimately committed to abstract objects in the form of mathematical structures. Therefore, the modal nominalist is unsuccessful in her attempts to eschew commitment to mathematical entities.

To add injury to insult, Shapiro draws attention to the fact that modal nominalists do not have available any nominalistically acceptable reductive bases for the modal notions they employ in their theories. It is commonly thought that the logical modalities—the kind of modality used by Hellman and Field—can be reduced model-theoretically.³ Although there is less agreement in the case of the metaphysical modalities—the kind of modality used by Chihara—some believe that these can be reduced by countenancing either possible worlds or one among a sundry of possible worlds surrogates. The possibility of reducing the modal notions to non-modal notions is significant in the following sense: A nominalist such as Hellman appears stuck with, e.g., the logical possibility of the existence of a model of Peano Arithmetic as a brute and unanalyzable fact—an assertion the truth of which is not amenable to explanation. However, if the logical modalities can be reduced to facts about models, then one can explain the logical possibility of p by pointing to the relevant non-modal facts about some model or other. Thus, the logical possibility of the existence of a model of Peano Arithmetic can be demonstrated by constructing a model satisfying the axioms of Peano Arithmetic. If the metaphysical modalities can be reduced, then one can give an explanation for why p is metaphysically possible by pointing to the relevant non-modal facts about some possible world (or possible world surrogate). Reduction thereby increases one's explanatory resources and would appear to free one from needing to devise a special epistemology for modality (because modal knowledge *just is* knowledge

³Thus the association with logical necessity as *truth-in-all-models* and logical possibility as *truth-in-a-model*.

about the reductive base).

According to Shapiro, the modal nominalists in question seem happy to accept primitive modal notions, thereby incurring the challenge of justifying the various primitive modal assertions that figure in their accounts of mathematics. Shapiro also insists that modal nominalists (overtly or covertly) rely on the platonistic, model-theoretic account of the logical modalities, and consequently, that they cannot coherently engage in modal reasoning in isolation from reasoning in model-theoretic systems of modal logic. Nominalists such as Chihara and Field have offered responses to the latter complaint, but none have done anything to rebut the justificatory criticism identified in the former complaint. Modal nominalists thereby do appear to be burdened with intractable problems involving modality. My overall aim in Part I of this dissertation (chapters one, two, and three) is to show that modal nominalists are not unique in carrying this justificatory burden.

There is ordinarily thought to be a tradeoff between ontology and ideology. One can deflate one's ontology by bloating one's ideology, and vice versa. In appealing to the modal notions to eschew commitment to mathematical objects, the modal nominalist accepts what appears to her to be a bargain; for the price of a small increase in ideology she can vastly simplify her ontology. But if Shapiro is right, matters are not nearly so simple. In the first place, the modal nominalist's reconstructions do not reduce her ontology—she is unwittingly committed to mathematical structures. In the second place, the theoretical resources she requires in order to produce her reconstructions inflate her ideology and burden her with a new challenge—that of explaining and justifying, in a non-platonistic way, why it is the case that various mathematical theories are possible (or that it is possible to construct open-sentence tokens of various kinds). In comparison with platonism (burdened only by its vast ontology), modal nominalism is the clear runner-up.

My response to Shapiro addresses both the criticism that modal nominalist reconstructions of mathematics do not reduce ontology, and also the accusation that the modal nominalist incurs a novel burden (one not shared with the platonist) concerning the

justification of modal assertions. After providing a detailed reconstruction of Shapiro's arguments, I start by questioning the idea that the intertranslatability between modal nominalist and platonist theories has ontological consequences. It seems obvious that two theories could be intertranslatable, and yet quantify over distinct domains. For instance, one might devise a theory about the depth-charts of two different baseball teams that happen to have the same number of players assigned to each position. Since both depth-charts realize the same "structure," the two theories have the same ontological commitments. But if the theories have the same ontological commitments, then these two theories are theories about identical players! This reply ultimately misses the point in more than one way. What is of interest for Shapiro are theories that are sufficiently *formal* (in a sense that is explained in the chapter); when two *formal* theories are intertranslatable they both realize the same *structure*. For Shapiro, the ontology of a mathematical theory is determined by the *structure* that it invokes. So two theories have identical ontological commitments only as far as their shared structure is concerned—no claim follows that their exemplifications must be identical.

As something of an aside, I consider how the dialectic might proceed if, contrary to Shapiro's intentions, the intertranslatability between modal nominalist and platonist theories unveils modal nominalist theories as no more than mere synonymous paraphrases of literal mathematical assertions. The thought here is that, if p is a synonymous paraphrase of q , then p and q both express the same proposition, and thereby have the same commitments. On the other hand, if p and q differ in their commitments, then p is not, in the end, an acceptable paraphrase of q . In application, the claim is that if modal nominalist theories are merely re-writes of platonist theories, then they are only acceptable re-writes if they say the same things as platonist theories. And since platonist theories are committed to mathematical objects, then so too are modal nominalist theories. My response is relatively straightforward: Modal nominalist theories of mathematics are not mere paraphrases of platonist theories.

Returning to the main event, I take a close look at both Michael Resnik's and Shapiro's structuralisms to determine whether there is any reason to grant credence to the alleged connections between translation and structure and between structure and ontology. What I discover is a rather surprising pattern: On both Resnik's and Shapiro's accounts of structuralism, the fundamental or core claims to be made are the following: It is a (naturalistic) *presupposition* of mathematics that various mathematical theories are *coherent*. Further, that a mathematical theory is *coherent* is a sufficient condition for postulating the existence of a structure of which the theory is a realization—this claim is hypostasized as an axiom of Shapiro's structuralism, and is known as the Coherence axiom. For Shapiro, coherence is a primitive notion, akin to satisfiability, but a notion that is nevertheless not terribly distinct from the primitive modality Hellman uses. I say these discoveries are surprising for the following reasons: It is part of Shapiro's criticism that modal nominalists lack a plausible means for justifying the modal assertions that figure in their theories. But according to Shapiro, it is perfectly acceptable to treat the coherence of mathematical theories as a presupposition of mathematics. Given the indistinctness of coherence and primitive logical possibility, there is no barrier to a modal nominalist assuming, as a presupposition of mathematics, that mathematical theories are logically possible. So if Shapiro is right, there is either no important problem for the modal nominalist, or it is a problem that the modal nominalist and structuralist face equally.

Moreover, the modal nominalist contends that she can get well enough along with *only* the assumption that mathematical theories are logically possible; meanwhile Shapiro must defend the additional thesis that, in mathematics, coherence suffices for existence—his Coherence axiom. Shapiro claims that his structuralist position is to be preferred for holistic reasons—that structuralism provides the best or most plausible account of the mathematical enterprise. Since the Coherence axiom is a component of Shapiro's structuralism, it gets justified along with the holistic justification for his structuralist view. I contend, however, that this holistic justification *uses* the Coherence axiom, and so

cannot provide independent support for it. Shapiro never explicitly constructs this holistic argument, but his extant work suggests that structuralism is thought to be more plausible than its competitors in large part because the competition—including the modal nominalist views defended in this dissertation—retain the ontological and epistemological problems facing platonism (including structuralist platonism). But the idea that modal nominalist theories are so troubled is the very claim for which justification is sought. Thus there is no independent support for the Coherence axiom, and subsequently, no independent support for the claim that modal nominalist theories are committed to the existence of mathematical structures.

So much for Shapiro's criticism that modal nominalism fails to effect a genuine reduction in ontology. What about his accusation that only a platonist position can incorporate the justificatory resources of the model-theoretic reduction of the logical modalities? Here Shapiro can be thought of as eliciting a challenge to the modal nominalist: For the modal nominalist to show that she in fact has nominalistically acceptable, non-platonic resources through which to justify the modal assertions that figure in her theories. I call this "Shapiro's Challenge," and although I take it as a serious threat to modal nominalism, I contend that this challenge ultimately need not be met.

In chapter three, which will be described in more detail in a moment, I argue that seeking a nominalistically acceptable reductive base for the modal notions is a fool's errand. The modal nominalist should not be troubled by her inability to find a *nominalistically* acceptable reductive base for the modal notions because reductive theories of modality do not provide effective vehicles with which to justify modal assertions. (Thus not even Shapiro can meet Shapiro's Challenge!) The upshot is that all accounts of mathematics that are in some way mediated by modality face the same kind of justificatory burdens that modal nominalist theories face.

Even for those who do not take platonism's intractable epistemology as a core motivating feature for adopting their modal nominalist views, the use of modality should still raise

eyebrows. For it is evident that the use of modality raises some difficulties that are not evidently present in non-modal philosophies of mathematics; the modal nominalist needs some degree of assurance that the new problems her view raises are less quarrelsome than the problems, whatever they happen to be, she takes herself to be avoiding in rejecting platonism. Imagine a philosopher who takes the Quine/Putnam scientific indispensability arguments to provide the best evidence for the existence of mathematical objects. Nevertheless, she is not convinced that mathematical objects exist, and feels compelled to make a case that mathematical objects are actually *dispensable* in scientific theorizing. She welcomes the labors of Chihara, Hellman, and Field, who she takes to have given promising accounts of how it is possible to carry out scientific reasoning without quantifying over mathematical objects. Certain categorical possibility and necessity statements are listed among the claims of these philosophers. But if, for example, the epistemology of categorical modal assertions is intractable, or she has no way of justifying the modal assertions made by Chihara, Hellman, and Field, then she cannot be fully confident that these views succeed in rebutting the indispensability arguments. She would do well to show, then, that her less-than-complete confidence in certain modal assertions is not something that should trouble her.

III.3 Modality and Reduction

The force of Shapiro's Challenge depends strongly on the assumption that the absence of a nominalistically acceptable reductive account of modality creates a uniquely challenging problem for modal nominalists, *qua* modal primitivists—*viz.*, that modal nominalists face the burden of justifying the modal assertions that figure in their accounts of mathematics. Is Shapiro warranted in concluding that this problem places a unique burden on the modal nominalist? He would only appear warranted in drawing such a conclusion under the assumption that reducing modality provides or constitutes a means for justifying modal assertions. The goal of chapter three is to argue that this assumption is unwarranted.

Shapiro advertises the objection that modal nominalists cannot justify the modal as-

sertions that figure in their theories as an *epistemological* objection to modal nominalism. But this diagnosis seems somewhat premature—the problem that Shapiro identifies for modal nominalists is that, because they espouse modal primitivism, they lack the means through which to describe the *content* of modal assertions. That is, modal nominalists are apparently in the dark regarding the *truth-conditions* of the modal assertions they make. It must be admitted that this would seem to suggest that modal nominalists cannot unproblematically claim to *know* that various modal assertions are true, but the problem is primarily metaphysical, rather than epistemic: The nominalist seems unable to justify the modal assertions she makes because she cannot in the first place state the truth-conditions for these assertions. In contrast, via the model- or set-theoretic reduction of the logical modalities, Shapiro is capable of stating the truth-conditions for assertions about what is logically possible.

Does the fact that the set-theoretic reduction can provide truth-conditions for assertions about what is logically possible show that these assertions are justified for those who endorse the set-theoretic reduction? For insight on how to approach this question I turn first to the most well-studied reduction of modality—David Lewis’s modal realism, which reduces the metaphysical modalities to possible worlds.

Lewis’s modal realism envisions a pluriverse of spatiotemporally disconnected universes which is so vast that any way that a universe could be is a way that some universe is. These universes are Lewis’s possible worlds, and Lewis analyses metaphysical necessity as what is true in every one of these worlds, and metaphysical possibility as what is true in at least one of these worlds. Lewis’s reduction is of interest because it constitutes a clear example of the idea that a reductive theory of modality, solely in virtue of its reductive character, does not provide the means for justifying assertions about what is metaphysically necessary or possible. This raises the prospect of applying similar reasoning in support of the claim that the set-theoretic reduction of the logical modalities, solely in virtue of its reductive character, does not provide the means for justifying assertions about

what is logically necessary or possible.

Lewis does give some indication of how he thinks it is possible to justify modal assertions. He claims that commonsense modal intuitions are perfectly justified as they come and that these intuitions are consequences of a principle of recombination (the idea that, for any two Humean distinct existences, there is a possible world containing those two existences). Commonsense modal intuitions, together with the principle of recombination, provide a window into Lewis's pluriverse of possible worlds. But the reductive portion of Lewis's overall theory of modality—the analyses of metaphysical necessity and possibility in terms of spatiotemporally disconnected universes—serves only as a *rewrite* rule for translating modal sentences into world sentences, and vice versa. The analyses themselves confer no justification upon lone modal assertions or upon lone world assertions. On Lewis's view, commonsense modal intuitions and other various accoutrements do all of the work justifying modal assertions (and thus, under translation, justifying world assertions). Thus, the fact that Lewis's theory of modality is reductive has nothing at all to do with whether it is possible to justify modal assertions under Lewis's overall theory of modality.

What has this to do with the set-theoretic reduction of the logical modalities? The set-theoretic reduction of the logical modalities views the set-theoretic hierarchy as providing the resources for grounding or constructing all possible models. Though set theory is only capable of functioning as a *representational device* concerning certain logical possibilities (e.g., one would not *identify* the logical possibility that Larry is a fishmonger with a pure set), it is nevertheless possible to view set theory as providing the truth-conditions for assertions about the logical consistency of mathematical theories and about the possible existence of mathematical objects.

The main point from my assessment of Lewis's reduction applies with little modification: The set-theoretic analyses of logical necessity and possibility in terms of models serve only as rewrite rules for translating modal sentences into set existence claims, and vice versa, and as such confer no justification upon lone modal assertions or upon lone set

existence claims. Where does this place Shapiro's objection to modal nominalism? Much seems to turn on exactly what it means to *justify* a modal assertion.

If the complaint is that modal nominalists, *qua* modal primitivists, cannot justify the modal assertions they make, in the sense that they cannot determine what it is that *makes* these assertions true, then I am happy to concede the point to Shapiro. Only I would point out that Shapiro is equally plagued by this problem (to whatever degree it is problematic)—Shapiro is after all required to justify *coherence* claims, and his notion of coherence is itself a modal primitive that is indistinct from a primitive notion of logical possibility.

But on the other hand, perhaps the complaint is that modal nominalists, *qua* modal primitivists, cannot justify modal assertions in the sense that they lack compelling reasons for believing that such assertions are true. But to say this would unveil Shapiro's *ante rem* structuralism as special-pleading. Shapiro claims that it is an uncontroversial *presupposition* of mathematics that set theory is coherent. Modal nominalists are certainly not blind to the presuppositions of mathematics—such presuppositions are open to all if they are open to anyone. Given the lack of any important distinction between Shapiro's notion of coherence and a primitive notion of logical possibility, the alleged mathematical presupposition that set theory is coherent can be appropriated by the modal nominalist as sufficient evidence for believing that set theory is logically possible. Thus, if Shapiro is justified, via mathematics, in believing that set theory is coherent, then modal nominalists are justified, via mathematics, in believing that set theory is logically consistent. Shapiro's "epistemic" criticism therefore ends in a wash. The modal nominalist is in no worse of a position than Shapiro when it comes to justifying modal assertions.

Let me recap. The modal nominalist eschews commitment to abstract objects by taking on the burden of an increased ideology of primitive modal notions. While this obviates the metaphysical and epistemological problems invoked by the postulation of mathematical objects, it would appear to raise new problems on the ideological front. The modal nominalist seems obligated to explain why it is, that, for example, she is justified

in believing that set theory is consistent or possible, and it is by no means clear that this is an improvement. In the case of Hellman's view, the problem is to justify assertions about the primitive logical possibility of the existence of models of mathematical theories. For Field, the problem is to justify assertions about the primitive logical consistency of conjunctions of axioms of mathematical theories. And for Chihara, the task is to provide an account of the metaphysical modalities that justifies the constructibility assertions that figure in his type-theoretic recovery of mathematical reasoning. Nevertheless these problems are not, in the end, especially problematic for modal nominalists. The inability of the set-theoretic reduction to provide a means for justifying assertions about what is logically possible shows that Shapiro's reliance on set theory ultimately provides no justificatory benefits over treating such assertions in a face-value, primitive manner. It follows that Shapiro cannot claim to reside in a position of superiority when compared to modal nominalism on point of justifying modal assertions—there is no avoiding the kind of justificatory questions that get raised under modal primitivism. Thus Shapiro's Challenge is not uniquely problematic for modal nominalists.

IV Naturalism

The philosophical orientation of naturalism is advocated by many philosophers, and indeed, by many philosophers of mathematics. There is some controversy over just what being a naturalist amounts to; every philosopher that calls herself a 'naturalist' seems to mean something slightly (or not-so-slightly) different by the term. Therefore it would be pointless to seek out the one "true" naturalism. Nevertheless, the fact remains that two influential advocates of naturalism have advanced characterizations of scientific and mathematical method that appear to conflict with modal nominalism. These two influential naturalists are John Burgess and Penelope Maddy.

It might at first seem ironic that anyone should think that nominalism (including modal nominalism) is inconsistent with naturalism. Is not nominalism partly motivated

to account for mathematical reasoning and the scientific applications of such reasoning in ways that do not utilize the kinds of mysterious epistemic faculties often present in non-nominalistic accounts of mathematics (e.g., under Gödel's platonism)? And is not nominalism also partly motivated by the idea, present in scientific methodology, that one should prefer simpler theories? Are these not in some sense *scientific* reasons for pursuing nominalism? In a sense, yes. But in another sense, no. One might worry that these are only pseudo-scientific platitudes that arise from a dangerously coarse-grained perspective of science and scientific methodology. Both Burgess and Maddy advance something like this worry as a naturalistic criticism of nominalism. Each is a naturalist in the Quinean tradition. That is, they both share Quine's belief that science is the ultimate arbiter of questions of existence. However, they disagree quite extensively on the details.

Though not treated in this dissertation, a few remarks on Quine's naturalism are in order.⁴ Quine's contention is that the evidential standards of science are the best means available for acquiring knowledge about the world. Thus Quine repudiates the Cartesian "dream" of finding an indubitable rational basis for all knowledge. On this view science—i.e., *natural* science—is only to be criticized through the use of its *own* methodology. What are the evidential standards implicit in scientific methodology? They are captured by the well-worn theoretical virtues (or criteria of theory selection): simplicity, familiarity, scope, fecundity, and agreement with observation. The mathematical existence debate is no exception for Quine; nominalism and platonism are both subject to acceptance or rejection based on whether and to what degree they possess the theoretical virtues.

Quine, initially sympathetic to nominalism, eventually came to reject the view and to admit the existence of mathematical objects. Two of Quine's other doctrines play an ineliminable role in explaining why he came to adopt platonism; his holism and his views on ontology. Quine's holism holds that scientific theories are only ever confirmed in their entirety. He includes applied mathematics in his best overall theory of the world. Indeed,

⁴Here and in the next paragraph I draw from (Maddy 2005b).

he is convinced that mathematics is *indispensable* to the best theory. Therefore applied mathematics gets confirmed alongside scientific theories. According to Quine's criterion of ontological commitment, a theory is ontologically committed to the ranges of its bound variables. That is, if a theory quantifies over xs , then that theory is ontologically committed to xs . Quine believes that one's ontology is determined by one's best overall theory of the world. Since the best overall theory involves mathematics, and since mathematical assertions quantify over mathematical objects, the naturalist is ontologically committed to mathematical objects.⁵

Each aspect of Quine's position has been criticized *extensively*. And although I too find many aspects of it disconcerting, I have only described the view so that it might serve as a backdrop for the ensuing discussion of the naturalisms of Burgess and Maddy. Both views are outgrowths of Quine's naturalism, but each departs from it in important ways. And each has its own unique reasons for objecting to modal nominalism, and in chapters four and five I respond to them in turn.

IV.1 Reflections on Burgess

Burgess departs from Quine on the status of unapplied mathematics. For Quine, much of unapplied mathematics is mere recreation. But for Burgess, mathematics—*every last bit* of mathematics—is a part of the best scientific picture of the world. Thus the success of science points to the truth not just of the mathematics that is applied in physical theories, but of pure mathematics as well.

Burgess's (2008b) criticism of nominalism (modal and non-modal alike) maintains that the view is unscientific, and hence inconsistent with the naturalist orientation in philosophy. On his view, the nominalist can be understood as attempting one of two projects. In the first case, the nominalist could be advancing a view about the correct understanding of mathematical language. Perhaps the surface syntax of mathematical

⁵This paragraph is an outline of Quine's Indispensability Argument for the existence of mathematical objects.

language quantifies over mathematical objects, but this need not imply that, deep down, mathematical assertions are *really about* numbers and functions. By providing novel interpretations of mathematical assertions the nominalist hopes to uncover the *true* content of mathematics. Burgess calls this the *hermeneutic* project. In the second case, the nominalist could be advancing a view about the methodology of science. In reformulating physical theories in ways that avoid commitment to mathematical objects she hopes to create a superior, less ontologically burdensome account of the world. Burgess calls this the *revolutionary* project.

Burgess argues that both projects are unscientific. The hermeneutic project is unscientific because there is no good linguistic evidence that the nominalist's interpretations capture the "true" content of mathematical assertions. The revolutionary project is unscientific because there is good reason to think that the scientific community would openly reject the nominalized versions of physical theories. Since these are the only two options available to the nominalist, her peculiar aversion to mathematical objects must be thrown out as an unscientific (and hence non-naturalistic) dogma.

The obvious reply to make is that none of the nominalist projects defended in this dissertation can be accurately described as either hermeneutic or revolutionary. But this reply will not hinder Burgess. Whatever are the best ways to describe nominalist projects, including modal nominalist projects, Burgess can always offer the fact that scientists are not engaging in the project of nominalization as good evidence that the eschewing commitment to mathematical objects is not a going scientific concern and so is not a principle of scientific methodology. Nominalism, including modal nominalism, is thereby guilty of being unscientific.

In response I argue that Burgess's account of what it is to be "scientific" is too vague to be of any help to him. Burgess gives very little in the way of a positive characterization of what his naturalism commits him to. He is clear in holding that one is being unscientific when one openly adopts a methodological principle that runs contrary to

one that is uniformly accepted by the scientific community. But it is not clear that this implies (as Burgess assumes) that one is thereby also being unscientific when one adopts a methodological principle that, though it is not explicitly used by the scientific community, nevertheless does *not* run contrary to any of those that are uniformly accepted by the scientific community. Modal nominalist philosophies of mathematics are designed not to interfere with the day-to-day practice of science and mathematics. That in addition they seek to eschew commitment to mathematical objects is not evidence that they are unscientific in any damaging sense of the word.

Another problem for Burgess emerges from this discussion. Burgess is a self-described *moderate* platonist. Moderate platonism is the view that when scientists and mathematicians accept, without reservation, assertions that appear to quantify over mathematical objects, that scientists and mathematicians thereby *accept* or acquiesce to the existence of mathematical objects. But if scientifically acceptable evidence is required for eschewing commitment to mathematical objects, then by parity of reasoning it would appear that assenting to commitment to mathematical objects similarly requires scientifically acceptable evidence. I argue that Burgess's moderate platonism adopts methodological principles about language and ontology that are not matters of going scientific concern. It follows that Burgess's platonism is self-undermining. One cannot at the same time accuse modal nominalism of being unscientific and fail to realize that a similar fate befalls platonism.

IV.2 Reflections on Maddy

Maddy's departure from Quine is more extreme.⁶ She holds with Burgess that Quine's naturalism does not do justice to pure mathematics, but she goes further in rejecting Quine's holism. Several factors motivate her to abandon this Quinean doctrine. One factor arises from a case study of the ontological status of atoms in the period before Perrin's experiments were widely known. The postulation of atoms enjoyed all of the theoretical virtues, and yet many scientists persisted in regarding their existence as a

⁶Here again I draw from (Maddy 2005b).

fiction. For Maddy this is evidence that the mere enjoyment of the theoretical virtues does not necessarily track the standards of warrant that scientists actually employ. A second factor involves the recognition that scientific applications of mathematics always involve some degree of idealization. For instance, the patently false assumption that fluids are continuous is often indispensable for making predictions, but this moves no one to believe that fluids are actually continuous. Maddy concludes that scientists do not treat all theoretical postulations as on a par. A third factor is Maddy's observation that science seems not to be done as though the existence of mathematical objects is at stake. This evidence does indeed suggest that the thesis of holism is misguided and that, even if it finds a more limited role elsewhere as a principle of scientific methodology, it should not be employed to confirm the existence of mathematical objects.

Maddy's naturalism, also known as Second Philosophy, embraces Maddy's entreaty that mathematics should be both *understood* and *evaluated* on its own terms. The entreaty to *evaluate* mathematics on its own terms comes to the idea that mathematical results and methods should be criticized and supported *only* on through the use of actual mathematical methods. That is, mathematical methods and results are immune from non-mathematical forms of criticism. The entreaty to *understand* mathematics on its own terms involves two somewhat stronger notions. First is the idea that metaphysical theorizing about mathematics must be consistent with the idea that mathematical forms of justification are themselves sufficient for justifying mathematical claims. Thus, Maddy's naturalist will reject theories of mathematics that introduce a justificatory gap between mathematical forms of justification and metaphysical analyses of mathematical claims. Second is the idea that metaphysical theorizing about mathematics must be *constrained* by the methodology of mathematics. Maddy's naturalist will reject metaphysical theories of mathematics that are not fully endemic to the methodology of mathematics.

Maddy's own position is that mathematical practice is constrained by the facts of mathematical depth—a collection of purportedly objective facts that serve to distinguish

the mathematical interesting and productive theories and concepts from their mathematically sterile cousins. These facts are purported to comprise the underlying reality or subject-matter of mathematics. On this position, mathematical forms of justification (which, Maddy claims, are responsive to considerations of depth or fruitfulness) suffice for justifying claims about what concepts and theories are mathematically deep, and so these forms of justification suffice for justifying mathematical claims. Further, this position is endemic to mathematics in the sense that, at least according to Maddy's assessment of the discipline, the facts of mathematical depth constrain mathematical practice, and also in the sense that mathematical methods are used to reveal these facts.

It stands to reason that Maddy's naturalist will object to modal nominalism on the grounds that it violates her entreaty to *understand* mathematics on its own terms. This involves pursuing two related criticisms. First, it is not a built-in feature of modal nominalism that mathematical forms of justification suffice (as Maddy insists) for justifying claims about what concepts and theories are mathematically deep, and it seems probable that there is a non-trivial justificatory gap between mathematical methods and the modal nominalist's metaphysical interpretation of mathematical claims. And second, it would seem that modal nominalism, given that it is nominalistic in the secondary sense of §II.2, requires extramathematical resources and motivation—presumably the desire to *eschew* commitment to mathematical objects is not a going item on the agenda of mathematical practice. Thus modal nominalism is not endemic to mathematics. The primary goal of chapter five is to assess to what extent these objections are damaging to modal nominalism. But since Maddy has not directly treated modal nominalism, I am forced to turn to her objections to the views she takes to be inconsistent with her naturalism, as well as to the arguments she gives in support of the views she takes to be consistent with her naturalism, in order to evaluate whether it is a serious objection to modal nominalism that it violates Maddy's entreaty to understand mathematics on its own terms.

Against the criticism that modal nominalism does not confirm the idea that math-

emational forms of justification are sufficient for justifying mathematical claims, I show, using Maddy's own examples, that this is only a Second-Philosophically objectionable diagnosis for views that (a) introduce a non-trivial justificatory gap between mathematical methods and the proffered metaphysical analyses of mathematical claims, and for views that preclude the possibility of both (b) acknowledging that considerations of fruitfulness provide an objective constraint on mathematics, and (c) acknowledging that mathematical forms of justification suffice for justifying claims about what concepts and theories are mathematically deep. I argue that modal nominalism is objectionable in none of these ways. Concerning (b) and (c), nothing precludes amending modal nominalism so as to embrace the idea that mathematical justification responds to objective considerations of depth—it is possible to account for these things without making the added claim that the facts of mathematical depth comprise *the* underlying reality of mathematics. Concerning (a), I argue that Maddy's depth-based account of mathematics and modal nominalism are all equally wanting *a propos* of an account of logical possibility. Such an account either will or will not be endemic to mathematics (an observation that is used in my response to the second criticism of modal nominalism). Either way, Maddy's depth-based account and modal nominalists accounts are helped (or harmed) equally.

Against the criticism that modal nominalism is not endemic to mathematical methodology, I argue that it is in general *not possible* to provide a coherent account of mathematics that is excised of all non-mathematical resources. As Maddy herself acknowledges, mathematical practice is constrained by the requirement of logical consistency. If mathematical practice is not in the business of justifying assertions about what is logically consistent or what is logically possible, then that modal nominalists must go beyond mathematics to justify assertions about what is logically possible cannot be a decisive reason for rejecting modal nominalism, because *everyone* must go beyond mathematics to justify these assertions. (Of course, if it *is* part of mathematics to justify assertions about what is logically possible, then it is not clear that modal nominalism fails to be contained or sanctioned by

the methods of mathematics).

That it appears implausible to provide a coherent account of mathematics that is excised of all non-mathematical resources suggests that Maddy's entreaty to *understand* mathematics on its own terms unveils Second Philosophy as an implausibly strong form of naturalism. Indeed, the entreaty itself does not appear to be a product of mathematics but is instead a philosophical proposal about mathematics and its methodology. It would be special-pleading to insist that Maddy's entreaty is the *only* philosophical claim that naturalists are permitted to make about mathematics. Thus, although I grant that modal nominalism is inconsistent with Second Philosophy (because it is not endemic to mathematics), nevertheless I do not think that modal nominalism is thereby objectionable—since modal nominalism is only incompatible with the strongest and least plausible aspects of Second Philosophy. I close the chapter (and dissertation) with some brief thoughts on why modal nominalism is compatible with other forms of naturalism that stop short of embracing the idea that metaphysical accounts of mathematics must be constructed and motivated using *only* internal mathematical resources.

V Conclusions

The use of modality in modal nominalist philosophies of mathematics is thought to produce intractable difficulties regarding the justification of modal assertions. Nevertheless these difficulties can be seen to be shared by platonists and indeed by all individuals tasked with justifying assertions about, e.g., which mathematical theories are coherent or logically consistent, which mathematical structures possibly exist, etc.

The philosophical orientation of naturalism is thought reveal the modal nominalist's ambitions as unscientific and therefore objectionable. Burgess's naturalism depends on an understanding of what it is to be "scientific" that at best is too vague to pronounce against modal nominalism, and at worst is self-undermining. Maddy's focus on the methodology of mathematics fails to betray modal nominalism as in any kind of conflict

with mathematical practice, even if her naturalism is capable of sustaining the judgment that modal nominalism is not endemic to mathematics. Of course, Maddy encounters difficulties in explaining why it is objectionable when an account of mathematics fails to be endemic to the methods of the discipline.

The upshot is the establishment of modal nominalism as a tenable nominalist approach to the philosophy of mathematics. But can anything be said in the way of arguing *for* nominalism in the primary sense of §II.2? Though I make no concerted effort to construct any such arguments, I can at least relay a *prima facie* justification for nominalism, which relies on considerations of overall plausibility. Philosophical theories are selected in part because of their ability to solve or avoid philosophical *problems*. If two theories T_1, T_2 share a common set of unresolved philosophical problems $\{P_1, \dots, P_n\}$, but where T_1 has additional problems $\{Q_1, \dots, Q_n\}$, while T_2 has no additional problems, as a general rule of thumb T_2 is preferable to T_1 . I would like to suggest here that something like this situation obtains with platonism as T_1 and nominalism as T_2 . Nominalism and platonism both have unresolved issues concerning how best to account for the practice of mathematics. Both have unresolved issues concerning how to account for the applications of mathematics in physical theories.⁷ And both views must provide an account of how it is possible to justify modal assertions. However, platonism is saddled with additional unresolved problems stemming from the postulation of abstract mathematical objects. Hence modal nominalism has fewer philosophical problems, and is thereby more likely to be true when compared to platonism.

⁷The matter of application is particularly delicate. Nominalists like Field completely reconstruct physical theories in qualitative terms. Others suggest that the mathematical apparatus of platonistic physical theories are fictional, while remaining realists about the “nominalistic content” of such theories. Structuralists maintain that physical objects exemplify mathematical structures. Traditional platonists believe that mathematical truths apply directly to physical phenomena. I am not suggesting that all of these views encounter the *same* problem of application. Intuitively the challenge to the traditional platonist is greater than the challenge to the structuralist. Someone like Field is arguably less burdened than someone who utilizes an under-articulated notion of “nominalistic content.” Broad generalizations aside, whether some nominalistic theory is superior to some platonistic theory must be decided on a case-by-case basis. Examining the subtleties of all of the various meters of success of philosophical accounts of mathematics is beyond the scope of this dissertation.

Part 1: Modality

There are logicians, myself among them, to whom the ideas of modal logic (e.g. Lewis's) are not intuitively clear until explained in non-modal terms.

—W.V. Quine.

The whole idea of possible worlds (perhaps laid out in space like raisins in a pudding) seems ludicrous.

—Larry Powers

Chapter 1

Modality in the Philosophy of Mathematics

1.1 Introduction

In this chapter I examine in varying detail the three major modal nominalist projects in the philosophy of mathematics—Charles Chihara's Constructibility Theory, Geoffrey Hellman's Modal Structuralism, and Hartry Field's fictionalism, discussed in sections two, three, and four (respectively). What is of interest is that each of these views explicitly utilizes various kinds of modal assertions to do critical work in eschewing commitment to mathematical objects. For instance, Chihara argues that mathematical reasoning can be carried out without reference to mathematical objects by developing a type-theoretic framework which replaces Russell's propositional functions with assertions about the *constructibility* of open sentence tokens. Hellman and Field each argue that the content of mathematical theories consists in modal assertions to the effect that various theories or structures are consistent or possible.

Given that each of these three views is vitally involved with modality, it is natural to wonder whether such involvement constitutes progress toward producing a coherent philosophical comprehension of mathematics. I show that there are plausible reasons for supposing that modal nominalist theories do not represent an increase in epistemic tractability over traditional platonist accounts of mathematics. A concern of note is that modal nominalists rely on assertions about what is *primitively* necessary or possible: They do not offer any reductive analyses of the modal notions they employ, which obscures the content of the modal assertions modal nominalists make. This raises a question about how modal nominalists propose to justify claims about what is primitively necessary or possible. It is far from clear that the modal nominalists are any more justified in making modal assertions (e.g., asserting the primitive logical possibility of the conjunction of the axioms of ZFC) than the platonist is justified in making existence assertions (e.g., asserting the existence of a model satisfying the axioms of ZFC). And the possibility is also raised that modal nominalists are actually in a *worse* justificatory position *a propos* of modal assertions when compared to platonists *a propos* of existence assertions.

Although I maintain that the modal nominalist is not in a worse justificatory position when compared to the platonist, I will not be in a position to fully justify this claim until the end of chapter three. So for the purposes of this first chapter I am content to note that modal nominalism raises questions about justifying modal assertions that are, potentially, at least as serious as the questions the platonist faces about justifying existence assertions: How does one come to know that various mathematical theories are possible or consistent? How can one *explain* why certain theories are consistent, while others are not, all the while utilizing a primitive notion of consistency? Questions of this tenor form the basis for the elaborate objection Stewart Shapiro constructs against modal nominalism, which I discuss in detail in the second chapter, with my reply occurring over the space of chapters two and three.

1.2 Chihara's Constructibility Theory

Chihara (1990) develops a philosophy of mathematics that is designed to avoid what he calls “the problem of existence in mathematics.” Chihara writes that this is the problem of, “how, in short, [we are] to understand existence assertions in mathematics” (ibid., 3). According to platonism, existence assertions in mathematics are understood as ordinary kinds of existence assertions: When one claims that ‘there exist three prime numbers greater than 7 but less 19’ one is in no uncertain terms asserting the *existence* of a certain set of abstract objects (the numbers 11, 13, and 17). But this gives rise to an epistemological question: If numbers are abstract objects—that is, objects that exist outside of space and time—then, Chihara asks, “how can the mathematician know that such things exist?” (ibid., 5). But why suppose that being unable to *know* that numbers actually exist is problematic? Chihara explains,

We seem to be committing ourselves to an impossible situation in which a person has knowledge of the properties of some objects even though this person is completely cut off from any sort of causal interaction with these objects. And how does the mathematician discover the various properties and relationships of these entities that the theorems seem to describe? By what powers does the mathematician arrive at mathematical knowledge? In short, *how is mathematical knowledge possible?* (ibid., emphasis added)

Thus, platonism is in epistemological debt. Chihara aims to avoid this burden by showing how it is possible to carry out mathematical reasoning in a system that does not quantify over mathematical objects.

1.2.1 The Constructibility Theory

Chihara's Constructibility Theory is inspired by simple type theory. The type theoretic approach to mathematics was first promulgated by Bertrand Russell in the wake of his discovery of the famous paradox that bears his name. In type theory, mathematical existence assertions are associated with claims involving individuals and propositional functions. Constructibility Theory is a variant of type theory in which Russell's propositional func-

tions are replaced with assertions about the constructibility of various open-sentence tokens. The upshot is that the existence assertions of ordinary mathematical languages can be replaced with assertions about the *constructibility* of open-sentence tokens. It is important to note, however, that Chihara does not intend his Constructibility Theory to provide an analysis or account of the ultimate meaning of mathematical assertions. Again, his interest is in *bypassing*—not in *solving*—the various problems that, according to him, undermine the platonist’s view of mathematics.

According to Chihara, instead of asserting that ‘there exists a number satisfying φ ,’ one can say instead that, ‘it is possible to construct an open-sentence ψ satisfying ϕ ,’ where the open-sentence property ϕ is a structural analog of the mathematical property φ . Allegedly, and in contrast with ordinary existence assertions, constructibility assertions are not ontologically committing. Sentences such as, ‘One of Quine’s anthologies is on the sofa,’ are true only if there exist things like *sofas* and *anthologies* of Quine’s work. But the truths of sentences such as, ‘It is possible to construct a sofa capable of holding one of Quine’s anthologies,’ do not require the existence of any sofas or of any anthologies.¹

The theoretical framework of Constructibility Theory is essentially that of first-order modal quantification theory, but designed to include what Chihara calls *constructibility quantifiers* in place of the modal operators of necessity and possibility. I will here only describe the syntactic and semantic features of Constructibility Theory that are *unique* to it.

Regarding syntax, the constructibility quantifiers **C**, **A**, function in much the same way as do the existential and universal quantifiers \exists, \forall . Let φ be the open-sentence ‘ x is a Quine anthology on the sofa.’ Then $(\mathbf{C}x)\varphi$ says that it is possible to construct something x such that x is a Quine anthology on the sofa. Similarly, $(\mathbf{A}x)\varphi$ says that it is always possible to construct something x such that x is a Quine anthology on the sofa. In this way there is a

¹Perhaps coming to understand what such claims *mean* requires becoming familiar with particular sofas and Quine anthologies, proceeding to form or apprehend general concepts under which these items fall, and then learning how to determine, in the abstract, what it would take for an object to fall under these concepts. Perhaps in this procedure abstract objects enter the fray at some point. *Perhaps*—but perhaps not. This issue falls beyond the purview this dissertation.

functional correlation between the quantifiers \mathbf{C} and \exists , as well as between the quantifiers \mathbf{A} and \forall . The constructibility quantifiers are introduced syntactically as follows: If φ is a formula and α is a variable, then $(\mathbf{C}\alpha)\varphi$ is a formula. And, if φ is a formula and α is a variable, then $(\mathbf{A}\alpha)\varphi$ is a formula. Sentences are restricted to those formulas containing no free variables.²

In order to provide a semantics for the constructibility quantifiers Chihara develops a modified version of modal quantification theory. He first identifies a \mathbf{K} -interpretation as an ordered quadruple $\langle \mathbf{W}, \mathbf{a}, \mathbf{U}, \mathbf{I} \rangle$ where (simplifying slightly) \mathbf{W} is a non-empty set, $\mathbf{a} \in \mathbf{W}$, and \mathbf{U} assigns a non-empty domain to each member of \mathbf{W} . \mathbf{I} is a function that assigns to constants members of the union of all $\mathbf{U}(\mathbf{w})$, where $\mathbf{w} \in \mathbf{W}$. \mathbf{I} also assigns extensions to predicates. Chihara notes that in this system, both constants and predicates are treated as rigid—that is, held fixed across all members of the domain \mathbf{W} (ibid., 28).³

Let both $\mathbf{M} = \langle \mathbf{W}, \mathbf{a}, \mathbf{U}, \mathbf{I} \rangle$ and $\mathbf{M}' = \langle \mathbf{W}', \mathbf{a}', \mathbf{U}', \mathbf{I}' \rangle$ be \mathbf{K} -interpretations with β an individual constant. If \mathbf{M}' differs from \mathbf{M} only in what it assigns to β , call \mathbf{M}' a β -variant of \mathbf{M} . Suppose α is a variable, β an individual symbol, and ψ a formula. Then $\psi_{\alpha/\beta}$ is the result of replacing with β all free occurrences of α in ψ . The semantics for the quantifiers \mathbf{C} and \mathbf{A} can now be given. For $\varphi = (\mathbf{C}\alpha)\psi$, φ is true just in case $\psi_{\alpha/\beta}$ is true under at least one β -variant of \mathbf{M} . For $\varphi = (\mathbf{A}\alpha)\psi$, φ is true just in case $\psi_{\alpha/\beta}$ is true in every β -variant of \mathbf{M} .⁴ The typical notions of consequence, validity, etc., are defined in relation to the \mathbf{K} -interpretations.

On the intuitive picture, \mathbf{W} is a set of possible worlds, \mathbf{a} is the actual world, \mathbf{U} is a function that assigns domains of sets of individuals to possible worlds, and \mathbf{I} assigns, in each world, extensions to predicates. Chihara is quick to point out, however, that the intuitive picture is *only* a picture:

²For the full treatment, see (ibid., 24-7).

³Predicate rigidity involves a predicate picking out the same property in every model (or possible world). This should not be confused with the idea of a predicate picking out the same *extension* in every model (or possible world).

⁴For the full treatment, consult (ibid., 27-39), with important amplifications on (ibid., 55-8).

... the above appeal to possible worlds was made to relate the constructibility quantifiers to familiar and heavily studied areas of semantical research. I, personally, do not take possible world semantics to be much more than a useful device to facilitate modal reasoning. Still, I hope to convince most philosophers by means of such analyses that the predicate calculus I shall be using is at least consistent and that it has a kind of coherence and intelligibility that warrants the study of such systems. (ibid., 38)⁵

Anyone familiar with modal quantification theory will recognize the structural similarities between the introduction of the modal operators for necessity and possibility and Chihara's constructibility quantifiers. However, as Chihara is correct in pointing out, the adoption of the logical framework commonly used in constructing modal logics does not, in itself, involve commitment to possible worlds in any metaphysically significant way.

In restricting the arena of discourse to open-sentence tokens, Chihara is able to develop an omega-sorted type theory in which level-0 entities are ordinary, nominalistically acceptable objects, and level- $(n + 1)$ entities are open-sentence tokens satisfiable by level- n entities. It is the objects of level- $(n_{\geq 1})$ over which the constructibility quantifiers range. He describes how to capture cardinality theory, number theory, and real analysis using his system.⁶ Cardinality theory is captured by associating level-2 objects with "cardinality attributes." For instance, a level-2 object Z is a zero-attribute just in case it is satisfiable by a level-1 object that is not satisfiable by any level-0 objects. Higher cardinalities are attained by defining hereditary relations on cardinality attributes. Number theory is then captured by proving cardinality attribute-correlates of the Peano Axioms. Real analysis can be captured just as in ordinary type theory, but again with assertions about the constructibility of open-sentence tokens replacing Russell's propositional functions. Rational numbers can be defined as higher-level open-sentences satisfiable by equivalence classes of pairs of cardinality attributes, and real numbers as Dedekind-cuts of rationals, etc.

Chihara's contention throughout is that, in principle, all existence assertions in math-

⁵Chihara would later see the need to write an entire book to justify this claim, his (1998). More on this shortly.

⁶For the details of cardinality and number theory, see (Chihara 1990, ch. 5). For analysis, see (ibid., ch. 6).

ematics can be replaced by ontologically neutral constructibility assertions.⁷ Thus he eschews commitment to mathematical objects via modality.

1.2.2 A Closer Look at Modality in Constructibility Theory

Having described the basic working parts of Constructibility Theory, it should be helpful to examine in greater detail how Chihara’s use of modality is alleged to eschew commitment to mathematical objects. What does it mean to say that it is possible to construct an open-sentence token? What kind of “possibility” must one invoke to account for the constructibility of open-sentence tokens? Is it appropriate to regard the constructibility quantifiers as ontologically neutral? And does Constructibility Theory represent a metaphysical and epistemological improvement over platonism? Let me address these questions in turn.

What does it mean to say that it is possible to construct an open-sentence token? According to the “intuitive picture” described above,

... to say that it is possible to construct an open-sentence of a certain sort is to say that in some possible world there is a token of the type of open-sentence in question. But what would it be for some token of some open-sentence type *to exist in a possible world*? Here, we can imagine a possible world in which some people, who have an appropriate language, do something that can be described as the production of the token: they may utter something, write something down, or even make some hand signals (Chihara 1990, 40).

So on the “intuitive picture,” what it means to say that it is possible to construct an open-sentence token is just that there exists a possible world in which this open-sentence token has been constructed. Of course, Chihara resists claiming that what transpires within this “intuitive picture” is really what it means to say that it is possible to construct an open-sentence token. If, then, one cannot depend on the “intuitive picture” to illuminate what it means to say that it is possible to construct an open-sentence token, what else is there to say? One might argue that what underpins the constructibility of open-sentence tokens is akin to what grounds assertability in intuitionistic settings—that there is available

⁷Cf. (Jacquette 2004).

(or describable) an effective procedure for constructing a particular open-sentence token. But Chihara regards effective procedures as unduly restrictive (*ibid.*, 45-7).

In fact, Chihara has quite little to say in the way of a positive account of what it means to say that it is possible to construct an open-sentence token. Most of what he has to say is negative:

... ('It is possible to construct an open-sentence ϕ such that ϕ satisfies ψ ') does not mean that *one knows how* to construct such an open-sentence or that one has a method for constructing such an open-sentence. Hence, the constructibility quantifier is not at all like the intuitionist's existential quantifier. Furthermore, it does not mean that one can always, or even for the most part, determine what particular objects would satisfy such an open-sentence... (Chihara 2004, 172)

Moreover, he is quite willing to deflect further inquiry into the nature of open-sentences and satisfiability,

... my Constructibility Theory is not designed to give us information about how to tell what is an open-sentence or what things satisfy any given open-sentence. It is just not that sort of theory. It has been designed to provide us with a way of understanding and analyzing mathematical reasoning, in a way that does not presuppose the existence of mathematical entities... (*ibid.*)

Chihara can perhaps be forgiven for avoiding a protracted discussion of the nature of open-sentences and the nature of satisfiability—these are indeed well-studied concepts and it seems there is little to be gained by examining them in the present setting. However, the constructibility quantifiers are the foundation of Chihara's project, and it is startling, to say the least, that he seems unwilling to directly and positively characterize what it means to say that it is possible to construct an open-sentence token. David McCarty presses this point quite forcefully:

For good or ill, I don't have much in the way of native ken when it comes to possible constructible sentence tokens. I don't have at my disposal a rich concept of unactualized sentence forms from which to work. There is no 'folk theory' of them that I know of. There is little or nothing of interest I am willing to assert with confidence about possible sentence forms... To remind us that Chihara's constructibles are nonexistent only makes matters worse... (1993, 260)

To better understand the significance of these remarks, recall that Chihara's principle motivation is to avoid "the problem of existence in mathematics," which is understood in part as the problem of how to *understand* existence assertions in mathematics. Chihara does not pretend to be offering an analysis of what mathematical existence assertions really mean; instead he thinks he can avoid the problem by exchanging existence assertions for *constructibility* assertions. But this would appear to give rise to a *new* problem—that of how to *understand* constructibility assertions. Is this new problem worse than the original? I shall (eventually) argue that it is not, but for the moment I want to continue in the exploration of modality in Constructibility Theory.

A few brief remarks about the different "kinds" of possibility are in order, if only to give context to my analysis of what kind of possibility Constructibility Theory requires. In the metaphysics of modality, it is common to draw a distinction between a number of different senses of the term 'possible.' Standardly distinguished in such discussions are *logical* possibility, *metaphysical* possibility, and *nomological* possibility. To say that p is logically possible is to say that p does not lead to a contradiction in classical logic. To say that p is metaphysically possible is to say that p is consistent relative to the selection of metaphysical principles. To say that p is nomologically possible is to say that p is consistent relative to the selection of natural laws. According to the orthodox view, logical possibility properly includes metaphysical possibility, and metaphysical possibility in turn properly includes nomological possibility. Another way of understanding this is to say that logical possibility is basic and captures the "widest range" of possibilities, while holding that metaphysical and nomological possibility represent important kinds of restrictions on logical possibility.⁸

These distinctions are worth raising for a number of reasons. For my purposes, the most important reason is that in chapter three I shall be concerned with the question of whether reducing modality provides a means for justifying modal assertions. To the

⁸This way of distinguishing the different sorts of possibility is controversial. For a different perspective (in particular, on the issue of the proper placement of nomological possibility), see (Fine 2002).

extent that one is interested in reducing the logical modalities, set theory is thought to provide a reductive base. If Constructibility Theory ultimately relies on logical possibility, and logical possibility rests on set theory, then a potential circularity arises in which one eschews commitment to mathematical ontology via modality, but where modality is only explicable via mathematical ontology. To the extent that one is interested in reducing the metaphysical modalities, possible worlds are thought to provide a reductive base. If Constructibility Theory ultimately relies on metaphysical possibility, and metaphysical possibility involves commitment to possible worlds, then one faces the worry that in eschewing commitment to mathematical objects one must accept the existence of possible worlds.

Chihara provides *some* illumination into the kind of possibility he envisions while discussing the possibility of constructing open-sentence tokens:

The possibility talked about in the Constructibility Theory is what is called ‘conceptual’ or ‘broadly logical’ possibility—a kind of metaphysical possibility, in so far as it is concerned with *how the world could have been*. (2004, 171).

Thus Chihara is committed to the idea of it being *metaphysically* possible to construct the open-sentence tokens described in (Chihara 1990). He takes metaphysical possibility to be explicable in terms of representations of *ways the world could have been* (Chihara 1998, 289). But what, exactly, is a *way the world could have been*? And why suppose that such a thing is importantly distinct from a possible world? I hope to uncover answers to these questions while considering the issue of whether Chihara’s constructibility quantifiers are indeed ontologically neutral.

Chihara claims that his constructibility quantifiers are not ontologically committing:

The sentences of my theory do assert the possibility of constructing open-sentences, actual or possible. If I say that it is possible to construct a playground next to my house, I am not referring to, or presupposing the existence of, a possible playground. In this sense, I am not “appealing” to a playground that has never been constructed. (1990, 78).

And this much seems correct—to claim that it is possible for someone to *do* something

is not to assert that someone actually *does* something; nor is it to assert the existence of whatever might be produced were someone to engage in such an action. But analysts have taken issue with the fact that the constructibility quantifiers depend on an overtly modal framework—Chihara’s constructibility quantifiers are defined in what is essentially the modal quantificational system of **S5**. **S5** systems are usually understood as connecting to modal concepts by way of objects known as possible worlds (hence the association of modal semantics with possible worlds semantics). The constructibility quantifiers, defined as they are in an **S5** system, thereby appear to inherit commitment to possible worlds. In a review of (Chihara 1990), Jan Woleński makes just this point:

Chihara claims that his system is neither nominalistic nor Platonic. However, this standpoint raises serious doubts. Though Chihara does not need to appeal to ‘classical’ Platonic objects, his ontology is committed to possible worlds. Thus, one may restate his question in the following form: whether real human beings, living today and with the science we have today, ought to believe in the existence of possible worlds? Nominalists answer no; but Platonists say that it is much better to believe into [sic] the existence of mathematical objects. And the whole problem comes back. (1992, 234)

Similarly, Donald Gillies writes in his review:

... Chihara’s reduction, though successful, appears to allow in objects, namely: ‘possible worlds’, which are just as problematic as those which he seeks to eliminate. Indeed possible worlds seem to me, if anything, more problematic than abstract mathematical entities. (1992, 269)

He does indeed talk of possible worlds, but this is only a *didactic device* used to facilitate the presentation and clarification of the crucial ideas of the system. ... I do not regard this reply as altogether satisfactory, for it does seem to me that Chihara to make his system work in various respects (to introduce a non-denumerable set of reals for example) is relying on possible worlds as a foundation, and not merely as a heuristic, communication device. (ibid., 270)

Gillies and Woleński are each making two related criticisms. The first consists of the accusation that Constructibility Theory, despite Chihara’s brief remarks to the contrary, is indeed committed to possible worlds. The second is that Constructibility Theory’s commitment to possible worlds creates new, more troublesome problems in the metaphysics

and epistemology of modality. The first criticism, in itself, is not altogether bothersome—given that Chihara is interested in eschewing commitment to abstract *mathematical* objects, and given that possible worlds and mathematical objects are distinct kinds of entities, that Chihara is committed to possible worlds does not detract from the fact that he has eliminated commitment to mathematical objects.⁹ But Chihara is not only interested in eschewing commitment to mathematical objects. Recall again that “the problem of existence in mathematics” is how one is to understand existence assertions in mathematics, which is partly unpacked as the problem of explaining how mathematical knowledge is possible. Chihara seeks to avoid rather than solve this problem, by replacing mathematical existence assertions with the relevant constructibility assertions. But if constructibility assertions are ultimately elliptical for claims about possible worlds, then coming to know whether the relevant constructibility assertions are true would seem to require prior knowledge about what happens in various possible worlds. And here is where the second criticism exerts its leverage—whether anyone is justified in believing that possible worlds exist and in believing claims about what transpires within them are questions that are, *at best*, at least as quarrelsome as the questions that get raised when contemplating whether anyone is justified in believing that mathematical objects exist. In effect, Chihara’s “solution” raises a new problem—of how to justify constructibility assertions (as elliptical for claims about possible worlds)—that on the surface may be even more quarrelsome than the one he set out to avoid.

Chihara blocks the second criticism by contesting the first. If he can show that he is not, in the end, committed to possible worlds, then it would make little sense to complain that his system inherits the mysteries that surround them. Why suppose that Chihara is committed to possible worlds? Because Constructibility Theory is constructed using a modal logical system. However, he argues that there is no reason to suppose that the use

⁹Chihara writes that, “the fact that the modal notions may possibly carry hidden commitments to abstract entities should not preclude us from investigating the sort of constructivist theory developed in this work” (1990, 180). So, for better or worse, there is a sense in which he does not find this situation problematic.

of such systems involves commitment to possible worlds.

In (Chihara 1998), he goes to great lengths to show that he is able to use possible worlds semantics as a merely didactic device. His strategy is to provide a novel, Natural Language (NL) interpretation of modal semantics. The idea is to understand a statement p 's possibility as expressing that "the world could have been such that p was true," and to understand a statement p 's necessity as expressing that "no matter how the world could have been, it would have been such that p was true." What, specifically, does providing NL interpretations accomplish? For Chihara, formal or pure modal ("possible world") semantics is just a piece of uninterpreted mathematical structure (ibid., 260). Modal systems, by themselves, are unable to generate any substantive modal claims.¹⁰ For instance, a formal modal system is unable to say whether it is indeed possible to construct an open-sentence satisfied by all and only cats in my house. True, modal systems are given formal interpretations—one can determine, using modal logic, whether there is some "world" in which a certain set of arbitrarily selected objects falls under the extension of an arbitrarily selected predicate. But this far from guarantees that one can, *salve veritate*, substitute for those objects the cats in my apartment and a particular open-sentence, and for that predicate the satisfaction relation. NL interpretations are altogether more powerful in that they are designed to provide what (Plantinga 1974, 127) describes as an *applied* modal semantics:

These "interpretations" do more than mathematical structures do: they not only assign the relevant sort of sets and objects to the parameters of the logical language in question, they also supply meanings or senses to the parameters. . . . When a logical language is given an NL interpretation, the sentences of the language can be regarded as expressing statements that are true or false. (Chihara 1998, 186-7)

As I remarked earlier, the *meaning* Chihara intends to provide for the modal operators

¹⁰Cf. Alvin Plantinga: "To accept the pure semantics, therefore, is not, as such, to acquiesce in any philosophical doctrine at all. The pure semantics commits itself to little more than a fragment of set theory. . . . The pure semantics does not give us a meaning for ' \Box ', or tell us under what condition a proposition is necessarily true, or what it is for an object to have a property essentially" (Plantinga 1974, 127).

involves statements about ways the world could have been. Nevertheless he recognizes that reasoning in his proposed system is somewhat laborious, and he goes on to expend a great deal of energy proving his “fundamental theorem,” aiming to show that one can use uninterpreted **S5** structures to draw inferences about his **NL** interpretation of modal semantics. Simplifying to a great extent, the idea is to show that the uninterpreted sentences of possible worlds semantics can serve as adequate representations of what is true about how the world could have been. The details of this lengthy proof shall be passed over here.¹¹ What is important is that in doing so Chihara eschews commitment to possible worlds. It follows that Constructibility Theory is not burdened by the ineliminable appeal to the metaphysically and epistemologically suspicious entities known as *possible worlds*. There is evidence, then, that Chihara can adopt the results of modal logic without encountering any new ontological commitments. This observation is relevant to addressing one of Shapiro’s objections (discussed in the next chapter) that nominalists are not entitled to use model-theoretic reasoning in the construction and advancement of their views. But it is far from clear that appealing to **NL** interpretations of modal systems represents a genuine improvement with respect to justifying metaphysical possibility claims (e.g., claims about the constructibility of the open sentence tokens required by Chihara’s reconstruction of type theory).

It seems to me that Gillies’ and Woleński’s objections, as well as Chihara’s replies, by and large miss the point. That is because *even if* it is the case, as I maintain, that Chihara avoids commitment to possible worlds, that does not automatically provide Chihara with an account of how it is possible to justify the metaphysical possibility claims made by his modal reconstruction of type theory. Chihara needs to explain why he or anyone else is justified in believing that there are ways the world could have been, such that, if the world had actually gone these ways, there would have been constructed the very open-sentence tokens that are asserted to be constructible by Constructibility Theory. The question “how

¹¹For the proof itself, see (ibid., 239-59). For an explication of terminology and conceptual resources employed, see (ibid., 182-239).

can the philosopher know that there are indeed ways the world could have been, such that, if the world had actually gone these ways, there would have been constructed the very open-sentence tokens that are asserted to be constructible by Constructibility Theory?" is suspiciously similar to the question about mathematical objects raised earlier by Chihara—"how can the mathematician know that such things exist?" (1990, 5). Chihara's concern in the latter case is that one is committed, "to an impossible situation in which a person has knowledge of the properties of some objects even though this person is completely cut off from any sort of causal interaction with the objects" (ibid.) But why is the situation *a propos* of ways the world could have been not just as "impossible"? Ways the world could have been are, presumably, no less causally isolated from humans. How does the modal metaphysicist discover the various properties of ways the world could have been that Constructibility Theory seems to describe? By what power does the modal metaphysicist arrive at modal knowledge? In short, *how is modal knowledge possible?*

It is one thing to argue that one can use modal semantics to carry out reasoning in Constructibility Theory (and to show that no commitment to possible worlds arises from this use of modal semantics), but that is only half of the battle. To say that Constructibility Theory is thereby cleared of all obligation to justify modal assertions would be no different than supposing that the platonist is cleared of all obligation to justify existence assertions by showing that she is entitled to use classical logic. But by doing this the platonist can only secure non-trivial knowledge about what conditionally exists—knowledge about what follows from existence assumptions. She would fail entirely to establish the truth of the *axioms* of mathematical theories. Likewise, Chihara does not address questions about the justification for the constructibility claims that function as the axioms of Constructibility Theory. But without an account here that does the platonist one better, I cannot make sense of the claim that Constructibility Theory genuinely avoids "the problem of existence in mathematics"—because it poses another problem that appears to be just as serious—the problem of what open-sentence tokens are in fact constructible. These are, to be sure,

different problems.¹² But the important issue is that they appear to be similar in the degree to which they threaten the views under which these problems arise. Constructibility Theory, in order to provide an account of mathematical reasoning that is more plausible than the account given by the platonist, therefore faces the burden of justifying assertions about ways the world could have been.¹³

In certain respects, reductive theories of modality are thought to provide a means for justifying modal assertions. If p 's necessity consists in p bearing certain (non-modal) relations to a reductive base R , then knowledge of p 's necessity consists in knowing certain (non-modal) facts about R and p . Global skepticism aside, if coming to possess the relevant bits of knowledge about R and p is unproblematic, it would seem reasonable to suppose that knowledge of p 's necessity is no great mystery. Unfortunately, Chihara is quite open about not having offered a reductive account of modality:

...I am not a foundationalist, attempting to justify all our modal principles from a certain set of self-evident or a priori truths. Nor am I attempting to provide a semantical foundation for modal logic from within some austere non-modal framework, such as set theory. Instead, I make free use of such modal notions as is expressed by 'the world could have been such that', and I have no compunctions about appealing to subjunctive conditionals. Thus, I do not attempt to provide analyses of modal notions in the way David Lewis does. (ibid., 207)

However, just because Chihara is not interested in reducing things like ways the world could have been and subjunctive conditionals to something more basic does not imply that it is impossible to provide a non-modal account of such things. Such an account would provide Chihara a way out of the criticism I am raising. Until he is in possession of such an account, the charge still stands that it is by no means clear that the justificatory problem Constructibility Theory faces is less onerous than the justificatory problem platonism faces.

Although Chihara does not offer Constructibility Theory as an account of the true

¹²But cf. Shapiro's objection in the next chapter.

¹³If, contrary to what most platonists and nominalists in the philosophy of mathematics maintain, there are no significant metaphysical or epistemological difficulties raised under platonism, then (at least without further argument) neither should Constructibility Theory be thought to raise any significant metaphysical or epistemological questions.

content of mathematics, nevertheless he does offer it as a means for accessing the truths of mathematics (whatever these truths ultimately consist in) and for carrying out mathematical reasoning. Modal assertions figure directly in his reconstruction of mathematical reasoning; modality is the *foundation* of Constructibility Theory. That he appears to incur the burden of justifying modal assertions is therefore a significant finding. Does a similar fortune befall Hellman and Field? I shall examine these views in the following two sections.

1.3 Hellman's Modal Structuralism

Structuralism in the philosophy of mathematics is a family of doctrines which holds that, “mathematical theories typically investigate relations holding among items of structures of a given type in abstraction from the identity of those individual items” (Hellman 1990, 309). The structuralist position is often linked to Richard Dedekind, who, in the late 19th century, showed that any two realizations of the Peano postulates are structurally isomorphic (1901, 31-115). Some have taken this to demonstrate that, in the case of arithmetic and number theory, the mathematical significance of numbers lies not with any particular objects that happen to satisfy the Peano postulates, but rather with the relations holding *between* such objects.¹⁴ Similarly, real analysis can be seen as the study of dense linear orders, where what in particular is being ordered is unimportant. This analysis is thought to generalize to most, if not all, of mathematics, hence the slogan: *mathematics is the science of structure*.

Structuralism can be seen in the modern era as stemming from a foundational worry. Assume that set theory provides an adequate foundation from which to carry out mathematical investigation. This implies that number theory can be constructed in set theory. Suppose also that one adopts the strongly reductive position that numbers *just are* sets. This, it seems, requires one to *identify* a given number with a particular set. The problem is that there are infinitely many ways of doing this, and choosing between these myriad

¹⁴This position can be contrasted with the views of someone such as Frege who held that the identity and nature of numbers is revealed by self-evident logical principles.

ways is seemingly a pragmatic matter.¹⁵ With what, then, is a number to be identified? The structuralist's answer is to say that a number is nothing more than a position in a structure; that a number, in any mathematically significant sense, is not to be identified with any object whatsoever!¹⁶

As described, whatever the merits of the structuralist position are, compatibility with nominalism is not listed among them. For what is a natural-number structure, or a real-number structure, if not another kind of abstract mathematical object? Structuralism can be understood as ontologically economical when compared with platonism; a countable infinity of natural numbers are exchanged for the natural number structure, an uncountable infinity of real numbers are exchanged for the real number structure, etc. But to be in ontological debt to a lesser degree is still to be in ontological debt. *Mutatis mutandis*, the nominalist's metaphysical and epistemological worries about mathematical objects can be applied to structures just as easily as they can be applied to numbers. How, then, does Hellman hope to provide a nominalistically acceptable articulation of mathematical structuralism? According to Hellman, it is possible to get by solely on the assumption that the relevant kinds of structures are *possible*:

The core intuition behind the [modal-structural] interpretation is that a sentence of the ordinary language of number theory (which we may take to be already codified in a suitable quantificational language) states what *would* hold in any structure of the appropriate type that there *might be*, without commitment that any such structures actually exist. (Hellman 1990, 314)

How is this to be done?¹⁷

¹⁵For the classical portrayal of this issue, consult (Benacerraf 1965). For doubts about whether this decision is merely pragmatic, see (Tappenden 2008b).

¹⁶This, at least, is the *philosopher's* route to structuralism. A more mathematically-sensitive motivation would have to stem from observing the extent of the fruitful research deriving from increased focus on, e.g., properties that are common to disparate mathematical structures, rather than any feelings of anxiety about the so-called "multiple reductions" problem just described. (The multiple reductions problem might be thought by philosophers to be particularly salient as an objection to, e.g., Frege's account of mathematics. But mathematicians today are generally not troubled by the foundational issues that occupied Frege and many other mathematicians up through the early 20th century—issues which, for better or worse, nevertheless persist as important matters of dispute in philosophical circles.)

¹⁷What follows is drawn from (ibid.). For the full treatment see (Hellman 1989).

In the case of number theory, one must first define an ω -sequence, that is, a sequence that satisfies the Peano postulates.

$$\omega\text{-seq}(X, f) \equiv_{\text{df}} [\wedge \text{PA}^2]^X (f) \quad (1.1)$$

This states that a unary function f over domain X is an ω -sequence just in case X satisfies f -correlates of the conjunction of the axioms of second-order PA (henceforth PA^2) (i.e., the behavior of the function f mimics the successor function s). Now let A be a consequence of PA^2 . That this is so can be given the modal-structural treatment,

$$\Box \forall X \forall f [\wedge \text{PA}^2 \supset A]^X (f), \quad (1.2)$$

which says that, necessarily, if any pair (X, f) is an ω -sequence, then A holds in (X, f) . In order to avoid the vacuous truth of (1.2), Hellman assumes the following:

$$\Diamond \exists X \exists f [\wedge \text{PA}^2]^X (f); \quad (1.3)$$

it is *possible* for there to exist an ω -sequence.¹⁸ With this assumption in hand one can carry out arithmetical reasoning without assuming the existence of any abstract mathematical objects. What of real analysis and set theory? Similar treatments are in order. One begins with an axiomatic characterization of a certain formal system and then asserts the possible existence of a model of those axioms.¹⁹ Thus,

$$\Diamond \exists X \exists f [\wedge \text{RA}^2]^X (f), \text{ and} \quad (1.4)$$

$$\Diamond \exists X \exists f [\wedge \text{ZF}^2]^X (f), \quad (1.5)$$

¹⁸Since the theory in question is second-order, reference to mathematical individuals is avoided. Hellman offers several nominalist strategies for explaining how it is possible that the successor-relation obtains in a way that generates an ω -sequence (1989, 47-52).

¹⁹Hellman notes that such models should be 'standard' as opposed to 'arbitrary' (ibid., 19).

where (1.4) is taken to assert the possible existence of a model of second-order real analysis, and (1.5) is taken to assert the possible existence of a model of second-order ZF set theory.²⁰

1.3.1 A Closer Look at Modality in Modal Structuralism

Hellman is quite open about his system requiring a primitive notion of logical modality. His critics are quick to pick up on this as well. Stewart Shapiro writes that Hellman, “demurs from the standard, model-theoretic accounts of the logical modalities. Instead, he takes the logical notions as *primitive*, not to be reduced to set theory” (1997, 89). Meanwhile Michael Resnik notes that, “to avoid introducing abstract objects into his metatheory, he eschews the possible world semantics and takes modal operators as primitive” (1997, 68). And they are both quick to levy criticisms against Hellman that are analogous to the criticisms raised above against Chihara—*viz.*, that it is unclear what possible justification Hellman could have for making the various modal-existence assertions described above:

Even if we eschew an ontology of possible objects, we surely need an epistemology of possible objects—just as the traditional realist needs an epistemology of actual abstract objects. How do we know what is possible? No reason is given to think that the modal route is any more tractable than the realist one—and there is reason to think it is not more tractable. (Shapiro 1997, 228-9)

Let us now turn to the epistemology of modal mathematics. We might expect that some advantage is to be gained by replacing the question of how we know that entities exhibiting mathematical structures exist by the question of how we know that such objects are logically possible. However, both Field and Hellman would be among the first to admit that they have little idea of how we know that objects having the structure of an iterative hierarchy of sets are logically possible. But they offer us the hope that the epistemology of logical possibility will prove more tractable than the epistemology of mathematics. I do not share their optimism. (Resnik 1997, 77-8)

Perhaps Hellman has succeeded in eschewing commitment to abstract mathematical objects, but in the process he has created new problems regarding knowledge about the

²⁰Those familiar with set theory will recognize that the ZF axioms do not exhaust the possible axiomatizations of set theory. Indeed, much interesting and fruitful research requires stronger axiomatizations. That is no problem for Hellman, however, as he can appeal to alternative modal-existence principles. It is best, then, to think of (1.5) as providing the basis for a schema of set-theoretic modal-existence postulations: $\diamond \exists X \exists f [\wedge \text{ZF}^2 + Y]^X (\epsilon_f)$, where one can substitute in for Y any (consistent!) strengthening of ZF.

possible existence of mathematical structures.

What kind of evidence, if any, can be used to support the various modal-existence assertions described above? Regarding (1.4), Hellman says that it, “is a strong assumption not reducible to a claim of formal consistency,” though it, “has its roots in our geometric experience,” but in the end it, “must be regarded as a working hypothesis of classical mathematics, not as a self-evident certainty” (1989, 45). And regarding (1.5), Hellman claims that it, “functions as a working hypothesis of realist set-theoretic practice, just as the possibility of ω -sequences or of separable ordered continua function, respectively, in the practice of number theory and real analysis,” but (1.5), “is much less directly tied to experience than these latter modal-existence assumptions, and is, in this respect, far more speculative” (ibid., 71-2). Eventually he admits that, “how best to describe and assess our ‘evidence’ for such a hypothesis remains one of the most difficult challenges confronting mathematical epistemology” (ibid., 72).

These remarks suggest that Hellman takes *mathematics* to be an important arbiter in deciding which structures are of mathematical *interest*.²¹ But admittedly, his description of these modal-existence claims as “working hypotheses” of mathematics does not amount to a substantive account of what *justifies* these modal-existence claims.²² One might, of course, argue that the fact that mathematicians are willing to carry out derivations from a particular set of axioms is evidence for the possible existence of a model of these axioms. But this would presumably just shift the inquiry over to the question as to what entitles mathematicians to use these axioms in the first place. Responding here that the embodied structure is possible, or consistent, would clearly be circular. Another option is to opt for an inductive justification for the possibility claims—that if, e.g., ZF implied a contradiction, mathematicians would have uncovered such a fact by now. Some may find this option unattractive—mathematics is thought by many to be one of the few—perhaps

²¹Penelope Maddy, in her more realistic moments, proposes that what is of mathematical interest is in fact a guide to mathematical existence. See chapter five for elaboration and discussion.

²²Thanks to Eric Hiddleston for pointing out several flaws in an earlier formulation of Hellman’s position on matters.

the only—remaining sources of indubitable knowledge, and for such individuals it would be unpleasant to discover that mathematical reasoning relies on *inductively* established theories. But this might be the unhappy fate that faces mathematical foundations; I will have somewhat more to say on this issue when discussing Field's nominalism in the next section.

A third option—one that Hellman himself endorses—is to argue that these modal-existence assertions are *indispensable* for describing and carrying out mathematical research (Hellman 1989, 96-7). One might worry that accepting the possible existence of a mathematical structure because doing so is needed to make sense of mathematical practice is essentially to postulate possibilities in the same objectionable way that Quine and Putnam postulated the existence of mathematical objects through their indispensability arguments. Should this worry prevent Hellman from invoking his modalized version of the indispensability argument? Hellman's contention is that the Quine/Putnam indispensability arguments are *unsound*—not that they are *invalid*. The purpose of his modal structuralist theory is to show that mathematical objects are dispensable in mathematics and science in favor of the weaker premise that modal-existence assertions suffice for mathematics and science (*ibid.*, 97). Still, to say that possible structures are required for the truth of scientific and mathematical claims is not to explain in detailed terms how human beings come to know that it is possible for these structures to exist and to know various things about them.²³

Much as with Chihara, modality plays a foundational role in Hellman's account of mathematics. The possible existence of a mathematical structure is predicated on the primitive logical possibility of the existence of a model exemplifying it, and the success of mathematical reasoning depends on facts about what necessarily follows from the axioms that characterize the structure. And just as it is unclear how someone in Chihara's

²³As discussed in the second chapter, Stewart Shapiro claims that it is a *presupposition* of mathematics that, e.g., set theory is consistent. If philosophers of mathematics are uncontroversially permitted to recognize such presuppositions, then Hellman's justificatory burden does not seem to be terribly great.

position is to justify assertions about which open-sentence tokens are indeed constructible, it is also unclear how someone in Hellman's position is to justify assertions about which structures are possible (just as it is unclear how the platonist is to justify assertions about the truth of mathematical axioms or the existence of mathematical objects). That Hellman's modal structuralism appears open to this criticism is therefore a significant finding—for Hellman, like Chihara, worries that traditional platonist views have difficulty providing, “a reasonable integration of mathematical knowledge with the rest of human knowledge” (ibid., 3). It is simply not evident that the difficulties raised by modal structuralism are any less insuperable than those raised by platonism. Does Field's fictionalism raise a similar set of difficulties? I turn now to his account of mathematics.

1.4 Field's Fictionalism

Both Chihara and Hellman can be understood as combating the platonist's supposition that the soundness of mathematical reasoning and the truth of mathematical theories require the existence of mathematical objects. Hartry Field addresses a variation on this theme—to him, it is the *prima facie objectivity* of mathematics that leads philosophers to adopt platonism (1998b, 387). Consider, for instance, the undecided status of Goldbach's Conjecture (that every even integer greater than two can be expressed as the sum of two primes). It seems reasonable to suppose that there is a correct (i.e., *objective*) answer to the question as to whether Goldbach's Conjecture is true. Of course this truth has to consist in something, and if that something is not objective then one risks undermining the *prima facie* objectivity of Goldbach's Conjecture. Abstract mathematical objects are a perfect fit: they have mind-independent existence and they would appear to settle whether Goldbach's Conjecture is true. Thus, the objectivity of mathematics is to be settled by regarding mathematics as true, and the truth of mathematics is to be explained in part through reference to abstract mathematical objects.

As motivated, then, platonism can be undermined in two ways. First, one can deny

that the truth of mathematical assertions requires the existence of mathematical objects, as Chihara and Hellman have done. Alternatively, one can deny that the objectivity of mathematics is to be settled by regarding mathematical assertions as true. It is this alternative strategy that Field implements. That is, Field does not regard mathematics as something that is true—for him, mathematics is just an elaborate fiction.

Field takes as motivation for his program the epistemological problems facing traditional platonism. He claims that it is not just the causal inertness of mathematical objects that makes mathematical knowledge so mysterious, but instead the absence of *any* account whatsoever for explaining how it is mathematicians come to possess *reliable* beliefs about the Third Realm (Field 1989a, 25). He explains:

... what causes the really serious epistemological problems is not merely the postulation of causally inaccessible entities; rather, it is the postulation of entities that are causally inaccessible *and* can't fall within our field of vision *and* do not bear any other physical relation to us that could possibly explain how we can have reliable information about them. (Field, 1989b, 69).

Now it is not clear that in saying this Field has truly extended the epistemological considerations beyond those about the lack of causal connection between human beings and mathematical objects; surely the complaint that mathematical objects do not bear any *physical* relation to humans which could explain one's reliable beliefs about them is not appreciably different from complaining that no one has any causal connection to mathematical objects through which to explain the reliability of one's mathematical beliefs. But I will not belabor the point—all that is important to note at the moment is that Field develops his nominalism largely as a measure against incurring the epistemological difficulties that besiege platonism.²⁴

Field's projects breaks into two major components. The first component involves the creation of nominalized versions of physical theories with the aim of demonstrating that it is possible to carry out scientific reasoning without assuming the existence of mathematical

²⁴For a slightly more mature account of Field's misgivings about platonist epistemology, see (Field 1989f, 230-9).

objects. The second component involves explaining how the use of ordinary mathematical reasoning is ontologically innocuous, even when applied in science. Modality plays a critical role only in the second component of Field's project; for this reason I shall be rather curt in discussing the first.

Although Field begins work on both components in his well known (1980), the majority of the book is given over to the development of a nominalistically acceptable version of physical theory. With the assumption that spacetime is substantival and mirrors the structure of the four-dimensional real numbers, \mathbb{R}^4 , he has at his disposal a remarkably powerful basic ontology of spacetime points from which to construct physical correlates of mathematical objects and concepts. He produces a nominalized version of Newtonian Gravitational Theory in flat spacetime. The success of this component of Field's project has been called into question on three important fronts. First, it is unclear whether Field's strategy for nominalization can be extended to cover more mathematically complicated physical theories, such as theories of quantum mechanics.²⁵ Second, some have questioned whether helping oneself to an ontology of spacetime points is nominalistically acceptable.²⁶ Finally, some have questioned whether Field's use of the "full logic of Goodmanian Sums"—what allegedly amounts to second-order logic—is consistent with nominalism.²⁷ But since I am interested in how the modal notions play a role in modal nominalist philosophies of mathematics, and these issues do not directly involve a dispute over modality, I shall not be concerned to discuss possible resolutions to the problems just raised.

I turn, now, to the second major component of Field's work—his account of how mathematics can be useful even though it is not true. Technical complications aside, Field's

²⁵This complaint is made quite often, but the *locus classicus* is (Malament 1982). Responses have been made on Field's behalf. Mark Balaguer has given some indication about how to nominalize quantum mechanics in (Balaguer 1998, 113-27). Mary Leng suggests that caution should be exercised in drawing metaphysical conclusions from quantum mechanics until the scientific community agrees on what parts of the theory are just mathematical formalism and what parts correspond to physical reality (2010, 57-75). Malament's criticism is certainly not the knock-down criticism many take it to be.

²⁶See, for instance, (Resnik 1985a). Field's response can be found in (Field 1989e).

²⁷See (Resnik 1985b, § 3).

central thesis is that mathematics is always *conservative*. Semantically, this can be explained by saying that,

Let S be a mathematical theory and let N be a nominalistic theory. Then $N + S$ is a conservative extension of N .

The idea here is that using a mathematical theory does not allow one to say something about the physical world that one could not already say (perhaps in a long-winded fashion) in a nominalistic language. The notion of conservativeness can also be explained deductively,

Let S be a mathematical theory and N be a nominalistic theory, and let $N + S \vdash A$. Then $N \vdash A$.

That is, any physical consequence of a nominalistic theory plus mathematics is a consequence of the nominalistic theory alone. Mathematics, while useful for facilitating inferences, is strictly unnecessary.²⁸ Of course, Field recognizes that it will not do to merely *state* that mathematics is conservative over nominalistic physical theory. It is by no means obvious that mathematics is conservative; a proof of this result is required. Unfortunately for Field, to prove that a theory $N + S$ is a conservative extension of a theory N requires the use of metalogic, and, as it is ordinarily understood, metalogic is riddled with mathematical objects (e.g., models and proofs). Thus to prove the conservativeness of mathematics requires having knowledge about the relevant pieces of metalogic, which in turn requires knowledge about some mathematical objects.

Field avoids this result by invoking modality. Instead of showing that a mathematical theory is consistent, for instance ZF, by showing that it has a model, and then proceeding to construct the conservativeness result, Field thinks that it is just as well, as far as consistency is concerned, to show that ZF is *logically possible*. And the logical possibility of the

²⁸Shapiro shows that mathematics is actually *not* conservative in the deductive sense (1983, 521-31). His strategy is to show that since arithmetic can be constructed in Field's physical theory the results of Gödel's first incompleteness theorem apply; there is a (Gödel) sentence of the arithmetical submodel of Field's physical theory that cannot be proven in the theory. For Field's response, see (Field 1989d).

conjunction of the axioms of ZF ($\diamond AX_{ZF}$) does a fine job in this regard. What this suggests is that one can understand claims about the semantic consistency of mathematical theories as *modal* claims about the logical possibility of the conjunction of their axioms. This frees him from commitment to mathematical objects in the production of proofs of the semantic conservativeness of mathematics. Similarly, by invoking certain modal assumptions, Field hopes to show that he can use proof theory in an ontologically innocuous way when securing his syntactic conservativeness result.

1.4.1 A Closer Look at Modality in Field's Fictionalism

It should be clear from the foregoing that the use of modality in Field's fictionalism is restricted to metalogic. The issue that must be addressed now is whether his use of modality presents any clear justificatory hurdles. There are two components to this discussion. The first involves saying a little more about how Field proposes to "modalize" the notion of provability and the semantic notions of consistency and implication. The second involves examining whether it is easier to justify assertions involving these modal notions than it is to justify ordinary, platonistic mathematical existence assertions.

Before setting out in earnest it is worth noting that Field is, at least early on, somewhat reluctant in his use of modality, much like Quine was reluctant in his acceptance of the existence of abstract objects. He writes:

... I do not entirely welcome the introduction of the notion of possibility even into metalogic; it seems much less bad than either the introduction of this notion into physics or the introduction of pieces of platonic protoplasm with no causal connection to us or to anything we observe and existing outside of space-time, but still it is something I would prefer to avoid if possible. (Field 1989b, 77)

Again the notion arises that the decision between accepting an ideology of modal notions and accepting an ontology of mathematical objects is something of a tradeoff; obviously Field is disposed to think of the choice of modality as less onerous. (He remarks on the same page that invoking possibility is the "best position available.") Despite this reluctance,

Field thinks that the modalized versions of the metalogical notions have independent appeal.

To say that φ is provable (or deducible) from a set of sentences Γ is ordinarily understood to mean that there exists a proof (or deduction) of φ from Γ . But according to Field, this treatment of provability is artificial, for it is

... more natural to characterize the *provability* of a sentence in terms of the *possible* existence of a string of symbols that meets the condition of being a proof of it, than in terms of the *actual* existence of an abstract sequence of abstract analogues of those symbols. (ibid., 76)

I am not certain that this is a very persuasive argument for taking provability to be a modal notion. That it is possible for there to be a string of symbols that meets the condition of being a proof that φ is undoubtedly a *sufficient condition* for it being provable that φ , but some might say this only calls for an explanation for why such a thing is possible in the first place. The platonist at least has the beginning of an explanation—she can point to the existence of a particular “abstract sequence” as a grounding for this possibility. (Of course, if one asks for an explanation about why that particular “abstract sequence” exists, she will quickly get into trouble, too.)

Similar remarks apply concerning why the semantic conception of consistency, ordinarily associated with truth in a model, should be taken to be a modal notion:

... isn't the semantic consistency of the theory of discrete linear orderings with no last element more naturally thought of in terms of the *possible* existence of entities that are ordered in the way this theory says, rather than in terms of the *actual* existence of an ordered pair whose first member is an infinite set and whose second member is a subset of the Cartesian product of that set with itself? (ibid.)

As before, it is unclear whether someone not already convinced of the wisdom of understanding semantic consistency modally would be won over by these comments. Why suppose that the modal versions of provability and consistency are “more natural” than their platonistic counterparts?

The semantic notion of implication holds that a set of sentences Γ implies φ when φ is true in all models in which every sentence of Γ is true. Against the standard model-theoretic account, Field advances the following “simple-minded” consideration:

Suppose someone were to assert each of the following:

- (a) ‘Snow is white’ does not logically imply ‘Grass is green.’
- (b) There are no mathematical entities such as sets.

Such a person would not appear to the untutored mind to be obviously contradicting him- or herself. . .but of course (a) and (b) would be in obvious contradiction if ‘logically imply’ was defined in terms of sets. . . (1989a, 33-4)

Field takes this consideration to show that the model-theoretic account of implication fails to deliver the *meaning* of the ordinary notion of logical implication, even if, by *accident*, it is extensionally adequate. As before I am not certain what to make of this argument. A platonist would not be much bothered by the inconsistency of (a) and (b)—she might even argue that (b) is necessarily false. That (a) and (b) *seem* not to be contradictory she can explain as an accident of psychology or of philosophical prejudice.

So much for Field’s reservations about the platonistic metalogical notions. How does he explain the sense in which implication, consistency, and provability are to be understood as modal notions? First, the way to learn about the modal operators \Box , \Diamond , is by learning the rules governing their manipulation. Knowledge of \Diamond , for instance, consists mainly in knowledge of certain patterns of inference (e.g., $A \rightarrow \Diamond A$). Moreover, Field claims that the only thing special about the modal operators is that they have the various rules governing their use that they in fact have. But if endorsing certain patterns of inference is the only thing special about the modal operators, then there is a sense in which any other kind of logical device that licenses the same patterns of inference ought to count as modal, too. The function of consistency in metalogic is analogous to that of possibility in modal logic, just as the function of implication in metalogic is analogous to that of necessity in modal logic (ibid., 34-5). Hence consistency and implication “fit the pattern” for being modal

concepts. Regarding how one comes to possess conceptual knowledge about consistency and implication, Field writes:

... there are “procedural rules” governing the use of these terms, and it is these rules that give the terms the meaning they have, not some alleged definitions, whether in terms of models or of proofs or of substitution instances. (1991, 5)

On the connection between metalogic and modality, he says:

... there is no need to explain this operator in other terms, any more than there is a need to explain the negation operator or the existential quantifier in other terms; instead we should regard the modal operator as simply a logical primitive, one that we come to understand not by defining it but in whatever way we come to understand the other logical primitives. (Field 1989b, 76)

And later:

I am not supposing that we have an independent notion of possibility clearer than the operator ‘it is consistent that’, and using this allegedly clearer notion to help illuminate consistency. Rather, I am saying that the consistency operator is clear on its own, and needs no explication. In calling it modal I am simply observing that the laws governing it include the familiar modal principles. (Field 1991, 9-10)

Rather than *reducing* consistency to possibility, Field sees these notions as *equivalent*. Therefore, for Field, knowledge of semantic consistency *just is* knowledge of logical possibility, and vice versa.

Field’s position on provability is more delicate.²⁹ He does not attempt to produce a nominalized proof theory, but instead maintains an instrumentalism regarding platonistic proof theory. According to Field, there are primitive modal facts about what can be proven in a mathematical theory. These facts point to the necessity of conditionals of the following form:

$$AX \supset \text{there is a proof of } \neg P \text{ in } F, \quad (1.6)$$

Where AX is the conjunction of the axioms of a theory and F is a proof theory. Thus the

²⁹In the following I draw from (Field 1989c).

schema,

$$\Box(A\bar{X} \supset \text{there is a proof of } \neg P \text{ in } F). \quad (1.7)$$

This, then, serves as modal/proof-theoretic evidence for the impossibility of P . For instance, suppose that there is a proof of $\neg S$ in ZFC. Then,

$$\Box(\text{ZFC} \supset \text{there is a proof of } \neg S \text{ in } F). \quad (1.8)$$

Under the assumption that ZFC is consistent, it follows that it is possible for there to be a proof of $\neg S$ in F , which Field offers as strong evidence that $\neg\Diamond S$. Thus, platonistic proof theory can be shown to be useful in acquiring knowledge about possibilities by means of modal assumptions that are strictly weaker than those required by the platonist (e.g., the consistency of ZFC versus the truth of ZFC).

With the connections between metalogic and modality finally forged, it is now possible to address the important justificatory concerns that Field's account of metalogic raises.

1.4.2 Field and Justifying Modal Assertions

As can be seen, the association of logical knowledge with modal knowledge performs important work for Field. It is only after metalogic has been purged of any trace of platonistic ontology that Field can claim to have eliminated the epistemological problems facing platonism. Nevertheless, his solution involves primitive modal and logical devices. Field believes that conceptual knowledge about these primitives can be acquired by learning the "procedural rules" governing their use. Already, however, there is reason to question the approach of eliminating logical ontology by appealing to primitive logical concepts. John Burgess complains that, "[n]ominalism threatens to become trivial and uninteresting if one is allowed to help oneself freely to primitive operators without any obligation to explain in more familiar terms what they are supposed to mean" (1993, 182). I suspect Burgess is worried about a potential slippery slope—if modal nominalists can get away with eschewing commitment to mathematical objects by appealing to primitive

modal concepts, what in principle prevents someone from adopting primitive notions to do away with other kinds of objects? On this picture, the modal nominalist appears to be advocating theft over honest toil. In response to this I would first like to stress that Field in fact agrees with Burgess about the slippery slope; he is in fact reluctant to appeal to modality. Field's concern is not so much that to do so would be cheating, but rather that the decision to "modalize away" a kind of entity is arbitrary—one can modalize away physical objects just as easily as one can modalize away mathematical objects.³⁰ What entitles Field to conclude that utilizing modality in metalogic does not set one down the slippery slope into modal oblivion is his claim that the metalogical concepts are inherently modal, whereas material objects are not inherently modal. While one could dispute this claim, it nevertheless suffices as a principled distinction for determining what can and cannot be modalized away. As a second response to Burgess, I should think that similar remarks apply to the platonist as well: If it is wrong to invoke primitive notions whenever one encounters a philosophical problem, why is it not for the same reason wrong to postulate the existence of some kind of object whenever one encounters a philosophical problem? Are not both of these strategies equally *ad hoc*? Why, therefore, is platonism not also "trivial and uninteresting," helping itself freely to abstract objects without any explanation as to how one can have knowledge about them?

I insist that if there is anything wrong with Field's strategy, it is not because his method for eliminating mathematical objects in metalogic is an instance of some generally suspect procedure. Rather, if his view raises any serious justificatory problems, it must have to do with the *particular* kinds of primitives he uses. In addition to becoming familiar with the "procedural rules" for the logical primitives, has Field any account of how it is possible to justify assertions about what is logically possible or consistent? In particular, does Field have the resources to justify assertions about the logical consistency of conjunctions of axioms of important mathematical theories? Unfortunately, Field's answer is that, "neither

³⁰See (Field 1989f, 252-68).

I nor anyone else that I know of has a great deal to say about the epistemology of modal claims” (1989d, 140). Again, to counter any reductive means one might have for explaining the logical primitives, he later offers the reminder that logical possibility, “is not something to be explained in terms of *entities* (e.g., models, formal derivations or possible worlds)” (Field 1989c, 86). What hope is there then for justifying assertions about what is logically possible? Field sees this as an epistemic issue:

Doubtless the epistemology of assertions of logical impossibility and logical possibility needs developing, and there are serious problems to be overcome in doing so; but I think that they are clearly *different* epistemological problems than the epistemological problems that are most characteristic of mathematics and that motivate anti-realist positions. (Field 1989f, 252)

According to Field, the hopes are dim for an unproblematic epistemology for modality. However, this should not trouble the modal nominalist too greatly on account of the fact that the epistemological problems she faces about logic and modality are *different* from those facing platonism.³¹ As I shall explain in chapter three, it is not entirely clear that this is primarily an epistemological problem. But granting for the moment that it is an epistemological problem, why should it be supposed that, on account of it being a *different* epistemological problem from the epistemological problem facing platonism, that it is thereby a more *tractable* problem? Could it not be the case that, all things considered, the epistemological problems facing logic and modality are at least as troublesome as (or even *worse* than) those facing platonism?

According to Resnik, it is unclear that Field has improved the situation:

There are too many unanswered questions and objections on the books concerning the scope of logic and the notion of logical possibility for me to believe that anyone can be more justified in appealing to modalities than he would be in appealing directly to abstract entities. (Resnik 1985b, 207)

³¹Of course, if Field is right about mathematical knowledge being logical knowledge, then it hardly makes any sense to say that the problem of logical and modal knowledge is different than the problem of mathematical knowledge. The proper interpretation of Field’s passage is that the epistemological problems of logic and modality, *as the modal nominalist understands logic and modality*, are distinct from the epistemological problems facing mathematics, *as the platonist understands mathematics*.

Resnik, unfortunately, does not make it entirely clear just what questions and objections make logical modality so dubious. In any event I do not think Field is terribly troubled by this sort of criticism. I imagine he would understand Resnik to be calling into question how one acquires conceptual knowledge about the logical modalities, and Field has a reply to that objection.³² But this kind of conceptual knowledge can only take one so far. For instance, Field can account for the fact that the consistency of a set of axioms has certain implications. But he cannot, it seems, account for the fact that a certain set of axioms is consistent. This prompts Resnik to levy the following criticism:

This leaves us with the question of how nominalists can come to know the possibilities required for their use of platonist meta-logic and mathematics. Unfortunately, Field has little to say about this. He thinks that there are some obvious principles involving logical form to which the nominalist can appeal. But since their use is clearly limited, he also mentions coming to know possibilities inductively (e.g., our experience with ZF might be part of our basis for our knowledge that it is possible). In the end, however, he admits that neither he ‘nor anyone else that I know of has a great deal to say about the epistemology of modal claims...’ I find this quite ironical in view of Field’s objection to mathematical objects on the grounds that we lack an epistemology for them. (ibid., 204)

It would seem that Field is in a position similar to that of Hellman concerning the justification of particular assertions of possibility. In order to clearly demonstrate the justificatory superiority of fictionalism, he would have to show that it is easier to justify logical possibility assertions than it is to justify mathematical existence assertions. And he certainly has not shown this. *Prima facie*, the fictionalist is unable to claim justificatory superiority over platonism.

In more recent work, Field has attempted to shed more light on the acquisition of logical knowledge. By construing logical knowledge as *a priori* knowledge Field thinks he can explain why it is that logical knowledge does not raise the same epistemological problems that platonism raises—i.e., that there is no Benacerraf-style epistemological

³²His reply, of course, is that one learns about logical modality through the rules governing the use of the modal operators.

problem for logical knowledge.³³ However, it is unclear that taking logical knowledge to be *a priori* knowledge is at all helpful in explaining how one acquires knowledge about the consistency of a mathematical theory—e.g., certainly the consistency of ZFC axioms cannot be justified by *a priori* insight alone. It is similarly unclear whether Field succeeds in purging logic of Benacerraf-style epistemological worries.

I qualified the conclusion of the argument at the end of the second-to-last paragraph as *prima facie*, and now I would like to explore whether the argument withstands scrutiny. The conclusion that fictionalism is not superior to platonism concerning basic issues of justification turns on the assumption that justifying that a mathematical theory is consistent is no less intractable than justifying that a mathematical theory is true.

Here is a *prima facie* argument for the conclusion that justifying consistency assertions (and hence knowledge of consistency), is less suspect than justifying existence assertions (and hence knowledge of truth). The argument's main premise is that set theorists have yet to discover a contradiction in, e.g., ZFC, despite decades of trying.³⁴ This premise is offered as inductive evidence for the following two claims:

C1. ZFC is true.

C2. ZFC is consistent.

Clearly, (C1) implies (C2), so assertions of consistency cannot be any more suspect than assertions of truth and existence. The remaining options are that the available evidence supports (C1) and (C2) equally, or, alternatively, that the evidence favors (C2) over (C1). Intuitively, when holding p fixed, the claim that p is consistent is *weaker* than the claim that p is true. And, holding one's evidence fixed, inductively arguing for a *weaker* conclusion produces a *stronger* argument, i.e., an argument with a conclusion that is more likely to

³³See (Field 1996), (Field 1998a), and (Field 2004).

³⁴During a presentation at the 2009 Midwest Philosophy of Mathematics Workshop, the set theorist Hugh Woodin claimed that no contradictions will be derived from ZFC even after 500 years and that therefore ZFC should be accepted as true. The following day he increased the time scale of his claim by a factor of ten. The finitists in attendance were not pleased by these claims.

be true. (C2) is weaker than (C1), ergo premises one and two inductively support (C2) more strongly than they support (C1). But then of the two possible conclusions, (C2) is more likely to be true. This would seem to imply that justifying assertions about what is consistent is *less* onerous than justifying assertions about the existence of mathematical objects and the truth of mathematical theories. End *prima facie* argument.

Is there anything wrong with *this prima facie* argument? Certain mathematical structuralists say yes. Indeed, they will call attention to the fact that this argument depends on the tacit assumption that it is *false* that (C2) implies (C1). If this assumption is dropped then of course it no longer follows that there is more reason to believe (C2) given the available evidence. How can the implication from (C2) to (C1) be justified? It would surely be a mistake to argue for the general philosophical thesis that possible truth implies actual truth; that would be to render the distinction between possibility and actuality completely meaningless. And, in S5, it would lead to the absurd conclusion that everything that is possibly true is *necessarily* true. (Of course, one philosopher's modus tollens is another's modus ponens!) But perhaps there is something special about mathematics; perhaps all one needs to do to prove the existence of a mathematical object is to show that it is *possible* for that mathematical object to exist. In the next chapter I will take up in detail several arguments that suggest that there is indeed something special about mathematical objects in that their existence *can* be inferred from their mere possibility.

A different argument a platonist might offer here is that if mathematical concepts were true of *nothing*, mathematicians and philosophers would be unable to fasten upon the refined concepts that they in fact develop (e.g., of function, natural number, real number, group, etc.). That is, the ease with which mathematicians can conceptualize mathematical concepts is evidence that they have extensions. I confess to a large degree of skepticism about this argument—I suppose that there exist no golden mountains in the universe of a shape and size similar to Mount Everest, but this certainly does not imply that the concept of such a golden mountain is somehow unconceptualizable. In fact, the concept is very

easy to conceptualize, and moreover, is refined enough to allow someone, in principle, to determine whether any particular object falls under the concept. If the platonist's justification is anything like this, she is obligated to show that mathematical objects are somehow *special* in constituting counterexamples to the generally false principle that it is not possible to conceptualize about concepts that do not have extensions.

One might reply that the example of the golden mountain misses the point, and that what is really at issue is the ability to form concepts based on well-developed *theories*. But that reply will not help the platonist. Consider that the history of science is riddled with cases of theories that have been rejected on account of the existence of recalcitrant observations. I presume, for instance, that the plum-pudding model of the atom was rejected because it failed to account for the results of various experiments and no longer provided the best explanation for various observed phenomena. But there is little sense to be had in saying that the model was rejected because there is something inherently unrationalizable about the *concept* of a plum-pudding atom. The model fails to cohere with actual observations, but so does the existence of a golden mountain of a shape and size similar to Mount Everest. So it is possible to construe a theory as false, even when one has an "intuitive grasp" of the concepts employed in it; J.J. Thomson surely had a developed understanding of the hypothesized relationship between the atom and his Christmas day dessert.

Still, the platonist might object that what is important here is not the development of theories in general, but that what is at issue is the development of mathematical theories and mathematical concepts. And she would not be without justification in making this claim—the development of the calculus, and, later, real analysis, can be understood as an attempt to come to a greater understanding of concepts like real number, infinite sum, etc.³⁵ It does not seem unreasonable to explain this period of mathematical history as an exercise in figuring out what conception of the real numbers is *true*. If the platonist

³⁵For further discussion on the role of concept formation in mathematics, see (Tappenden 2008a) and (Tappenden 2008b).

is to understand this as the *most reasonable* conclusion to draw, she must explain why the drawing of alternative conclusions (e.g., that under certain conceptualizations of the real numbers it is easier to prove theorems in analysis) is somehow less reasonable. Nevertheless, even if the platonist is successful in doing this, she will only have succeeded in showing that *sometimes* mathematicians reject a mathematical concept because it fails to be true of anything (of mathematical interest). In all probability, there are infinitely many (relatively) consistent extensions of ZF, and each such extension would appear to correspond to a determinate conceptualization of 'set.' Many platonists are inclined to regard these extensions as true of nothing³⁶ and of no mathematical interest. But it hardly sounds reasonable to say that each such conceptualization is unintelligible. How many of these conceptualizations must the platonist give up?

Mary Leng agrees with my assessment of this kind of platonist reasoning, writing that, "our ability to form a *concept* of objects of a particular kind in no way guarantees the independent existence of such objects" (2007, 106). However, she thinks the same does not hold when one is concerned to show that mathematical theories are *consistent*:

Thus, examination of our concepts of number and of set, and finding ways to envisage how objects satisfying those concepts could be arranged (e.g. as in the iterative hierarchy) can at least plausibly count as (defeasible) evidence for the consistency of these notions. At the very least, such considerations suggest that the prospects for defending consistency claims on such grounds are greater than the prospects of defending claims concerning the face-value truth of the theories we are considering. (ibid., 107)

And this is indeed a plausible claim. No one, except for those inclined to very strict conceptions of nomological possibility, would doubt that it is *possible* for there to exist a golden mountain of a size and shape similar to Mount Everest (one could even imagine it constructed on a planet inhabited by some advanced intelligent beings). And if the laws of physics are really up for grabs, then it is *prima facie* possible that the building blocks of molecules more closely resemble Christmas pudding than they do planetary orbits.

³⁶An exception is Balaguer's "plenitudinous" platonism, examined in detail in his (1998).

Why should the possible existence of the set-theoretic hierarchy be regarded as any more suspect than the previously entertained possibilities?

I have one reservation about Leng's reply on behalf of the fictionalist. I am not certain Leng has shown *enough* about how one can have knowledge of consistency. By my lights all she has shown is that there is evidence suggesting that mathematical theories are *epistemically* possible. To say that p is *epistemically* possible for a person S is to say that p is not (known to be) inconsistent with everything else S knows.³⁷ In general, ' p is epistemically possible' is not equivalent to ' p is logically possible.' And what is at issue is not whether mathematical theories are *epistemically* possible, but instead whether mathematical theories are logically possible. Knowledge of the former is not the same as knowledge of the latter; considering the former as reliable *evidence* of the latter—even if it is ultimately *defeasible* evidence—is to commit oneself to a controversial thesis in modal epistemology. Controversial as all of this is, Leng's considerations about mathematical concepts at least demonstrate a *possible* way to justify assertions about the consistency of mathematical theories, and this is welcome news for the fictionalist.

All things considered, it would appear that Field's account of mathematics raises unanswered questions regarding the justification of assertions about the consistency of mathematical theories. But whereas modality plays a foundational role in the work of Chihara and Hellman, its use is rather limited in Field's fictionalism. Field only uses modality in order to establish the conservativeness of mathematics to science and to affirm the consistency of mathematical theories. His project of producing nominalized versions of physical theories is left untouched. If this project can be successfully extended to incorporate contemporary scientific theories, then he can describe physical phenomena without quantifying over mathematical objects and without invoking primitive modal

³⁷Alternative conceptions of epistemic possibility abound; I would like to say that the details are unimportant to what I have to say here, except for the fact that some have proposed a link between epistemic possibility and conceivability, and some have gone on to suggest that, in certain circumstances, conceivability is evidence of possibility. If it is acceptable to move from p 's epistemic possibility to p 's conceivability, and then from p 's conceivability to p 's logical possibility, then what I have to say next might well be false. For more on the connection between conceivability and possibility, consult (Chalmers 2002).

concepts. Nevertheless I am concerned in this dissertation to assess the prospects for modal nominalist accounts of *mathematics*, even if Field, who takes all mathematical assertions to state falsehoods, would see this as a fool's errand. And Field's account of *mathematics* does involve unsubstantiated modal assertions; a significant finding, even if it is not as injurious to Field as similar results are to Chihara and Hellman.

1.5 Recent Developments

The platonism/nominalism debate has died down in recent years. Although a handful of novel positions have been presented since the mid 1990's, including Jody Azzouni's notable contributions,³⁸ focus has shifted somewhat more toward paying closer attention to the practice of mathematics and to the role mathematics plays in scientific explanations. However, there have been several recent articulations of mathematical fictionalism and I would like to say something about them.

1.5.1 Balaguer's Fictionalism

In (Balaguer 1998), Mark Balaguer motivates two philosophies of mathematics. One of these is a platonist view according to which all *possible* mathematical objects exist, and it has come to be known as "plenitudinous" platonism. The second is a fictionalist version of plenitudinous platonism according to which no mathematical objects exist, and according to which the acceptability of a mathematical theory can be partially explained by showing that the theory is consistent. Balaguer argues at length that plenitudinous platonism overcomes the most decisive objection to traditional platonism, and that his version of fictionalism overcomes the most decisive objection to traditional fictionalism.³⁹

According to Balaguer, the most important problem facing platonism is the Benacerraf epistemological argument, "which holds, in a nutshell, that platonism cannot be right

³⁸See, for example, (Azzouni 2004), where he argues that mathematical assertions are not ontologically committing.

³⁹As a general observation, Balaguer argues that the apparent success of both platonism and fictionalism is evidence that there is no fact of the matter about whether mathematical objects exist. My discussion of Balaguer is cursory and I will not address his general conclusions. Cf. my discussion of a related parity result, due to Penelope Maddy, in chapter five.

because it precludes the possibility of mathematical knowledge” (1998, 21). His understanding of this problem comes to the argument that from the assumptions that (1) humans exist only in spacetime, and that (2) if any abstract mathematical objects existed they would exist outside of spacetime, it follows that (3) if there exist any abstract mathematical objects, human beings cannot have knowledge of them. Like Field, Balaguer is not convinced that (3) only follows from (1) and (2) provided that one also assumes a causal theory of knowledge. He thinks that as stated, (1) and (2) already provide *prima facie* evidence for (3) (ibid., 23). How does plenitudinous platonism avoid this epistemological objection? Well, if every possible mathematical object exists, then any consistent mathematical theory is true of some district of the vast platonic realm. Therefore, coming to know that a mathematical theory is consistent—an item of logical knowledge, according to Balaguer—is all the evidence one needs in order to know that certain kinds of mathematical objects exist. Compare this result with traditional platonism: Traditional platonism assumes that only certain mathematical objects exist, which means that not all consistent mathematical theories will have representatives in platonic heaven. Thus one is cut off from a guarantee that the objects described in a particular mathematical theory actually exist. By moving from the assumption that *some* mathematical objects exist to the stronger premise that *all possible* mathematical objects exist, the guarantee is recovered. There is still the issue of the status of Balaguer’s controversial assumption that all possible mathematical objects exist. He defends this assumption by analogy, arguing that the plenitudinous platonist’s epistemological situation with respect to knowing that all possible mathematical objects exist is just like the external-world theorist’s epistemological situation with respect to knowing that the physical world exists (ibid., 53-8). I find this analogy highly implausible. But whether this defense of plenitudinous platonism is successful is ultimately unimportant for my purposes; I am only trying to give a sketch of Balaguer’s platonism.

Balaguer argues that the most serious problem facing fictionalism (and nominalist philosophies of mathematics in general) is what he calls the “Fregean Argument” against

fictionalism. This is the argument that the best explanation for the applicability of mathematics in science involves regarding mathematics as true, and that the best explanation as to why mathematics is true is the one given by the platonist (ibid., 95). Balaguer's response to this argument largely consists of amplifying and improving the sort of considerations advanced previously by Field, and I will not spend any time discussing them. And, as with Field, Balaguer recognizes the important role the notion of consistency plays in the fictionalist's account of mathematics; he also understands consistency as a modal primitive. Against those who claim that primitive modality is not a nominalistically acceptable notion, he remarks that, "since that notion is a *primitive* notion, it is entirely obvious that it isn't defined in terms of abstract objects, because it doesn't have any definition at all" (ibid., 71).⁴⁰

If Balaguer is right, then modality plays an even more important role in mathematics than one would have previously thought. If fictionalism is correct, then knowledge about logical possibility is needed in order to underpin the consistency of mathematical theories. If plenitudinous platonism is correct, then knowledge about logical possibility is *still* required in order to underpin the consistency of mathematical theories, for it is only through knowing that a mathematical theory is consistent that one can learn that it is true. This makes deciding between platonism and fictionalism a delicate matter. Nevertheless the selfsame modal knowledge is the epistemological foundation for both views. What this implies is that if skepticism about logical possibility is warranted, then the platonist and the fictionalist are affected *equally*. There is subsequently no sense at all to be made of the various reservations voiced earlier in this chapter about how nominalists are *particularly* troubled by the justificatory questions raised by their use of modality.⁴¹

⁴⁰Cf. my discussion of nominalism and primitive modality in chapter 3, §6.

⁴¹This last point is an important theme of the following two chapters.

1.5.2 Leng's Fictionalism

The most recent and thorough defense of the fictionalist position is found in (Leng 2010). Under the auspices of naturalistic philosophy she defends two important theses of the fictionalist program. First, she argues that nothing internal to the practice of mathematics supports the claim that mathematical theories are *true*. Second, in response to the indispensability arguments, she argues *on naturalistic grounds* that confirmational holism and scientific realism are false. The upshot is that fictionalism is a naturalistically acceptable position in the philosophies of pure and applied mathematics.

Leng believes that Field's "mathematical knowledge as logical knowledge" approach can be extended to the whole of mathematical practice. She uncritically endorses Field's arguments for accepting primitive logical notions. However, she expends some effort in defense of the idea that logical possibility is a primitive notion. She argues that a set-theoretic reduction of logical possibility is liable to confuse the order of explanation. The set theoretic reduction of logical modality explains possibilities and necessities as facts about what sets exist. On this picture, it is impossible for there to exist a set of all sets because... no such set exists! But on the contrary, according to Leng, the fact that it is impossible for there to exist a set of all sets is something that is explanatorily prior to the fact that in platonic heaven there is no set of all sets (ibid., 52). Leng concludes that the set-theoretic reduction of the logical modalities fails as a reduction, and thus requires a modal primitive. I suspect that this conclusion is unwarranted. But in chapter three I shall pursue the related claim that the set-theoretic reduction, even if true, does not provide a means for justifying assertions about what is logically possible, and hence does not provide an escape route away from the embarrassing kinds of questions that modal nominalism raises about what justifies modal assertions. But before I do this I want to come to a greater understanding of exactly why modal nominalists should be troubled by their use of modality. I take up this issue in the next chapter, where I discuss Stewart Shapiro's objections to modal nominalism.

Chapter 2

Shapiro's Challenge to the use of Modality in Nominalist Theories

I am inclined to think that existence in mathematics is "Consistency + X" but I do not know how to solve for X.

—Peter Koellner

2.1 Introduction

In this chapter I examine Stewart Shapiro's criticism of modal nominalist theories of mathematics. The criticism has three parts, which are described in section two. The first part involves showing that modal nominalist theories are "definitionally equivalent" to ordinary platonist mathematical theories in the sense that structure-preserving translations can be constructed between them. The second part argues that such translations preserve much more—in particular they preserve both the ontology and the epistemological problems of platonism. The third part of the criticism holds that platonists have, whereas modal nominalists lack, the necessary ontological resources (the set-theoretic reduction of the logical modalities) for justifying and ascribing content to the modal assertions that figure in modal nominalist accounts of mathematics. Together these three parts purport to show that (a) modal nominalists face epistemological problems that are just as serious as those facing platonists, (b) modal nominalists are unsuccessful in their attempts to eschew commitment to mathematical objects, and (c) concerning modality, that modal nominalists are particularly burdened by their use of primitive modality.

Section three provides my first response, which challenges Shapiro's claim that definitionally equivalent theories share an ontology. This discussion serves to expose and clarify an important element of Shapiro's criticism—his reliance on the structuralist view that,

as far as mathematical ontology is concerned, structure is all that matters. In fact what Shapiro believes is that definitionally equivalent theories have the same *structure* and are consequently committed to whatever ontological and epistemological burdens are to be had in virtue of their structural commitments; later sections are devoted to describing and criticizing these structuralist conclusions about the ontology of mathematical theories.

Shapiro does not maintain that his formal notion of definitional equivalence preserves meaning. Nevertheless as an aside I consider what moves are available if one does assume that the formal relationship which obtains between platonist and modal nominalist theories preserves meaning. In section four I discuss some arguments to the effect that modal nominalist theories are mere paraphrases of ordinary mathematical languages and that paraphrase is incapable of discharging ontological commitments. My response in section five is to reject this line of reasoning, ultimately because no modal nominalist of which I am aware is actually attempting to produce a synonymous paraphrase of ordinary mathematical language.

Returning to the main event, Shapiro's criticism of modal nominalism is best understood in the context of his overall structuralist position in the philosophy of mathematics. In section six I begin to explore whether the structuralist position is capable of validating the connections between structure and ontology that are alleged to raise problems for modal nominalism. I examine both the views of Michael Resnik and Shapiro. Ultimately they have very similar things to say about the connection between structure and ontology: That for a mathematical structure to exist *just is* for that structure to be possible. Shapiro in particular speaks of the *coherence* (a primitive notion akin to second-order satisfiability) of an implicit definition of a structure (a set of axioms characterizing a mathematical theory). Shapiro believes that all coherent implicit definitions characterize actually existing mathematical structures—objects that resemble *ante rem* universals. This belief is codified as an axiom of Shapiro's structuralism: the Coherence axiom.

The success of the first two parts of Shapiro's criticism turns on whether the Coherence

axiom can be justified. In section seven I argue that nothing in the practice of mathematics lends direct support to the Coherence axiom. Though mathematicians may use something like this axiom to establish the existence (or good standing) of certain kinds of mathematical entities, this does not lend plausibility to Shapiro's existential claims about *ante rem* structures. He suggests that the Coherence axiom can be justified on account of its inclusion in a philosophy of mathematics that provides the most plausible explanation of the overall enterprise of mathematics. However, this plausibility result is available only *after* the criticisms of modal nominalism run through, so this holistic justification for the Coherence axiom is ultimately circular.

Section eight ends the chapter with a discussion of what is to be made of the third part of the criticism—that modal nominalists are particularly burdened by their use of primitive modality. This section also lays down a codification of Shapiro's Challenge, which comes to the following: For the modal nominalist to explain, on nominalistically acceptable grounds, why she is justified in asserting the modal claims that figure in her theories, and further to explain, again on nominalistically acceptable grounds, why she is entitled to apply the results of modal logic when constructing and applying her theories. Although a case can be made that modal nominalists have addressed the latter challenge—e.g., Chihara by way of his natural language interpretation of modal logic (see the previous chapter for discussion)—nevertheless modal nominalists have seldom bothered to address the former. Shapiro is correct to claim that modal nominalists lack clear justifications for the modal assertions that figure in their theories. However, it does not follow that the modal nominalist thereby resides in a uniquely damaging position. This is because *no one*—the platonist included—seems to be any better justified in asserting the relevant modal claims (including the platonistic counterparts of the nominalist's modal claims). I begin to make the case that the set-theoretic reduction of the logical modalities fails to provide an acceptable means through which to justify claims about, e.g., the consistency of PA, a case that is completed in the proceeding chapter.

2.2 Modality and Ontology

In (Shapiro 1993) and in the seventh chapter of (Shapiro 1997)¹ Shapiro examines what should be made of an apparent tradeoff in the nominalism/platonism debate.² Platonism is thought to face intractable epistemological problems stemming from its postulation of abstract mathematical objects. If mathematics is about a realm of abstract objects, and if concrete human beings have no direct epistemic access to this realm, then it follows that human beings do not have knowledge about mathematical objects, and hence do not have mathematical knowledge. But surely humans do possess a great deal of mathematical knowledge. Platonists are burdened with the problem of accounting for the apparent reliability on the part of mathematicians to form beliefs about abstract mathematical objects, without violating any constraints imposed by naturalized epistemology. Thus platonists may not avail themselves of any purported ability to directly apprehend the mathematical realm—no use may be made of any mysterious faculties of intuition. Nominalists, including modal nominalists, say that the benefits (if there be any) of the platonist's added ontology come at too high a cost. Nevertheless nominalists, including modal nominalists, agree that they must say *something* about mathematics. The particular strategy I defend in this dissertation is one that requires taking on some measure of primitive modality. Thus, platonists are burdened by an increased ontology and the epistemological difficulties that come along with this ontology; meanwhile modal nominalists are burdened by irreducible modal concepts and apparently unjustified claims about the consistency of mathematical theories. According to Shapiro, “we are invited to consider a tradeoff between a vast ontology and an increased ideology” (1993, 456). But, *prima facie*, the ontological commitments of the platonist and the ideological commitments of the modal nominalist are incommensurate, and it is subsequently unclear how to assess such a tradeoff. That is, it is

¹Essentially a rewrite of (Shapiro 1993); the main arguments remain unchanged, and in many cases they are repeated verbatim.

²Shapiro prefers the terms ‘anti-realism’ and ‘realism’ in place of ‘nominalism’ and ‘platonism.’ I adopt both usages in this chapter.

by no means obvious which burden is greater, or if the burdens are the same, making the choice between modal nominalism and platonism itself intractably difficult!

Shapiro reaches a series of conclusions that, taken together, imply modal that nominalism is not to be preferred on either ontological or ideological grounds. The first of these conclusions is that the modal nominalist's invocation of primitive modality produces novel epistemological difficulties concerning how humans acquire knowledge about the modal claims that figure in modal nominalist theories, and further that these novel epistemological difficulties are *just as* troublesome as the epistemological difficulties facing platonism. At this stage, the ontology/ideology tradeoff is balanced on the epistemological fulcrum, and if this were the end of the matter, Shapiro would be willing to entertain the idea that modal nominalism is to be preferred on account of its ontological asceticism. However, a subsequent conclusion holds that modal nominalists *do not* succeed in eschewing commitment to mathematical entities. Therefore modal nominalism is not to be preferred on ontological grounds, leaving the modal nominalist to search after some other medium in which to limn the superiority of her views. Finally, Shapiro argues that the platonist resides in a *superior* situation with respect to modality, for via the set- or model-theoretic reduction of the logical modalities, she can give content to and provide justifications for the relevant modal claims, e.g., the claim that PA is consistent, whereas the modal nominalist appears to incur the consistency of PA as a brute and unanalyzable assertion.

2.2.1 The Emperor's New Epistemology

A theme from the previous chapter is that it is possible to eschew commitment to mathematical objects and thereby escape the epistemological difficulties facing platonism by appealing to modality in some way. Charles Chihara's escape route endorses the metaphysical possibility of constructing open-sentence tokens; Geoffrey Hellman's method appeals to the primitive logical possibility of the existence of models of mathematical theories; and Hartry Field's strategy reads the consistency of a mathematical theory as an assertion of the primitive logical possibility of the conjunction of its axioms. In the previous chapter I

also suggested that the appeal to primitive modality raises new questions concerning how humans are supposed to justify and gain knowledge about the various modal assertions that Chihara, Hellman, and Field make.³ Shapiro recognizes this latter point, noting that the modal nominalist, “seems to require an epistemology of the actual and possible, and it not clear that this is a gain” (ibid., 462). Furthermore, modal nominalists, “accept a *primitive* notion of possibility, and we are left with very little idea of what this notion comes to” (ibid., 465; emphasis added). I take these remarks to be consonant with the results of the previous chapter, and I will not belabor the point. However, Shapiro argues that a stronger conclusion is warranted—that the modal nominalist faces epistemological problems that are *identical* to the epistemological problems facing the platonist.

Shapiro aims to show that the, “epistemological problems facing the anti realist programmes are just as serious and troublesome as those facing realism. Moreover, the problems are, in a sense, *equivalent* to those of realism” (ibid., 456).⁴ How is this to be done?

I show that there are straightforward, often trivial, translations from the set-theoretic language of the realist to the proposed modal language, and vice-versa. The translations preserve warranted belief, at least, and probably truth . . . Under certain conditions, the regimented languages are definitionally equivalent, in the sense that if one translates a sentence ϕ of one language into the other, and then translates the result back into the original language, the end result is equivalent (in the original language) to ϕ . The contention is that, because of these translations, neither system can claim a major epistemological advantage over the other. Any insight the modalist claims for his system can be immediately appropriated by the realist, and vice-versa. The problem, however, lies with the “negative” consequences of the translations. The epistemological *problems* with realism get “translated” as well. The prima facie intractability of knowledge of abstract objects indicates an intractability concerning knowledge of the modal notions, at least as they are developed in the works in question here. (ibid., 457)

Shapiro’s reasoning runs roughly as follows: Begin with an ordinary mathematical assertion p that follows from a certain set of axioms AX . A nominalistically acceptable

³Moreover, these new questions get raised regardless of one’s motivation in reinterpreting mathematics. Modal nominalists who do not view platonism’s defective epistemology as a rallying call are nevertheless required to address concerns about the justification of modal assertions.

⁴Later on the same page he adds that, “[p]erhaps the source of the epistemological difficulties lies in the richness of mathematics itself.”

rendering of the content of p may be obtained by any number of strategies, but here I restrict my attention just to the major views discussed in the previous chapter—Chihara’s Constructibility Theory, Hellman’s Modal Structuralism, and Field’s fictionalism. In Field’s case, p becomes $\diamond(AX \wedge p)$, an assertion of the consistency of p together with the axioms AX (ibid., 462). In Hellman’s case, slightly more is needed. One must state the possible existence of a model of AX , $\diamond AX$, together with the claim that it is necessary that p follows from AX , $\Box(AX \rightarrow p)$. One is then licensed to assert $\diamond p$ (ibid., 466). Still more is needed in Chihara’s case. One must identify the type-theoretic version of p , say, p_t , and then: exchange all of the quantifiers of p_t with constructibility quantifiers, replace any predication symbols in p_t with satisfaction symbols, and replace any mention of types in p_t with open-sentence tokens (ibid., 468). The upshot is a smooth translation scheme from the language of set theory into the languages of the various modal nominalist theories.

At this point Shapiro suggests that modal nominalism, “faces direct counterparts of every epistemic problem with realism” (ibid., 462). He explains:

A fundamental problem for the realist is “How do we know ϕ ” or, to be philosophically explicit, “How do we know that ϕ holds of the highly abstract ontology?” Under the translation, this becomes “How do we know that ϕ is possible?” or “How do we know that the conjunction of ϕ with the axioms of the background theory is possible?” (ibid., 463)

This suggestion amounts to the claim, that, for example, any reason one has for doubting the truth of the axioms of ZF can be recast as a reason for doubting the consistency of the axioms of ZF. To say otherwise would be tantamount to averring that the major epistemological problems of mathematics can be solved by inserting modal operators into mathematical formulae. But Shapiro insists that, “inserting boxes and diamonds into formulas, or changing the quantifiers, does not, by itself, add epistemic tractability” (ibid., 474).

The real trouble begins with the recognition that the translations from the platonist’s language into the modal nominalist’s language can be reversed. In Field’s case, replace

possibility with satisfiability (ibid., 462). In Hellman's case, replace possibility with satisfiability and necessity with logical truth (ibid., 466). And in Chihara's case, replace open sentences with types of the appropriate level, exchange satisfaction for predication, and change the constructibility quantifiers to existential quantifiers (ibid., 468). The upshot is a smooth translation scheme from the languages of the various modal nominalist theories into the language of set theory. Moreover, if a sentence p of set theory is translated into the sentence p' of a modal nominalist language, and p' is then translated into a sentence p'' of the original set-theoretic language, p is equivalent in set theory to p'' . According to Shapiro it then follows that

... the fact that there are such smooth and straightforward transformations between the ontologically rich language of the realist and the supposedly austere language of the [modal nominalist] indicates that neither of them can claim a major epistemological advantage over the other. (ibid., 463)

He reasons thusly: The platonism \rightarrow modal nominalism translations show that any major epistemological problem of platonism is correlated to some epistemological problem for modal nominalism. The modal nominalism \rightarrow platonism translations show that any major epistemological problem for modal nominalism is correlated to some epistemological problem for platonism. Moreover, any important epistemological *insights* can be shuffled around as well. Any support the modal nominalist can muster for some particular modal assertion can be appropriated by the platonist on account of the modal nominalism \rightarrow platonism translations, just as any support the platonist can muster for some particular set-theoretic claim can be appropriated by the modal nominalist on account of the platonism \rightarrow modal nominalism translations.⁵

Shapiro observes throughout that modal nominalists must invoke *primitive* modal concepts. Platonists, meanwhile, can appeal to the model- or set-theoretic reduction of the

⁵ "... unless the realist invokes some sort of (non-natural) direct apprehension of the mathematical realm, any sort of evidence one can cite for believing in a mathematical assertion ϕ can be invoked by the [modal nominalist] in defence of belief in ϕ' ... But given the other translation—[modal nominalism] to realism—doesn't the reverse apply as well?" (ibid).

modal notions. Necessary truths are those that come out true in every model. Possible truths are those that come out true in at least one model.⁶ Shapiro moreover claims that,

... once the model-theoretic reduction is in place, the realist has a *lot* to say about logical possibility and logical consequence. It is a gross understatement to point out that mathematical logic has been a productive enterprise. It is not clear that [the modal nominalist] can use the results of model theory, as they bear on his (primitive) modal notion. (ibid., 464)

The upshot, Shapiro alleges, is that modal nominalists are *particularly* burdened by their use of modal concepts. Modal nominalists have not provided any rigorous analyses of the various modal notions employed in their theories. However, modal locutions are well-entrenched in natural languages. Competent speakers routinely use, believe, and act upon modal propositions. Nevertheless these, “everyday modal... notions, by themselves, are too vague to support the detailed applications to surrogates of set theory or type theory as envisioned by [modal nominalists]” (ibid., 475).⁷ Commonsense beliefs about modality may be well-enough equipped to speak about mundane modal facts, such as whether it is possible for Ohio State to win at Notre Dame, but that is a far and distant cry from asserting the possibility of the conjunction of the axioms of ZFC. What is more,

... in practice, our grasp of modal... terminology, as applied to mathematics at least, is very much mediated by mathematics, set theory in particular. We inherit the language/framework with the connections to set theory already forged—and to use a worn metaphor, we can’t get off the ship of Neurath. Surely, our [modal nominalists] don’t claim that we still have some sort of pre-theoretic intuitions of these notions, intuitions that remain uncorrupted, or at least unmodified, by set theory. (Shapiro 1993, 475)⁸

Consider Paul Cohen’s proof that the Continuum Hypothesis (CH) is independent of the axioms of ZFC. He did this by constructing *models*. That ZFC±CH is consistent is thereby knowledge about derivations of set theory—what entitles the modal nominalist to proclaim this item as knowledge about some *primitive* possibility claim? And how could

⁶For a more detailed account of the model-theoretic reduction, see chapter three, §2 and §5.

⁷See also (Hale 1996).

⁸Similar remarks are expressed in (McKeon 2004, 421).

a modal nominalist ever arrive at Cohen's result without using model theory? Against Chihara, Shapiro responds that,

... we only understand how the constructibility locutions work in *Chihara's application to mathematics* because we have a well-developed theory of logical possibility, satisfiability, etc. And, once again, this theory is rooted in set theory, via model theory. That is the source of the precision. (Shapiro 1993, 470; Shapiro's emphasis)

Similar remarks apply to Hellman and Field; the manipulation of the modal apparatus of either view takes advantage of modal-logical inferences best understood in reference to model theory. One might adopt a fictionalism with respect to model theory, but this would presumably invoke higher-order inferences, forcing a modal nominalism-platonism standoff ascending through "a hierarchy of metalanguages" (*ibid.*, 465). Shapiro's final judgment on the modal nominalist's use of modality is that she,

... owes us some account of how we plausibly could come to understand the notions in question (as applied here) *as we in fact do*, independent of our mathematics. Without this, it is empty to use a word like "primitive" and, without this, we can't give a positive assessment of the progress of anti-realist programs, or even a judgement that they have achieved a balanced tradeoff. (*ibid.*, 475)

Notice that Shapiro speaks of the modal nominalist encountering difficulty in *understanding* the modal notions. But it must be recognized that the resources by which someone comes to *understand* a proposition p need not be the very same resources that are responsible for *grounding* or *making it the case* that p .⁹ Shapiro takes it as a given that if the modal nominalist were right about modality, then modal assertions would have primitive grounds. For instance, if the modal nominalist is right, then the consistency of PA is a brute and unanalyzable fact. Nevertheless it appears to be a (relatively) uncontroversial datum that most mathematicians know what it means to assert the consistency of PA, and this is a datum for which the modal nominalist can provide no explanation. She might hope to salvage an explanation via model theory, but if she rejects the purported ontology

⁹Thanks to Susan Vineberg for pressing this distinction.

of model theory she is left with an unexplained correlation between the primitive modal facts and the facts of model theory (e.g., she seems to have no explanation for why the model theory reliably tracks the facts about primitive modality). I think it is this kind of unexplained correlation that drives Shapiro to speak against the plausibility of the modal nominalist's reliance on primitive modality. Since the nominalist can give no explanation of the uncontroversial datum, her view ought to be rejected in favor of a view that *can* explain the datum. The platonist offers such a view in identifying assertions of consistency with certain model- or set-theoretic constructions. So the complaint is, in essence, that the platonist has explanatory resources that the nominalist lacks, and moreover that any plausibility the modal nominalist's explanations might have is (perhaps covertly) owed to model theory (of a platonist bent).

Shapiro, then, lodges two kinds of complaints against the modal nominalist *a propos* of modality. One complaint is that it is something of a mystery as to how modal nominalists propose to justify the modal assertions that figure in their theories, perhaps as a consequence of the fact that modal nominalists have rather little to say about the content of these claims. Meanwhile, if Shapiro is right, the platonist can appeal to her prior beliefs about models or sets in justifying modal assertions. For example, the claim that PA is consistent can be identified with the claim that PA has a model; the existence of a model satisfying PA then serves as evidence for the consistency of PA. A second complaint is that modal nominalists have not earned the right to employ the results of model theory and modal logic to make derivations from their primitive modal premises. In both cases the complaint is that modal nominalism is importantly impoverished when compared to platonism. Modal nominalists have only made serious attempts to combat the second criticism—e.g., (Chihara 1998)'s NL interpretation of modal semantics and Field's insistence that the metalogical notions are themselves primitively modal (see the previous chapter for discussion). But that is only to go halfway toward responding to Shapiro's criticism. Modal nominalists still face core questions that stem not from

the simple fact of their *use* of modal *reasoning* but instead from the categorical modal *assertions* they make—e.g., in Hellman’s case, the claim that it is possible for there to exist a model of the axioms of second-order PA. What content can a modal nominalist ascribe to such an assertion? And how could a nominalist justify such an assertion? Shapiro’s allegation is that the metaphysical resources of platonist model theory provide the most (and perhaps the only) insightful answers to these questions—something that I contest in this dissertation. (To anticipate developments from chapter three, I should add that I also contest the idea that the problem Shapiro identifies for the modal nominalist is, principally, an *epistemological* problem. That modal nominalists appear unable to justify assertions about what is possible would seem to be a symptom of a deeper, metaphysical problem, *viz.*, that modal nominalists, in espousing modal primitivism, fail to describe the *content* of modal assertions. I will overlook this detail for the remainder of the chapter, proceeding with Shapiro in describing the modal nominalist’s justificatory problems as *epistemological*, rather than metaphysical in nature.)

If Shapiro is right, then the modal nominalist’s ploy in no way helps her view gain tractability over platonism. At best, she faces epistemological problems that are just as burdensome as those facing the platonist. At worst, she has no story whatsoever to tell about how human beings can explain and acquire knowledge about the consistency of mathematical theories, whereas the platonist can present her model-theoretic constructions.¹⁰ The epistemological fulcrum is therefore an equivocal measure of the merits of modal nominalism and platonism. Shapiro believes that the natural next step for the modal nominalist would be to argue for modal nominalism on account of its more economical ontology. Now, as I have indicated previously, I do not myself take this to be the only or most effective way of arguing for the modal nominalist theories defended in this dissertation. Rather, my sympathy for the modal nominalist approach comes to the idea that one should not believe any claim without the right kind of evidence, and there are good reasons

¹⁰Of course, wherefore art the platonist’s warrant in the underlying set theory needed to produce the model-theoretic constructions? Of this, more later.

for thinking that platonists have not provided compelling evidence for the existence of mathematical objects. Shapiro, as discussed below, views mathematical structures as free-standing abstract objects akin to *ante rem* universals; and Burgess, as I argue in the fourth chapter, approaches mathematical languages with literalist-platonist presuppositions. The problem is that these resources—Shapiro’s conception of abstract structure, Burgess’s literalist presuppositions—do not have a plausible source in the practice of mathematics itself, and consequently, without cogent supplementary reasoning, they ought to have no purchase on the naturalist’s convictions. Given the naturalistic subtext of this dissertation, these are important results. The motivation for modal nominalist theorizing, then, arises from recognizing that (a) mathematics and science internally provide no good reasons for supposing that mathematical objects exist, and that (b) there is nevertheless interest in accounting for mathematical reasoning and the content of mathematical theories in a fashion that coheres with the absence, internal to mathematics and science, of reasons for supposing that mathematical objects exist. I will have occasion to expand on these remarks later in this chapter and in the naturalism section of the dissertation. However, at the moment I am not trying to argue *for* modal nominalism but instead to deflect objections to certain modal nominalist views. Modal nominalists do maintain that they are not ontologically committed to mathematical objects, but appearances can be deceiving; perhaps they are not even successful in eschewing commitment to mathematical objects in the first place—a conclusion that Shapiro goes to great lengths to establish.

2.2.2 The Emperor’s New Ontology

According to Quine’s criterion of ontological commitment, a theory is ontologically committed to the objects over which it quantifies. When the logical apparatus of a theory is restricted to non-extended first-order logics, one can read the existential quantifier as apportioning such commitments. The ontological commitments of the traditional platonist position are clear in this respect; mathematical existence assertions are expressible in first-order languages. Meanwhile, Shapiro champions a structuralist position according to

which the “logic of mathematics” is second-order logic, which muddies the ontological waters as far as Quine is concerned.¹¹ Similarly, modal nominalists envision an expansion of the logical apparatus to include modality. (Hellman’s account of mathematics uses *second-order* modal logic—a departure from Quine in two respects!) It is subsequently unclear, via Quine, to what the various modal nominalist accounts of mathematics are ontologically committed. This prompts Shapiro to make the following assessment:

... we need a new tool to assess the ontology/ideology of a philosophical/scientific/mathematical theory. Quite simply, the Quinean ontology-through-bound-variables thesis fails if ideology is not held fixed, if not [held] minimal. Indeed, the criterion is outright misleading. (Shapiro 1993, 476)

If Quine’s criterion is incapable of delivering a decision as to which position has the most economical ontology, what else is there to be said on matters? Should one simply turn one’s back to the epistemological debate and give up on ontology? But, at least as far as platonism is concerned, the epistemological questions are *driven* by the view’s abstract ontology. So the ontological question is propaedeutical.

Shapiro hopes to remedy this situation by concocting a novel measure of the ontological *and* ideological commitments of a theory. This remedy is unapologetically inspired by his structuralist views:

As a first approximation, the proposed criterion of ontology/ideology is this: A theory is committed to at least the structure or structures that it invokes and uses. If two theories involve the same structures or if the systems described by them exemplify the same structures, then, at least as far as mathematics goes, their ontologies/ideologies are identical. (Shapiro 1997, 238)

So if two theories invoke or exemplify the same structure, then they have identical ontological *and* ideological commitments. Of course, what is missing from this first attempt are criteria of structure-identity and structure-individuation; without such criteria it would be

¹¹(Quine 1986, 67) warns against treating quantification over predicates as carrying ontological commitment to predicates, propositions, or attributes, instead preferring to treat second-order assertions as elliptical for claims about sets of individuals; hence the slogan, “second order logic is set theory in sheep’s clothing.” Thanks to Michael McKinsey for bringing this issue to my attention.

impossible to determine whether any two theories invoke the same or different structures. Recall also that epistemological problems are alleged *not* to be lost in the translations between nominalist and platonist languages. The technical maneuver Shapiro utilizes in support of this claim is showing that the theories are *definitionally equivalent*. In precise terms,

Two theories T, T' are said to be *definitionally equivalent* if there is a function f_1 from the class of sentences of T into the class of sentences of T' and a function f_2 from the class of sentences of T' into the class of sentences of T , such that (1) f_1 and f_2 both preserve truth (or theoremhood if the theories do not have intended interpretations) and (2) for any sentence ϕ of T , $f_2 f_1(\phi)$ is equivalent in T to ϕ , and for any sentence ψ of T' , $f_1 f_2(\psi)$ is equivalent in T' to ψ . (Shapiro 1993, 479)

What is the significance of learning that two theories are definitionally equivalent?

I propose that definitional equivalence serve as a criterion of the formal strength of modal and nonmodal theories and... that this notion be used as an indication that the intended structures, and thus the ontology/ideology of different theories, are the same. If T is definitionally equivalent to T' , then neither is to be preferred to the other on ontological/ideological grounds. (Shapiro 1997, 242)

It follows that if a modal nominalist theory is definitionally equivalent to set theory (or type theory), then these two theories have identical ontological commitments; both theories are committed to the same *structure*. Thus modal nominalists like Chihara, Field, and Hellman do not succeed in eschewing commitment to mathematical objects—each must countenance mathematical structures.

Shapiro has constructed a broad-ranging criticism of modal nominalist theories of mathematics. That modal nominalist theories are definitionally equivalent to set theory allegedly shows that (a) modal nominalists face correlates of the epistemological difficulties facing platonists, and (b) modal nominalists do not succeed in eschewing commitment to mathematical objects. Moreover, the absence of a nominalistically kosher reductive account of logical possibility means that (c) platonism is superior to modal nominalism

with respect to the justification of modal assertions. If his arguments are sound, then he has seriously undermined the modal nominalist approach in philosophy of mathematics. In the remaining parts of this chapter I respond to all three developments. After considering various unsuccessful ways of amplifying Shapiro's arguments, I conclude that he fails to show that modal nominalists are unsuccessful in eschewing commitment to mathematical objects, vitiating claim (b). In particular, I argue that Shapiro's ontological parity result is only available once his Coherence axiom is up and running—an axiom that owes its plausibility in large part to the criticisms described above. And, although I concede that modal nominalists face justificatory difficulties stemming from their invocation of primitive modality, I make a preliminary case—to be filled out more fully in chapter three—for thinking that the modal nominalist's burdens are no more taxing than those facing the platonist, countering (c) and undermining the significance of result (a).

2.3 First Reply

My first reply concerns the purported implications of definitional equivalence. Shapiro offers definitional equivalence as a criterion of structure-identity. Any two theories that are definitionally equivalent invoke the same structure, and therefore have identical ontological commitments.

According to John Corcoran, definitional equivalence is characterized in the following way:

Let DST be a set of (nominal) definitions of the (uninterpreted) primitives of S in terms of those of T. DTS is likewise defined. Suppose that when DST is adjoined to T, the axioms of S *and* the definitions of DTS follow. Suppose also that when DTS is adjoined to S, the axioms of T *and* the definitions DST follow. When T and S are so related, I call them *definitionally equivalent*. (1980, 231)

Do Shapiro's translations show that set theory is definitionally equivalent to modal nominalist theories in this sense? One would have to *prove* as theorems of the modal nominalist theories (extended to include nominalized set-theoretic primitives), (a) the axioms of set theory, and (b) the set-theoretic definitions of the nominalized primitives. As well, one

would have to *prove* as theorems of an extended set theory (a') the axioms of the modal nominalist theories, and (b') the nominalized definitions of the set-theoretic primitives. What 'proof' means here, as far as Shapiro is concerned, is somewhat unclear. Are such proofs to be constructed in first-order or second-order logic? And what other logical resources must be made available? Corcoran says that his notion is neutral between the underlying logic of the theories in question (*ibid.*). I am not confident whether he means by this that (i) it does not matter if either theory uses the same or different underlying logics, or (ii) it does not matter what the underlying logic of both theories are, provided that both use the same underlying logic.

In any case Shapiro does not endeavor to explain how one is supposed to embed his translations in the actual theoretical apparatus of either theory. All that Shapiro requires is that the equivalences (between ϕ and $f_2 f_1(\phi)$ in T , and between ψ and $f_1 f_2(\psi)$ in T') can be appreciated by neutral observers (Shapiro 1997, 225). In order to actually construct the requisite proofs (on Corcoran's picture), Shapiro's translations must be viewed as interpreting definitions. In the case of a modal nominalist theory, the modal nominalism \rightarrow platonism translations constitute the definitions of set-theoretic primitives in terms of the primitives of the modal nominalist theory. The result needed is that when the modal nominalist theory is adjoined with this translation scheme, the axioms of set theory *and* the definitions of the primitives of the modal nominalist theory in terms of the primitives of set theory follow *as theorems* (and vice-versa for the other direction). That the set-theoretic axioms follow can be shown simply by translating the appropriate sentences. This is most easy to see in Hellman's case. Take the statement of the possible existence of a model of the axioms of mathematical theory and "translate" it into an assertion of the *existence* of a model of the axioms a mathematical theory. Reversing the translations provides the means for deducing the definitions of the primitives of the modal nominalist theory in terms of the primitives of set theory. A similar story can be told by starting with set theory and adjoining to it the set-theoretic definitions of the primitives of the

modal nominalist theory. But if the underlying logics of the theories are different, then it is *prima facie* implausible that these translations alone preserve theoremhood (in the sense that anything that can be proven in one theory can also be proven in the other). Thus the preservation of theoremhood must be viewed as an additional criterion that such translations must meet in order to count as definitionally equivalent in Shapiro's sense.

I am concerned about the degree to which it is coherent to think of Shapiro's translations as mere definitional extensions. Are these translations to be thought of as conservative extensions? That surely cannot be the case, because the variables of the platonist's theory (set theory) and the variables of the modal nominalist's theory range over disjoint sets of objects. But then in what sense is the reader to believe that Shapiro's translations capture the set-theoretic primitives *in terms only* of the primitives of the nominalist theory, and vice versa? And if Shapiro's translations are not conservative extensions, then why should proponents of either theory grant them any credence in the first place?

What is the upshot of all of this? Corcoran remarks that definitional equivalence, "preserves categoricity, decidability and completeness" (1980, 231).¹² None of these notions are explicit factors concerning the ontological commitments of a theory (at least when the theory in question is first-order). Thus, if two theories are definitionally equivalent in Corcoran's sense, they need not have identical ontological commitments.

Perhaps the comparison with Corcoran is misleading; perhaps Shapiro is not attempting to capture a pre-existing notion of definitional equivalence but is instead attaching a new sense to the term.¹³ Granting Shapiro's usage, Chihara provides a *prima facie* example of two theories that appear to meet Shapiro's conditions for definitional equivalence (or, "Shapiro equivalence" in Chihara's terminology), but clearly have distinct commitments:

Suppose theory *A* is a first-order theory about Mr. Jones's cats and theory *B* is a first-order theory about Mr. Smith's dogs. And suppose that *A* and *B* are Shapiro equivalent (the domain of *A* consists of the cats of Jones, and the

¹²For a similar result, consult (Pinter 1987).

¹³Charles Chihara writes that, "it should be noted that some logicians have questioned the appropriateness of Shapiro's conditions for *definitional equivalence*" (2004, 186).

domain of B consists of the dogs of Smith; what A says about the number of males Jones has, B says about the number of males Smith has—and it just happens to be the case that both theories are true). Theory A is no mere notational variant of theory B , for B is a theory about dogs, not cats. One could not explain the barking emanating from Smith's house by appealing to theory A . (ibid., 188)

Although I am sympathetic with this response, I imagine Shapiro would not be much troubled by it. On the epistemic front, I suppose one could bite the bullet and accept theory A as providing a good explanation for the barking emanating from Smith's house. Suppose that 'Some dog barks' is provable in B . If A and B are definitionally/Shapiro equivalent, there must be a translation of this theorem into A , say, that 'Some cat meows,' that is provable in A . If one is in a position to *know* that A and B are related in this way, then I see no reason why 'Some cat meows' fails to constitute evidence for 'Some dog barks.' As an *explanation* for 'Some dog barks,' it might not be accorded the same virtues as an explanation solely in terms of B , but that does not imply that the explanation in terms of A is altogether useless. Shapiro endorses a similar reply regarding mathematical theories:

The neutral observer sees that if the realist finds good reason to believe Φ , according to his own lights, then the [nominalist] can find good reason to believe Φ' in her framework. The neutral observer sees that the fictionalist has a good reason to believe Ψ if and only if the realist has a good reason to believe Ψ' . (Shapiro 1997, 225)

The damaging suggestion, however, is that in Chihara's example, the "Shapiro equivalence" between A and B implies that Mr. Jones's cats are *identical* to Mr. Smith's dogs (and on the ideological front, that doghood is indistinct from cathood). I suspect Shapiro would maintain that this conclusion misses the point—what is of interest are the ontological/ideological commitments of *mathematical* theories, and not of theories in general. Indeed, Shapiro gives no indication that definitional equivalence has ontological implications for theories in general; his result holds only "as far as mathematics goes" (ibid., 238). A truer test would involve concocting a *mathematical* example of definitionally equivalent

theories that nevertheless have distinct ontological commitments.

Chihara invites the reader to compare Russell's simple theory of types with the standard set-theoretic version of type theory (2004, 188-91). According to Chihara, there are relatively smooth translations between the language of Russell's simple theory of types and the set-theoretic version of type theory. However, he points out that Zermelo's Well Ordering Theorem is not, strictly speaking, a theorem of Russell's simple theory of types. In Russell's type theory, a proof of well-ordering requires the conditional assumption of the Axiom of Choice. Meanwhile, the set-theoretic version of type theory assumes Choice as an axiom, and subsequently includes well-ordering as a theorem. It follows that inter-translatable theories, even in mathematics, do not preserve theoremhood. However, on my understanding of Shapiro, the preservation of theoremhood is a *condition* for definitional equivalence, and not a *consequence* of it. Thus, one could dispute whether Chihara's mathematical example poses a genuine problem for Shapiro.¹⁴ Nevertheless Chihara claims that the moral is that,

All that [Shapiro] has done is to provide us with a method for translating sentences of the Constructibility Theory into sentences of simple type theory and a method for translating sentences of simple type theory into sentences of the Constructibility Theory, these translations preserving certain mathematically significant relationships. . . We can have such methods of translation, even when a sentence in one theory is quite different in meaning and logical significance from the sentence of the other theory into which it is translated. (2004, 192)

Shapiro does not claim that his translations preserve meaning (Shapiro 1997, 224),¹⁵ and one would search in vain for any passage in which he denies or appears to disagree with any statement in the previous quotation. Nevertheless, the ontological commitments of a theory are *typically* exposed by *interpreting* the theory, i.e., by providing a semantics for the theory. Thus ontological commitment is, ordinarily, linked to meaning. The thought that Constructibility Theory and simple type theory have distinct ontological commitments is

¹⁴It would be revealing if, e.g, Shapiro somewhere argued that Russell's simple theory of types exhibited the same structure as the set-theoretic version of type theory, but to my knowledge he has never done this.

¹⁵In the next section I explore what moves are available if this claim is dropped.

bound up with Chihara's contention that sentences of simple type theory are distinct in meaning from their correlates in Constructibility Theory.

This discussion raises an important distinction between the ontological/ideological commitments held by a theory in virtue of its quantifiers/predicates, and the ontological/ideological commitments held by a theory in virtue of its structure. The traditional Quinean ontological project exposes commitments solely in virtue of quantifiers (or, to be more specific, solely in virtue of *first-order* quantifiers in appropriately regimented languages). But Shapiro is not a typical ontologist when it comes to mathematics: Two theories are said to have the same *mathematical* ontology when they are definitionally equivalent. The import here is not that, despite appearances to the contrary, the theories are quantifier-committed to the same ontology. Rather, the point is that, in virtue of being definitionally equivalent, each theory describes the same *structure*. Subsequently both theories share in any commitments to be had in virtue of their exemplifying the same structure. And, *prima facie*, two theories can share in such structure-commitments while having distinct quantifier-commitments.

Thus the "certain mathematically significant relationships"—i.e., the *structural properties* of mathematical theories—are what inflate the mathematical realm. Two definitionally equivalent theories are said to have the same ontological commitments, as far as mathematics goes, because of the commitments they have *in virtue of exemplifying the same structure*. Moreover, these are kinds of relationships that Chihara seems happy to recognize. Therefore, it again seems possible for Shapiro to accuse Chihara of missing the point. *Mathematical* ontology is not determined by the semantics of mathematical theories but instead by the structural properties of mathematical theories. But this claim is as bold as it is unobvious. What is Shapiro's motivation for contending that the ontology of mathematics is highlighted by the *structure* of mathematical theories, when for most other kinds of theories this is not thought to be the case? And is this contention plausible on independent grounds? In a short while I shall consider in detail what the mathematical structuralist

position is and how mathematical structuralism relates to mathematical ontology, but first I would like to examine another possible reply that might be made on Shapiro's behalf. Those not wishing to lose the thread of the main dialectic may proceed directly to §6 without loss.

2.4 The Paraphrase Response

Modal nominalist accounts of mathematics are often viewed as strategies for *paraphrasing* ordinary mathematical assertions into ontologically innocuous assertions. John Burgess, Bob Hale, and Gideon Rosen have all pursued objections to nominalism (including modal nominalism) on the grounds that paraphrase is incapable of paring down ontological commitments. Something like this view of modal nominalism is suggested by Shapiro's insistence that it is possible to translate sentences of set theory into sentences of modal nominalist theories, and vice versa. I would like to consider whether one of Shapiro's conclusions—that modal nominalist accounts of mathematics fail to eschew commitment to mathematical objects—can be better supported by viewing modal nominalist accounts of mathematics as paraphrases of ordinary mathematical assertions. The reader should be aware that Shapiro nowhere endorses this tactic, and he openly acknowledges that the translations needed to establish definitional equivalence are not meaning-preserving (*ibid.*). I argue that these objections ultimately fail because modal nominalist accounts of mathematics are not mere paraphrases of ordinary mathematical languages.

What important insight is available if modal nominalist accounts of mathematics are viewed as paraphrases of ordinary mathematical assertions? Intuitively, if a sentence S' is a paraphrase of a sentence S , then S and S' can both be used to express the very same proposition; S and S' make the same claim about the world. And if S and S' make the same claim about the world, then it is hard to imagine how S and S' could differ in their commitments. On the other hand, if S and S' differ in their commitments, then intuitively they do not make the same claim about the world. It is subsequently difficult to see why

S' should be regarded as an acceptable paraphrase of S . In the present case, either modal nominalist accounts of mathematics are acceptable paraphrases and so do not differ in their commitments from platonism, or else modal nominalists accounts of mathematics reduce commitments and so fail as paraphrases.

The dilemma of the previous paragraph was posed quite some time ago by William Alston. Alston asks the reader to compare pairs of sentences like the following:

1. There is a possibility that James will come.
2. The statement that James will come is not certainly false.

On the surface, (1) appears to be committed to the existence of things called 'possibilities,' whereas (2) appears to have no such commitment. However, both assertions appear to the untutored eye to be identical in meaning, and those who are loathe to admit possibilities into their ontology are likely to regard (2) as an acceptable and ontologically innocuous rendering of what one says when uttering (1). In response to this Alston remarks,

Now it is puzzling to me that anyone should claim that these translations "show that we need not assert the existence of" possibilities... "in communicating what we wanted to communicate." For if the translation of (1) into (2), for example, is adequate, then they are normally used to make the same assertion. In uttering (2) we would be making the same assertion as we would make if we uttered (1), i.e., the assertion that there is a possibility (committing ourselves to the existence of a possibility) just as much by using (2) as by using (1). If, on the other hand, the translation is not adequate, it has not been shown that we can, by uttering (2), communicate what we wanted to communicate when we uttered (1). Hence the point of the translation cannot be put in terms of some assertion or commitment from which it saves us. (1958, 10)¹⁶

The ontological reductionist faces a dilemma: Either accept that the translations preserve the statement being expressed, and so preserve ontological commitments, or admit that the translations eschew ontological commitments but subsequently fail to preserve the statement expressed. What is it that determines whether a sentence is ontologically committed to possibilities?

¹⁶I have made inessential changes in numbering.

... whether a man admits (asserts) the existence of possibilities depends on what statement he makes, not on what sentence he uses to make that statement. One admits that possibilities exist whenever he assertorically utters (1), *or any other sentence which means the same*... It is a question of *what* he says, not of *how* he says it. (ibid., 13)

Thus, Alston locates ontological commitment in the propositional contents expressed by sentences. Since (1) and (2) intuitively both express the same proposition, they have identical commitments. Alston goes on to suggest that the value of such translations consists in their psychological effects:

It is the seductive grammatical family likeness of sentences like (1) which render them objectionable, not any assertion of the existence of possibilities they carry with them, in any intelligible sense of that term. (ibid., 16)

Compare (1) with the following:

3. There is a beer that Larry will drink tonight.

Both (1) and (3) share the grammatical structure 'There is a/an x that F s.' It is perfectly acceptable to ask, about the beer mentioned in (3), where it is located in space and time, whether it is an ale or a lager, when it was brewed and bottled, etc. Experience with (3) suggests that sentences with a similar structure will also admit as intelligible similar kinds of questions about the objects denoted by terms that occupy the same position in the sentences. Upon returning to (1) it is found to be rather nonsensical to ask, about the possibility that James will come, where this possibility is located in space and time, whether it is an ale or a lager, etc. Such questions lack clear answers and lead to skepticism about the existence of possibilities. Alston claims that the translation from (1) to (2) frees one from having to regard such questions as well-formed, and so alleviates much of the mystery surrounding entities like possibilities.

I am not inclined to comment on Alston's hypothesis about the substratal value of translation, however I do not see how entities like possibilities are any less mysterious just because certain awkward questions about them are evaded—if anything, the fact

that ordinary kinds of questions need not be asked about them make possibilities *more* mysterious. In any case, Alston's discussion is predicated on a skepticism about reductive paraphrase. A similar position is advanced by Frank Jackson:

In short, the crucial question is not what one assents to in the object language, but what one assents to in the metalanguage which explicitly states the semantical roles of the terms in the object language. (1980, 310)

For Jackson, a metalanguage is ontologically committed to Ks, "when it entails 'that there are things which "is K" applies to' " (ibid., 311). Although the details are distinct, I take Jackson to be making essentially the same point as Alston, for I understand the function of the metalanguage, on Jackson's account, to be exposing "what it is we are trying to communicate" in uttering a sentence in the object language. Cindy Stern also makes essentially the same point:

... answers to ontological questions cannot be found by looking at representations of statements in a formal language we take to be logically perspicuous. ... what is needed is to answer the question of what the truth conditions are for the relevant statements, regardless of how we represent them. (1989, 42)¹⁷

The success of these general remarks as applied to the case of modal nominalism depends on the nature of the relationship between ordinary mathematical assertions and their modal nominalist counterparts. The proposed case against modal nominalism envisions modal nominalist accounts of mathematics as *synonymous* with or *analytically equivalent* to ordinary mathematical assertions.

2.4.1 Nominalist Paraphrase as Synonymy

If modal nominalist accounts of mathematics are taken to be synonymous with ordinary mathematical assertions, then Alston's result applies directly: Assertions in modal nominalist theories mean the same things as ordinary mathematical assertions, and so modal

¹⁷It should be noted that Stern does not follow Alston in completely abandoning the idea that paraphrase is incapable of paring down ontological commitments; her concern is more to show that providing paraphrases cannot ever constitute a *positive* reason for eschewing commitments. According to Stern, paraphrase is a defensive measure to be supplemented by ulterior reasons for being suspicious about the entities in question (ibid, 36).

nominalist theories do not eschew commitment to mathematical objects. Arguments that fit this mold have been made by John Burgess, Bob Hale, and Gideon Rosen.

Burgess begins by assuming that the acceptance of a statement such as,

4. There are numbers that are greater than $10^{10^{10}}$ and are prime.

clearly entails accepting statements such as,

5. There are (such things as) numbers (Burgess 2001, 429).

However, one cannot accept (5) without being ontologically committed to numbers. According to Burgess, nominalists seek to avoid the commitments implicit in (5) by adopting a paraphrase of (4). He offers a possible nominalist paraphrase of (4):

6. There could be constructed numerals that were greater than $10^{10^{10}}$ and were prime.

which entails only that,

7. There could be constructed (such things as) numerals (ibid., 431).

Against nominalism, Burgess argues that,

If the argument of... nominalists is supposed to be that they are warranted (despite their professed nominalism) in asserting (4) because its paraphrase (6) is assertable, parity of reasoning would suggest that one is warranted also in asserting (5), since its paraphrase (7) is assertable (ibid., 437).¹⁸

Burgess targets this argument at a kind of view he elsewhere describes as hermeneutic nominalism.¹⁹ The hermeneutic nominalist alleges that her account of mathematics provides an attractive interpretation of ordinary mathematical languages; she claims to be giving an account of what mathematical assertions *really* mean. In this sense the nominalist is providing an account of why it is acceptable to assert statements such as (4) and (5), because all these statements really mean can be captured by asserting statements like (6)

¹⁸I have made inessential changes in numbering.

¹⁹See chapter four for more details on Burgess's arguments against nominalism.

and (7). Burgess, like Alston before him, maintains that the tables can be turned and that the paraphrases (6) and (7) make only an illusory gesture at ontological reduction.

Bob Hale presses a very similar point while discussing Hellman's method for providing a modal-structural interpretation of mathematical theories. Recall that Hellman posits two kinds of modal claims: First are the necessary conditionals of the form $AX \rightarrow p$ where p is a consequence of axioms AX . And second, to avoid vacuity, are claims stating the possible existence of models of the axioms AX . These latter claims are known as "Modal Existence" claims. Hale writes,

It is a necessary condition for its adequacy that the scheme takes ordinary arithmetic truths into truths and arithmetic falsehoods into falsehoods. Since this condition is certainly not met unless the Modal Existence claim is true, reason to think the scheme adequate must include reason to think that claim true. (1996, 133)

Hale's official target in writing this is Hellman's justification for supposing that the modal existence claims are true. Extending Hale's remarks somewhat, any thought that the modal existence claims are true, via the translation scheme, is just as much a thought that the relevant mathematical assertions are true. Otherwise, the scheme might take arithmetic falsehoods into truths, or falsehoods into arithmetic truths, etc. Any reason to be confident that the modal existence claims are true requires believing that the translation scheme is adequate, which entails believing that certain mathematical claims are true.

The argument I extract from Hale can be dealt with rather quickly. What is preserved in the translation scheme between ordinary mathematical assertions and modal-structural assertions is not truth *simpliciter* but *theoremhood*. Reason for thinking the scheme adequate need not involve assuming that the relevant mathematical assertions are true, but instead that they are truly theorems of the mathematical theory in question. The theory itself need not be regarded as true.

A similar response is available for the modal nominalist concerned by Burgess's arguments. Let me first say that I am unaware of any nominalist (modal or otherwise)

who endorses statements such as (6) and (7). Moreover, (6) seems to embody a category mistake—numerals are not the kinds of things that can have properties like being prime, or being greater than $10^{10^{10}}$.²⁰ Nevertheless modal nominalist theories are bound to include pairs of assertions, (6^i) and (7^j) , that occupy the same mathematical roles as (4) and (5) do in ordinary mathematics. The question raised here is *just what* the modal nominalist paraphrases are lauded as preserving. If modal nominalist paraphrases are understood as synonymous with ordinary mathematical assertions, then they ought to preserve *meaning* in both directions. The problem with understanding modal nominalist paraphrases in this way is that no modal nominalist that I am aware of has ever described their account of mathematics as a meaning-preserving paraphrase of mathematical language. Nor am I aware of any modal nominalist who maintains that her account of mathematics is designed to uncover the true meaning of mathematical assertions. Rather, the modal nominalist views I defend in this dissertation are interested in answering modal questions about whether it is possible to carry out various kinds of scientific and mathematical reasoning without quantifying over mathematical objects. And even if modal nominalists *were* in the business of attempting to tell the world what mathematics is really about, there is no reason to suppose that they would accept the inference from (4) to (5). If all (4) really means is what is said in (6^i) , then (7^j) does not imply the truth of (5) in any ontologically significant way.

Gideon Rosen presents a similar case against nominalism. Using a thought experiment dating back to Frege, Rosen asks the reader to compare two rather simple geometric languages, L and L^* . The language L contains vocabulary for attributing properties to lines and also includes the two-place relation $x||y$ that is to read ' x is parallel to y .' Intuitively, to say that line a is parallel to line b is to say that a and b have the same *direction*. However, L does not contain any terms that refer to any such things as *directions*. L^* is an extension of L which is to contain a new predicate $d(x)$ that is to read 'the direction of x .'

²⁰Thanks to Susan Vineberg for this observation.

which is a referring phrase that not only refers to a line x but also to an object that is the *direction* of the line x . It seems obviously true that the following two statements mean the same thing:

8. $a \parallel b$,

9. $d(a) = d(b)$,

and, that subsequently, the move from L to L^* does not importantly distort assertions about lines. However, if Frege's analysis of language is granted, then a statement like (9) is committed to the existence of directions, because 'the direction of...' functions in the statement as a singular term. So (9) is only meaningful and true if there exist things like directions. But (8) and (9) mean the same thing; they are analytically equivalent. According to Rosen,

The fact that such a move was always possible shows that a commitment to abstract objects was in a sense already implicit in the original language. True, L contains no devices for referring *explicitly* to abstract objects. Still, what we've just seen is that the obvious truths of L express propositions that are true only if abstract objects exist, in the sense that they are analytically equivalent to statements which refer explicitly to *DIRECTIONS*. (1993, 167)

Thus, the nominalist who asserts both (8) and the analytical equivalence of (8) and (9) and who, "denies the existence of *DIRECTIONS* implicitly contradicts himself whenever he so much as asserts that two lines are parallel" (ibid).

If pressed, I would reject the notion that 'the direction of...' functions as a genuine referring term, but that is a discussion for another occasion. My response to Rosen is similar to my response to Burgess and Hale: The kinds of modal nominalist views I defend in this dissertation are not in the business of providing analytically equivalent expressions of platonistically construed mathematical propositions. Neither Field nor Chihara nor Hellman claim that their accounts of pure mathematics involve assertions that are identical in meaning to mathematical assertions interpreted platonistically. Again, the purpose of modal nominalist reinterpretation and reconstruction is primarily to show that

it is possible to account for the obtaining of mathematical truths and the applicability of mathematics to science²¹ without invoking platonistic ideas about mathematical truth and scientific applications. What is under dispute is the *presumption* often made by platonists that mathematics comes with a platonistic interpretation *directly from the mathematicians*. Rosen's Fregean case against nominalism is guilty of making this presumption, and therefore does not address the views about which I am concerned.

2.5 Reply to the Paraphrase Response

My reply to the paraphrase response, then, is rather curt: It is an uncharitable distortion to view modal nominalist accounts of mathematics as mere paraphrases of ordinary mathematical assertions; paraphrase is not the vehicle through which modal nominalists eschew commitment to mathematical objects. Thus the arguments of the previous section do not touch any of the modal nominalist views I defend in this dissertation.

Nevertheless it is not clear to me that the arguments of the previous section are ultimately effective against a paraphrase-style nominalism (modal or otherwise). It seems to me that these arguments turn on two important presuppositions: that mathematical assertions are true *simpliciter* and that these assertions are ontologically committing. But surely a paraphrase-style nominalist would be interested in challenging both presuppositions. If, in opposition to the view that mathematical assertions are true *simpliciter*, it is more appropriate to view mathematical assertions as, e.g., truly theorems or consequences of the axioms of mathematical theories, then nominalist paraphrase can proceed by preserving theoremhood (as opposed to truth *simpliciter*). And if one does not presuppose that the face-value or literalist interpretation of mathematical language is ontologically committing, then it cannot be complained that nominalist paraphrases do not eschew commitment to mathematical objects, since this commitment is not there to begin with.²² I happen to

²¹In Field's case only the second of these activities is attempted.

²²Of course, a nominalist who did not already believe that mathematical language was not ontologically committing would probably not see the need to produce nominalist paraphrases (except perhaps to convince her platonist foes that such commitment is not in fact present).

believe that both presuppositions should be challenged. I argue elsewhere in the dissertation²³ that, on naturalist grounds, it is no presupposition of *mathematics* that mathematical languages are ontologically committing. In this section I would like to offer some broadly naturalistic reasons against the presupposition that mathematical theories and assertions should be regarded as true *simpliciter*.²⁴ The upshot is that, from a naturalistic perspective, neither presupposition is legitimate, and hence that paraphrase-style nominalism constitutes a coherent position from which to eschew commitment to mathematical objects.

As I understand matters, the arbiters in the nominalism/platonism dispute are the implications of *professional* mathematical discourse, and not the implications of ordinary mathematical assertions as understood by lay individuals. The simple fact is that the mathematician's understanding of basic arithmetical assertions is not the same as the layperson's understanding of mathematical assertions. There is no compelling reason to believe that both kinds of assertions should be treated as semantically on a par. For the mathematician, " $2+2=4$ " is a rather uninteresting theorem of arithmetic. It is not the kind of assertion made in a vacuum. For the layperson, " $2+2=4$ " is a rather simple assertion often made in isolation. The same goes for epistemology. How the lay person comes to acquire justified beliefs about mathematics is altogether different from how the mathematician does this. And there is no guarantee that they arrive at what are recognizably the same beliefs. For the mathematician comes to believe that " $2+2=4$ is a theorem of arithmetic," whereas the layperson comes to believe just that " $2+2=4$." Even supposing that it is acceptable to assume that lay persons actually know that statements like " $2+2=4$ " are true in a mathematically perspicuous way, there is a problem of accounting for what justifies these beliefs. If " $2+2=4$ " makes a true statement about abstract mathematical objects, then any account of how one comes to believe that " $2+2=4$ " must also specify how it is one comes to know that abstract objects exist—there are no epistemological free lunches. I take it that "intuitive perception of a realm of abstract objects" is an unacceptable response here,

²³Chapter four, §5.

²⁴Cf. my discussion of Penelope Maddy's related position, Arealism, in chapter five, §4.

but most especially if offered on behalf of lay persons. The problem is, how humans actually form beliefs about statements like “ $2+2=4$ ” seems to have nothing whatsoever to do with abstract objects. Why should belief in “ $2+2=4$ ” compel anyone to believe that there exist abstract objects?

Frege’s response (and the response of those who follow him) is that “ $2+2=4$ ” is an obvious truth, indeed even a logical truth, and that the platonist’s semantics provides the best account of what the statement means. But what explains the obviousness of “ $2+2=4$ ”? The best explanation, I submit, must not posit any mysterious powers either on the part of mathematicians or on the part of laypersons (and so again, intuitive perception of a realm of abstract objects is out of the question). What evidence do lay people have for believing that $2+2=4$? I suppose their evidence is primarily authoritative, as is much of their evidence concerning the truths of mathematics. Where their evidence is not authoritative it is experiential. Whenever they have collected two objects with another two objects they have always (or for the most part) found there to be four objects all together. It is *prima facie* implausible that either source of warrant justifies the layperson in believing that abstract mathematical objects exist.

What evidence do the “authorities” have? Likely the fact that it is a theorem of arithmetic that “ $2+2=4$.” But now the question becomes, is arithmetic a theory whose truth can be known through something resembling *a priori* insight alone? The answer is a firm *no*. Mathematical theories are constructed with some intuitions in mind—the numbers ought to have properties x, y, z , and so the axioms are designed to accommodate this. But there is no guarantee that these axioms are free from contradiction, as evidenced by the early stages of set theory. And often enough, intuition is too poor a guide for pronouncing on the nature and properties of the mathematical objects in question—again, the development of set theory is evidence of this—for what, if anything, does the notion of ‘set’ amount to? Just what extension of ZFC falls under the concept ‘set’?

Any argument for the existence of mathematical objects that proceeds from citing

supposed obvious, apparent, or commonsense truths of mathematics fails to place mathematical assertions in their proper context. Mathematically, assertions such as “ $2+2=4$ ” are *consequences* of mathematical *theories*. And no amount of reflection on the obviousness of truths such as “ $2+2=4$ ” will ever deductively entail the truth of any interesting mathematical theory. One reason for this is that many of the obvious or commonsense truths of arithmetic are also logical truths of first-order logic with identity—detracting from the idea that Fregean *semantics* provides the best explanation for the indubitability of the basic truths of arithmetic.²⁵ Nevertheless I do not deny that some notion of self-evidence plays a role in the formulation of mathematical theories (and hence, in the generation of mathematical knowledge). If a theory is finitely axiomatizable then it is perhaps possible that one could regard each of its axioms as self-evident. However, most first-order mathematical theories include axiom schemas and so are not finitely axiomatizable from an epistemic vantage point—for instance, the first-order Peano axioms include an induction schema.²⁶ What is the evidence showing that humans are justified in believing that axiom schema are true *simpliciter*? Is it possible to avoid appealing to mysterious “mathematical intuitions” (the likes of which Kurt Gödel championed) in justifying beliefs in axiom schema? Can it even be guaranteed that axiom schema are ultimately justified on *a priori* grounds rather than on pragmatic grounds?

I am suggesting that self-evidence has its limits, especially in mathematics. For instance, Penelope Maddy presents a case study of the Axiom of Choice.²⁷ Her findings indicate that Choice became accepted, not because mathematicians found it to be self-evident, but instead because of the fruitful consequences of adopting the axiom. If that is correct, then mathematical theories need not, and in many cases, are not guided by self-evidence

²⁵For a more detailed argument to this effect, see (Leng 2010, 90-4). Frege, of course, thought arithmetic was reducible to self-evident logical principles, but he was infamously wrong about that—Frege’s system, as is well known, is inconsistent.

²⁶Second-order PA *is* finitely axiomatizable, but its induction axiom includes universal quantification over properties. I understand there to be negligible epistemic gap between confidence in the first-order induction schema and confidence in the second-order induction axiom.

²⁷See (Maddy 1997) and (Maddy 2011).

alone, but also by considerations of mathematical fruitfulness. But then mathematical theories cannot be justified wholly (or perhaps at all) by self-evident principles—some sort of “mathematical experience” is inevitably necessary.²⁸

I say these things to suggest that mathematical assertions—even the existential ones—must be understood *in their proper context*. And the proper context for making mathematical assertions is within the grips of mathematical *theories*. If it is not granted that mathematical theories are true *simpliciter*, then paraphrases can accomplish their goals while only preserving theoremhood. The platonist needs to provide persuasive reasons either for regarding mathematical theories as true *simpliciter*, or for regarding assertions like “ $2+2=4$ ” as statements that are about mathematical objects, but that nevertheless are made true in isolation from their status as theorems of mathematical theories. A paraphrase-style nominalist should express interest only in the ontological commitments of mathematical *theories*, and not the ontological commitments of miscellaneous mathematical-looking assertions. Thus the best scenario for the platonist involves showing that nominalist paraphrases are paraphrases of mathematical theories that are themselves true *simpliciter*. But then a question is raised about the source of warrant for regarding such theories as true *simpliciter*. What I have called into question is the idea that platonists are entitled to be confident that mathematical theories are true *simpliciter* simply because some mathematical assertions appear to be obvious or self-evident. I admit that this looks like unhelpful burden-shifting, and it very well may be.²⁹ Nevertheless there appears to be space for a successful program of nominalist paraphrase, if it can be shown that the platonist is not straightaway entitled to herald mathematical theories as true *simpliciter* (and if, as I argue in chapter

²⁸For more discussion, see (Shapiro 2009). Shapiro argues that even Frege appealed to considerations of what Maddy would call “mathematical fruitfulness” in justifying his Basic Law V.

²⁹Should one invoke Quine’s notion that ontological commitment be tied to the “best overall theory,” then simplicity considerations can be mustered in support of nominalist theories, provided that one distinguishes between two senses of “best overall theory”: (1) The best overall theory as the “true” theory, and (2) the best overall theory as the theory that is most propitious in practice. As far as I can tell, the nominalist (including the paraphrase-nominalist) claims only that her theories are better in the first sense, and that the fitness of theories in the second sense is irrelevant when it comes to matters of ontology. My sense is that the arguments I have entertained against the paraphrase-nominalist are not Quinean in spirit.

four, that platonists are not entitled to the presupposition that mathematical language is ontologically committing). Still, it is worth reminding the reader that neither Chihara nor Hellman nor Field are interested in providing mere paraphrases of ordinary mathematical assertions, and it is for this reason that the Paraphrase Response ultimately fails as a way of bolstering Shapiro's objections to modal nominalism. Of course, Shapiro himself is likely to bolster his objections in a way that relies on his structuralist understanding of mathematics, and it is to Shapiro's structuralism that I now turn.

2.6 The Structuralist Response

Mathematical structuralists are inspired by the slogan that "mathematics is the science of structure up to isomorphism" or some other similar rallying call. Structuralists find significance in the fact that, as far as mathematics goes, mathematicians are more interested in constructing and examining various kinds of relationships between mathematical objects than in studying mathematical objects themselves. That is to say that mathematicians are most interested in the *structural* properties described by mathematical theories. This view raises a number of questions that include, but are certainly not limited to, the following: What is a mathematical structure? Is it a kind of mathematical object? And if so, what is a structure like? Do there exist any interesting relationships that hold *between* structures, say, between the natural number structure and the real number structure? Do structures exist over and above ordinary mathematical objects like numbers? Or does a structure depend for its existence on a collection of objects that stand in the appropriate kinds of relations so as to constitute a structure of that kind? Why suppose that structures exist in the first place?

I cannot address all of these questions in detail here; mathematical structuralism is much discussed in the literature. In particular, the question regarding whether there exist any interesting relationships *between* mathematical structures has led to a great deal of discussion. For instance, it is rather intuitive to suppose that the natural number 2 is

identical to the real number 2. However, 2 has properties *qua* natural number that it lacks *qua* real number, and vice versa. If the only mathematical significance of the number 2 lies in the relations in which it stands to other members of the natural/real number structures, then by the non-identity of discernibles, the 2 of the naturals is *not* the same as the 2 of the reals. Structuralists usually respond that in mathematics, ontology is always relative to structure, and that identity conditions for mathematical objects cannot be given in generality. The debate rages on.³⁰ For better or worse I am not much interested in this debate. My interests instead lie in coming to understand just what sorts of things mathematical structures are and whether there exists an account of structure that is capable of validating Shapiro's criticism that modal nominalists are committed to mathematical structures.

The two major structuralist views to be examined are Shapiro's *ante rem* structuralism and Michael Resnik's structuralism. Each hopes to shed light on the epistemology of mathematics by constructing a philosophy of mathematics that makes important use of the human ability to recognize patterns. Since pattern recognition is allegedly a naturalistically kosher faculty, if mathematical knowledge is ultimately knowledge of patterns, then perhaps a structuralist version of platonism can solve the epistemological difficulties facing the traditional, "objects" platonist views. I begin with Resnik's account of structures before moving on to Shapiro's. The reader is again reminded that my primary interest is in plumbing these views for a conception of structure (and the relationship between structure and ontology) that validates Shapiro's objections to modal nominalism; what follows is not offered as a comprehensive discussion of mathematical structuralism.

2.6.1 Resnik and Patterns

According to Resnik, humans possess the capacity to recognize all kinds of patterns in the natural world. It is an uncontroversial datum that humans form more or less justified beliefs on the basis of this capacity. It is through pattern recognition that I judge my

³⁰For discussion, see (Hellman 2005) and (MacBride 2005), and the references therein.

television screen to be similarly proportioned to the screen of my smart phone, that I recognize the fourth word of this clause to be identical to the fourteenth, etc. Resnik proposes to include mathematics in the canon of what can be known through the faculty of pattern recognition. For, according to Resnik, mathematical theories exhibit patterns—mathematical patterns—and these “are known in the same way as, say, linguistic or musical patterns. . . by putting mathematics in the same epistemological context as music or language we remove some of the mystery enveloping platonism” (1975, 34).

But what is a pattern? Resnik says that a pattern³¹ consists of one or more objects—the *positions* of the patterns—that stand in certain relationships (1997, 202-3). And, as far as a pattern is concerned, each position is distinguished only through the relationships it bears to the other positions of the pattern. A favorite example of Resnik’s is a triangle ABC . Relative to this triangle one can identify the points that serve as the positions for its vertices, but A , B , and C , when considered independently of any reference to the triangle, “are indistinguishable from each other and any other points” (ibid., 203).

Another example is the natural numbers under the successor relation (\mathbb{N}, S) . The positions of this pattern are the numbers, and the numbers are distinguished only through the relationships they bear to one another. For instance, all that it is to be the number zero is to be the position that is not in the range of the successor relation. All that it is to be the number one is to be the successor of zero. All that it is to be the number two is to be the successor of one, etc. Provided that there exist enough of them, any objects whatsoever can fill the positions of the natural number structure. Other mathematical patterns include the real numbers under the less-than relation $(\mathbb{R}, <)$ and the cumulative hierarchy under epsilon (V, ϵ) . For Resnik, \mathbb{N} , \mathbb{R} , and V are not antecedently given items of mathematical ontology. Natural numbers, real numbers, and sets do not have identity criteria on their own; their identification is always *relative* to fixing on a particular pattern (Resnik 1981, 545).

³¹Resnik prefers to use the term ‘pattern’ where Shapiro would use the term ‘structure’; nevertheless Resnik uses these terms interchangeably, and I follow his lead.

Although Resnik demurs from constructing a formal theory of patterns,³² he does articulate several relations that hold between them.³³ The most fundamental relationship between patterns is *occurrence*. Occurrence is a transitive and reflexive relation that holds between two patterns; P is said to *occur* within Q when there is a third pattern R such that P is isomorphic to R , any position of R is a position of Q , and all relations of R are definable in Q . For example, the natural numbers under successor (N, S) occur within the natural numbers under less-than $(N, <)$ because the successor relation is definable relative to the less-than relation.³⁴ A special case of occurrence is the *sub-pattern* relation. P is a sub-pattern of Q if and only if P *occurs* within Q and every position of P is a position of Q . Finally, two patterns are said to be *equivalent* or “essentially the same” under the following conditions:

... let us call a pattern P a truncation of a pattern Q if every position and relation of P is also one of Q . Then a pattern P will be said to be equivalent to a pattern Q just in case there is a pattern R which is a sub-pattern of both P and Q and of which both P and Q are respective truncations. (Resnik 1997, 209)

(N, S) and $(N, <)$ are both truncations of $(N, S, <)$, and the positions of $(N, S, <)$ are the positions of (N, S) and $(N, <)$. Hence (N, S) and $(N, <)$ are equivalent or “essentially the same” patterns.

It turns out that Resnik has quite a bit to say about when a particular pattern exists and about when one is justified in believing that a theory of a pattern is true. In an early paper, he writes:

³²The reason for this is that Resnik is not officially a realist about patterns. According to him, mathematical theories do not quantify over patterns, and so accepting the existence of mathematical patterns is a distortion of practice (Resnik 1997, 211). However, it is quite common for Resnik to speak as though he is a realist about patterns, and in this section I will treat him as such. Inevitably this discussion will involve some distortion of Resnik’s actual position, but I hope the reader will agree that the perturbation is not specious—especially given the passages to come.

³³What follows is drawn from (ibid., 205-9).

³⁴In English, x is the successor of y means that y is less than x and there is no z such that y is less than z and z is less than x . Now the ordinary notion of definability is bi-directional. However, the less-than relation cannot be defined relative to the successor relation in first-order languages. Thus Resnik must appeal to set-theoretic or second-order definability to get the result that $<$ and S are definable relative to one another. See (ibid., 207).

When we “construct” a pattern we will formulate some tentative beliefs about it. Hence our conception of the pattern and our beliefs about it can be subjected to a deductive study during which we attempt to formulate our conception as precisely as we can and examine its logical consequences. This investigation will indicate the degree to which our conception is internally coherent and how well it accords with established mathematics. If it turns out to be highly coherent and confirmed by our knowledge of the finite patterns from which it arose, then our belief in the existence of the pattern is justified. (Resnik 1975, 36-7)

Similar sentiments are expressed in a later articulation of his position:

We go through a series of stages during which we conceptualize our experience in successively more abstract turns. At the last stage we leave experience far enough behind that our theories are best construed as theories of abstract entities. (Resnik 1982, 99)

... a pure theory of a pattern is justified to the degree to which we have evidence for its consistency and categoricity, with the former having a higher priority. (ibid., 101)

For example, early on in their lives humans form the ability to mentally group or collect together objects. At some point in time they form the ability to enumerate the members of these groups, if there are few enough objects in them. Thus they become familiar with the early stages of the progression of the cardinal numbers: one, two, three, ... This is still a concrete activity. Shortly enough, however, comes the recognition that this sequence of counting numbers can be represented in written language as numerals, and that these numerals can be used to count or list arbitrary sets of objects. Thus, a process of abstraction begins. Some may take an interest in the progression itself and begin a more studious investigation. These individuals soon realize that the progression has no natural ending place, but that due to physical limitations, they could never envision the progression to be “completed.” Still, there is an interest in answering the question, “How long does this progression go on?” Answering this question requires another leap of abstraction, from numerals to numbers. Soon enough the Dedekind/Peano axioms are proposed as capturing the important properties of the natural numbers. It is at this point that the

“deductive study” of the pattern of natural numbers begins. No contradiction is found in the derivations of the Dedekind/Peano axioms, and the various arithmetical consequences prove to be highly useful for ordinary and scientific purposes. It follows that a belief in the existence of the natural number pattern (N, S) is justified. Nevertheless,

... all this depends upon our having unconditional knowledge of some initial structures, and with respect to those structures our evidence must be non-deductive and indirect. . . I conjecture that at least our early evidence in favor of a pure theory of an “initial pattern” is furnished by the degree of coherence between the theory and our beliefs concerning the experience from which it has been abstracted. (ibid., 101-2)

In this regard, Resnik notes that simpler theories—theories of patterns with fewer positions and/or less complicated relations (whatever the word ‘complicated’ means here)—are more likely to be true (ibid., 102).

Resnik is asking the reader to believe something quite astounding—one is asked to believe that a pattern *exists* because its postulation is *coherent*.³⁵ Surely that is an oversight; possibility does not imply actuality. But as astounding as this sounds, it is not an oversight. According to Resnik, “in mathematics for a structure to be possible *is* for it to be actual” (1985a, 176). He maintains that, “the sort of reasons I advanced for holding that a given mathematical structure is possible are also (or almost) reasons for holding that it is actual” (ibid). What is his evidence for these remarkable claims?

But suppose that we grant that there are abstract structures. Then to justify the existence of a particular abstract structure it suffices to exhibit a template for things which instantiate that pattern. Thus to show that a specific pattern for houses exists, it suffices to exhibit a blueprint for houses of that pattern. . .

These considerations show that once we countenance structures, giving a consistent description or representation of a particular structure is all we need do to show that such a structure exists. When it comes to structures, the distinction between the possible and the actual lapses. (ibid., 178)

It is not immediately clear how these remarks are supposed to resolve any of the mys-

³⁵A similar belief is shared by Mark Balaguer in defense of his “plenitudinous platonism” (see the first chapter for a brief overview). And, as will be seen below, Shapiro ends up in a similar position in defense of his “Coherence axiom.”

tery surrounding the claim that for mathematics, possibility implies actuality. Perhaps Resnik is conveying an analog of existence claims in set theory. One way of showing that a mathematical object is coherent is to show that it can be constructed in set theory (or in a consistent extension of set theory). If sets are antecedently postulated items of mathematical ontology—that is, if the full set-theoretic hierarchy exists—then anything “constructible” out of sets already exists. On this interpretation, Resnik’s claim amounts to the hypothesis that if structures are antecedently given objects, then any patterns abstracted from them “already exist” in some sense. But just as it is reasonable to ask, “Why suppose that sets exist in the first place?” so too it is reasonable to ask, “Why suppose that patterns or structures exist in the first place?” Resnik’s answer is that, “recognizing them, at the very least, greatly facilitates our theorizing and is in all probability indispensable to it” (ibid). He later elaborates, explaining that the, “decision to allow [patterns] to serve as a part of the evidential basis for further mathematical developments would have to be made from the more global perspective of the benefits to mathematics and science generally” (Resnik 1997, 238).

Can Resnik’s account of structure existence help Shapiro? Shapiro’s strategy is to concoct a variety of structure-equivalence between set theory and modal nominalist theories, and argue that this equivalence precludes modal nominalists from eschewing commitment to mathematical objects and prevents them from providing a more tractable epistemology for mathematics. Resnik provides a relation of pattern-equivalence that itself is structurally similar to Shapiro’s conception of definitional equivalence. Resnik’s idea is that two patterns are equivalent when both are truncations of some larger pattern. What matters here are the positions and relations of the patterns in question. A case can indeed be made that the modal nominalist theories in question are pattern-equivalent to some platonist theory. Chihara’s Constructibility Theory is pattern-equivalent to simple type theory: Chihara’s positions are filled by open-sentence tokens, whereas the positions of type-theory are propositional functions. Chihara uses the relation of satisfaction, while

type theory uses the relation of predication. Otherwise the structural developments of the theories are basically the same. Hellman's modal-structural interpretation of set theory is pattern-equivalent to ordinary set theory, for Hellman posits the possible existence of a model of the set-theoretic axioms. Thus Hellman's positions are just whatever objects exist in the possible-model, and his relations are those that hold in the possible-model; the set-theoretic structure remains much the same (this being Hellman's goal, after all). Field, it is remembered, interprets mathematical assertions literally but regards them as false. However, his account of how mathematical *knowledge* is acquired appeals to modal constructions that very closely resemble Hellman's modal structural interpretations. This element of Field's position involves asserting the possibility of the conjunction of the axioms of a mathematical theory while at the same time sanctioning certain modal inferences on the bases of such possibility claims. These modal assertions invoke much of the structure of ordinary mathematical theories.

Resnik does not claim that structurally equivalent theories share an ontology, at least at the objects-level. If the ontological component of Shapiro's objection to modal nominalism is that modal nominalists are unable to avoid commitment to individual mathematical objects, then availing himself of Resnik's account of structure would not be to his benefit. However, Shapiro never asserts that modal nominalists are committed to the existence of individual mathematical objects; he does nonetheless claim that modal nominalists are committed to the existence of structures.³⁶ Resnik (*qua* realist about structures) agrees with Shapiro that the modal nominalist is committed to structures, although for slightly different reasons. Shapiro's guiding principle is that definitionally equivalent theories share an ontology. This is bolstered by the accusation that the respective epistemological problems facing modal nominalism and platonism are commensurate. Resnik is sympathetic only

³⁶At least, he never *directly* asserts that nominalists are committed to individual mathematical objects. He does suggest this *indirectly*, however. According to Shapiro's conception of structure (discussed below), mathematical structures are free-standing objects and are mathematical correlates of *ante rem* universals. The positions of these structures are full-fledged objects. Thus, any theory that is committed to mathematical structures is subsequently committed to the existence of the objects which instantiate them.

with the epistemological tactic. In his review of Hellman's *Mathematics Without Numbers*, he writes that,

... in most non-mathematical cases it is easier to show that something is possible (or consistent) than to show that it is actual (or true)... Let us take Hellman's simplest case, the possibility of an infinite progression. Given our experience with set theory and mathematical logic during this century, we can be confident that we cannot deduce this possibility from our knowledge of the finite... once we set aside our mathematical knowledge, I do not see how we could count this belief as justified. (Resnik 1992, 118)

What is this mathematical knowledge that he speaks of?

... how do we know that infinite progressions are logically possible? Because no contradiction follows from the the supposition that they exist, I presume. And how do we know this? Well, we can rest with logical intuitions and our deductive experience or we can turn to mathematical models. Historically, we have taken the latter course and have appealed to mathematical objects to clarify intuitive notions of possibility. (ibid., 118)

... once we have gone so far as to postulate the possibility of such extraordinary objects, I would think that it would be simpler to take them as actual and enjoy the benefits of standard applied mathematics. (ibid., 119)

Here Resnik proffers remarks very similar to those voiced by Shapiro concerning the role model theory has played in the advancement of knowledge about logical modality. As I will argue after having described Shapiro's structuralism, I do not see how this kind of reply can possibly represent a convincing case for the existence of either structures or more ordinary kinds of mathematical objects. No one is denying that model theory has allowed for the fruitful development of modal logic. But model theory alone is incapable of making any categorical pronouncements about what is possible. The model-theoretic approach to modal logic is an incredibly useful and rigorous tool for analyzing modal *inferences*, but those inferences have to start from somewhere. In model theory, the starting point is usually set theory; sets are simply assumed to exist. To instantiate Resnik's account of structure into Shapiro's criticism of nominalism would be asking the reader to believe that modal nominalism is defeated because knowledge of the possibility (and hence actuality)

of a structure is only justified assuming a starting point in model theory. But that would just be to assume that sets exist in the first place, clearly begging the question against the modal nominalist. Why should this be lauded an epistemologically superior starting point when compared with the initial assumption that it is *possible* for sets to exist?

Ultimately, Resnik's justification for the existence of sets, models, and patterns is pragmatic. That, for patterns, possible existence implies actual existence, is admissible only *after* it has been conceded that there exist certain initial patterns by way of model theory and set theory. But the modal nominalist will object at precisely this point, and claim that the mere possibility of these kinds of objects is sufficient for capturing mathematics. So Resnik's account of structure and his criticisms of modal nominalism do not, in the end, diminish Shapiro's burden.³⁷ Next I examine Shapiro's own structuralist theory. The upshot is rather anticlimactic, given what has just been said in response to Resnik; Shapiro winds up deferring to set theory in the same way and for the same reasons as Resnik, and consequently he makes no compelling case against the modal nominalist.

2.6.2 Shapiro and Structure

Shapiro does not share Resnik's resistance to providing a formal theory of structure. Resnik resists doing this because he does not believe that mathematical theories literally quantify over structures. On this point, Shapiro and Resnik are in clear disagreement:

My outlook towards this [structure] language (and theory) may be called *working realism*. Classical logic, impredicative definition, the axiom of choice, and extensionality are freely employed. There is no apology for this language, and no major regimentation is envisioned. That is, structures are not thought of as reinterpreted versions of anything else. As I see it, structures are part of the ship of Neurath, and the structuralist language is useful for describing other parts of the vessel [sic]—including the semantics of mathematical theories. (Shapiro 1989b, 162)

Moreover, Shapiro believes that there are strong ties between the language, logic, and ontology of mathematical theories:

³⁷Resnik even admits that it is possible to be, "a structuralist without being a realist about mathematical objects" (1997, 270).

... my view is that the sorts of 'objects' studied by a branch of mathematics—the sorts of structures—are determined by the allowed constructions and sanctioned inferences. In short, the logic and the objects share a common source—the moves available to the ideal constructor. (1989a, 32)

Shapiro forges these connections in conjunction with his belief that second-order logic underlies the practice of mathematics.³⁸ Although in first-order logic there are no categorical theories with infinite models (due to the Löwenheim-Skolem Theorem), nevertheless there are such theories in second-order logic. The categoricity of second-order theories comports well with the structuralist manifesto that mathematicians are only concerned with structure up to isomorphism. For better or worse I am not much interested in examining whether Shapiro is correct in holding that second-order logic is the underlying logic of mathematics.³⁹ As in Resnik's case, I am mostly interested in determining whether Shapiro's conception of structure is at all capable of validating his criticisms of modal nominalism.

Shapiro begins by introducing the idea of a *system*. A *system* is a collection of objects with certain relations (Shapiro 1997, 73). Shapiro's favorite example of a system is a baseball defense: a collection of nine baseball players in some particular spatial arrangement at a ballpark. Other examples include extended families, arrangements of chess pieces, and symphonies. Structures are defined in relation to systems: A *structure* is the *abstract form of a system* (ibid., 74). This definition alone is not entirely helpful. For what, if anything, is the *abstract form* of a system? According to Shapiro, these are to be understood as objects akin to *ante rem* universals; he goes as far as to label his view "*ante rem* structuralism." Thus, for Shapiro, *ante rem* structures are freestanding objects—objects that exist independently of their exemplifications. Moreover, he also views the places or positions of structures as bona fide objects (ibid., 83).

For two systems to exemplify the same structure it is sufficient, but not necessary, that the systems be isomorphic (ibid., 91). Shapiro also admits Resnik's criterion of structure-

³⁸A view he defends in detail in (Shapiro 1991).

³⁹For discussion, see (Shapiro 2005) and (Jané 2005).

equivalence as capturing a more coarse-grained notion of when two systems instantiate “essentially the same” structure. And of course, Shapiro’s own notion of definitional equivalence provides an account of sameness of structure for systems. However, Shapiro diverges sharply from Resnik in constructing an axiomatic theory of structure. To do this Shapiro develops structuralist correlates of the set-theoretic axioms:

Infinity: There is at least one structure that has an infinite number of places.

Subtraction: If S is a structure and R is a relation of S , then there is a structure S' isomorphic to the system that consists of the places, functions, and relations of S except R . If S is a structure and f is a function of S , then there is a structure S'' isomorphic to the system consisting of the places, functions, and relations of S except f .

Subclass: If S is a structure and c is a subclass of the places of S , then there is a structure isomorphic to the system that consists of c but with no relations and functions.

Addition: If S is a structure and R is any relation on the places of S , then there is a structure S' isomorphic to the system that consists of the places, functions, and relations of S together with R . If S is a structure and f is any function from the places of S to places of S , then there is a structure S'' isomorphic to the system that consists of the places, functions, and relations of S together with f .

Powerstructure: Let S be a structure and s its collections of places. Then there is a structure T and a binary relation R such that for each subset $s' \subseteq s$ there is a place x of T such that $\forall z(z \in s' \equiv Rxz)$.

Replacement: Let S be a structure and f a function such that for each place of x of S , fx is a place of a structure, which we may call S_x . Then there is a structure T that is (at least) the size of the union of the places in the structures S_x . That is, there is a function g such that for every place z in each S_x there is a place y in T such that $gy = z$.

Coherence: If Φ is a coherent formula in a second-order language, then there is a structure that satisfies Φ .

Reflection: [For any first- or second-order Φ in the language of structure theory:] If Φ , then there is a structure S that satisfies the (other) axioms of structure theory and Φ . (ibid., 93-5)

The most curious of these axioms is almost certainly the Coherence axiom (also known as the “coherence principle”). According to the Coherence axiom, if a formula in a second-order language is coherent, then there exists a structure that satisfies it. But why should

anyone suppose that the Coherence axiom is true—that the coherence of a formula suffices for the existence of a structure that satisfies it? And what does it mean in the first place to say that a formula is coherent?

Doubts about the truth of the Coherence axiom are likely to stem from adherence to the almost universally held belief that possibility does not imply actuality, even for structures. One possible baseball defense involves each outfielder crowding the left-field foul line and each baseman laying prostrate at a distance behind their respective bases determined in inches by the closest number of the Fibonacci sequence to the number on their jersey, with the shortstop running figure-eights in uneven paces in shallow center. A colleague that will go unnamed assures me that this is a legal defense in Major League Baseball, but to both his knowledge and mine, no manager has ever adopted it. So the abstract form of this possible system has no actual representatives. If possibility implies actuality for structures, and the system in question is possible, then the system must have an actually existing abstract form.⁴⁰ And for Shapiro, the positions of structures are *bona fide* objects. So if the structure in question exists, then so do its positions. But by Shapiro's own admission, a baseball defense is a type of structure that can be realized only by having *baseball players* in its positions. Without players there can be no defense. Is there no avoiding the awkward conclusion that the mere *possibility* of such a baseball defense necessitates the existence of a group of players that have at least once implemented it?

A very similar situation arose above in conjunction with Shapiro's conception of definitional equivalence outlined earlier in the chapter. Shapiro claims that any two definitionally equivalent theories have identical ontological commitments. In response, Chihara gestures at the possibility of constructing two theories—one about cats, the other about dogs—that meet Shapiro's conditions for definitional equivalence. The awkward conclusion in that case was that certain cats are identical to certain dogs. But that criticism was not terribly bothersome, for Shapiro does not claim that definitionally equivalent theories have

⁴⁰Thanks to Larry Powers for pointing out an error in an earlier articulation of this point.

identical ontological commitments *in general* but only in the case of mathematical theories. Moreover, Shapiro is concerned mostly with the ontological commitments a theory has due to the *structure* that it invokes, as opposed to the ontological commitments that a theory has in virtue of the ranges of its first-order quantifiers. My ultimate suggestion was that Chihara's response was unsuccessful barring a more detailed investigation of Shapiro's views on ontology. I remind the reader of this earlier component of the dialectic because I believe a similar strategy might allow Shapiro to avoid the awkward conclusion of the previous paragraph. *Pace* Resnik, Shapiro can grant that no one should believe that, *in general*, the possibility of a structure is evidence of its actual existence. Nevertheless there may be reason to believe that as far as *mathematical* structures are concerned, there is no appreciable gap between the possible and the actual. But this maneuver requires for its success two further steps. First, it must be shown how it is possible to determine, about a particular structure, whether it is of the mathematical or non-mathematical variety. Second, reason must be given for supposing that *even in* the case of mathematical structures, possible existence suffices for actual existence.

Shapiro believes that he has hit upon a relevant distinction between mathematical and non-mathematical structures:

In mathematical structures, on the other hand, the relations are all *formal*, or *structural*. The only requirements on the successor relation, for example, are that it be a one-to-one function, that the item in the zero place not be in its range, and that the induction principle hold. No spatiotemporal, mental, personal, or spiritual properties of any exemplification of the successor function are relevant to its being the successor function. (*ibid.*, 98)

A system does not count as a baseball defense unless it meets the non-formal requirement that its positions be filled by entities capable of playing baseball. Meanwhile, provided that there are a denumerable infinity of them, any old objects can count as a system the abstract form of which is the natural number structure. The relations of a baseball defense are restricted in that they can only be filled by a certain class of spatiotemporal objects. No such restriction at all is made by the successor relation. This qualifies the natural number

structure as *formal*. *Formality*, then, is the criterion by which a structure's actual existence can be inferred from its possible existence. But through which means can it be determined whether a relation is *formal*?

If each relation of a structure can be completely defined using only logical terminology and the other objects and relations of the system, then they are all formal in the requisite sense. (ibid.)

And what does Shapiro mean by 'logical terminology'?

... the present proposal is that a relation is *formal* if it can be completely defined in a higher-order language, using only terminology that denotes Tarski-logical notions and the other objects and relations of the system, with the other objects and relations completely defined at the same time. All relations in a mathematical structure are formal in this sense. (ibid., 99)

One hesitates to rest so much on a choice of the logical vocabulary. I happen to have the intuition that logic is lordly and so is not in the business of making existential claims. But if Shapiro is to be trusted, mathematical ontology is relative to the selection of logical vocabulary, because logic decides what is formal, and what is formal decides what exists in mathematics. For better or worse, Shapiro does not appear share my views on the lordliness of logic. This is a very high-level disagreement—Shapiro's structuralism is wedded to a picture of mathematics according to which language, logic, and ontology are deeply intertwined—a picture that I am not at all interested in defending. So if pressed, here is where I would dig my trenches. Unfortunately, there is not room for this battle in this dissertation, so I must pass over matters in relative silence. Thus, for the sake of argument, I am willing to grant Shapiro his connections between language, logic, and ontology. As I shall argue in the sections to come, even with this concession Shapiro is not able to make a convincing case against modal nominalism.

That mathematical structures are characterized exclusively by *formal* relations helps to distinguish them from non-mathematical structures. So Shapiro's quest is not yet exposed as unsatisfiable. Nevertheless he still faces the task of explaining why, even in the case

of mathematical structures, possible existence suffices for actual existence. That is, he must still justify the truth of the Coherence axiom. And, of course, he still faces the task of explaining what it means in the first place to say that a structure is possible or coherent.

Evidence for the coherence of a theory of a structure can come in a variety of forms: Simple abstraction and pattern recognition, linguistic abstraction, and implicit definition. Shapiro claims that small finite structures can be apprehended directly via pattern recognition or simple abstraction. For instance, the various manifestations of the letter 'E' (e , e , \mathcal{E} , \mathcal{E}) can all be recognized as such despite the fact that, "there is nothing like a common shape to focus on" (ibid., 114). What these various 'E's do have in common, according to Shapiro, is position in an alphabet structure. Thus learning the function of the letter 'E' involves learning about a particular kind of structure. However, pattern recognition and abstraction are only capable of illuminating small, finite structures. For large finite structures, and for structures up to the size of the continuum, one must enlist the aid of linguistic abstraction. With linguistic abstraction, one can describe large finite patterns. For example, linguistic tokens can evince structural relations of very large numbers that could not be recognized via pattern recognition. Using numerals it is a trivial task to distinguish the 999,999 cardinal structure from the 1,000,000 cardinal structure. However, few, if any humans, could quickly distinguish a circular array of 999,999 dots from a circular array of 1,000,000 dots. To extend matters to the infinite, it can be observed that there is no natural limit to the size of a cardinal (or ordinal) structure. Linguistic abstraction permits the coherent description of denumerably infinite structures, and structures constructible from denumerably infinite structures (e.g., the rationals, Cauchy sequences of rationals, etc.). A key point underlying this discussion is the idea that simple abstraction, pattern recognition, and linguistic abstraction all provide the resources for giving *coherent descriptions* of structures. That structures exist is a further claim that is inferred only after the Coherence axiom is assumed (ibid., 118; 120).

Finally, a set of axioms can be thought of as implicitly defining a structure. The axioms can then be subjected to deductive study. One is justified in believing that a theory of a structure is coherent if and only if, after a rigorous examination, no contradictions are derived from its axioms. Implicit definition is really the most important notion here: Many interesting mathematical structures, especially in set theory, involve large infinite cardinalities, which implies that pattern recognition and linguistic abstraction will never be capable of justifying beliefs in the coherence of many mathematically interesting structures. But what does the coherence of an implicit definition come to? Shapiro admits that coherence is not to be understood as deductive consistency; *ante rem* structuralism is a creature of second-order logic, and there is no completeness proof in second-order logic ($PA + \neg\text{Con}(PA)$ is deductively consistent, but lacking models, it is arguably *not* coherent). Shapiro would prefer coherence to be thought of an analogue of satisfiability, but the notion of satisfiability involves a circularity; satisfiability is a model-theoretic notion:

Normally, to say that a sentence Φ is satisfiable is to say that *there exists* a model of Φ . The locution “exists” here is understood as “is a member of the set-theoretic hierarchy,” which is just another structure. What makes us think that set theory itself is coherent/satisfiable? (ibid., 135)

Shapiro’s ultimate move is to concede that coherence is an undefinable primitive:

... there is no getting around this situation. We cannot ground mathematics in any domain or theory that is more secure than mathematics itself. All attempts to do so have failed, and once again, foundationalism is dead. The circle that we are stuck with, involving second-order logic and implicit definition, is not vicious and we can live with it. I take “coherence” to be a primitive, intuitive notion, not reduced to something formal, and so I do not venture a rigorous definition. (ibid.)

Shapiro, like Resnik, requires noninferential belief in the coherence of some mathematical structures. For Shapiro, that structure is set theory:

In mathematics as practiced, set theory (or something equivalent) is taken to be the ultimate court of appeal for existence questions. Doubts over whether a certain type of mathematical object exists are resolved by showing that objects of this type can be found or modeled in the set-theoretic hierarchy. (ibid., 136)

Once set theory is in place—that is, once sets are assumed to exist—coherence can qualify as a criterion for the existence of structures that are constructible out of sets. But whether the coherence of set theory?

Surely, however, we cannot justify the coherence of set theory itself by modeling in the set-theoretic hierarchy. Rather, the coherence of set theory is *presupposed* by much of the foundational activity in contemporary mathematics. Rightly or wrongly (rightly), the thesis that satisfiability is sufficient for existence underlies the background for model theory and mathematical logic generally. Structuralists accept this presupposition and make use of it like everyone else, and we are in no better (and no worse) of a position to justify it. The presupposition is not vicious, even if it lacks external justification. (ibid.)

The *coherence* of set theory, then, is a basic *presupposition* of mathematics. Even a modal nominalist can agree with that. But coherence qualifies as a criterion of existence only *after* it is assumed that sets exist. No reason is given to suppose that, for set theory itself, coherence implies existence. If Shapiro thinks otherwise he has conflated two importantly distinct things: The status of the Coherence axiom on the supposition that sets exist; and whether the coherence of set theory implies the existence of sets. As I argue below, it is not clear that Shapiro is entitled to use the Coherence axiom to establish the existence of *ante rem* structures, even presuming that sets do indeed exist. Moreover to make the leap and claim that sets do indeed exist is to go well beyond what is literally “presupposed” in mathematics—which makes Shapiro’s platonism about structures rather difficult to square with things he says earlier:

... at no time did the mathematical community don philosophical hats and decide that mathematical objects—numbers, for example—really do exist... (ibid., 25)

One cannot “read off” the correct way to do mathematics from the true ontology, nor can one “read off” the true ontology from mathematics as practiced. The same goes for semantics, epistemology, and even methodology. (ibid., 34)

What, then, is his source of warrant for supposing that sets do indeed exist? What is the justification for his platonism?

I present an account of the existence of structures, according to which an ability to coherently discuss a structure is evidence that the structure exists... the argument for realism is an inference to the best explanation. (ibid., 118)

The reader is asked to believe that sets and structures exist because doing so best explains various aspects of mathematics and its practice. What makes the actual existence of sets and structures the best explanation here? That story is captured by his criticism of modal nominalism: Shapiro sees the epistemological difficulties stemming from the postulation of abstract mathematical objects as the principle obstacle to his structuralist platonism. He thinks that these difficulties translate into epistemological difficulties for modal nominalists stemming from their appeal to assertions about what is primitively possible. And, of course, he thinks that the structural similarities between set theory and the various modal nominalist accounts of mathematics exemplify the same structures. And since structure is all that matters as far as mathematical ontology is concerned, modal nominalists are just as ontologically burdened as structuralists. In the end, he claims that there is simply not much to choose from:

The fact that any of a number of background theories will do is a reason to adopt the program of *ante rem* structuralism. *Ante rem* structuralism is more perspicuous in that the background is, in a sense, minimal. On this option, we need not assume any more about the background ontology of mathematics than is required by structuralism itself. (ibid., 96)

Left unexamined throughout this discussion thus far is what justification Shapiro has for holding that, as far as mathematics goes, structure is all that matters. Shapiro's primary contention against the modal nominalist comes with his proposal that

... definitional equivalence serve[s] as a criterion of the formal strength of modal and nonmodal theories and... that this notion be used as an indication that the intended structures, and thus the ontology/ideology of different theories, are the same. If T is definitionally equivalent to T' , then neither is to be preferred to the other on ontological/ideological grounds. (ibid., 242)

On the same page he refers to (Wilson 1981) as furnishing a justification for the idea that definitional equivalence has these curious powers. It should be worthwhile to assess

Wilson's evidence on matters.

Wilson claims to be able to show that,

... the ontology of a mathematician is to be determined by the *formal* properties of the structure he postulates, not by the names he employs in the description of it. The claim is that any two theories meeting the conditions of several paragraphs back⁴¹ share an ontology, I shall call *structuralism*, since this thesis represents a plausible reading of the jingle "Mathematics is only interested in structure up to isomorphism" (ibid., 414).

As an example, he considers two versions of set theory: One of these is ZF, containing only pure sets. The other is ZP, which is identical to ZF except that it contains natural numbers as urelements. Wilson imagines presenting both theories to a group of mathematicians and asking them if they believe that ZF is ontologically impoverished when compared to ZP. He hypothesizes that,

Many (or most) mathematicians would probably demur, arguing that the ontology of ZF actually does include the numbers, etc., because it includes an ω -sequence and methods for building the needed sets from it. (ibid., 413-4)

This is offered as convincing evidence that,

...if one accepts a theory of a certain *formal strength*, one cannot deny it its standard ontology, no matter in what syntactic guise its assertions may appear. (ibid., 419)

Wilson draws the conclusion that ZF and ZP have identical ontological commitments because he thinks that mathematicians would argue that both include the numbers. But what designation is to be attached to the phrase 'the numbers'? If "the numbers" are supposed to be the traditional platonist's mathematical objects, then according to Wilson, the structuralist position on arithmetic comes to the surprisingly non-structuralist slogan that: "Any ω -sequence contains the numbers as objects." But this appears to me to be a

⁴¹"... we shall only be concerned with interdefinable theories T, T' which claim to represent the *total* ontology for mathematics and whose intended structures $\mathcal{S}(T)$ and $\mathcal{S}(T')$ are such that if a structure S is built from $\mathcal{S}(T)$ based upon the definitions T provides for the terms of T' , S will be isomorphic to $\mathcal{S}(T')$ " (ibid., 413).

misleading interpretation of what mathematicians would mean in saying that ZF and ZP both contain the numbers. An interpretation that is more in the spirit of structuralism holds that the differences between ZF and ZP are rather uninteresting, not for any ontological reasons, but rather because as far as mathematics goes, either version of set theory is adequate for capturing number theory. But the attendant slogan here is not that “any ω -sequence contains the numbers,” but instead the ontologically innocuous claim that “any ω -sequence satisfies the properties that are important to number theorists.”

Wilson’s evidence is altogether weak; that formally similar theories have identical ontological commitments depends upon a controversial understanding of what mathematicians *might* say in the context of a thought experiment. I conclude that Shapiro’s deference to Wilson is misplaced. Wilson’s remarks on structure and ontology lend very little credence to Shapiro’s claim that definitionally equivalent theories share an ontology. Frankly, it is not clear to me what justification there is for propounding structuralism as a view that says that, as far as mathematical *objects* go, structure is all that matters. I do not object to structuralism as a view that highlights the kinds of *relationships* that mathematicians find important and interesting; in that regard I think the view is superior to many other philosophical accounts of mathematics. However, Shapiro is happy to admit that mathematicians have never decided that mathematical objects exist. In light of that I do not see how he finds it admissible to claim that as far as mathematical *objects* go, there is much of *anything* that matters! As a view about the *ontology* of mathematics, structuralism receives very little—if any—support from mathematical practice.

2.7 Reply to the Structuralist Response

The Structuralist Response holds that modal nominalists are committed to the existence of mathematical objects—*viz.*, structures—because modal nominalist accounts of mathematics implicitly characterize coherent mathematical structures. The following argument captures the basics of this response:

10. Modal nominalists assume that theories of mathematics are primitively possible (or coherent).
11. Shapiro's Coherence axiom; any coherent formula in a mathematical language is satisfied by some actually existing structure.
12. By (10) and (11), formulae of modal nominalist theories of mathematics are satisfied by actually existing structures.
13. By (12), commitment to modal nominalist theories of mathematics necessitates commitment to actually existing structures.
14. By (13), modal nominalist theories do not succeed in eschewing commitment to mathematical objects.

The Structuralist Response also has the capacity to bolster Shapiro's claim that modal nominalism and platonism are on an equal epistemological footing:

15. The modal nominalists defended here maintain that an epistemologically defensible account of mathematics must assume no more than the primitive possibility, or coherence, of mathematical theories.
16. By (11) and (15), any evidence the modal nominalist provides for the coherence of mathematical theories can be appropriated by the platonist as evidence for the existence of mathematical structures.
17. Since actuality implies possibility, any evidence the platonist has for positing the existence of mathematical structures can be appropriated by the modal nominalist as evidence for the coherence of the appropriate mathematical theories.
18. By (16) and (17), modal nominalism and platonism are on an equal epistemic footing.

Finally, the Structuralist Response can make sense of Shapiro's accusation that modal nominalism is inferior *a propos* of justifying modal assertions because modal nominalists accept *primitive* modal notions.

19. The modal nominalists defended here rely on *primitive* modality.
20. Platonists can reduce modal concepts set-theoretically.
21. By (19) and (20), platonists are better equipped than modal nominalists for justifying modal assertions.

If sound, these arguments are incredibly damaging to the modal nominalist approach. It is fairly clear that in the first two arguments, premise (11)—Shapiro's Coherence axiom—does all of the important work—and the bulk of the remainder of this chapter is dedicated to demonstrating that (11) should be rejected. I shall eventually argue that the third argument is invalid—that platonism can provide a reductive account of modality does not show that platonism provides a means for justifying modal assertions. But this response involves prospecting more broadly issues related to reductionism about modality, and so I shall delay developing it in much detail until the next chapter (although I shall outline my thinking on matters at the end of this chapter).

2.7.1 Withering Coherence

Note that Shapiro accepts coherence as a *primitive* notion. Modal nominalists, meanwhile, require primitive modality. Thus the battle over primitives is between coherence and primitive modality. But these two notions are essentially no different from one another. Would it make sense to call a theory coherent, but not possible (at least using whichever sense of the word 'possible' that nominalists have in mind)? Or vice versa? Here, anyway, I am thinking of the theories that characterize the structures of accepted mathematics, of which modal nominalists and platonist both seek accounts. If that is right, then as far as primitives go, there is not a whole lot of difference between Shapiro's account of mathematics and the various modal nominalist theories I defend in this dissertation.

The main difference between Shapiro and modal nominalists is Shapiro's adherence to his Coherence axiom; that any coherent second-order formula is satisfied by some actually existing structure. Hellman approaches this axiom with a degree of skepticism:

Why should coherence suffice for existence of mathematical structures whereas in virtually any other domain of inquiry, coherence does not suffice for existence? (2001b, 196)

Shapiro's unsatisfying response is that the Coherence axiom holds for structures the relations of which are exclusively *formal*. According to Harold Hodes, this maneuver raises some important unanswered questions:

Is formality an absolute property of relations (as the surrounding material suggests), or is it relative to systems? And if the latter, to which systems? Or is there only one relevant system? and if so what is it? Perhaps that structure itself? What resources are allowed for this definition in a higher-order language? Without answers to these questions, Shapiro's explanation of formality remains unilluminating. (2002, 470)

But even if Shapiro can be allotted an intuitively palatable conception of formality, the general worry remains. Criticism comes from a number of sources. Hellman elsewhere writes,

[The Coherence axiom] can be thought of as a post-Gödelian substitute for formal consistency: the axiom mimics Hilbert's idea that consistency suffices for mathematical existence. But of course it is not a formal notion and seems no clearer than a primitive notion of (second-order) logical possibility—indeed, perhaps less so, for do we have anything as developed as modal logic governing “coherent?” And if we identify these notions, then the Coherence Axiom appears even more problematic, for why should mere logical possibility suffice for existence? Indeed, why not just rest with the former...? (2005, 546)

Izabela Bondecka-Krzykowska and Roman Murawski ask,

Can one claim that a structure defined by an implicit definition, hence by a set of axioms, does exist by appealing to the consistency of the axioms and to the completeness theorem...? No “normal” mathematician is doing this... If such methods were rejected so where from should we know then that structures defined by implicit definitions do exist? (2006, 36-7)

And Fraser MacBride suggests that,

... even if coherent categorical descriptions are guaranteed to be non-empty, there still remains an epistemological issue about how it can be established that descriptions are coherent and categorical. For if we cannot know *which* descriptions of structures are coherent and categorical, then there is little epistemic comfort to be had from the reflection that such descriptions—whichever, without our knowing, they may be—are inevitably satisfied. (2008, 162)

As seen in the last section, Shapiro has a response to these concerns. He claims that existence questions in mathematics can be deferred to set theory. If set theory is itself coherent and if sets do indeed exist, much of the mystery surrounding the Coherence axiom is removed. MacBride finds a certain irony in Shapiro's deference to set theory:

It is also noteworthy that the ontology of *ante rem* structuralism performs no substantial role in this, the most fundamental component of Shapiro's epistemology for mathematics. For when there is a need to establish whether a description is coherent and categorical, structures drop out of sight, and it is questions of set existence that come to the fore. (ibid., 163)

Ironical or not, is Shapiro's flight to set theory an acceptable move? In a recent paper, Shapiro says that he is, "content to have my account, as a whole, judged alongside other philosophies of mathematics on the overall score of what does best in accounting for mathematics, and its role in our intellectual and personal lives, using whatever resources are available for this endeavor" (2011, 138). This comes as a result of his acknowledgment that he

... cannot deduce the coherence principle from non-mathematical premises which any opponent will accept. Nor do I accept the burden of justifying the coherence principle on such grounds. The coherence principle is part of an overall philosophy of mathematics which, I claim, well explains the enterprise. (ibid., 147)

This holistic inference to the best explanation is worth unpacking.

Shapiro's evidence for the Coherence axiom is alleged to come from the foundational activity within mathematics itself. It is worth repeating a passage already quoted from before:

In mathematics as practiced, set theory (or something equivalent) is taken to be the ultimate court of appeal for existence questions. Doubts over whether a certain type of mathematical object exists are resolved by showing that objects of this type can be found or modeled in the set-theoretic hierarchy. Examples include the “construction” of erstwhile problematic entities, like complex numbers. (Shapiro 1997, 136)

As written, this passage does contain an argument for platonism:

22. Set theory serves as a court of appeal for existence questions.
23. Existence questions about X s are resolved by showing how to construct X s in the set-theoretic hierarchy.
24. Therefore, if X s can be constructed in the set-theoretic hierarchy, then X s exist.

Three points are in order. First, the inference from (22) and (23) to (24) is ampliative. The suggestion appears to be that because set theory has served as the court of appeal for existence questions in recent professional mathematical practice, it will continue to do so for other existence questions. Second, this argument only establishes platonism if it is granted in advance that sets exist. If sets do not exist, then the fact that X s can be modeled in the set-theoretic hierarchy is not evidence that X s exist. Third, and finally, this argument is not by itself sufficient to establish the existence of *ante rem* structures, even if the first two concerns can be addressed. When constructing models in set theory, one is always constructing particular mathematical objects, or particular isomorphism types—one is not directly constructing *ante rem* structures. If sound, this argument establishes a form of *objects-platonism*, and not necessarily a form of a platonism about structures, potentially undermining the ability of the Coherence axiom to expose the would-be structural commitments of modal nominalist theories. Nevertheless, what this suggests is that the principle embodied by (24) is not clearly equivalent to Shapiro’s Coherence axiom (line (11) from the arguments above). Shapiro even appears to recognize that (24) is not equivalent to the Coherence axiom:

To “model” a structure is to find a system that exemplifies it. If a structure is exemplified, then surely the axiomatization is coherent and the structure exists. Set theory is the appropriate court of appeal because it is comprehensive. The set-theoretic hierarchy is so big that just about any structures can be modeled or exemplified there. (ibid.)

Here, ‘model’ is in quotes, suggesting that the “modeling” of a structure is not the same kind of activity as the modeling of complex numbers. This is made clear by his indication that what things are actually constructed in set theory are *systems* or *exemplifications* of structures, and not the structures themselves. Thus the inference that there exist *ante rem* structures is something that is to occur over and above whatever constructions are completed within the set-theoretic hierarchy.

These three points raise some important questions: First—what is the actual role of a principle like (24) in the mathematical community, and does its role lend support to using the Coherence axiom in a critique of modal nominalism, or in an argument for the existence of *ante rem* structures? Second—what is the evidence for the existence of sets? And third—supposing that (24) and the Coherence axiom are indeed *distinct*, what other sort of justification can be provided for Shapiro’s Coherence axiom? I will examine each of these questions in turn.

2.7.1.1 Satisfiability and Mathematics

What is the mathematical role of (24)? And does its role support using the Coherence axiom to criticize modal nominalism and to argue for the existence of *ante rem* structures? Shapiro is on record as claiming that something like (24) is at work in mathematics. Indeed, he claims that it “underlies mathematical practice” (ibid.). Under what conditions is it appropriate to appeal to the Coherence axiom for producing answers to existence questions? For Shapiro, whenever a theory is sufficiently *formal*, the existence of a structure can be inferred from the ability to coherently discuss the theory. Thus the scope of the Coherence axiom is not limited just to the existence questions that have *actually* arisen in mathematics, or even just to those existence questions that would *likely* arise in practice;

it applies generally to sufficiently formal theories. He acknowledges this as a point of contact with Mark Balaguer's full-blooded platonism:

Mathematical objects are tied to structures, and a structure exists if there is a coherent axiomatization of it. A seemingly helpful consequence of this is that if it is possible for a structure to exist, then it does. Once we are satisfied that an implicit definition is coherent, there is no further question concerning whether it characterizes a structure. Thus, structure theory is allied with what Balaguer calls "full-blooded platonism" if we read his "consistency" as "coherence." (ibid., 134)

However, mathematicians are typically not interested in *just any* consistent extension of set theory, nor are they interested in just any consistent theory of a sufficiently formal stripe. If any sort of coherence-implies-existence principle is used in mathematics, it is restricted just to theories that serve a recognized goal in mathematics. David Corfield makes a related observation *a propos* of set theory:

While it is impressive enough to be able to represent more or less any desired construction, it has a problem in that it does not know how to say 'No'. It cannot distinguish between those constructions that the mathematically literate will realise are patently pointless and those that stand at least some chance of gainful employment. (2003, 239)⁴²

I take it that the best possible evidence for using the Coherence axiom to establish the existence of *ante rem* structures (and to criticize modal nominalism) would involve showing that the existence of *ante rem* structures matters to mathematicians in some important way, or that mathematicians themselves use the Coherence axiom to establish the existence of *ante rem* structures. But mathematicians appear unconcerned with establishing the existence of *ante rem* structures; mathematicians have not packed *ante rem* structures in *their* luggage for their voyage on Neurath's ship. I take this as evidence that Shapiro's use of the Coherence axiom is not straightforwardly analogous to the relatively uncontroversial uses of (24) (for establishing, e.g., that complex numbers exist). That is, the practice of mathematics lends no *direct* support to the use of the Coherence axiom for establishing the

⁴²See also (Maddy 1997, 202).

existence of *ante rem* structures. What sort of indirect support is available to Shapiro will be discussed shortly.

2.7.1.2 The Existence of Sets

What is the evidence for the existence of sets? The argument from (22) and (23) to (24) is most plausible as an argument for a reductionist variety of objects-platonism, according to which mathematical objects can be reduced to sets. But, as I have argued previously, it is only convincing evidence for platonism if it is granted in advance that sets exist. Although Shapiro does not argue directly for the existence of the set-theoretic hierarchy, he does come close to doing so in the following passage:

I take it that I can assume the correctness of standard accepted mathematics along the way, and I can assume that mathematicians know the bulk of standard, accepted mathematics. . . My use of set theory at this juncture is based on the belief that set theory itself is coherent, a belief shared by mathematicians. . . It follows from the coherence of set theory that if we show that any proposed implicit definition *D* is satisfiable, then *D* is itself coherent, and thus *D* describes a structure, or a class of structures. (Shapiro 2011, 149)

Although perhaps not intended as such, this nevertheless is not convincing evidence that sets exist. Shapiro claims that the *coherence* of set theory is a *presupposition* of mathematics. But, by itself, *coherence* is just a primitive notion akin to second-order satisfiability. No claim follows about the *existence* of sets unless it is assumed in addition, and *as a presupposition of mathematics*, that the *Coherence axiom* applies to set theory. But Shapiro does not claim the application of the Coherence axiom to set theory as a presupposition of mathematics (and nor should he, given that mathematicians do not use the principle in the first place)—he claims only that mathematics presupposes the *coherence* of set theory. But if all that is known is that set theory is coherent, then all that strictly follows is that *if* sets exist, then anything coherently describable in set theory *would* exist as well.

If Shapiro's criticism of modal nominalism (along with his case for the existence of *ante rem* structures) depends on this detour through set theory, then *ante rem* structuralism is at risk of begging the question against nominalists, who are willing to grant *at most*

that set theory is coherent. In the present setting, the criticism that modal nominalists are committed to structures only has force if it is granted that either (a) sets exist, or (b) that the Coherence axiom applies to set theory. Option (a) is clearly question-begging. Option (b) is equivalent to option (a), and moreover, is circular in the context of exploring whether anyone is justified in appealing to the Coherence axiom. The question naturally arises as to whether this circularity is vicious. Shapiro claims that it is not:

... *ante rem* structuralism is itself no more secure than is set theory. . . So if I were looking to provide some sort of extra-mathematical justification or security for set theory, making use of set theory would be viciously circular, and structures would have dropped out of the picture. The grounding would take place in set theory. But that is not my game. (ibid.)

The security of set theory, platonism about sets, and the existence of *ante rem* structures all seems to hinge on just what is presupposed in mathematical practice. I reiterate that the mere *coherence* of set theory is not sufficient grounds for extracting the ontological results that must be extracted in order to establish the existence both of sets and of *ante rem* structures. Moreover the mere coherence of set theory is not sufficient to use these (unavailable) results to figure in Shapiro's criticism of modal nominalism, since that criticism requires the Coherence axiom. This would settle matters, save for the fact that Shapiro also musters indirect, holistic support for the Coherence axiom and more generally for his *ante rem* structuralism, and it is to these considerations that I now turn.

2.7.1.3 Holistic Justifications for the Coherence Axiom

So, given that (24) and the Coherence axiom are distinct principles, what sort of justification is left for the Coherence axiom? Reflecting on the burdens he faces in defending *ante rem* structuralism, Shapiro writes that he is, "content to have my account, as a whole, judged alongside other philosophies of mathematics on the overall score of what does best in accounting for mathematics, and its role in our intellectual and personal lives, using whatever resources are available for this endeavor" (ibid., 138), and that, "[t]he coherence principle is part of an overall philosophy of mathematics which, I claim, well explains the

enterprise" (ibid., 147). He further remarks that he is out to, "provide a justification for a philosophical interpretation of mathematics," and that such activity, "is not a deductive enterprise, where I would have to start with non-mathematical, self-evident principles" (ibid., 149). These are points often reiterated in the the fourth chapter of (Shapiro 1997)—that the application of the Coherence axiom to *ante rem* structures is part of a highly plausible account of the overall mathematical enterprise. But what are his reasons for supposing that his *ante rem* structuralism is *more* plausible than its competitors?

Unfortunately, what lies behind these plausibility considerations are nothing other than the criticisms of nominalism described above in §2. One of these criticism is that modal nominalists do not eschew commitment to mathematical objects because they share in the *structural* commitments of mathematical theories. But why should a modal nominalist (or anyone else) feel compelled to recognize and acquiesce to the structural commitments of a mathematical theory? One possible reply says that if modal nominalist theories are to be successful they must, at a minimum, provide *coherent descriptions* of mathematical theories, and thus via the Coherence axiom, they must acknowledge that there exist structures described by these theories. But it has already been established that this use of the Coherence axiom goes beyond the presumably uncontroversial uses of a principle like (24). Whether anyone is justified in so using the Coherence axiom is precisely what is at issue. But without assuming that the Coherence axiom can be used to establish the existence of *ante rem* structures, I have difficulty understanding why someone such as a modal nominalist should be willing to grant that her theories are ontologically committed to *ante rem* structures.

I think this poses a dilemma regarding the potential justifications for using the Coherence axiom in a criticism of modal nominalism. On the one hand, it can be maintained that it is just a fact about definitional equivalence that definitionally equivalent theories have identical ontological commitments, at least when it comes to mathematical theories. On the other hand, it can be maintained that two mathematical theories have identical

ontological commitments when both (a) the theories are definitionally equivalent to one another, and (b) both theories are sufficiently formal so that the Coherence axiom applies to them. The first option is undesirable because in order for it to be the case that definitionally equivalent theories share an ontology, it must be supposed that one means something entirely different than most logicians and mathematicians when one speaks of 'definitional equivalence,' and this undermines the idea that, so justified, one's conception of definitional equivalence is consistent with or supported by the practice of mathematics (where such qualities are presumed desiderata). The second option is circular—but is it viciously circular? Shapiro's desired result is that the Coherence axiom can be used to establish the existence of *ante rem* structures, and so bloat the modal nominalist's universe. This principle is embedded in an overall account of mathematics which is alleged to be more plausible than the alternatives, in large part because the alternatives are ontologically on a par. But that the alternatives are ontologically on a par involves inferring, e.g., that because modal nominalist theories posit coherent descriptions of mathematical theories, modal nominalist theories are committed to the existence of *ante rem* structures. This kind of use of the Coherence axiom is no different in kind from the uses of it that stand in need of justification, so this response, if advocated, would indeed be viciously circular. And it is unclear whether there are any other reasons why it should be supposed that, for instance, modal nominalists must recognize the alleged structural commitments of their views. Absent such reasons, Shapiro is not entitled to claim that alternative views are ontologically on a par with *ante rem* structuralism, vitiating premise (11) in the above arguments.

But suppose Shapiro is right that the primitive notion of coherence by itself plays an important foundational role in mathematics (in the sense that the *coherence* of set theory is a basic presupposition of mathematics). Since structuralists, "accept this presupposition and make use of it like everyone else, and [are] in no better (and no worse) of a position to justify it" (*ibid.*, 136), it would appear to follow that all interested philosophical parties are

privity to the presuppositions of mathematical practice. But I cannot help but recognize how little conceptual distance there is between a primitive notion of coherence and, for example, a primitive notion of logical possibility. It would indeed make little sense to maintain that the *coherence* of set theory is a presupposition of mathematics, but then argue that some additional evidence is required for licensing belief in the *primitive logical possibility* of set theory. Indeed, there seems to be little to no additional conceptual space between the notions of primitive logical possibility and satisfiability than there is between coherence and satisfiability—here it is worth recalling that Shapiro takes structural similarities to be indicators of *ideological* similarities. If that is right, then mathematics itself provides all of the evidence certain modal nominalists require in order to be justified in asserting the primitive logical possibility of mathematical theories. This suggests to me that the scaffolding of *ante rem* structures is an eliminable component of philosophical interpretation of mathematics, since philosophical accounts of mathematics can get well enough along with just a notion like coherence (or primitive logical possibility), without needing something like the Coherence axiom. This means that a view incorporating the Coherence axiom faces a burden not shared by the alternatives—that of justifying the Coherence axiom itself. Although these considerations by no means settle the dispute between modal nominalism and platonism, I do take them to show that there is room for preferring certain modal nominalist views on ontological grounds.⁴³ In any case, the burden of justifying the Coherence axiom is a burden that remains for *ante rem* structuralism irrespective of one’s attitude toward modal nominalism. Let me expand on these remarks in the next section.

2.8 Shapiro’s Challenge: What Exactly is the Problem?

Shapiro levies several accusations against modal nominalist accounts of mathematics. The first of these is that modal nominalist accounts of mathematics raise epistemological questions that are just as serious as those facing platonism. A second is that the modal nominalist languages ultimately do not avoid reference to mathematical objects. The first

⁴³Thanks to Eric Hiddleston for suggesting an important clarification here.

accusation arises because modal nominalist accounts of mathematics invoke primitive modality and require undefended assumptions about what is possible or what it is possible to do. The second accusation follows from Shapiro's structuralist views; modal nominalist theories are definitionally equivalent to set theory, and subsequently share in set theory's ontological commitments. The upshot is a not-so-balanced tradeoff: Platonists are burdened by a vast ontology, whereas modal nominalists are burdened by *both* primitive modality *and* by a vast ontology.

I have argued that the mere fact that two theories are definitionally equivalent lends no support to the claim that such theories must have identical ontological commitments. I then considered two alternative ways of reaching this conclusion. One alternative holds that modal nominalist theories are no more than synonymous paraphrases of mathematical languages. If two sentences are synonymous, then they both mean the same thing, and so cannot differ in their ontological commitments. This alternative makes the mistaken assumption that modal nominalist theories are to be regarded as mere paraphrases of ordinary mathematical languages. A second alternative delves further into the structuralist's conception of mathematical objects. According to Shapiro's conception of structure, one is justified in believing that a structure exists provided that one has good evidence that the structure is coherent. Modal nominalist theories posit what is essentially the logical possibility (or coherence) of mathematical theories, which via Shapiro's Coherence axiom, commits these theories to the existence of mathematical structures. This second alternative ultimately begs the question against modal nominalism, for in order for the coherence of a mathematical structure to imply its existence, it must be assumed that the Coherence axiom applies *to* set theory, which is equivalent to the assumption that sets exist.

At best, the tradeoff between modal nominalism and platonism is the following: Platonists are burdened by a vast ontology, whereas modal nominalists are burdened by primitive modality. And, if the reader is keeping track of primitives, the tradeoff is actually balanced *in favor* of nominalism: Platonists (à la Shapiro) accept both the modal notion of

coherence and the Coherence axiom as basic postulates, whereas modal nominalists only require primitive modal notions. And it is doubtful that there are any important differences (epistemological, ontological) between the logical modalities modal nominalists use and the notion of coherence Shapiro invokes. The upshot is that modal nominalists require only primitive modality, whereas Shapiro requires primitive modality *and* the contentious assumption that for some kinds of objects—mathematical structures—possibility implies actuality. I should think this welcome news for modal nominalists.

A third accusation lurks in the background, and that is that modal nominalists are somehow *particularly* burdened by their invocation of primitive modality. Why? Because once the platonist has available the assumption that sets exist, she can avail herself of the set-theoretic reduction of the modal notions. According to the platonist, one can explain what is logically possible by appealing to one's knowledge about sets. So the problems she faces in coming to terms with how she can have knowledge about the coherence of mathematical structures are just the same problems she faced at the outset—of how she is to come to know that sets exist and to know various things about them. The modal nominalist, meanwhile, has no obvious route for explaining or justifying the modal assertions that figure in her theories. *Shapiro's Challenge to the nominalist is this: For the modal nominalist to show that she can, on nominalistically acceptable grounds, explain how it is possible for her to justify the modal assertions that figure in her theories, and to explain, again on nominalistically acceptable grounds, why she is entitled to apply the results of modal logic when constructing and applying her theories.* As I have explained previously, my focus is on the former component of the challenge. As I argue in the next chapter, this component presents itself to the modal nominalist as *more* than a merely epistemological challenge. Rather, it presents two related tasks: To provide an account of the content of modal claims, and to provide an account of how to justify claims about this content. The ironic result, according to Shapiro, is that platonists are actually in a better position to complete both tasks.

It is unclear whether the platonist's purported ability to generate a reductive account

of modality is really the boon that Shapiro makes it out to be. For what is important in the dispute at hand is not the status of the modal notions *in general*, but rather the status of *particular* claims of possibility (or coherence). For Shapiro to present an effective objection to modal nominalism, he must assume that some reductive account of modality genuinely illuminates the epistemology of modality (this assumption is contested in the next chapter). Suppose it is granted that Lewisian possible worlds provide a reductive base for the metaphysical modalities. Then it is incontestable that p is possible just in case p is true in some possible world. Thus, the question about whether a statement p *really is possible* can be answered by determining whether *there is some possible world in which p is true*. Unfortunately this does nothing to reduce the mystery about how knowledge that $\Diamond p$ is acquired, indeed, it appears to add to it; now one must claim to know about the particular constitution of some possible world. An analogous situation arises in the case of the set-theoretic reduction of the logical modalities. First, it must be granted that sets exist and that it is possible for humans to know various things about them (this, of course, is the selfsame epistemological problem platonism faces at the outset). But now knowledge that $\Diamond p$ is knowledge that there exists a model that satisfies p , i.e., it is knowledge about some particular construction in set theory. If logical possibility is read as consistency, then this knowledge can be acquired by constructing a consistency proof of p . However, not even Shapiro can benefit from such particular claims of possibility. Shapiro requires particular claims of *coherence*. And if the logic in question is second-order (as it must be for Shapiro), consistency is no guarantee of coherence. But serving as the foundation for *consistency* proofs is the only means by which the set-theoretic reduction can assist humans in determining which formulae (and theories) are coherent and which formulae (and theories) are not coherent. The problem is that without a reductive theory of coherence, Shapiro is in no better position than anyone else for justifying *particular* claims of coherence. I suppose one could posit “coherence proofs” as a novel proof-theoretic concept, akin to consistency proofs, but which guarantee the coherence of their conclusions. But that would

presuppose some method of distinguishing coherence proofs from consistency proofs. Since Shapiro does not say what coherence is, this strategy is not likely to benefit him in any way.⁴⁴

Shapiro is also on record as claiming that pre-theoretic intuitions about modality are much too vague to support the particular claims of possibility that modal nominalists make (1993, 475). Against this, Chihara writes (on behalf of Constructibility Theory),

Let us consider the development of finite cardinality theory given [in Constructibility Theory]. Notice that the exposition and discussions of this theory (including all the proofs of theorems) are given without any appeals to any model-theoretic notions or to results from possible worlds semantics. Deductions and inferences are made using modal reasoning and without any mention of set theory or set-theoretical results. Yet, it can be seen that standard theorems of number theory can be obtained within this system. (2004, 205)

Chihara's point is that he never explicitly utilizes any set-theoretic results in developing cardinality theory. It is true that his development *mirrors* previous *platonist* developments, and it is plausible that had there never been any previous developments of cardinality theory that Chihara would never have happened upon his own development of cardinality theory. But to say that this means that Constructibility Theory is deeply enmeshed in set theory—including the purported ontological commitments of set theory—is to commit the genetic fallacy. As Chihara is quick to remind the reader, reasoning in mathematics did not begin with set theory (*ibid.*). In principle, the historical development of mathematics *could* have occurred using a modal framework. Modal locutions would then be the dominant vehicles for making and evaluating mathematical assertions. In this hypothetical situation,

⁴⁴An alternative possibility involves establishing the coherence of mathematical theories using infinitary logics. Infinitary logics allow for infinite conjunctions and disjunctions, as well as (in many cases) quantification over infinitely many variables. Let λ, κ be cardinals, with $\lambda \leq \kappa$. An infinitary base language $L(\kappa, \lambda)$ permits conjunctions and disjunctions of sets of formulae of cardinality ℓ where $\ell < \kappa$, and permits quantification over sets of variables of cardinality j where $j < \lambda$. The "largest" infinitary language that possesses the completeness property is $L(\omega_1, \omega)$. See (Keisler and Knight 2004, 18) and (Bell 2012). $L(\omega_1, \omega)$ can be used, with the completeness property, to demonstrate the coherence of PA. However, Shapiro remarks that $L(\omega_1, \omega)$ cannot even characterize the reals up to isomorphism (1991, 241). Thus, even in infinitary logics, the coherence of most mathematical structures cannot be demonstrated via consistency proofs, thanks to the failure of completeness for languages "larger" than $L(\omega_1, \omega)$. In any case, Shapiro does not appear to be terribly sympathetic to infinitary logics (*ibid.*, 240), and so there is reason to doubt that he would accept them as a means for demonstrating the coherence of mathematical theories.

it might be complained that the objects of platonist set theory—sets—are only clearly understood because of the prior work done in modal mathematics, and that set theorists would not have been able to construct anything resembling a useful mathematical theory without depending on the prior insights of modal mathematics. Shapiro’s anti-nominalist argument fares no better than this anti-platonist argument. I take it that both are equally bad, and ought to be rejected.⁴⁵

These concerns aside, I understand Shapiro to have made an important criticism of modal nominalism. Modal nominalists must say *something* about why they are justified in making modal assertions, and they must say *something* about why this places them in a less onerous position when compared to platonism. To give some account of the flavor of this challenge, consider what Bob Hale has said about the epistemological burdens of Hellman’s Modal Structuralism:

It is easy—but an error—to slide . . . into supposing that acceptability is, quite generally, the default status for possibility claims—that such claims are invariably to be presumed innocent until proven guilty, and stand in no need of defence unless and until a case is made against them. . . it is far from evidently true that there could be a complete ω -sequence of concrete objects: it is accordingly reasonable to require grounds for belief that it is so. (Hale 1996, 132)

Confidence that arithmetic is consistent, however well founded, goes no way at all towards resolving the present issue—for all we so far know or have reason to believe, it may be that, *as a matter of necessity*, arithmetic admits of no *concrete* models. (ibid., 134)

Hale can be understood as demanding of the modal nominalist that she elicit some kind of evidence for her possibility claims—evidence that must avoid implying or assuming the existence of mathematical objects.

How is a modal nominalist to respond to Shapiro’s and Hale’s demands? Mary Leng lights the one way while discussing the epistemology of set theory:

⁴⁵Cf. my discussion of this issue in chapter three, §5.

...if we look for direct reasons for believing in the consistency of set theory, or in the consistency of particular models that set theory provides for other mathematical theories, it is plausible that this problem is somewhat more tractable than the problem of finding reasons for believing that set theory is true. (2007, 105)

According to Leng, such “direct reasons” can arise in two important ways. In the first place, inductive considerations can be mustered in support of the consistency of set theory:

So far we’ve not come across a derivation of a contradiction from the axioms of (for example), ZFC, despite many mathematicians working on set theory (and, indeed, looking for constructions that might produce a contradiction) for many years. (ibid.)

An alternative form of inductive justification comes from applications:

... the best explanation of the applicability of a mathematical theory requires only that that theory is consistent with the non-mathematical facts (and therefore that it is consistent *per se*). But if the best explanation of the successful application of a piece of mathematics requires the mathematical theory that we apply to be consistent, then an application of inference to the best explanation would provide an inductive justification for our belief in the consistency of that theory. (ibid., 106)

Second, arguments from intuition can support possibility claims:

... arguments from intuition are poor arguments for the truth of mathematical theories. However, our ability to conceptualize configurations of objects that satisfy the axioms of set theory or number theory might well provide us with some evidence that those theories are consistent. (ibid.)

Leng reminds the reader that the iterative conception of sets, “appears to us to involve no contradiction,” and claims on behalf of arithmetic that, “our ability to conceive of an ω -sequence seems to provide particularly good reason for believing the consistency of PA” (ibid.). The upshot is that,

At the very least, such considerations suggest that the prospects for defending consistency claims on such grounds are greater than the prospects of defending claims concerning the face-value-truth of the theories we are considering. (ibid., 107)

Presumably all of this is the selfsame evidence that someone such as Shapiro would offer on behalf of the claim that set theory and arithmetic are *true* theories. Does that unveil platonism and modal nominalism as residing in a comparable justificatory position? I should think not. Again, Shapiro forges the link between the consistency of a mathematical theory and the truth of a mathematical theory via his Coherence axiom. But his Coherence axiom is only in a position to secure this result if (a) set theory is coherent, and (b) the Coherence axiom *applies to* set theory. Together (a) and (b) are equivalent to the assumption that set theory is true. But whether set theory is true (or whether it is merely consistent) is what is at issue. Moreover, defending (b) presents a burden for the platonist that is not shared with the modal nominalist.

Nevertheless Shapiro can still claim that platonism fares better at explaining how one justifies assertions about the coherence or consistency of mathematical theories. And his support for such a claim comes to the pronouncement that modal nominalists must accept *primitive* modality, whereas platonists can avail themselves of the set-theoretic reduction of the logical modalities. This strategy raises important questions about what a reduction of modality consists in and whether reductive theories of modality are capable of performing the justificatory work that Shapiro claims that the set-theoretic reduction is capable of performing. In the next chapter I argue that the set-theoretic reduction is *not* capable of assisting the platonist in justifying assertions about what is logically possible. (And nor is a reduction like Lewis's capable of assisting someone in justifying assertions about what is metaphysically possible.) If modal-mathematical assertions are unjustified under modal primitivism, then they become no less unjustified under a platonistic reduction of modality. It follows from this that modal nominalists are not particularly burdened by their use of primitive modality. But then modal nominalists and platonists like Shapiro are in a similar position with respect to justifying assertions about the coherence or consistency of mathematical theories. And so, on Shapiro's picture, modal nominalists are the *least* burdened, because they are not forced to defend dubitable views about the relationship

between the consistency of mathematical theories and the existence of mathematical objects.

Chapter 3

Reducing Modality as a Solution to Shapiro's Challenge

No one shall expel us from the Paradise that Lewis has created.

—Anonymous

I wouldn't dream of trying to drive anyone out of this paradise... I would try to show you that it is not a paradise—so that you'll leave of your own accord.

—Ludwig Wittgenstein

3.1 Introduction

Shapiro's objection to modal nominalism purports to show that modal nominalists both (a) do not succeed in evading the ontological commitments of *ante rem* structuralism, and (b) encounter serious difficulties on account of their use of modality. I have shown in the previous chapter that Shapiro is not successful in demonstrating that modal nominalist accounts of mathematics share in the ontological commitments of *ante rem* structuralism, vitiating (a). The general goal of this chapter is to explore the significance the remaining component (b) of Shapiro's objection to modal nominalism. Precisely what is problematic about the modal nominalist's use of primitive modality? Can these problems—whatever they may be—be avoided by appealing to a reductive account of modality? In particular, does *Shapiro* successfully avoid these problems through his appeal to the set- or model-theoretic reduction of the logical modalities? The main contention of this chapter is that Shapiro is unsuccessful in showing that the modal nominalist, by appealing to primitive modality, faces particularly challenging obstacles concerning the justification of modal assertions.

In section two I provide a recapitulation of Shapiro's Challenge with the aim of precisely eliciting what is troubling about the modal nominalist's use of primitive modality. The

issue for the modal nominalist (*qua* modal primitivist) is that she is incapable of giving *content* to the modal assertions that figure in her theories of mathematics, in the sense that she does not provide an account of the truth-conditions for these assertions. Without such an account, the modal nominalist appears unable to provide any compelling justifications for the modal assertions that figure in her theories. Thus, modal assertions under modal nominalism threaten to be completely unjustified, and perhaps, unjustifiable. Meanwhile, the platonist can appeal to the set-theoretic reduction of the logical modalities in order to provide truth-conditions for modal assertions, including those made by modal nominalists. Thus the platonist can at least provide an account of what makes these assertions true or false, making it possible to identify what challenges her modal epistemology faces (e.g., knowledge about what kinds of sets exist and what can be modeled in set theory).

In section three I set precedent for my response to Shapiro through a discussion of David Lewis's well-studied reduction of modality. Lewis argues that assertions about what is metaphysically necessary and possible can be reduced to assertions about what is true in a vast realm of spatiotemporally disconnected universes—Lewis's possible worlds. Lewis proposes that knowledge about what transpires in merely possible worlds can be justified on the basis of already justified commonsense modal intuitions, together with the further metaphysical hypotheses Lewis makes about the nature of his worlds. What is of significance here is that his analyses of metaphysical possibility and necessity in terms of spatiotemporally disconnected universes (i.e., possible worlds) serve only as a *rewrite rules* for translating back and forth between ordinary modal assertions and assertions about what transpires in possible worlds. The analyses themselves confer no justification upon lone modal assertions or upon lone assertions about what transpires in possible worlds. Thus, the mere fact that Lewis is in possession of a *reductive* account of modality provides no reason for supposing that Lewis, when compared to a primitivist about the metaphysical modalities, is in a preferable position with respect to his ability to justify modal assertions. This conclusion is not in conflict with anything Lewis has said about his

reduction of modality, but it does raise the prospect of applying similar reasoning against Shapiro. Perhaps the analyses of the logical modalities in terms of models and sets does not in fact place set-theoretic reductionists, when compared to modal primitivists, in a preferable position *a propos* of justifying assertions about what is logically possible.

As I explain in section four, the conclusion of section three applies to possible worlds style reductions of modality more generally. Possible worlds style reductions—that is, reductions that take advantage of basic Kripkean ideas about modal semantics—share a common structure: A non-empty universe or domain of φ s is postulated; necessity is analyzed as truth in all φ s, possibility as truth in a φ , etc. What differentiates one such reduction from another is the particular metaphysical resources it substitutes for φ . The analyses themselves are again merely rewrite rules and confer no justification upon lone modal assertions or upon lone assertions about what is the case in or according to the non-empty domain of φ s.

In section five I argue that the general conclusion of section four applies in the particular case of the set-theoretic reduction of the logical modalities. The basic idea of the set-theoretic reduction is that assertions about what is logically possible can be reduced to claims about what can be modeled in set theory (though as I explain in the section, this reduction may only be useful for claims about the logical consistency of mathematical theories and about the possible existence of mathematical objects). I explain that the analyses of the logical modalities in terms of sets, like Lewis's analyses of the metaphysical modalities in terms of worlds, serve only as rewrite rules for translating back and forth between assertions about what is logically necessary or possible and set existence assertions. The analyses themselves perform no work justifying lone modal assertions or lone set existence assertions. Thus, it would be wrong to insist that Shapiro is in a privileged position *a propos* of justifying logical possibility assertions, *solely* in virtue of the fact that he can appeal to a reductive theory of the logical modalities.

Actually, it is possible to show that Shapiro's situation is *at least as* undesirable as the

situation facing the modal nominalist (*qua* modal primitivist). Set existence claims, for Shapiro, ultimately reduce to claims about which sets can be coherently described, and Shapiro's notion of coherence is a primitive notion that is indistinct from a primitive notion of logical possibility. If modal nominalists are unjustified in using modal assertions, then it stands to reason that Shapiro is no less unjustified in using coherence assertions. On the other hand, if Shapiro is justified in using coherence assertions (and he believes that he is in virtue of his contention that the coherence of set theory is a *presupposition* of mathematics), then, given the indistinctness of coherence and logical possibility, there seem to be no obstacles preventing the modal nominalist from using modal assertions.

In section six I consider some reasons for doubting that modal primitivism is nominalistically acceptable. A viable modal primitivism appears committed to the idea that there is *something* to which modal operators or modal properties apply, and it seems unlikely that the primitivist can guarantee that this something is nominalistically acceptable. However, I contend that the modal nominalist is still entitled to the response to Shapiro that I offer in section five—that Shapiro has given no reason for supposing that the problems nominalists face by using primitive modality can be avoided by appealing to the set-theoretic reduction. But what might these problems be? To answer this question it should be helpful to revisit Shapiro's Challenge.

3.2 Reduction and Shapiro's Challenge

Recall from the previous chapter that I left the epistemic component of Shapiro's objection to modal nominalism largely intact. What is this criticism? Loosely, that whenever the platonist faces questions such as "how do we know that p ?" (where p involves reference to mathematical objects), the modal nominalist faces questions such as "how do we know that $\diamond p$?" According to Shapiro, "it is hard to see how adding primitive possibility operators to the formation of epistemic problems can make them any more tractable," which implies that, "it is hard to see how the [nominalist] has made any progress over the [platonist] on

the sticky epistemic problems” (1997, 226). Thus, the modal nominalist appears saddled with seemingly intractable epistemological questions about what is categorically possible (or coherent, or constructible, etc.). Related to this concern is Shapiro’s hypothesis that the modal nominalist’s use of primitive modality is (perhaps unwittingly) *facilitated* by her understanding of set theory and model theory. It is not clear why the modal nominalist is entitled to use familiar systems of modal reasoning in application to her particular modal primitives. Shapiro claims that modal nominalism engenders an “epistemic loss” because, “it is hard to see what grounds our antirealists would use to support the modal assertions, given that they do not believe in models” (ibid., 237). Consider what Shapiro says against Hellman’s modal structuralism:

... because Hellman is out to drop the realist perspective, it is not clear why he is entitled to the traditional, model-theoretic explications of the modal operators of logical necessity and logical possibility. For example, the usual way of establishing that some sentence is possible is to show that it has a model. For Hellman, presumably, a sentence is possible if it might have a model (or if, possibly, it has a model). It is not clear what this move brings us. (ibid., 229)

Shapiro, by contrast, holds that set theory is, “the source of the precision we bring to modal locutions” (ibid., 232). Thus, the platonist does have *grounds* for supporting the modal assertions that she makes, *viz.*, the set- or model-theoretic reduction of the logical modalities. A provisional account of Shapiro’s Challenge, then, is that modal nominalists must somehow demonstrate that they have nominalistically acceptable grounds for the modal reasoning they employ and the modal assertions which figure in their theories. (As I have indicated previously in the dissertation, I take Chihara and Field to have offered a response to the charge that modal nominalists lack nominalistically acceptable grounds for employing modal *reasoning*—my concern here is with the charge that modal nominalists lack nominalistically acceptable grounds for the modal *assertions* which figure in their theories.)

What is not clear is whether this criticism of modal nominalism is accurately described as a purely epistemic criticism. Consider Shapiro’s use of the terms ‘grounds’ and ‘sup-

porting’ in the following sentence, quoted above: “it is hard to see what grounds our antirealists would use to support the modal assertions, given that they do not believe in models” (ibid., 237). It should be noted that Shapiro is not using the term ‘grounds’ in a technical or formal way (*sensu* (Rosen 2010) or (Fine 2012)), but rather as a synonym for ‘resource.’ So the complaint is that it is unclear what *resources* modal nominalists might use for *supporting* the modal assertions they make. But this complaint is still ambiguous between a metaphysical and an epistemic reading.¹ On the epistemic reading, to have grounds or resources for supporting an assertion means something like having a sufficient reason for believing that the assertion is true. On the metaphysical reading, to have grounds or resources for supporting an assertion means something like being able to identify and describe the *content* of the assertion using more basic principles. This ambiguity runs throughout Shapiro’s “epistemic” criticism of modal nominalism. Nevertheless the epistemic and metaphysical readings are related in a *prima facie* way—presumably having the *metaphysical* resources for supporting an assertion *p*—that is, understanding what *p* means, what it is that makes claims like *p* true, etc.—goes a long way towards generating the *epistemic* resources for supporting *p*—that is, having a sufficient reason for believing that *p* (which could come, for instance, from prior confidence that the truth-conditions for *p* obtain).

Below I will often be concerned with the *justification* of modal assertions. Let me stipulate now that my use of ‘justify’ throughout the remainder of this chapter primarily concerns the *epistemic* sense of “grounds” just described. That is, I shall principally be concerned with the question of whether, e.g., modal nominalists have sufficient reasons for believing that the modal assertions they use are true—any unmodified instance of ‘justify’ (or any instances modified by epistemic vocabulary) should be assumed to have this epistemic connotation. When my concern is specifically with the attribution of content or truth-conditions to modal assertions, I will either say so directly, or use modified

¹Thanks to Eric Hiddleston for pointing out this ambiguity.

expressions such as ‘metaphysical grounds,’ or ‘metaphysically justify.’ Thus, to say that someone is metaphysically justified in asserting p (or to say that someone has metaphysical grounds for p) is to say that this person is capable of limning the truth-conditions for p .

What is the contribution of primitive modality in support of Shapiro’s contention that the modal nominalist has no resources for supporting the modal assertions which figure in modal nominalist theories? Recall from the previous chapter that Shapiro takes the alleged fact of the definitional equivalence between set theory and modal nominalist theories to be itself sufficient evidence for placing an epistemic burden on the modal nominalist *a propos* of her use of modality. This gives the appearance that a fundamental issue for the modal nominalist is that she is *epistemically* unjustified in making modal assertions. Indeed, Shapiro presents this charge as standing independently from the claim that set theory is the “source of the precision” of the modal notions. However, and perhaps in distinction from Shapiro’s actual dialectic, I would like to suggest that the modal nominalist’s purported lack of epistemic justification is entirely derivative of the modal nominalist’s (*qua* modal primitivist) lack of *metaphysical* justification for modal assertions.²

Notice that the claim that set theory is the “source of the precision” of the modal notions is naturally interpreted as saying that the platonist has resources for justifying modal assertions according to the *metaphysical* reading—rather than as a claim that the platonist has epistemic resources for justifying modal assertions. What the set- or model-theoretic reduction provides is an account of the truth-conditions of logical possibility claims.³ These metaphysical resources provide epistemic gains in a certain sense—they help the platonist to determine *just what* modal knowledge consists in—e.g., knowledge

²On this point let me be clear: I am not disputing that Shapiro objects to modal nominalism on account of its apparent inability to ground modality in both epistemic and metaphysical senses (though he would likely describe both senses as ‘epistemic’)—it is only that it is not obvious to what degree Shapiro takes these objections to be related. It seems consistent with his criticism that these objections are less-closely related than I maintain below.

³I will describe the set-theoretic reduction in greater detail in §5, but the following characterization should be sufficient for the moment: The set-theoretic reduction essentially holds that the set-theoretic hierarchy exists and what can be constructed in the set-theoretic hierarchy produces all possible models. The notions of logical possibility and consistency can then be analyzed as truth in a model; the notion of logical necessity can be analyzed as truth in all models.

about sets and what is or can be modeled in set theory. The platonist's metaphysical resources, then, help her to gain traction on just how intractable (or not) her modal epistemology is. Meanwhile the modal nominalist, *qua* modal primitivist, provides *no* metaphysical resources for justifying the modal assertions she makes. That is, the modal nominalist deprives herself of the ability to provide an account of the truth-conditions of modal assertions. This situation does represent an "epistemic loss" in the sense that now the modal nominalist seems incapable of even circumscribing the basic form of her modal epistemology, and consequently, she is not able to make reliable judgments about what epistemological difficulties she might face (if any) and how these difficulties compare to those faced by platonistic reductionists. This problem seems more accurately described as a metaphysical problem as opposed to an epistemic one.⁴ The worry is that modal nominalists (*qua* modal primitivists) are unable to provide an account of what it is that makes modal assertions true or false. This does, however, suggest a related epistemic concern—if modal nominalists cannot explain what it is that makes modal assertions true or false, how could they ever have sufficient reasons for believing that modal assertions are true?

But given that modal epistemology under the set-theoretic reduction simply reduces to the epistemology of platonist set theory, it seems premature to claim, as Shapiro suggests, that the set-theoretic reductionist is in a preferable position when compared to the modal primitivist. That is because it is doubtful that the set-theoretic reductionist can show that assertions about the existence and character of the set-theoretic hierarchy are any better epistemically justified under platonism than are modal assertions under modal nominalism.⁵ What, then, should be made of Shapiro's "epistemic" objection to modal nominalism? I argue below that little is ultimately to be made of this criticism because the problems modal nominalists encounter when advancing claims about what is primitively possible are not clearly worse than the problems platonists encounter when advancing

⁴Thanks to Eric Hiddleston for this observation.

⁵Of this, more later.

claims about what can be modeled in the set-theoretic hierarchy. The ultimate goal of this chapter, then, is to undermine the idea that the set-theoretic reduction of the logical modalities, solely in virtue of its reductive character (i.e., solely in virtue of its capacity to *metaphysically* justify modal assertions), provides a means for *epistemically* justifying assertions about what is logically possible. If that is right, i.e., if the set-theoretic *reduction* itself does no epistemic-justificatory work, then it would be misleading to criticize modal nominalism on the grounds that modal nominalists, *qua* modal primitivists, employ epistemically unjustified (or unjustifiable) modal assertions.

3.2.1 Mission Planning

Unfortunately it is not clear how best to proceed with this discussion. One reason for this is that Shapiro does not explain in much detail how the set-theoretic reduction is supposed to help illuminate the epistemology of the logical modalities, but instead appears content to observe that, “[w]e inherit the language/framework [of modal terminology in its application to mathematics], with the connection to set theory already forged” (1997, 238). What this suggests is that assertions about what is logically possible are likely for Shapiro to be justified, both metaphysically and epistemically, along the same lines as he proposes to justify beliefs about the existence of *ante rem* structures: That structures exist allegedly follows from the *coherence* (Shapiro’s own modal primitive) of descriptions of structures. The existence of the set-theoretic hierarchy is ultimately demonstrated by positing that the set-theoretic hierarchy can be coherently described, perhaps by the ZFC axioms. But this strategy would lead to a clear circularity if used to justify the set-theoretic reduction: Why is λ logically possible? Because λ can be modeled in the set-theoretic hierarchy. Why does the set-theoretic hierarchy exist? Because set theory is coherent. Recalling here that coherence is a *modal primitive* should be sufficient for explaining why the previous explanation is circular, but perhaps somewhat more can be said. Shapiro describes coherence as an informal correlate of the notion of satisfiability, and further, that coherence can be explicated model-theoretically (ibid., 135). But there is virtually nothing

that distinguishes coherence from a primitive notion of (second-order) logical possibility (which can similarly be “explicated” model-theoretically). For example, the content of the assertion that ‘ZFC is coherent’ is not appreciably distinct from the content of the assertion that ‘ZFC is (second-order) logically possible.’ (That these notions can be explicated model-theoretically is of little help because for Shapiro the existence of models *derives from* the coherence of set theory.) So if Shapiro is right that modal nominalists are metaphysically and epistemically in the dark about logical possibility, then there would appear to be serious room for doubting that Shapiro himself is any less in the dark (metaphysically and epistemically) when it come to coherence. And further, given the purported reductive relationships between modality and set theory, and between set theory and coherence, Shapiro would also be in the dark about logical possibility. (More on this in §5.)

A second reason why it is not clear how to proceed from here is that questions are seldom raised about the use of set theory as an appropriate vehicle for justifying assertions about what is logically possible. However, there is another reduction for which questions of justification *have* often been discussed—David Lewis’s reduction of the metaphysical modalities to possible worlds. There are many structural affinities between the set-theoretic reduction of the logical modalities and Lewis’s possible worlds reduction of the metaphysical modalities. For instance, each reduction is usually proposed as vindicating the core semantical and inferential properties attributed to necessity and possibility under **S5** systems of modal logic (in which the accessibility relation between “worlds” is reflexive, transitive, and symmetric). In the next section I shall argue that Lewis’s view presents a clear example of the idea that reductive theories of modality, solely in virtue of their reductive characters (i.e., solely in virtue of their ability to *metaphysically* justify or ground modal assertions), do not have any advantages over modal primitivism when it comes to *epistemically* justifying modal assertions. In §4 I identify the structural elements of this reasoning, which I then apply in §5 to the set-theoretic reduction of the logical modalities.

3.3 Justifying Modal Assertions Under Lewis's Reduction

Perhaps no theory of modality has attracted more attention than the modal realism elaborated and defended by David Lewis. According to Lewis, in addition to the actual world, there exist countless other possible worlds of (nearly) all shapes and sizes—“*every way that a world could possibly be is a way that some world is*” (1986, 2). Lewis's worlds are offered as fodder for a reduction of the *metaphysical* modalities—to be metaphysically possible is to be true in a possible world; to be metaphysically necessary is to be true in all possible worlds—and such talk of possible worlds is to be construed literally. After describing the basic working parts of Lewis's reduction, I show that the reductive elements of modal realism—the analyses of metaphysical possibility and necessity in terms of possible worlds—serve only as rewrite rules, and confer no justification upon lone assertions about what is metaphysically possible or upon lone assertions about what transpires in possible worlds. Thus, Lewis's reduction of the metaphysical modalities is not in itself a useful implement for justifying assertions about what is metaphysically necessary or possible. For the remainder of this section the reader should assume, unless told otherwise, that the modalities under discussion are the metaphysical modalities.

3.3.1 Lewis's Reduction

The salient features of Lewis's account are as follows: A *possible world* is the mereological sum of a series of spatiotemporally related individuals.⁶ No two spatiotemporally unrelated individuals form parts of the same world. A principle of recombination holds that for any two individuals, there is a possible world containing spatiotemporally related qualitative duplicates of both individuals.⁷ Actuality is indexical. The following truth-conditions apply for *de dicto* modal assertions:

1. Possibly-*p* is true iff *p* is true in a possible world.

⁶For a succinct account of the mereological principles required, see (Maguire, *forthcoming*).

⁷“Size and shape permitting.” See (Lewis 1986, 89-90).

2. Necessarily- p is true iff p is true in every possible world.

Lewis's account also purports to provide truth-conditions for *de re* modal assertions. For Lewis, individuals exist in one and only one world—transworld individuals (at least, transworld *concreta*) are not countenanced. Instead, he invokes intransitive transworld similarity relations known as counterpart relations. For x and y belonging to distinct worlds, x is a counterpart of y just in case nothing in x 's world more closely resembles y than x itself (x may not be unique in this regard, and there may be worlds containing nothing similar enough to y to count as one of y 's counterparts). The truth-conditions for *de re* modal assertions may now be stated:

3. y is possibly- P iff for some counterpart x of y , x has P .
4. y is necessarily- or essentially- P iff for every counterpart x of y , x has P .

In addition to providing a reductive account of the metaphysical modalities, Lewis's modal realism is alleged to provide parsimonious accounts of counterfactual conditionals, supervenience, propositions, and properties. For instance, a proposition can be identified with the set of all possible worlds in which it is true; similarly, a property can be identified with the set of all individuals that have the property. In Lewis's words, his universe of possible worlds is a "philosopher's paradise."

That Lewis's modal realism possesses all of these allegedly useful qualities is offered as an inference to the best explanation style argument for the existence of possible worlds, and, more generally, for the truth of Lewis's overall theory of modality.⁸ I must confess to a large degree of skepticism about this justification for modal realism—both about the idea

⁸Lewis's appeal to parsimony is somewhat ironic, to say the least, since Lewis's ideological parsimony comes at the cost of an *extravagant* ontology. Lewis claims that this increase in ontology is quantitative rather than qualitative (1973, 87), but this illusion evaporates when one's attention is drawn to the denizens of Lewis's worlds. Charles Chihara writes that, "Lewis does think there are unicorns, spirits, and all manner of strange creatures. *The ontological commitments of his doctrines are as heavy as any theory any philosopher I know has ever espoused.* This is a very high theoretical price to pay for something as nebulous and questionable as a parsimonious set of primitives" (1998, 102). There seems to be little to recommend the idea that considerations of parsimony support Lewis's modal realism (unless one is contrasting it with an even more ontologically extravagant view!).

that Lewis's account genuinely provides the *best* overall account of modality *and* about the idea that the use of inference to the best explanation is an acceptable method for justifying the kind of metaphysical existence claims Lewis's theory asserts. But it would fall beyond the purview of this dissertation to evaluate Lewis's use of IBE-style reasoning in support of modal realism.⁹ Moreover, I think that on this occasion it is appropriate to sidestep the issue about whether, in general, Lewis is justified in claiming that there exist possible worlds. This is because even if Lewis has a plausible case for the general claim that there exist possible worlds, that would nevertheless fail to address the specific issue of whether it is acceptable to appeal to his reduction of modality in terms of worlds in order to justify assertions about what is possible.

My interest shall be, then, on what Lewis has to say about justifying modal assertions. As it turns out, to whatever extent that it is possible to justify modal assertions under Lewis's reduction, this justification accrues in relative isolation from his analysis of modality in terms of worlds, and has instead to do with some of the ancillary components of modal realism, *viz.*, the hypothesis that commonsense modal intuitions are largely justified as they come. Note that most of what I say below is *not* intended as a criticism of modal realism, which was not constructed for the purpose of (epistemically) justifying modal assertions.¹⁰ Rather, I am attempting to establish a more general precedent for my reply to Shapiro, which is to deny that reductions of modality, solely in virtue of their reductive characters, provide a means for justifying modal assertions.

3.3.2 Lewis and Justifying Modal Assertions

According to an early objection due to Tom Richards, even commonsense beliefs about modality are largely unjustified under Lewis's reduction. Given that other possible worlds are spatiotemporally isolated from the actual world, it stands to reason that no one is

⁹See (Shalkowski 2010) and (Shalkowski 2012) for criticism of Lewis's IBE/theoretical-utility justification for modal realism.

¹⁰Though it does, of course, offer *metaphysical* justifications or grounds for modal assertions. So it is worth recalling here that lone instances of 'justify' refer to the *epistemic* sense of the term as described in §2.

capable of “inspecting” other possible worlds in order to find out what holds true in them. Thus, if being possible genuinely *is* being true in a possible world, then no one knows what is merely possible, since no one has knowledge of what transpires in other worlds (Richards 1975, 109-10). Lewis replies by arguing that since commonsense beliefs about modality *are* largely justified, then the truth of his theory shows that knowledge about what transpires in other worlds is unproblematic in at least some cases. For instance, the intuition that it is possible for there to be talking donkeys is all the evidence that is required under Lewis’s theory for asserting that there indeed exist possible worlds (*countless* worlds, thinks Lewis) in which there are talking donkeys (Lewis 1986, 110).

Lewis’s response to Richards, then, is to argue that it *is* possible to have knowledge about what transpires in other worlds, because already existing and justified modal knowledge, together with the left-to-right hand readings of (1) and (2), justify some assertions about what is true in some merely possible worlds. This, however, is not to contest Richards’s objection, which is that modal *knowledge* is not acquired by inspecting merely possible worlds. As Richards observes, if Lewis is right about how knowledge about possible worlds is acquired—i.e., that this knowledge is acquired, “according to one’s lights about what is possible”—then possible worlds, “can shed no light on the question of what is possible” (Richards 1975, 112). Lewis does not contest this, and further, there does not appear to be any important reason why he must contest this.¹¹ After all, to claim that possibility can be analyzed in terms of possible worlds, and that what transpires in other worlds comprises the truth-conditions of modal assertions, does not require a theory about the extent of possibility—a theory about what is and is not possible. Nor does such an analysis require a concomitant theory of how anyone comes to *learn* about what is and is not possible.¹² It is somewhat surprising, then, to see Richards draw the further conclusion that Lewis’s theory, “does not explain the meaning of ‘possible,’ ” because Lewis’s worlds shed no light on what is possible (Richards 1975, 112). To borrow from

¹¹Thanks to Michael McKinsey for some useful suggestions here.

¹²See (Cameron 2012) for a more detailed working out of this line of reasoning.

a related observation of (Cameron 2012), Richards appears to run together three distinct tasks—an account of the *extent* of possibility, an account of the *epistemology* of possibility, and an account of the meaning of the term ‘possible.’ It is perfectly reasonable that a reductive theory of modality could complete the last of these tasks without completing either of the first two. (Of course, it is reasonable to wonder what a theory of modality is good for if it does not help carry out either of the first two tasks—this is perhaps a more charitable reading of Richards’s criticism.)

The relevance of this discussion to the question of whether reduction assists in the justification of modal assertions is as follows: By Lewis’s own admission, knowledge about what transpires in other worlds is derivative of previously established modal knowledge. Lewis’s reduction of modality to worlds, then, does not do any work *justifying* modal assertions. The reduction merely serves as a device for *translating* modal assertions into assertions in terms of worlds, and vice versa, and in the process purports to reveal the truth-conditions for modal assertions. The role of the reduction, then, is to serve simply as a rewrite rule: Prior confidence that ‘*p* is true in a possible world’ can be used as evidence that ‘*p* is possible;’ and prior confidence that ‘*p* is possible’ can be used as evidence that ‘*p* is true in a possible world.’ But that ‘*p* is possible just in case *p* is true in a possible world’ nowhere provides *independent support* for lone instances of ‘*p* is possible’ or lone instances of ‘*p* is true in a possible world.’

Now, I think it is worth pointing out that there are good reasons for supposing that, at least in an epistemic sense, Lewis is justified neither in deducing assertions about worlds from modal assertions, nor in deducing modal assertions from assertions about worlds.¹³ This suggests that, if Lewis’s reductive analysis is true, then modal assertions are fundamentally *unjustifiable*. This is because once the connection from modal assertions to

¹³For criticism that one cannot deduce claims about worlds from modal claims, see (Bueno and Shalkowski 2000). The criticism that one cannot deduce modal claims from claims about worlds is hinted at in (van Inwagen 1998), but not made explicit. I am confident that the criticism can be made explicit (and persuasive), but since establishing this criticism is more than is required for my purposes, I will have to leave matters as they stand.

worlds has been severed, no longer can Lewis insist that commonsense modal intuitions yield reliable insights into what transpires in other worlds, cutting off entirely any access one might have to other worlds—worlds that still must provide the truth-conditions for modal assertions, according to Lewis’s analysis. It follows that it is impossible to justify modal assertions (barring what can be deduced from the actual world together with modal logic), at least under the assumption that justifying modal assertions presupposes or otherwise involves confidence that their truth-conditions obtain or are genuinely satisfied. This is, however, a stronger conclusion than is required for my purposes, as I only wish to substantiate the claim that reduction on its own does not help justify modal assertions.

Nevertheless it would be incorrect to claim that Lewis says nothing further about how to justify modal assertions, and it would also be incorrect to claim that Lewis says nothing further about the extent of possibility. He provides, at least in a preliminary way, an account of both items, and each involves his principle of recombination. Recall that this principle states that for any two individuals, there is a possible world containing qualitatively identical duplicates of those individuals. This principle is what generates or constructs Lewis’s entire modal universe from his basic mereological resources, thereby providing him with an account of the *extent* of possibility. Thus, if p is possible, then p refers to some state of affairs that can be constructed from Lewis’s basic mereological resources by repeated application of the principle of recombination. In other words, ‘being a state of affairs constructed from basic mereological resources by (repeated) application of the principle of recombination’ is coextensive with, ‘being true in a possible world.’

With respect to the justification of modal assertions, Lewis hypothesizes that commonsense modal intuitions are largely justified as they come, and further, that commonsense modal intuitions are *consequences* of his principle of recombination (or something like it) (Lewis 1986, 113). There are at least two ways of interpreting what it means to say that commonsense modal intuitions are consequences of the principle of recombination. According to one interpretation, the principle of recombination is evidentially and justificationaly

prior to commonsense modal intuitions. To use a mathematical analogy, this would be to hold that Lewis's basic mereological resources are like "axioms," that his principle of recombination is like a "proof theory," and that commonsense modal intuitions are derivable "theorems" that would be entirely unsupported (in both the metaphysical and epistemic senses of §2) without the former resources. According to a second interpretation, commonsense modal intuitions abductively justify the principle of recombination, with this principle providing something like a backwards-looking account of the content of these intuitions. To again use a mathematical analogy, commonsense modal intuitions would be equivalent to the kinds of arithmetical and number-theoretic intuitions (e.g., zero has no predecessor, $2+2=4$) that might be thought to abductively justify the Peano axioms. The Peano axioms, once abductively justified, can be used to derive rather surprising and highly non-trivial arithmetical assertions, but will generally also turn some of the original intuitions (e.g., $2+2=4$) into rather uninteresting theorems. So too might the principle of recombination, once abductively justified, be used to "derive" some rather surprising and highly non-trivial modal "consequences," in addition to being usable for recapturing commonsense modal intuitions (e.g., talking donkeys are possible) as "consequences."

Which interpretation should be attributed to Lewis? Note that Lewis says that the principle of recombination has uses in "imaginative experiments" in which one tries, "to think of how duplicates of things already accepted as possible... might be arranged to fit the description of an alleged possibility" (ibid., 113-4).¹⁴ That commonsense modal intuitions, by definition, fall under the province of commonsense, suggests that such intuitions are perfectly justified as they come. In other words, these intuitions serve to identify a loose conglomeration of assertions that are already accepted as possible. The principle of recombination is not required, then, in order to justify these intuitions (even if it is possible to use the principle of recombination in this way)—commonsense modal

¹⁴Lewis acknowledges that, "[f]or more far-fetched possibilities, recombination is less useful" (ibid., 114), presumably because it is unclear just what far-fetched possibilities are recombinations of. Of course, what counts as a far-fetched possibility appears to be unhelpfully subjective—perhaps talking donkeys are not far-fetched for Lewis, but (van Inwagen 1998) seems to think otherwise.

intuitions instead are fodder for the derivation of further modal “consequences.” This suggests that Lewis would opt for the second interpretation over the first.

But I do not see that, as far as my purposes are concerned, there is much that hangs on sorting out, under Lewis’s overall theory of modality, the precise relationship between commonsense modal intuitions and the principle of recombination. According to the first interpretation, all of the justificatory work is performed by the principle of recombination. According to the second interpretation, most (if not all) of the justificatory work is performed by commonsense modal intuitions (perhaps with the principle of recombination helping to extend modal knowledge beyond commonsense modal intuitions). But again, in neither case does the reduction of modality to worlds serve as anything more than a rule for translating modal assertions into worldly assertions (which are elliptical for descriptions of complex products of the principle of recombination) and for translating worldly assertions into modal assertions. So whether Lewis’s principle of recombination provides an acceptable means for justifying modal assertions is largely immaterial, because the *reductive* portion of his theory still serves as a mere rewrite rule, and does no justificatory work. That work is left entirely to some admixture of commonsense modal intuitions and applications of the principle of recombination.

Let me reemphasize that I have not attempted to mount either a criticism of Lewis’s reduction of modality in terms of worlds or a criticism of Lewis’s views about the justification of modal assertions. What I have identified is only the idea that the fact that modal realism is a *reductive* theory of modality is of no significance whatsoever to modal realism’s prospects for justifying modal assertions. Nonetheless, this idea does suggest a criticism of Shapiro’s “epistemic” objections to nominalism, which holds that reduction facilitates a more tractable modal epistemology.

3.4 Possible Worlds and Functional Roles

The upshot of the previous section is that although Lewis's reduction is capable of metaphysically justifying modal assertions (because it provides truth-conditions for modal assertions), the reduction itself does nothing at all to epistemically justify modal assertions. I believe that this observation generalizes to most, if not all reductions of modality—at least, to most or all reductions that utilize the possible worlds metaphor—because, as I shall argue, the observation has important structural elements.

Nearly every theory of modality grants credence to the notion that the possible worlds metaphor is useful for identifying the functional roles that analyses of necessity and possibility ought to fill.¹⁵ Structurally, there is little that distinguishes Lewis's reduction of modality from all other accounts of modality that seek to vindicate Kripke-style modal semantics, including the accounts given by (Armstrong 1989), (Peacocke 1999), (Plantinga 1974), (Rosen 1990), and (Stalnaker 1984). It would seem that Kripke-style modal semantics constitutes something of a shared or background theory that provides implicit, functional accounts of necessity and possibility: There is a non-empty domain of φ s; that p is necessary means that p is true in all φ s; that p possible means that p is true in a at least one φ . Each theory then analyzes necessity and possibility in terms of the particular entities they substitute for φ . In the case of (Armstrong 1989), the entities are maximal states of affairs; for (Plantinga 1974) the entities are maximally consistent sets of propositions. Of course, in Lewis's case, the entities are spatiotemporally disconnected universes. What distinguishes these views, then, is not the structure of their accounts of modality, but rather the particular metaphysical commitments they take to be required for eliciting the truth-conditions of modal assertions.

For each theory, then, it is a mere triviality that necessity *just is* truth in all φ s and that possibility *just is* truth in a φ (substituting for φ). Before substituting for φ , the structure

¹⁵Thanks to Eric Hiddleston for suggesting this way of looking at the debate. For a full-blown functionalist account of modality, see (Gregory 2006).

simply describes a functional role—a role that is to be filled by different entities depending on which theory is adopted. But notice that these general analyses are mere rewrite rules—that possibility *just is* truth in a φ does not provide independent support for claims like ‘ p is possible’ or for claims like ‘ p is true in a φ .’ It is true that prior confidence that ‘ p is true in a φ ’ together with the truth of the general analysis will entail that ‘ p is possible.’ But that would not be an explanation for why one is justified in holding that ‘ p is true in a φ ,’ which almost certainly would be in doubt if one were in doubt about whether p is possible.

What this means is that, in general, reduction is of little use when one’s interest lie in justifying modal assertions. Reduction can be used to translate an already justified modal assertion into a justified assertion about the domain of φ s, and it can be used to translate an already justified assertion about the domain of φ s into a justified modal assertion. But reduction cannot be used to provide initial justification to any modal assertion or to any assertion about what transpires within the domain of φ s. To say otherwise would be to confuse an account of the truth-conditions for modal assertion for an account of what justifies modal assertions.

In the next section I shall argue that the set-theoretic reduction of the logical modalities, like Lewis’s reduction of the metaphysical modalities, provides another example of the general idea that reduction does not serve to justify modal assertions. If that is right, then since the truth of set theory is in at least as much doubt as the truth of the modal assertions modal nominalists make, it follows that it would be incorrect for Shapiro to insist that someone in his position is better able to justify the modal assertions nominalists make. If these assertions are unjustified for modal nominalists, they are no less unjustified for set-theoretic reductionists. Moreover, since the truth of set theory is in doubt, Shapiro cannot establish with any kind of certainty that the reductive truth conditions for logical possibility assertions are in fact realized.

3.5 Lessons for Shapiro and Set Theory

According to Shapiro, set theory provides the metaphysical resources or grounds for supporting assertions about the logical modalities—if not in general, then at least in application to logical possibility and necessity claims about the truth or not of mathematical theories and the existence or not of mathematical objects. The idea here is that the truth-conditions for such claims are supplied by facts about what kinds of set-theoretic constructions can be modeled in set theory. The notion of logical possibility is linked to being *true in a model*; logical necessity to being *true in all models*. To paraphrase Shapiro, the idea is that the set-theoretic universe is so rich and expansive that it provides the materials to construct or model every possible mathematical object. After describing the basic working components of the set-theoretic reduction, I show that the analyses of logical necessity and possibility in terms of models serve as mere rewrite rules and confer no justification upon lone assertions about what is logically possible or necessary. Thus, the set-theoretic reduction is not in itself a useful implement for justifying these assertions. For the remainder of this section the reader should assume, unless told otherwise, that the modalities under discussion are the logical modalities.

3.5.1 The Set-theoretic Reduction

The model-theoretic treatment of the logical modalities has quite a tumultuous history—C.I. Lewis’s pioneering work in modal logic precipitated Quine’s famous criticism¹⁶ and set numerous philosophers and logicians to work attempting to produce a viable, non-modal interpretation for the semantics of modal logic. I will not attempt an overview of the dispute between Quine and the modal logicians.¹⁷ The upshot is that Kripke-style possible-worlds semantics have become the standard formal vehicles through which to

¹⁶“There are logicians, myself among them, to whom the ideas of modal logic (e.g. Lewis’s) are not intuitively clear until explained in non-modal terms” (Quine 1947, 43).

¹⁷The interested reader is encouraged to consult (Marcus 1993b), (Ballarin 2004), and (Ballarin 2005) for discussion and references.

explicate the logical modalities.¹⁸

For modal propositional logic,¹⁹ one defines a *model structure* as an ordered triple (G, K, R) in which K is a non-empty set, G is a member of K , and R is a relation on K . A *model* is a binary function $\varphi(p, H \in K)$ (for atomic formulae p) that produces as values T or F. Non-atomic formulae are assigned values inductively. \Box and \Diamond are sentential operators that obey the following equivalences:

5. $\varphi(\Box p, H) = T \equiv \varphi(p, H') = T$ for every $H' \in K : HRH'$; otherwise $\varphi(\Box p, H) = F$.

6. $\varphi(\Diamond p, H) = T \equiv \varphi(p, H') = T$ for some $H' \in K : HRH'$; otherwise $\varphi(\Diamond p, H) = F$.

Intuitively, K is a set of possible worlds, G is the actual world, and R is an accessibility relation on K (a number of systems of modal logic can be differentiated on the basis of whether this relation is transitive, symmetric, etc.). Here ‘accessibility’ can be interpreted as ‘possible relative to’—thus to say that GRH is to say that H is a possible world relative to the actual world. A model, then, determines what propositions are true in what possible worlds. (5) and (6) state, respectively, that a sentence is necessarily true just in case it is true in every possible world (and not necessarily true otherwise); and that a sentence is possibly true just in case it is true in at least one possible world (and not possibly true otherwise).²⁰ Of course, as many expositors note, this intuitive picture is offered merely for heuristic purposes—in describing modal semantics readers are often warned against supposing that one is literally quantifying over things called ‘possible worlds.’

How can the model-theoretic treatment of the logical modalities serve as a reduction? First I need to clear up an ambiguity between two different senses or uses of the term ‘model.’ In Kripke-style semantics, a model is defined as a function from propositions and worlds to truth-values. Below I will refer to this use of ‘model’ by the term ‘modal-model.’

¹⁸Similar formalisms were antecedently developed by Jaakko Hintikka and Stig Kanger.

¹⁹What follows is drawn from Kripke (1963).

²⁰For modal predicate logic, the formalism must be expanded; each ‘world’ must be assigned a domain of objects and these objects must be assigned predicates. A tremendous amount of ink has been spilled over whether this domain is allowed to vary in size and makeup from world to world.

This modal use of the term ‘model’ is distinct from the ordinary, non-modal use of the term, in which a model is merely an *interpretation* of the non-logical vocabulary of some sentence or set of sentences. I will use the non-modified ‘model’ to refer to this ordinary use of the term. Note that Kripke-style semantics invokes both usages. Each of Kripke’s “worlds” can be thought of as a model (in the ordinary sense); meanwhile, each modal-model includes a *set* of models.²¹ I mentioned previously that the logical notions of possibility and necessity can be linked to being true in a model and being true in every model (respectively). These associations should now appear ambiguous—is logical possibility to be analyzed as truth in at least one *modal-model*, or simply as truth in at least one *model*? The answer is the latter. The set-theoretic reductionist’s gambit is that the universe of sets contains the resources to construct or model all of the logical possibilities. Thus, the set-theoretic hierarchy constitutes the domain for the *alethic* modal-model.

Note that to be effective as a reduction of logical modality, set theory must furnish the *truth-conditions* for claims of logical possibility and necessity—otherwise set theory at best serves as a mere representational device. However, there is a certain awkwardness associated with insisting that set theory provides the truth-conditions for all logical possibility claims. Presumably it is logically possible that Larry had become a fishmonger rather than a philosopher, but it would be implausible to argue that the content of the claim that ‘it is logically possible that Larry is a fishmonger’ is that there exists a pure set of a certain sort. Thus, it is implausible to claim that set-theory provides reductive truth-conditions for all logical possibility assertions. (Though it could be said that set theory provides the means for *modeling* or *representing* all logical possibilities—e.g., as a means for making it evident that ‘Larry is a fishmonger’ does not lead to a logical contradiction.) On the other hand, there seems to be little awkwardness associated with the claim that set theory provides truth-conditions for logical possibility claims about the existence of mathematical objects and for logical possibility claims about the existence of models of mathematical theories.

²¹For a similar observation, see (Ballarin 2005, 284).

Indeed, Shapiro seems confident that set theory is comprehensive in this way (1997, 136).²²

3.5.2 Justifying Modal Assertions Under the Set-Theoretic Reduction

Scott Shalkowski (2004) has advanced something like a set-theoretic analogue of the kind of epistemic objection Richards's raises against Lewis's reduction. Recall that Richards's objection to Lewis is essentially that, if knowledge about what is metaphysically possible requires some prior means of inspecting Lewis's possible worlds, then since no one is capable of making such inspections, it follows that no one knows very much about what is metaphysically possible. In application to set theory, Richards's objection would be that if knowledge about what is logically possible requires some prior means of inspecting sets and models, then if no one is capable of making such inspections, then no one knows very much about what is logically possible. What Shalkowski purports to show is that the only means anyone has for inspecting sets and models is *via* prior intuitions about logic and the logical modalities.

According to Shalkowski, "[t]he epistemology of model theory relies on what mathematicians and logicians have antecedently taken to be valid inference as well as what they have taken to be initially plausible candidates for axioms of set theory" (ibid., 70-1). "For instance," he continues somewhat later,

... it is assumed by classical logicians that models obey the standard principles of classical logic: Noncontradiction and Excluded Middle. When we arrive at

²²One important obstacle to the idea that set theory provides a bona fide modal-model (even for logical possibility claims about mathematical theories) is that there are multiple non-isomorphic set theories. Presumably more than just one of these theories is consistent. But if that is the case, then which set theory is it that produces the alethic modal-model for logical possibility? Or should logical possibility be analyzed as what can be modeled or constructed in *any* consistent set theory? This multiplies the difficulties on two fronts: First, in order to be genuinely reductive, all of these consistent set theories must be true. Since they are non-isomorphic theories, this new reduction is now committed to the existence of multiple set-theoretic hierarchies, *sensu* the full-blooded or plenitudinous platonism of (Balaguer 1998). Second, the notion of consistency as it applies to the various set theories cannot be treated set- or model-theoretically. The trouble is that the model-theoretic account of the logical modalities is *theory-relative*. It provides a means for deciding the modal profiles of sentences *only under the assumption of some background set theory—but, importantly, it does not appear to have anything to say about the modal profile of this background theory*. One could attempt to move to a meta-set theory in order to capture something like a model-theoretic reduction for all of these modal-models. But I do not think this would be a feasible option except under the assumption of a universal domain—a highly problematic assumption and one that I do not have space to evaluate in any detail.

surprising results like the Löwenheim-Skolem theorems, our use of assumed logical facts do all the work in telling us about the number and nature of models. Whether the process is one of construction or discovery, the logical dog is wagging the model-theoretic tail. . . Those with nonstandard ideas about logical principles will, understandably, have nonstandard ideas about the existence and representative capacities of the set of models. (ibid., 71)

Shalkowski's moral is relatively clear, at least from an epistemological point of view: Insight into the nature of the set-theoretic hierarchy is entirely dependent on prior assumptions about logic and logical possibility. Why does the set-theoretic hierarchy contain the particular members that it does? Well, because the membership of this hierarchy can be characterized by a set of jointly logically possible axioms together with some or other account of logical consequence. But what makes those axioms jointly logically possible? It would clearly be circular to insist at this point that the axioms of set theory, e.g. ZFC, are jointly logically possible *because* they have a model. What this suggests is that the set-theoretic reduction itself does not assist in the justification of modal assertions (or of assertions about set existence).

Shalkowski, like Richards, opts for a somewhat stronger conclusion, *viz.*, that it is not possible to non-circularly justify the truth of the set-theoretic analyses of the logical modalities, because no one can guarantee that the logical principles used to construct the set-theoretic hierarchy are genuinely in accord with the facts of logical possibility (ibid., 72). I suspect that this charge of circularity makes something like the mistake Richards makes—to conflate an identification of the truth-conditions for modal assertions with an account of the extent and epistemology of modality—but I shall not press the issue. I am content to note that the set-theoretic reduction itself confers no justification upon modal assertions, and I think this point can be sustained without accusing the set-theoretic reduction of engendering circularity. The explanation for why the set-theoretic reduction itself confers no justification upon modal assertions is simply that the set-theoretic analyses of the logical modalities, like Lewis's analyses of the metaphysical modalities, serve only as rewrite rules. The only difference is that in this case the analyses are rewrite rules

for translating modal assertions into set-theoretic assertions, and vice versa. Thus, that possibility can be *analyzed* as truth in a model does nothing to justify lone modal assertions, or lone assertions about what models exist.

What is the significance of these developments for Shapiro's criticism of nominalism? Well, if it is possible to justify some set existence claims, then it is possible to use the right-to-left readings of (5) and (6) to justify some modal assertions. For this reason, Shapiro would have a decisive advantage over the modal nominalist if he were in a position to justify set existence claims—in particular, if he were able to justify the set-theoretic correlates of the modal assertions modal nominalists make. Therefore it is worth restating the case for why Shapiro is *not* in a position to justify set existence claims—a case made in some detail in the previous chapter. On Shapiro's view, the positions of *ante rem* structures are themselves bona fide mathematical objects, which means the existence or not of sets is a consequence of the existence or not of the *ante rem* set-theoretic structure. That such a structure exists for Shapiro follows from the *coherence*—a modal primitive—of set theory.²³ What I argued in the previous chapter was that the inference from the *coherence* of set theory to the *existence* of the set-theoretic structure (and hence, the existence of sets) is entirely unjustified. Coherence does not imply existence, even for structures. Thus, Shapiro is not himself in a position to justify set existence claims, even if he is in a position to justify claims about what mathematical theories are coherent. It follows that Shapiro does not have any kind of advantage over the modal nominalist in virtue of his insight into the set-theoretic hierarchy.

Is Shapiro in a position to justify coherence assertions? And might he claim some kind of advantage over modal nominalists concerning the justification of modal assertions, by way of any facility he might have for justifying coherence assertions? I think that both prospects must be rejected. It must be recalled that coherence itself is a primitive notion, and given its indistinctness from second-order logical possibility, it would seem to be a

²³The general idea here is codified as an axiom of Shapiro's structuralism—The Coherence axiom, which states that coherent formulae of second-order languages are satisfied by actually existing structures.

kind of *modal* primitive.²⁴ Thus, the same metaphysical and epistemic concerns that apply to modal nominalists, *qua* modal primitivists, also apply to Shapiro, *qua* modal primitivist (*re* coherence). Shapiro is therefore as much in the dark about coherence, metaphysically and epistemically, as modal nominalists are about primitive modality.

Shapiro, of course, insists that it is a *presupposition* of mathematics that set theory is coherent (1997, 136), which he appropriates in support of the idea that he is *epistemically* justified in making coherence assertions (despite apparently lacking *metaphysical* justifications for coherence assertions). But given the indistinctness of coherence and second-order logical possibility, this presupposition can be appropriated by modal nominalists in support of the claim that the axioms of set theory are jointly logically possible. Thus, modal nominalists would appear to have an equally reliable *epistemic* justification for logical possibility claims (despite their nevertheless having no *metaphysical* justification for these claims). Shapiro's insistence that it is a presupposition of mathematics that set theory is coherent, then, concedes quite a lot to the modal nominalist. This would be, in Lewisian lingo, to identify the coherence or logical possibility of set theory as a commonsense intuition about the logical modalities.²⁵ Such commonsense intuitions are justified for all, if they are justified for anyone. Modal nominalists are not blind to the presuppositions of mathematics, whatever these might be, and it would be special pleading were Shapiro or anyone else to insist otherwise. Whether it *is* a commonsense modal intuition that set theory is coherent is debatable, but nothing here turns on how this debate gets resolved: If the coherence of set theory is a commonsense modal intuition, then Shapiro

²⁴And given this, it is reasonable to wonder whether the set-theoretic reduction of the logical modalities would be genuinely reductive under Shapiro's structuralism—Shapiro is in effect making the dual proposal that logical possibility is reduced to set existence, and set existence is reduced to coherence. This chain is *very* tight, even if it does not double back upon itself. I find it somewhat curious that Shapiro recognizes that it would beg the question to reduce coherence to set theory (1997, 135), while at the same time failing to recognize that the inherent affinities between coherence and logical possibility suggest that it would similarly be question-begging to reduce logical possibility to set theory. Of course, even if these notions were distinct enough to make it plausible that logical possibility *is* reducible to set theory, while coherence *is not*, Shapiro is nonetheless stuck with the modal primitive of coherence.

²⁵Cf. Shapiro: "Structuralists accept this presupposition and make use of it like everyone else, and we are in no better (and no worse) of a position to justify it" (*ibid.*, 136).

and the modal nominalists are on an equal, positive footing concerning their ability to justify coherence and logical possibility claims about set theory; if the coherence of set theory is not a commonsense modal intuition, then Shapiro and the modal nominalists are on an equal, negative footing concerning their ability to justify coherence and logical possibility claims about set theory. Either way, Shapiro's elaborate "epistemic" criticism of modal nominalism does not show that the modal nominalist (*qua* modal primitivist) is in a uniquely disparaging position regarding the justifiability of modal assertions, because not even Shapiro (*qua* set-theoretic reductionist) is in a better position to justify modal assertions. In any case, it would certainly be false to say that the set-theoretic reduction itself does any work for Shapiro justifying modal assertions—that work seems entirely derivative of what is presupposed in mathematics.

My basic point is this: A reduction of logical possibility to set theory does no epistemic-justificatory work on its own. If Shapiro is correct to insist that commonsense modal intuitions are too vague to support the modal assertions modal nominalists make (*ibid.*, 232), then the same must be said about the relationship between commonsense intuitions and the coherence assertions Shapiro makes. Since coherence assertions provide the metaphysical and epistemological grounds for his set existence claims, and these existence claims in turn provide the truth-conditions for modal assertions (under the set-theoretic reduction), then any disconnect between commonsense intuitions and coherence assertions is ultimately parasitic on both set existence claims and modal assertions. If Shapiro cannot justify coherence assertions, then he can justify neither set existence claims nor modal assertions.

Of course, in all probability Shapiro is correct to say that an understanding of the modal notions is mediated by mathematics and by set theory (*ibid.*)—regardless of whether 'understanding the modal notions' here means the ability to metaphysically or epistemically justify modal assertions. At least as far as the epistemic justification of modal assertions goes, it is also likely that the best evidence available for or against the coherence or logical

possibility of a mathematical theory comes from the theory's fruitful employment in mathematics (perhaps in conjunction with extensive, failed efforts to prove the inconsistency of the theory). But Shapiro is only entitled to say these things *without* the prejudicial reading according to which an understanding of the modal notions is only possible when mediated by a *platonistic* interpretation of mathematics and set theory. Although the coherence or logical possibility of set theory and other mathematical theories *may* be presuppositions of mathematics, it is no presupposition of mathematics that sets and other mathematical objects *exist*.²⁶ It would be an error to claim that it is a violation of *nominalism* to use established mathematics to inform debate about coherence and modality.

Modal nominalists, *qua* modal primitivists, might be unable to metaphysically justify or ground modal assertions in the sense that they are unable to articulate the truth-conditions for these assertions. But as far as modal assertions about mathematics are concerned, they would appear to be able to epistemically justify modal assertions in the sense that mathematical practice provides a *reason to believe* that these assertions are true. What extinguishes Shapiro's criticism, then, is (a) that Shapiro similarly cannot articulate the truth-conditions for coherence assertions—these assertions for him remain metaphysically unjustified—and (b) that the only clear epistemic justification he has for coherence assertions comes from an appeal to mathematical practice. Shapiro and the modal nominalists, then, are essentially in the same boat, hence it is incorrect to accuse modal nominalism of raising particularly vexing questions about the metaphysical and epistemic justification of modal assertions.

3.6 Living With Primitive Modality

I suspect that nominalists are driven to accept primitivism about modality because no reduction on record avoids commitment to some kind of abstract ontology. A basic presupposition of the debate between modal nominalists and platonists like Shapiro is that primitive modality is not ontologically committing. For instance, Mark Balaguer insists

²⁶For more here, see chapters 4 and 5, *passim*.

that primitive modality is nominalistically acceptable because, “it is entirely obvious that [modal primitives are not] defined in terms of abstract objects, because [they do not] have any definition at all” (1998, 71). But is it true that modal primitivism avoids commitment to any kind of abstract ontology?

Consider again the functional understanding of possibility and necessity from §4: Kripke semantics provides an attractive structure or framework with which to construct an account of modality: There is some domain of φ s; necessity is truth in all φ s; possibility is truth in a φ . Nominalist modal primitivists, just like most other modal theorists, share an interest in using this kind of structure in order to vindicate the inferential patterns governing the modal operators. How is the primitivist position to be understood in reference to this idea? Is modal primitivism best understood as *denying* that there is anything that could fill the φ role? Alternatively, is modal primitivism best understood as the bare claim that *something* fills the φ role without any further comment about what that something is? Or is modal primitivism to be understood in some other way entirely?²⁷

To insist that modal primitivism commits one to denying that *anything* fills the φ role is tantamount to rejecting the idea that modal assertions have truth-conditions and would imply either that modal truths do not require truthmakers or that modal discourse is not truth-apt (McLeod 2001, 27). I suspect that neither implication is desirable. The latter implication—that modal discourse is not truth-apt—is clearly in conflict with Chihara’s and Hellman’s motivations to vindicate the claim that mathematics comprises a body of truths. For instance, if Hellman is correct that the goal of mathematical theorizing is to describe structural possibilities, then realism in truth-value for mathematical assertions becomes, under modal structuralism, realism in truth-value for logical possibility assertions.

But alternatively, to insist that modal primitivism comes to the bare assertion that *something* fills the φ role is potentially inconsistent with nominalism. Without further specification about just what this something is, modal nominalists can hardly be confident

²⁷Thanks to Eric Hiddleston for helping me to appreciate the significance of these questions.

that it is nominalistically acceptable. Consider the collection of all nominalistically acceptable entities in the actual universe. Is it doubtful that this collection possess the requisite structure to fill the φ role. Further, it is implausible that there are enough nominalistically acceptable entities to even serve as *proxies* for all genuinely possible assertions. Some kind of increase in ontology appears required in order to account for every possibility. Thus it is not clear that primitive modality is as ontologically innocent as modal nominalists presume. For instance, Chihara's modal primitives—ways the world might have been—sound much like uninstantiated properties.²⁸ Whether it is possible to provide a nominalistically acceptable theory of modality therefore appears in doubt and remains an open question—one that I cannot settle at the moment.

Independent of the question of whether the view is nominalistically acceptable, it is still worth considering whether modal primitivism is an attractive account of modality. Perhaps modal primitivism presents the most ontologically austere account of modality—perhaps it is committed to the smallest ontology necessary for filling the φ roles.²⁹ The primitivist cannot state exactly what this ontology is, but she can be confident that it falls well short of Lewis's full-blooded possible worlds. Perhaps she can get away with a pure form of structuralism about modality in which she is committed to only to what Shapiro would describe as an *ante rem* possible worlds structure.³⁰ How does the primitivist justify the bare claim that there is something (rather than nothing) that plays the φ role? According to Shalkowski, it is not altogether difficult to justify this claim:

Whether we modalize, then, rests on whether it is conducive to a general framework of understanding. For those who resist modal skepticism the activity of modalizing should be relinquished only at the greatest cost, since logical truth and elementary arithmetic seem to require modality. . . If we disabuse ourselves of the thought that modal knowledge must, somehow, be certain knowledge or that it must, at least, be knowledge more certain than empirical knowledge, then we can allow that primitive modality is required by a comprehensive

²⁸Thanks to Eric Hiddleston for this observation.

²⁹(Gregory 2006) argues that the set-theoretic hierarchy is the least controversial ontological posit that is capable of filling the φ roles.

³⁰(Sider 2002) advances a similar proposal.

and satisfactory theory which enables our understanding of the world and our place in it. (1996, 389)

Shalkowski suggests that there is sufficient holistic evidence for supposing that something plays the φ role.³¹ Of course, it is open to Lewis and Shapiro to defend their reductions on holistic grounds. But the analyses that facilitate these reductions do no extra work justifying modal assertions. And their added metaphysical hypotheses—the existence of possible worlds and the existence of sets, respectively—represent increased risks without any consequent gains. Modal primitivism is weaker, and hence more likely to be true. But crucially, modal primitivism does not sacrifice any modal content that is actually pivotal for justifying modal assertions. Assertions about the existence of the relevant content (for justifying that the truth-conditions for modal assertions obtain) appear to be no easier to justify than modal assertions in the first place.

There are provisional grounds, then, for thinking both that modal primitivism is a defensible position and also that it is preferable to theories that make substantial metaphysical hypothesis about the content of modal assertions. Nevertheless it is not clear that modal primitivism is genuinely nominalistic—primitive modal operators and primitive modal properties must apply to *something*, and there is no guarantee that this something is something a nominalist would be comfortable with. However, I think the modal nominalist can comfortably dismiss Shapiro’s criticism that the modal nominalist’s use of modality presents uniquely challenging metaphysical and epistemological difficulties: Any problem the modal nominalist (*qua* modal primitivist) faces *a propos* of justifying modal assertions is likely to be faced by any theorist interested in producing an account of modality. Crucially, since the set-theoretic reduction does no work justifying modal assertions, Shapiro fails to show that the set-theoretic reductionist is in a superior position with respect to justifying these assertions.

³¹For Shalkowski this comes to the rather imprecise claim that, “some form of modality is a basic, irreducible—primitive—feature of reality” (ibid., 388).

Part 2: Naturalism

Neurath has likened science to a boat which, if we are to rebuild it, we must rebuild plank by plank while staying afloat in it. The philosopher and the scientist are in the same boat.

—W.V. Quine

... if we are sailors rebuilding our ship plank by plank on the open sea, then I know of some cargo we might want to jettison.

—George Boolos

Naturalism in Mathematics

It is often said that the majority of philosophy from antiquity to the present can be described as a sequence of reactions to Plato. On a less grand scale—but with greater conviction—it can be said the last 60 years of metaphysics and logic can be described as a sequence of reactions to Quine. This point holds true for the philosophy of mathematics, perhaps more so than for any other subdiscipline of analytic philosophy. It is impossible to deny Quine’s influence in contemporary philosophy of mathematics. His views on science and ontology pervade the discipline, even if many scholars ultimately reject them. His greatest legacy to the field is arguably his development and endorsement of the philosophical orientation of *naturalism*. Quine offers a picture of philosophical methodology according to which the methods of empirical science can and should restrict philosophy in important ways. Philosophers must abandon the “Cartesian Dream” of lusting after *a priori* certainty in any area of inquiry. Beliefs of any kind are subject to confirmation or disconfirmation on the basis of how well they comport with the theory that does the best job of making sense of one’s sensory experiences. Whether a theory does the “best job” is to be determined by subjecting it to *scientific* criteria of theory selection

(fecundity, familiarity, scope, simplicity, and of course accordance with observation). In sum, Quine calls for the abandonment of inherently *philosophical* standards of warrant and instead defers to *scientific* standards of warrant.

The influence of Quine's naturalism in the philosophy of mathematics is partly responsible for focusing the discipline away from foundational questions and refocusing it on questions about the *practice of professional* mathematics. According to the dominant view, the practice of mathematics is something to be *explained* as opposed to something in need of *justification*. Thus the task of philosophy of mathematics is to explain the proper role of mathematics as a part of the overall scientific enterprise. This could involve determining what conceptual, logical, and ontological resources mathematics requires. To use one of Quine's metaphors, the philosopher of mathematics is adrift on the Ship of Neurath, and must determine what kind of a hull enables her to erect the mast of mathematics.

For better or worse, Quine viewed mathematics as a mere handmaiden of empirical science. This is a view that many naturalists in the philosophy of mathematics reject. Contra Quine, mathematics is a successful, freestanding discipline, and does not require the approval and use of the empirical-scientific community to count as an intellectually worthy pursuit. Nevertheless, as far as influential, well-developed naturalistic accounts of mathematics go, Quine's was to stand alone for some 40 years. Only in the past few decades have new views come out which provide an overall picture of the relationship between mathematics and the scientific community at-large. In this section of the dissertation, I reflect upon the challenges that the two most developed post-Quinean naturalists pose to modal nominalism: John Burgess's naturalistic objections to modal nominalism and some objections derived from Penelope Maddy's "Second Philosophy." Burgess's views are taken up in chapter four; Maddy's are discussed in chapter five. In essence, Burgess argues that modal nominalism works *against* science and mathematics, whereas Maddy can be understood as arguing that modal nominalism does not work *with* science and mathematics. My principal ambition is to show that modal nominalism is *consistent* with

the naturalist orientation in philosophy in the sense that it does not produce any kind of conflict with the practice of mathematics.

Chapter 4

Reflections on Burgess

4.1 Introduction

In this chapter I examine the considerations that have motivated John Burgess to reject nominalist and modal nominalist accounts of mathematics.¹ Burgess has on numerous occasions advanced the complaint that nominalist theories of mathematics, including the modal nominalist theories defended in this dissertation, are unscientific, which he takes to be a reason to believe that nominalism (modal or otherwise) and naturalism are incompatible doctrines. Although never explicitly laid out in his work, his various criticisms of nominalist reinterpretations of mathematics fit a pattern of reasoning that I call Burgess's "Master Argument" against nominalism. Note that Burgess's arguments tend to be targeted at nominalist theories of mathematics in general, and not solely at those theories that fall under the modal nominalist heading. Further, I think it is possible to respond to Burgess's arguments on equally general terms. Thus, the defense I offer for modal nominalism can be appropriated by other kinds of nominalist theories of mathematics. For these reasons I will only use the term 'modal nominalism' when do so is dialectically important.

In section two I provide an exposition of the Master Argument and of how it arises out of Burgess's various criticisms of nominalism. The basic idea is that there seems to be no direct scientific evidence in support of the various nominalist accounts of mathematics,

¹Most of Burgess's positive views about science and scientific methodology appear in work he has co-authored with Gideon Rosen. In the citations below I have taken pains to indicate when the cited text is Burgess's alone, and when the cited text is a joint work with Rosen. I do not attach much importance to whether I am successful in describing the views of Burgess or whether I only succeed in characterizing a view he and Rosen have in common. The point remains that the corpus of Burgess's work (including his collaborations with Rosen) contains sustained criticisms of nominalism and modal nominalism that deserve examination. Nevertheless I should call the reader's attention to the fact that Burgess has indicated in print where his views depart from Rosen's (in his introduction to (Burgess 2008a, 3-5)). As best as I can tell, none of the views I attribute to him are listed among these departures.

including the modal nominalist accounts I aim to defend. This allegedly suffices to demonstrate that nominalism, including modal nominalism, is *unscientific*, and hence must be rejected by naturalists.

Section three examines several of the extant responses to Burgess's criticism of nominalism. I consider two replies due to Charles Chihara. One of these replies holds that there in fact does exist scientific and mathematical evidence in support of Constructibility Theory. I argue that the kind of evidence Chihara provides should be largely unmoving to Burgess's naturalist. The second of these replies holds that withholding belief in the truth *simpliciter* of mathematical theories is consistent with scientific and mathematical methodology. I endorse this reply, later funneling it into an *ad hominem* against Burgess in the closing section. The most popular response to the various instances of Burgess's Master Argument hold that each instance targets a characterization of nominalism that no nominalist has ever endorsed. That is, nominalists have argued that all of the examples of the Master Argument that Burgess has *actually* presented are unsound in that they depend on mischaracterizations of the rationale behind nominalist philosophies of mathematics. I argue that this reply, which I call the "False Dilemma" reply, is ultimately insufficient, because Burgess can adopt the following response: First, the nominalist must supplant Burgess's mischaracterizations with clear and identifiable aims for her account of mathematics. Second, she must show that these clear and identifiable aims are not open to some further manifestation of Burgess's Master Argument. On behalf of Burgess I argue that even if a nominalist were to be successful in meeting the first condition, she would be unlikely to succeed in meeting the second.

I nevertheless contend that Burgess's criticism is seriously flawed. In section four I argue that the Master Argument depends on an inadequately articulated position on what it takes for a statement to be naturalistically unacceptable. Burgess's evidence that nominalism is unscientific amounts to little more than promulgating his suspicions that no scientific journal would ever publish a work on nominalism. His use of expressions such

as ‘ p is unscientific’ is equivocal between ‘ p is not endorsed by the scientific community or by scientific methodology,’ and ‘ $\neg p$ is endorsed by the scientific community or by scientific methodology.’ I argue that he succeeds only in showing that nominalism is unscientific in the former sense, and that the naturalist has no *prima facie* reason to reject claims that are unscientific only in this sense. Thus, Burgess does not show that nominalism, including modal nominalism, is incompatible with his naturalism.

I close the chapter in section five with an *ad hominem* against Burgess’s own favored view, moderate realism. The moderate realist is a platonist who adopts a literalist reading of mathematical language. Since no scientific journal would ever publish a work on platonism, Burgess’s moderate realism is open to the same kind of criticism he levies against nominalism. Hence, if my objections from section four are erroneous and nominalism is indeed unscientific (in the damaging sense of the term), then moderate platonism is similarly unscientific (also in the damaging sense of the term). A key observation here is that mathematical practice is largely indifferent to matters of interpretation—ontological presuppositions are not built into the discipline.

Before beginning in earnest, a brief recapitulation is in order of the modal nominalist strategies.² In general terms it is useful to distinguish between two sorts of projects—reinterpretative projects, on the one hand, and reconstructionist projects on the other. Reinterpretative projects involve constructing novel interpretations of mathematical languages. Thus reinterpretations constitute a revision to the *semantics* of mathematical languages, and need not alter the surface grammar of mathematical assertions. Reconstructionist projects advocate the ground-up recreation of mathematical theories using nominalistically kosher primitives. Such projects revise the surface syntax of mathematical assertions and so offer a more dramatic contrast to standard mathematical languages. An important point to recognize is that advocates of either sort of project are not committed to advertising reinterpretation or reconstruction as kinds of activities in which mathemati-

²See the first chapter for a more thorough discussion.

cians must engage. Both can be used as tools for advancing the *philosophical* understanding of mathematics. Burgess is not always attentive to these distinctions; I implore the reader to keep them in mind.

Chihara's Constructibility Theory replaces existence assertions in mathematical languages with assertions about the constructibility of open-sentence tokens of various sorts, his goal being to capture a nominalistic type-theoretic framework. Constructibility Theory is *not* offered as an account of the meaning of ordinary mathematical assertions, but instead as a device for showing that it is *possible* to engage in mathematical reasoning without quantifying over mathematical objects. This involves the (at least in principle) *reconstruction* (or recreation) of the relevant bits of mathematics in Constructibility Theory. Nevertheless Chihara does not advocate a revision to the practice of mathematics. Constructibility Theory is designed principally to rebut the various indispensability arguments.

Hellman's Modal Structuralism constitutes a nominalized version of mathematical structuralism that postulates the primitive logical possibility of the existence of models of mathematical theories (as opposed to the *actual* existence of such models), in addition to the necessitation of conditionals of the form $AX \rightarrow p$, where p is a theorem of the theory characterized by axioms AX . Hellman advocates Modal Structuralism as an attractive *interpretation* of mathematical languages, that is, as a novel reading of ordinary mathematical language fit to compete with the literalist or realist reading favored by platonists. As in Chihara's case, Hellman does not advocate any revision to the practice of mathematics.

Field's fictionalist account of mathematics holds that mathematical language is to be interpreted literally. However, he does not assume that any mathematical objects exist, and hence regards all mathematical existence assertions as false. Field maintains that mathematical knowledge is a genus of logical knowledge that comes in two basic species: knowledge of the consistency of mathematical theories (where consistency is taken to be a primitive logical notion) and knowledge of the consequences of mathematical theories. Neither reinterpretive nor reconstructive maneuvers are implemented in Field's account

of mathematics.³ Consequently, Field does not advocate a revision to the practice of mathematics.

Each of these philosophies of mathematics purport to advance the *philosophical* understanding of mathematics in various ways. Chihara, by showing that it is possible to engage in mathematical reasoning without quantifying over mathematical objects. Hellman, by showing that the platonist's interpretation of mathematical language can be dispensed with in favor of a modal-structural interpretation. And Field, by showing that mathematical theories need not be thought of as expressing truths about mathematical objects. How are these projects to be evaluated? Burgess's answer is that each view must answer to the evidential standards of natural science.

4.2 The Master Argument Against Nominalism

Burgess's criticism of nominalism is intended to unveil nominalistic philosophies of mathematics as unscientific, and hence as inconsistent with the naturalistic outlook in philosophy. The general argumentative strategy he uses, which I am here calling his "Master Argument" against nominalism, works in the following way. First, Burgess advances a thesis about the motivation for nominalist reinterpretation. Next, he offers evidence that the cited motivation is not captured by any evident principle of scientific methodology. From this he infers that the nominalist's *credo* is un- or anti-scientific. In Burgess's eyes this suffices to expose nominalism as inconsistent with naturalism, and in turn, betrays nominalism as a misguided philosophical position.

Burgess is a scientific naturalist, i.e., he believes that natural science is the only reliable tool for obtaining knowledge about the world, and that matters of science are only to be criticized from *within*. Thus he denies that the provenance of philosophy qualifies it as a provincial authority when it comes to matters of scientific importance. But 'naturalism' is a term much bandied about in philosophy; nearly every philosopher that calls herself a

³Field champions a reconstructionist program in *physical* theory, to be sure, but he does not do this in his account of pure mathematics.

'naturalist' seems to mean something different by the term. Burgess's overarching goal in criticizing nominalism is to show that nominalism is untenable from the naturalist orientation—that is to say, he hopes to show that the combination of nominalism and naturalism is untenable on *his* understanding of these views. Although his naturalism is broadly Quinean in outline, he departs from Quine over the status of pure mathematics. For Quine, pure mathematics is a form of intellectual recreation. In contrast, Burgess (along with Maddy) regards mathematics as an important, freestanding discipline, one that qualifies as an important component of the naturalist's "web of belief" independently of any applications it has to other parts of the web. Burgess's work constitutes one of the more serious and developed attempts to answer philosophical questions about mathematics using what are allegedly the methodological principles of natural science and mathematics. Ultimately I think he goes too far in this regard, for he tries to extract from the practice of mathematics what is not there to be extracted in the first place—answers to philosophical existence questions about mathematical objects. This is a close-cousin of the problem I raised in the second chapter against Stewart Shapiro's attempts to justify the existence of *ante rem* structures (by his appeal to some set-theoretic existence principles that he claims underlie mathematical practice). I believe Burgess's criticism of nominalism is instructive because it helps the naturalist and nominalist alike to understand to what extent the methodological principles of natural science can help answer philosophical questions. Moreover, it provides an interesting contrast to another well-known post-Quinean naturalist, Penelope Maddy, whose views I examine in the next chapter.

4.2.1 Burgess's Naturalism

Burgess's perspective on naturalism is scattered throughout various papers spanning from the early 1980's to the late 2000's. What he has to say about the naturalist position occurs almost exclusively in papers that are primarily criticisms of nominalism. In this section I undertake the project of reconstructing Burgess's position on matters, using earlier and later sources to illuminate one another.

In an early paper outlining his criticism of nominalism, he proffers the following as evidence *for* platonism, and also as evidence *against* nominalism,

... science *could* be done without numbers. I maintain, however, that science at present *is* done with numbers, and that there is no scientific reason why in future science *should* be done without them. (Burgess 2008b, 34)

Interpreted as an argument against nominalism, the idea is that the use of mathematics in the sciences licenses accepting the existence of mathematical objects. I can imagine a nominalist offering the following reply: "It is just question-begging to assert that science is in fact done with *numbers*. I grant that science is done with *mathematics*, and there is likely to never be any *scientific* evidence that science should be done without it, but the conclusion does not follow unless our best understanding of the practice of mathematics requires the postulation of mathematical objects." However, it is hard to imagine that Burgess would find this reply troubling; surely the practice of mathematics abounds in existence claims, allowing Burgess to making the following riposte:

The nominalist might be able to show that mathematics *could* be done without mathematical objects. I maintain, however, that mathematics at present *is* done with mathematical objects, and that there is no mathematical reason why in future mathematics *should* be done without them.

So the whole problem comes back. Burgess is essentially eliciting a challenge to the naturalist who also wants to be a nominalist: she must provide scientifically acceptable reasons for abjuring mathematical objects, or drop her nominalist sympathies.

But what does the naturalist position come to? Writing with Gideon Rosen, naturalism is introduced as a view that is committed

... at most to the comparatively modest proposition that when science speaks with a firm and unified voice, the philosopher is either obliged to accept its conclusions or to offer what are recognizably scientific reasons for resisting them. (Burgess and Rosen 1997, 65)

A question is immediately raised concerning the content of this characterization of naturalism. What interpretation is to be given to the notion that "science speaks with a firm

and unified voice”? Surely Burgess is not suggesting that the scientific community gathers together (or has ever gathered together) to make broad, univocally endorsed pronouncements about what claims are acceptable and what claim are not acceptable. But then what is the source of warrant in attributing to some particular claim p the universal approbation of the scientific community? Burgess suggests that matters are to be illuminated by examining the history and practice of science. His first datum is that the evidential standards of science are the best available and that, “there is no philosophical argument powerful enough to override or overrule... scientific standards of acceptability...” (Burgess and Rosen 2005, 517). On this picture, the status of a particular claim p in relation to the scientific community is to be determined using—what else?—scientific standards of warrant. But what are the scientific standards of warrant? Burgess’s answer is that these standards are captured by the methodological principles of the scientific community.⁴

Burgess is reluctant to engage in descriptive methodology of science; he views this as a job for scientists and historians of science—not philosophers.⁵ With Rosen he remarks that, “ultimately the judgment on the scientific merits of a theory must be made by the scientific community” (Burgess and Rosen 1997, 206). Nevertheless, “what mathematical physicists must judge in the long run, naturalized epistemologists may consider in the short run” (Burgess 1990, 8). Burgess and Rosen enumerate a list of methodological principles guiding theory choice very much reminiscent of Quine’s criteria of theory selection,

- (i) correctness and accuracy of observable predictions
- (ii) precision of those predictions and breadth of the range of phenomena for which such predictions are forthcoming, or more generally, of interesting questions for which answers are forthcoming
- (iii) internal rigor and consistency or coherence
- (iv) minimality or economy of assumptions in various respects
- (v) consistency or coherence with familiar, established theories, or where these must be amended, minimality of the amendment

⁴It should be noted that, for Burgess, there is not much difference, if any, between acceptability and belief. If scientists accept p for scientifically acceptable reasons, then naturalists are warranted in believing p .

⁵This disposition is the theme of two of Burgess’s papers, (Burgess 1990) and (Burgess 1998).

- (vi) perspicuity of the basic notions and assumptions
- (vii) fruitfulness, or capacity for being extended to answer new questions
(Burgess and Rosen 1997, 209)⁶

Assuming that these methodological criteria capture a univocally endorsed account of scientific warrant, it is possible to pronounce on the status of the nominalistic reinterpretations and reconstructions of mathematics. This judgment, of course, is to be made on the basis of how well nominalist theories fare at meeting (i)-(vii). For Burgess this examination is not exhausted by the simple-minded observation that nominalistic reinterpretations fare well on (iv)—economy of assumptions. Rather, nominalistic reinterpretations—just like any other theory up for adoption—must be judged on how satisfactorily they cohere with *all* of these important methodological principles. And what such satisfactory coherence consists in is in manifesting (i)-(vii) *in proportion* to their importance to the scientific community (as can be gleaned from the past and current decisions of scientists). Nominalists, including modal nominalists, ought to be worried because it appears as though their views give, “far more weight to factor (iv)... than do working scientists” (Burgess and Rosen 1997, 210).

Should one assume with Burgess that these methodological criteria capture the universally endorsed standards of scientific warrant?⁷ It is far from clear. Burgess must be granted two additional assumptions. The first assumption is that the scientific community uniformly accepts (i)-(vii) as capturing the evidential standards of science. The second assumption is that scientists consistently grant (i)-(vii) the same relative weight when engaged in the process of theory selection. If the first assumption is questionable, then it is hardly reasonable to suppose that one should ever expect to see scientists come to universal agreement.⁸ To be sure, the scientific community appears to agree on the reliability of low-level tools like statistical analysis and double-blind experiments. Perhaps

⁶Quine’s criteria are given in (Quine 1976, 247).

⁷Thanks to Susan Vineberg for suggesting the following points.

⁸While discussing an earlier version of this chapter, the cosmologist David Garfinkle remarked, “I’ve never seen that happen!”

this is evidence that scientists also agree on higher-level general methodological principles. My concern is that Burgess has not offered any *evidence* for the claim that scientists are in universal agreement on the status of (i)-(vii). Similarly, Burgess offers no evidence which would compel someone to believe that scientists are consistent in the way that they weight (i)-(vii) when adjudicating between rival theories. But absent this evidence it is unreasonable to be confident that the assessment of nominalist theories would betray any inconsistency with the diverse ways in which scientists might weight (i)-(vii).

Like Burgess I am hesitant to engage in the descriptive methodology of science, and so I do not presume to offer the final say on matters. Burgess's account of scientific methodology raises some unanswered questions, and some skeptics might hope to answer them and so castigate his naturalism from the very start. One line of reasoning to which I am sympathetic holds that science is not the coherent, unified community that Burgess apparently takes it to be (along with most Quineans, I might add), and that the goals and methods of science are as numerous and diverse as are the many scientific disciplines. Still, many in the philosophical community are attracted, via Quine, to the idea that something like (i)-(vii) describe some of the important methodological tools of working scientists. And so, for the sake of discussion, I am willing to grant that Burgess's naturalism is coherent and to continue on in my investigation of whether he is justified even in believing that his naturalism conflicts with nominalism (and whether he is justified in believing that his platonism *is consistent* with his naturalism).

Returning to the dialectic, there is good reason to ask what Burgess's naturalism has to say about mathematical objects; science, at least on the surface, is tasked with investigating the *concrete* world, and it would indeed be surprising if it ended up implying the existence of *abstract* mathematical objects. Doubly surprising, too, as Burgess claims that science is an outgrowth of common sense (Burgess 2008b, 32). Surely the claim that there exist non-spatiotemporal objects is no teaching of common sense. Or is it? According to Burgess, mathematics is in no way to be set apart from science; mathematics is a part of science-

proper.⁹ Thus mathematics and its existence claims are also outgrowths of common sense. And since a naturalist is someone who is, “prepared to reaffirm while doing philosophy whatever was affirmed while doing science” (Burgess 2008a, 2), naturalists are committed to mathematical objects. On this score, Burgess says that he should be taken to

... affirm only (what even some self-described anti-realists concede) that the existence of [mathematical] objects and obtaining of [mathematical] truths is an implication or presupposition of science and scientifically informed common sense, while denying that philosophy has any access to exterior, ulterior, and superior sources of knowledge from which to “correct” science and scientifically informed common sense. (ibid.)

Nominalism, as Burgess understands the view, involves a prejudice for ontological economy that is rooted exclusively in philosophy. Thus nominalism requires “taking back” while doing philosophy the existence claims one makes while doing science, *viz.*, while doing the science of mathematics. Such a prejudice is just the sort of exterior (or ulterior, or superior) philosophizing that is disallowed under naturalism.

Before going on to discuss Burgess’s Master Argument in detail, three concerns are worth raising. First, one could accuse Burgess of not being precise about the relationship between the attitude scientists adopt toward a statement p and the subsequent attitude naturalists must adopt toward p . Suppose that it is possible to make clear the conditions under which “science speaks with a firm and unified voice.” This provides an account of when a naturalist is *compelled* to accept a statement p . But nothing Burgess says informs the discussion when attention shifts from deciding which beliefs are naturalistically *required* to deciding which beliefs are naturalistically *acceptable, though not required*. What is a naturalist to say about a statement p that receives from the scientific community neither universal approbation nor universal disapprobation? Or, better yet, what is a naturalist to say about a statement p that invites *no sentiments at all* from the scientific community? Suppose a small group of scientists decry mathematics as just a useful tool, not to be taken

⁹Burgess and Rosen are quite clear in rejecting any view that “expels” mathematics from the “circle of sciences” (Burgess and Rosen 1997, 211).

seriously; suppose a small group of mathematicians think that mathematics is just an exercise in drawing derivations in various formal systems, the axioms of which need not be taken to assert truths—what is the naturalist to say, then? Can she abjure mathematical objects?

Second, given the obvious differences between the practice of mathematics and the practice of empirical science, one might question Burgess's use of the methodological principles of empirical science in pronouncing on mathematical matters. If the methodology of mathematics is not sufficiently similar to that of empirical science, then whether nominalism strikes the right balance between (i)-(vii) is immaterial; what matters for nominalist philosophies of *mathematics* is how well they fare when confronted with the methods of *mathematics*. Of course, this is only good news for the nominalist if it turns out that the methodology of mathematics involves sympathy for ontological economy, and on Burgess's view this result seems unlikely.¹⁰

Finally, one gets the feeling that somehow platonism has already been smuggled in under the table on Burgess's picture. If some kind of scientifically acceptable evidence is required for *eschewing* commitment to mathematical objects, then by parity of reasoning it would appear as though some kind of scientifically acceptable evidence is required for *taking on* commitment to mathematical objects. Saying that such commitments are "presupposed" in science and scientifically informed commonsense sounds like cheating. For what is the status of this presupposition? Is it subject to acceptance or rejection on the basis of the attitudes the scientific community adopts toward it? Burgess appears to conflate several distinct tasks of descriptive methodology: Providing an account of the *evidential* standards of science, determining what *presuppositions* are made by scientists,

¹⁰A brief note on the second concern: Most of Burgess's remarks on methodology are targeted at nominalists who seek to *replace* current *scientific* theories with *nominalized* theories. Thus the charge that such views are to be judged by the methodological standards of empirical science is not entirely out of place. However, none of the modal nominalists that I defend have ever endorsed such a view. Thus *my* concern stands when one is interested in discussing the views that nominalists have actually presented. I will have an opportunity to revisit these issues in the next chapter while discussing Maddy's views on the methodology of mathematics.

and addressing what counts as “common sense” to the scientific community. Burgess chides nominalists for doubting what is to him a basic presupposition of science—the existence of mathematical objects. This presupposition is what appears to be doing most of the work in Burgess’s criticisms of nominalism, and therein lies the irony. If this presupposition is not subject to the evidential standards of science (presumably what (i)–(vii) are intended to capture), then it hardly makes sense to complain that when nominalists question this presupposition they are being un- or anti-scientific. On the other hand, if this presupposition *is* subject to the evidential standards of science, what makes Burgess so confident that it will pass muster? These concerns will be taken up shortly, but now I would finally like to begin reconstructing Burgess’s arguments against nominalism.

4.2.2 The Arguments¹¹

Burgess does not claim to provide a single argument against nominalistic reinterpretations of mathematics. What he does instead is advance a family of arguments against the various nominalist proposals. I begin by articulating the arguments that Burgess has actually given so that I may show that these arguments involve a common inference. From these arguments I then abstract a generalized argument form that I am here calling Burgess’s “Master Argument” against nominalism. My ultimate aim is to call into question the common inference on which all of Burgess’s arguments—including the Master Argument—depend.

In an early paper Burgess attempts a taxonomy of nominalist positions in the philosophy of mathematics (2008b). Burgess allows the nominalist three possible positions. First, the nominalist could have her sights on *replacing* existing scientific theories with new

¹¹A note on terminology: (Paseau 2005) correctly points out that the terms ‘reinterpretation’ and ‘reconstruction’ refer to different activities. Reinterpretation is, “interpretation different from the standard interpretation, which is the interpretation of mathematics accepted by mathematicians when doing mathematics” (ibid., 378). Reconstructions, “change the actual mathematics as opposed to its interpretation” (ibid.). Nevertheless, Burgess and Rosen, along with most commentators, use the terms ‘reinterpretation’ and ‘reconstruction’ interchangeably as blanket terms encompassing both reinterpretation and reconstruction in Paseau’s senses. Below I shall attempt to be clear about which criticism attaches to which sort of activity; the reader should keep in mind that doing this, in certain circumstances, involves (charitably) distorting the actual arguments Burgess has given.

nominalistic theories that do not quantify over mathematical objects. Such a nominalist is called a *revolutionary* reconstructionist. Second, the nominalist could have her sights on uncovering the ultimate *meaning* of mathematical assertions. A nominalist who alleges that, when properly understood, mathematical assertions do not quantify over mathematical objects, is called a *hermeneutic* reinterpretationist. Finally, the nominalist could be advancing the view that mathematical and scientific theories are just useful fictions. Such a nominalist is called an *instrumentalist*.¹²

4.2.2.1 Against Hermeneutic Reinterpretation

The thesis of hermeneutic reinterpretation is that,

1. Nominalistic interpretations of mathematics unveil the true meaning or “deep structure” of mathematical assertions.

But for Burgess, “as a thesis about the language of science, hermeneutic nominalism is...subject to evaluation by the science of language, linguistics” (ibid., 36). And although he is no linguist, he is happy to announce that such a view is, “seldom entertained by students of language,” and assures the reader that the hermeneutic thesis lacks the kind of evidence that would convince linguists of its truth (ibid). Thus it is clear Burgess believes that,

2. There is no relevant (linguistic) scientific evidence supporting hermeneutic nominalism.

In defending her views the hermeneutic reinterpretationist is thereby committed to defending an “implausible hypothesis in linguistics” (ibid., 41). It should be plain from the

¹²Burgess is rather curt in dismissing instrumentalism, claiming that, “[t]he philosopher who begins by rejecting theoretical physics as fiction will find no logical place to stop, and in the end will be unable, without further inconsistency and self-contradiction, to accept commonsense belief as fact” (ibid., 32). As none of the major nominalistic projects advance the sort of instrumentalism that cascades down this slippery slope, I will pass over this argument just as curtly as it was introduced. Burgess’s criticism of instrumentalism should be read more as a critique of Bas van Fraassen’s views on the philosophy of science than as an objection to any particular nominalist philosophy of mathematics. Some of Burgess’s mature views on mathematical instrumentalism will be touched upon in the closing discussion of Burgess’s platonism. To anticipate as-yet undefined terminology, Burgess now recognizes a hermeneutic version of instrumentalism known as “attitude-hermeneuticism.” More on this below.

previous discussion of Burgess's naturalism that this means that,

3. Hermeneutic nominalism is thereby un- or anti-scientific,

which clearly entails that,

4. Hermeneutic nominalism is unacceptable to someone who adopts the philosophical orientation of naturalism.

The following conclusion is unavoidable,

5. Hermeneutic nominalism and naturalism are inconsistent doctrines.

And that is why, as a naturalist, Burgess rejects hermeneutic nominalism.¹³

4.2.2.2 Against Revolutionary Reconstruction

The thesis of revolutionary reconstruction is that,

6. Nominalized scientific theories ought to be adopted in favor of their platonistic counterparts.

As I have already remarked, Burgess (with Rosen) worries that defending this thesis places too much importance on the value of ontological economy (Burgess and Rosen 1997, 210). For instance, there is good reason for supposing that a paper on nominalistic physics would never be accepted by a physics journal (Burgess 2008b, 37).¹⁴ Moreover, the adoption of nominalized theories would likely hinder the progress of science. There would be great costs associated with revising scientific curricula. New breakthroughs would be less frequent due to the adoption of more complicated theoretical apparati (Burgess 2008b, 38-9). Thus Burgess seems confident that,

¹³Burgess no longer believes that the argument adduced in this section presents the most powerful case against hermeneutic nominalism. His current view is that hermeneutic reinterpretations are to be understood as *paraphrases* of ordinary mathematical language and that paraphrase is not an acceptable vehicle for eschewing commitment to mathematical objects. Since I have discussed these matters already in the second chapter of this dissertation, I will not pause to rehearse and respond to Burgess's ulterior case against hermeneutic reinterpretation.

¹⁴This point is reiterated with Rosen in (Burgess and Rosen 1997, 210).

7. There is no relevant scientific evidence supporting revolutionary nominalism.

This means that,

8. Revolutionary nominalism is thereby un- or anti-scientific.

And the rest of the argument proceeds as in the case of hermeneutic nominalism:

9. Revolutionary nominalism is unacceptable to someone who adopts the philosophical orientation of naturalism.

10. Revolutionary nominalism and naturalism are inconsistent doctrines.

And that is why, as a naturalist, Burgess rejects revolutionary nominalism.¹⁵

4.2.2.3 Against Tract Housing

In his most recent collaboration with Rosen, the catch-all categories of “hermeneutic nominalism” and “revolutionary nominalism” are expanded and refined (Burgess and Rosen 2005, 517). The hermeneutic position is split up into *attitude-hermeneutic* nominalism and *content-hermeneutic* nominalism. The revolutionary position is split up into *naturalized revolutionary* nominalism and *alienated revolutionary* nominalism.¹⁶

¹⁵Would Quine follow Burgess here? Arguably not. If revolutionary nominalism turns out to be the “best overall theory” then Quine is obliged to profess nominalism. That adopting revolutionary nominalism might hinder the progress of science speaks against the *plausibility* of regarding revolutionary nominalism as candidate for the “best overall theory.” Whether this is convincing evidence against the *possibility* of revolutionary nominalism turning out to be the “best overall theory” depends on whether Quine would follow Burgess in holding that theories must be judged according to whether they manifest criteria (i)-(vii) in proportion to how scientists wield (i)-(vii). Of course, Quine ultimately came to reject nominalism because he believed that it was not possible *in principle* to eschew commitment to mathematical objects. Quine did not reject nominalism because he believed nominalist physics would be theoretically uneconomical; Quine never required that the practicing scientist actually *use* the best overall theory while doing science. This, then, in another way in which Burgess departs from Quine.

¹⁶This refined terminology comes from Rosen, as Burgess acknowledges in his introduction to (Burgess 2008a, 4). Nevertheless he appears to endorse the terminology and cites Rosen as an influential figure (*ibid.*, 3). He also cites Rosen as “correctly” pointing out that instrumentalism, “itself comes in a revolutionary version (this is the attitude philosophers *ought* to adopt) and a hermeneutic version (this is the attitude commonsense and scientific thinkers *already do* adopt)” (*ibid.*, 4). I will pass over these subtleties as they in no way affect what I have to say in the remaining sections of this chapter. For the record, Burgess indicates that he now associates the hermeneutic position of (Burgess 2008b) with Rosen’s *content-hermeneutic* nominalism, and he also says that hermeneutic instrumentalism should be associated with *attitude-hermeneutic* nominalism (Burgess 2008a, 4). For a defense of revolutionary instrumentalism, see (Daly 2006).

The *content-hermeneutic* position is identical to the position known simply as “hermeneutic nominalism” above; a content-hermeneutic nominalist believes that, deep down, mathematical assertions are not really about mathematical objects. Similarly, the *naturalized-revolutionary* position is identical to the position known simply as “revolutionary nominalism” above; a naturalized-revolutionary nominalist believes that ontological economy is a guiding methodological principle of science. Since I have already given Burgess’s reasons for rejecting these views I will move on and briefly discuss the other two positions. (It is worth noting that some nominalists have described themselves as “revolutionary.” For instance, Mary Leng advocates a view she sometimes calls “revolutionary fictionalism.” But as discussed below, she does not attach the same sense to the term as does Burgess; her revolution is a revolution in the *understanding* of mathematics and not in its practice. Thus, Burgess’s arguments against “revolutionary nominalism” should not be taken to apply to all nominalists that call themselves ‘revolutionaries.’)

Alienated-revolutionary nominalists seek to replace scientific standards of warrant with philosophical standards of warrant. In this way they advocate the abandonment of the naturalistic orientation and argue that the justificatory standards of mathematics, though acceptable to scientists, are not really acceptable after all. Such a view is clearly inconsistent with Burgess’s naturalism. As my interest here is in examining Burgess’s objections to nominalistic views that purport to cohere with the philosophical orientation of naturalism, I will give just the bare outline of what Burgess has to say against the alienated-revolutionary. The alienated-revolutionary nominalist is typically motivated by the apparent epistemological quandary identified in (Benacerraf 1973). Since mathematical objects are acausal, it is unclear how it is possible for human beings to have knowledge about them. Burgess thinks that this concern depends critically on causal theories of knowledge and that there are good reasons for rejecting such theories. Moreover, any puzzle about how mathematicians come to possess justified mathematical beliefs is to be solved by examining the actual means by which mathematicians arrive at their beliefs. Just

as physicists can avail themselves of the *prima facie* reliability of perception (and so can avoid having to refute the external-world skeptic), mathematicians may avail themselves of the *prima facie* reliability of mathematical beliefs (and so can avoid having to refute the mathematical-knowledge skeptic). On Burgess's view, the best hope for the alienated-revolutionary is to achieve a stalemate concerning the ultimate justificatory status of mathematics.¹⁷ It is unclear what sort of evidence would be acceptable to both parties, one way or the other, on the topic of whether mathematics needs a philosophical foundation.

The *attitude-hermeneutic* nominalist argues that the fact that scientists and mathematicians *accept* mathematical assertions should not be taken to imply that scientists and mathematicians *believe* in the face-value truth of these assertions. For instance, a set theorist while on the job may sincerely utter a statement implying the existence of an inaccessible cardinal, but the attitude-hermeneutic nominalist will maintain that, upon reflection, this set theorist may consistently maintain that there are no such things as sets. As before, the question of what to make of this position comes down to whether it is recommended by the available evidence. *Prima facie*, the evidence speaks against the attitude-hermeneuticist:

... when a person understands a sentence *S*, confidently affirms it without qualification and without conscious insincerity, organizes serious activity just as would be done if it were believed, and so on, then we have a *powerful* case for attributing to that person a belief that *S*... (Burgess and Rosen 2005, 526)

Absent a direct disavowal of existence assertions on the part of scientists (including mathematicians—but apparently, not including formalists or Alfred Tarski), the conclusion is drawn that there is not sufficient evidence for recommending the adoption of the attitude-hermeneutic position. It should be clear how to construct an argument against the attitude-hermeneutic position similar to those given in the previous two sections.

¹⁷See (Burgess and Rosen 1997, 26-49) and (Burgess and Rosen 2005, 523).

4.2.2.4 The Master Argument

On the assumption that the above revolutionary and hermeneutic strategies exhaust the viable options for the nominalist, Burgess concludes that nominalism is untenable. It should be obvious, however, that the arguments against both the hermeneutic nominalist and the revolutionary nominalist take the same form. Here are the first two arguments again:

Against (content) hermeneutic nominalism:	Against (naturalized) revolutionary nominalism:
1. Nominalistic interpretations of mathematics unveil the true meaning or “deep structure” of mathematical assertions.	6. Nominalized scientific theories ought to be adopted in favor of their platonistic counterparts.
2. There is no relevant (linguistic) scientific evidence supporting hermeneutic nominalism.	7. There is no relevant scientific evidence supporting revolutionary nominalism.
3. Hermeneutic nominalism is thereby un- or anti-scientific.	8. Revolutionary nominalism is thereby un- or anti-scientific.
4. Hermeneutic nominalism is thus unacceptable to someone who adopts the philosophical orientation of naturalism.	9. Revolutionary nominalism is thus unacceptable to someone who adopts the philosophical orientation of naturalism.
5. Therefore hermeneutic nominalism and naturalism are inconsistent doctrines.	10. Therefore revolutionary nominalism and naturalism are inconsistent doctrines.

There is much these arguments have in common. Each share the same basic structure, and it is this structure that I am here calling Burgess’s “Master Argument” against nominalism. The Master Argument looks like this:

11. Nominalists advance thesis T .
12. There is no scientific evidence in support of T .
13. It follows that T is un- or anti-scientific.
14. Thus T cannot be accepted by a naturalist.
15. Therefore nominalism and naturalism are inconsistent doctrines.¹⁸

In a short while I will make a case for thinking that Burgess's naturalism is incapable of validating two of the Master Argument's important inferences—the inferences from (12) to (13) and from (13) to (14). In order to motivate this response I first examine several of the ways in which nominalists have responded to Burgess's arguments. I then indicate why I think that most of these responses are unsatisfactory.

4.3 Replies in the Literature

Of the three modal nominalists examined in the first chapter, two—Charles Chihara and Geoffrey Hellman—have offered responses to Burgess's criticisms. These responses come in two varieties. First are the replies, stemming primarily from Chihara, to the various claims Burgess has made about scientific methodology and the scientific merits of nominalistic reinterpretations. Second are the replies that Burgess's subdivisions (including Rosen's refinements) mischaracterize the rationale behind nominalist philosophies of mathematics and therefore fail to pronounce on any of the views that philosophers like Chihara and Hellman have actually proposed.¹⁹ Both kinds of responses purport to unveil Burgess's arguments as unsound.

¹⁸Cf. Burgess and Rosen's "schematic argument" for *accepting* the existence of mathematical objects (2005, 516-7).

¹⁹Hartry Field should be included in this list, too. Though often one of Burgess's targets, I am not aware of any place in Field's work where he has bothered to respond to Burgess's arguments. For this reason I give Field's fictionalism very little attention in this chapter.

4.3.1 The Scientific Merits of Nominalistic Reinterpretation

A common point of emphasis in Burgess's writings is that nominalistic reinterpretations and reconstructions of mathematics would not be regarded as progress by scientists (Burgess and Rosen 1997, 210). Thus it would be sophomoric to expect to see a work on nominalism published in the *Physical Review*. Burgess's suggestion seems to be that the cash value of nominalistic reconstruction and reinterpretation is to be determined by practicing scientists.

Chihara notes that most of the nominalistic literature is self-consciously aimed at responding to the Quine/Putnam indispensability arguments. Seen in this context, Chihara asks,

Are empirical scientists or mathematicians best equipped intellectually and by training to deal with the many and complex philosophical issues that such a dispute may involve? Should they be entrusted with the final word on the merits (scientific, logical, and philosophical) of such responses? Would sending papers detailing these reconstructions to physics journals constitute "true tests" of the merits of these works? (2004, 163)

He responds that an editor's decision to reject a paper on nominalism would not so much betray ontological economy as a second-class methodological principle as it would reflect the fact that, "the journal's readership would not be an appropriate audience for a philosophical paper of that sort" (ibid.).²⁰ Chihara's accusation is that Burgess mislocates the significance of nominalistic reinterpretation in viewing it as an attempt at science rather than as an attempt at philosophy. Nevertheless Burgess regards the underlying assumption of the indispensability arguments—that the dispensability *in principle* (as opposed to the dispensability *in practice*) of mathematical entities suffices to demonstrate their non-existence—as a concession to nominalism that he does not feel compelled to

²⁰Alexander Paseau claims that, "the question of an adequate construal of mathematics is not considered by science journals" (2007, 139). If Paseau is right, this is not just bad news for nominalism, it is bad news for mathematics as a whole! I suppose Burgess's case could be improved by shifting the thought experiment from science journals to mathematics journals and pointing to cases of published articles that discuss matters of interpretation. Paseau is skeptical of Burgess's prospects.

make (Burgess and Rosen 1997, 213). For, granting this concession would imply that ontological economy is an overriding principle of scientific methodology, a move that Burgess clearly desires to avoid.

Perhaps in anticipation of this response, Chihara has made efforts to show how his Constructibility Theory *can* produce advances in science and mathematics. He begins with an analogy involving nonstandard analysis, an alternative form of analysis that introduces hyperreal numbers.²¹ Although the nonstandard model of the continuum is thought by some mathematicians to be, “so counter-intuitive as to be repugnant,” it turns out that in nonstandard analysis, “the basic concepts of the calculus can be given simpler and more intuitive definitions” (Chihara 2007, 62).²² Doctoral research carried out by Kathleen Sullivan found that,

[T]here does seem to be considerable evidence to support the thesis that this is indeed a viable alternative approach to teaching calculus. Any fears on the part of a would-be experimenter that students who learn calculus by way of infinitesimals will achieve less mastery of basic skills have surely been allayed. And it even appears highly probable that using the infinitesimal approach will make the calculus course a lot more fun both for the teachers and for students.²³

²¹A number is a hyperreal number if it is either an infinite number or an infinitesimal. An infinite number is one that is greater than any sum of finite numbers. A number is an infinitesimal when it is the multiplicative inverse of an infinite number.

²²Chihara gives the example of continuity. Consider the following definition of continuity from Edward Gaghan’s *Introduction to Analysis* (Pacific Grove, CA: Brooks/Cole Publishing Company, 1998), 83:

Suppose $E \subset R$ and $f : E \mapsto R$. If $x_0 \in E$, then f is *continuous* at x_0 iff for each $\epsilon > 0$, there is a $\delta > 0$ such that if

$$|x - x_0| < \delta, x \in E,$$

then

$$|f(x) - f(x_0)| < \epsilon.$$

Compare this to Robinson’s nonstandard definition given in (Chihara 2007, 63):

A function f is said to be *continuous* at c iff:

- (i) f is defined at c ; and
- (ii) whenever x is infinitely close to c , $f(x)$ is infinitely close to $f(c)$.

I shall leave it to the reader to decide which of these definitions is simpler and more intuitive.

²³“The Teaching of Elementary Calculus Using the Nonstandard Analysis Approach,” *American Mathematical Monthly* 83 (1976), 375. As quoted in (Chihara 2007, 64).

If Sullivan's findings are to be trusted, nonstandard analysis has a pedagogical value over and above any interest mathematicians might have in the subject.

Chihara hypothesizes that Burgess would see things differently if he were to approach the case of nonstandard analysis in the same way that he approaches nominalism. What is nonstandard analysis good for?

Evidently, there are two principal answers that should be considered:

- (A) Nonstandard analysis provides us with a hermeneutical 'analysis of the ordinary meaning of scientific language'...
- (B) It provides us with an alternative version of science, which is better than, and to be preferred to, our present-day versions of science... (Chihara 2007, 64)

Neither (A) nor (B) constitute attractive options. There is inadequate linguistic evidence for the hermeneutic option (A). And the revolutionary option (B) would involve a costly revision of the mathematical curriculum. He then gives the following Burgess-inspired conclusion:

Since anti-nonstandard analysts reject all the hermeneutic and revolutionary claims of the nonstandard analysts, from their viewpoint, *nonstandard analysis is distinct from and inferior to standard analysis*. What is accomplished by producing a series of such distinct and inferior theories? *No advancement of science proper, certainly...* (ibid., 66, Chihara's emphasis)

Chihara intimates that this is a ludicrous way of looking at nonstandard analysis because doing so fails to capture the pedagogical value it possesses, to say nothing about the actual uses of nonstandard analysis in applications.²⁴ His suggestion is that assessing the value of nominalistic reconstructions in this way is similarly ludicrous.

Before responding to this analogy I would like first to point out that Burgess would very likely agree with Chihara on the various merits of nonstandard analysis. Indeed, papers on nonstandard analysis are routinely published in mathematics journals—what

²⁴For a brief list see (ibid., 67).

better evidence could Burgess ask for? I make this point purely as a clarification; I do not imagine Chihara was attempting to divine Burgess's views on nonstandard analysis.

If this analogy is supposed to show that it is possible for Chihara's Constructibility Theory to contribute to one's understanding of mathematics and science, then it is important for him to show that the case of nonstandard analysis is actually analogous to the case of Constructibility Theory. But I cannot help thinking of all the important—that is, important from *Burgess's* perspective—disanalogies between the case of nonstandard analysis and Chihara's Constructibility Theory. To begin with, the basic concepts of nonstandard analysis can be clearly and succinctly articulated.²⁵ The basic concepts of Constructibility Theory include inadequately studied modal notions.²⁶ Second, nonstandard analysis was formulated by a mathematician—Abraham Robinson, whereas Constructibility Theory was created by a philosopher. Although of freestanding significance only to those adopting a crass scientism, this nevertheless is *prima facie* evidence that the theories were constructed with entirely different motives, serve entirely different goals, and invoke entirely different methodological principles and standards of evidence. Or, at the very least, it is evidence that nonstandard analysis and Constructibility Theory stress different aspects of the goals of the scientific community. In either case the burden is on Chihara to demonstrate that his project is one which complements the scientific enterprise, rather than one which runs contrary to it. In line with this point is the observation available to Burgess that nonstandard analysis contributes to the education of mathematicians and serves important roles in scientific applications. One would therefore expect to see Chihara point to cases where Constructibility Theory has contributed to the education of mathematicians and to cases where the theory has played an important role in scientific applications of mathematics. Unfortunately I can find no example of Constructibility Theory being used in these ways,

²⁵History aside. Infinitesimals were quite controversial postulations during the development of the fundamental ideas of calculus.

²⁶This may not be a relevant point of disanalogy if the conclusion is supposed to be that Constructibility Theory is unscientific. To show this one need only point to the sundry of poorly articulated concepts in particle physics, quantum mechanics, and many other scientific disciplines.

aside from Chihara's hopeful (though yet unconfirmed!) speculation that some day it may help in

... getting one to have doubts about a dominant way of understanding something [that] can free the mind to explore other alternatives—even lead to the discovery of a new paradigm—opening the way to any number of important insights (just as erasing an entry in a partially completed crossword puzzle can open up a flood of new possibilities). (ibid., 69)

At best, Chihara has shown that his view has the ability to address many of the *philosophical* problems one encounters in the philosophy of mathematics—problems that Burgess, for better or worse, is not much troubled by. To be sure, one of these philosophical problems is the “problem of application,” that is, the problem of how to explain why mathematical reasoning is so successful in scientific applications. Chihara goes to great lengths to show how his view can *recover* such applications (2004, ch. 9). But on my understanding of Burgess, seeking after the *recovery* of mathematical applications is a fool's errand. Mathematics is applied successfully in science, end of story. For Burgess the question is not, “Can this view of mathematics *recover* applications of mathematics?,” but instead, “Can this view of mathematics be *applied* profitably *in* science?” That Chihara can show how Constructibility Theory handles applications is evidence that the theory can, in principle, actually be put to use in applications, but it is difficult to imagine that it would provide a *practical* or *economical* framework in which to carry out scientific reasoning.

If the remarks of this section are interpreted as a case that Constructibility Theory can, by Burgess's standards, contribute to the advancement of science, my judgment is that Chihara is unsuccessful. However, these remarks might suggest an alternative conclusion, one which holds that Burgess misplaces the significance of what it means to say that a philosophical position succeeds only in advancing the philosophical debate. I will take up this clue after discussing two other responses to the Master Argument.

4.3.2 Scientific Merits Redux: Chihara and the Attitude-Hermeneuticist

The attitude-hermeneuticist contends that scientists and mathematicians can be consistently described as not fully believing in mathematical existence assertions despite the fact that, according to Burgess, the same scientists and mathematicians find the same assertions acceptable by mathematico-scientific standards. Burgess replies that scientists and mathematicians accept existence assertions without any conscious reservations and that the default presumption in this situation is that scientists and mathematicians *believe* these existence assertions—a presumption that nominalists have not defeated.

Chihara does not deny that mathematicians both accept *and* believe assertions such as

16. The null set exists.

Instead he argues that Burgess's criticism of the attitude-hermeneuticist fails to account for the context in which mathematicians accept existence assertions like (16). Such assertions imply the existence of mathematical objects *only if* they are, "true statement[s] about what exists in the actual world" (Chihara 2006, 323). But assertions like (16) are never accepted in a vacuum; (16) is a theorem of Zermelo Fränkel set theory. Thus,

Just because a mathematician assents verbally to (16) 'without conscious silent reservations', one cannot conclude that she believes (16) to be actually true. Her assent could consist merely in the acceptance that (16) is true in a relative sense, *e.g.*, is truly a theorem of ZF or is true for the iterative concept. So much more would have to be argued to yield the conclusion that almost all mathematicians and scientists accept (16) as actually true. (*ibid.*, 325)²⁷

Notice that this response does not rest on any novel hermeneutical claim about assertions like (16); that (16) is a theorem of ZF is an uncontroversial truth Chihara takes at face-value. Resolving whether mathematicians accept the axioms of ZF as truths of the world—as opposed to, say, assumptions that yield fruitful research—is a further matter.²⁸ Burgess

²⁷I have made inessential changes in numbering.

²⁸Cf. David Corfield's observation that, "were we to ask a mathematician today whether he considered a system of axioms to be true, he would most probably tell us that he thought it had interesting models or that it described an important construction" (2003, 182). Also, cf. Maddy's set-theoretic naturalism examined in the next chapter.

floats the reply that the axioms of well-studied mathematical theories enjoy an immunity from skepticism that is analogous to the justificatory status of perceptual judgments; “perceptual judgments are justified by default, innocent until proven guilty,” and so mathematical theories are justified by default, innocent until proven guilty (Burgess and Rosen 2005, 523). If this remark is on point, it serves only as an attempt to shift the burden of proof and fails to provide any explanation for *why* mathematical theories occupy a privileged justificatory status and why this status conveys mathematical theories as true *simpliciter*.

Chihara’s intent throughout is to undermine the claim that there is no scientific evidence on record for thinking that scientists and mathematicians adopt an attitude other than belief in existence assertions as truths about the world. What evidence does he offer for thinking that scientists may adopt an attitude other than belief on matters?

I have been struck by the fact that, in discussing the nominalism-Platonism dispute with non-philosophers, the scientists among the discussants are frequently surprised, if not downright amazed, to learn that there are many eminent contemporary mathematicians and logicians who maintain not only that the axioms of mathematical theories such as set theory and number theory are genuine assertions about the real world, but also that there exist in the real world such things as sets, functions, numbers, spaces, and the like. The impression I have gotten, as a result of such discussions, is that it had never occurred to these scientists that mathematical axioms—especially the existential ones—can be reasonably understood to have such metaphysical implications. (Chihara 2004, 289, emphasis added)

If these anecdotal remarks are indicative of the general attitude that the scientific community espouses toward mathematics, then there is in fact good evidence that scientists believe that mathematical theories are not ultimately truths about the real world, vitiating Burgess’s claim that the existence of mathematical objects is a “presupposition” of science.²⁹ This unveils the second premise of the attitude-hermeneutic instance of the

²⁹David Liggins reaches a related conclusion in (Liggins 2007). Liggins understands Burgess and Rosen as aiming to show that, “philosophers are *required* to believe those existence theorems which are acceptable by mathematical and scientific standards” (ibid., 107). He responds that, “if there actually have been competent mathematicians who did not accept the existence theorems, this tends to undermine the idea that accepting

Master Argument (that there is no scientific evidence in favor of adopting the thesis of attitude-hermeneuticism) as false. As well, it speaks against the idea that economy of mathematical ontology is entirely foreign to the scientific community as a methodological principle.

I am inclined to agree with Chihara on these matters; he appears to have shown that Burgess's argument against the attitude-hermeneuticist includes a false premise (line (11) from the Master Argument). Moreover, the above remarks suggest a further point. If it is questionable that scientists believe in mathematical theories as truths about the real world, then the platonist must elicit some kind of *evidence* against the contrasting view that scientists only believe in existence assertions as theorems of mathematical theories. The trouble is that producing this kind of evidence is likely to involve the advancement of positive theses about the meaning of mathematical assertions, and in doing *this* one is liable to utilize overtly *philosophical* views about language and ontology that are not evidently principles of scientific methodology. Might Burgess's platonism be inconsistent with his naturalism? I take up this issue in the closing section of this chapter, after I give my reasons for thinking that the Master Argument is not validated by Burgess's naturalism.

4.3.3 The False Dilemma Reply

The success of Burgess's Master Argument against nominalism depends on the plausibility of generating a comprehensive list of nominalistic theses T_1, \dots, T_n , showing that any given nominalist is committed to at least one of these, and arguing that for each such T_i there exists a corresponding instance of the Master Argument. A popular reply is that neither the hermeneutic nor revolutionary positions described above provide accurate characterizations of the nominalist philosophies of mathematics. That is, Burgess argues from a *false dilemma* when he requires that nominalists colonize his subdivisions.

Both Chihara and Hellman have made the False Dilemma reply. In particular, they

them is required by the standards of mathematics" (ibid., 110). Formalist mathematicians are given as a supporting example.

have both addressed Burgess's characterization of their views as being either hermeneutic or revolutionary. In his response, Chihara claims to, "accept neither of the alternatives he allows his opponents" (1990, 189). Meanwhile, Hellman alleges that, "this dichotomy leaves no room for what I should have thought was the proper category for most of these programs, namely a kind of 'rational reconstruction' " (1998, 342).

Chihara explains in broad terms the motivation behind his Constructibility Theory:

The nominalistic reconstructivists of the sorts I have in mind do not attempt to judge common sense and science from some higher, better, and further standpoint. They seek to piece together their account of mathematics in a way that is compatible with both what science teaches us about how we humans obtain knowledge and also what we already know about how humans learn and develop mathematical theories. Furthermore, these nominalists do not reject mathematics—a fortiori, they do not reject mathematics on the basis of "some higher and better and further standpoint". On the contrary, their goal is to understand the nature of mathematics in a way that is compatible with the other features of the Big Picture they are attempting to construct. (2004, 159)

Several pages later, he identifies in specific terms what he is up to:

... I was proposing an answer to a highly theoretical and deeply philosophical question: can our contemporary scientific theories be reformulated or reconstructed in a way that will not require the assertion or the presupposition of abstract mathematical objects? This is a modal question. It is not a practical question of how best to teach physics in our secondary schools, colleges, and universities. (ibid., 165-6)

Clearly, Chihara is not a revolutionary nominalist; his concern is to rebut the indispensability arguments and not at all to offer a substantive proposal for how mathematics (and science) ought to be done. Chihara also explains that he is not a hermeneutic nominalist, stating that his theory, "was not meant to be an analysis of the mathematical statements asserted by practicing mathematicians" (ibid., 165).³⁰ What is the proper setting in which to approach Constructibility Theory? According to Chihara, his, "work is directed at *philosophers* who put forward ontological claims based upon the use of mathematics in

³⁰One might worry that if Chihara is not interested in analyzing the meaning of mathematical assertions then he will not be able to "understand the nature of mathematics." I raise a related worry at the end of this chapter.

empirical science" (1998, 319). Burgess, of course, would not be satisfied by this reply. As has been seen, for Burgess the true authority of mathematical existence claims comes not from the empirical sciences but from the mathematical sciences.³¹

The purpose of Hellman's Modal Structuralism, "is to help answer certain *metamathematical* or *metascientific* questions, not normally entertained in pure and applied mathematical work proper" (Hellman 1998, 342). He elaborates,

If a reconstruction is not intended as a *replacement* or even *revision* of ongoing theory, if it is intended rather as a coexisting proposal for understanding such theory or its accomplishments, preserving its substance while facilitating accommodation within a naturalistic epistemology, then it would indeed be fairly preposterous for the author of such a reconstruction to submit it to a physics or mathematics journal. (ibid., 344)

And elsewhere he insists that,

... nominalistic *reconstructions* need be neither hermeneutical nor revolutionary but can be—and in most cases in question *are*—*preservationist* while attempting to solve or avoid certain epistemological, metamathematical, or metascientific problems not (or not yet) treated within science itself. (Hellman 2001a, 703)

Thus, Hellman does not see his view (or other nominalist views) as fitting neatly into Burgess's subdivisions.

These observations are spot-on. Neither of the major nominalist projects in the philosophy of mathematics can be accurately classified either as hermeneutic or as revolutionary; the disjunction Burgess poses to the nominalist is indeed a false dilemma.³² Thus, this response soundly refutes Burgess's original arguments. However, I am not convinced that this response is enough. Why? Because it seems perfectly reasonable to suppose that Burgess can always enlarge the scope of his list of nominalistic theses, making the whole problem come back. Whatever the best characterizations are of the motives and

³¹Cf. Chihara's defense of attitude hermeneuticism in the previous section.

³²A similar defense is available to Field, who is also self-consciously motivated by the indispensability arguments. Field does not describe his fictionalism as a view about how mathematics or science ought to be done, but instead as a demonstration of the *possibility* of doing mathematics and science without quantifying over mathematical objects. One might, however, accuse Field of hermeneuticism in describing mathematical knowledge as logical knowledge.

goals behind nominalistic reinterpretations—and I worry with Burgess about how clearly nominalists have articulated themselves on these details—Burgess will always have recourse to the reply that the very fact that *scientists* are not engaging in such activity is all the evidence he needs in order to demonstrate that nominalist philosophies of mathematics are *unscientific*. In Chihara’s case, Burgess can quite reasonably voice skepticism about whether there exist any *scientific* reasons which might motivate expending one’s intellectual resources examining the modal question about whether it is possible to avoid commitment to mathematical objects. That is, unless Chihara can convince *scientists* of the *scientific* importance of *asking* his modal question, it appears as though Burgess can easily formulate a new instance of his Master Argument against Chihara-style reconstructions. Similar remarks are in order against Hellman. That he seeks answers to questions “not normally entertained” in mathematics is kindling for the fire; Burgess will see this as just more evidence that Hellman’s goals are unscientific.³³

Simply put, there is something else wrong about Burgess’s reasoning that has yet to be addressed in the literature.³⁴ Each of the above replies maintains, with varying degrees of success, that the published instances of Burgess’s Master Argument contain false assumptions. I know of no serious attempts to show that Burgess’s naturalism is incapable of validating some of the key inferences of the Master Argument—a possibility I would like to raise in the next section.

4.4 My Reply

I argued in the previous section that a demonstration of the scientific merits of nominalistic reconstruction requires, on Burgess’s terms, *more* than showing that it is *possible* to carry out scientific reasoning in nominalistic theories; nominalists must point to *actual cases* of scientists putting their theories to use. I also argued that advancing the False

³³For skepticism about whether a project like Hellman’s even qualifies as *metascientific*, see (Marquis 2000).

³⁴As I show below, there is, nevertheless, an appropriate setting in which to advance the False Dilemma reply.

Dilemma reply requires *more* than showing that Burgess has mischaracterized the aims of reinterpretation. His criticism is charitably interpreted as beginning from a justifiable position of unclarity concerning what nominalist accounts of mathematics have to say about the actual content of mathematics. Burgess's subdivisions constitute his efforts toward understanding what nominalists are up to when they present novel constructions and interpretations of mathematics. And so, first, nominalists must precisely locate the arena of discourse in which nominalist reinterpretations and reconstructions of mathematics are put forward. And second, such a discussion must occur within an organic extension of scientific methodology. It is *prima facie* unlikely that nominalists will succeed in these endeavors, and so some suitable version of the Master Argument is bound to run its course. How, then, is a nominalist to respond to Burgess's arguments, if she wishes to do so without rejecting his naturalism *in toto*?

Here again is the argument; perhaps on closer inspection a previously overlooked error will be uncovered.

11. Nominalists advance thesis *T*.
12. There is no scientific evidence in support of *T*.
13. It follows that *T* is un- or anti-scientific.
14. Thus *T* cannot be accepted by a naturalist.
15. Therefore nominalism and naturalism are inconsistent doctrines.

I suggested above that there is likely to be some possible instance of (11) that a nominalist would accept, and that given such a starting point, it is unlikely that she can show that (12) is false. And that is all Burgess needs in order to get an instance of the argument up and running. But attacking (11) or (12) is the only way to show that the argument contains a false premise. The only possible response left to the nominalist is to argue that the argument is *invalid*. But how to proceed? It is clear that the inference from (14)

to (15) is beyond dispute. This leaves the inferences from (12) to (13) and from (13) to (14). These two inferences are the heart and soul of Burgess's criticism of nominalism. If either should fail, the Master Argument is invalid. I have intimated at various points above that my intention is to show that Burgess's position on the relationship between the attitude scientists adopt toward a statement p is, at best, equivocally related to the attitude naturalists must adopt toward p . More specifically, the equivocality of this relationship has as a consequence that Burgess's naturalism is singularly incapable of validating at the same time both the inference from (12) to (13) *and* the inference from (13) to (14). In making good on this promise it should be instructive to examine Burgess's clearest account of the relationship between the attitudes scientists adopt toward a statement p and the attitudes naturalists must adopt toward p , which is presented in (Burgess 1998).

4.4.1 The Tonsorial Question

For Burgess, the success of nominalist reinterpretations and reconstructions of mathematics turns on the status of Ockham's razor. If ontological parsimony is a governing principle of scientific methodology, then the nominalist can, in principle, use Ockham's razor to validate her various T_i 's as scientific; otherwise *she* faces the chopping block. Burgess maintains that, "nominalism is no teaching of science" (2005, 90). His account of why this is so should make it as clear as is possible precisely how he uses the terms 'scientific' and 'unscientific'.

In Burgess's writings one finds an apparent inconsistency. On the one hand, he holds that Ockham's razor is an acceptable scientific principle, agreeing that, "explanations in terms of extraordinary agencies are not to be resorted to until explanations in terms of ordinary agencies have been exhausted" (Burgess 1998, 210). This is explained as the idea that scientists avoid gratuitous assumptions, be they ontological or ideological (*ibid.*, 211). On the other hand, Burgess also clearly maintains that science is chin-deep in abstract objects, writing (with Rosen) that, "a thoroughgoing naturalist would take the fact that abstracta are customary and convenient for the mathematical (as well as other) sciences to

be sufficient to warrant acquiescing in their existence” (Burgess and Rosen 1997, 212). But if scientists accept Ockham’s razor, and the nominalistic reinterpretations of mathematics succeed in eschewing commitment to mathematical objects, why is Burgess not compelled to conclude that the “customariness” and “convenience” of mathematical objects in science make only an illusory gesture at their existence?

The key to dissolving this apparent inconsistency is to recognize, with Burgess, that Ockham’s razor is a *general* principle that can be applied in any number of distinct ways. Ockham’s razor can be wielded to evict the mathematical realm of anything except sets, or except functions. It can be wielded to banish unnecessary causal agencies. It can be wielded to avoid nonsense postulations, such as the invisible purple dinosaur that undetectably manipulates the firing of neurons in a way that convinces certain philosophers of the existence of mathematical objects. (Why is the imagined beast always purple?) The nominalist condones the use of Ockham’s razor as a general rule calling her to avoid unnecessarily postulating extraordinary agencies, to be sure, but she is also interested in putting it to use voiding the blank ontological checks that scientists and mathematicians write themselves while doing science and mathematics.³⁵ According to Burgess, whether scientists care about the economy of *mathematical* ontology, “must be tested directly against the evidence of past decisions of physicists” (1990, 11).

How, then, does Ockham’s razor fare as a scientific principle tasked with dispensing with mathematical objects? Burgess is skeptical of this all the way back in 1983 when “Why I am not a Nominalist” was first published:

... the avoidance of ontological commitments to abstract entities does not seem to have won recognition in the scientific community as being in itself a goal of the scientific enterprise on a par with scope and accuracy, and convenience and efficiency, in the prediction and control of experience. It seems distinctively and exclusively a preoccupation of philosophers of a certain type. (Burgess

³⁵Recently, at least one nominalist has explicitly endorsed Ockham’s razor as a scientific principle that favors nominalism. Mary Leng writes that, “adopting our ordinary scientific standards of inquiry surely requires us to adopt the principle of Ockham’s razor... I feel justified... in moving beyond mere agnosticism and concluding that we are justified in denying the existence of mathematical objects” (2010, 260)—but cf. (Balaguer 1998, 144-8).

2008b, 37)

Seven years later, he again voices his reservations about the scientific value of the nominalist's use of Ockham's razor:

...since rigor and consistency are already usually conceded by descriptive methodologists to be weighty scientific standards, the burden of proof seems to be more on those who would insist that ontological economy of mathematical apparatus is also a weighty scientific standard. This burden of proof has not yet been fully met. (Burgess 1990, 12)

Nevertheless it is not until 8 years later that Burgess finally provides evidence for these claims. Quoting Gideon Rosen, he asserts that, "purging physics of a commitment to numbers serves no goal recognized either implicitly or explicitly in the practice of science" (Burgess 1998, 204). The lesson of (Burgess 1998) is that the philosopher's use of Ockham's razor is importantly distinct from the scientist's use of that same principle. Scientists, Burgess claims, implement Ockham's razor to rid the world of unnecessary *causal* agencies. The nominalist, meanwhile, uses Ockham's razor to rid the world of unnecessary *abstract* entities. With Rosen, Burgess asks, "has abstract ontology ever been what was at issue in any important case of dispute between proponents of rival theories in empirical science?" (Burgess and Rosen 1997, 217). Their answer? That they, "know of no clear example of striving after economy of abstract ontology in any domain of science, and we are dubious that there is one" (*ibid.*, 225). The outlook is even worse when turning to the mathematical sciences:

It is not part of the practice of ordinary mathematics to take steps to disavow belief in the standard existence theorems, to warn students against believing, or the like. Thus we lack what would be the best kind of direct evidence that the practice of mathematics and science involves something less than belief in existence theorems. (Burgess and Rosen 2005, 526)

That students of mathematics are not warned "against believing" is rather poor evidence for Burgess's conclusion; the resources that are useful for getting students to understand and prove theorems are not necessarily those resources which best highlight the nature

of mathematics. Nevertheless, absent the direct evidence in question, “it strains credulity to suggest that the internal norms governing scientific inquiry demand such disavowal nonetheless” (ibid., 528). The main point underlying these remarks is best summarized in (Burgess and Rosen 1997, 213):

... reconstructive nominalists who profess ‘naturalism’ have not themselves much said it. They have not proceeded by first presenting studies of the distinctions and divisions observed within the community of working scientists, and then citing these as warrant for discarding pure mathematics and ignoring familiarity, perspicuity, and fruitfulness. So one may well ask what the source of their warrant is supposed to be.

In essence, Burgess’s complaint is that there is simply no evidence that scientists, including mathematicians, condone the use of Ockham’s razor for eschewing commitment to mathematical objects.³⁶ The nominalist is alleged to commit the fallacy of accident when she presumes it is right and good to use this otherwise acceptable principle of scientific methodology in felling numbers and sets (of course, the reader has no assurances that Burgess has himself conducted a scientific study of science in making this allegation).

In order to avoid falling into this trap, nominalists must first uncover evidence that avoiding commitment to *abstract* objects is a governing principle of scientific methodology. Burgess does not expect that the nominalist will be successful in this endeavor. Moreover, even if the nominalist *were* to be successful in convincing the likes of Burgess that eschewing commitment to mathematical objects is a going scientific concern, she faces the subsequent task of uncovering evidence that avoiding commitment to abstract objects is an *important* enough criterion of theory-selection to compete with other well-established criteria such as familiarity and scope. It is at this point that Burgess can entertain the question as to whether nominalist reinterpretations of mathematics might ever win the approval of the scientific community; most nominalists are even willing to grant that the answer to this question

³⁶Cf. Stewart Shapiro: “The serious point underlying Burgess and Rosen’s suggestion is that only *scientists* (including editors of professional scientific journals) are to determine what counts as *scientific* merit. And for a naturalist, what else counts as *merit*? The fact is that scientists are not much interested in eliminating reference to mathematical objects. The nominalist has to show them that, by standards which they have implicitly adopted, scientists should eschew reference to mathematical objects” (2000, 247-8).

is 'no.' The upshot is that nominalist philosophies of mathematics and the concerns that motivate them are thoroughly *unscientific*. (If Chihara is to be trusted, many scientists and mathematicians espouse agnosticism toward the ontological commitments of mathematics. Agnosticism is a form of disavowal, and so there is at least some evidence suggesting that scientists and mathematicians adopt an attitude that amounts to less than full belief toward existence theorems. Burgess could suggest that these scientists and mathematicians are guilty of not faithfully applying their own standards of evidence. However, Burgess is already on record as stating that the existence of mathematical objects is a *presupposition* of science. Is this presupposition subject to confirmation or disconfirmation on the basis of evidence? If not, then agnosticism toward it is not to be denigrated on the basis of the evidential standards of science. On the other hand, if this presupposition *is* subject to confirmation or disconfirmation on the basis of evidence, then the presence of agnosticism in the scientific community ought to count as *evidence* against it. This, I take it, is Chihara's response on behalf of the attitude-hermeneuticist. These concerns will be revisited in the last section of the chapter.)

But in this context, what does the term 'unscientific' really mean? And what is Burgess implying when calling a claim 'unscientific'? Burgess has made a strong case that the particular fashion in which nominalists wield Ockham's razor does not emblemize any going principle of scientific methodology, apparently confirming the assumption of the Master Argument that there is no scientific evidence in support of the theses advanced by nominalists. He subsequently advertises nominalism as un- or anti-scientific. This appears to recommend the following meaning of the expression '*p* is un- or anti-scientific':

17. A statement *p* is un- or anti-scientific if and only if *p* has not been justified by any evident principles of scientific methodology.

But this characterization is much too strong; it rules as un- or anti-scientific all unrecognized consequences of current accepted scientific theories, to say nothing of what it implies about the status of statements belonging to any provisional theories or to any superior theories

that may be developed in the future. And it is thoroughly unclear how plausibly this principle applies to episodes of theory change. Moreover, Burgess does not pretend to have performed an exhaustive study of the history of scientific methodology; perhaps, unknown to philosophers, there *are* cases of scientists using Ockham's razor to pare down abstract ontology. A moderate revision to (17) fixes this error:

18. A statement p is un- or anti-scientific if and only if p can be justified only by principles that are *not* evidently principles of scientific methodology.

This definition does a better job of encapsulating Burgess's misgivings about nominalism. He indeed asserts that no nominalist has *actually* produced a scientifically acceptable justification for reconstructing mathematics, moreover he is convinced that all *possible* justifications for the available nominalist reconstructions are likely to cite views about ontology or ontological commitment that are entirely foreign to the scientific community. (Presuming that (a) good sense can be made of who in particular belongs to the "scientific community," and (b) nominalist philosophers of mathematics are (for the most part) not members of the "scientific community." Ostensibly Burgess would consider someone to be a member of the scientific community when they carry out research in some scientific discipline while adopting the methodological principles of natural science, apportioning the same weight to criteria (i)-(vii) as do other members of the scientific community (which poses a potential circularity issue). Nominalist philosophers of mathematics are allegedly disqualified because they place too much weight on the criterion of economy of abstract ontology.) Now, (18) does not imply that as-yet underived consequences of current accepted theories are un- or anti-scientific; nor does it imply that statements of provisional theories or as-yet undeveloped superior theories are un- or anti-scientific.³⁷ Most importantly, however, is that (18) is sufficient to justify the inference from (12) to (13) in the Master Argument. Can it also secure the inference from (13) to (14)?

³⁷This latter point may only be true provided that the class of evident principles of scientific methodology is permitted to vary as science progresses.

Recall again Burgess's understanding of naturalism as a view that commits one

... at most to the comparatively modest proposition that when science speaks with a firm and unified voice, the philosopher is either obliged to accept its conclusions or to offer what are recognizably scientific reasons for resisting them. (Burgess and Rosen 1997, 65)

This appears to recommend the following position on when a statement is naturalistically acceptable:

19. If p is univocally endorsed by the scientific community, then acceptance of p is naturalistically required.

As things stand, (19) only implies that p is naturalistically *unacceptable* when $\text{not-}p$ is univocally endorsed by the scientific community. Hence the following position on when a statement is naturalistically *unacceptable*:

20. If p is univocally *rejected* by the scientific community, then *rejection* of p is naturalistically required.

To some, (19) and (20) might appear too strong; nothing essential would be lost by moving their antecedents into the subjunctive mood:

21. If p is such that it would be univocally endorsed by the scientific community, then acceptance of p is naturalistically required.
22. If p is such that it would be univocally rejected by the scientific community, then rejection of p is naturalistically required.³⁸

In order to get from (13) to (14), Burgess must show that nominalism is unscientific in a sense that satisfies the antecedent of either (20) or (22). It turns out that it is to Burgess's benefit to focus just on (22); no comprehensive poll of the scientific community has ever taken place on the realism debate, so it would seem that there is not sufficient evidence

³⁸Perhaps both of these subjunctive conditionals should end with: "... of an ideal observer."

for holding that nominalism *is actually* the object of the universal disapprobation of the scientific community—(20) is at best vacuously true.³⁹

According to (18), to say that p is unscientific means that p cannot be justified by any principle of scientific methodology. This could mean one of two things. In the first place, it could mean that p implies the contradiction of some statement q univocally accepted by the scientific community (for instance, an explanation given by an astrologer for why a relationship ended poorly). Call this the *strong* reading of (18). Whenever a statement p is unscientific according to the strong reading of (18), it is clear that p ought to be univocally rejected by the scientific community, and hence supports the inference from (13) to (14). In the second place, it could mean that p , though unsupported directly by scientific methodology, is nevertheless justified in such a way as not to interfere with any of the pronouncements of science (for instance, the utilitarian association of happiness with pleasure). Call this the *weak* reading of (18). Whenever a statement p is unscientific according to the weak reading of (18), there can be no *prima facie* claim made about how p is viewed by the scientific community. But then (22) (even if it is restated as a biconditional) does not imply that such a statement is naturalistically unacceptable.⁴⁰ And so claims that are unscientific only according to the weak reading of (18) fail to validate the inference

³⁹Perhaps there is reason for thinking that (22) is also vacuously true. Earlier, while motivating Burgess's naturalism, I briefly recounted the criticism that it is a distortion of the practice of science to suppose that the scientific community would ever come to uniform agreement about nontrivial matters. If this criticism can be sustained, then (22) will be non-vacuous in very few cases. But then, for most statements p , rejection of p is not naturalistically required. On this picture, sympathy to nominalism is naturalistically permissible because sympathy to nearly everything is naturalistically permissible! Below I try to flesh out a more attractive account of why sympathy for nominalism is consistent with Burgess's naturalism.

⁴⁰Not even (19) and (21), reformulated as biconditionals, imply that claims satisfying the weak sense of (18) are naturalistically unacceptable; all that is implied is that naturalists are not compelled to accept such claims. In a paper arguing that *trump naturalism* (accept p if science sanctions p) does not imply *biconditional naturalism* (accept p if and only if science sanctions p)—principles analogous to (19) and (21) and their biconditional reformulations—Paseau comes to a very similar conclusion: "Silence regarding p —neither sanctioning p nor sanctioning $\neg p$ —is not the same as sanctioning suspension of belief about p " (2010, 647). Paseau's discussion is not sensitive to the distinct ways in which a claim could fail to gain the acceptance of the scientific community. He offers theological and astrological claims as examples of claims that, "may be acceptable even if science does not sanction [them]" (*ibid.*, 643). Nevertheless I suspect that astrology and theology are inconsistent with science at the level of methodology, in the sense that science *rules against* the evidential standards of astrology and theology, whereas it is by no means clear that a similar verdict is warranted in the case of nominalist philosophies of mathematics.

from (13) to (14).⁴¹

An important worry remains: Why suppose that *anything* satisfies the strong reading of (18)? Or, what comes to the same question, why suppose that anything *fails* to satisfy the weak reading of (18)? What solace is to be had in discovering that nominalism satisfies only the weak reading of (18) if it turns out that just about everything else satisfies this reading as well? The nominalist appears to need, along with Burgess, some mechanism by which she can ensure that the scientific community comes to uniform agreement about at least some things. Without such a mechanism it is empty to use terms like “naturalistically acceptable” and “naturalistically required.” This is no small task—indeed, it might be thought of as the greatest obstacle facing anyone interested in developing a coherent methodological naturalism. My aspirations are not so ambitious. Although the scientific community is not necessarily unified on general matters like methodology, I am going to go out on a limb and assume that the scientific community is unified on many matters of fact about the physical world: that there exist electrons, that the actual physical constants are approximately what science says they are, that planetary conjunctions are not importantly causally related to interpersonal relationships, etc.

I submit, accordingly, that a preliminary test of whether a methodological principle is naturalistically unacceptable is whether its implementation produces in its advocates beliefs that conflict with scientifically accepted facts. I claim that this unveils most theological and astrological belief-forming mechanisms as unacceptable. Nominalist philosophies of mathematics involve no such tomfoolery. Of course, this is not a sufficient defense of nominalism. Naturalism is not only a view about what one should believe but also a view about how one should form beliefs. Consider the epistemological freak Trusci, who for no discernible reason is able to reliably form beliefs about the microphysical world that are later confirmed by particle physicists. Trusci acquires no belief that conflicts with any mat-

⁴¹Cf. Balaguer’s defense of fictionalism in (Balaguer 2009). Balaguer argues that the thesis of fictionalism is mathematically *unimportant* and that philosophers—not mathematicians—are best equipped to assess its merits.

ter of fact, but intuitively, a naturalist should not adopt belief in p solely on the authority of Trusci. The case of Trusci proves that although one can and should reject *unreliable* methods, one should not accept just *any* reliable methods. I am not certain how to rule out wonders like Trusci. But how Trusci forms beliefs is altogether distinct from the possible routes nominalists, including modal nominalists, envision for the epistemology of mathematics. Nominalists are motivated by attempting to account for mathematical knowledge using already familiar and naturalistically acceptable faculties, such as the human competence for recognizing patterns and for making logical and modal inferences. And so it strikes me as unreasonable in the highest degree to group nominalist philosophies of mathematics together with the likes of Trusci. Reasons for thinking that Trusci's belief-forming methods satisfy the strong reading of (18) do not transfer to the philosophical considerations which underlie nominalist philosophies of mathematics. Nonetheless, I suspect that there do exist reasons for thinking that Trusci's belief forming methods satisfy the strong reading of (18), and so I do suppose that the scientific community would uniformly disapprove of Trusci-followers—thus I believe that the scientific community does come to uniform agreement on some things that are more robust than mere matters of fact. I admit that this belief is one that I am not certain how to justify in a rigorous fashion.

So what has Burgess shown? Has he unveiled nominalistic reinterpretations as satisfying the strong reading of (18)? Or has he only shown that nominalistic reinterpretations satisfy to the weak reading of (18)? Revolutionary strands of nominalism clearly satisfy the strong reading of (18), but no nominalist that I know of has ever endorsed a revolutionary view. The case against the hermeneuticist is less clear. Would mathematicians and scientists univocally assent to the platonist's semantical analysis of mathematical existence assertions? If so, then content-hermeneuticists satisfy the strong reading of (18). Would mathematicians and scientists univocally assent to the view that mathematical existence assertions are truths about the world (as opposed to truths as theorems of mathematical theories)? If so, then attitude-hermeneuticism satisfies the strong reading of (18). But, as

the saying goes, these are big ‘ifs’; mathematicians and scientists are known to adopt a variety of views about the nature of mathematics, and it would be naïve to expect universal agreement on these matters. Nevertheless, it is not clear that any nominalist that I defend in this dissertation is engaging in either kind of hermeneutical project. Here, then, is the appropriate venue for advancing the False Dilemma reply. Burgess has not accurately depicted any of the nominalist views he claims to reject. *A fortiori*, he cannot, with confidence, assert that these views are unscientific according to the strong reading of (18). This last point gains traction in conjunction with the observation that nominalistic reinterpretations of mathematics are designed *not* to interfere with the day-to-day practices of scientists and mathematicians. At best, Burgess has a case that nominalism satisfies the weak reading of (18); but then he has failed to show that nominalism is un- or anti-scientific in any sense that compels a naturalist to reject nominalism!

David Liggins reaches an assessment of Burgess’s arguments that appears to be very similar to my own (Liggins 2007).⁴² He gives the following gloss on Burgess’s account of naturalistic acceptability:

23. When mathematical and scientific standards require mathematicians and scientists to accept an existence theorem t , philosophers are justified in accepting t regardless of what philosophical arguments are offered against it. (Liggins 2007, 107)

Liggins remarks the last clause of (23) assumes a deference to science that many philosophers will find “too much to take,” and that a more plausible principle should read,

24. When mathematical and scientific standards require mathematicians and scientists to accept an existence theorem t , philosophers are justified in accepting t unless there is a sufficiently strong philosophical argument against it. (ibid., 110)

⁴²Liggins’s explicit target is Burgess and Rosen’s positive argument for *accepting* the existence of mathematical objects, as opposed to their negative arguments *against* nominalism (Burgess and Rosen 2005, 516-17). Ostensibly these arguments depend on the same views about when a naturalist is compelled to accept a claim.

Against both (23) and (24) he argues that the acceptance of existence theorems is *not* required by the standards of mathematics. He maintains that, e.g., formalist mathematicians may reject existence theorems without thereby abandoning or countermanding the evidential standards of mathematics. Of course, most mathematicians do not identify themselves as formalists; many in fact accept existence theorems without reservations of any kind. Although the statements that mathematicians are *required* to accept may fail to speak against nominalism, perhaps some of the statements mathematicians are *permitted* to accept discredit nominalism. To deal with this wrinkle, Liggins entertains one final gloss on naturalistic acceptability:

25. When mathematical and scientific standards permit mathematicians and scientists to accept an existence theorem t , philosophers are justified in accepting t unless there is a sufficiently strong philosophical argument against it. (ibid., 111)

But (25) holds philosophers to *higher* standards than those to which mathematicians and scientists are held; it implies that statements *optionally* accepted by scientists are nevertheless *required* for philosophers.⁴³ Liggins alleges that (25) goes, “beyond mere respect for the internal standards of mathematics,” and is not consonant with Burgess’s “comparatively modest” naturalism (ibid).

How does Liggins’s response compare to mine? His main contention is that the standards of mathematics do not require mathematicians to accept the platonist’s interpretation of mathematical assertions; his example of formalist mathematicians is offered as evidence that there is actual mathematical evidence in favor of anti-platonist interpretations of mathematics. In order for this to be convincing evidence that naturalist philosophers are *permitted* to adopt formalist views on mathematics, something like the following principle must be assumed:

26. When mathematical and scientific standards permit mathematicians and scientists to

⁴³And worse; if there is some proposition such that acceptance of both p and $\neg p$ is permitted in science, then philosophers are *obliged* to accept both p and $\neg p$!

accept t , philosophers are permitted to accept t as well.

I do not find anything particularly objectionable about (26); I welcome evidence suggesting that acceptance of nominalism is mathematically and scientifically permissible.⁴⁴ Nevertheless my reply is distinct in relying on a principle that is even more generous than (26). I have argued that even if there is *no* evidence that scientists and mathematicians would accept a claim p , it does not thereby follow that p is naturalistically unacceptable, provided that p does not imply the contradiction of anything scientists and mathematicians would uniformly accept (i.e., provided that p satisfies (18) on only the weak reading). I submit that Liggins's response, rather than competing with mine, serves instead to complement it. However, my response has the advantage of not depending on any (actual or hypothetical) cases of mathematicians and scientists accepting nominalism.

Mary Leng's response to Burgess is also quite similar to my response. Leng aims to show that the revolutionary fictionalist, although advancing a revolution in the *understanding* of the practice of science, nevertheless proposes no revolution in the *practice* of science. After attempting to undermine Burgess's positive arguments for the existence of mathematical objects, she writes that the fictionalist's

...scepticism about literalism as the default interpretation of mathematics weakens Burgess's position here, since it is not entirely clear that the revolutionary fictionalist is denying anything that mathematicians and scientists have sincerely asserted. (Leng 2005, 282)

If she is right, Leng has shown that revolutionary fictionalism satisfies only the weak reading of (18). What conclusion does she allege follows from this observation?

It is, we can accept, a mistake to advocate the *abandonment* of a successful discipline on the basis of philosophical scruples about, for example, the ontological assumptions of that discipline. To this extent, we may be modest. However,

⁴⁴But here I must say that I would like to hear quite a bit more about the conditions under which scientific and mathematical standards permit—without *requiring*—practitioners to adopt beliefs. Without a precise account of such conditions one cannot be confident that, e.g., the formalist beliefs of certain mathematicians are actually permissible *as far as scientific and mathematical standards are concerned*. Cf. my discussion in chapter five of how Maddy's uses mathematical methodology as a rein on metaphysical theorizing about mathematics.

so long as we can explain why a practice that is in some respects misguided is nevertheless successful, we need not advocate the abandonment of that practice just because it falls short of some of our expectations. (ibid., 283)

On the assumption that the revolutionary fictionalist is indeed successful in vindicating the utility of mathematics, both pure and applied, Leng asserts that fictionalism is compatible with accepting the practices of mathematics and science. This reply is offered only on behalf of the revolutionary fictionalist—very well; then I have indicated the way in which a similar response can be made on behalf of many other kinds of nominalist projects.

I have argued that Burgess has not done enough to show that nominalism is unscientific in a damaging way. I have done so by putting pressure on an aspect of his view about which he is unclear: the naturalistic acceptability of statements that receive from the scientific community neither universal approval nor universal disapproval. The onus is on Burgess to articulate a more refined notion of naturalistic acceptability that provides an intuitively palatable account of why nominalism should be thought of as inconsistent with naturalism despite the purgatorial status of nominalism in relationship to the scientific community. Surely there exist many naturalistically acceptable (and unacceptable) claims that receive from the scientific community neither universal approval nor universal disapproval, and there ought to be a way of sorting the ripe from the rotten. Burgess's naturalism is insufficiently equipped to handle this task, and so is not equipped to betray nominalism as naturalistically unacceptable. Paseau picks up on this theme, writing that,

The debate must be decided not by table-thumping declarations from either side that the question is or is not scientific, but by examining the exact boundary science itself posits between questions within the scientific realm and those outside it. (2007, 144)

Saying these things does not, however, suffice to show that naturalism and nominalism are indeed consistent. The nominalist still must clearly articulate the motives and goals of her reinterpretations, moreover, she would do well to seek out evidence that hers is the kind of view that *at worst* satisfies only the weak reading of (18), and *at best* plays some

positive role in contemporary science and mathematics.

4.5 The One Hope for the Vanquished: Why Burgess is not a (Moderate) Platonist

Sinon's false promise of peace has been heeded, despite Laocoön's fear of Greeks (even those bearing gifts!). The great false horse has been drawn within the city gates. Achaeans soon disembark from the bowels of the wooden beast and terrorize the city. Troy is engulfed in flames. Aeneas utters the following words to his few remaining warriors: *una salus victis nullam sperare salutem*.⁴⁵

Suppose, for the sake of argument, that the remarks of the previous section are wildly implausible, and that Burgess's criticism of nominalism does not rest on an insufficiently articulated naturalist position. That is, suppose that the Master Argument is permitted to run its course against the nominalist, much as the Trojans foolishly took Sinon at his word. Then the nominalist has no choice but to admit defeat; her views are unscientific in such a way that she cannot maintain her nominalism and still call herself a naturalist. But a glimmer of hope remains; though her views are unscientific, perhaps her adversary fairs no better. Perhaps there is hope for neither the nominalist nor the platonist.

Burgess describes himself as a *moderate* platonist. Moderate platonism is that the view that when scientists and mathematicians accept a claim that appears to posit the existence of mathematical objects, e.g., "there exist infinitely many prime numbers," that one ought to thereby acquiesce to the existence of mathematical objects. Moderate platonism is to be preferred to nominalism because nominalism is unscientific. However, this preference for moderate platonism requires for *its* justification some kind of evidence that the view is *not* unscientific in any important sense of the term. Unfortunately, by Burgess's own lights, moderate platonism counts as unscientific. In reply to the hermeneutic nominalist, he alleges that,

⁴⁵The one hope for the conquered is hoping for no hope.

...there is a considerable problem of *evidence* for any claim about “depth” analysis. And perhaps one needs to be *especially* careful about evidence when the claim is made not by a linguist for whom an understanding of hidden mechanisms of language is an end in itself, but rather by a philosopher with ulterior ontological motives. (Burgess 2001, 440)

Burgess should find these remarks troubling; platonism—even moderate platonism—is just as entangled in claims about “depth analysis” as is hermeneutic nominalism, and Burgess never once points to linguistic evidence in favor of his interpretation of mathematical language. Chihara presses this point nicely:

...where is the evidence that supposedly supports belief in the entities? Here, I mean evidence of the sort that would convince the physicist or biologist. Certainly, Burgess supplies none at all. So it is hard to see why he is confident that these mathematical objects exist. And if he is able to provide some kind of philosophical reason for holding on to such a belief, despite the absence of any scientific evidence, it is hard to see why he should claim that his position is the scientific one and that his opponent’s position is superstitious and dogmatic. (1990, 189)

What is more, an important part of Burgess’s evidence that nominalism is unscientific consists in expressing his doubts that nominalistic interpretations of mathematics and physics would ever be published in physics and linguistics journals. The conclusion drawn is that eschewing commitment to mathematical objects is not a going concern of science. But Burgess should be the first to admit that in order for platonism to count *as* scientific, he must evince evidence that the *postulation* of mathematical objects *is* a going concern of science. Chihara’s insight here is incisive: “If, as Mathematical Realists claim, mathematical objects must be postulated to account for physics, it is striking that physicists do not publish papers in which the existence of mathematical objects is postulated” (1998, 319). Leng adds, “we might label Burgess’s alternative view ‘hermeneutic literalism’, and wonder whether this view fares any better than hermeneutic fictionalism as an account of what mathematicians really mean” (2005, 280).

There is hope yet for Burgess, and that is to show that the attitudes scientists and mathematicians adopt toward mathematical ontology will somehow favor his platonism.

If scientists see their work as confirming the existence of mathematical objects, then the preference for moderate platonism counts as scientific, other things being equal. Unfortunately, Burgess explicitly discounts the testimony of scientists as reliable evidence on these matters:

... there may be serious difficulties with the methodology of pestering scientists for opinions on philosophical issues to which they may have given little or no thought, and accepting their answers as indicative of their intentions in putting forward the affirmations that they do put forward when philosophers leave the scene and let them get back to work. (Burgess 2008c, 55)

This is curious. Burgess asks the nominalist to believe that the decisions of the editors and referees of scientific journals are capable of apportioning the value of economy of mathematical ontology, while also insinuating that these editors and referees are not especially well-qualified to address the question of ontology.⁴⁶ Things only get worse when it is remembered that Burgess holds that striving after economy of mathematical ontology is, “a matter to which most working scientists attach no importance whatsoever” (Burgess 2008b, 37). Penelope Maddy agrees with this last point, stating that, “scientists feel free to adopt any mathematical apparatus that is convenient and effective, without concern for its abstract ontology” (2005b, 451).⁴⁷ What is difficult to comprehend is why anyone should be compelled to believe that this general lack of concern for abstract ontology is unequivocally evidence *for* platonism. Why, for instance, should the carefree attitude scientists take toward abstract postulations *not* count as evidence *for* nominalism? It is not unreasonable to believe that the very fact that scientists are so carefree about their use of mathematics is evidence that they do not have the same attitude about mathematical existence theorems as they do about the existence of, e.g., subatomic particles.⁴⁸ Absent a good reason for discounting the carefree ontological attitudes of scientists as evidence

⁴⁶Even those who are qualified, “tend to disagree with each other quite as much as professional philosophers do” (ibid.).

⁴⁷Cf. (Chihara 2004, 289).

⁴⁸John Halpin suggested (in conversation) that the carefree attitudes taken by scientists toward abstract objects actually constitute evidence *for* nominalism.

for nominalism, no conclusion should be drawn about whether these attitudes support a particular philosophical position on the existence of mathematical objects. In the present context (in which the validity of the Master Argument is granted), the upshot is that platonism and nominalism are equally unscientific, and hence both views are inconsistent with Burgess's naturalism.

If the opinions of scientists are inconclusive (or worse, if they end up showing that *any* ontological position is unscientific), what other considerations could move a naturalist to adopt the platonist's position? In response to the rise in popularity of fictionalist accounts of mathematics, Burgess remarks that what is, "in actual fact very doubtful is whether mathematicians who assert that there are prime numbers greater than $10^{10^{10}}$ intend their assertion only as something 'non-literal' " (2008c, 54). Burgess's case here is quite clear. Mathematicians assert that 'there exist numbers greater than $10^{10^{10}}$,' and there is no evidence that they mean anything other than that there exist numbers greater than $10^{10^{10}}$. But for Burgess the literal assertion that 'there exist numbers greater than $10^{10^{10}}$ ' is not to be understood by appealing to Fregean considerations about truth and singular terms. This would be unscientific, not to mention question-begging. Rather, Burgess understands the term 'literal' to be indicative of what one *is not* doing:

The force of "literally" is not to assert that one is doing something more besides, but to deny that one is doing something else instead: meaning something *other* than what one says, as when one speaks metaphorically... One does not have to think anything *extra* in order to speak literally; one has to think something extra in order to speak *non-literally*. (ibid., n. 9)

What is more, there is, in general, a *preference* for understanding assertions literally:

... the "literal" interpretation is not just one interpretation among others. It is the *default* interpretation. There is a *presumption* that people mean and believe what they say. It is, to be sure, a *defeasible* presumption, but some *evidence* is needed to defeat it. The *burden of proof* is on those who would suggest that people intend what they say only as a good yarn, to produce some actual evidence that this is indeed their intention. (ibid., 54)

This is a restating of the idea that the existence of mathematical objects is a "presumption"

of science. In reply to this, it is worth remarking for a second time that mathematical existence assertions are never uttered in a vacuum. Existence assertions are licensed by theorems, the ultimate warrant of which come from the axioms of mathematical theories; it is certainly no distortion of mathematical practice to understand existence assertions in this context. Focusing on lone existence assertions, then, is a poor test of whether the evidential standards of science support this presumption; it would be more appropriate to investigate whether mathematicians ever seriously endorse the literal truth of the axioms of mathematical theories.

Nevertheless, a question remains about just what the literal (or “standard”) interpretation of a mathematical assertion comes to, and why it should be supposed that the existence of mathematical objects is presupposed under such an interpretation. David Corfield observes that in the mathematical development of the groupoid concept (important in category theory and homotopy theory), “arguments for the ‘existence’ of groupoids did not figure in the array surveyed. . . mathematicians make no use of the idea in their advocacy of the groupoid concept” (2003, 230). If this observation generalizes, then considerations relating to the existence of mathematical objects need not be seen as underlying a literalist treatment of mathematical language. Nevertheless, Hellman is skeptical about whether *any* interpretation deserves the title “standard interpretation”:

Of course, natural science takes mathematics for granted and uses it opportunistically without questioning its foundations or its interpretation. But this suggests to me that it does not worry about how to interpret mathematics at all, not that it accepts in any considered way a face-value, literal, platonist reading of mathematics. Even working classical mathematicians don't universally agree on such a reading. Most scientists, I would wager, have never even thought about the issue. We should not, therefore, even say that “platonistic mathematics” is practically indispensable; at best we may be able to say that the compact, standard languages of mathematics are practically indispensable. This does not tell us that any particular reading or interpretation of such languages is required. (2001a, 703)

Alexander Paseau voices a related skepticism,

Which norms, after all, *have* been successful when it comes to questions of interpretation? Patently, mathematical norms have been effective in the generation of successful mathematics; but the question is whether they have been effective in the generation of successful interpretations of mathematics. That they have a better track record here than philosophical norms remains to be seen. (2005, 392)

... the standard interpretation of mathematics does not command the same allegiance as the cherished tenets of our worldview. Only the most deluded philosopher will deny the Moorean observation that we are more certain that $2+3=5$ (and other mathematical truths) than any philosophy that tells us otherwise. But certainty about the mathematical content of the statement that $2+3=5$ does *not* extend to certainty about any metaphysical claim the statement might make. It would not extend, for instance, to the claim that there really exist non-spatiotemporal, acausal objects standing in the addition relation. (ibid.)

It is unclear what Paseau intends to communicate by using the term 'mathematical content.' What exactly *is* the mathematical content of the statement ' $2+3=5$ ' and how can this content be captured without giving an interpretation to the symbols '2', '3', '5', '+', and '='? He responds to the objection that the standard content of such assertions is platonistic (Paseau 2007, 146-9). Nevertheless he provides very little information about what that content is (other than that it is somehow captured equally well by nominalist and platonist interpretations). This is another instance of a worry I have voiced at several points above concerning the ability (or willingness) on the part of nominalists to provide an account of what mathematics *is about*. (Not that I am suggesting they are alone in this struggle; if it is true that the platonistic interpretation of mathematical language is optional, then presumably such an interpretation does not explicate the content of mathematical assertions in greater detail than nominalistic interpretations.) It is one thing to show that the practice of mathematics is indifferent to the interpretation of mathematical language, but until a view becomes available that delineates mathematical content in precise terms, an important piece of the puzzle is missing.

Nevertheless there is a wide body of evidence suggesting that mathematics, contra Burgess, does not come with its own interpretation. And so *without* something like

Frege's analysis of mathematical language, it is not clear precisely how one proceeds from the endorsement of a mathematical assertion to the existence of mathematical objects. The question is this: Without the additional assumption of some kind of platonism, why is the presumption in favor of the literal a presumption that has anything *at all* to do with ontology? When push comes to shove, Burgess always falls back on this presumption. However, he is happy to discount the philosophical musings of scientists and mathematicians concerning the ontological presuppositions of their theories. But what other kind of evidence is admissible for a naturalist? All that is left over are *philosophical* principles and *philosophical* intuitions. And that is my point—one does not come to accept the existence of mathematical objects unless one views science and mathematics through platonistic goggles. But then even moderate platonism needs for its support theses that are not emblematic of any principles of scientific methodology: the same deadly accusation alleged to extirpate nominalism. And that is why Burgess is not a (moderate) platonist.

Chapter 5

Reflections on Maddy

5.1 Introduction

The upshot of my examination of Burgess is that his particular interpretation of naturalism does not, in the end, have the resources to speak against nominalist theories of mathematics, including the modal nominalist theories I defend in this dissertation. As a defense of modal nominalism, my response to Burgess relied on the on the assumption that modal nominalism is not unscientific in the sense that it requires the rejection of any claims univocally accepted by the scientific community. Relativized to mathematics, the assumption is that modal nominalism is not *unmathematical* in the sense that it requires the rejection of any claims univocally accepted by the mathematical community—modal nominalist theorizing must avoid interfering with mathematical practice. However, it is not *a priori* given that modal nominalism does not interfere with mathematical practice. In fact, an argument to the effect that nominalism *does* interfere with mathematical practice—one that does not rely on any specious appeal to general theoretical virtues—can be distilled from Penelope Maddy’s interpretation of naturalism. My aim in this chapter is to show that the naturalistic resources embraced by Maddy’s naturalism are not capable of being used to show that modal nominalism is in any kind of malevolent conflict with mathematics.

Maddy’s prefers to use the label “Second Philosophy” to denote her naturalism. The term “Second Philosophy” is used in contrast with “first philosophy.” The first-philosophical perspective treats philosophical inquiry as an autonomous enterprise, one that contains its own norms and ideals, and one that is to be conducted using its own methods and standards of evidence (e.g., Descartes’ method of doubt). Instead, the Second-Philosophical perspective holds that philosophical inquiry is only legitimate when it arises from *within* the broad and open-ended enterprise of empirical science. A salient item

here is Maddy's rejection of *a priori* knowledge, because such knowledge is not clearly generated by the use of the methods and evidential standards of empirical science. In contrast with Burgess's broadly Quinean understanding of scientific method, the Second Philosopher aims for discipline-specific engagement with the methodologies of the various disciplines of science. Maddy finds it particularly striking that although mathematics is extremely useful in empirical science, nevertheless much of mathematics is pursued as an autonomous discipline—thus the Second Philosopher is compelled to come to terms with the actual methods adopted by practicing mathematicians. For Maddy, questions about the metaphysics of mathematics (to the extent that they are naturalistically legitimate questions), including questions about the existence or not of mathematical objects, are to be referred first (and perhaps only) to the methods of mathematics.

I begin in section two by providing an overview of the basic naturalistic resources Second Philosophy employs in its assessment of mathematics. The Second Philosopher insists that mathematics is to be understood and evaluated on its own terms. To say that mathematics must be *evaluated* on its own terms means, roughly, that mathematical methods and results are not to be criticized on extramathematical grounds. To say that mathematics must be *understood* on its own terms means, roughly, that a characterization of the methods and subject-matter of the discipline must be conducted using only the methods of mathematics. How do these naturalistic scruples enter into Maddy's own assessment of the methodology and metaphysics of mathematics? Maddy argues that mathematical practice (in particular, the pursuit of new axioms for set theory) is constrained by objective facts of mathematical depth—facts about what makes for fruitful and promising avenues of mathematical research. She posits these facts as the underlying reality of mathematics, and they in turn function as reins on metaphysical and philosophical theorizing about mathematics: For example, metaphysical theorizing about the nature and existence of mathematical objects is only in good Second-Philosophical standing when such theorizing is compatible with the idea that purely mathematical forms of justification are sufficient for revealing

the nature and existence of mathematical objects (as described under the hypothesized metaphysical theory). In other words, metaphysical theorizing about mathematics is to be rejected if such theorizing introduces a justificatory gap between mathematics and its metaphysics.

Section two closes with the construction of two provisional objections to modal nominalism. Both objections stress different aspects of the apparent fact that modal nominalism violates the Second Philosopher's entreaty to *understand* mathematics on its own terms. On the one hand, it is not a built-in feature of modal nominalism that mathematical forms of justification suffice for justifying the modal claims modal nominalists make about mathematics. On the other hand, the construction and advancement of modal nominalist views is not conducted using only the methods of mathematics but instead appeals to explicitly philosophical resources and forms of motivation. The remainder of the chapter is given over to providing answers to the following questions: How damaging are these objections to modal nominalism? Do they show that modal nominalism is incompatible with Second Philosophy? Do they show that modal nominalism is incompatible with other, weaker forms of naturalism? And do they show that modal nominalism is in any kind of malevolent conflict with mathematics or its practice?

In section three I consider the objection that modal nominalism does not confirm the idea that mathematical forms of justification suffice for justifying the modal claims modal nominalists make about mathematics. Unfortunately Maddy has not directly addressed this issue in the course of the construction and elaboration of Second Philosophy. Nevertheless how she would compartmentalize the transgression the modal nominalist makes can be gleaned from the objections she has raised against other views. I argue, through an analysis of Maddy's own examples, that it is not inherently objectionable to produce a metaphysical account of mathematics that merely fails to uphold or incorporate the idea that mathematical forms of justification suffice for justifying metaphysical claims about mathematics. Metaphysical accounts of mathematics are only objectionable when

they straightaway *preclude* the possibility that mathematical forms of justification suffice for justifying metaphysical claims about mathematics. Ultimately the concern here is not that such metaphysical accounts violate Maddy's entreaty to *understand* mathematics on its own terms; instead the concern is that such metaphysical accounts violate Maddy's entreaty to *evaluate* mathematics on its own terms. I argue that modal nominalism, at least in certain of its formulations, is such that it does *not preclude* the possibility of using mathematical forms of justification to justify the modal claims modal nominalists make about mathematics, and moreover that modal nominalism can be coherently amended so as to accommodate Maddy's claim that considerations of depth or fruitfulness play an important evidential role in mathematics. Thus, modal nominalism does not engender any kind of conflict with mathematical practice.

In section four I consider the objection that modal nominalism cannot be described or motivated using only the methods of mathematics. In order to understand what is purportedly beneficial about views that *are* capable of being described and motivated using only the methods of mathematics, I discuss the two views Maddy takes to have inherent affinities with mathematical practice—Thin Realism and Arealism. Maddy argues that there is no substantive difference between Thin Realism and Arealism, and this argument takes advantage of the claim that the facts of mathematical depth are *the* facts that matter when it comes to appraising accounts of mathematics. Thus, it is a hypostasized feature of Second Philosophy that theorizing about mathematics is only legitimate when such theorizing is sanctioned or contained by mathematical methods. Therefore modal nominalism is *not* compatible with Second Philosophy. Is the incompatibility of modal nominalism with Second Philosophy a reason to reject modal nominalism? In sections five and six I make a provisional case for answering this question in the negative.

In section five I question the overall coherence of Maddy's naturalism, given that it requires a rather strong interpretation of what it means to understand mathematics on its own terms. I am only able to find one genus of evidence in support of the idea that

metaphysical accounts of mathematics must be contained by the methods of mathematics. This is that using extramathematical resources in the construction and advancement of metaphysical accounts of mathematics places the metaphysicist at an increased risk of rejecting mathematical results and methods on non-mathematical grounds. But, as I shall have argued in previous sections, this risk does not inhere in all metaphysical accounts of mathematics. In particular, it does not inhere in all modal nominalist accounts of mathematics. That Maddy nevertheless continues to object to all extramathematical methods seems to me to be evidence that Maddy's entreaty to understand mathematics on its own terms is *itself* a philosophical claim, as opposed to a mathematical claim. As such, this entreaty is either self-undermining (if posed as a claim about mathematics and its methods), or special-pleading (if posed as a philosophical claim about mathematics). Either way, this component of Second Philosophy appears to be implausibly strong. Why is the Second Philosopher permitted to make *this* philosophical claim about mathematics, while at the same time modal nominalists are not permitted to make the kinds of philosophical claims about mathematics they wish to advance?

Section six closes the chapter and dissertation with a brief discussion about the compatibility of modal nominalism with weaker forms of naturalism. I argue that modal nominalism is compatible with forms of naturalism that embrace the entreaty to *evaluate* mathematics on its own terms, and further that modal nominalism is compatible with forms of naturalism that seek to find a place for the methodology of mathematics *alongside* the metaphysics of mathematics. Modal nominalism is only incompatible with Maddy's particularly strong version of naturalism, which unnecessarily runs together a study of the methods of mathematics and a study of the metaphysics of mathematics.

But what precisely does Second Philosophy have to say about mathematics? Why in particular does Maddy run together mathematical methodology and mathematical metaphysics? And what might this mean for modal nominalism? Let me turn now to these issues.

5.2 Second Philosophy of Mathematics

Second Philosophy embodies Maddy's characterization of the "fundamental naturalistic impulse," which is, "the conviction that a successful practice should be understood and evaluated on its own terms" (Maddy 1997, 201). For Maddy's naturalist, then, the only legitimate methods are those that come from successful practices. Though she does not offer the notion of a "successful practice" up for analysis, she intends the term to refer to the set of open-ended, constantly evolving, and well-confirmed methods used in empirical science (Maddy 2011, 39). Science therefore is not to be trusted *because* it is science; rather, science is to be trusted to the extent that it employs the best of the available evidential practices.¹

The Second Philosopher becomes interested in mathematics because it plays a vital role in many of her best scientific theories and evidential practices. But at this point the Second Philosopher makes an observation that would be somewhat alien to her Quinean predecessors—she sees that mathematics is practiced *autonomously*, using methods quite different from those used in empirical science.² Mathematics is thereby to be counted as a successful discipline in its own right, and consequently compels the Second Philosopher to understand and evaluate mathematics on its own terms.

Unfortunately, Maddy does not explain in much detail, at least in a general way, what it means to understand and evaluate mathematics on its own terms, but instead relies on numerous examples and case studies that she takes to exemplify the kinds of naturalistic scruples with which the Second Philosopher identifies. For this reason I am not certain that it is possible to fully understand and appreciate Maddy's naturalism *without* surveying the specific examples and case studies she discusses. Nonetheless it should be helpful to

¹Some take this to be a glaring defect—what prevents a theologian or an astrologer from reckoning their evidential practices as producing successes in their own disciplines? What ultimately distinguishes the methods of these disciplines from the "authoritative" methods used in empirical science? For this kind of criticism, see (Dieterle 1999), (Marfori 2012), (Rosen 1999), and (Weir 2005). For a response, see (Maddy 2007,345-7) and (Tappenden 2001).

²Maddy further argues that the autonomy of mathematics is an important aspect of its usefulness to science (2007, 330-1).

identify and describe up front what kinds of naturalistic scruples are embraced by the Second Philosopher. In doing this I ask the reader's indulgence—Second Philosophy is a particularly strong version of naturalism and it may not be initially obvious why Maddy adopts the stronger of the principles described below.

Consider first Maddy's entreaty that mathematics be *evaluated* on its own terms. Essentially what this means is that mathematical results and forms of reasoning are not to be criticized on extramathematical grounds—sound mathematical results and methods, once established, are immune to criticism from outside of mathematics. To use a simplistic example, it would be a failure to evaluate mathematics on its own terms if one were to reject Euclid's proof of the existence of infinitely many prime numbers solely on the basis of the prior conviction that mathematical objects do not exist. It would also be a failure to evaluate mathematics on its own terms to reject non-constructive forms of mathematical reasoning on the basis of the kinds of metaphysical and epistemological concerns that motivate intuitionists.³ The Second Philosopher, then, objects to all accounts of mathematics that involve the rejection, on non-mathematical grounds, of established mathematical results and methods. Let me label these two transgressions as "result-rejecting" and "method-rejecting," respectively. Avoiding both method- and result-rejecting is a necessary, but not sufficient, condition for a philosophical account of mathematics to be consistent with Second Philosophy.

Consider next Maddy's entreaty that mathematics be *understood* on its own terms. The basic idea here is that various issues related to mathematics and its practice, including an account of the subject-matter of mathematics, are to be identified and described using *only* the methods of mathematics. But Maddy's naturalism does not only place reins on the admissible *methods* for conducting inquiries about mathematics, it also places restrictions on the legitimate *subjects* of inquiry. Roughly, a subject of inquiry is only legitimate when

³It would not, of course, be a failure to evaluate mathematics on its own terms to pursue intuitionistic mathematics as a form of mathematics in itself—the problem is with those who seek to *replace* classical mathematics with intuitionistic mathematics.

its investigation facilitates the realization of some identifiable mathematical goal. If there are no perspicuous mathematical reasons for investigating X , then the Second Philosopher denies that there is any motivation for or wisdom in investigating X .⁴ I will use the term “method-contained” as a label for accounts of mathematics that embrace Maddy’s entreaty that mathematical methods are to place firm restrictions on the admissible methods and subjects of inquiry for investigating mathematics.

There is a weaker notion that Maddy sometimes invokes when attempting to “understand” mathematics on its own terms. This is the idea that a required feature of philosophical accounts of mathematics is that they either actively pursue or leave open the possibility of explaining how it is that mathematical forms of justification—which, Maddy claims, are sufficient for establishing mathematical results and are innocent of any philosophical prejudices—are furthermore *sufficient* for generating reliable beliefs about the underlying reality of mathematics. In other words, it must be possible to show, without using extramathematical resources, that the mathematical facts—according to the philosophical accounting of these facts—are in genuine agreement with established mathematical results. For instance, if a philosophical proposal says that mathematics describes a realm of abstract mathematical objects, then this proposal is only compatible with Maddy’s naturalism if it is plausible that mathematical forms of evidence—*unsupplemented* by philosophical views about, e.g., *a priori* knowledge—are sufficient for justifying claims about abstract mathematical objects. I will use the term “method-affirming” to describe philosophical accounts of mathematics that are consistent with Maddy’s entreaty that mathematical forms of evidence are sufficient in the way just described.

Though it is reasonably clear that avoiding both method- and result-rejecting are necessary conditions for compatibility with Maddy’s naturalism, it is less clear that being both method-affirming and method-contained are necessary conditions for compatibility with Maddy’s naturalism. Perhaps these latter two are sufficient, but not necessary, condi-

⁴Unless, of course, X is relevant to some other naturalistically legitimate area of inquiry, e.g., chemistry or physics.

tions for being consistent with Maddy's naturalism. What is of primary interest here is how these four conditions can be used to support or criticize philosophical accounts of mathematics, and, in particular, how these conditions might be used to support or criticize modal nominalist accounts of mathematics. In a moment I shall lodge the provisional Second-Philosophical objection that modal nominalism fails to be both method-affirming and method-contained. The bulk of this chapter will be given over to understanding just how objectionable (or not) modal nominalism is on account of its apparent failures to be both method-affirming and method-contained. But before developing this criticism, it should be worthwhile to examine Maddy's own outlook on the metaphysics of mathematics in closer detail. Doing this should help shed further light on what is involved in being method-contained and method-affirming, and will provide important background information in support of the claim that modal nominalism fails to be both method-affirming and method-contained.

Maddy's particular focus within mathematics has been on the methodology of the pursuit of new axioms for set theory. A persistent feature of this work involves the development of an account of mathematics that is what I have described as being method-contained and method-affirming. For Maddy, whatever actually constrains or influences mathematical work should be accorded the status of the underlying reality of mathematics. And based on her observations of the work of leading set theorists, what actually constrains mathematical practice is not, contrary to traditional platonist accounts of mathematics, some independently-existing realm of timeless or eternal abstract mathematical objects, but instead a collection of objective facts about what makes for deep or fruitful mathematics—the facts of mathematical depth:⁵

... this account of the objective underpinning of mathematics—the phenomenon of mathematical fruitfulness—is closer to the actual constraint experienced by mathematicians than any sense of ontology, epistemology or semantics; what presents itself to them is the depth, the importance, the illumination provided by a given mathematical concept, theorem, or method. A mathematician may

⁵Thanks to Susan Vineberg for suggesting this way of framing Maddy's project.

blanch and stammer, unsure of himself, when confronted with questions of truth and existence, but on judgments of mathematical importance and depth he brims with conviction. For this reason alone, a philosophical position that puts this notion center stage should be worthy of our attention. (Maddy 2011, 116-7)⁶

Here Maddy trades on the idea that since mathematics is an autonomous discipline the relevant evidence for or against a mathematical claim never involves exterior philosophical input but is instead all and only *mathematical* evidence. One of her central claims is that considerations of fruitfulness play an important evidential role in mathematics—e.g., the acceptability of a new axiom or concept is a function of whether the proposed axiom or concept advances the practice in a tangible, objective way. Moreover, proper advancement consists not in realizing arbitrary or merely fashionable mathematical goals (Maddy 2011, 81), rather, “mathematical goals are only proper insofar as satisfying them furthers our grasp of the underlying strains of mathematical fruitfulness” (ibid., 82). Clearly, then, Maddy views judgments about fruitfulness and importance as part of sound mathematical methodology. This methodology can thereby function as a guide to mathematical metaphysics—the underlying reality of mathematics is given by the facts of mathematical depth, and one important function of mathematical methodology is to reveal these facts.⁷

Maddy provides the group concept as one example of a mathematically deep concept:

In the logical neighborhood of any central mathematical concept, say the concept of a group, there are innumerable alternatives and slight alterations that simply aren't comparable in their mathematical importance. . . 'group' stands out from the crowd as getting at the important similarities between structures in widely differing areas of mathematics and allowing those similarities to be developed into a rich and fruitful theory. . . 'group' effectively opens the door to deep mathematics in ways the others don't. So what guides our concept formation, beyond the logical requirement of consistency, is the way some logically possible concepts track deep mathematical strains that the others miss.

⁶Cf. David Corfield's realism about 'inherent structure' (2003, 31).

⁷Thanks again to Susan Vineberg for suggesting this way of presenting Maddy's views on methodology and metaphysics.

(ibid., 79)⁸

Similar considerations are mustered in support of various new axiom candidates for set theory, including the historical case for the Axiom of Choice, and the more contemporary cases for several of the (less than full-blown) determinacy axioms that are thought to be consistent with Choice (Maddy 2011, 80-1).

It is difficult to say precisely what kinds of claims that considerations of depth or fruitfulness are alleged to justify or support. Does the fact that the group concept is mathematically deep justify the truth *simpliciter* of claims about groups, including existence claims? Or does the fact that the group concept is mathematically deep justify only the more pragmatic claim that studying groups helps to realize various kinds of mathematical goals? Or something else entirely? As I shall explain in more detail later in the chapter, Maddy's official position is that answering these questions falls outside the province of mathematics, and given her advocacy of method-contained accounts of mathematics, there is consequently no fact of the matter about how they should be answered. On her analysis, all that mathematical methods strictly license are depth claims and mathematical claims unadorned by any kind of philosophical interpretation. (I should note here that Maddy *denies* that mathematical methods positively support the idea that mathematics is comprised of a body of truths. More on this and related issues in §4 and in §5.)

In any case, one might worry that Maddy's observations about the role of fruitfulness in concept formation and axiom selection do not generalize and arise because of idiosyncratic features of set theory and group theory. Some have indeed complained that Maddy's approach to methodology is unduly influenced by her focus on set theory and that this approach risks becoming itself irrelevant to the rest of mathematics.⁹ However there is evidence that fruitfulness has a role to play in mathematical concept formation more generally. For instance, Jamie Tappenden points to several examples in number theory and abstract algebra (2008a, 2008b); meanwhile, David Corfield offers examples from algebraic

⁸For a similar observation about the group concept, see (Corfield 2003, 30).

⁹See, e.g., (Riskin 1994) and (Decock 2002).

topology (2003). If Tappenden and Corfield are correct, Maddy's approach to methodology in set theory—that proper method is constrained by considerations of mathematical fruitfulness—is likely to have exemplifications throughout the various subdisciplines of mathematics. Thus the claim that mathematical methodology (along with the purported metaphysical implications of this methodology) responds to fruitfulness considerations is unlikely to constitute a violation of any of the four conditions described above.

5.2.1 Two Provisional Second-Philosophical Objections to Modal Nominalism

Identifying the various naturalistic principles at work in Second Philosophy provides a means for determining how alternative accounts of mathematics can come into conflict with Maddy's naturalism. If modal nominalism is inconsistent with Second Philosophy, then it stands to reason that this is because modal nominalism is either method- or result-rejecting, or else modal nominalism either fails to be method-affirming or fails to be method-contained. Here I raise two objections to modal nominalism. First, that modal nominalism is not method-affirming, and second, that modal nominalism is not method-contained. Both objections are offered as evidence that modal nominalism is incompatible with Second Philosophy. The extent to which these objections are damaging to modal nominalism will be explored in the remaining sections of the chapter.

I think it is important to recall that my project in this dissertation—to defend the modal nominalist approach in philosophy of mathematics—is not to decisively establish the major nominalist thesis that mathematical objects do not exist. Rather, my aim is to show that modal nominalist *theories* are coherent and are capable of overcoming the kinds of objections that Stewart Shapiro, Burgess, Maddy, and others raise (or might raise) against them. (I do of course hope that, along the way, I have offered, and will continue to offer, reasons for supposing that modal nominalist theories are preferable to the competition.) It seems unlikely that the major thesis of nominalism can be established without substantive appeal to simplicity principles like Ockham's razor, but I reject the idea that simplicity principles are required for advocating *modal nominalist theories*—theories that are consistent

with, but do not require, the non-existence of mathematical objects. Other motivations are available for modal nominalist theories.

Now the alternate motivation I have proposed at various times in this dissertation—restraining from making existence assertions without proper evidence—does seem of a piece of the general idea, much a part of any plausible form of naturalism, that one should only make assertions when one's evidence adequately supports one's assertions. Indeed, Maddy's own case studies suggest, contra Quine's Indispensability Argument for the existence of mathematical objects, that the indispensable inclusion of a posit in a scientific theory that is in possession of the "scientific virtues" is not by itself the right kind of evidence for drawing the conclusion that the posited object exists (Maddy 1997, 133-57). For instance, scientific idealizations, including the continuity of spacetime, may perform an indispensable role in scientific theorizing, nevertheless scientists consider it an open question as to whether spacetime is actually continuous (*ibid.*, 151-2). Similarly, the atomic theory did not gain widespread acceptance in the scientific community until Perrin's experiments provided it with decisive observational support, even though prior to these experiments it could have been argued that the atomic theory was in possession of the "scientific virtues" (*ibid.*, 135-43). So my sympathy for modal nominalist theories is analogous to Maddy's position concerning scientific idealizations. Further, Maddy herself acknowledges that mathematical methods do not support robust answers to questions about the existence or not of mathematical objects. This confirms an oft-voiced claim of this dissertation, *viz.*, that it is no presumption of mathematics proper that mathematical objects exist. This suggests a somewhat more explicit motivation for modal nominalist theories—that they facilitate the goal of providing an account of the content of mathematical theories without making such a presumption either.

Nevertheless it is unclear that this manner of motivating the modal nominalist approach is capable of positively supporting any of the particular metaphysical claims modal nominalists make about mathematics. Modal nominalists, even though they need not

appeal to Ockhamite considerations, nevertheless appeal to *more* than just the methodology of mathematics in the construction and advancement of their views. For instance, Hartry Field imposes the judgment that mathematical knowledge is a combination of modal and logical knowledge (1989c). Meanwhile Geoffrey Hellman suggests that, “*mathematics is the free exploration of structural possibilities, pursued by (more or less) rigorous deductive means*” (1989, 6). But, on the Second Philosopher’s analysis, mathematical methods themselves support only very limited metaphysical claims about the content of mathematics, *viz.*, claims about the fruitfulness or depth of various axioms and concepts. Thus, under Second Philosophy, the underlying reality of mathematics consists neither in the truths of logic nor in the facts about structural possibilities, but instead in the facts of mathematical depth. It follows that Field’s fictionalism and Hellman’s Modal Structuralism are *not* endemic to the methods of mathematics. Therefore both fictionalism and Modal Structuralism fail to be method-contained, and therefore appear to be incompatible with Second Philosophy.

Unlike Field and Hellman, Charles Chihara’s Constructibility Theory does not provide an account of the underlying reality of mathematics but instead offers a framework for carrying out mathematical reasoning in a way that (a) treats mathematics as a body of truths, and (b) is not committed to the existence of mathematical objects.¹⁰ Still, it is unclear why a Second Philosopher should be moved to reconstruct mathematical reasoning so as to avoid reference to mathematical objects. If it is correct that the existence of mathematical objects is no presumption of mathematics but is instead a certain kind of *philosopher’s* presumption about mathematics, then the impulse to combat this philosophical presumption is not a source of motivation for Constructibility Theory that is endemic to the methods of mathematics. Thus, Constructibility Theory itself is not endemic to mathematical methodology. Therefore, Constructibility fails to be method-contained and consequently appears to be incompatible with Second Philosophy.

All three of these views also fail to be method-affirming. It is not a built-in feature of ei-

¹⁰Of course, Hellman is also attempting (a) and (b), and Field (b), but each is doing more besides—offering an account of the underlying reality of mathematics.

ther Modal Structuralism, fictionalism, or Constructibility Theory that mathematical forms of justification—which are, allegedly, responsive to the facts of mathematical depth—are sufficient for justifying the content of the modal nominalist *interpretations* of mathematical claims. It is not clear, for instance, why the alleged fact of the fruitfulness of the ZFC axioms is thereby evidence that ZFC is primitively logically possible (in Field’s and Hellman’s cases), or is thereby evidence for the constructibility claims that Chihara would advance in place of the ZFC axioms. That modal nominalism fails to be method-affirming, then, presents a further ostensible reason for supposing that modal nominalism is incompatible with Second Philosophy.

That modal nominalism fails to be both method-contained and method-affirming forms the basis for two arguments which purport to unveil modal nominalism as incompatible with Second Philosophy. But how damaging are these conclusions? My goal in the remainder of the chapter is to chart the extent of the damages by determining how the Second Philosopher would answer the following questions, and whether the Second Philosopher is justified in giving the answers that she would in fact give: Is the alleged incompatibility of modal nominalism with Second Philosophy a mere flesh wound or is it evidence of an underlying, mortal defect of modal nominalism? In particular, are the purported facts that modal nominalism fails to be method-contained and fails to be method-affirming evidence that modal nominalism not only conflicts with Second Philosophy, but also with mathematics itself? That is, are these facts evidence that modal nominalism is also method- and result-rejecting? Unfortunately, Maddy nowhere directly addresses these questions. She does however address similar questions regarding various alternative accounts of mathematics. So before I can address the precise nature of the difficulties (or lack thereof) that modal nominalism faces on account of its purported failure to be both method-contained and method-affirming, it is first necessary to consider Maddy’s actual uses these naturalistic scruples in judgment against alternative accounts of mathematics.

In the next section I use Maddy's objections to competing accounts of mathematics to determine whether accounts of mathematics that fail to be method-affirming are thereby genuinely incompatible with Second Philosophy, and also to determine what other kinds of objections might be raised against such accounts. What I find is that Maddy's decisive objection to most competing accounts of mathematics is not that they fail to be method-affirming, but instead that they are either method- or result-rejecting in a *malevolent* way. (N.B., there are *benevolent* forms of method- and result-rejecting.) It is possible for an account of mathematics to fail to be method-affirming and yet avoid both method- and result-rejecting. In §4 I use Maddy's discussion of her favored views—Thin Realism and Arealism—to show that being method-contained is a genuine requirement as far as consistency with Second Philosophy is concerned, but that accounts of mathematics that fail to be method-contained are not for that reason either method- or result-rejecting. These developments are used in §5 to provide a more general appraisal of Maddy's naturalism, and are also used in §6 to determine the compatibility of modal nominalism with various forms of naturalism that relax some of Maddy's stronger naturalistic principles.

5.3 The Method-Affirming Objection to Modal Nominalism

Given that modal nominalism fails to be method-affirming, should this count as a decisive reason to reject modal nominalism, even from the Second-Philosophical perspective? Why should being method-affirming be considered a virtue for accounts of mathematics? In this section I argue that being method-affirming is virtuous primarily for its prophylactic qualities—method-affirming views run very little risk of method- and result-rejecting. In order to appreciate this point it is necessary to examine some of Maddy's objections to various views that fail to be method-affirming—this should function as an aide to understanding the ways in which accounts of mathematics do and do not come to be method- and result-rejecting.

5.3.1 A Miscellany of Objections

Consider Maddy's analysis of the array of positions she refers to by the blanket term 'Robust Realism.' The "defining feature" of Robust Realist theories is that they view mathematics as, "the study of some objective, independent reality" and that they analyze the justification for beliefs about this reality as arising *other than* trivially from sound mathematical reasoning (Maddy 2007, 365). This reality—the entities about which such theories profess realism—could be comprised of abstract mathematical objects, of mathematical structures, of modal facts, or of something else entirely (Maddy 2011, 56-7). The basic idea unifying Robust Realist theories is that purely mathematical reasons are *not sufficient* for justifying mathematical assertions, because the existence or not of mathematical objects or structures (or the obtaining or not of modal facts) is independent from mathematical theorizing—theorizing that, according to Maddy, tracks facts about what makes for deep or fruitful mathematics. It should be obvious that Robust Realism thereby fails to be method-affirming (in addition to failing to be method-contained). Interest here is in what is objectionable about Robust Realism *in virtue of* its failing to be method-affirming. Maddy argues that Robust Realism consequently faces two related sets of difficulties.

The first problem is the creation of epistemological difficulties. If mathematical methods are *not sufficient* for justifying claims about the underlying reality of mathematics, then the source of knowledge about this underlying reality must come from somewhere other than the methods of mathematics. Consider traditional platonism. What the platonist requires is some *independent* reason for thinking that the mathematical realm is genuinely in agreement with the accepted assertions of mathematics. But it is doubtful that the platonist can provide such reasons without appealing to naturalistically occult faculties (e.g., *a priori* forms of justification). Thus the platonist seems unable to justify, on naturalistically acceptable grounds, that mathematical objects exist, making it something of a mystery how anyone is supposed to justify assertions that refer to these objects. The *general* moral for Robust Realism is that since there is a gulf between mathematical methods and the Robust

Realist's underlying reality, "we have no way of ruling out the possibility that reality is sadly uncooperative, that [in the case of set theory] much as we'd like to use sets in our mathematical pursuits, they just don't happen to exist" (Maddy 2011, 58). Nevertheless this does not show that the Robust Realist is, in principle, in material disagreement with mathematics—the Robust Realist might be lucky to defend a metaphysical position that perfectly matches the accepted assertions of mathematics. But Maddy thinks there would be something awry even if the Robust Realist did wind up in material agreement with practice, which brings me to the second problem.

The second problem is that there is something untenable about the Robust Realist's initial isolation from method (even prior to the creation of the aforementioned epistemological difficulties). And this is that the very notion that mathematical assertions require non-mathematical supplementation is problematic under Second Philosophy:

But if the Robust Realist is right, if the goal of set theory is to describe an independently-existing reality of some kind, then it appears that Cantor's evidence needs supplementation, and not supplementation of the same sort, like adding in Dedekind's grounds and so on, but supplementation of an entirely different kind: we need an account of how the fact that sets serve this or that particular mathematical goal makes it more likely that they exist... To the Second Philosopher, this hesitation seems misplaced: why should perfectly sound mathematical reasoning require supplementation? Hasn't something gone wrong when rational mathematical methods are called into question in this way? (ibid., 58)

Maddy's assessment here is that, for the Robust Realist, mathematical evidence is not *good enough*, all things considered. In other words, the Robust Realist maintains that mathematical claims must be *evaluated* on extramathematical grounds. However, according to the Second Philosopher, mathematical methods are internally sufficient for justifying mathematical assertions. This is a feature of mathematical practice that Robust Realists do not simply *fail to affirm* but that they allegedly *reject*. Thus, Robust Realism is *method-*

rejecting, beyond its evident failure to be method-affirming.^{11,12}

Consider next Maddy's objection to Quine's indispensability-style realism about mathematical objects. Quine's view can be described as a form of Robust Realism in the sense that Quine elicits non-mathematical justification in order to describe the underlying reality of mathematics—which mathematical objects exist depend on what bits of mathematics get applied in empirical science. This means that Quine's realism threatens to be method-rejecting for the same reasons as Robust Realist views in general threaten to be method-rejecting: purely mathematical considerations are not given primary authority when it comes to justifying mathematical assertions.¹³ Nevertheless the Quinean realist (at least, Quine himself) commits a further transgression: Quine argues for the Axiom of Constructibility ($V = L$) as an extension of ZFC because it is an economical cut off point for the set-theoretic hierarchy—allegedly, $V = L$ can accommodate all of the mathematics that is needed for science.¹⁴ Maddy's detailed examination of set theory, however, suggests

¹¹Maddy describes Shapiro's structuralism, Hellman's Modal Structuralism, and Gödel's platonism each as a form of Robust Realism (ibid., 56-7). These analyses may prove problematic. Gödel seems willing to allow depth considerations a role in justifying mathematical axioms (though his views on mathematical intuition seem to support Maddy's description of him as a Robust Realist). Hellman does not attempt to construct an epistemology for his primitive modal claims, but I think he is not appropriately described as a Robust Realist for reasons I will give later in the chapter (at least, if Hellman is a Robust Realist, then I am not convinced that the objections presented here apply to all Robust Realist views). Shapiro's coherence-style justifications for the existence of structures (see chapter two) is sufficiently non-trivial for the Robust Realist label to stick (a situation not helped by Shapiro's pattern-based epistemology). However, it is not clear that *ante rem* structuralism is method-rejecting, because it defers (perhaps controversially) to *mathematics* in order to determine what structures are coherent, and hence, what structures exist. It is an open question what views are genuinely Robust Realist in the sense of being method-rejecting, and hence in conflict with practice—so there is a possibility of using my defense of modal nominalism as a springboard for a defense of other kinds of philosophical accounts of mathematics.

¹²Similar reasoning is alleged to defeat the neo-Fregeanism espoused by Crispin Wright. According to Maddy, Wright's argument for thinking that set theory is truth-apt is that, "a minimalist truth predicate can be defined for any such discourse in such a way that statements assertable by its standards come out true," and that set theory, "enjoys certain syntactic resources and displays well-established standards of assertion" (ibid., 70). Thus, a minimalist truth predicate can be defined for set theory. But, "[i]n contrast, the Thin Realist take [sic] set theory to be a body of truths, not because of some general syntactic and structural features it shares with other discourses, but because of its particular relations with the defining empirical inquiry from which she begins" (ibid.). The objection, then, is that Wright's strategy for justifying the assertoric status of set-theoretic claims is in conflict with the strategy implemented by the Second Philosopher (in particular, the Second Philosopher *qua* Thin Realist). Wright's minimalist account of mathematical truth, then, is method-rejecting.

¹³And so Quine's realism is also trivially neither method-contained nor method-affirming.

¹⁴On this score, Quine writes that, "[s]o much of mathematics as is wanted for use in empirical science is for me on a par with the rest of science. Transfinite ramifications are on the same footing insofar as they

that there are sound mathematical reasons for rejecting $V = L$ in favor of much more expansive extensions (e.g., determinacy axioms that are consistent with choice). Thus, Quine's realism seeks to reject, on *extramathematical* grounds, a group of mathematical results or assertions that are nevertheless *mathematically* supported. Quine's realism, then, commits the further transgression of being *result-rejecting*.

Both method- and result-rejecting are potential factors in Maddy's assessment of fictionalism:

The central challenge is to delineate and defend the proper ways of extending what the fictionalist calls the 'set theoretic story', but calling it that, rather than just 'set theory', doesn't appear to advance our understanding on this point... the value of the fictionalist analogy is limited, and keeping it at the forefront of our thinking about set theory might tempt us to impose categories and judgments foreign to our subject and to ignore important features without correlates in fiction. (Maddy 2011, 98)

Note the entreaty to enact tariffs against fictionalism—treating mathematics as an elaborate fiction risks imposing foreign (i.e., extramathematical) judgments on mathematics, which would unveil fictionalism as result-rejecting or method-rejecting, depending on the nature of these judgments. For instance, the blanket judgment that Field makes—that all mathematical existence assertions are false—could be construed as result-rejecting. Alternatively, to insist (as a fictionalist might) that certain fictionalist notions, e.g., truth-in-a-story, apply in the assessment of mathematical assertions, could be construed as method-rejecting.

At this point it would seem that the more (and perhaps the only) decisive objection on offer for the views just entertained is *not* that they fail to be method-affirming, but *rather* that they engage in either method- or result-rejecting. This raises two questions. The first question concerns the substratal virtue of method-affirming accounts of mathematics—perhaps being method-affirming is a mere prophylactic against being either method- or result-rejecting. After all, merely failing to support a method is not equivalent to rejecting a method. Are there nevertheless substantive examples in which a failure to come of a simplificatory rounding out, but anything further is on a par rather with uninterpreted systems" (1984, 788).

be method-affirming *by itself* is responsible for some kind of objectionable conflict with mathematics? The second question concerns whether being method- and result-rejecting are the objectionable qualities that Maddy takes them to be. Are *all* forms of method- and result-rejecting naturalistically objectionable? Or are there benevolent forms of method- and result-rejecting? Let me address these questions in turn.

5.3.2 Method-Affirming as a Prophylactic

The failure to be method-affirming appears to factor prominently in Maddy's criticisms of if-thenist and Carnapian-framework-style accounts of mathematics. In reference to the suggestion that various axiomatizations of set theory merely constitute alternative set-theoretic "frameworks":

The trouble with this suggestion is that it fails to capture one of the elements of set-theoretic practice we're most eager to describe and assess: the addition of new axioms. On this view, one axiom wouldn't be selected over another for compelling set-theoretic reasons—these are all internal to the framework—but as a pragmatic, conventional decision to move from one linguistic framework to another. Obviously this is not a suitable path for the Second Philosopher. (ibid., 69)

Two problems are identified in this passage. The first is that the framework-style account of mathematics "fails to capture" an important element of set-theoretic practice. The second is that the pragmatic-conventional-style justifications given under the framework-style account of mathematics undercut "compelling set-theoretic" forms of justification. The second problem is simply that this account of mathematics is method-rejecting. But what about the first problem? That the framework-style account of mathematics fails to capture the methodology of axiom addition implies that the account cannot capture the idea that mathematical forms of justification—which respond to considerations of fruitfulness—suffice for justifying mathematical claims. This, then, is an accusation that the framework-style account of mathematics fails to be method-affirming. But what precisely is the force of this component of the objection? Is the framework-style account objectionable simply because it fails to be method-affirming, or because its failure to be

method-affirming is somehow related to its being method-rejecting? A clue perhaps arises in Maddy's discussion of if-thenism.

If-thenism, also known as deductivism, is the view that mathematics can be described as the study of what follows from what. The if-thenist reinterprets all categorical mathematical assertions as conditionals—instead of “the Well-Ordering Theorem is true,” the if-thenist instead claims that “if the axioms of ZFC are true, then the Well-Ordering Theorem is true,” or that “if the set-theoretic hierarchy exists, then every set can be well-ordered.” If-thenism is compatible with agnosticism about (or even disavowal of) the truth of the antecedents of such conditionals. Maddy's objection to if-thenism, at least in its “crude” form, is that

... though mathematicians are often engaged in proving one thing from another, they obviously don't regard any starting point, even any consistent starting point, as equally worthy of investigation; if one characterizes set-theoretic practice as that of deriving theorems in one or another axiomatic setting, one ignores the very features of that practice that have been [my] focus, namely, the forces that shape the concepts and assumptions of the setting itself. (ibid., 99)

If-thenism, like the framework-style account, does not capture the methodology of axiom addition. Thus, if-thenism does not support the idea that mathematical forms of justification—which respond to considerations of fruitfulness—suffice for justifying mathematical claims. If-thenism consequently fails to be method-affirming. However, Maddy does not believe that this is a life-threatening diagnosis in the case of if-thenism. That if-thenism fails to capture the methodology of axiom addition is only debilitating for those who offer if-thenism as a complete and non-amendable account of the nature and methodology of mathematics. Maddy admits to a degree of sympathy with a more “sophisticated” if-thenism that admits

... that mathematics is more than a matter of determining what follows from what, that mathematicians are also engaged in forming those concepts and selecting those assumptions, and would then assume responsibility for explaining how this process is constrained, what principles should guide it and why. (ibid.)

So long as the if-thenist acknowledges her methodological lacunae, her view is not ultimately objectionable (even to the Second Philosopher). It would thus appear that, even under Second Philosophy, *merely* failing to include the methodology of axiom selection is not grounds for dismissal, provided that it is possible to supplement the view under consideration with an acceptable account of this methodology. So what distinguishes the if-thenist from the framework-stylist is that latter precludes, but the former does not preclude, using internal set-theoretic modes of argument to arrive at methodological judgments, *where the force of these judgments is more than solely pragmatic*. The if-thenist can, then, acknowledge non-pragmatic, mathematical reasons for or against formulating new concepts and adopting new axioms. But the framework-stylist denies in principle that there exist any such non-pragmatic reasons (or at least that any such reasons could be given within a set-theoretic framework). Though both views fails to be method-affirming, only the framework-stylist view is thereby also method-rejecting.¹⁵

The framework-stylist, then, removes any possibility of supporting the objective existence of the facts of mathematical depth. Even if Maddy is incorrect to suppose that the ultimate, underlying reality of mathematics consists of the facts of mathematical depth, one could still nevertheless agree that there are such facts and that they do provide an objective constraint on mathematical work in the sense that considerations of fruitfulness play an important evidential role in mathematical practice. Any view which denies the objective existence of the facts of mathematical depth would thereby appear to involve some form of method- or result-rejecting. In other words, views that in-principle *prohibit* legitimizing sound method—including the existence of the objective facts of mathematical depth as a

¹⁵Can it be objected that Maddy's account of mathematics is equally pragmatic? I am not certain. Her objection is that the framework-stylist has it wrong in saying that it is a mere conventional decision when it comes to selecting an extension of ZFC. On the other hand, Maddy describes the underlying reality of mathematics as a set of facts about what makes from *fruitful* mathematics. Is this a substantive distinction? Maddy suspects so—she believes that it is an open question for the framework-stylist which extension of ZFC is most fruitful; for Maddy this is not open—the facts of mathematical depth determine which extension is most deep. This distinction turns on the assumption that the framework-stylist cannot admit *objective* facts about the comparative worth of her available choices concerning extensions of ZFC. It is not obvious to me that the framework-stylist cannot admit such facts, but I do not have space to pursue the matter.

component of sound method—are guilty of being either method- or result-rejecting. In the case of such views, although the initial evidence of trouble may come from observing that they fail to be method-affirming, deep down the problem is that they are method-rejecting. In the case of framework-style accounts, their failure to be method-affirming serves to expose covert instances of method-rejecting. Not so with if-thenist accounts. Their failure to be method-affirming does not arrogate the forthright rejection of if-thenism, but merely serves to indicate a provisional deficiency. Something of a challenge is presented: For the if-thenist to explain how her account of mathematics can be supplemented to become *method-affirming*, i.e., for the if-thenist to explain how mathematical forms of justification distinguish the mathematically salient “if”s from the mathematically sterile “if”s.¹⁶

5.3.3 Method- and Result-Rejecting: The Good and The Bad

I have thus far taken for granted that result- and method-rejecting are, at least from a naturalistic perspective, inherently objectionable activities. But it is not obvious that all method- and result-rejecting accounts of mathematics are necessarily objectionable or in any kind of *malevolent* conflict with mathematics and its practice. For instance, Russell’s discovery of the set-theoretical paradoxes is plausibly described as a result- and method-rejecting episode in the history of philosophy of mathematics, but the sense in which this discovery interfered with mathematics was beneficial, rather than malevolent.¹⁷

The case of Russell’s discovery of the set-theoretical paradoxes is evidence that the Second Philosopher and naturalists more generally share an interest in maintaining that not *all* forms of philosophical interference with mathematics are bad. What makes Russell’s method- and result-rejecting acceptable, but, e.g., Quine’s method- and result-rejecting unacceptable? I suspect that Maddy’s answer would be that Russell’s intervention helped

¹⁶It should be noted that Hellman’s Modal Structuralism is a modalized version of if-thenism, which treats its conditionals (e.g., necessarily, if there exists a model of ZFC, then the Well-Ordering theorem is true in the model) as non-trivially satisfied by modal-existence postulates (e.g., it is possible for there to exist a model of ZFC). See below for my thoughts on why Hellman’s position does not preclude the same kind of “sophisticated” amendment that is available to the if-thenist.

¹⁷Thanks to Susan Vineberg for suggesting this example.

to excise a mathematically *fruitless* line of research (naïve set theory),¹⁸ whereas Quine's conflict threatened to excise mathematically *fruitful* lines of research (large-cardinal and determinacy axioms). The moral suggested here is that conflicts with practice are only objectionable when they threaten to direct practitioners away from fruitful avenues of research—either by excising genuinely fruitful lines of research, or by forcing mathematicians onto fruitless lines of research. This means that method- and result-rejecting accounts, just like accounts that fail to be method-affirming, are not automatically objectionable. The only method- and result-rejecting accounts that are objectionable are those that call for practitioners to abandon or ignore mathematically fruitful research.

There is evidence, then, that the only accounts of mathematics that are naturalistically and Second-Philosophically objectionable are those that are either method- or result-rejecting in a malevolent way. The same appears to hold true for accounts of mathematics that fail to be method-affirming. The above discussion of if-thenism shows that the slope from failing to be method-affirming to method- and result-rejecting is not a slippery one—it is possible for an account of mathematics that fails to be method-affirming to nevertheless avoid malevolent forms of method- and result-rejecting. This suggests an avenue for defending modal nominalism against the charge that it fails to be method-affirming: To argue that modal nominalism fails to be method-affirming in a way that *does not* engender any malevolent forms of method- or result-rejecting.

5.3.4 The Method-Affirming Objection Reconsidered

Whether modal nominalism's failure to be method-affirming is evidence that modal nominalism is ultimately objectionable (to the Second Philosopher or to other naturalists) depends on whether there is evidence for any of the following claims: That there are clear obstacles to appending modal nominalism with an account of mathematical method; that modal nominalism calls for the rejection of mathematical methods or results; and that

¹⁸At least, Russell's intervention helped the mathematical community to realize that the unrestricted comprehension axiom of naïve set theory violated an already established mathematical precept—the requirement of logical consistency.

modal nominalism presents an underlying reality for mathematics that could be “sadly uncooperative” (*sensu* Robust Realism).

5.3.4.1 Clear Obstacles to Affirming Methods?

I am aware of no convincing reasons for supposing that either of the three modal nominalist strategies preclude the possibility of incorporating information which would help sort the mathematically interesting theories and structures from the mathematically uninteresting theories and structures. From Hellman’s and Field’s perspectives, there is nothing that *logically* distinguishes one mathematical theory or structure from another. But that is not to deny forthwith there are good mathematical reasons for opting to study one theory or structure over another. The issue here is whether Hellman and Field are, respectively, committed to the claims that mathematics is *just* the study of structural possibilities, and that mathematics is *just* an elaborate fiction. If so, then both views would appear to be method-rejecting. But whether Hellman and Field actually make such claims is immaterial: There is no reason why Hellman and Field could not take the relaxed attitude that recognizes a role for considerations of mathematical fruitfulness. Indeed, by Maddy’s own admission, that there are objective facts about what theories and concepts are mathematically deep is consistent with treating these theories and concepts as logically on a par (Maddy 2011, 83). From Chihara’s perspective, there is nothing that *metaphysically* distinguishes one true constructibility assertion from another. But again, that is not to deny forthwith that Chihara is incapable of recognizing good mathematical reasons for opting to study one kind of constructible theory or structure over another. It must be admitted that an account of mathematical method will not fall trivially out of the metaphysics of either of these three views, but the point here is that this does not itself preclude the possibility of theorizing about mathematical method *alongside* modal nominalist theorizing about mathematical metaphysics. No reason has been given for rejecting the possibility that modal nominalism, like if-thenism, admits of “sophisticated” amendment. Thus, no reason has been given for rejecting the idea that mathematical forms of justification could be used to distinguish

(and justify claims about) the mathematically salient modal claims.

5.3.4.2 Result- or Method-Rejecting?

Modal Structuralism presents the most promising candidate for a modal nominalist account of mathematics that avoids being both result- and method-rejecting. One reason for this is that Modal Structuralism is method-*affirming* in a number of respects. For instance, large tracts of algebra and topology focus almost exclusively on shared or structural properties of mathematical objects, rather than on the idiosyncratic features of, e.g., particular groups, rings, or topological spaces. Moreover, Modal Structuralism accommodates mathematicians' focus on abstraction, their absence of concern about foreseeable applications, etc.¹⁹ A second reason is that the background assumption Modal Structuralism makes—that mathematical theories and structures form subsets of the logical possibilities—is a background assumption that is *shared* by Maddy's depth-based account of mathematics.

Though fictionalism shares in the background assumption that mathematical theories form subsets of the logical possibilities, nevertheless the fictionalist position is somewhat riskier. As has already been discussed, there is a concern that indiscriminate reliance on the fictionalist metaphor runs the risk of both method- and result-rejecting. For this reason, fictionalism is the *least* promising candidate for a modal nominalist view that avoids result- and method-rejecting.

Recall that Chihara's Constructibility Theory does not attempt to describe the true content of mathematical theories. That is, he does not argue that mathematical existence assertions are, deep down, "really" constructibility claims. Instead, his motivation is to

... achieve for mathematics what philosophers of language hope to achieve for language: they seek to produce a coherent overall general account of the nature of mathematics (where by 'mathematics' I mean the actual mathematics practiced and developed by current mathematicians)—one that is consistent not only with our present-day theoretical and scientific views about the world and also our place in the world as organisms with sense organs of the sort characterized by our best scientific theories, but also with what we know about

¹⁹Thanks to Geoffrey Hellman for suggesting these examples.

how our mastery of mathematics is acquired and tested. (2004, 6).

Importantly, he notes that such activity should not, “contradict any of our prevailing views of science and scientific knowledge without very compelling reasons” (ibid., 2). If these remarks are to be trusted, Chihara’s account is neither method- nor result-rejecting. However, though it can be granted that Chihara does not *seek* to overturn mathematical results or undermine mathematical reasoning, it might be that the philosophical commitments of Constructibility Theory place mathematically arbitrary limits on the size or complexity of mathematical structures.²⁰ For instance, (Jacquette 2004) argues that Constructibility Theory cannot account for transfinite mathematics without relaxing its nominalistic scruples. That Constructibility Theory fails to be method-rejecting would seem to depend on the optimistic assumption that this view does not run into this kind of trouble.

It must be admitted that what has just been said is principally in defense of the three modal nominalist *theories* of mathematics, rather than a defense of the *motivations* Chihara, Field, and Hellman have used to develop these theories.²¹ Can anything be said in defense of the claim that the motivation for modal nominalism is neither method- nor result-rejecting? I am not certain if this is the best question to ask. As I have suggested earlier, what nominalists tend to object to in the nominalism/platonism dispute are the various *philosophical* presumptions platonists make about mathematics and mathematical language, e.g., that mathematical assertions are true *simpliciter*, or that mathematical truths presuppose or require the existence of mathematical objects. Thus, when the nominalist (modal or otherwise) objects to Quine’s indispensability argument, or to Frege’s analysis of mathematical language, she is not rejecting any of the methods or results of mathematics. Rather, she is objecting to a certain kind of *philosophical understanding* of mathematics. She might have one of several motivations for pursuing such objections: Some kind of *a priori* prejudice against abstract objects, some set of epistemological concerns about knowledge of abstracta, or the motivation to which I am most sympathetic: to account for the truths

²⁰Thanks to Susan Vineberg for bringing this issue to my attention.

²¹Thanks to Michael McKinsey for suggesting this clarification.

of mathematics in an ontologically innocuous way. But the basic point here is that, once it is granted (as Maddy seems happy to grant) that it is no presumption of *mathematics* that mathematical objects exist, it is difficult to insist that the motivation for nominalism (modal or otherwise) involves any kind of *mathematical* method- or result-rejecting.

Of course, if one takes seriously Maddy's entreaty that mathematics be understood solely on its own terms, then it is fair to ask why a naturalist of any stripe should be willing to embrace any of the nominalist motivations just described, given that they are principally *philosophical* motivations.²² But to pursue this line of reasoning further would be to object to nominalism (including modal nominalism) on the grounds that it fails to be method-contained, rather than on the grounds that it is either method- or result-rejecting. The method-contained objection to modal nominalism will be discussed in §4. However, I should think that it is largely immaterial whether the motivations Chihara, Field, and Hellman have actually proposed are method- or result-rejecting. That is because, even if these motivations are method- or result-rejecting, I believe that they can be replaced in favor of a motivation that is clearly *not* method- or result-rejecting: Namely, that modal nominalism facilitates the goal of providing an account of the content of mathematical assertions that, just like mathematics, does not require that mathematical objects exist. Of course, this goal is only worthy to the extent that modal nominalist analyses of mathematical assertions do not raise the same epistemological difficulties that the Robust Realist's analyses raise. Whether this is so shall now be considered.

5.3.4.3 Underlying Reality is Sadly Uncooperative?

Does modal nominalism raise the possibility that the underlying reality of mathematics—assertions about what is logically possible or what open-sentence tokens are constructible—is “sadly uncooperative” in the sense that one could be perfectly *mathematically* justified in asserting a mathematical claim, but nevertheless on that basis fail to be justified in making

²²In full disclosure, this observation also applies to motivations for philosophical accounts of mathematics more generally.

the modal assertion that provides the metaphysical grounds for this claim?

Again, I believe that Modal Structuralism presents the best example of a modal nominalist view for which this problem does not arise. The Modal Structuralist analysis of mathematical assertions (as describing structural possibilities) might appear to be more contentful than the analysis given by Maddy's depth-based account, but I am not convinced that this is actually the case. It must be remembered that Modal Structuralism is committed to the *primitive* logical possibility of the existence of models of mathematical theories. Thus the Modal Structuralist is committed to quite little in the way of a positive characterization of mathematical structures. I am inclined to read Hellman as providing the most metaphysically austere conception of structure on record.²³ Since Maddy acknowledges that the facts of mathematical depth canvass theories and concepts that constitute a subset of the logical possibilities (2011, 83), and further that classical logic and logical consistency are important constraints on mathematical practice (alongside the facts of mathematical depth) (*ibid.*, 78), not even the Second Philosopher is immune from the obligation to justify the same kind of possibility or consistency claims that underlie the Modal Structuralist's analysis of mathematics.

Now such justification will either accrue in the everyday practice of mathematics or it will require some kind of extramathematical supplementation. If the justification for logical possibility claims accrues in mathematics itself, then it is trivial that Modal Structuralism avoids the possibility that the facts of logical possibility are sadly uncooperative. But if the justification for logical possibility claims requires extramathematical supplementation, then Modal Structuralism faces the possibility that the facts of logical possibility are indeed sadly uncooperative—but then, *Maddy (and seemingly everyone else) faces this possibility as well.* So either mathematics itself provides sufficient reason for thinking that various theories and structures are logically possible, or else it is not possible to provide a full account of the constraints on mathematical practice by appealing solely to the methodology of

²³Cf. (Hellman 2005) and (McLarty 2008a, 2008b).

mathematics. Modal structuralism, therefore, is no more “uncooperative” than Maddy’s depth-based metaphysics.²⁴

Field’s fictionalism—at least its account of pure mathematics—can be given a similar defense to the extent that it interprets mathematical knowledge as a genus of logical knowledge. This knowledge comes in two forms: Knowledge of the consistency of mathematical theories, and knowledge about which theorems are logical implications of mathematical axioms. For Field, both consistency and implication are themselves understood to be primitive modal notions. Importantly, he does not take himself to be *replacing* the ordinary notions of consistency and implication with new, primitive modal notions. Rather, he argues that the ordinary notions of consistency and implication are already primitive modal notions. If that argument is correct, then Field, like Hellman and Maddy, can take advantage of internal mathematical justifications for assertions about what is logically consistent, if any such justifications are available.

Chihara’s Constructibility Theory poses the greatest risk of positing a metaphysics that is “sadly uncooperative.” In a sense, this is a pseudo-problem, because Constructibility Theory is not offered as a genuine account of the underlying reality of mathematics, but instead as an account of how it is possible to engage in mathematical reasoning without quantifying over mathematical objects. Nevertheless it can still be objected that the metaphysical modalities invoked by Chihara are uncooperative in the sense that Chihara cannot guarantee that it is metaphysically possible to construct a sufficient number and variety of open-sentence tokens to capture constructibility correlates of all accepted

²⁴Though it is correct to say that Maddy and Hellman should each be concerned to justify beliefs about the consistency of mathematical theories, it would not be correct to say that they both accept the same analysis of the logical and/or modal notions. Maddy adopts a rather unusual position on the status of classical logic (2007, 197-302). She believes that the logical truths of a system resembling Kleene three-valued logic can be justified through appeal to facts about the structure of the physical world and facts about innate human cognitive abilities. Classical logic is justified only when certain idealizing assumptions have been made (one being a prohibition against vague predicates); such idealizing assumptions are made because, “they make it possible to achieve results that would otherwise be impossible or impractical” (ibid., 288). Thus, on Maddy’s view, the ground of classical logic is ultimately pragmatic. Though I have very great reservations about this account of classical logic and the extent to which it avoids the epistemological problems raised by primitivism about logic and modal logic, I do not have space to describe them here.

mathematical results. It seems rather unlikely that the methods of mathematics have much to say about constructing open-sentence tokens, and it is therefore extremely doubtful that one could appeal to mathematics in order to justify the constructibility assertions that underlie Constructibility Theory.

I think the lesson here is that neither Maddy nor anyone else can preclude the possibility that modal facts *in general* are uncooperative.²⁵ So although the task before Chihara—justifying constructibility assertions—appears to be different in kind from the task before Hellman, Field, and Maddy—justifying logical possibility assertions, it is far from clear that these tasks are distinguishable on the basis of their level of difficulty. Indeed, one of the morals of chapter three is that both kinds of modal assertions raise a very similar set of metaphysical and epistemological problems. In any case, it can at least be maintained that Modal Structuralism and fictionalism present a metaphysics that is no more uncooperative than Maddy’s depth-based account, which means that it would be inappropriate to object to these views on the grounds that there is a great gulf between the modal facts and the accepted assertions of mathematics.

5.3.5 The Method-Affirming Objection: Coda

That modal nominalism fails to be method-affirming, then, does not constitute a compelling reason for thinking that modal nominalism is either method- or result-rejecting. (Though I acknowledge that this claim is strongest in the case of Modal Structuralism, since it is the only account that does not clearly face any of the three kinds of objections discussed above.) It is open to a modal nominalist to theorize about mathematical methodology *alongside* her metaphysical account of the nature of mathematical objects, and there is no *prima facie* reason to believe that these dual pursuits will conflict with one another. Just as in the case of if-thenism, there is nothing inherently problematic about the fact that modal nominalism fails to be method-affirming.

Given, then, that modal nominalism, in at least some of its formulations, is neither

²⁵This is a point that I will revisit in my response to the method-contained objection to modal nominalism.

method- nor result-rejecting, might it be further argued that modal nominalism is consistent with Second Philosophy? It must be admitted that, as far as compatibility with *mathematical practice* is concerned, being method-affirming functions as a mere prophylactic against method- and result-rejecting. Does a similar judgment hold for consistency with Second Philosophy? That a “sophisticated” if-thenism appears to be compatible with Second Philosophy (ibid., 99) suggests, by analogy, that modal nominalism is also compatible with Second Philosophy.

However, that modal nominalism is compatible with Second Philosophy is difficult to square with Maddy’s use the method-contained component of her entreaty that mathematics be understood on its own terms. To see why this is so it is necessary to examine the two views Maddy takes to be compatible with Second Philosophy—Thin Realism and Arealism—and it is also necessary to examine Maddy’s reasons for thinking that there is no substantive difference between these views. I shall argue that, as far as compatibility with mathematical practice is concerned, being method-contained is a mere prophylactic against method- and result-rejecting. Nevertheless the method-contained component factors prominently in Maddy’s assessment that, all things considered, there is no substantive difference between Thin Realism and Arealism. Therefore modal nominalism, because it fails to be method-contained, is not compatible with Second Philosophy.

5.4 The Method-Contained Objection to Modal Nominalism

Recall that the method-contained objection to modal nominalism states that modal nominalism is objectionable because modal nominalist accounts of mathematics are not endemic to mathematical methodology but instead require explicitly philosophical resources and motivation. Are views that fail to be method-contained objectionable for this very reason? And are they incompatible with Second Philosophy for this very reason? Or is failing to be method-contained, like failing to be method-affirming, just a defeasible indicator of method- and result-rejecting?

If the function of the method-contained component of Maddy's entreaty that mathematics be understood on its own terms is, like the method-affirming component, just a prophylactic against method- and result-rejecting, then the defense of modal nominalism from the previous section can be appropriated. The method-contained component is likely to combine the method-/result-rejecting and "sadly uncooperative" concerns: To go beyond mathematics creates the risk that one's metaphysics is sadly uncooperative, threatening to discount the idea that internal mathematical forms of reasoning provide sufficient justifications for mathematical assertions. It should be clear that modal nominalism, at least in certain of its formulations, carries minimal risk on both counts.

But, as it turns out, the function of the method-contained component of Second Philosophy is altogether distinct from the method-affirming component, and for this reason I think that it is worthwhile to examine how the method-contained component factors into Maddy's discussion of the two positions she takes to be inherently compatible both with mathematics and with Second Philosophy—Thin Realism and Arealism. These two views share a common metaphysical core, *viz.*, that under both views the facts of mathematical depth comprise the underlying reality of mathematics. In this regard Thin Realism and Arealism are both method-affirming. Maddy uses the method-contained component to argue that there is no substantive difference between these views, because what distinguishes them from one another are the claims they make about mathematics from a more general empirical perspective (i.e., from *outside* mathematics): The Thin Realist claims that mathematics is a body of truths, the Arealist denies that mathematics is a body of truths. Maddy officially endorses only the common methodological core of Thin Realism and Arealism. In the final two sections I make a provisional case that this use of the method-contained component unveils Second Philosophy as an implausibly strong, and possibly incoherent form of naturalism, and that modal nominalism is compatible with forms of naturalism which omit the method-contained component of Maddy's entreaty.

5.4.1 Thin Realism

Thin Realism is a moderate platonism that has many points of contact with Burgess's moderate platonism, discussed in the previous chapter. Maddy's motivation for developing a "thin" version of realism is to produce, "a satisfying form of realism without the shortcomings of the Robust versions" (ibid., 60), where such satisfaction accrues when this realism, "genuinely accounts for the nature of set-theoretic language and practice," and, "respects the actual structure of set-theoretic justifications" (ibid., 61). As discussed above, the shortcomings of robust versions of realism consist in their various forms of method- and result-rejecting. If the overriding virtue of Thin Realism is that it lacks these shortcomings, then it would appear that the primary force recommending Thin Realism is that the view *avoids* method- and result-rejecting.

The main theses of Thin Realism are that set theory expresses a body of truths; that sets exist; and that, "sets are just the sort of thing set theory describes; this is all there is to them; for questions about sets, set theory is the only relevant authority" (ibid., 61). Moreover, on this view, "set-theoretic methods are *the* reliable avenue to the facts about sets... no external guarantee is necessary or possible" (ibid., 63), and further that, "the evidence for the existence of sets is all and only linked to their mathematical virtues, to the mathematical jobs they are able to perform" (ibid., 73). These descriptions of Thin Realism rather directly identify the view as both method-affirming and method-contained. However, I think that the issue of whether Thin Realism is method-contained is rather more subtle than the above quotations indicate.

The Thin Realist's reason for regarding mathematics as a body of truths is *not* because of any reasons internal to the practice of mathematics, but instead, "because of [mathematics'] particular relations with the defining empirical inquiry from which she begins" (ibid., 70). The idea here is that the Second Philosopher, with a starting point in natural science, bears witness to the remarkable interplay between science and mathematics, eventually coming to respect mathematics as a successful, freestanding discipline, itself worthy of the honorific

'scientific.' A decision is then made to extend the applications of concepts like 'truth' and 'existence' to the assertions of set theory and the rest of mathematics. Thus, the decision to regard mathematics as a body of truths arises from the broader perspective afforded by the Thin Realist's empirical origins. It must be admitted, then, that Thin Realism is *not* method-contained in a purely mathematical sense—but it is ostensibly method-contained in a broader empirical sense, because the reasons for thinking that mathematics is a body of truths are alleged to come from the broader empirical perspective.

Concerning the nature of sets in particular, they are, "maximally effective trackers of certain strains of mathematical depth" (ibid., 82). On this account of set theory, depth functions as a guide to existence:

... the fact of measurable cardinals being mathematically fruitful in ways x,y,z (and these advantages not being outweighed by accompanying disadvantages) is evidence for their existence. Why? Because of what sets are: repositories of mathematical depth. They mark off a mathematically rich vein within the indiscriminate network of logical possibilities. (ibid., 82-3)

Thus, evidence for the depth or fruitfulness of an axiom is also evidence for the truth of that axiom and thereby is also evidence for the existence of those objects whose existence is implied by that axiom. Thin Realism is thus a combination of realism about the facts of mathematical depth and realism about mathematical objects, under which knowledge of the former serves as evidence for knowledge about the latter. The claim here is that all deep theories and concepts have exemplifications in the Thin Realist's ontology. Linking existence claims to the facts of mathematical depth in this way allows the Thin Realist to avoid the epistemological problems raised by the Robust Realist's desire for a further, extramathematical justification for these existence claims. However, one might wonder if the converse is true—whether all true mathematical existence claims are included in some or other deep mathematical theory.²⁶ For example, According to Maddy, the ground of elementary arithmetic comes not from the facts of mathematical depth but instead from

²⁶Thanks to Susan Vineberg for raising this issue.

more mundane facts about the physical world and human cognition (2007, 318-328). If Maddy's Thin Realist also adopts realism about the objects referred to in the existence claims of elementary arithmetic, then it would appear as though depth is not the *only* guide to mathematical existence.²⁷

For this reason care must be taken to distinguish questions about the existence or not of mathematical objects *in general* and the truth or not of mathematics *in general* from questions about the existence or not of *specific* mathematical objects and the truth or not of *specific* mathematical assertions. The role of mathematics in science is what validates mathematics as a body of truths (along with the existential ones); the facts of depth serve as evidence only in specific cases. The possibility remains, then, that there is more to the Thin Realist's ontology than can be gleaned from the facts of mathematical depth, which would further support my contention that Thin Realism is not method-contained in a purely mathematical sense. This would be especially troubling if it turned out that the Thin Realist's trivialist mathematical epistemology cannot access the full domain of the Thin Realist's ontology. To solve the problem for existence claims about sets is not to solve (or give the form of the solution for) the problem for all mathematical existence claims—unless one assumes in addition that *only* mathematically deep theories and concepts have exemplifications in the Thin Realist's mathematical ontology. Since it is unclear whether Maddy *qua* Thin Realist makes this assumption, I will not pursue this concern further.

Why is Thin Realism open to the Second Philosopher? Largely because of the minimality of its metaphysical and epistemological commitments. Maddy identifies it as the “simplest” realistic hypothesis about the nature of sets (Maddy 2011, 61). Given that sets track deep strains of mathematics, and that good mathematical reasoning (usually) accesses these strains, it becomes something of a triviality that good mathematical reasoning

²⁷Or, at least, depth is not the only guide to existence claims about *numbers*. If the facts of mathematical depth are what distinguish mathematical truths from non-mathematical truths, and if elementary arithmetic is not mathematically deep, then the numbers of elementary arithmetic are not, strictly speaking, *mathematical* objects. Of course, elementary arithmetic is embedded in numerous other theories that Maddy would describe as deep, so perhaps my distinguishing between number-existence and mathematical-existence points to nothing of significance. Cf. (*ibid.*, 362).

arrives at justified beliefs about assertions sets. To this extent, at least, Thin Realism is method-contained. But that these are inherently virtuous qualities of Thin Realism conflicts with Maddy's explanation for why one should avoid appealing to extramathematical considerations:

Philosophers often begin from a more elevated perspective: rather than examining the day-to-day practices, they content themselves with classifying mathematics as a non-empirical, a priori discipline, concerning a robust abstract ontology, then begin to wonder how we could possibly come to know such things, how what mathematicians actually do could have any connection to the subject matter they're attempting to describe. . . Roughly put, they begin with the metaphysics and are led to confusion about the methods. In contrast, the Second Philosopher begins with the methods, finds them good, then devises a minimal metaphysics to suit the case. (ibid., 86)

What is suggested here is that, in being method-contained, Thin Realism removes most (if not all) of the philosophical *interference* present in competing views, e.g., in Robust Realism. The problem with beginning from an "elevated perspective" is precisely that starting from this perspective dramatically increases the chances one will endorse method- and result-rejecting (because, e.g., mathematical methods might conflict with first-philosophical intuitions about what mathematical knowledge must be). So a virtue of views that are method-contained is that they dramatically *reduce* or *eliminate entirely* the risk of method- and result-rejecting. But this would be to laud being method-contained for its prophylactic qualities, rather than as an inherently virtuous quality of philosophical accounts of mathematics.

Another example of a use of the method-contained component is when Maddy claims that the justification for claims about mathematical metaphysics must arise from mathematical methods—and not the reverse (ibid., 87)—but this assertion is immediately followed by a reminder that purely metaphysical considerations have not restricted the "free pursuit of pure mathematics" (ibid.). Nowhere in her discussion of Thin Realism does the method-contained component play an independent role in establishing the virtues of Thin Realism—Thin Realism is always virtuous by contrast with Robust Realism, or by contrast

with some other view that is objectionable from the Second-Philosophical point of view for being in some kind of conflict with mathematics.

5.4.2 Arealism

In contrast with Thin Realism, Maddy argues that it also consistent with mathematical practice and with Second Philosophy to say that mathematics is *not* a body of truths. Her label for this alternative position is Arealism. The Arealist and the Thin Realist both agree about what makes for sound mathematical methodology. The Arealist does not contest that the pressures guiding mathematical concept formation and axiom selection are the facts of mathematical depth—the Arealist, like the Thin Realist, is a realist about the facts of mathematical depth. Thus Arealism, like Thin Realism, is method-affirming. Further, the Arealist does not approach mathematics with a prior prejudice against abstract objects, unlike Ockhamite forms of nominalism (*ibid.*, 97), which suggests that Arealism is also method-contained.

But just like in the case of Thin Realism, it is not clear that Arealism is method-contained in a purely mathematical sense. This is because the Arealist denies that sets exist (*ibid.*, 100). This judgment is based, not on any explicitly mathematical grounds, but instead on the attitude the Arealist adopts toward mathematics from her initial empirical perspective:

... [the Arealist] begins her investigation with ordinary perception, graduates to more sophisticated forms of observation, theory formation... eventually she turns to mathematical methods, and from there, to the pursuit of mathematics itself... she finds her mathematical inquiries broadening to include structures and methods without immediate application, which eventually leads her to set theory along the path of Cantor, Dedekind, Zermelo, and the rest... When she notices that its methods are quite different, that its claims aren't supported by her familiar observation, experimentation, theory formation, and so on... might she not simply conclude that whatever its merits, pure mathematics isn't in the business of uncovering truths? (*ibid.*, 88-9)

In other words, the Arealist bears witness to the remarkable methodological *differences* between natural science and mathematics, differences that suggest to her that it is inappropriate to apply words like 'true' and 'exists' to mathematics and mathematical objects;

Maddy has described the view as, “taking back in one’s scientific moments what one says in one’s mathematical moments” (2005a, 368; 2007, 390). Arealism, therefore, is not method-contained in a purely mathematical sense, but, as in the case of Thin Realism, it is method-contained in a broader empirical sense.

Though not identical to traditional forms of nominalism, Arealism does face a similar set of hurdles in accounting for scientific applications of mathematics. Maddy’s Arealist account of applications resembles other fictionalist accounts of applications.²⁸ The basic idea is that, “what we need to know isn’t so much that the advanced mathematics is true, but that the more esoteric features it reveals will continue to be effective in modeling the world” (Maddy 2011, 92). I do not intend to discuss the problem of application here. However, it is important to note that this kind of fictionalist account of modeling is not clearly unproblematic,²⁹ and if the Arealist’s account of applications ultimately proves untenable, then Arealism does not appear to be a viable option for the Second Philosopher. Nevertheless, insofar as, “the truth (or not) of mathematics is irrelevant to explaining its role in scientific application, it appears that Arealism is open to our Second Philosopher” (Maddy 2011, 96).

The permissibility of Arealism derives largely from the fact that it leaves the practice of mathematics intact—as a realist about the facts of mathematical depth, the Arealist is fully capable of coming to sound judgments about which axioms and concepts make for deep or fruitful mathematics. One might question whether this is so. Is it not part of the practice of mathematics to regard its accepted assertions as true (and its refuted claims as false)? Would it not be a form of method- or result-rejecting to deny this? Just what is the Arealist denying when she denies that mathematics is a body of truths? Note that Maddy describes terms like ‘true’ and ‘exists’ as *honorifics* (ibid., 111; 2005a, 368); seemingly to be bestowed (or not) at the whim of the attitude the Second Philosopher adopts from her empirical perspective.

²⁸See, e.g., (Balaguer 1998) and (Leng 2010).

²⁹See (Vineberg 2008).

Whether this is a problem for Arealism depends on whether there is evidence that a plausible use of mathematical methodology is to assert the truth *simpliciter* of mathematical existence claims. At least on Maddy's perspective, the answer to this question seems to be no—even under Thin Realism. When the Thin Realist states that mathematics is a body of truths, she does not do so for internal mathematical reasons. Instead this is the product of her judgment about the relationship between mathematics and science. If there is an internal mathematical conception of truth (or some other honorific that functions like truth), then she happily adopts it as continuous with whatever conception of truth she has inherited from her empirical origins. Similarly, when the Arealist denies that mathematics is a body of truths, she does not do so for internal mathematical reasons. If there is an internal mathematical conception of truth, she will not deny the use of this conception as it applies to claims within mathematics. But since, "her well-developed methods of confirming existence and truth aren't even in play" in mathematics (Maddy 2011, 89), she decides to regard this internal mathematical conception as *discontinuous* with her ordinary empirical conception of truth. If this practice turns out to be method- or result-rejecting, then so much the worse for Arealism. Otherwise, the view is only involved in metaphysics to the extent required by its inherent realism about the methodologically-driven facts of mathematical depth; and the benefit of this metaphysical austerity is that it prevents method- and result-rejecting.

5.4.3 There is no Difference Here

One of the more fascinating allegations of (Maddy 2011) is that there is no substantive difference between Thin Realism and Arealism—both views are "indistinguishable at the level of method" (*ibid.*, 100), and are ultimately indistinguishable to Maddy's naturalist. Maddy's reasoning here is rather subtle, and relies on claims she makes from both a perspective *within* mathematics as well as a perspective of an inquirer assessing mathematics from a more general empirical or scientific setting. To borrow the eponymous phrase of (Ferreirós 2010), this parity result arises from the adoption of naturalism both *in* and *about*

mathematics. The following passage is paradigmatic of Maddy's observations from *within* mathematics:

This methodological agreement [between Thin Realism and Arealism] reflects a deeper metaphysical bond: the objective facts that underlie these two positions are exactly the same, namely, the topography of mathematical depth. . . For both positions, the development of set theory responds to an objective reality—and indeed the same objective reality. (Maddy 2011, 100)

Thus, any decision between these two views is not to be arbitrated on the basis of the methodology of mathematics, for it is this methodology that is singularly incapable of recognizing any relevant distinction between Thin Realism and Arealism.

At this point Maddy has established her parity result from *within* mathematics. But, as it has already been mentioned, Maddy also recognizes the legitimacy of a perspective outside of mathematics, *viz.*, the empirical origins of the Second Philosopher. The dispute between the Thin Realist and the Arealist, “takes place not within set theory, but in the judgments they form as they regard set-theoretic language and practice from an empirical perspective” (*ibid.*, 100). From this perspective, Maddy claims, there is no decisive reason to favor Thin Realism over Arealism, or vice versa—nothing about the role of mathematics in the scientific world view forces the Second Philosopher to apply the honorifics ‘true’ and ‘exists’ to the assertions of mathematics. This is because

. . . the two idioms are equally well-supported by precisely the same objective reality: those facts of mathematical depth. These facts are what matter, what make pure mathematics the distinctive discipline that it is. . . Thin Realism and Arealism are equally accurate Second-Philosophical descriptions of the nature of pure mathematics. They are alternate ways of expressing the very same account of the objective facts that underlie mathematical practice. (*ibid.*, 112)

Not even *outside* of mathematics does there arise a legitimate preference for Thin Realism over Arealism (or vice versa). So although both views are method-contained in a broader empirical sense, the broader empirical methods parlay agnosticism. Since both the methodology of mathematics *and* the empirical perspective prove facile, there is nothing further for Maddy's naturalist to say on matters. Of course, if a salient difference were

discovered between Thin Realism and Arealism, then the Second Philosopher would have a reason to choose between the views. Thus Maddy should not be read as claiming that it is impossible in principle, from the empirical perspective, to decide between alternate metaphysical accounts of mathematics. Rather, if Maddy is right, it simply happens to be the case that there are no salient differences between Thin Realism and Arealism.

I would like to offer the following analysis of Maddy's parity result: Maddy's claim that the facts of mathematical depth are the only facts that "matter" factors prominently in reaching her conclusion that, both from within mathematics and from without, there is no substantive difference between the two views. Maddy thinks that it is a crucial finding that none of the methods of *mathematics proper* decide between Thin Realism and Arealism. This is evidence that, for the Second Philosopher, the fact that Thin Realism and Arealism are method-contained in a broader empirical sense is ultimately of little significance, because what distinguishes these two views *cannot* be appreciated from a perspective that is method-contained in a strictly mathematical sense. The only aspects of Thin Realism and Arealism that are ultimately positively sanctioned by the Second Philosopher are the strictly mathematical components of these views—that the facts of mathematical depth comprise the underlying reality of mathematics, that it is part of the methods of mathematics to reveal these facts, etc.

Both Thin Realism and Arealism, then, constitute examples of the idea that failing to be method-contained in a strictly mathematical sense does not induce method- and result-rejecting (malevolently or otherwise). But the Second Philosopher still seeks to excise the extramathematical components of Thin Realism and Arealism, even though these components pose no risk of method- and result-rejecting. For Maddy, then, the claim that the facts of mathematical depth are *the* facts that matter—the method-contained component of her entreaty to understand mathematics on its own terms—plays *more* than a merely prophylactic role. Excised of this component of her entreaty, Maddy's naturalism cannot establish the indistinguishability of Thin Realism and Arealism.

But why suppose that the facts of mathematical depth are the facts that matter? Why does the Second Philosopher insist that the method-contained component plays an independent and decisive (or, perhaps, *indecisive?*) role in apportioning merit to accounts of mathematics? Without a justification for the claim that mathematical metaphysics is *wholly* constituted by the pressures underlying mathematical concept formation and axiom selection, it simply does not follow that there is no substantive difference between Thin Realism and Arealism. I am forced to conclude, then, that Maddy intends the method-contained component of her entreaty to stand on its own, as an independent and vital feature of Second Philosophy. *It follows that, since modal nominalism fails to be method-contained, then it is not consistent with Second Philosophy, even though modal nominalism does not (in certain of its formulations) appear to be either method- or result-rejecting.*

5.4.4 The Method-Contained Objection: Coda

I will not contest the claim that modal nominalism, because it fails to be method-contained, is inconsistent with Second Philosophy. Nevertheless there are four reasons why I maintain that modal nominalists should not be bothered by this inconsistency.

First, modal nominalism appears to be defensible by analogy with Thin Realism and Arealism on the grounds that its failure to be method-contained is not sufficient evidence for thinking that modal nominalism is either method- or result-rejecting. There is a potentially crucial difference between modal nominalism and Maddy's favored views, however. And this is that both Thin Realism and Arealism are, allegedly, method-contained in a broader empirical sense, whereas no such claim has been made on behalf of modal nominalism. But even if modal nominalism fails to be method-contained in a broader empirical sense—and whether this is so is very much an open question—I would still insist, in light of the results of §3, that modal nominalism poses very little risk of method- and result-rejecting.

Second, there is reason to suppose that the method-contained component of Maddy's entreaty to understand mathematics on its own terms is not a coherent feature of Second

Philosophy. It is worth restating that Maddy herself recognizes that a full account of the objective constraints on mathematical practice involves legitimizing not only the facts of mathematical depth *but also* legitimizing claims of logical consistency (Maddy 2011, 73; 78). The latter of which is, presumably, the only significant constraint on the Modal Structuralist version of modal nominalism. So it is not entirely clear that modal nominalism (at least via Hellman) requires *more* than the Second Philosopher's depth-based metaphysics requires, at least from a logical or conceptual point of view. If the justification for logical consistency claims is to come from somewhere other than their pragmatic uses in mathematics, then *everyone* must look beyond mathematics to fully understand the nature of mathematics—the facts of depth included.³⁰ If this poses any kind of risk of method- and result-rejecting, then it is a risk that Maddy shares with the Modal Structuralist.

Third, I suspect that Maddy's entreaty that philosophical accounts of mathematics be contained by the methods of mathematics unveils Second Philosophy as an implausibly strong form of naturalism. For reasons already given, I doubt that it is possible to produce a coherent philosophy of mathematics using *only* the methods of mathematics.

Fourth, and finally, given that modal nominalism appears to be compatible with every element of Second Philosophy *except* the method-contained component of the impulse to *understand* mathematics on its own terms, it stands to reason that modal nominalism is consistent with any form of naturalism that adopts only the weaker Second Philosophical principles—to avoid method- and result-rejecting, and to be method-affirming—whether individually or jointly.

Reasons three and four raise deep questions about the overall coherence of Maddy's naturalism and more generally about which components of Maddy's naturalism represent desirable features of the naturalistic perspective. In the short space remaining I cannot hope to provide any decisive objections to Second Philosophy, nor can I hope to provide a detailed circumscription of the alternative naturalist perspectives that are available, but I

³⁰Of course, if the legitimization of claims of logical consistency is *part* of mathematical method, then there would be grounds for thinking that Modal Structuralism *is* method-contained.

can at least relay some provisional remarks on both items.

5.5 How Much Naturalism is Too Much Naturalism?

One thing I would like to suggest is that it is not possible to produce a coherent philosophy of mathematics excised of everything except the methods of mathematics. Something extramathematical appears required for producing an account of logical consistency. Further evidence that the facts of mathematical depth do not provide a self-sustaining basis for the metaphysics of mathematics was alluded to during my discussion of Thin Realism, *viz.*, that there are both deep *and* shallow truths of mathematics.³¹ Are basic truths of arithmetic and analysis any less mathematical for not generating much in the way of mathematical interest? I should think not—but then it is not clear that the facts of mathematical depth are reliable guides for distinguishing between mathematical and non-mathematical claims. If that is right, then there is no guarantee that Maddy's method-contained, depth-style trivialist epistemology will suffice for all mathematical claims—it would seem only appropriate for *deep* mathematical claims. For this reason it appears unduly restrictive to object to a metaphysical account of mathematics simply because it outstrips the province of mathematical methodology, *i.e.*, simply because it fails to be method-contained.

But acknowledging this would place Second Philosophy on very shaky foundations—is it not a central tenet of Second Philosophy that the *only* methods for understanding and evaluating mathematics are mathematical methods? Would challenging this conviction not simply be tantamount to objecting to Second Philosophy (and to naturalism more generally)? Perhaps. But if the Second Philosopher does not have a cogent case that *all* extramathematical analyses of mathematics are assailed by irrelevance and conflict, then merely highlighting a view's appeal to something extramathematical cannot be a decisive reason—even for the Second Philosopher—to object to a philosophical account of mathematics. Maddy has provided compelling reasons for rejecting certain extramathematical perspectives, *e.g.*, Quine's views on ontological economy in set theory, but a *general*

³¹Thanks again to Susan Vineberg for this observation.

prohibition has only been established by fiat.

One claim I would like to make is that naturalism should not simply be the dogmatic rejection of first philosophy or of extramathematical considerations. The rejection of such considerations should be decided on a case-by-case basis. For instance, by summarily rejecting first-philosophy, Maddy does evict numerous considerations that are irrelevant to mathematical practice (e.g., preoccupation with establishing the truth of mathematics on *a priori* grounds), but she also evicts potentially helpful first-philosophical considerations.³² One might even go so far as to argue, contra Maddy, that philosophical beliefs are part of the evidential structure of the subject. Favorable cases do suggest themselves—for instance, Gödel’s belief in a “well-determined reality” (1983, 476) underlying the set theoretic axioms (which was inseparable from his interest in whether the Continuum Hypothesis was true or false of this reality), or the beliefs of Heyting and Brouwer, which led to the development of intuitionistic logic and mathematics.³³ Maddy is of course skeptical on this point:

Given the wide range of views mathematicians tend to hold on these matters, it seems unlikely that the many analysts, algebraists, and set theorists ultimately led to embrace sets would all agree on any single conception of the nature of mathematical objects in general, or of sets in particular; the Second Philosopher concludes that such remarks should be treated as colorful asides or heuristic aides, but not as part of the evidential structure of the subject. (Maddy 2011, 52-3)³⁴

Maddy is right to point out that mathematicians hold a “wide range of views” on matters, but the mere fact of philosophical disunity in the mathematical community is not alone evidence that philosophical (including first-philosophical) considerations are always or are even usually irrelevant to mathematics and its methodology. It might just be that some mathematicians are right and others are wrong, when it comes to their philosophical beliefs; and it might be that philosophical beliefs can be mathematically productive, even if they are wrong.³⁵

³²Cf. (Moore 2006).

³³Thanks to Susan Vineberg for suggesting these cases.

³⁴See also (Maddy 1997, 192-3).

³⁵This latter point echoes the observation of (Schlimm 2013) that, despite the value of consistency to the

Why, then, does Maddy object so strongly to the first-philosophical point of view? It cannot simply be that the perspective is first-philosophical! My sole aim in distinguishing between the ideas that mathematics should be *understood* and *evaluated* on its own terms (in concert with the decomposition of the former into the method-affirming and method-contained components, and the decomposition of the latter into the method- and result-rejecting components) was to provide a rationalization for Maddy's rejection of various philosophical accounts of mathematics. *But the various philosophical accounts that utilized extramathematical resources were seen to cause problems with practice only when they were either method- or result-rejecting.* Failing to be either method-affirming or method-contained is not essentially linked to being method- and result-rejecting. Why, then, is the most reasonable perspective one that abandons first philosophy entirely? Why is it not more reasonable to apply something like a "no-harm" policy regarding first philosophy? It is true that some uses of first philosophy malevolently conflict with mathematics, and the Second Philosopher (along with all dutiful naturalists) should censure such uses; but that is not the situation that appears to be presented by Hellman's Modal Structuralism (and perhaps also Chihara's Constructibility Theory). I have difficulty understanding what the objection to these views is, if it is something other than anti-first-philosophical foot stamping.

What, then, is the ground of the method-contained component of Maddy's entreaty to understand mathematics on its own terms? It does not appear, for instance, to be a claim of *mathematics proper* that the metaphysics of mathematics is wholly constituted by the facts of mathematical depth. Rather, this is what (Daly and Liggins 2011) would describe as a "philosophical imposition" concerning the nature of mathematical objectivity—albeit one that happens to avoid many of the (alleged) irrelevancies of Maddy's ontology-preoccupied predecessors. If it is indeed true that the only resources available to the Second Philosopher *a propos* of mathematics are the methods of mathematics, then since the method-contained

mathematical community, inconsistent sets of axioms may provide the starting point for fruitful mathematical work. Cf. (Easwaran 2008), where it is argued that mathematicians adopt axioms to *avoid* philosophical disputes.

component is *not* endemic to mathematics, the method-contained component is self-undermining. If Maddy is genuinely committed to the method-contained component of her entreaty, then her naturalism is implausibly strong.

But perhaps the Second Philosopher intends to leave *just enough* philosophical wiggle room to advance or otherwise justify the method-contained component of Maddy's entreaty. This, however, only serves to raise the question as to why the Second Philosopher is permitted to make *this* philosophical claim, but is not permitted to make the kinds of philosophical claims modal nominalists make. I do not know of any clear way of addressing this question, aside from either (a) reiterating that accounts of mathematics ought to be method-contained, or (b) reanimating the worry that when an account fails to be method-contained that it thereby induces method- and result-rejecting. Both strategies are unconvincing. Are there other options available? One might propose that considerations of simplicity offer support for the method-contained component and its sequester on extramathematical metaphysical theorizing about mathematics. Since a depth-based metaphysics accounts for the metaphysics of mathematics *and* the facts of mathematical depth in one fell swoop, it appears to qualify as a *simpler* account of mathematics when compared to modal nominalism. But why, in the circumstances at hand, should simplicity count as a virtue? And what notion of simplicity should be invoked? One argument for thinking that simplicity is a virtue is the observation that, as the complexity of one's mathematical metaphysics increases, so too does the likelihood that one's metaphysics will run into conflict with mathematical practice. But this is just to say that simplicity—and ultimately, being method-contained—is a mere prophylactic against method- and result-rejecting. So it is difficult to see at the same time both what the justification for the method-contained component is, and why it should be thought that violating the method-contained component is inherently problematic. Though the method-contained component may be *advanced* as a valuable component of Second Philosophy, I do not see that there is any independent evidence for this attribution of value. It would be special-pleading to insist that the *only*

philosophical claim one is allowed to make about mathematics is that an understanding of the subject must be confined by its own methods.

It would appear then, that justifying the method-contained component of Maddy's entreaty presents serious difficulties. Not only does it seem implausible that one could produce a coherent metaphysical account of mathematics using only the methods of mathematics, it is also implausible that one could motivate, using only the methods of mathematics, the *idea* that one should use only the methods of mathematics when constructing a metaphysical theory of the subject. What this suggests is that any form of naturalism that embraces the method-contained component of Maddy's naturalism is implausibly strong—the method-contained component of Maddy's entreaty must be dropped. If Maddy's naturalism is implausibly strong, then what might weaker forms of naturalism look like? And with what kinds of naturalism would modal nominalism be compatible?

5.6 Conclusion: Naturalism and Modal Nominalism

I should think that Maddy's entreaty that mathematics be *evaluated* on its own terms comprises the foundation of basic naturalist dogma. A naturalism that permitted wanton philosophical upheaval of mathematical methods or results would be no naturalism at all. Thus, the weakest form of naturalism seems to be one which merely prohibits method- and result-rejecting, at least in their malevolent instances. I hope I have made a plausible case for thinking that modal nominalism poses little risk of method- and result-rejecting, and would therefore be compatible with this weak form of naturalism.

Nevertheless I suspect that Maddy is correct to judge that an account of the actual methods of mathematics *is* a philosophical novelty. Perhaps prior to (Kitcher 1983) or (Lakatos 1976), an account of the practice and methods of mathematics was not on the agenda of philosophy of mathematics, and only in the last decade or so has the topic been pursued with vigor and as something that portends important lessons for philosophy.

Thus it would not be entirely insensible for naturalists to seek to account for the methods of mathematics. And if Maddy's analysis of mathematical method is correct, then accounting for the methods of mathematics involves explaining how the facts of mathematical depth constrain or influence mathematical work. This is all to say that the method-affirming component of Maddy's entreaty to understand mathematics on its own terms is not an unreasonable component of her (or any other) naturalism. Given that certain strands of modal nominalism—at least Hellman's Modal Structuralism—are method-affirming in many senses, and given that modal nominalism certainly does not preclude taking up the study of the methods of mathematics, it stands to reason that modal nominalism is compatible with a somewhat stronger version of naturalism that embraces the method-affirming component of Maddy's entreaty, but stops short of embracing the method-contained component.

There is no need to run together, as Maddy appears to do, an account of the methods of mathematics and an account of the subject-matter of mathematics. That is not to say that these two items are unrelated—presumably the methods of mathematics help to illuminate its subject-matter, and its subject-matter constrains its methods. But it would be implausible to insist that a complete and non-amendable account of the subject-matter of mathematics could be had solely from a characterization of the methods of mathematics, just as it would be implausible to insist that a complete and non-amendable account of the methods of mathematics could be had solely from a characterization of the subject-matter of mathematics. To play on a famous Lakatos quote, the metaphysics of mathematics, lacking the guidance of methodology, would become blind, but it would also become blind without the guidance of philosophy.

All this is to say that although modal nominalism may be inconsistent with Second Philosophy, nevertheless the modal nominalist should not be bothered by this inconsistency. Second Philosophy appears to be an implausibly strong form of naturalism, and moreover the naturalistic resources available to the Second Philosopher do not provide any

compelling reasons for supposing that modal nominalism runs into any kind of conflict with mathematics itself. Given that the only decisive objection the Second Philosopher can sustain against modal nominalism relies on the strongest and least plausible aspect of Second Philosophy of mathematics—its excision of extramathematical methods—it would appear that all weaker forms of naturalism are incapable of producing decisive objections to modal nominalism. Thus, modal nominalism is compatible with naturalisms that do not require that mathematics be understood *solely* using the methods of mathematics.

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ABSTRACT**NOMINALISM IN MATHEMATICS: MODALITY AND NATURALISM**

by

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I defend modal nominalism in philosophy of mathematics—under which quantification over mathematical ontology is replaced with various modal assertions—against two sources of resistance: that modal nominalists face difficulties justifying the modal assertions that figure in their theories, and that modal nominalism is incompatible with mathematical naturalism.

Shapiro argues that modal nominalists invoke *primitive* modal concepts and that they are thereby unable to justify the various modal assertions that figure in their theories. The platonist, meanwhile, can appeal to the set-theoretic reduction of modality, and so can justify assertions about what is logically possible through an appeal to what exists in the set-theoretic hierarchy. In chapter one, I illustrate the modal involvement of the major modal nominalist views (Chihara's Constructibility Theory, Field's fictionalism, and Hellman's Modal Structuralism). Chapter two provides an analysis of Shapiro's criticism, and a partial response to it. A response is provided in full in chapter three, in which I argue that reducing modality does *not* provide a means for justifying modal assertions, vitiating the accusation that modal nominalists are particularly burdened by their inability to justify modal assertions.

Chapter four discusses Burgess's naturalistic objection that nominalism is unscientific. I argue that Burgess's naturalism is inadequately resourced to expose nominalism (modal or otherwise) as unscientific in a way that would compel a naturalist to reject nominalism.

I also argue that Burgess's favored moderate platonism is also guilty of being unscientific. Chapter five discusses some objections derived from Maddy's naturalism, one according to which modal nominalism fails to affirm or support mathematical method, and a second according to which modal nominalism fails to be contained or accommodated by mathematical method. Though both objections serve as evidence that modal nominalism is incompatible with Maddy's naturalism, I argue that Maddy's naturalism is implausibly strong and that modal nominalism is compatible with forms of naturalism that relax the stronger of Maddy's naturalistic principles.

AUTOBIOGRAPHICAL STATEMENT

Schwartz was born and raised in Mid-Michigan and attended Michigan State University for his undergraduate studies, where he received a B.A. in philosophy in 2007 with cognates in mathematics and statistics. He completed his graduate work at Wayne State University where in 2010 he received his M.A. in philosophy and where in 2013 he received his Ph.D. in philosophy with a minor in mathematics. Schwartz's primary areas of research are philosophy of mathematics and modal metaphysics; other areas of interest include metaphysics and environmental ethics, in particular in the application of environmental thought to space policy. His publications have appeared in the journals *Environmental Ethics* and *Ethics and the Environment*.