PRACTICE-DEPENDENT REALISM AND MATHEMATICS

DISSERTATION

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By

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ABSTRACT

I present a solution to a puzzle concerning the interpretation of mathematical practices. Mathematical claims seem to be objective and their surface grammar suggests that they are about objects or structures. Furthermore, these objects or structures do not appear to be located in the spatio-temporal world. For these reasons, Platonists have suggested that mathematical claims concern domains or structures that are abstract and are in no sense constituted by our intellectual activities. Yet if Platonists are correct, then something very peculiar is going on. Items that are abstract and not constituted by our intellectual activities cannot influence spatio-temporally instantiated activities like mathematical practices. Contemporary authors have responded to this observation in two distinct (and incompatible) ways. Some have argued that mathematics should be understood as a fiction that helps us represent and reason about the spatio-temporal world. Others hold that the surface grammar of mathematical claims is misleading; they are really claims about what is logically possible and/or necessary. A philosopher who accepts either suggestion must pay a high price. Specifically, (s)he expresses a problematic lack of respect for actual mathematical practice: either (s)he doesn't take mathematicians' claims literally or (s)he interprets mathematicians as offering extremely misleading characterizations of their subject matter. Neither option is acceptable.

As a solution to this puzzle, I articulate and defend a new metaphysical interpretation of mathematics. According to this interpretation, mathematical domains (structures) are constituted by the mathematical activities of rational beings in a way analogous to the constitution of laws and legal borders by the legislative activities of rational beings. The presence of appropriate types of mathematical activity is both a necessary and sufficient condition for the existence of a mathematical domain (structure), just as the presence of appropriate types of legislative activity is both necessary and sufficient for the existence of particular laws. Yet while laws are constituted by explicit stipulation, mathematicians constitute mathematical domains by providing coherent and adequate characterizations of those domains. My interpretation of mathematics offers an authentic solution to this puzzle, since my solution takes mathematics to be about the very things it seems to be about, i.e., mathematical domains (structures). It interprets mathematicians as making literal, i.e., non-fictional, claims about these domains. And, it is not affected by the peculiarity associated with Platonism, because it takes mathematical domains to be constituted by mathematical activities. As a consequence, while mathematical domains themselves are acausal, the mathematical activities that constitute those domains enter into causal relations. Finally, this view, which I call *practice-dependent realism*, contains the resources for a novel account of the objectivity, necessity, and applicability of mathematical truth.

I dedicate this dissertation to my brother Nicholas.

His love of all things academic molded me into who I have become.

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Cole, J. (2004). Review of Corfield, D. (2003) *Towards a Philosophy of Real Mathematics.* Cambridge University Press, New York, NY. *Math. Rev.* 2004f:00005.

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Cole, J. and Shapiro, S. (2003). Review of Colyvan, M. (2001). *The Indispensability of Mathematics.* Oxford University Press, New York, NY. *Mind* 112:331-336.

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Cole, J. (2000b). Review of Auyang, S. Y. (1998). *Foundations of Complex-System Theories in Economics, Evolutionary Biology, and Statistical Physics.* Cambridge University Press, New York, NY. *Brit. J. Phil. Sci.* 51:187-190.

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INTRODUCTION

DISSERTATION PRELIMINARIES

0.1 Introduction

At some point in a mathematicians' early career, (s)he will take an introductory course in real analysis. Among the topics that will be discussed in that course are various number systems and continuous and differentiable functions. My first class in real analysis was based on Rod Haggarty's *Fundamentals of Mathematical Analysis* (Haggarty, 1989). Here are some theorems that were proved in that class — any mathematician will be familiar with them:

> 1.1.3 Theorem
>
> Between any two distinct rationals there is an irrational. (Haggarty, 1989, p. 4)
>
> 1.1.4 Theorem
>
> Between any two distinct irrationals there is a rational. (Haggarty, 1989, p. 5)
>
> 3.3.2 The intermediate value property
>
> Let f be continuous on $[a, b]$ and suppose that $f(a) = \alpha$ and $f(b) = \beta$. For every real number γ between α and β there exists a [real] number c, $a < c < b$ with $f(c) = \gamma$. (Haggarty, 1989, p. 81)
>
> 3.3.4 The fixed point theorem
>
> Let $f : [0, 1] \to [0, 1]$ be a continuous function. Then there is at least one [real] number c which is fixed by f. That is $f(c) = c$. (Haggarty, 1989, p. 84)

4.2.2 Mean value theorem

Let f be differentiable on (a, b) and continuous on $[a, b]$. Then there exists a [real number] c, $a < c < b$, such that

$$f'(c) = \frac{f(b) - f(a)}{b - a}$$

(Haggarty, 1989, p. 103)

Consider first Theorems 1.1.3 and 1.1.4. The implicit first-order universal quantifiers within these theorems range over both rational and irrational numbers. Next consider Theorems 3.3.2, 3.3.4, and 4.2.2. It is explicit that the first-order quantifiers in these theorems include real numbers within their range, and it is implicit that continuous functions on closed intervals (of real numbers) and differentiable functions on open intervals (of real numbers) are also within that the range of these quantifiers.[1] Anyone who has any familiarity with mathematics will know that one need only open any reasonably advanced mathematical text to find statements such as these, i.e., statements that include mathematical entities within the range of their first-order quantifiers.

For a contemporary analytic philosopher, the appearance of mathematical entities within the range of the first-order quantifiers of mathematical theories immediately generates the following question: Do the mathematical entities that fall within the range of the first-order quantifiers of mathematical theories exist? For, following the widespread influence of W.V.O. Quine's "On What There Is" (Quine, 1948), it has become popular to accept that theories engender ontological commitment to all and only those entities that fall within the range of their first-order quantifiers. This question

[1]It might be that, in a formal statement of these informal mathematical theorems, functions would be replaced by sets or some other type of entity. This does not undermine the point that I am making. On all plausible formalizations of mathematics, mathematical theories will quantify over mathematical entities.

becomes all the more pressing once it is observed that it is common for mathematical theories to include such bold — and unrestricted — existential statements as: "there exist infinitely many prime numbers" and "the empty set exists."

Given these facts, a philosophically naïve reader might maintain that the answer to the above question is obvious: of course these mathematical entities exist. How could statements such as "there exist infinitely many prime numbers" and "the empty set exists" be (objectively) true — as all evidence suggests they are — and mathematical entities not exist? Despite this, many philosophers have questioned the existence of some or all mathematical entities. The first observation that most of these skeptics make is that if mathematical entities do exist, then our evidence for their existence can't be the same as the evidence that we have for the existence of chairs, apples, electrons, and genes.[2] We don't "observe" mathematical entities, either directly or indirectly, in the way in which we "observe" spatio-temporal entities. Mathematical entities aren't the same kind of entities that spatio-temporal entities are. They are, rather, abstract entities. Among other things, this means that they are acausal, non-spatio-temporal, eternal, and changeless. Next these skeptics will observe that if mathematical entities are abstract, then they can't possibly exert any influence over human beings or their activities. Finally, they will ask how we could possibly be justified in believing that mathematical entities exist if mathematical entities can exert no influence over human beings or their activities.

Thus begin debates concerning the metaphysical interpretation of mathematical practices. In broad outline, mainstream philosophy of mathematics is dominated — in terms of number of proponents — by the following three positions on how these

[2] In recent years, Quinean philosophers of mathematics have rejected this claim (cf., e.g., (Resnik, 1997)).

debates should be settled. First, there is Platonism. **(Mathematical) Platonists** maintain that mathematical domains exist, are abstract, and are metaphysically independent of all social practices, i.e., would exist even if there were no social practices. Platonism is usually combined with semantic realism — the view that mathematical statements have their truth values independently of the abilities of rational beings to know those truth values. Platonists who accept semantic realism maintain that mathematical statements are objectively[3] true or false in virtue of whether or not they correctly represent mathematical domains that are metaphysically independent of social practices (cf., e.g., (Shapiro, 1997)). Yet not all varieties of Platonism advocated in the literature accept semantic realism. For example, Neil Tennant (cf. (Tennant, 1997)) advocates a position that combines Platonism with a variety of semantic anti-realism. The arguments that I offer in this dissertation concerning Platonism only turn on the metaphysical commitments of Platonism. They are independent of the distinction between semantic realism and anti-realism.

Second, there is Fictionalism. **(Mathematical) Fictionalists** maintain that positive existential pure mathematical statements are fictional or metaphorical — non-literal in some sense — and, as such, either are not, or need not be, answerable to any distinctively mathematical ontology (cf., e.g., (Field, 1980), (Field, 1989), (Yablo, 2002a), (Yablo, 2002b), and (Yablo, ToAp)).

Third, there is Modal Nominalism. **Modal Nominalists** maintain that mathematical statements are objectively true or false not in virtue of their relationship to mathematical domains that are metaphysically independent of social practices, but

[3]The above definition of semantic realism provides a fairly well-known way of spelling out what it is for a statement to be objectively true or false. Yet this is by no means uniformly accepted as the best way to capture the relevant sense in which mathematical statements can be seen to be objectively true or false. I shall address this issue briefly in Part IV of this dissertation.

rather in virtue of certain objective modal facts (cf., e.g., (Chihara, 1990) and (Hellman, 1989)). These modal facts are taken to obtain in the absence of any variety of possibilia from the world and in the absence of mathematical domains from the world. As such, Modal Nominalism is a form of semantic realism.

In the last ten years, two books have been published advocating a somewhat different metaphysical interpretation of mathematical practices than any of these three. One of these books was published by a professional mathematician, the other by an educationalist interested in how mathematics should be taught.[4] The details of these two accounts of mathematics are in some ways quite different, yet both authors promote the view that mathematical domains are socially constructed[5] by mathematical practices, i.e., the collection of normatively constrained activities that mathematicians *qua* mathematicians engage in. What are Ernest and Hersh suggesting? Here are some quotes that express Ernest's position:

> According to the social constructivist view the discourse of mathematics creates a cultural domain within which the objects of mathematics are constituted by mathematical signs in use. (Ernest, 1998, p. 193)

> . . . signifiers have ontological priority over the signified — especially in mathematics, for the signifiers can be inscribed and produced, or at least instantiated, whereas the signified can be indicated only indirectly, mediated through signifiers. (Ernest, 1998, p. 196)

> . . . the ontology of mathematics is given by the discursive realm of mathematics, which is populated by cultural objects, which have real existence in that domain . . . mathematical discourse as a living cultural entity creates the ontology of mathematics. (Ernest, 1998, p. 202)

[4]Respectively, Reuben Hersh, *What is Mathematics, Really?* (Hersh, 1997) and Paul Ernest, *Social Constructivism as a Philosophy of Mathematics* (Ernest, 1998).

[5]I shall provide an extensive discussion of social construction in Chapter 1.

There is much in these quotes that is unclear. For example, what is a cultural domain, and what does Ernest mean when he talks about signs and signifiers? But these quotes do clearly illustrate Ernest's belief that mathematical ontology is constructed by mathematical practices. This is the basic metaphysical thesis of social constructivism. These quotes also make it clear that Ernest takes mathematical discourses to be central to the construction of mathematical ontology. Indeed, the first and second quotes indicate that Ernest believes that the constructive work of mathematical practices is done, at least primarily, by the presence of mathematical signs and signifiers in mathematical discourses.

At least under the natural interpretation that mathematical signs and signifiers are lexical items within mathematical discourses, this last suggestion is problematic. Mathematicians and mathematical practices are finite. As such, the whole history of mathematics only contains a finite number of signs and signifiers. Further, many mathematical discourses contain very few, if any, singular terms. Consider, for example, Euclidean geometry; any point is as good as any other, so we don't introduce singular terms to denote specific Euclidean points. Thus, unless Ernest is promoting a radical revision to mathematics, there simply aren't enough signs and signifiers in mathematical practices for those signs and signifiers to be responsible for constructing all mathematical ontology.

Perhaps, however, Hersh can provide us with a clearer understanding of the social constructivists' metaphysical account of mathematics. Here are some quotes from his book:

> *Fact 1: Mathematical objects are created by humans. Not arbitrarily, but from activity with existing mathematical objects, and from the needs of science and daily life.*

Fact 2: Once created, mathematical objects can have properties that are difficult for us to discover. (Hersh, 1997, p. 16)

4. Mathematical objects are a distinct variety of social-historical objects. They're a special part of culture. (Hersh, 1997, p. 22)

In Fact 1, Hersh expresses the basic social constructivist metaphysical thesis with a minor twist; he recognizes the need to account for why human beings created mathematical domains[6] and hints at such an account. In Fact 2, Hersh indicates his sensitivity to a certain type of independence that mathematical domains have from mathematical practices — let us call it **epistemic independence**. Just below Fact 2 he tells us

> Once created and communicated, mathematical objects are *there*. They detach from their originator and become part of human culture. We learn of them as external objects, with known and unknown properties. Of the unknown properties, there are some that we are able to discover. Some we can't discover, even though they are our own creations. (Hersh, 1997, p. 16)

While the second part of this quote reinforces Hersh's sensitivity to the epistemic independence of mathematical domains from mathematical practices, the first part goes further than this by indicating that mathematical domains detach — in some sense — from their specific creator. We shall return to this point later.

[6]The observant reader will have noticed that Hersh — and Ernest in some places — talks about the construction of mathematical objects, while I talk about the construction of mathematical domains. There are two reasons for this. First, (at least most) mathematical objects are the objects they are in virtue of their relationships to the other objects in the domain containing them. So, in constructing a particular mathematical object, one really needs to construct the whole domain containing that object. Some constructivist philosophers concerned about completed totalities would maintain that it suffices to construct all of the objects in the domain not the domain itself. For expositional convenience I am going to ignore these constructivist worries and talk about domains. It is easy enough to modify what I say to deal with these constructivist worries if you have them. Second, in constructing some aspect of mathematical reality, one is presumably not only constructing the objects in that aspect of mathematical reality, but also the properties of those objects and the relationships that obtain between those objects. A domain, at least as I am using this notion, is a collection of objects which have properties and stand in relations to one another.

Perhaps Hersh's most interesting claim, however, is that "mathematical objects are . . . social-historical objects." What are we to make of this claim? I believe that the following quote is helpful:

> Frege showed that mathematical objects are neither physical nor mental. He labeled them "abstract objects." What did he tell us about abstract objects? Only this: They're neither physical nor mental.
>
> Are there other things besides numbers that aren't mental or physical?
>
> Yes! Sonatas. Prices. Eviction notices. Declarations of war.
>
> Not mental or physical, but not abstract either!
>
> The U.S. Supreme Court exists. It can condemn you to death!
>
> Is the court physical? If the Court building were blown up and the justices moved to the Pentagon, the Court would go on. Is it mental? If all nine justices expired in a suicide cult, they'd be replaced. The court would go on.
>
> The Court isn't the stones of its building, nor is it anyone's minds and bodies. Physical and mental embodiment are necessary to it, but they're not *it*. *It's a social institution.* Mental and physical categories are insufficient to understand it. It's comprehensible only in the context of American society.
>
> What matters to people nowadays?
>
> Marriage, divorce, child care.
> Advertising and shopping.
> Jobs, salaries, money.
> The news, and other television entertainment.
> War and peace.
>
> All these entities have mental and physical aspects, but none is a mental or a physical entity. Every one is a social[-historical] entity. (Hersh, 1997, pp. 13-14)

In this passage Hersh mentions a wide variety of social-historical entities, some legal, e.g., eviction notices and the U.S. Supreme Court, some political, e.g., declarations of war and peace, some financial, e.g., money and salaries, and others recreational, e.g., sonatas and television programs. All of these items exist, and their existence has very real consequences, but they owe their existence to certain types of acts, decisions or practices undertaken by human beings. In suggesting that mathematical objects are social-historical objects, Hersh is suggesting that the same is true of mathematical domains, that is, mathematical domains exist, but they owe their existence to the mathematical practices of human (and other rational) beings. Let us call this the **social-institutional understanding of the metaphysics of mathematics**. In what follows, when I talk about mathematical domains as social constructs, I shall have in mind Hersh's social-institutional understanding of the metaphysics of mathematics.

Hersh's social-institutional understanding of the metaphysics of mathematics is more promising than Ernest's signifier-signified understanding of that metaphysics, for while many of the practices that constitute social-historical entities involve linguistic or discursive elements, at least in general these entities are not real in virtue of the linguistic and discursive elements of those practices. The above discussion of Ernest's metaphysical account of mathematics certainly suggests that if the basic social constructivist metaphysical insight is correct, then the same is true of mathematical domains.

The suggestion that mathematical domains are social-historical entities is radical and has not been given serious attention in the contemporary philosophy of mathematics literature. Indeed, what little attention it, or closely related theses, has received has been somewhat dismissive. Consider, for example, the following quote from Mark Colyvan's *The Indispensability of Mathematics* (Colyvan, 2001):

> There is . . . a second anti-realist position. According to this view, mathematical objects exist but they are mind- or language-dependent. I agree with Hartry Field (1989, pp. 1-2) that this last position is of little interest. The important question is whether mind- and language-independent

> mathematical objects exist or not. Having noted this second position, I will now largely ignore it. (p. 4)

I take Colyvan's (and Field's) sentiments to be typical. Yet the fact that the suggestion that mathematical domains are social-historical entities has been ignored or dismissed in the literature does not entail that it is without merit. Perhaps it has greater merits than have been recognized. Unfortunately, I cannot claim that either Ernest or Hersh has produced convincing arguments in favor of their radical suggestion. But Hersh has, at least, pointed in the direction of a motivation for accepting the thesis that mathematical domains are social-historical entities.

Let us consider for a moment legal and political borders, eviction orders and notices, and declarations of war and peace. Prior to the development of human societies no items of these types existed. The practices responsible for the existence of these items were introduced for a specific kind of reason. The development of complex human societies forced more and more interaction between human beings. This interaction needed to be regulated for everybody's benefit. Legal and political borders, eviction orders and notices, and declarations of war and peace were constructed as tools to aid in this regulation.

Furthermore, unlike practices that allow us to talk about the items that early human beings found around them, e.g., plants, animals, rivers, and rocks, the introduction of these practices was not motivated by early human beings' need to communicate about items external to human social practices. Neither was the introduction of these practices motivated by the need to incorporate the items constructed by these practices into scientific explanations, as were, for example, the introduction of the practices that include theories of atoms, tectonic plates, and genes.

Next, let us consider mathematics. Mathematicians are problem solvers. Mathematicians precisely formulate questions and then provide answers to those questions. For contemporary pure mathematicians, most of these questions are questions raised by existing pieces of mathematics. For early mathematicians and contemporary applied mathematicians, the questions were or are more likely to have been or be raised by topics outside of mathematics, e.g., counting your belongings, or precisely doubling the size of the local altar. Further, while some mathematical questions could or can be answered with existing tools, many required or require new tools. In fact, new mathematical tools are nearly always required for answering tough mathematical questions. The history of mathematics reveals that nearly all new branches of mathematics are introduced as tools for solving the types of problems addressed by mathematicians, e.g., a vast amount of abstract algebra — ring theory, field theory, etc. — was introduced in the hope that it would help in providing a proof of "Fermat's Last Theorem", and Fourier analysis was introduced to help solve a particular differential equation. Frequently, a new branch of mathematics will quantify over new mathematical entities. Thus, nearly all new mathematical entities are introduced as tools for answering questions addressed by mathematicians. It is this that I believe Hersh has in mind when he claims "*[m]athematical objects are created by humans. Not arbitrarily, but from activity with existing mathematical objects, and from the needs of science and daily life*" (Hersh, 1997, p. 16).

We can now observe a similarity between mathematical domains and legal and political borders, eviction orders and notices, and declarations of war and peace; these items are all tools. In fact, the similarity between mathematical domains and these social-historical items is more extensive. Once again, early human beings did not find mathematical domains in their environment. Admittedly, they found collections with various cardinality properties in that environment, and they found objects that had shapes that resembled those treated by Euclidean geometry in that environment, but there were no natural numbers or Euclidean shapes. Thus, the introduction of mathematical practices was not motivated by human beings' need to represent items external to human social practices *directly*. Of course, some of these practices might have been — in fact, probably were — motivated by human beings' need to represent items external to human social practices *indirectly*. So, for example, the introduction of mathematical practices that involve quantifying over natural numbers was probably motivated by the aid that these practices provided in indirectly representing and reasoning about cardinality properties of collections, and the introduction of geometrical practices was probably motivated by the assistance that geometrical practices provided in reallocating land following rivers flooding their banks.[7]

Furthermore, while some new mathematical tools were and are introduced to play a role in scientific explanations, e.g., Newton's development of the calculus, the role mathematical items play in scientific explanations is quite different to the role played by atoms, tectonic plates, and genes, etc., i.e., theoretical explainers, in such explanations. Mathematical tools serve a primarily representational role in scientific theories and explanations; they are needed for their ability to help represent spatio-temporal states of affairs *indirectly*. Theoretical explainers play a primarily causal-explanatory role in scientific explanations.

[7] A number of standard histories of mathematics note that early geometric practices arose on the banks of rivers and suggest that these agricultural needs might have motivated their introduction (see, e.g., (Eves, 1976)).

In addition, while it is standard for contemporary philosophers of mathematics to think about the application of mathematics in terms of natural science it should not be forgotten that much mathematics was introduced before science played the ideological role in western culture that it now plays. The examples of social-historical entities that I have been considering in the last couple of paragraphs are legal and political social constructs. Another aspect of the development of complex societies was the growth of complex practices of trade and commerce. These, too, motivated the introduction of social constructs, e.g., money and salaries. There is extensive evidence that, at its peak, Babylonian mathematics was more advanced than Greek mathematics. The widely accepted explanation of this fact is that the major Babylonian cities were far better located for the purposes of trade and commerce than were Greek cities (see, e.g., (Eves, 1976)). If this explanation is correct, then it provides us with good reason to believe that early mathematical development is linked with trade and commerce. That is, the introduction of early mathematical tools was motivated by the very thing that motivated various financial social constructs, among others. This certainly seems reasonable. Surely one needs efficient ways of representing and reasoning about cardinality properties of collections if one is going to engage in commercial exchange — you want to know that you get what you pay for. In a land-based economy, the ability to divide land in an equitable way is going to be important and is going to be greatly assisted by a reasonable knowledge of geometry. And if the practice of money-lending is going to thrive — an important aspect of any complex economy — then the ability to calculate interest on loans is going to be essential.

In the last few paragraphs I have been highlighting a variety of ways in which mathematical domains and the practices that quantify over their members are similar to social constructs and the practices that constitute them, and a variety of ways in which they are dissimilar to spatio-temporal entities and the practices that quantify over those entities. These analogies and disanalogies in no way establish the thesis that mathematical domains are social-historical entities, but I believe that they at least motivate our taking this thesis seriously. Further, it is time that this thesis was seriously investigated by an analytic philosopher. This is precisely what I intend to do in this dissertation.

The result of my investigation will, perhaps, be surprising, for I shall reach the conclusion that *an account of mathematical domains that takes them to be social-historical entities offers a better metaphysical interpretation of mathematical practices than do all Platonistic and Fictionalistic metaphysical interpretations of mathematical practices*. It is better also than the interpretation offered by Modal Nominalists, but unfortunately I shall not have the space to provide a comprehensive argument for this thesis in this dissertation.

In this dissertation I shall articulate and defend a new metaphysical interpretation of mathematical practices. I call this new interpretation **practice-dependent realism (PDR)**. PDRists advocate the thesis that *mathematical domains are pure constitutive social constructs constituted by mathematical practices*.[8] In essence, what this means is that PDRists maintain that all that there is to a mathematical domain existing is there being a mathematical practice that satisfies certain important constraints.

While the above analogies and disanalogies provide some motivation for accepting PDR, my primary strategy for defending PDR is arguing that, from the perspective of

[8]This terminology will be explicated in Chapter 1.

a certain, rather plausible, form of methodological naturalism, PDR's merits outweigh those of Platonism, Fictionalism, and Modal Nominalism. In what follows I shall argue for this claim with respect to Platonism and Fictionalism. In Part IV I shall hint at how this argument should go with respect to Modal Nominalism.

Ultimately, decisions concerning which metaphysical interpretation's merits are greatest are a matter of which reflective equilibrium has the greatest merits. Undoubtedly, each interpretation will have some positive points and some negative points. So, more precisely, I shall be offering arguments that suggest that a reflective equilibrium that sustains PDR has a number of advantages over reflective equilibria that sustain any form of Platonism or Fictionalism. In future, when I claim that PDR is preferable or superior to one of these metaphysical interpretations of mathematics, I hope that the reader will bear this in mind in forming a charitable interpretation of my claim.

Unfortunately, providing a complete justification of the fact that the reflective equilibrium that sustains PDR is preferable to any other reflective equilibrium would require significantly more argumentation than can be given in this dissertation. So my defense of PDR will, at best, be partial. Yet I shall be happy, and take myself to have achieved something, if I can at least show that there is a reflective equilibrium that sustains PDR, that that reflective equilibrium deserves to be taken seriously, and that that reflective equilibrium has some advantages over reflective equilibria that sustain any form of Platonism or Fictionalism.

0.2 Egalitarian Naturalism: A starting point

The starting point for my project is a variety of methodological naturalism. Methodological naturalism involves the

> ... abandonment of the goal of a first philosophy. It sees natural science as an inquiry into reality, fallible and corrigible but not answerable to any supra-scientific tribunal ...
>
> The naturalist philosopher begins his reasoning within the inherited world theory as a going concern. He tentatively believes all of it, but believes that some unidentified portions are wrong. He tries to improve, clarify, and understand the system from within. He is the busy sailor adrift on Neurath's boat. (Quine, 1981, p. 72)

What is it to abandon 'the goal of a first philosophy'? On the basis of how Quine continues the above quotation, it would be natural to suggest that it is simply to affirm the methodology of natural science and to claim that natural science is not answerable to any supra-scientific tribunal. Undoubtedly, most methodological naturalists do accord the pronouncements of natural scientists and their methodologies a certain type of respect. Yet, at least as I understand methodological naturalism, this respect is a consequence of methodological naturalism, not what is constitutive of it.

Descartes' reflections on first philosophy sought to provide 'external/alienated' justifications for far more than claims accepted on the basis of natural scientific investigations of reality; they sought to provide 'external/alienated' justifications for the pronouncements of common sense, e.g., they sought to provide 'external/alienated' justifications for the veridicality of everyday observations. Thus, the abandonment of the goal of a first philosophy is the abandonment of the goal of providing 'external/alienated' justifications for the pronouncements of both common sense and natural science. Abandoning this goal does not, of course, involve accepting that the pronouncements of common sense (or natural science) are infallible and incorrigible. In fact, it involves quite the contrary. Among the pronouncements of common sense are ones that clearly identify certain (other) pronouncements of common sense as false. Because of this, common sense is taken by methodological naturalists to incorporate a kind of self-regulation, a self-regulation which has resulted in its refinement. The pronouncements of natural scientists and the methodologies used in generating those pronouncements are accorded a certain type of respect by methodological naturalists precisely because those methodologies — and consequently, in a derivative sense, those pronouncements — are a product of this process of refinement. Common sense and natural science are continuous with one another from the perspective of a methodological naturalist.

One of the key ideas underwriting methodological naturalism, if not the key idea, is that, from the naturalist's perspective, this natural-scientific/common sense enterprise of understanding the world is open to neither criticism nor support from any vantage point external to that enterprise. Yet this does not mean that it is not open to criticism or support. On the contrary, it is the responsibility of a naturalistic philosopher to scrutinize rigorously this enterprise from within and to make explicit the support, both positive and negative, that various theories find within that enterprise. This is the case even if such support is not made explicit by (other) participants in that enterprise, including natural scientists. The point is merely that any criticism and support offered by a naturalistic philosopher must be made using standards that are at least implicit, if not explicit, within common sense or the practices of scientists.

Nowadays there are a number of methodological naturalisms on offer, and there is an increasingly subtle debate raging over which should be adopted and how best to formulate the commitments of a methodological naturalist.[9] While this debate is interesting, it is not the primary purpose of this dissertation to engage in it. I want

[9]See, e.g., (Maddy, 2005).

merely to say enough about the methodological naturalism that will be the starting point of my project to convey to the reader my commitments on this very intricate issue.

The methodological naturalism that I favor incorporates a number of the features of the methodological naturalism being espoused at present by Penelope Maddy (cf. (Maddy, 2005)[10]). It should be noted, however, that while most of Maddy's recent writings have been devoted to discussing what she calls the naturalized methodology of mathematics, this dissertation will primarily be part of the project that she identifies as naturalized philosophy of mathematics. The first project concerns the accurate description of mathematical practices and the evaluation of those practices on mathematical grounds. The second project concerns the role of a naturalized philosopher in evaluating, and accounting for, the mathematical aspects of the natural-scientific enterprise.

The reader familiar with Maddy's speculations concerning the metaphysical outcome of naturalized philosophy of mathematics should be aware that I do not agree with them. She speculates: "My guess is that, in the end, the explanations and accounts of naturalized philosophy of mathematics will not involve a literal appeal to the objects of pure mathematics" (Maddy, 2005, p. 457). *I intend to articulate and defend a naturalized philosophy of mathematics that maintains that mathematical domains exist in a literal, though non-Platonistic, sense.* A discussion of the mistake made by Maddy in her speculations on the metaphysical outcome of naturalized philosophy of mathematics will appear in Chapter 6.

[10]The foundations of this naturalism can be found in (Maddy, 1997), yet I feel that (Maddy, 2005) promotes a slightly different variety of naturalism than the one promoted in (Maddy, 1997).

Like all methodological naturalists, Maddy's, who for convenience I shall call Pen, and mine, who for convenience I shall call Jules, agree with Quine's on the fundamental idea that methodological naturalism involves the rejection of the goal of developing a first philosophy. Yet our methodological naturalists diverge from his with respect to their understanding of the role that mathematics plays in the natural scientific enterprise. Maddy explains the divergence as follows:

> My naturalist ... begins, as Quine's does, within empirical science, and eventually turns, as Quine's does, to the scientific study of that science. She is struck by two phenomena: first, most of her best theories involve at least some mathematics, and many of her most prized and effective theories can only be stated in highly mathematical language; second, mathematics, as a practice, uses methods different from those she's turned up in her study of empirical science. She could, like the Quinean, ignore those distinctive methods and hold mathematics to the same standards as natural science, but this seems to her misguided. The methods responsible for the existence of the mathematics she now sees before her are distinctively mathematical methods; she feels her responsibility is to examine, understand and evaluate those methods on their own terms, to investigate how the resulting mathematics does (and doesn't) work in its empirical applications, and to understand how and why it is that a body of statements generated in this way can (and can't) be applied as they are. (Maddy, 2005, p. 448)

While Jules has some concerns about the individuation of methods and methodologies, he is substantially in agreement with Pen about these observations. Jules would merely add an extra point to Pen's second observation. It is not merely that mathematicians use slightly different methods than most non-mathematical natural scientists. Mathematicians accept (and insist on) different standards of justification than non-mathematical natural scientists. Mathematicians' standards of justification are to a large extent, though not completely,[11] removed from empirical observation.

[11] The purpose of this caveat is to recognize that while pure mathematics now progresses in a manner that is, in places, almost completely independent of the needs of the non-mathematical

An important acknowledgement made by Pen is that she is privileging the non-mathematical natural sciences and only paying attention to mathematics because of its role in the enterprise of the natural sciences (cf. (Maddy, 2005, p. 449)). Yet, *pace* Quine's naturalist, Pen

> ... does not hold that those parts of mathematics that have been used in applications should be treated differently from the rest. She notes that branches of mathematics once thought to be far removed from applications have gone on to enjoy central roles in science, and perhaps more importantly, that the methods that have led to the impressive practice she now observes, the practice so liberally applied in our current science, are the actual methods of mathematics, not the methods of natural science (as the Quinean naturalist would have it) nor some artificially gerrymandered subset of mathematical methods (as exclusive attention to the methods of applied mathematics, as distinct from pure mathematics, would require). She concludes that the entire practice of mathematics should be taken seriously ... (Maddy, 2005, pp. 448-9)

Further, this privileging is important for Pen in answering questions such as the following: "If the naturalist, engaged in her scientific study of science, discovers that one practice of human beings (namely mathematics) is carried out using methods different from those of her natural science, why should she view this mathematical practice as different in kind from other practices with methods of their own, like astrology or theology?" (Maddy, 2005, p. 449) The answer, of course, is that "mathematics is used in science, so the naturalist's scientific study of science must include an account of how its methods work and how the theories so generated manage to contribute as they do to scientific knowledge. Astrology and theology are not used in

sciences, this was not always so. The early development of mathematics was to a much larger extent constrained by questions of application, and so was more closely linked to natural scientific investigation and theorizing. Further, the methods and standards of justification now accepted have their foundation in those earlier mathematical practices.

science — indeed in some variations they contradict science — so the naturalist need only approach them sociologically or psychologically" (Maddy, 2005, p. 449).

Jules has but one concern about the above, and it is in this respect that Jules disagrees with Pen. Pen takes her methodological home base, i.e., the group of sciences whose methods she adopts as her own, to be the natural sciences narrowly construed, where what this amounts to is the non-mathematical natural sciences. In contrast, Jules takes his methodological home base to be the natural-scientific enterprise of investigating and understanding the (spatio-temporal) world, where this enterprise includes both the mathematical and the non-mathematical aspects of the natural-scientific enterprise.

This difference between Pen and Jules is manifest in an important difference in attitude concerning what Pen calls "distinctively mathematical methods". Pen views "distinctively mathematical methods" as in some sense alien to her methodological practices because of their difference from the methods of the natural sciences narrowly construed. In contrast, Jules views "distinctively mathematical methods" as among the methods that constitute the very enterprise that he is engaged in, i.e., that of developing a natural-scientific understanding of the (spatio-temporal) world. Fundamentally, Pen is committed to the methods of the practitioners of the natural sciences narrowly construed, whereas Jules is committed to the methods of the practitioners of all of the sciences that play a role in the natural-scientific enterprise, i.e., the methods of both mathematical and non-mathematical scientists. For both Pen and Jules, the commitment is both to acknowledging that these standards are correct and constitutive of the practices being studied and to adopting these standards as her/his own for the pursuit of her/his study. Pen quite correctly acknowledges that the mathematical sciences are among the sciences that are part of the natural-scientific enterprise. And Pen and Jules agree that it is for this very reason that we should be interested in the methods and, Jules would add, standards of justification, of mathematicians. Yet despite Pen's acknowledgement that the mathematical sciences are among the sciences that are part of the natural-scientific enterprise, she is content, as we shall see in the next paragraph, to relegate the methods of mathematicians and the claims generated by those methods in a way that Jules finds to be peculiar and misguided.

One of the important observations that Pen makes in her discussion of the naturalized methodology of mathematics is that she recognizes that mathematicians are committed to various existential claims, such as "there are numbers and there are sets" (cf. (Maddy, 2005, p. 453 and p. 456)). Further, Maddy tells us that a "naturalized philosopher of mathematics must respect [mathematical] practice" (Maddy, 2005, p. 454). Given this, one would expect that Pen's naturalized philosopher of mathematics would respect the mathematicians' existential/ontological commitments. Indeed, how could Pen respect mathematical practice and not respect mathematicians' existential/ontological commitments? Unfortunately, Pen's naturalized philosopher of mathematics shows no such respect for the existential/ontological commitments of mathematicians. Instead, Maddy tells us: "for my naturalist, natural science [narrowly construed] is the final arbiter of what there is, and it doesn't seem to support its mathematical ontology" (Maddy, 2005, p. 456). It is this move that Jules finds particularly peculiar and misguided. Jules sees no need to take mathematicians to be second class citizens in the natural-scientific enterprise in the way in which Pen does by making these claims.

I'm sure that Maddy, and Pen, would acknowledge that talk about there being only one methodology adopted within the non-mathematical natural sciences is a simplification. In reality, different branches of the non-mathematical natural sciences adopt somewhat different methods and standards of justification, and, at times, there are conflicts between these different branches as a result. I suspect, however, that this simplification is relatively harmless, because the different branches' methods share important features in common. Pen wants, in answering ontological questions, to restrict herself to methods and standards of justification that are in some sense an idealization of the methods and standards of justification found in the non-mathematical natural sciences.[12] Jules, by contrast, sees no reason for this restriction. Admittedly, there are greater differences between the methods and standards of justification adopted by mathematical and non-mathematical scientists (considered as a unified group) than there are between the methods and standards of justification adopted by practitioners of different branches of the non-mathematical natural sciences. Yet this doesn't mean that mathematicians are not participants in the enterprise that constitutes our natural-scientific investigation of the world. And so, as we shall see, it doesn't mean that they shouldn't be shown the same respect as other participants in that enterprise.

It is common for scientists to adopt assumptions, very frequently idealized assumptions. If challenged to explain or justify their acceptance of these assumptions, they frequently pass this burden to scientists in more fundamental branches of science. Similarly, many non-mathematical natural scientists use mathematical theories with ontological commitments. If challenged to explain or justify their acceptance of

[12]It is a very tough issue to be precise about what is involved in this idealization. I am not going to attempt to do that in this dissertation.

these theories, they either temporarily put on mathematical hats or pass the explanatory or justificatory burden to mathematicians.[13] It is precisely this justificational deference that places mathematicians squarely in the camp of participants in the natural-scientific enterprise and mathematical practices among the practices that constitute the natural-scientific enterprise. Further, this justificational deference seems to not only relate to the issue of what is a consequence of what, but also to which axioms should be adopted and, very frequently as a consequence, which mathematical domains participants in the natural-scientific enterprise should be ontologically committed to. To deny that such justificational deference is present, Jules claims, is to make a mistake in describing the naturalized methodology of the natural scientific enterprise. As a consequence, Pen must either (i) provide evidence that Jules is mistaken with regard to the structure of justificational deference present in natural scientific practices; (ii) provide a good natural-scientific justification for this structure of justificational deference being misguided; or (iii) withdraw her claim that the non-mathematical natural sciences "are the final arbiter of what there is" (Maddy, 2005, p. 456).

The upshot of the above discussion is that mathematical practices are part of Jules' natural-scientific enterprise and, as such, mathematicians and their methods should be accorded the same respect as other practitioners involved in that enterprise and their methods. In the same way that Jules wouldn't dream of giving a non-mathematical natural scientist anything but reasons acceptable to practitioners of her science for denying one of her ontological commitments, he wouldn't dream of giving a mathematician anything but a mathematical reason for denying one of her

[13]Examples include Dirac's delta function and infinitesimals.

ontological commitments. Physicists, or, more precisely, the standards operative within their practices, get to decide whether electrons exist. And mathematicians, or, more precisely, the standards operative within *their* practices, get to decide whether numbers, sets, etc. exist. In an important sense, ontological commitment is to be settled by standards that are determined locally.[14] In this respect, my naturalism is more closely allied with the one advocated by John Burgess than it is with Maddy's. His position is illustrated by the following quote taken from his book with Gideon Rosen: "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, p. 212).

One might wonder how this difference between Jules and Pen is going to affect the various commitments and reasons outlined above on behalf of Pen in her rejection of Quine's attitude towards mathematics. The answer is, very little. Jules will follow Pen by making the same observations about the role of mathematics in the natural scientific enterprise and the distinctiveness of certain methods and standards of justification operative within mathematical practices. Jules will also follow Pen in taking the Quinean to be misguided in assessing mathematics holistically by the standards of the non-mathematical natural sciences and thus, in turn, will follow Pen in taking it to be his responsibility to investigate fully the methods and standards of justification operative within the practices of mathematicians. Indeed, with respect to the project of discussing the naturalized methodology of mathematics, Jules and Pen should be in full agreement.

[14]This places me at odds with indispensability theorists. Unfortunately, I shall not be able to discuss this disagreement more fully in this dissertation.

Further, Jules' understanding of the methods and standards of justification operative in mathematics will, in turn, lead him to have to address a number of questions as to how those methods and standards of justification, and the results generated by using them, can effectively be applied in his enterprise of investigating and understanding the spatio-temporal world. These questions might have to be asked in a slightly different way by Jules than by Pen, but they are essentially the same questions. Jules will also follow Pen in rejecting any kind of move, such as Quine's, that would divide mathematics artificially into two parts.

Indeed, so far as I can tell, the only even mildly substantive difference between Jules and Pen with respect to the above commitments will be with respect to how he answers the challenge concerning why he doesn't show the same respect for the practices of astrology and theology as he does for the mathematical sciences. The straightforward answer that Jules will provide is that these practices are not part of the natural-scientific enterprise of investigating and understanding the world in the way in which non-mathematical natural scientists' justificational deference to mathematicians makes mathematical practices part of that enterprise. It should be clear that even this answer does not constitute a radical difference in perspective to the answer that Pen would provide.

In summary, I have argued that any adequate methodological naturalism must both find a way to understand the mathematical and non-mathematical sciences as constituting a unified approach to the natural-scientific investigation of the world and, at the same time, recognize and respect significant methodological and justificatory differences between these two groups of sciences.[15] It is my hope that those who, like

[15]Perhaps it would be more accurate to say that I have sketched such an argument.

myself, are inclined towards some form of methodological naturalism will agree with the observations that I have made above. Those who do will find themselves inclined to adopt a form of methodological naturalism that fulfills the minimal constraints that I have just outlined. Of course, it has to be recognized that these are merely minimal constraints. What I have said leaves many interesting questions about the specific form of my methodological naturalism unanswered. For my immediate purposes I see no need to answer these questions. For convenience, let us call any variety of methodological naturalism that satisfies the constraints outlined above an Egalitarian methodological naturalism, or, for brevity, simply an **Egalitarian naturalism**. And let me call any proponent of a variety of Egalitarian naturalism an **Egalitarian naturalist**.

Earlier I argued that naturalists' respect for the pronouncements of natural science is a consequence of natural science's being a refinement of common sense. With this in mind, the Egalitarian variety of naturalism that I advocate should be seen to be more plausible than a variety of naturalism that advocates an asymmetry in attitude between the mathematical and non-mathematical aspects of the natural scientific enterprise. Mathematics and its methods are as much a refinement of common sense as are the methods of any non-mathematical natural science. It is clear that the truths of arithmetic are as pre-theoretically legitimate as any ordinary observation.

Above, in passing, I mentioned that the assumption that the non-mathematical natural sciences share a single methodology is relatively harmless. One potential harm of this assumption can now be made explicit, however. A lack of appreciation of the differences in methodology between the various branches of the non-mathematical natural sciences is likely to obscure the diversity of ways in which common sense has been refined. This, in turn, is likely to obscure the fact that mathematics and mathematical methods are just one more collection of ways in which common sense has been refined. This, in turn, can lead to the kind of privileging of the non-mathematical sciences over the mathematical sciences of which I take Maddy to be guilty.

0.3 Structural Preliminaries

In recent years, one form of Platonism that has found a small but influential group of supporters is Neo-Fregean logicism. Crispin Wright's *Frege's Conception of Numbers as Objects* (Wright, 1983) gave a new impetus to a Fregean project in the philosophy of mathematics. And Wright has been joined by Bob Hale in the steadfast defense of the Neo-Fregean project he outlined in that book. Many of the most central essays written by Hale and Wright are collected in (Hale and Wright, 2001).

Perhaps the most important feature of Neo-Fregean logicism is Hale's and Wright's interpretation of Frege's application of his infamous context principle in Sections 56-69 of his *Grundlagen* (Frege, 1884). In Section 62 of his *Grundlagen*, Frege converts an epistemological enquiry about numbers into an investigation of the senses of numerals:

> How then, are numbers to be given to us, if we cannot have any ideas or intuitions about them? Since it is only in the context of a proposition that words have any meaning, our problem becomes this: To define the sense of a proposition in which a number word occurs. (Frege, 1884, Section 62)

In this passage, Hale and Wright take Frege to be proposing a linguistic answer to a traditional metaphysical question, i.e., what is it to *be* an object? That is, what is the nature of the category object? An author whose interpretation of Frege is similar to Hale's and Wright's on this matter is Thomas Ricketts. Here are a couple of quotes from his paper *Objectivity and Objecthood: Frege's Metaphysics of Judgment* (Ricketts, 1986) that I believe offer a clear explication of what he takes Frege's answer to have been to the above metaphysical question:

> The crucial feature of [the Platonistic] line of interpretation is its taking ontological notions, especially that of an independently existing thing, as prior to and available apart from logical ones, from notions of judgment, assertion, inference, and truth. The explanatory priority of ontological notions renders intelligible and inevitable the questions, "How does language hook on to reality?" and "How do we know that the ontological presuppositions of our discourse are satisfied?" ...
>
> There is another philosophically more interesting and historically more apt construal of Frege's work, one which denies to ontological notions the independence and primacy they have on the Platonist interpretation. As I read Frege, ontological categories are wholly supervenient on logical ones. This supervenience is the product of the fundamental status that Frege assigns to judgment. ... The priority of judgment is to guarantee its objectivity, as exhibited in the linguistic practice of assertion, against general challenge. Thus, it is meant to render unintelligible the chasm between thought and reality that is the consequence of the Platonist reading. (Ricketts, 1986, p. 66)

> We are now in a position to understand how ontological categories are, for Frege, supervenient on logical ones. The logico-syntactic source of the notion of an object lies in first-level generality. To be an object is to be indefinitely indicated by first-level generality. Our grasp of the notion of an object — simply the notion of an object, not an object of this or that kind — is exhausted by the apprehension of inference patterns and the truth of basic logical laws in which these variables figure. ...
>
> Similar remarks hold for the notion of a concept. The logico-syntactic source of this notion lies in our apprehending basic inference patterns turning on second level variables. (Ricketts, 1986, p. 89)

Hale and Wright take Frege to have provided a similar, yet slightly different, answer. They take Frege to have claimed that to be an object is to be that for which a singular term — a proper name in Frege's terminology — stands. Wright dubs the slogan 'objects are that for which singular terms stand' and some attendant theses, including one making a similar claim about the *Bedeutung* of predicates —

concepts in Frege's terminology — the syntactic priority thesis. Hale's and Wright's interpretation differs from Ricketts' in emphasizing individual reference as achieved by singular terms over generalized reference as achieved by first-level generality. Yet it shares with Rickett's interpretation of Frege the recognition that our grasp of certain ontological categories, e.g., object, is exhausted by our familiarity with certain inference patterns. Hale and Wright would talk about these inference patterns as syntactic features of our discourses.

By endorsing the syntactic priority thesis, Hale and Wright are at least pointing in the direction of, if not explicitly endorsing, an important and new metaphysics, one which, at least partially, inverts the order of explanatory priority traditionally accepted by Platonist philosophers between discursive practices[16] and the metaphysical domains they represent. One[17] useful way of conceiving of PDR is as a more radical metaphysical step in the direction pointed towards by Hale and Wright in endorsing the syntactic priority thesis. As a consequence, PDRists face many of the same explanatory burdens that Neo-Fregean logicists do. For this reason, it will be useful to consider these burdens and use them to structure my exposition of PDR.

For convenience, I shall use Fraser MacBride's formulation of the metaphysics underwriting Neo-Fregean logicism (cf. *Speaking with Shadows: A Study of Neo-Logicism* (MacBride, 2003)) to motivate some of the questions that projects that attempt the kind of explanatory inversion characteristic of PDR must face. I shall

[16]By a discursive practice I mean a practice that embodies all of the inferential trappings of assertoric content. For a reasonable indication of what I have in mind, I refer the reader to Crispin Wright's discussion of assertoric content and minimal truth in the opening chapters of (Wright, 1992). Wright emphasizes embedding in negations and conditionals; an ideal account should also mention some criteria concerning quantification.

[17]There are several other ways of conceiving of PDR that should become clear in the course of this dissertation.

articulate PDR by answering these questions. After answering some of these questions directly, I shall offer arguments that suggest the superiority of PDR to Platonism and Fictionalism. These arguments will provide answers to my other questions.

I want to be candid in admitting that it is a challenging interpretative issue to assess whether the metaphysics that MacBride ascribes to Neo-Fregean logicism, which he dubs "**neo-fregeanism**", is explicitly endorsed by either Hale or Wright, or is one to which either of them is committed by the claims that they do explicitly endorse. My interest in neo-fregean metaphysics relates to its ability to serve as a useful foil against which I can develop practice-dependent metaphysics. Given this interest, I shall not address this challenging interpretative issue in this dissertation. For clarity, when referring to MacBride's metaphysical proposal concerning the metaphysics of Neo-Fregean logicism I shall use lower-case letters, while when referring to Neo-Fregean logicism itself I shall capitalize the N and the F.

The remainder of these preliminaries will be structured in the following way. In Section 0.4 I shall provide a summary of MacBride's formulation of the metaphysics underwriting Neo-Fregean logicism. In Section 0.5 I shall identify questions that naturally arise for this project and articulate the structure of my dissertation.

0.4 MacBride's Characterization of Neo-Fregean Logicism

According to MacBride's formulation of Neo-Fregean logicism, eight theses play a central role. We shall consider only the first three here since these three encapsulate the central idea behind neo-fregeanism. First, we have:

> (SP1) *Syntactic Decisiveness*: if an expression exhibits the characteristic syntactic features of a singular term then that fact decisively determines

> that the expression in question has the semantic function of a singular term (reference).[18] (MacBride, 2003, p. 108)

This thesis is silent on the matter of what the characteristic syntactic features of a singular term are. The actual Neo-Fregean logicists, following Michael Dummett (cf. (Dummett, 1973)), have adopted an inferential-role understanding of singular termhood. That is, they maintain that playing a particular role in certain types of inferences is characteristic of singular termhood (cf. (Hale, 1994) and (Hale, 1996)).

The second thesis is as follows:

> (SP2) *Referential Minimalism*: the mere fact that a referring expression[19] figures in a true (extensional) atomic sentence determines that there is an item in the world to respond to the referential probing of that expression. (MacBride, 2003, p. 108)

While this thesis certainly expresses an important idea that is central to neo-fregeanism, as it stands it is incomplete in that it fails to mention three conditions that are important to the project. In particular, the sentence in question should be a sentence of a well-disciplined, contentful, and assertoric, i.e., discursive, practice. These three conditions embody Wright's commitment to the thesis that there is no deeper notion of assertoric content than that generated by practices that fulfill certain minimal criteria. For textual evidence that these three conditions are indeed part of Wright's metaphysical conception of the relationship between a discursive practice and the domain it represents, I refer the reader to (Wright, 1992, Chapter 1). In light of this observation, a better formulation of (SP2) is as follows:

[18] As we shall see, I believe that the Neo-Fregeans have made a mistake here in choosing singular terms rather than first-order quantifiers.

[19] The phrase 'referring expression' should not be understood as a success term in this thesis, thesis (SP2'), or in my discussion below.

> (SP2') *Referential Minimalism*: the mere fact that a referring expression figures in a true (extensional) atomic sentence of a well-disciplined, contentful, assertoric discourse determines that there is an item in the world to respond to the referential probing of that expression.

One might be tempted to claim that this thesis is trivial. After all, an atomic[20] sentence containing a referring expression cannot be true unless there is an item in the world which that referring expression picks out. Yet this line of reasoning is prone to misunderstand the force of this thesis by failing to appreciate how this thesis interacts with the third thesis that is characteristic of neo-fregeanism:

> (SP3) *Linguistic Priority*: linguistic categories are prior to ontological ones; an item belongs to the category of object[21] if it is possible that a singular term refer to it. (MacBride, 2003, p. 108)

In conceiving of (SP2') as trivial, one is tempted to rely implicitly on the idea that there is a reality that is metaphysically independent of our discursive practices in the sense that those practices do not affect its **ontological structure** — its structuring into objects, properties and relations. Further, one relies on the idea that the truth of a sentence is a matter of that sentence correctly representing that independent reality, where 'correct representation' can be understood on the model provided by model-theoretic semantics. If the sentence in question is an atomic sentence containing a referring expression, then correct representation requires the referring expression to be appropriately related to, i.e., to refer to, an object in that independent reality.

[20]There is an issue about what it is for a sentence of a natural language to be atomic that I shall not address here.

[21]Macbride writes objects, but I believe that this is a typographical error. I am also tempted to ask whether the if in this thesis should be an only if, or indeed an if and only if.

This understanding of the situation places the independent reality prior to, and independent of, our discursive practices. Well, it does this unless discursive practices are the subject matter of the discourse.

The neo-fregean does not accept this understanding of the relationship between our discursive practices and the domains they represent. She takes discursive practices — specifically, well-disciplined, contentful, assertoric practices that embody well accepted standards of correct usage — to exert an influence over reality. Whether an atomic sentence in such a practice is true or not is not determined by whether it accurately represents an independent reality, but is rather determined by whether or not it conforms to established standards concerning correct usage of the expression in question.[22] If it does, and it contains a referring expression, then reality is such that it will inevitably contain an object to which that referring expression refers.

One might legitimately wonder how it could be that under the said conditions reality will inevitably contain an object to which that referring expression refers. Here are two attempts by MacBride to characterize the neo-fregean idea:

> The [opponent] assumes that the structure of states of affairs is *crystalline* — fixed quite independently of language. By contrast, the neo-[fregean] assumes that states of affairs lack an independent structure, that states of affairs are somehow *plastic* and have structure imposed upon them by language. (MacBride, 2003, p. 127)

> . . . the neo-fregean claims, reality and language are so related that, if we speak truly, the structure of reality inevitably mirrors the contours of our speech. (MacBride, 2003, p. 108)

[22] While this kind of idea has few proponents for discursive practices that are almost universally considered to be objective, such as mathematics and the physical sciences, it does have a number of proponents for discursive practices that are generally thought of as less objective, such as ethics.

Of course, all that we really have here, at least in the first quote, is a metaphor. Yet it is this metaphor that I intend to investigate further to see what sense I can make of it because, I believe that it will be useful in explicating PDR.

MacBride's characterization of the impact of the acceptance of the syntactic priority thesis in the above explicitly metaphysical terms is bold, for one aspect of Hale's and Wright's project is the interpretation of Frege. And, as Ricketts makes clear in the passages above, the type of interpretation of Frege that Hale and Wright offer takes certain types of explicitly metaphysical concerns to be illegitimate. It is for this reason that it is a controversial issue to assess whether MacBride's characterization of the metaphysics underwriting Neo-Fregean logicism is correct.

Yet in fairness to MacBride, Hale and Wright do, on occasion, make explicitly metaphysical claims while articulating and defending Neo-Fregean logicism. Further, given that Neo-Fregean logicism is recommended to the contemporary community as a philosophy of mathematics it should endorse, it is certainly legitimate for that community to investigate it using contemporary methods and standards. Thus, it is legitimate for a contemporary metaphysician to investigate whether there is a metaphysical account of mathematical domains according to which language and reality are as closely related as Hale and Wright take Frege to have believed them to be. The above characterization of neo-fregeanism is, I take it, the result of this type of investigation by MacBride. And I suspect that it differs from a characterization of the metaphysics of Neo-Fregean logicism that Hale or Wright would provide in virtue of their not having taken the thorough interest in the metaphysics of the context principle that MacBride has in providing his characterization of neo-fregeanism.

As mentioned above, my interest in neo-fregean metaphysics relates to its ability to serve as a useful foil against which I can develop practice-dependent metaphysics. Given this interest, I, like MacBride, will adopt the perspective of a contemporary metaphysician and seek to use the tools I have available to me as such in offering an account of the metaphysics underwriting Neo-Fregean logicism. This perspective will lead me also to characterize neo-fregean metaphysics differently from Hale and Wright. Yet my interest is neither in Frege exegesis nor in providing textual evidence for my characterization of neo-fregeanism. Indeed, I am not even interested in establishing the genuine intelligibility of the neo-fregean metaphor — ultimately, I severely doubt that it is intelligible. My interest is in using neo-fregeanism, which is one reasonable attempt to offer an interpretation of the metaphysics of Neo-Fregean logicism, to articulate the metaphysics of PDR.

With these caveats in place, let us now consider how MacBride's three theses can be put together to obtain the neo-fregean's conception of the close relationship between discursive practices and the domains they are about. First, let us suppose that our analysis of singular termhood yields a collection $C_1, \ldots, C_m$ of logico-syntactic conditions that are jointly sufficient for an expression "n" to be a singular term. Second, let us suppose that "t" is an expression that a) satisfies conditions $C_1, \ldots, C_m$, and b) appears in at least one true (extensional) atomic sentence of a discursive practice. Then, in virtue of "t" satisfying $C_1, \ldots, C_m$, it is a singular term (*Syntactic Decisiveness*), and, in virtue of "t" satisfying b), there is an item in the world to which it refers (*Referential Minimalism*). Finally, since "t" is a singular term, the item to which it refers is an object (*Linguistic Priority*). As Macbride puts it, "Together these doctrines establish that the syntactic form of our (true) sentences cannot deceive us; reality cannot fail to include the objects . . . these sentences apparently describe" (MacBride, 2003, p. 108).

0.5 Overview of this Dissertation

MacBride's formulation of neo-fregeanism leaves me with three natural groups of questions and/or concerns about the relationship that (SP1) - (SP3) serve to give expression to between discursive practices and the domains they are about. First, and perhaps most fundamentally, there are some basic metaphysical concerns. How could it be that the simple appearance of a referring expression in a true extensional atomic sentence of a discursive practice is sufficient to guarantee the existence of an object in the world to which that expression refers? Can MacBride's reading of neo-fregeanism as requiring that reality be in some sense plastic help us to make sense of this?

In Part I of this dissertation I shall explore various senses in which domains might be metaphysically dependent on (socially constructed by), and metaphysically independent of, discursive practices. This exploration will help me to provide a more detailed characterization of Platonism and neo-fregeanism (cf. Chapter 1). In addition, it will allow me to offer an account of the metaphysics of PDR (cf. Chapter 2).

The second group of concerns that I have concerning neo-fregeanism surrounds the technical details underwriting its implementation. Theses (SP1) and (SP3) are formulated in terms of singular terms. Following W.V.O. Quine's work on ontological commitment (cf. (Quine, 1948)), it has become standard to conceive of projects that seek to characterize ontological commitments to objects in terms of the range of first-order quantifiers as opposed to the referents of singular terms. Is there a conflict here between the neo-fregean project and Quine's writings on ontological commitments? Whether or not there is a conflict with Quine, the suggestion that singular terms be seen as a guide to ontological commitment seems to run into the following problem. It is well known that several mathematical domains are accepted by mathematicians to be uncountable. Yet no known discursive practice (at least, none that could be spoken by human beings) could have uncountably many singular terms. How is a neo-fregean or PDRist to deal with the domains associated with these mathematical practices? I shall conclude Part I, and my initial articulation of PDR, by answering these questions.

Part II of this dissertation will consist in a defense of the thesis that *Platonism is a philosophy of mathematics that should not be advocated by an Egalitarian naturalist.* My argument against a Platonist presents her with a dilemma surrounding her acceptance or rejection of the following assumption concerning mathematical theories, which I call **CYF**:

> It is possible for a mathematical theory to be coherent yet false.

The argument against those Platonists who accept CYF is the epistemological argument against Platonism. In Chapter Three I shall articulate Hartry Field's version of this argument. I shall then defend and develop Field's version of the epistemological argument by discussing and rejecting two criticisms of it: one offered by Jerrold Katz (cf. (Katz, 1981)) and David Lewis (cf. (Lewis, 1986)), the other by John Burgess and Gideon Rosen (cf. (Burgess and Rosen, 1997)). *The epistemological argument will be sustained against Platonisms accepting CYF.*

I shall end Part II of this dissertation in Chapter 4 by arguing that Egalitarian naturalists should not accept varieties of Platonism that solve the epistemological challenge to Platonism by rejecting CYF. Perhaps the most prominent variety of Platonism to reject CYF is so-called plenitudinous, or full-blooded, Platonism (cf. (Balaguer, 1998)). I shall provide an extensive discussion of this variety of Platonism in Section 4.3. The distinctions that I draw relating to metaphysical dependence and social construction in Chapter 1 will allow me to present a second potential way in which a Platonist might reject CYF. The upshot of the discussions found in Part II of this dissertation will be that *all forms of Platonism should be rejected by any Egalitarian naturalist*.

The third group of questions I have concerning neo-fregeanism surrounds the explanatory inversion envisioned by it. Traditionally, Platonistic accounts of aspects of reality have been taken to provide some sort of metaphysical grounding for mathematical practices. The notion of a metaphysical grounding for a discursive practice is far from transparent. Here is a rough characterization. A domain (or collection of facts) X serves to metaphysically ground a discursive practice Y if X exists completely independently of Y and Y serves to represent X. Traditionally, Platonistic authors have also required one or both of the following conditions to hold of a domain that metaphysically grounds a practice: i) the domain (or collection of facts) serves to explain why the practice is the way it is; ii) the domain (or collection of facts) serves to justify why the practice is the way it is. So, for example, one might be interested in explaining why we have mathematical practices at all. One answer that might be offered by an individual who takes our mathematical practices to be metaphysically grounded by mathematical domains that exist quite independently of social practices is the following: we introduce mathematical discourses to represent mathematical domains that exist quite independently of social practices. A second question that one might be interested in is why mathematical practices are the way they are. For example, one might be interested in an explanation or justification of why set theorists take the axiom of choice to be true rather than false. A Platonist who takes mathematical discourses to be metaphysically grounded by mathematical domains that exist quite independently of social practices might offer the following type of explanation or justification: mathematical discourses take the form they do, because that is the form they must take in order for them to be true of the mathematical domains that they represent. So, for example, set theorists take the axiom of choice to be true, because it *is* true of the domain of sets. As an instance of an individual who offers this type of explanation/justification, one can consider Kurt Gödel. He insisted that the set-theoretic hierarchy was such that features of it determined whether set theorists should accept or deny the continuum hypothesis (cf. (Gödel, 1947)).

While it may be that Neo-Fregean logicists can offer the first of these types of explanations/justifications, it is clear that they cannot offer the second. The explanatory inversion embodied in neo-fregeanism amounts to the rejection of at least the second type of explanation/justification. The explanatory inversion characteristic of PDR requires that explanations of the first type also be rejected. In light of this, it seems legitimate to ask: what kinds of answers can we provide to these types of questions? If mathematical practices don't exist to represent accurately mathematical domains that are metaphysically independent of all social practices, then why do we have such practices? Further, why do mathematical practices take the form that they do? And, what role do mathematical practices play in our cognitive economy?

These are all questions that Fictionalist philosophers of mathematics, among others, have been engaging for years. It is to one particular variety of Fictionalism, the one that has recently been articulated and defended by Stephen Yablo (cf. (Yablo, 2002a), (Yablo, 2002b), and (Yablo, ToAp)), that this PDRist turns for the inspiration for his answers to these questions.

In Part III of this dissertation I shall discuss Fictionalism. Part III will open, in Chapter 5, with a discussion of Yablo's Hermeneutic Fictionalism. This discussion will detail the basic flavor of his answers to the above questions and present the reader with a conception of one particular variety of Fictionalism. During this discussion I shall identify a feature of Yablo's Fictionalism that I take to be characteristic of Fictionalist philosophies of mathematics in general. I shall then argue, in Chapter 6, that this feature of Fictionalist philosophies is responsible for their inferiority to PDR from the perspective of an Egalitarian naturalist. My diagnosis of Fictionalism will also serve as a diagnosis of Maddy's attitude towards the ontological commitments of mathematics.[23] My discussion will, in addition, make it clear that PDRists can offer the answers to the above questions that Yablo offers on behalf of his Fictionalism.

Finally, in Part IV, I shall provide a brief discussion of a number of topics that I shall be unable to treat in full detail in this dissertation. Perhaps the most important of these are the topics of the objectivity and necessity of mathematics. I shall also highlight a number of interpretative issues that need further attention from a PDRist. For example, how should the notion of coherence that plays such a fundamental role

[23]Recall (cf. Section 0.2) that Maddy claims that "natural science [narrowly construed] is the final arbiter of what there is, and it doesn't seem to support its mathematical ontology" (Maddy, 2005, p. 35).

in this dissertation be understood? These discussions will complete my treatment of PDR.

PART I

THE METAPHYSICS OF MATHEMATICAL DOMAINS

Overview of Part I

The primary purpose of Part I of this dissertation is to articulate the interpretation of mathematical practices that I call practice-dependent realism (PDR). Since PDR's novelty is primarily metaphysical, much of Part I will concentrate on metaphysics. After articulating the metaphysics of PDR, I shall also discuss its implementation for mathematical practices.

In order to make the metaphysics of PDR clear it will be useful to contrast it with traditional forms of Platonism and neo-fregean Platonism. Consequently, the topic of Chapter 1 is the metaphysics of Platonism. In order both to distinguish traditional forms of Platonism from neo-fregean Platonism and to show what these Platonisms have in common, it will be necessary to discuss social construction and metaphysical dependence. It is with these topics that I shall open Chapter 1. I shall begin by articulating a distinction found in Sally Haslanger's work (cf. (Haslanger, 1995)) between causal and constitutive social construction (cf. Section 1.1). With this distinction in place, I shall use it to provide a general discussion of metaphysical dependence and independence (cf. Section 1.2). I shall then use these discussions to provide a general characterization of Platonism and offer an interpretation of neo-fregean metaphysics (cf. Section 1.3). Finally, in Section 1.4, I shall discuss the intelligibility of neo-fregean metaphysics.

The topic of Chapter 2 is PDR. In the opening sections I shall draw on the metaphysical discussions found in Chapter 1 to articulate the metaphysics of PDR and distinguish PDR from traditional forms of Platonism and neo-fregean Platonism. Once the metaphysical proposal underlying PDR is clear, I shall discuss some of the details surrounding its implementation for mathematical practices. By the end of

Chapter 2, the reader should have a reasonably detailed understanding of PDR and how it answers some of the more challenging questions raised in the Dissertation Preliminaries.

CHAPTER 1

THE METAPHYSICS OF PLATONISM

1.1 Causal and Constitutive Social Construction

In "Ontology and Social Construction" (Haslanger 1995), Sally Haslanger gives expression to a variety of ways in which social activities might be involved in the social construction of existent items. The most basic distinction she makes is that between "causal social construction" and "constitutive social construction". All of the examples of social-historical items that Hersh mentions in the long quote in the Dissertation Preliminaries are constitutive social constructs, for they involve some kind of constitutive social construction. Other examples of constitutive social constructs include legal borders between pieces of property (land), political borders between countries, pieces of property, countries, laws (in the sense of statutes),[24] and games (like baseball and tennis). These items exist, but owe their existence to the constitutive significance of acts, decisions or practices of social importance, where by a **practice** I mean simply a collection of activities governed by normative constraints.

As examples of causal social constructs, consider items like houses, cars, scissors, and chairs, i.e., artifacts, where an artifact is a spatio-temporal entity that has been

[24]Whenever I talk about laws in this dissertation, I shall be talking about laws in the sense of statutes rather than laws in the sense of laws of nature, or laws in the sense of the laws of probability.

manufactured for some particular purpose. Artifacts of this type are causally constructed in the sense that their creators causally manipulated the spatio-temporal world to bring them into existence. This causal construction is social in nature if either the purpose for which this construction took place was social, or the implementation of this construction was social.[25]

A more interesting, and correspondingly controversial, example of causal social construction is argued for in the feminist literature. In the feminist literature, categorization according to gender is distinguished from categorization according to sex. And, at least until recently (cf., e.g., (Butler, 1993)), the features relevant to determining the sex of an individual were or are biological in nature, while the features relevant to determining the gender of an individual were or are social in nature. Many feminists argue that verbally classifying human beings as women or men, i.e., according to gender, has a causal impact on them resulting in their changing their behavior to conform to social norms concerning gender roles (cf., e.g., (Haslanger, 1995)). These feminists argue that, as a consequence, the genders of *particular individuals* are causal social constructs. The important point is that the verbal acts of classification have a *causal impact* on the human beings affected by them, causing them to take on new characteristics.

Haslanger offers the following characterizations of causal and constitutive social construction:

[25] In claiming that artifacts are causal social constructs, I am not excluding that they are also constitutive social constructs. On the contrary, many, if not most, artifacts are also constitutive social constructs. So, for example, roughly speaking, something is a chair in virtue of it playing (fulfilling) a recognized *social* role (function). This fact about chairs makes them constitutive social constructs.

Causal social construction: Something is causally socially constructed if and only if social factors play a causal role in bringing it into existence or, to some substantial extent, in its being the way that it is.

Constitutive social construction: Something is constitutively socially constructed if and only if a correct definition or account of what it is for something to be an item of the type in question must make reference to social factors.[26]

Legal borders between pieces of property, political borders between countries, property, countries, laws, and games are constitutive social constructs, because the social significance of acts, decisions or practices in constituting these items ensures that social factors have to be adverted to in a correct definition or account of what they are.

In case it is not clear, let me attempt to pinpoint the exact difference between these two varieties of social construction. Toward this end, consider Plato's famous discussion of piety in his *Euthyphro*. One thesis promoted by Euthyphro in that dialogue is that an act is pious if and only if the Gods love it. After Euthyphro states this thesis, Socrates asks him whether an act is pious because the Gods love it, or the Gods love it because it is pious. At one point in the dialogue, Euthyphro opts for the first option, thereby suggesting that the Gods' loving of an action could make it the case that the action in question is pious. While Plato does not explore this in the *Euthyphro*, there are in fact two ways in which the Gods' loving of an action might make it the case that the action in question is pious. These two ways correspond to the two varieties of social construction I am explicating.[27] First, the Gods' loving of

[26]These definitions are taken from (Haslanger, 1995, p. 98), though I have slightly modified the second.

[27]I borrow this way of explicating the distinction between causal and constitutive social construction from an interaction with Louise Antony, whom I thank for a number of useful conversations on this topic.

an action could have a "causal" or mechanistic impact on the action, mechanistically changing its characteristics in such a way as to make it take on a new characteristic, "piety". Such mechanistic or causal modifications of features of the world are the mode of construction characteristic of causal construction. Some collection of social factors playing this type of causal or mechanistic role in the determination of the characteristics of an item — *whether it is intended or not* — is what it is for that item to be causally socially constructed.

The second way in which the Gods' loving of an action might make it pious is the following: it could be that an action is pious wholly in virtue of the Gods' loving of it, or, to put this another way, there is nothing to the act's being pious over and above the Gods' loving of it. So, in particular, there is no need for the Gods to change in any mechanistic way the characteristics of the act in question.[28] Some collection of social factors playing this kind of role in the determination of an item is what it is for the item in question to be constitutively socially constructed.[29]

Given that constitutive social construction is here being contrasted with causal social construction, I should note that it is not that constitutive social constructs can have no influence over how the spatio-temporal world is or, indeed, that the spatio-temporal world can have no influence over which items we construct constitutively. One only need reflect on the impact of declarations of war to recognize this. What our contrast emphasizes is that the mechanism by which an item becomes a constitutive

[28]This is the option that I believe Plato had in mind in the *Euthyphro*.

[29]The reader might be concerned about how this characterization of constitutive social construction matches with Haslanger's definition. The thought is that it is because of the significance of some type of social act or decision that social factors have to be adverted to in a correct account of the item in question. Even if the explication given here is not coextensive with Haslanger's definition, the kinds of examples pointed to by this explication are the ones of central importance to this work.

social construct is not causal in nature. Rather, this status is achieved by means of acts, decisions or practices of social import.

While my exposition so far might suggest that constitutive and causal social construction are mutually exclusive, this is not the case. Many cases of social construction involve both elements, though one or the other might be dominant in any particular case. An excellent example of this is a regulation baseball for major league play. Two distinct types of considerations are involved in something's being a regulation baseball. First, the ball in question must have certain physical characteristics, e.g., it must be a certain size, shape, color, etc. Baseballs are manufactured to have these characteristics. Thus, they are causal social constructs. The second consideration is that the ball has to have been deemed regulation by an individual acting on behalf of the league and be signed by the league's commissioner. This consideration makes regulation baseballs constitutive social constructs. A similar situation surrounds a piece of paper being a five-dollar bill. More is required than the treasury department's putting it into circulation — roughly speaking, the action that makes it a constitutive social construct. For example, the paper must be of a certain quality, have a certain shape, and have a certain design printed on it with a certain type of ink. Five-dollar bills are causal social constructs, because we manufacture them to have these features.

Instances of causal social construction where the constructed item is not, in addition, constitutively socially constructed are rare. These are rare, because we causally construct items for socially recognized goals and purposes. And this fact about them makes them constitutive social constructs as well as causal social constructs.

Most instances of constitutive social construction are, as a matter of fact, accompanied by an instance of causal social construction. For example, in composing a sonata a composer will usually write a score, when declaring war a country will usually produce a written proclamation of war, and in creating a new legal border between two pieces of property the owners of the properties will usually construct a barrier between them. Some constitutive social construction *requires* an accompanying instance of casual social construction, e.g., you can't create many games without their accompanying props, and you don't have an eviction *notice* — as opposed to an eviction *order* — without the piece of paper on which the eviction order is written. But many acts of constitutive social construction do not require the acts of causal social construction that usually accompany them. So, for example, in legally dividing a single property into two smaller properties there is no need to place a barrier between the two properties, and in declaring war there is no need to write a proclamation.

Let us call constitutive social constructs that do not require an accompanying instance of causal social construction **pure constitutive social constructs**, and those that do **impure constitutive social constructs**. An important feature of pure constitutive social constructs is that the following is true of them: If X is a pure constitutive social construct or Xs are a type of pure constitutive social construct, X exists or Xs exist wholly in virtue of the presence of certain acts, decisions, or practices of social significance. Legal statutes are pure constitutive social constructs: roughly speaking,[30] a collection of statements has the property of being a legal statute wholly in virtue of its having appropriately proceeded through the process of approval and

[30]There are other considerations involved. For example, a statute must not be declared unconstitutional and it must not be overridden by later legislative activities. None of these further considerations undermine the claim that legal statutes are pure constitutive social constructs.

having been passed by a legitimate legislative authority. Political borders are also pure constitutive social constructs. A certain line's marking a political border is wholly a matter of certain decisions made by relevant political groups. There is no need for a real geographical change to take place upon the decision to draw a political border in a particular way.

I hope that the distinction between causal and constitutive social construction is now clear and that it is also clear what it is for something to be a pure constitutive social construct. I want now to use the above discussion of social construction to provide a detailed account of the notions of metaphysical dependence on, and independence from, a social practice. A prerequisite for such an account is an understanding of what a social practice is. Exploring this would take us too far afield. For the purposes of this dissertation, let us take a **social practice** to be any collection of social activities governed by normative constraints. Further, let us call a (social) practice **representational** if its purpose is to represent a domain or realm — usually this domain or realm will be external to the practice in question. In addition, let us call a (social) practice **discursive** if there are features of it that are identifiable as declarative sentences, i.e., are used with the assertoric force associated with truth and have all of the inferential trappings of assertoric content, e.g., these sentences get embedded in negations and conditions. The reader is referred to Chapters 1 and 2 of (Wright, 1992) for more details concerning this notion of a discursive practice.

1.2 Metaphysical Dependence and Independence

The question that we must address is: What would it be, at least roughly speaking, for a domain to be either metaphysically dependent on a social practice, or metaphysically independent of a, or all, social practice(s)? As a first, and ultimately unacceptable, approximation, let us begin with the following: An item X is metaphysically independent (in a weak sense) of a social practice Y if and only if X would exist even if there were no practice Y. Corresponding to this definition of weak metaphysical independence is the following definition of strong metaphysical dependence: An item X is metaphysically dependent (in a strong sense) on a social practice Y if and only if X would not have existed if Y had not existed. So, for someone to maintain that the domain of natural numbers is metaphysically dependent on arithmetical practices in a strong sense is for her to maintain that there being arithmetical practices is a requirement on there being a domain of natural numbers. Please note that the subjunctive conditional relevant to strong metaphysical dependence is not trivial. Specifically, its consequent is stronger than the thesis that there would be no one talking about, thinking about, or, to put the point more generally, engaging in representational practices with respect to the domain in question.

Potentially, a second half could be added to the above characterization of strong metaphysical dependence and it be claimed that the presence of social practice Y is not only necessary for the existence of domain X, but also sufficient. The cases of strong metaphysical dependence that we are interested in are cases in which a social practice Y is not only necessary for the existence of domain X, but also sufficient for its existence.

Unfortunately, the above definitions of the notions of strong metaphysical dependence and weak metaphysical independence will not do. Spherical objects with the physical characteristics of regulation baseballs and, to take a more famous example, lumps of clay in the shape of human beings, would not exist without the causal practices of manufacturing baseballs and sculpting statues of human beings. Yet both types of objects, while dependent on these causal practices, are not *metaphysically* dependent on these practices. That is, these causal practices do not contribute to the metaphysical essence or being, for want of a better word, of these items. This metaphysical essence or being, ultimately fundamental particles, exists independently of the practices of manufacturing baseballs and sculpting statues. The reason why these spherical objects and lumps of clay are dependent on these causal practices is that they need to be causally constructed by these practices out of raw materials that exist independently of these social practices.

The above definitions provide an account of the strong dependence and weak independence aspects of strong metaphysical dependence and weak metaphysical independence. What we still need is an account of the modifier 'metaphysical'. Let us begin with the following observation: the above discussion of spherical objects and lumps of clay strongly suggests that an item's mere *causal* dependence on a social practice should not count against the weak *metaphysical* independence of that item from that practice. If the only influence that a practice has on an item is to causally manipulate its features, then it does not change the metaphysical essence or being of that item — at least, not if the raw materials of the item in question are spatio-temporal entities. By way of contrast, let us consider regulation baseballs and statues of human beings. The constitutive significance of the practices of major league baseball and artistic evaluation in bringing regulation baseballs and statues of human beings into existence does contribute to their metaphysical essence or being. Spherical objects of an appropriate kind and lumps of clay of an appropriate kind become objects with different modal properties in virtue of their incorporation into, respectively, the practice of major league baseball and the practices of our artistic community. Examples of this type strongly suggest that a constitutive dependence of an item on a social practice should count against the weak metaphysical independence of that item from that practice. Items of this type have a different metaphysical nature in virtue of their relations to the practices in question.

In light of these considerations, the following are *better* accounts of weak metaphysical independence from, and strong metaphysical dependence on, a social practice: An item **X is weakly metaphysically independent of a social practice Y** if and only if either X would exist if there were no practice Y, or X is the product, intended or otherwise, of the participants of Y causally influencing items, by participating in Y, that would exist even if there were no practice Y; and, an item **X is strongly metaphysically dependent on a social practice Y** if and only if X is constitutively socially constructed by practice Y.

I want to emphasize that I take these merely to be better definitions of weak metaphysical independence and strong metaphysical dependence than my first attempts. I make no claim that they are fully adequate definitions of these notions. I have made no attempt to investigate whether some variety of construction other than causal and constitutive should be taken into consideration in offering fully adequate definitions of these notions. For the purposes of this dissertation, however, there is no need for me to undertake such an investigation. These definitions will allow me to provide adequate characterizations of Platonism and PDR.

So far my discussion of metaphysical dependence and independence has been exclusively concerned with weak metaphysical independence and strong metaphysical dependence. Why have I used the modifiers 'weak' and 'strong'? Not surprisingly, because there is a stronger sense in which a domain might be metaphysically independent of a social practice and a weaker sense in which a domain might be metaphysically dependent on a social practice. Recall from Section 0.4 that the ontological structure of a domain is the way in which it is made up of objects, properties, and relations. Further, recall MacBride's suggestion that "the neo-[fregean] assumes that states of affairs lack an independent [ontological] structure, that states of affairs are somehow *plastic* and have [ontological] structure imposed upon them by language" (MacBride, 2003, p. 127).

In this suggestion, MacBride alludes to a second sense in which a domain might be metaphysically independent of, or dependent on, a representational practice. Let us say that an item **X is strongly metaphysically independent of a representational practice Y** if and only if not only would X exist if there were no practice Y, but X's ontological structure would be as it in fact is if there were either no practice Y or the practice Y were other than how it in fact is. Corresponding to this definition of strong metaphysical independence is a definition of weak metaphysical dependence: An item **X is weakly metaphysically dependent on a representational practice Y** if and only if differences in the logico-inferential features of Y are necessary for differences in the ontological structure of X. That is, the ontological structure of the domain X is supervenient on the logico-inferential features of Y. This characterization of weak metaphysical dependence certainly captures a sense in which the ontological structure of a domain could be dependent on the logico-inferential features of a representational practice, yet it seems clear that MacBride has a slightly stronger dependence relation in mind when he suggests that "states of affairs . . . have [ontological] structure imposed upon them by language." He is suggesting that certain differences in the logico-inferential features of a representational practice are sufficient to bring about changes in the ontological structure of the domain it represents.[31] Obviously, these claims raise tricky issues concerning the individuation of representational practices and domains. I am going to leave most of these issues unaddressed. For the purposes of this dissertation, it will suffice that we can, and do, make assessments of sameness and difference with respect to both representational practices and domains.

In order to illustrate the sense of dependence MacBride has in mind, let us consider an example concerning sets. Suppose, for the purposes of illustration, that both $ZFC + CH$ and $ZFC + \neg CH$ are coherent.[32] The set-theoretic hierarchy would be metaphysically dependent on set-theoretic practices in the particular weak sense MacBride has in mind if the following were true. If set theorists were to come to a consensus that the continuum hypothesis is true of the set-theoretic hierarchy, that would make it the case that the continuum hypothesis is true of the set-theoretic hierarchy. On the other hand, if set theorists were to come to a consensus that the continuum hypothesis is false of the set-theoretic hierarchy, that would make it the case that the continuum hypothesis is false of the set-theoretic hierarchy. Note

[31] Addressing what the relevant logico-inferential features of a representational practice are will have to wait until Chapter 2.

[32] The relevant notion of coherence is something like the technical one developed by Stewart Shapiro in (Shapiro, 1997). Deductive consistency and model-theoretic satisfiability model this notion, but neither is an adequate definition of it. I shall discuss this notion in more detail in Section 4.3.

that the suggestion here is that the *very same* part of the mathematical realm, i.e., that part that is known as the set-theoretic hierarchy, would, as a result of particular choices of or decisions by set theorists, either change in a way that made the continuum hypothesis true of it or change in a way that made the continuum hypothesis false of it. The suggestion is not that if set theorists were to make one choice or decision concerning the continuum hypothesis that they would be talking about one part of the mathematical realm, while if set theorists were to make a different choice or decision they would be talking about a different part of the mathematical realm.

The reader might legitimately ask: How is it possible for the *very same* (part of a) realm to make a claim true if a representational practice is one way and that very same claim false if that very same representational practice is another way? One of the purposes of this chapter is to provide some sort of an answer to this question. As a first step, I suggest that the reader think about Play-Doh. A child leaves a lump of Play-Doh in the shape of a car. It is true of that Play-Doh that it has the shape of a car. Yet, had the child chosen to shape that Play-Doh differently, it could have been false of that very same lump of Play-Doh that it has the shape of a car. What is it about the Play-Doh that provides it with these features? Well, the very same stuff can be manipulated to have different properties. Indeed, the activities of a child playing with some Play-Doh are the kind of activities that can bring about these kinds of changes in its properties.

We are interested in domains or (parts of) realms not Play-Doh. Which claims are true or false of a domain or (part of a) realm depends on which objects, properties, and relations make up that domain or (part of that) realm and how they are arranged, i.e., which objects have which properties and stand in which relations to one another.[33] That is, which claims are true or false of a domain or (part of a) realm depends on the ontological structure of that domain or (that part of) that realm. Thinking about Play-Doh and weak metaphysical dependence suggests that a proponent of the weak metaphysical dependence of a domain X on a representational practice Y is proposing that the ontological structure of X is manipulated or determined by the representational practice Y in something like the way that the properties of a lump of Play-Doh are manipulated or determined by a child playing with it.

Without doubt, a child playing with Play-Doh causally manipulates the ontological structure of that Play-Doh, and most certainly it could be maintained that our representational practices causally manipulate the ontological structure of the domains represented by those practices. Yet the discussion in Section 1.1 offers us an alternative to this suggestion. Instead of taking the ontological structure of domain X to be causally socially constructed by representational practice Y, we could instead take the ontological structure of domain X to be constitutively socially constructed by representational practice Y. The idea would be that a domain would have a particular ontological structure wholly in virtue of its being the target of a representational practice that represented it as having that ontological structure. Just as on one understanding of Euthyphro's proposal there is no need for the Gods to in any way manipulate an act in order for their loving of it to make it pious, on this proposal there is no need for our representational practices to manipulate a domain or (part of a) realm in order for them to make it the case that it has a particular ontological structure.

[33]Here I am taking truth in a model as a model for truth.

The astute reader will have noticed that, in talking about weak metaphysical dependence on some occasions I talked about domains and on other occasions I talked about realms or parts of realms. The reason for this is that I take a domain to have a determinate ontological structure, while I take it to be an open question as to whether a realm has a determinate ontological structure. It should be noted, however, that even given this terminological concern it is legitimate to talk about a domain X as being weakly metaphysically dependent on a representational practice Y. For, in such a situation, the practice will determine the ontological structure of the domain in question and so that domain will have an ontological structure.

1.3 The Metaphysics of Platonism and Neo-Fregeanism

The primary purpose of this section is to offer and defend an interpretation of neo-fregean metaphysics. Before turning to neo-fregean metaphysics, however, let me first provide a general characterization of Platonism. For my purposes, **Platonism** is the conjunction of three theses about mathematical domains:

1. they exist;
2. they are at least weakly metaphysically independent of all social practices; and,
3. they are (and contain only) abstract entities.

For convenience, let us call mathematical domains — and, derivatively, other types of mathematical entities — that exist, are at least weakly metaphysically independent of social practices, and are abstract, **Platonistically construed** mathematical domains. Please note that Thesis 2 in the above characterization of Platonism is compatible with all mathematical domains being strongly metaphysically independent of social practices. Indeed, I suspect that most Platonists would be far more likely to accept that mathematical domains are strongly metaphysically independent of social practices than that they are weakly metaphysically independent of social practices. Further, note that rejecting the counterfactual that if there were no mathematical practices, then there would be no mathematical domains is the heart of Platonism. That is, Platonism maintains that even if there were no mathematical practices, indeed no social practices at all, there would still be mathematical domains.

The reader should also note that while a correct definition of weak metaphysical independence required taking into consideration items that are causally dependent on social practices without being metaphysically dependent on social practices, the extra clause in the better definition of weak metaphysical independence offered in the last section is superfluous in its application to Platonism, because Platonistically construed mathematical domains are acausal in virtue of being abstract.

This, of course, raises the issue of how we should understand the notion of being abstract. This is a tricky issue, but I favor the following account. 'Abstract' is a cluster concept, and items are abstract if they satisfy some or all of the members of the cluster. Items that satisfy all members of the cluster are paradigm cases of abstracta, while those satisfying just some fit the concept less well. It is difficult to specify all members of the cluster, but the following are the most important members: **acausality** — the item neither exerts a causally influence over other items, i.e., it is causally inactive, nor does any other item causally influence it, i.e., it is causally passive; **non-spatio-temporality** — the item does not stand in spatio-temporal relations to other items; **eternality** — the item exists timelessly; and **changelessness**

— none of the item's (intrinsic)[34] properties change. Traditionally, mathematical entities have been taken to be paradigm cases of abstracta, i.e., they have been taken to satisfy all of the members of the cluster constitutive of the abstract.[35]

In addition to the bare details above, I think that it is helpful to note the following about 'abstract': in addition to 'abstract' being a cluster concept, all the notions in the cluster are negative notions. They involve abstract items' failing to have some type of property, or to stand in some type of relation, that is either typical of, or at least common among, the items found in the spatio-temporal world. So, for instance, non-spatio-temporality relates to abstract items' not standing in spatio-temporal relations to other items; acausality relates to abstract items' not standing in causal relations to other items, etc., etc. Thus, in essence, 'abstract' is defined in opposition to 'spatio-temporal'. As a consequence, a Platonist's commitment to mathematical domains being abstract — indeed, according to many Platonists, mathematical domains are paradigm instances of abstracta in that they have none of the properties and relations typical of spatio-temporal reality — is a commitment to mathematical domains not being identical with any aspect of the spatio-temporal world. To put this point another way, Platonism is committed to mathematical domains being ontologically distinct from the spatio-temporal reality that we observe with our senses.

In recent years, a number of **realist** philosophers of mathematics, by which I mean philosophers of mathematics who accept Theses 1 and 2 in my characterization

[34]The intrinsic properties of an item are those that it has independently of its relationships to other items. This modifier is needed, because it is clear that the extrinsic properties of (nearly) all things change. For example, the extrinsic properties of the number 7 would change were I to decide that it is no longer my favorite number.

[35]For recent discussions on the nature of the abstract the reader is referred to (Lewis, 1986) and (Burgess and Rosen, 1997). While none of these authors express their claims about the abstract in the way that I do, there is certainly an overlap between the points that I make here and those to be found in these texts.

of Platonism, have rejected Thesis 3, mainly as a result of the influence of W.V.O. Quine (cf., e.g., (Resnik, 1997)). Thus, according to my characterization of Platonism, these philosophers are not Platonists. In most cases the motivation for this move is skepticism about the legitimacy of the abstract-concrete distinction combined with an acceptance of some form of holism, though the philosophers in question are not always clear about exactly what form of holism is involved. Nearly all accept a form of confirmational holism, yet what really seems to be required in order to draw the conclusion that they draw is an acceptance of something like semantic holism. I do not wish to investigate either variety of holism here. I hope, elsewhere, to argue that neither form of holism extends to cover mathematics in the way that these philosophers imagine.[36] I also believe that these philosophers have confused the fact that 'abstract' is a cluster concept with the illegitimacy of the abstract-concrete distinction.

We shall be particularly concerned with Platonisms of two different metaphysical varieties. The first of these I shall call traditional Platonism. **Traditional Platonism** maintains that mathematical domains are strongly metaphysically independent of all social practices, i.e., mathematical domains exist and their ontological structure is fixed independently of all social practices. The other variety of Platonism that we shall be concerned with is neo-fregean Platonism. Let us now turn to the investigation of the metaphysics of neo-fregean Platonism.

Recall from Section 0.4 that MacBride characterizes neo-fregean metaphysics using the following metaphor:

[36] I have begun this process in (Cole, 2001).

> The neo-[fregean] assumes that states of affairs lack an independent structure, that states of affairs are somehow *plastic* and have structure imposed upon them by language. (MacBride, 2003, p. 127)

MacBride's talk of the structure of reality being imposed upon it by our mathematical language suggests that neo-fregeanism is a metaphysics that takes at least part of the ontological structure of mathematical domains to be socially constructed by mathematical practices. Indeed, it would seem that some feature, or features, of our mathematical practices is, or are, sufficient for determining the ontological structure of mathematical domains. Specifically, the (potential?) presence of singular terms and n-place predicates within true atomic sentences of the mathematical practice representing the domain in question seem to be sufficient for determining the ontological structure of that domain. Yet this interpretative point leaves open two levels of social construction that could be involved in determining the ontological structure of mathematical domains. Neo-fregeans could maintain that some, but not all, of the ontological structure of mathematical domains is socially constructed by mathematical practices, or they could maintain that all of the ontological structure of mathematical domains is socially constructed by mathematical practices. Charity dictates that we take neo-fregeans to maintain the latter, because the former would require some motivation. And it is not clear that any feature of any mathematical practice does justify us in taking there to be a distinction of the type needed relating to the ontological structure of any mathematical domain.

Let us now turn to the issue of the existence of the mathematical realm. MacBride's metaphor takes there to be something there that he describes as being plastic and as having ontological structure imposed upon it. Thus, I take it that neo-fregeans maintain that at least some of the mathematical realm exists independently of mathematical practices. This point, once again, leaves open two options: neo-fregeans could maintain that some part of, but not all of, the mathematical realm exists independently of social practices, or they could maintain that all of the mathematical realm exists independently of social practices. Once again, charity dictates that we interpret neo-fregeans as maintaining the latter.[37]

We are now faced with the question of whether neo-fregeans take the ontological structure of mathematical domains to be causally socially constructed, constitutively socially constructed, or perhaps both. To my mind, the second option is by far the most plausible interpretation, for neo-fregeans, following the Neo-Fregean logicists, take mathematical domains to be acausal. So if they sought to suggest that the social construction in question involves a causal element, then they would need to provide an account of how causally instantiated mathematical practices could causally or mechanistically modify acausal domains. Such an account would need to discuss a mechanism by which the causal aspect of the construction in question could take place. Yet if the mathematical domains that are being constructed are acausal, then the mechanism doing the construction could not be causal in any strict sense. The appropriate questions to ask, at least from the perspective of an Egalitarian naturalist, are: what kind of mechanism could do the work that neo-fregeans (understood in this way) want this mechanism to do, and what kind of account of this mechanism can proponents of neo-fregeanism provide?

[37]Quine would most probably disagree with the suggestion that all parts of the mathematical realm need to be treated in a uniform way. For example, he would take there to be a distinction between those parts of the set-theoretic universe which find application in science and those which do not. Penelope Maddy has provided convincing arguments against Quine on this point (cf. (Maddy, 1997)).

In essence, I am now raising a dual of the epistemological challenge that Paul Benacerraf raised in (Benacerraf, 1973) and Hartry Field fine-tuned in (Field, 1989). In both cases there is an acausal realm that exists independently of mathematical practices and there are mathematical practices that are causally instantiated. To solve the epistemological challenge, Benacerraf and Field maintain, a Platonist must hold that this acausal mathematical realm can have an influence on these mathematical practices qua causal instantiated activities. The heart of the epistemological challenge is to provide an explanation of this given that the acausal (and at least weakly metaphysically independent)[38] nature of the realm excludes the possibility of a casual explanation. Neo-fregeanism, as it would be if it endorsed the thesis that part of the mechanism of construction of the ontological structure of mathematical domains is causal, would maintain that causally instantiated mathematical practices have an influence over an acausal realm. For this to be true, proponents of neo-fregeanism would need to be able to provide an explanation of how this could be, given that the practices are causally instantiated and the realm is acausal and weakly metaphysically independent of mathematical practices. In essence, neo-fregeans, under the construal under consideration, and Platonists, as characterized by proponents of the epistemological argument against Platonism, would need the same thing in order to make their respective accounts plausible, i.e., a satisfying account of a mechanism that could systematically relate — in one way or another — an acausal realm with a causal realm. To my mind, no account of this type that could satisfy an Egalitarian naturalist has been provided, or indeed could be provided. My discussion of the epistemological argument against Platonism in Chapter 3 will establish this fact. So,

[38]The reason I include this qualifier will become clear in Chapter 3, where I discuss the epistemological argument against Platonism.

I maintain, neo-fregeans take the social construction of the ontological structure of mathematical domains to be of the constitutive variety.

For clarity, let me be a little more specific about how mathematical practices constitutively construct ontological structure on the neo-fregean conception of the metaphysics of mathematical domains. A neo-fregean, following the Neo-Fregean logicists, begins with the observation that mathematical practices embody standards for true assertion. In addition, she observes that mathematical practices have all of the logico-inferential features characteristic of assertoric and ontic content.[39] This latter point entails that mathematical practices sustain inferences relevant to identifying certain terms within them as singular terms, and other terms as n-place predicates. Now consider what model theory dictates the relationship to be between a sentence of a discourse and the ontological structure of the domain associated with that discourse in order for that sentence to be true. A neo-fregean takes the standards of true assertion sustained within mathematical practices and the relationship between true assertions and ontological structure provided by model theory and uses them to determine what the ontological structure of mathematical domains must be in order for the standards for true assertion embodied in mathematical practices to turn out to be correct. So, for example, in order for an atomic sentence[40] containing a singular term to be true, the domain must contain an object corresponding to that singular term. A neo-fregean takes the presence of such a singular term in a mathematical

[39]Wright's discussion of assertoric content and minimal truth can be found in Chapters 1 and 2 of (Wright 1992). Wright emphasizes embedding in negations and conditionals; an ideal account should also mention some criteria concerning quantification. By ontic content, I mean simply that these practices inferentially determine the structure of the range of their first- and second-order quantifiers.

[40]There is a concern about what an atomic sentence of a natural language is, which I shall not address here.

discourse to be constitutive of the mathematical domain in question containing the object to which that singular term refers. Of course, this cannot, strictly speaking, be correct, for there are many mathematical domains whose cardinality exceeds $\aleph_0$. Yet it could be close to correct. The discussions in Chapter 2 should indicate how neo-fregeanism can be changed to overcome this concern.

Above I have provided brief[41] arguments for the thesis that neo-fregeanism is a metaphysical account of mathematical domains that takes the mathematical realm — or at least those parts covered by neo-fregeanism — to exist completely independently of all social practices — this is what makes it a form of Platonism — yet takes the ontological structure of these domains to be constitutively constructed by the practices of mathematicians — this is the content of various metaphors about projection and reflection associated with neo-fregeanism.

1.4 The Intelligibility of Neo-Fregean Metaphysics

There is a long tradition of philosophers theorizing about how the ontological structure of domains is imposed upon them by things external to them. Consider, for example, Plato's conception of the relationship between the forms and the world of becoming, or Kant's conception of the relationship between features of beings with faculties like ours and spatio-temporal reality. There is also a long tradition of worrying about the intelligibility of suggestions of this type. In recent times, W.V.O. Quine and Hilary Putnam have been among the participants in this dispute. In

[41] I acknowledge that these arguments have been briefer than would be ideal. Yet I defend this brevity by noting the following: The important issue for my overall project is that practice-dependent metaphysics is a superior account of the metaphysics of mathematical domains than any Platonistic account. The argument that I provide in Chapters 3 and 4 will demonstrate its superiority to all combinations of social construction compatible with the basic theses of Platonism.

an unpublished manuscript, *Identity* (Quine, 1972), Quine talks about reality as 'a seething, shimmering mass or mess devoid of intrinsic individuation.' His underlying suggestion is that the way in which the world is ontologically structured — which is what I take him to mean by individuated — is extrinsic. Given his commitment to the dictum that 'to be is to be within the range of a bound variable,' the mechanism of extrinsic individuation he has in mind is the discursive/representational practices that we use to "represent" the world.

In part as a response to Quine, Putnam introduced, and rejected, what is frequently referred to as the "cookie-cutter model" of reality (cf. (Putnam, 1981)). The "cookie-cutter model" can be explicated using the terminology that I just introduced in the following way: the world is a realm whose existence is quite independent of the activities of rational beings, yet whose ontological structure is constitutively socially constructed by our discursive/representational practices. That is, the world is metaphysically dependent on our discursive/representational practices in a weak sense. This is precisely the neo-fregean conception of how mathematical practices relate to mathematical domains. Putnam's main concern about the "cookie-cutter model" was that it supposed that there is something there that can be considered in isolation from its ontological structure. He doubted that a realm could be considered in isolation from its ontological structure, because our representing it would require us to represent it as having ontological structure.

A whole dissertation could be written on the legitimacy and intelligibility, or lack thereof, of Quine's metaphor concerning the extrinsic individuation of reality. I shall limit myself to a few brief comments. First, it will turn out that practice-dependent metaphysics does not require that this metaphor be intelligible, while, if the interpretative proposal of the last section is correct, neo-fregean metaphysics does, for MacBride's metaphysical proposal is (more-or-less) the same as Quine's metaphysical proposal restricted to mathematical reality. There are, however, varieties of Platonism that do not require that either Quine's metaphor or MacBride's metaphor be intelligible. Thus, characterizations of both Platonistic metaphysics and practice-dependent metaphysics can be, and in the first case have been, provided which do not invoke either metaphor.

Second, while I am sympathetic to something like the Quinean conception of the relationship between discursive practices and reality when restricted to spatio-temporal reality, I am inclined to believe that the features of theorizing about the spatio-temporal world that make this conception of that relationship intelligible are not present in mathematical theorizing. Consequently, I am highly dubious about the appropriateness of applying Quine's metaphor to the mathematical realm in the way in which MacBride does in his interpretation of the Neo-Fregean logicists. Perhaps one might want to claim that the Quinean metaphor is unintelligible when applied to the mathematical realm. I'm not sure whether this is true or false. If you believe that Quine's metaphor is unintelligible when applied to the mathematical realm, I have no interest in arguing with you about that. For my part I am instead going to accept a weaker thesis, one which is in line with my defense of PDR, that it is inappropriate to apply Quine's metaphor to the mathematical realm.

Yet, despite my belief in the inappropriateness of applying Quine's metaphor to the mathematical realm, I am going to explore this metaphor as a potential option for the correct metaphysical account of mathematical domains. Further, I am going to grant that this metaphor is intelligible for the purposes of this exploration. As I have mentioned on a number of occasions, if this metaphor is, ultimately, unintelligible when applied to the mathematical realm, then little harm will be done, because neither my characterization of Platonism nor my characterization of PDR rely on it in an essential way.

1.5 Summary

In summary, then, note that the strong and weak senses of metaphysical dependence distinguished above relate to the metaphysical dependence of two distinct features of domains. A domain is metaphysically dependent on a practice in a strong sense if it is the existence of the domain that is metaphysically dependent on the practice. Yet if a domain's existence is metaphysically dependent on a social practice, then so is its ontological structure. A domain is metaphysically dependent on a practice in a weak sense if it is merely the ontological structure of that domain that is metaphysically dependent on the practice.

Along with these two strengths of metaphysical dependence, there are two strengths of metaphysical independence. In the weak sense, what it is for a domain X to be metaphysically independent of a practice Y is the following: the presence of practice Y is not a necessary condition for the existence of (the part of) the realm that is domain X. So, if there were no practice Y, (that part of) that realm would still exist. In this weak sense, what is at stake is the metaphysical independence of (the part of) the realm that is domain X. In the strong sense, what it is for a domain X to be metaphysically independent of a practice Y is the following: the presence of practice Y is not a necessary condition for the existence of (the part of) the realm that is domain X *and* differences in logico-inferential features of practice Y are not necessary for differences in the ontological structure of (the part of) the realm that is domain X. So, if the world were different with respect to practice Y, either with regard to that practice having significantly different logico-inferential features or with regard to there being no practice Y, it would both be the case that (the part of) the realm that is domain X would still exist and the ontological structure of that realm would be the same as it actually is. In this strong sense, the metaphysical independence of both (the part of) the realm that is domain X and the ontological structure of (the part of) the realm that is domain X are at stake.

Traditional forms of Platonism maintain that mathematical domains are metaphysically independent of all social practices in the strong sense, while, I have argued, neo-fregean Platonism maintains that mathematical domains are only metaphysically independent of social practices in the weak sense. Neo-fregean Platonism takes the ontological structure of mathematical domains to be constitutively socially constructed by mathematical practices.

Now that we have a reasonabe understanding of the metaphysics of Platonism, our next topic must be the metaphysics of PDR. It is to this topic that we shall turn at the beginning of Chapter 2.

CHAPTER 2

PRACTICE-DEPENDENT REALISM

2.1 The Metaphysics of Practice-Dependent Realism

Now that we have a reasonable understanding of the metaphysics of Platonism, let us turn to the metaphysics of PDR. It differs from neo-fregean metaphysics in the following respect: not only does it take the ontological structure of mathematical domains to be constitutively constructed by mathematical practices, it takes mathematical domains themselves — with their ontological structures intact — to be constitutively constructed by mathematical practices. In fact, the **central metaphysical thesis of PDR** is that *mathematical domains are pure constitutive social constructs constructed by mathematical practices.*[42] So, according to a PDRists, mathematical domains exist wholly in virtue of there being mathematical practices of a certain kind. Consequently, according to a PDRist, mathematical domains are strongly metaphysically dependent on mathematical practices.

The central metaphysical thesis of PDR distinguishes PDR from all forms of Platonism, for Platonists maintain that mathematical domains are (at least weakly) metaphysically independent of mathematical practices. Platonists also hold that

[42]Please note that this thesis does not rely on the intelligibility of either Quine's metaphor or MacBride's metaphor. So, as promised, both Platonism and PDR can be explicated without reliance on either metaphor.

mathematical domains are abstract entities. Before discussing in more detail what a PDRist takes the relationship between mathematical practices and mathematical domains to be, I think that it is worth exploring whether PDRists should hold that mathematical domains are abstract entities.

You might recall (cf. Section 0.1) that Hersh denies that social-historical entities are abstract entities, yet his argument for this thesis is peculiar. First, all he tells us about abstract entities is that they are neither mental nor physical. Second, he maintains that social-historical entities are neither mental nor physical. Why, then, does Hersh deny that social-historical entities are abstract? The reason, I suspect, is that Hersh's concept of an abstract entity is what in Section 1.3 I called the concept of a paradigm case of an abstract entity. For Hersh, in order for something to be an abstract entity, it must satisfy all of the members of the cluster constitutive of 'abstract'. So, among other things, it must be acausal, non-spatio-temporal, eternal, and changeless.

Hersh's confusion of 'abstract' and 'paradigm case of abstract' is quite understandable. After all, most philosophers of mathematics have meant to claim that mathematical entities are paradigm cases of abstract entities when they have in fact claimed that mathematical entities are abstract entities. Further, on my interpretation of Hersh, his claim that social-historical entities are not abstract is reasonable. Many social-historical entities fail to satisfy some of the members of the cluster constitutive of 'abstract'. For example, sonatas have a causal impact on people — think about how they make you feel, are created at a certain time and so are not eternal, and, perhaps,[43] go through revisions of their intrinsic properties — composers

[43]There is a very tricky issue here about whether such changes result in a new sonata or a modified version of the old sonata. This issue need not concern us further.

frequently change sonatas during their composition. Hersh also claims that mathematical entities are not abstract. This, too, is reasonable in light of Hersh's confusion of 'abstract' and 'paradigm case of abstract', and his belief that mathematical entities are social-historical entities, for it is reasonable that he should take mathematical entities to be like other social-historical entities in this regard.

I'm not sure that Hersh is wrong to maintain that mathematical domains are not paradigm cases of abstract entities. I certainly think that he can muster an argument for why we have *taken* mathematical domains to be paradigm cases of abstract entities despite the fact that they are not. Yet, equally, I don't see any wholly convincing reason why a **social constructivist** — an individual who believes that mathematical domains are social-historical entities — has to deny that mathematical domains are in fact paradigm cases of abstract entities. Specifically, I don't see why a social constructivist has to deny that mathematical domains are acausal, non-spatio-temporal, eternal (or at least timeless), and changeless.

At least in general, [44] pure constitutive social constructs have the features — have the properties and stand in the relations — that those responsible for their constitution wish them to have, for those responsible for their constitution constitute them to have the features they wish them to have. Further, pure constitutive social constructs are frequently constituted to have quite different features to the acts, decisions, or practices that constitute them. One kind of feature of constitutive social constructs that can be quite different to the acts, decisions, or practices that

[44] I am aware of only one logical restriction on our ability to construct pure constitutive social constructs to have the features we wish them to have: on pain of contradiction, we cannot construct them so that they are not constructed. Further, intuition tells against our ability to construct pure constitutive social constructs so that they are necessary existents, but I am not sure whether it is actually impossible to constitute a necessary existent.

constitute them is their spatio-temporal features. For example, legal and political borders usually have different spatio-temporal features to the legislative and political activities that constitute them. These legislative and political activities take place at a given time and place, while the borders that they constitute usually occupy a different location and might not come into existence until some time after the cessation of the activities responsible for their existence.

Another kind of feature of constitutive social constructs that can be quite different to the acts, decisions, or practices that constitute them is their causal features. For example, consider Beethoven's Moonlight Sonata. The acts responsible for constitutively constructing that sonata are causally responsible for the consumption of a variety of foods, yet the sonata itself has no such causal property. Similar claims are true of many constitutive social constructs.

Given these types of examples, I see no reason why we could not constitute a pure constitutive social construct that has neither causal nor spatio-temporal features. Such a pure constitutive social construct would be acausal and non-spatio-temporal. Further, being created at a particular time, ceasing to exist at a particular time, and changing an intrinsic property at a given time are all spatio-temporal features of entities. Therefore, since the type of pure constitutive social construct under consideration does not have spatio-temporal features, it does not have any of these features. In a sense, its existence is timeless and changeless.

If the above is correct, a social constructivist could maintain that mathematical domains are (at least close to) paradigm cases of abstract entities, for she could maintain that we have constituted them to be the type of pure constitutive social construct considered in the last paragraph. Indeed, there are a variety of reasons why a social constructivist might want to maintain that mathematical domains are constituted to be acausal, non-spatio-temporal, eternal (or at least timeless), and changeless. One important piece of evidence that would support this contention is the abundance of tenseless forms of representation in mathematical practices. Another advantage of this contention is that it allows for the vindication of the intuition that $2 + 2 = 4$ has always been true, as have all well-established mathematical truths.

In addition, maintaining that mathematical domains are (at least close to) paradigm cases of abstract entities would allow a social constructivist to sidestep some tricky issues. For example, it is well known that Newton's and Leibniz's early developments of calculus were riddled with inconsistencies, yet practiced users of Newton's and Leibniz's tools were able to avoid these inconsistencies. Does the presence of this stable mathematical practice *force* a social constructivist to acknowledge the existence of a domain of infinitesimals with inconsistent properties constituted by this practice? On the present proposal, the answer is no. She *could*[45] take Newton and Leibniz to have offered an inconsistent characterization of the real numbers as constituted by our contemporary practice of real analysis — presuming, of course, that our practice of real analysis does constitute the domain of real numbers. Further, the contention that mathematical domains are (at least close to) paradigm cases of abstract entities would allow a social constructivist to account for mathematical practices progressing towards optimal characterizations of mathematical domains, and provide a sense in which she could account for early participants of such a practice — like Newton and Leibniz — getting things wrong about the domain the practice they are engaged in

[45]She is not, however, forced to offer this answer. A careful investigation of the early practices surrounding the calculus might warrant her accepting the constitution of a domain having inconsistent properties.

concerns. These are both claims that find widespread acceptance in our everyday thought about mathematics.[46]

A further piece of evidence in favor of the contention that mathematical domains are (at least close to) paradigm cases of abstract entities is the role that mathematical entities play in science and everyday life. The primary function of mathematical domains in science and everyday life is representational. This is perhaps most easily seen with regard to the natural numbers, which are used to aid us in representing cardinality properties of collections — both collections of spatio-temporal entities and collections that contain mathematical entities. Numerals function in two distinct grammatical roles in arithmetical discourses. In some uses they function as nouns — indicating that they refer to objects — while in other uses they function as adjectives — indicating that they denote properties. Consider the biconditional the number of Fs is equal to 3 if and only if there are 3 Fs.[47] This biconditional incorporates both uses of the numeral 3. On the left hand side of the biconditional, 3 is used as a noun and refers to the natural number 3. On the right hand side of the biconditional, 3 is used as an adjective and denotes the property of having cardinality 3 — this is a property had by certain collections of entities. Biconditionals of the above type relate natural numbers — understood as objects — to cardinality properties of collections. The truth of biconditionals of this type ensures that we can represent facts about the cardinality of collections — there are nine planets in our solar system — using natural numbers — the number of planets in our solar system is nine. This is a

[46]The arguments of this paragraph rely on the assumption that a mathematical discursive practice is able to pick out a mathematical domain as the one it is about, even if it does not characterize that domain perfectly. This is a controversial assumption. Yet my overall argument on behalf of a social constructivist can be provided without the support of the arguments made in this paragraph.

[47]This is a specific instance of Neil Tennant's Schema N (cf. (Tennant, 1987)).

perfect example of what is meant by the claim that mathematical domains — in this case, the domain of natural numbers — have representational benefits.

The representational role that mathematical domains play in scientific and everyday practices is quite different from the causal role that spatio-temporal entities play in those practices. A social constructivist proponent of the thesis that mathematical domains are (at least close to) paradigm cases of abstract entities could argue that it is for this reason that we constitute mathematical domains as acausal and non-spatio-temporal.

In addition, many of the features of the world that we use mathematical domains to help represent are the way they are eternally and unchangingly. For example, $2 + 2 = 4$ aids us in representing the following relationship between cardinality properties of collections: if the Fs have cardinality 2 and the Gs have cardinality 2 and there are no items that are both Fs and Gs, then the Fs or Gs have cardinality 4. Clearly, this fact about cardinality properties is eternal and unchanging. A social constructivist proponent of the thesis that mathematical domains are (at least close to) paradigm cases of abstract entities could argue that the eternal and unchanging nature of (some of) the facts that we use mathematical domains to help represent provides further motivation for constituting mathematical domains as eternal and unchanging — indeed, as standing in no spatio-temporal relations whatsoever.

There is one obvious concern about the suggestion that mathematical domains are constituted as acausal, non-spatio-temporal, eternal, and changeless: the practices responsible for the existence of mathematical domains are not acausal, non-spatio-temporal, eternal, or changeless. This is true, but not a genuine problem, for constitutive social constructs can, as noted above, have different causal and spatio-temporal features to the acts, decisions, and practices that constitute them. So, the acausal, non-spatio-temporal, eternal and unchanging nature of mathematical domains is quite compatible with mathematical practices having none of these features.

Further, a social constructivist proponent of the thesis that mathematical domains are paradigm cases of abstract entities could acknowledge that it is natural to think of constitutive social constructs as coming into existence when the practice that constitutes them comes into existence. Yet, she could continue, there is no need for us to do so. Nothing about the constitution of an item by a practice requires that the construct come into existence when — or after — the practice does.

In addition, she will point out that if we reject the thesis that mathematical domains come into existence at the moment that — or after — the practice that constitutes them comes into existence — granting temporarily that mathematical domains are constitutive social constructs — then we must be careful in characterizing the relationship between mathematical domains and mathematical practices. The introduction of a mathematical practice characterizing a new structure does not, strictly speaking, create or construct — where these are understood to have the temporal connotations — a domain with that structure, but is, rather, responsible for the existence of such a domain, where it is responsible in the sense that if no such practice were to occupy some region of four-dimensional space-time, then no such mathematical domain would exist.

So, the thesis that mathematical domains are constituted by mathematical practices to be (at least close to) paradigm cases of abstract entities has merit. Despite this, I'm not sure that a social constructivist should accept it. My worry about accepting this thesis is the following: it distinguishes the sense in which mathematical social-historical entities are constituted by mathematical practices from the sense in which other social-historical entities are constituted by social practices. While it is not at all peculiar to acknowledge that social-historical entities continue to exist after the practices that constitute them go out of existence — consider, for example, musical compositions — it is peculiar in the extreme to claim that those practices are responsible for the existence of entities prior to their own existence, which is precisely what a proponent of the thesis that mathematical domains are constituted by mathematical practices to be (at least close to) paradigm cases of abstract entities is doing.

Further, once one has acknowledged that — at least potentially — there are future mathematical practices that are responsible for the current existence of mathematical domains — as a proponent of the thesis that mathematical domains are constituted by mathematical practices to be (at least close to) paradigm cases of abstract entities must — why stop there? Why not take the next step and hold that it is not the presence of a mathematical practice occupying some time-slice of the actual world that is constitutive of the existence of a mathematical domain, but rather the presence of a mathematical practice occupying some time-slice of a possible world that is constitutive of the existence of a mathematical domain? This, too, is a position that takes mathematical domains to exist and which is distinct from any form of Platonism. What distinguishes it from Platonism is that while a Platonist takes the Socratic side in a certain Euthyphronic debate, a proponent of the modal social constructivist position under consideration sides with Euthyphro; she takes the possibility of a mathematical practice of a certain kind to be constitutive of the existence of a mathematical domain of a certain kind rather than taking the existence of a mathematical domain of a certain kind to account for the (logical) possibility of a mathematical practice of the relevant kind.

With this modal social constructivist position explicit, my initial worry about a social constructivist promoting the thesis that mathematical domains are (at least close to) paradigm cases of abstract entities can, perhaps, be understood more clearly. If the reader is anything like me, (s)he will be wondering — or will have wondered — whether (s)he really understands the above modal social constructivist position. What exactly would it be for the possibility of a mathematical practice of a certain type to be constitutive of the existence of a mathematical domain of a certain type? I'm pretty sure I understand what it would be for an actual mathematical practice that exists at present to constitute a mathematical domain that is actual and exists at present, because I can understand this constitutive relationship in the same way that I understand the constitutive relationship between, say, political activities and political borders. But once one has abstracted in the two ways required to reach the above modal social constructivist position, I'm not sure that I still have any grip on the constitutive relationship in question. My worry is, in essence, that even if one only takes the first step in this process, one's understanding of the constitution relationship in question will be undermined.

It should be recognized, however, that this worry is in no way fatal. Nothing I have said in the last couple of paragraphs establishes that even the modal social constructivist position I outlined above is false or unintelligible. If anything, it merely establishes that a social constructivist proponent of the thesis that mathematical domains are constituted by mathematical practices to be (at least close to) paradigm cases of abstract entities might have some work to do to make their view fully intelligible and plausible. I am not sure how to do this. So, for safety, I am not going to make it an official part of PDR that mathematical domains are (close to) paradigm cases of abstract entities. What I do take to be an official part of the metaphysics of PDR is the following: mathematical domains are abstract entities. Specifically, they are constituted to stand in no causal relations to other items, they are constituted to stand in few, if any, spatio-temporal relations to other items, and they are constituted to be changeless. These features of mathematical domains are sufficient to make them abstract entities — as are the features of many social-historical entities.

So, officially, **practice-dependent realism (PDR)** is the conjunction of the following three theses about mathematical domains:

1. they exist;

2. they are metaphysically dependent on mathematical practices — in fact, they are pure constitutive social constructs constituted by mathematical practices; and,

3. they are (and contain only) abstract entities.

2.2 The Applicability of Mathematics in Counterfactual Situations

The fact that PDRists hold that there are no mathematical domains in counterfactual situations in which there are no mathematical practices might cause the reader to worry about the applicability of mathematics to such counterfactual situations if PDR is true. In order to allay this worry, I want to point out the following. I mentioned in the last section that, in essence, PDR maintains that early mathematical domains were socially constructed to serve important representational purposes. When we discuss and represent counterfactual situations, we do so with our own representational repertoire. Thus, since the use of mathematical domains for representational purposes is clearly within our representational repertoire, mathematical domains are available to us in representing counterfactual situations. This even applies to many counterfactual situations that contain no mathematical practices, and counterfactual situations in which there are mathematical practices that are different from our own, e.g., the set theorists in these situations reject the axiom of choice.

All that is needed in order for our mathematical concepts to be applicable in the representation of a counterfactual situation is that certain facts about sameness and difference be determinate. So, for instance, in order to use natural numbers in the representation of a counterfactual situation, all that is needed is that certain facts about the cardinality of collections in the counterfactual situation in question be determinate. A recent work by Agustin Rayo (Rayo, 2002) establishes that this point is generalizable to all mathematics used in the representation of the spatio-temporal world. In light of this, it is perfectly coherent for me to use mathematical representational tools in characterizing and reasoning about counterfactual situations in which there are no mathematical practices, even when my discussion concerns how the *actual* world could have been.

In case the argument in the above paragraphs isn't clear, the reader might find it useful to see the point made with a less controversial case.[48] Consider our practice

[48] I thank Crispin Wright for suggesting that I discuss this example.

of representing lengths using the metric scale of meters. There can be little doubt that what length a meter is is a matter of social convention. Our practice of metric representation constitutively constructs the meter system of representation. As it turns out, the conventions surrounding what length a meter is have changed a number of times in the history of this convention's use. Yet, for all such conventions, it is a contingent fact that the item(s) used to fix the length of one meter exist(s). The classic example of such an item is the meter stick located in Paris. Surely, the said meter stick might not have existed. Yet despite the contingency of the items that we use to identify the length of one meter, and the contingency of our using this particular system of representation, there is no problem in our using this constitutively constructed system of representation in representing at least most possible situations. Included among these counterfactual situations are ones in which none of the items used to determine the length of a meter exist and in which there are no — or different — conventions concerning the use of meters. There would not even be a problem in our using meters to represent the actual world as it would have been, had no such items existed and no collection of conventions relating to meters been adopted.

All that is needed in order for us to be able to use the constitutively constructed system of representation known as meters to represent lengths in a counterfactual situation is that there be determinate facts about an object that does exist having the same length as an object that is supposed to exist in the counterfactual situation. The same holds true for our use of constitutively constructed mathematical domains in representing counterfactual situations: all that needs to be in place is certain types of determinate facts about sameness and difference.

2.3 Logic and Ontological Structure

We now have a basic understanding of the metaphysical account of mathematical domains offered by PDRists. A question that might have occurred to the reader is the following: why is the purported construction that takes place within mathematics social in nature rather than individual in nature? After all, aren't the majority of mathematical domains introduced by individual mathematicians rather than by groups of mathematicians? For example, didn't one individual, William Hamilton, introduce the domain of quaternions? And didn't one individual, Georg Cantor, introduce the domain of transfinite numbers?

In general, one individual does introduce a mathematical domain for the first time, and, consequently, is responsible for introducing the mathematical practice that constitutes that domain. Yet this does not have the consequence that mathematical domains fail to be social in nature. Consider for a moment another class of constitutive social constructs, sonatas. At least in general, one individual is responsible for composing any given sonata, yet this does not undermine the social nature of sonatas. Why not? Well, because sonatas are composed using socially recognized musical tools, e.g., sonatas are composed using the twelve-tone scale — a social convention standardized around "middle C" corresponding to the frequency of 440Hz; most sonatas are composed for standard — socially recognized — musical instruments; and sonatas have a particular socially recognized musical structure. The composition of a sonata is shot through with these, and other, conventional musical tools. It is precisely because musicians make use of socially recognized tools in their creations that their creations can be shared by many, and thus are social in nature.

A similar situation arises in mathematics, though the shared tools are logico-inferential rather than musical. Mathematical statements have a sharable — in fact, shared — content in virtue of their standing in important logical relations to one another. These logical relations include not only those relations characteristic of assertoric content, i.e., embedding in negations, conditionals, etc.,[49] but also those relations characteristic of ontic content. As discussed in Section 0.3, Frege recognized over a century ago that certain patterns of inference are characteristic of the category of object, and other patterns of inference characteristic of the category of property or relation.[50] As Ricketts notes (cf. (Ricketts, 1986)), the relevant inferences are inferences to and from, respectively, statements of first-order generality and statements of higher-order generality, i.e., statements that we represent formally using first- and higher-order universal quantifiers as the dominant logical operator. Further, while inferences to and from statements that we represent formally with universal quantifiers dominant characterize the *categories* of object, and property or relation, inferences to and from statements that we represent formally with existential quantifiers dominant characterize the particular objects, properties, and relations present in the domain, which the statements in question are about.

A **structure** is a collection of places where those places are determined by their properties and the relations they stand in to one another. It is a well known fact that categorical axiom sets characterize a single structure. Further, such axiom sets are a formal mechanism for codifying an informal practice that, from its inception, represented and incorporated reasoning about the structure in question. Ultimately,

[49]For further information about this notion of assertoric content, the reader is referred to Chapters 1 and 2 of (Wright, 1992).

[50]Frege talked about concepts rather than properties or relations.

it is the use of logico-inferential tools appropriately formalized using universal and existential quantifiers, i.e., the ones constitutive of ontic content, which accounts for an informal mathematical practice's ability to characterize individual structures. The inferences offered as legitimate in informal proofs concerning a new subject matter make precise the ontological structure of that subject matter. Once precise, this structure can be formally characterized, and the process of discovering an optimal axiom set for that subject matter can begin.

It can therefore be seen that it is an implicit assumption of contemporary axiomatic mathematics that logico-inferential features of informal (and formal) mathematical practices have the ability to characterize the subject matter(s) of those practices. Hence, at least in the mathematical case, the assumption that logico-inferential categories are prior to ontological categories is legitimate. It is an assumption that PDRists share not only with neo-fregeans, but also mathematicians.

Please note the following features of the description of mathematical practices that I have just sketched. First, it accounts for the following well known features of mathematical practices: a) they develop informally, b) proof is an important part of them and their development, and c) they eventually come to include formal axioms characterizing the subject matters they concern. All of these are natural corollaries of the account of mathematics just sketched.

Second, this account of mathematical practices vindicates Hersh's claim that "Once created and communicated, mathematical objects are *there*. They detach from their originator and become part of human culture. We learn of them as external objects, with known and unknown properties. Of the unknown properties, there are some that we are able to discover. Some we can't discover, even though they are our own creations" (Hersh, 1997, p. 16). Mathematical domains detach from their originator in the same way that a piece of music detaches from its composer; both types of detachment are made possible by the use of social tools in the construction/constitution of the respective items. Further, this detachment allows for the epistemic independence of mathematical domains from mathematical practices, as highlighted in Section 0.1. Our imperfect epistemic situation with respect to mathematical domains is accounted for within this framework by mathematical domains being constituted by logico-inferential tools and human beings being in an imperfect epistemic situation with respect to logical consequence. Moreover, the significance of logic in constituting mathematical domains will provide an important tool in providing an account of the objectivity of mathematics. Roughly speaking, a PDRist should argue that mathematics inherits the objectivity of logic, because mathematical domains are constituted by logico-inferential tools.[51]

2.4 Mathematical Domains and Mathematical Practices

While important, the metaphysical theses characteristic of PDR form only a part of an adequate interpretation of mathematical practices. Asserting them — particularly Thesis 2. — is rather like a philosopher of mind asserting that the mental is supervenient on the physical. In fact, it is a lot like this in that one of the very few interesting truths that you can infer from them is that mathematical domains are supervenient on mathematical practices. In the same way that a philosopher of mind's supervenience claim should be greeted with "Very well, but what is the nature of this supervenience relation?", so too the PDRist's theses should be greeted with

[51] Issues concerning the objectivity of logic and, consequently, the inherited objectivity of mathematics are complex. Unfortunately, I cannot hope to treat them adequately in this dissertation.

"Very well, but what more can you tell me about the dependence relation between mathematical domains and mathematical practices?"

It is, of course, impossible for me to provide a full answer to this question. Yet it will be instructive to at least provide initial answers to some of the more pressing questions that one might have about the relationship in question. Among these more pressing questions are the following: (1) Why take mathematical practices to construct domains rather than individual mathematical objects? (2) Do all mathematical practices, or all of the activities of mathematicians, constitutively socially construct mathematical domains, or just some? (3) If a given mathematical practice does, or did, construct a domain, which domain does it, or did it, construct? Let me attempt initial answers to these three questions.

Question (1):

Question (1) is the easiest of these questions to answer. What features are of central *mathematical* importance to, say, the number two? Among them are that it is the successor of one, the predecessor of three, the first prime number, the only even prime number, etc. etc.[52] Take a look at these important mathematical features of the number two. All of them relate the number two to other natural numbers. This is a specific instance of a very general phenomenon, viz., that the individual objects of a mathematical domain are identified (mathematically) by means of their relations to other objects in the domain of which they are a part. In other words, mathematical domains are inherently relational. To use terminology that will be familiar to those in the philosophy of mind, they are defined functionally. It is for this reason that a

[52] I recognize that other features of the number two are important to its application in counting. Most important among these features is its conceptual link to pairs of items. Yet number theorists are far more interested in the relational characteristics mentioned above than these features.

PDRist takes mathematical practices to construct whole mathematical domains, not individual mathematical objects. The inherently relational nature of mathematical domains results in it making no sense to construct just one, or a few, mathematical objects from a larger domain. Without the other objects in the domain of which a particular mathematical object is a member for that object to be related to, it fails to stand in many of the relations that make it the mathematical object that it is.

Question (2):

The answer to Question (2) is "No, not all of the activities of mathematicians are involved in the constitutive social construction of mathematical domains." Not even all mathematicians' mathematical activities are so involved. Getting clear about which activities are involved in the constitutive social construction of mathematical domains is, however, a non-trivial matter. A good place to start is with the observation that while mathematicians make some of their assertions, e.g., $2+2 = 4$, intending to be asserting truths about a single, specific subject matter, others of their assertions are made with no such intention. For example, in stating the axioms of group theory, a mathematician does not intend them to characterize a single, specific mathematical subject matter, as she does when she states the Peano-Dedekind axioms. Rather the axioms of group theory are intended to pick out features that a wide variety of subject matters share. Indeed, group theorists as a whole explicitly reject the idea that individual representations of groups have a subject matter in the same sense that the Peano-Dedekind axioms do. In such cases, it seems perverse to take the activities in question to be responsible for constructing mathematical domains. On the other hand, those activities of mathematicians intimately related to making genuine assertions about some single, specific mathematical domain are involved in the constitutive social construction of mathematical domains. As for mathematical activities that fall into neither of these categories, I take it to be a substantial interpretative question to determine whether any of these mathematical activities are involved in the constitutive social construction of mathematical domains.

The above considerations are not the whole story, however. At least two other classes of constraints are relevant to identifying which of the activities of mathematicians are constitutively constructive. The first class of constraints is internal to mathematical practices, while the second is external. Let us begin with the internal constraints. Two such constraints can be easily motivated by considering mathematical practices focused on the articulation of truths concerning a single, specific subject matter. Two ways in which mathematicians could fail in their intention to make genuine assertions about some single, specific mathematical subject matter are 1) their characterization of the intended subject matter could fail to be coherent, and 2) what they offer as a characterization of a single, specific subject matter could fail to characterize such a subject matter. These two ways of failing generate what I call, respectively, the **coherence constraint** and the **characterization constraint**.

As an illustration of a characterization that fails the coherence constraint, consider Gottlob Frege's basic laws (cf. (Frege, 1893)). These can be seen as an attempt to characterize extensions — though Frege would not have accepted this description of those axioms; he would have claimed that they were a collection of truths about extensions. These basic laws fail to so characterize extensions because Basic Law V is inconsistent.

As an illustration of a practice with problems satisfying the characterization constraint, consider set theory. It is no accident that we talk about THE set theoretic hierarchy. Early developers of set theory clearly intended it to characterize a single, specific domain. Yet, as is well known, almost a century after Ernest Zermelo's early axiomatization of contemporary set theory (cf. (Zermelo, 1908)), nobody has succeeded[53] in providing a collection of set-theoretic axioms that characterizes a unique domain that is such that the practitioners of set theory will accept those axioms as a characterization of THE set-theoretic hierarchy.

Among contemporary classical mathematicians, the standard most frequently accepted as establishing the coherence of a particular mathematical characterization is classical satisfiability, i.e., the availability of a classical model of that characterization in standard set theory.[54] This standard is not, however, applicable to set theory itself (or, for that matter, to set theory's foundational competitors). And it could be that classical satisfiability is an inappropriate standard to apply to some mathematical practices.

There are at least two axes along which appropriate standards of coherence might vary: one semantic, the other logical. As an illustration of the first type of variation, consider intuitionistic mathematics from the early part of the twentieth century. These practices might more appropriately be assessed for coherence by means of a proof-theoretic, rather than a model-theoretic, notion of consistency. As an illustration of the second type of variation, consider the small group of paraconsistent mathematicians working on infinitesimals in New Zealand and Australia. While at

[53]Indeed, the situation is worse than this in that nobody can succeed in this goal (cf. (Tennant, 2000)).

[54]Another method sometimes used is a proof-theoretic reduction to a fragment of arithmetic.

least some of them — Graham Priest, for example — would be happy with using a model-theoretic semantics, they would want the logic governing this semantics to be paraconsistent rather than classical.

PDR, as a metaphysical interpretation of mathematical practices, should not rule out *a priori* the appropriateness of other standards of coherence for certain types of mathematical practices. Yet in answering Question (3) I shall focus on classical mathematical practices. I leave it to those engaged in these other types of mathematical practices to provide an appropriate modification of my answer for those practices.

One safe way of fulfilling the characterization constraint is to provide a categorical axiomatization of the subject matter in question. Yet this cannot be the final word on the characterization constraint, for it is a standard that cannot be applied to characterizations of sets themselves. Further, at least intuitively, set-theorist's construction of "models" of the standard set-theoretic axioms, such as Gödel's constructive "model" (cf. (Gödel, 1940)), give specific enough characterizations of these "models" for the activities surrounding these constructions to count as fulfilling the characterization constraint. Unfortunately, seeking a formulation of the characterization constraint that would allow me to vindicate this intuition is a task that must be left for another occasion. The purpose of this dissertation is, after all, to *introduce* a new metaphysical interpretation of mathematical practices, not to offer the final word on that interpretation. For safety, in answering Question (3), I am going to concentrate on mathematical practices that focus on categorical axiom systems. An answer to this question for these practices should suffice to communicate the spirit of the PDRist's philosophy of mathematics.

Let us next turn to external constraints. In Section 0.2, I made my commitment to Egalitarian naturalism explicit. This form of naturalism plays an important role in motivating and legitimizing PDR. It should be clear that it holds mathematics as worthy of the same kind of methodological respect of which non-mathematical natural sciences are worthy, because of the role that mathematics plays in the natural scientific enterprise. As a consequence, if mathematics were to evolve so that it could no longer play the role that it plays at present in the natural scientific enterprise, it would, from the perspective of an Egalitarian naturalist, be subject to criticism. An Egalitarian naturalist might even go so far as to deny existential import to "mathematical" practices that have evolved in this way. For this reason I believe that there must be some external constraint on mathematical practices that enforces their usability in the natural scientific enterprise if their presence is to have existential import.

Providing an exact formulation of this external constraint will be incredibly difficult, however, for it is clear that pure mathematicians at the frontiers of mathematical research are not, in any direct sense, interested in the applicability of their theories to the spatio-temporal world. Yet this observation should not deter us, for even though pure mathematicians do not have this kind of applicability in mind while developing their theories they do seek applicability in another sense.

My own mathematical research was in the young subject of multifractal geometry. I shall never forget the excitement on my Ph.D. supervisor's face when he proudly announced to me that multifractal geometry had finally 'come of age'. What he meant was that the methods and techniques of multifractal geometry had finally found application in solving an open question in number theory.

Reflection on the history of pure mathematics will quickly reveal that applicability of this type, i.e., applicability to other, generally more central, mathematical topics, has played a central role in its development. Take, for instance, the introduction of ever more abstract algebraic structures. These introductions were, and still are, invariably motivated by some sought after applicability to less abstract algebraic structures. Furthermore, if applicability of either this internal type or some other is not forthcoming within a reasonable amount of time, a field of pure mathematical research will invariably be dropped as an unprofitable dead-end.

It is the importance of internal applicability of this kind that results in mathematics being a unified enterprise. And because the most central parts of mathematics are, as we shall see in more detail in Part III, linked to applicability to the spatio-temporal world, even the most distant aspects of this unified enterprise aid in mathematics being able to play its role in the natural scientific enterprise. Thus, while it might be difficult to formulate, I have no doubt that some kind of external constraint linking mathematical practices to their role in the natural scientific enterprise can be found.

Question (3):

With the restrictions mentioned above in place, the answer to Question (3) is well-known. Recall that a structure is a collection of places where those places are determined by their properties and the relations they stand in to one another. All categorical axiom systems characterize a structure in this sense (see, e.g., (Shapiro, 1997)). The domain constitutively socially constructed by a mathematical practice surrounding a categorical axiom set has the ontological structure that that axiom set characterizes. The places in this structure are the objects of the mathematical domain constitutively socially constructed by that practice, while the properties of and relations between these places are the properties and relations of the mathematical domain constitutively socially constructed by that practice.

More generally, a PDRist will maintain that a mathematical practice fulfilling all of the constraints mentioned above will constitutively socially construct the domain(s) that it coherently characterizes. This generalization is twofold. First, it allows for the possibility of a mathematical practice characterizing more than one domain. Second, it provides for the possibility of mathematical practices constituting mathematical domains before their having developed to the point at which they are centered about formal axiom sets. The second point is important, for much mathematics has been done — and continues to be done — that is not fully axiomatic.

2.5 Some Final Comments

I have already explicitly addressed one of the concerns raised in Section 0.5 about PDR, i.e., that it places logico-inferential categories prior to ontological categories. In Section 2.3, I argued that this assumption is legitimate, at least from the perspective of an Egalitarian naturalist, because it is a working assumption of mathematicians.

Implicitly, I have also addressed a second concern raised in that section, viz., the neo-fregean reliance on singular terms and predicates rather than first- and higher-order generality. PDRists side with Quine, Ricketts, and the majority of other contemporary analytic philosophers on this point. The appropriate way to formulate the priority of logico-inferential categories over ontological categories is in terms of first- and higher-order generality, not singular terms and individual predicates.

This answer also deflects a third worry raised in Section 0.5, viz., how a PDRist should treat domains whose cardinality is uncountable. The simple answer is that because a PDRist does not take the ontological structure of a domain to be a projection of the singular terms of a discourse, this worry — at least in its original form — does not arise. There is, however, a related worry, viz., how mathematical practices — finite collections of activities — succeed in characterizing the ontological structures of domains whose cardinality is uncountable. Yet this is not a challenge to PDR so much as a challenge to mathematicians, for they clearly take themselves and their practices to be able to so characterize such domains. And, as an Egalitarian naturalist, a PDRists is happy to accept that mathematicians are in fact able to do both what they claim to be able to do and what their practices indicate they are in fact able to do.

I hope that the above discussion has provided the reader with a reasonable grasp of PDR — both of its metaphysics and the relationship it takes there to be between mathematical domains and mathematical practices — for it completes my primary exposition of PDR. I shall now turn to my arguments that PDR offers a superior interpretation of mathematical practices than do Platonism and Fictionalism.

PART II

EGALITARIAN NATURALISM VS. PLATONISM

Overview

The primary purpose of Part II of this dissertation is to offer an argument that, by the standards of an Egalitarian naturalist, PDR offers a superior interpretation of mathematical practices than does any variety of Platonism. This argument will take the form of a dilemma surrounding a Platonist's acceptance or rejection of the following assumption concerning mathematical theories, which I call **CYF**:

> It is possible for a mathematical theory to be coherent yet false.

Those who have offered, or supported, epistemological challenges to Platonism, such as the ones found in (Benacerraf, 1973) and (Field, 1989), have, if only implicitly, taken Platonists to be committed to CYF, for these challenges only have force if CYF is true. Further, if a Platonist does accept CYF, then she takes on a burden to provide an explanation of how, to take Field's version of the epistemological challenge, mathematicians can be justified in taking their theories to be systematically true of Platonistically construed mathematical domains. It is the purpose of Chapter 3 to argue that this is an explanatory burden that no Platonist can meet to the satisfaction of an Egalitarian naturalist. Thus, given that we do have mathematical knowledge and it would be nearly impossible to motivate any form of Platonism if you didn't believe that we do, there is a damming epistemological argument that can be leveled against a Platonist who accepts CYF.

For those who rest their rejection of Platonism on the epistemological argument, it is unfortunate that many Platonists reject CYF (cf., e.g., (Balaguer, 1998) and (Shapiro, 1997)). Even worse for such individuals is the fact that Platonists have good reason to do so, for mathematicians reject CYF.

Hilbert made famous the dictum that the consistency of the theory representing a mathematical domain is sufficient for the existence of that mathematical domain. And while Gödel's work on incompleteness certainly showed that the notion of "consistency" required in order to sustain this dictum is not deductive consistency, mathematical practices continue to reflect a modified form of Hilbert's dictum. Contemporary (classical) mathematicians merely use a different notion of "consistency" — what I am calling coherence — one closely linked to the notion of classical satisfiability, i.e., the availability of a classical model in standard set theory. For further details concerning this standard of existence, including an historical overview of the development of this standard, the reader is referred to Chapter 5 of (Shapiro, 1997). It is precisely this notion of coherence — the one closely linked with classical satisfiability — that I am using in CYF. I shall provide more details concerning this notion in Section 4.3.

Please note that if the coherence of a theory so understood is sufficient for the existence of the domain that that theory represents, then it is also sufficient for the truth of that theory, for the theory is true of the domain whose existence that theory's presence is sufficient to ensure. And thus, according to mathematical practice, it is impossible for there to be a mathematical theory that is coherent yet false.

Perhaps an inchoate recognition that CYF is false is the reason why many Platonists have not taken the epistemological challenge to be as worrisome as their Nominalist colleagues. Though if Platonists have recognized this fact, then few have made their recognition explicit. Regardless of whether Platonists have recognized that CYF is false, one must wonder why those offering epistemological challenges have not recognized this fact. Undoubtedly the answer lies in their having overestimated the level of similarity between the mathematical and non-mathematical natural sciences. It is a trivial observation that there are coherent yet false empirical theories. Indeed, it is well known that the majority of empirical theories are of this type.

Yet I believe that there is a deeper explanation to be found than this one. Associated with realism is an intuitive thought that we can fix the subject matter under consideration and then have disputes as to which theory is true of that subject matter. For instance, consider the spatio-temporal world. Intuitively it seems that we can fix it as the thing we want to theorize about and then offer competing theories of it. Newton's theory of space and time and Einstein's theory of space-time are, at least intuitively, two theories that have, at least historically, been taken to be correct theories of the spatio-temporal world. We know now that Newton's theory misrepresents that world, while Einstein's theory remains our favored theory of that world. Yet, despite our present knowledge that Newton's theory is false, there is an ahistorical sense in which these two theories are rivals with one another for the representation of space-time.

Let us attempt to be more precise about this notion of rivalry. Let us say that two theories are **rivals** if and only if they i) attempt to represent the same part of reality, ii) are individually coherent, and iii) are jointly incompatible. Obviously each of these three conditions calls out for further explication. Yet, as they stand, they capture an intuitive notion of rivalry. We have already said something about how condition ii) can be made more precise. More or less,[55] we can claim that it amounts to the theory having a classical model in standard set theory. For spatio-temporal theories

[55]This qualification is needed because of worries about mathematicians' uses of proper classes. Perhaps a more accurate claim would be that having a classical model in standard set theory is a good "model", where this term is used in its non-set-theoretic sense, for the notion of coherence.

it is likely that the other two conditions can also be made at least reasonably precise. There are a range of observable phenomena that pairs of theories like Newton's and Einstein's are both supposed to predict. Condition i) can be made more precise by exploiting the shared vocabulary associated with these theories, because of this substantial predictive overlap. Condition iii) can be made more precise by means of the observation that theories that are rivals in the relevant sense have Ramsey sentences whose conjunction is inconsistent. Thus, the above discussion indicates that Newton's theory and Einstein's theory are rivals — in a reasonably precise sense — for the representation of the spatio-temporal world.

Let us now think about mathematics and the intuitive sense of rivalry captured by the three conditions mentioned at the beginning of the last paragraph. Given this intuitive sense of rivalry, one might think that there are mathematical theories that are rivals in this sense. Consider, for example, the following collections of statements taken as axioms: $ZFC + CH$, i.e., Zermelo-Frankel set theory with choice plus the continuum hypothesis, and $ZFC + \neg CH$, i.e., Zermelo-Frankel set theory with choice plus the negation of the continuum hypothesis. Let us suppose, which is not completely unreasonable given Paul Cohen's independence results (cf. (Cohen, 1963)), that both of these collections of statements are coherent, i.e., have a classical model in standard set theory. Under this supposition, condition ii) is true of this pair of mathematical theories. Further, intuitively, both theories concern the domain of sets. That is, intuitively, there is a single thing, i.e., the set-theoretic hierarchy, which they both attempt to offer a theory of. Or, to put this point another way, intuitively, condition i) is true of this pair of theories. In addition, at least intuitively, given that one holds that the continuum hypothesis is true of the sets and the other holds that the continuum hypothesis is false of the sets, only one of these theories is true. So, intuitively, condition iii) is true of this pair of theories. Thus, intuitively, these two theories are rivals for the correct representation of the sets in a way that is similar to the one in which Newton's theory and Einstein's theory are rivals for the correct representation of space-time. Given this, it is most certainly plausible that some philosophers would accept that there are mathematical theories that are rivals in the intuitive sense captured by the three conditions mentioned above.

Now note that if one were to accept that there are pairs of mathematical theories that are rivals in this intuitive sense, then it would follow immediately that there are coherent yet false mathematical theories, for by condition ii), both theories in such a pair would be coherent. And by condition i) and condition iii), at most one of the pair would be true. Given this — and bivalence — at least one of the pair would have to be false. Thus, to the extent that the above discussion makes it plausible that some philosophers might accept that there are rival mathematical theories, the above discussion also makes it plausible that some philosophers might accept CYF.

I conjecture, with the above as support, that it is the acceptance of a belief in something like this type of rivalry between certain mathematical theories, a rivalry that I take to be linked in an intuitive way with realism, which has driven Nominalists, or more precisely those who offer epistemological challenges to Platonism, to accept CYF.

In light of the above discussion, one might legitimately wonder why there are no mathematical theories that are rivals, for, of course, there cannot be, if CYF is false. This is a tricky issue. Yet the reason, I believe, is that it is impossible to make the three conditions mentioned above precise in a way that is compatible with mathematical practices. Consider first the issue of capturing the idea that two mathematical theories are about the same part of reality. Unlike empirical theories, there are no observable predictive consequences that two mathematical theories must both account for. Mathematical theories are not in the business of predicting observations in the sense in which empirical theories are. Thus, this condition cannot be made precise in the same way that I suggested it could be made precise when it came to empirical theories.

Given that we have no observation terms that can be used in capturing the intuition behind condition i), we must look to the theoretical terms of our mathematical theories in order to capture this intuition. About the best that I can think to do on this front would be to pick out certain theoretical, and hence non-logical, terminology that is central to the subject matter in question and exempt such terminology from the kind of (re)interpretation that goes on in determining whether a theory is coherent. So, for example, if the theories being considered for rivalry were concerned with sets, one might insist that the relation of set membership be exempt from (re)interpretation. Or, to give a second example, if the theories being considered were concerned with the natural numbers, one might insist that zero and the successor relation were exempt from (re)interpretation.

Unfortunately, to do this would be to give up on condition ii), for the notion of a classical model is linked with the ability to provide whatever interpretation of all of the non-logical vocabulary that one wants. It is precisely such (re)interpretation of non-logical vocabulary that underwrites mathematicians' assessments of coherence. So, the suggestion that we take certain non-logical terms to be exempt from (re)interpretation is alien to the actual practice of mathematicians. As such this suggestion is unacceptable to Egalitarian naturalists. It seems, therefore, that there is no way to make condition i) precise that is compatible with the practices of mathematicians.

In light of the above argument, it seems that a Platonist should reject CYF and instead affirm that all individually coherent mathematical theories are true of some Platonistically construed mathematical domain. Such a Platonist, let us call her Con for convenience, I shall grant, is able to answer the kind of epistemological challenge mentioned above. Con's strategy will be something like the following. She will note that if the kind of Platonism she espouses is correct, then all that she would need to do in order to provide an account of mathematicians' knowledge of some Platonistically construed mathematical domain would be to show that there is good reason to believe that certain mathematical theories are coherent. Further, Con will claim, mathematicians provide adequate justification for taking their theories to be coherent on a daily basis.[56] And so, Con may maintain that the epistemological argument, which so crippled her colleague who accepted CYF, provides no reason to believe that she cannot account for mathematical knowledge.

Given the above argument that Platonists should accept that all coherent mathematical theories are true, one might legitimately ask: why consider the epistemological argument at all? Why not just note that Platonists who are sensitive to mathematical practices will adopt a form of Platonism that is able to overcome the epistemological

[56]Whether this claim is true depends in an important way on exactly how the notion of coherence is spelled out. Considering this technical question is an issue for elsewhere. For the purposes of this dissertation I shall grant a Platonist who rejects CYF that she can answer the epistemological challenge mentioned above.

argument? The simple answer is that my discussion of the epistemological argument will be important to the project of undermining Platonisms that do not accept CYF

At the opening of Chapter 4, after briefly surveying and rejecting other attempted solutions to the epistemological challenge mentioned above, I shall outline two strategies that Platonists might follow in rejecting CYF. And hence — I shall grant — in solving the epistemological challenge. One of these strategies is familiar; it is the strategy explored by Mark Balaguer (cf. (Balaguer, 1998)) and Stewart Shapiro (cf. (Shapiro, 1997)). The other is, to my knowledge, new, though related to Neo-Fregean logicism. The remainder of Chapter 4 will be an argument to the effect that, by the standards of an Egalitarian naturalist, Platonisms that reject CYF are inferior to PDR. This argument will be centered about the explanatory burdens imposed by the acceptance of the extra metaphysical commitments of these varieties of Platonism when compared with the metaphysical commitments of PDR. It is this argument that will invoke my discussion of the epistemological argument in Chapter 3.

CHAPTER 3

THE TRUE MORAL OF THE EPISTEMOLOGICAL ARGUMENT AGAINST PLATONISM

3.1 Overview

The purpose of this chapter is to argue that provided that a Platonist accepts that it is possible for a mathematical theory to be coherent yet false, her Platonism[57] is not a philosophy of mathematics that should be accepted by any Egalitarian naturalist.[58] Consequently, for the remainder of this chapter, we shall assume the thesis that it is possible for a mathematical theory to be coherent yet false (CYF). We shall consider, and argue against, Platonists who deny CYF in Chapter 4.

In order for a Platonist to accept CYF, she must maintain that the mathematical realm is strongly metaphysically independent of at least mathematical practices. We shall see in Chapter 4 that a Platonist who holds that the mathematical realm is only weakly metaphysically independent of mathematical practices automatically rejects

[57]Recall from Section 1.3 that Platonism is the conjunction of three theses about mathematical domains: 1) they exist; 2) they are at least weakly metaphysically independent of all social practices; and 3) they are abstract entities.

[58]See Section 0.2 for a detailed discussion of Egalitarian naturalism. Two theses are particularly important to it: 1) the mathematical and non-mathematical sciences use different methodologies and accept different standards of justification; and 2) mathematical and non-mathematical scientists deserve equal methodological respect from a methodological naturalist, because their methodological practices constitute a unified methodological approach to a natural scientific understanding of the world.

CYF. As a result, for convenience during this chapter, when I talk about mathematical domains being metaphysically independent of social practices, I shall mean by this that they are strongly metaphysically independent of social practices.

In Section 2 of this chapter I develop Hartry Field's version of the epistemological argument (cf. (Field, 1989)). I then defend this challenge against two criticisms: one offered by Jerrold Katz (cf. (Katz, 1981)) and David Lewis (cf. (Lewis, 1986)), which I shall consider in Section 3; the other offered by John Burgess and Gideon Rosen (cf. (Burgess and Rosen, 1997)), which I shall consider in Section 4. The conclusion of these discussions will be that the epistemological argument against Platonism can be sustained provided that CYF is true.

3.2 Field's Version of The Epistemological Argument against Platonism

Understanding the commitments of Platonism (cf. Section 1.3) places us in a position to understand the intuition at the heart of the epistemological argument against Platonism. According to Platonists, there is a metaphysical gulf between human beings, who on modern conceptions are non-abstract beings, and mathematical domains, which are abstract and metaphysically independent of all social practices. In maintaining that mathematical domains are abstract and metaphysically independent of all social practices, a Platonist is maintaining that a mathematician uttering a true pure[59] mathematical statement is a matter of her uttering something true of a domain that is abstract and metaphysically independent of social practices. The epistemological challenge to Platonists is to provide some account of her (and our)

[59]A statement is a statement of pure mathematics if and only if it contains only terms that refer to, denote, or range over mathematical entities, properties, or relations.

knowledge of pure mathematical truths given this fact. The intuitive worry is that it is unclear how she (or we) could be justified in taking pure mathematical statements to be true, given the metaphysical gap just mentioned between her (and us) and the domains in virtue of which pure mathematical statements are true.

The seminal formulation of this challenge (cf. Paul Benacerraf's *Mathematical Truth* (Benacerraf, 1973)), explicitly relied on the then popular causal theory of knowledge. According to this account of knowledge, for an individual X to know that a statement S is true, "some causal relation [must] obtain between X and the referents of the names, predicates, and quantifiers of S" (Benacerraf, 1973, p. 412).[60] Clearly, if mathematical domains are acausal, then no such causal relation can obtain between a human knower and the referents of names, predicates, or quantifiers in any pure mathematical statement, for the entities, properties, and relations such names, predicates and quantifiers refer to are part of a domain all of whose members fail to stand in causal relations to any other items. Thus, in particular, they fail to stand in causal relations to any human being.

So, provided that both the causal theory of knowledge (for mathematics) and the metaphysical account of mathematical domains provided by Platonists are correct, knowledge of statements referring to, denoting or ranging over mathematical entities, properties, or relations is impossible. Of course, at least according to any Egalitarian naturalist, mathematical knowledge is possible, and so a proponent of any Egalitarian naturalism must maintain that either the causal theory of knowledge (for mathematics) is false or Platonism is false.

[60]All page numbers concerning (Benacerraf, 1973) in this dissertation relate to the version reprinted in (Benacerraf and Putnam, 1983).

At the time when Benacerraf formulated this challenge, the popularity of the causal theory[61] meant that this argument at least seemed like a significant challenge to Platonists. Yet history has not been kind to the causal theory of knowledge. At least most epistemologists have rejected it in favor of other accounts, most prevalently reliabilist accounts of knowledge. Thus, given a choice between rejecting the causal theory of knowledge (for mathematics) or Platonism, the causal theory of knowledge (for mathematics) is, by now — with good reason — by far the more likely to be rejected. And Platonism appears to survive unscathed by its encounter with Benacerraf's challenge.

A more forceful articulation of the intuitive epistemological problem with Platonism was developed by Hartry Field in a couple of the papers collected in his *Realism, Mathematics, and Modality* (Field, 1989). The heart of Field's formulation of the epistemological challenge to Platonists is a request for an explanation of the systematic truth of mathematicians' (and our) mathematical beliefs. According to Platonists, there are two ontologically distinct realms that are connected in an appropriate way: first, a mathematical realm consisting of Platonistically construed mathematical domains, and second, a collection of beliefs, shared by many mathematicians (and others), about this mathematical realm. The appropriate connection required is that many of these beliefs be true. Given mathematicians' causal isolation from this mathematical realm, the realm in virtue of which these mathematical beliefs are true, Field's challenge to Platonists is to provide a naturalistically acceptable explanation of mathematicians (and others) having any systematically true collection

[61] In fact, the causal theory was only popular for perceptual knowledge. Nobody had seriously proposed it as an account of mathematical knowledge.

of beliefs about this realm, i.e., an explanation that includes no elements that natural scientists would find unacceptable.

The force of Field's challenge arises from the abstract mathematical realm countenanced by Platonists being that in virtue of which mathematicians' (and our) pure mathematical beliefs are true. No similar challenge can be offered to a PDRist because, according to her, mathematical practices are that in virtue of which mathematicians' (and our) pure mathematical beliefs are true. So, Benacerraf-type challenges rely not only on mathematical domains being abstract, but also on mathematical domains being metaphysically independent of social practices. Mathematical practices are spatio-temporally and causally instantiated, and so can influence human beings. Specifically, a mathematical practice can influence an individual human being so that she becomes a competent participant in the practice in question. And, according to PDRists, all that is involved in having mathematical knowledge is being a competent participant in a coherent mathematical practice. Obviously, an individual's level of competency can vary from minimal to expert, and thus individuals can have varying amounts and levels of mathematical knowledge.

Further, the reader should note that Field's challenge does indeed rely on the truth of CYF, for if all coherent mathematical theories are true, then there is a trivial explanation of the systematic truth of mathematicians' (and our) pure mathematical beliefs. The truth of CYF is required in order for there be something in need of explanation.[62]

In the above characterization of Field's challenge, I have represented him as requesting a naturalistically acceptable explanation of the systematic correctness of

[62]A more extensive discussion of the very brief argument contained in this paragraph will be provided in Chapter 4.

mathematicians' (and our) pure mathematical beliefs. In fact, at least to my knowledge, Field never commits himself to the explanation needing to be naturalistically acceptable. Yet some kind of higher-level commitment like naturalism must be underwriting claims like the following: "there is nothing wrong with supposing that some facts about mathematical entities are just brute facts, but to accept that facts about the relationship between mathematical entities and human beings are brute and inexplicable is another matter entirely" (Field, 1989, p. 232). My treatment of Field's version of the epistemological challenge will assume that the problem with taking such relational facts to be brute is that it would be in conflict with any Egalitarian naturalism. I shall explain why momentarily.

Before I do so, the reader should note that Field's formulation of the challenge takes a different logical form to Benacerraf's. Field is not claiming that it is a necessary condition on knowledge that the knower, or some group of experts within her community, be able to provide a naturalistically acceptable explanation, no matter how schematic in nature, of the systematic correctness of her beliefs. This is important, because either condition would have Field placing stronger constraints on mathematical knowledge than on ordinary observational knowledge. Human beings had all kinds of observational knowledge prior to anybody being able to provide a naturalistically acceptable explanation of their systematic correctness concerning these facts.

What is central to Field's challenge, as I understand it, is the following: natural scientists share a belief, which is embodied in their methodological practices, to the effect that naturalistically acceptable explanations are, in principle, available for many types of relationships. Among the relationships covered by this methodological assumption are certain types of epistemic relationships. It would shake the foundations of our scientific understanding of the world were there to be an agreement between an aspect of reality and our beliefs about that aspect of reality without there being, in principle, a naturalistically acceptable explanation of that agreement. So, Field's naturalistic assumption is not that we must be able to provide an account of our ability to have systematically true beliefs about some aspect of reality before we can have such systematically true beliefs. It is rather that there ought to be, in principle, a naturalistically acceptable explanation of any such relationship obtaining.

It is this naturalistic assumption that Field draws upon in formulating his challenge to Platonists. Given this assumption, it is anti-naturalistic to provide an account of the metaphysical nature of the mathematical realm that rules out the possibility of there being a naturalistically acceptable explanation of our having systematically true beliefs about that realm. Field challenges Platonists to show that they have not made this anti-naturalistic move. By asking them to point in the direction of some collection of mechanisms that are naturalistically investigable and which have the hope of explaining the relationship in question, Field is asking for what he believes to be the only possible evidence that this anti-naturalistic move has not been made.

Ultimately, if Platonists cannot answer this challenge, then no Platonist can legitimately appeal to human beings having mathematical knowledge in providing a naturalistically acceptable explanation of any feature of the world. Given scientists' desire to appeal to such knowledge in their explanations, the conclusion to draw is that we should provide a different account of the metaphysical commitments of mathematical practices. Field offers his Fictionalism as the best candidate for such an account;[63] I offer PDR as an alternative and superior account of the metaphysical commitments of mathematical practices.

The above discussion should make it clear that the epistemological argument against Platonism arises from a perspective that is internal to any variety of Egalitarian naturalism. Specifically, it is only because an Egalitarian naturalist is committed to respecting the methodological assumption of non-mathematical natural scientists that certain types of explanations are, in principle, available, that adopting a Platonistic account of the metaphysical nature of mathematical domains is problematic.

It should be recalled that one of the main aims of an Egalitarian naturalist is to adopt a perspective that takes the mathematical and non-mathematical sciences to constitute a unified natural scientific approach to understanding the world, while at the same time respecting the differences between the practices of the scientists working within these two groups of sciences. The main motivation for adopting a variety of Egalitarian naturalism is the way in which non-mathematical natural scientists use mathematics in their practices. In essence, the above arguments show that if CYF is true, and Platonism is the correct metaphysical account of mathematical domains, then there is an internal tension within any Egalitarian naturalistic perspective. Thus, if the argumentative strategy outlined above can be defended against challenges, then it will justify the thesis that a proponent of any variety of Egalitarian naturalism that

[63]It should be noted that it is a very strange dialectical move to offer an epistemological argument against Platonism as an argument for Fictionalism. The basis of the epistemological argument is that Platonism cannot provide an account of mathematical knowledge, yet, in this respect, Fictionalism is not better off, because according to a Fictionalist there is no genuine mathematical knowledge. The epistemological argument against Platonism is better suited to being an argument in favor of a philosophy of mathematics that can offer an account of mathematical knowledge. If the epistemological argument can be sustained, such a philosophy would have an advantage over Platonism. In this respect, PDR is a much more suitable conclusion to the epistemological argument than Fictionalism. I thank Crispin Wright for making this point clear to me.

accepts CYF should not promote a Platonistic account of the metaphysical nature of mathematical domains.

A further thing that the reader should note is that Field's formulation of the epistemological challenge to Platonists does not rely in any particular way on the causal theory of knowledge, or, indeed, on any particular theory of knowledge. Yet it is our lack of causal connection with a Platonistically construed mathematical realm that motivates Field's challenge. It is reasonable to assume that a challenge to provide a schematic account of the systematic correctness of our beliefs about the spatio-temporal world would be answered with an account that appeals, in an indispensable way, to direct and indirect causal connections between human beings and some aspects of the spatio-temporal world. Thus, were we to provide a schematic explanation of the systematic correctness of our spatio-temporal beliefs, it is reasonable to assume that we would appeal to connections that are not available to a Platonist in answering a challenge concerning the systematic correctness of our mathematical beliefs.

It is this asymmetry that drives Field's version of the epistemological challenge, though the asymmetry that Field points toward is broader than a mere difference with respect to causal relations. Field, and any well informed Egalitarian naturalist, is well aware that scientists provide other types of explanations than causal explanations in discussing spatio-temporal matters, and, I am sure, would be happy to accept any such explanation. Unfortunately for a Platonist, the features that are constitutive of 'abstract' all involve a denial of features typical of spatio-temporal entities. Consequently, the abstract nature of Platonistically construed mathematical entities makes it likely that *all* explanations grounded in features of the spatio-temporal world are unavailable to a Platonist in answering Field's challenge. Thus, it seems likely that a substantive answer to Field's challenge would require non-mathematical natural scientists to accept a type of relationship that is nothing like the ones that they recognize and work with at present. In other words, it is highly likely that there is no naturalistically acceptable sense in which Platonistically construed mathematical domains can be thought to influence mathematicians and their practices.

The reader should further note that an individual offering Field's version of the epistemological challenge need not claim that Platonists can make no headway in answering it. Derivation is an important aspect of the practices of mathematicians. The challenger can perfectly well acknowledge that this activity and others — such as concept formation and conjecture formulation — can be provided with legitimate naturalistic explanations. This maneuver leaves Platonists with a comparable task to achieve however, i.e., providing an explanation of the systematic truth of the specific beliefs that mathematicians take as the axioms of pure mathematical theories. These axioms include terms that refer to, denote, and range over items in mathematical domains. The systematic truth of these beliefs remains a mystery for Platonists to explain once they have offered a naturalistically acceptable account of derivation (concept formation, conjecture formation etc. etc.).

A natural move for a Platonist to make is to claim that these axioms are true by definition. Yet this will not do, or at least it will not do unless one adopts a form of Platonism that rejects CYF, because the challenger can easily respond by asking: why it is that there are Platonistically construed entities, properties and relations answering to the terms in these stipulative definitions?

3.3 The Katz-Lewis Challenge

Some, most prominently Jerrold Katz (cf. (Katz, 1981)) and David Lewis (cf. (Lewis, 1986)), have argued that a request for an explanation of the systematic correctness of our foundational beliefs about some subject matter only makes sense for contingent subject matters. The idea seems to be that the call for explanation only makes sense when certain counterfactual dependence relations obtain, and, if the subject matter in question is not contingent, there are no appropriate counterfactual dependence relations. Here is a quote from Lewis:

> ... nothing can depend counterfactually on non-contingent matters. For instance nothing can depend counterfactually on what mathematical objects there are ... Nothing sensible can be said about how our opinions would be different if there were no number seventeen. (Lewis, 1986, p. 111)

I suspect that what is going on here, at least for Lewis, is that he interprets Field as requesting a causal explanation of the systematic correctness of mathematicians' (and our) mathematical beliefs. This interpretation, when combined with Lewis' endorsement of a counterfactual analysis of causation, results in Field's request making no sense. Whether this is what is going on or not, if the Katz-Lewis line of reasoning were correct, it would follow that Field's challenge does not make sense because, at least most would maintain, mathematical truth is necessary.

Field addresses this challenge at length on pages 233 to 239 of (Field, 1989). Yet I believe that there is no need to discuss his responses in order to understand that these challengers are wrong in claiming that there is nothing that needs to be explained regarding the systematic correctness of mathematicians' axiomatic beliefs. The history of mathematics is replete with debates about the legitimacy, and truth, of numerous definitions and axioms. Certain pure mathematical axioms now accepted by mathematicians as true could have been rejected as false. Perhaps the most famous example is the so-called axiom of choice, whose truth was widely debated at the beginning of the twentieth century. Almost certainly, if one accepts CYF, then there will be a set theory that is coherent and yet false. For convenience, I am going to assume that $ZF + \neg C$ is such a theory. If you are inclined to accept CYF, yet question this particular choice of theory, then simply replace the arguments I give using ZFC and $ZF + \neg C$ with arguments that use your favored theory and a companion that correctly describes the mathematical domain that the false theory misrepresents.[64]

According to the suppositions in the last paragraph, mathematicians could have accepted different axioms concerning sets than the ones that they actually do. And if they had accepted such different axioms, then the collection of set-theoretic beliefs that they would have taken to be true would be quite different to what they actually are. The axiom of choice is required in the derivation of many important mathematical truths. If it is false, then there are many beliefs that are now taken to be true that are in fact false, and likewise many beliefs that are now taken to be false that are in fact true. Given the truth of these counterfactuals, one thing that Platonists must explain is why the axioms that mathematicians actually settled on, rather than ones which mathematicians could have settled on but did not, are the ones that are true of a mathematical domain that is abstract and metaphysically independent of social practices.

Here we find a response to the Katz-Lewis challenge. While Katz and Lewis might be correct that there is no counterfactual variation in what mathematical domains

[64] If you are not inclined to accept CYF, then please indulge me until Chapter 4.

there are, there is most certainly counterfactual variation among the pure mathematical beliefs of mathematicians. Given this counterfactual variation, there is a need for an explanation of why the axiomatic pure mathematical beliefs actually held by mathematicians are true of domains that are abstract and metaphysically independent of all social practices.[65]

In light of this discussion, it is perhaps best to formulate Field's version of the epistemological challenge in the following way. Any reasonable Platonist must hold that there are two ontologically distinct realms that are linked in an appropriate way: first, a realm consisting of a collection of mathematical domains that are abstract and metaphysically independent of all social practices, and second, a collection of axiomatic beliefs about these mathematical domains that are held by non-abstract mathematicians, beliefs whose negations could have been held instead. The challenge to Platonists is to provide a naturalistically acceptable schematic explanation of why it is that the actual beliefs that mathematicians accept as axioms of pure mathematical theories are, for the most part, true of these mathematical domains. Another way to put Field's formulation of the epistemological challenge is the following: Platonists must provide a naturalistically acceptable schematic explanation of how the contingent historical development of mathematics has resulted in mathematicians having mostly true axiomatic beliefs about mathematical domains that are abstract and metaphysically independent of all social practices. The force of both ways of expressing the challenge lies in the fact that according to a Platonist's conception of mathematical domains, they are abstract and metaphysically independent of social practices. These two features of mathematical domains ensure that those things in

[65]We shall briefly address the Katz-Lewis challenge again in Chapter 4, where we shall be able to appreciate more fully the force of this response to their challenge.

virtue of which mathematicians' (and our) pure mathematical beliefs are true are abstract and metaphysically independent of social practices, while mathematicians are non-abstract beings.

3.4 Burgess' and Rosen's Criticism of Field's Formulation of the Epistemological Challenge

I want now to turn our attention to an important critique of Field's formulation of the epistemological challenge — the one offered by John Burgess and Gideon Rosen (cf. (Burgess and Rosen, 1997)). Burgess' and Rosen's treatment of Field's challenge is premised on its being an argument for Nominalism, as opposed to an argument against Platonism, which is how the argument should be understood.[66] In fact, it isn't clear that Burgess' and Rosen's stereotypical anti-nominalist, the individual whom they are defending against the epistemological challenge, is a Platonist in the sense defined in Section 1.3. Burgess and Rosen simply leave it underdetermined what commitments their stereotypical anti-nominalist has on certain important issues. Throughout my discussion in this section, I am going to assess how their arguments fare when the epistemological challenge is understood as a challenge to Platonism. This is because understanding them in this way will help to provide further motivation for PDR. This might mean that, at times, I shall offer criticisms of Burgess' and Rosen's arguments that should not, properly speaking, be seen as addressed at them, because it isn't clear that they are defending Platonism.

Burgess and Rosen begin their discussion by reducing the challenge further than I did above. Specifically, they recognize that if the epistemological challenge can be

[66] In fairness to Burgess and Rosen, Field — and many others — have believed it to be an argument for Nominalism.

answered for one Platonistically construed mathematical domain, then that suffices to undermine the challenge. For if the challenge can be answered for one Platonistically construed mathematical domain, then the epistemological difficulties surrounding mathematical domains that are abstract and metaphysically independent of social practices cannot be as severe as originally thought. They suggest that sets be used as the test case. Further, they claim that the axioms of set theory can be reduced to just one; as they put it, "the full cumulative hierarchy of sets exist." Thus, Field's request for an explanation of the systematic truth of mathematicians' pure mathematical beliefs about sets can be reduced to a request for an explanation of a conjunction, where the first conjunct is: "it is true that the full cumulative hierarchy of sets exist," and the second conjunct is: "it is believed that the full cumulative hierarchy of sets exist."

This is acceptable. But in making, and accepting, this move, one must be careful not to take it to reduce the explanatory burden on Platonists. Specifically, in light of my earlier discussion of axioms, one must understand their existence claim as shorthand for the conjunction of the set-theoretic axioms actually accepted by set theorists, as opposed to some other potential selection such as $ZF + \neg C$.[67] This is important, because the challenge was to explain why mathematicians' pure mathematical beliefs are systematically true of mathematical domains. Given the fundamental role that axioms play in mathematics, if it turned out that one of the axioms concerning a mathematical domain that mathematicians accept is false, then they would not

[67]Here, once again, the argument relies on CYF. For convenience, I continue to use $ZF + \neg C$ as my example of a theory that is coherent yet false.

have systematically true beliefs about the mathematical domain in question — mathematicians' (and our) beliefs about the mathematical domain that the false axiom concerned would be systematically false.

Having reduced Field's challenge to a request for an explanation of the above conjunction, Burgess and Rosen note that if mathematical domains exist, "it makes very questionable sense to demand why they do, as if they could easily have failed to do so" (Burgess and Rosen, 1997, p. 45). This is akin to something I chose not to question during my discussion of Katz's and Lewis' challenge, so I shall grant them this point. Thus, the request for an explanation of the conjunction, it seems, reduces to a request for an explanation of why mathematicians came to believe the standard set-theoretic axioms in a manner that would give us reason to believe that the axioms in question are systematically true of a domain that is abstract and metaphysically independent of social practices. Yet surely, Burgess and Rosen maintain, at least much of such an explanation can be provided by looking at the history of set theory.

Having reached this conclusion, they ask: "Is there then anything left that needs explaining but hasn't been explained?" (Burgess and Rosen, 1997, p. 46) Their answer:

> Well, the *connection* between the two conjuncts has not been explained. Without such an explanation it may appear mere accident or luck that the theory we have come to believe is a theory that is true. The implicit suggestion will then be that if this has to be acknowledged to be just a lucky accident, then continued belief in standard set theory is not justifiable. (Burgess and Rosen, 1997, p. 46)

Thus far, Burgess and Rosen seem close to correct. The concern is that the connection between mathematical domains and mathematicians' beliefs about those domains must not be completely accidental, for if it is a matter of pure luck that the mathematicians' pure mathematical beliefs are one way, and true of a domain that is abstract and metaphysically independent of social practices, then given that they could easily have accepted different axioms, it could have been the case that they believed different axioms, ones that were false of that domain.[68] Yet if this is the case, then the belief that the axioms that set theorists actually accept are true of some domain that is abstract and metaphysically independent of social practices is unjustified in the sense that a belief is only justified if its presence is non-accidental in a certain way.

Further, the exact thing that is needed in order for the said belief to change status from being unjustified to being justified is an explanation of why mathematicians' pure mathematical beliefs are systematically true of a domain that is abstract and metaphysically independent of social practices, i.e., the very thing that Field is challenging Platonists to provide. Thus, we see that it is a consequence of Field's challenge that there is a natural sense in which a Platonists' belief that mathematicians have systematically true beliefs about domains that are abstract and metaphysically independent of social practices is unjustified (provided that CYF is true).

So, how do Burgess and Rosen respond to the above concern? They tell us:

> One can hardly avoid acknowledging that standard set theory is the end product of an immensely complex historical process that could have gone differently in countless ways. It was lucky that Cantor came along when he did with the key concepts; that opposing forces, which kept him from obtaining a major professorship and from publishing in some major journals, did not silence him altogether; that unlike some of his forerunners, he found contemporaries with the capacity to understand and appreciate his theories. But what we have just said about the cumulative hierarchy of set theory, in which Field does not believe, could equally be said about the warped space of general relativity, in which he does believe. Surely it

[68]Once again, CYF is being invoked here.

> is to a large degree a matter of luck that Einstein came along when he did with the key concepts; that the Nazi campaign against 'Jewish physics', with Einstein as its foremost target, did not succeed; that the remark, attributed by legend to Einstein himself, that only a dozen people in the world would have the capacity to understand the mind-bending implications of warped space, proved unfounded. If there is an argument for anything in the fact that accident and luck plays a large role in the history of science, it is an argument not just against set theory but against general relativity as well: it is an argument not for nominalism in particular, but for scepticism in general. (Burgess and Rosen, 1997, p. 46)

The sentiments expressed at the beginning of this quote are correct. Yet, I ask, how do they address the issue at hand? That there is an aspect of luck involved in the development of both types of sciences does not mean that all aspects of both types of sciences are governed by accident. So it doesn't rule out the possibility that there is some non-accidental feature of the development of the non-mathematical natural sciences that justifies us in taking spatio-temporal theories to be true of a reality that is (at least weakly) metaphysically independent of social practices, while there is no such feature present in the development of the mathematical sciences.[69]

Above, I emphasized that the epistemological challenger is not seeking an explanation of why it is that set theorists believe that *some* set theory is true, but rather why it is that they believe that *one particular* set theory is true, i.e., ZFC, as opposed to, for example, $ZF + \neg C$. The analogous question with regard to the theory of gravity is something like, why do physicists believe in Einstein's general relativity, as opposed to, for example, Newton's theory?

Once this is seen to be the relevant question, an answer immediately comes to mind. It is because physicists make predictions about, and observations of, a reality

[69] I shall discuss what that non-accidental feature is in the case of the non-mathematical natural sciences later in this section.

that both theories of gravity are meant to be theories of, and find that the one describes it better than the other. This is why we are justified in believing that Einstein's theory does a much better job of making systematically true claims about a reality that is at least weakly metaphysically independent of social practices than does Newton's.

Further, the reliance of the non-mathematical natural sciences on prediction and observation is a general feature of the non-mathematical natural sciences, and its presence is non-accidental. Thus, there is a non-accidental feature of the development of the non-mathematical natural sciences that is responsible for our confidence in the truth, or at least the approximate truth, of the claims endorsed by the practitioners of such sciences. In addition, this non-accidental feature links these claims to a reality that is at least weakly metaphysically independent of social practices, for decisions about which theory to accept are made on the basis of predictions about, and observations of, *that* independent reality.[70]

Having recognized this about the non-mathematical natural sciences, we must now ask: do the mathematical sciences also have some non-accidental feature that justifies our taking mathematical theories to be true of domains that are metaphysically independent of social practices? First things first: if the mathematical sciences do have such a feature, then Platonists can't claim that it is the same one that the non-mathematical natural sciences have, for *that* one relies on there being a causal relationship between human beings and the reality in question, and Platonists deny that there is such a relationship between mathematical domains and human beings. Second, to expand observation to cover acausal domains, a Platonist would need to

[70]The above argument obviously assumes a very mild variety of scientific realism, yet this should be a harmless assumption for any Egalitarian naturalist.

posit some sort of direct[71] epistemic access to mathematical domains, and she can only legitimately do this if she can answer Field's challenge. Third, the traditional Quinean move of claiming that justification in science is holistic can't help Platonists here,[72] for even if it could establish the truth of pure mathematical statements,[73] it couldn't possibly establish that these statements are true of domains that are metaphysically independent of social practices. This requires a separate argument in both the mathematical and the non-mathematical cases.

In the non-mathematical case, a scientific realist can offer arguments that the theoretical posits of the non-mathematical natural sciences are at least weakly metaphysically independent of social practices because of the relationship that these posits have to the entities that we do observe and take to have that status. For instance, here is such a (perhaps naïve) argument. We are justified not only in believing that electrons exist in virtue of their ability to play an indispensable role in scientific explanations but also in believing that they are at least weakly metaphysically independent of social practices. The justification for this latter claim is something like the following: electrons are among the spatio-temporal constituents of the entities that we observe and believe to be at least weakly metaphysically independent of social practices, and it is difficult to see how observable entities could be at least weakly

[71] Platonists need not claim that mathematicians have direct epistemic access to all aspects of mathematical reality, but, if in following this suggestion they don't claim that mathematicians have direct epistemic access to some aspects of that reality, I simply don't see how the suggested feature we are dealing with is sufficiently like observation.

[72] This Quinean argument looks something like this: confirmation in science is holistic and we gain justification for ontological commitments to entities that we don't directly observe by means of them playing an indispensable role in making predictions about the aspects of reality that we do observe. Further, mathematical entities play such an indispensable role in science. Thus, our ontological commitment to mathematical entities is justified.

[73] The essential ingredients of an argument that holistic justification does not extend to pure mathematical claims can be found in my *The Quine-Putnam Indispensability Argument* (Cole, 2001).

metaphysically independent of social practices if their spatio-temporal constituents were not. Here is another, perhaps once again naïve, instance of such an argument. We are justified in believing that galactic superstructures are at least weakly metaphysically independent of social practices, because their spatio-temporal constituents are entities that are at least weakly metaphysically independent of social practices, and it is difficult to see how a spatio-temporal entity could be strongly metaphysically dependent on social practices if all of its spatio-temporal constituents were not.

No similar argument, however, can be provided by Platonists for the metaphysical independence of mathematical domains from social practices. This is because, according to Platonists, mathematical domains are ontologically distinct from the spatio-temporal world. Mathematical domains (and their constituents) are not among the spatio-temporal constituents of observable entities, and mathematical domains do not have observable entities among their constituents. So this line of resistance is not helpful to Platonists.

Even though the mathematical sciences do not have the same features as the non-mathematical sciences, it doesn't follow that they don't have *some* non-accidental feature that justifies us in believing that mathematicians' claims are systematically true of domains that are abstract and metaphysically independent of all social practices. In order to investigate whether or not they do, we should ask what methodological principles are actually used by mathematicians in deciding between different axiom systems.

Fortunately, Penelope Maddy (cf. (Maddy, 1997)) has investigated this question with respect to the very branch of mathematics that has been the focus of our discussion, i.e., set theory. Her relevant conclusions are that there is a pattern in set theorists' choices of new axioms. Specifically, they are guided by two methodological maxims. These maxims are MAXIMISE, which states that set theorists implicitly accept that the set-theoretic universe is as large as it can be — specifically, they tend to accept axioms that maximize the number and variety of isomorphism classes present in the set-theoretic hierarchy; and UNIFY, which states that set theorists implicitly accept that there is only one set-theoretic universe. Further, set theorists' acceptance of both maxims reflects a conception of set theory as a mathematical foundation for the mathematical sciences. By this I mean a foundation in which all objects, structures, and theories of mathematical interest can be modeled and investigated. In addition, the structure of justification within set theory can be divided into two varieties: **intrinsic justification**, which appeals to the thing being justified yielding a set theory that conforms to set theorists' conception of the set-theoretic hierarchy, and **extrinsic justification**, which appeals to the thing being justified having valuable consequences, most importantly mathematical consequences, but perhaps also scientific consequences outside mathematics. If Maddy is correct, and in broad outline she is, then there are non-accidental features of set-theoretic practices that guide the development of set theory.[74]

The question of interest to us, however, is: do these non-accidental features give us any reason to believe that the set-theoretic hierarchy exists independently of social practices? So far as I can tell, they do not, for the structure of justification underlying the application of both maxims has only two formats, intrinsic and extrinsic

[74]This is not to say that these methodological maxims completely determine the development of set theory, merely that there are legitimate reasons behind certain of the choices that set theorists make.

justification, and neither intrinsic nor extrinsic justification within set-theoretic practices (nor a combination of the two) can give us reason to believe that set theory is systematically true of a domain that is abstract and metaphysically independent of social practices.

To see that intrinsic justification alone cannot provide us with such a reason, reflect on a group of authors engaged in a project of writing a fiction. There is little doubt that in such cases the world described by the authors, if it exists, is not even weakly metaphysically independent of their activities. Yet it is common in such settings to hear ideas accepted and rejected because they are either in agreement with, or contrary to, the authors' partially determinate collective conception of the fiction. For example, Emma has throughout the early sections of a book shown confidence and strength, and there has been no sign of these being anything but central to her character. A suggestion that she now run scared from dealing with a problem simply can't be included in the book, no matter how convenient, because it simply wouldn't be in character for Emma to act in this way. Thus, if it were suggested in the setting of a cooperative creation, the authors would reject it as failing to be in accord with the fictional world being described in the book. The authors of this book share a conception of Emma as confident and strong, and their choices concerning how the writing of the book should go on reflect this. While a cooperative group of authors may not be in complete agreement about all aspects of the fictional world described in their book, there are many that they are in agreement about, and this consensus drives their writing. If this kind of partial determinacy of conception can be present in the cooperative writing of a fiction, then set theorists sharing a partially determinate conception of the sets can't, by itself, provide evidence that the set-theoretic universe is metaphysically independent of social practices. Yet providing an intrinsic justification for some aspect of set-theoretic practices is nothing more than appealing to set theorists' having a partially determinate conception of the set-theoretic hierarchy.

Next consider extrinsic justification alone. If choosing to adopt axioms that are valuable by either mathematical or (non-mathematical) scientific standards is likely to be choosing to adopt axioms that are true of a domain that is metaphysically independent of social practices, then an argument for this fact is certainly required.[75] *Prima facie* it would be a particularly fortuitous accident if these two actions coincided. Thus, barring some argument for their coincidence, extrinsic forms of justification within set theory are also incapable of linking set theory to a domain that is metaphysically independent of social practices.

It might be objected that non-mathematical scientists choose theories on the basis of extrinsic considerations all of the time. To the extent that this is really the best way to describe the practices of non-mathematical natural scientists, and that the theories generated by such practices are true, this feature of those practices would indicate that the spatio-temporal reality theorized about in such theories is only weakly independent of social practices. Our decisions are contributing something to that reality in the way that Quine suggests (cf. Section 1.4). Yet, if Quine is correct, there is a difference between the spatio-temporal case and the mathematical case. It is the reliance of spatio-temporal theorizing on prediction about, and observation of, spatio-temporal reality. This reliance ensures that spatio-temporal reality is weakly independent of our theorizing, while the lack of these features, or any other such

[75]Here, once again, the argument relies on CYF.

features, in mathematical practices results in mathematical theories being wholly dependent on social (mathematical) practices.[76]

Further, combining intrinsic and extrinsic justification seems to be of no use in justifying the suggestion that set theory is a theory that is true of a domain that is abstract and metaphysically independent of social practices. They don't seem to combine in any way that could lead to a justification beyond that which could be given by either alone.

One might worry that Maddy has not accounted for all aspects of set-theoretic practices in her discussion. Undoubtedly, this is a possibility. Yet the burden in this matter must be on Platonists. If there is some aspect of set-theoretic practices that supports the idea that set theory is systematically true of a domain that is abstract and metaphysically independent of social practices, then it is for a Platonist to tell us what it is and how it supports this conclusion. Barring such an argument, we are justified in believing that set-theoretic practices provide us with no reason to believe that the set-theoretic universe is abstract and metaphysically independent of social practices, despite the fact that, by the standards of an Egalitarian naturalist, set theorists' standards are justified and yield true claims about the set-theoretic universe.

I believe that other branches of mathematics are similar in relevant respects to set theory. That is, there are non-arbitrary features that guide and justify their development, but, once again, these non-arbitrary features do not give us reason to believe that they are true of domains that are abstract and metaphysically independent of social practices.

[76]The discussions in Chapter 4 should provide further justification for this thesis.

The bottom line is that Burgess' and Rosen's first response to the question of accounting for the relationship between the belief that the set-theoretic hierarchy exists, and its actually existing, misses the point. After a further short discussion of the long passage quoted above, they once again ask the question: "is there anything left that needs explaining but hasn't been explained?" (Burgess and Rosen, 1997, p. 47) Once again, they answer:

> Well, there is a *connection* that has not been explained. It is the connection between set theory's being something that creatures with intellectual capacities and histories like ours might, given favourable conditions for the further exercise of their capacities, come to believe, and set theory's being something that is true.
>
> (Burgess and Rosen, 1997, p. 47)

This is just to repeat the content of the original request, which, I argued above, is legitimate. Burgess' and Rosen's interesting move comes next. They continue:

> Standard set theory, once it has been thought of, may be a very good theory by scientific standards. But those standards . . . include simplicity. And that can only mean simplicity as felt by creatures with capacities and histories like ours. But one may then demand an explanation:
>
>> [W]hy should one believe that the universe of sets . . . is so nicely arranged that there is a preestablished harmony between our feelings of simplicity, etc., and truth?[77]
>
> The implicit suggestion is that in the absence of a response, continued belief in the truth of standard set theory would be unjustified. What is being asked is thus in effect:
>
>> (xi) Granted that belief in standard set theory is justified by scientific standards, is belief in the truth of standard set theory justified?
>
> (Burgess and Rosen, 1997, p. 47)

[77]This quote is from (Benacerraf and Putnam, 1983, p. 35).

Burgess and Rosen go on to argue that if one wants to accept naturalism, then one must answer this question affirmatively. Their point is that the naturalism that they take as their starting point counsels that, barring some cogent set-theoretic argument to the contrary, if belief in the truth of the standard axioms of set theory is justified by the standards of set theorists, then we are justified in believing that standard set theory is true.[78] Egalitarian naturalists will agree with them on this point. This naturalistic point is important and tells against Field, who advocates the thesis that all mathematical assertions, bar those that are vacuously true, are false. Field's acceptance of this thesis is anti-naturalistic, and should be rejected as such.[79]

It should be noted, however, that this point against Field does not in any way undermine my arguments against Burgess' and Rosen's invocation of Einstein and warped space. In order to vindicate Platonism, one does not need to argue merely that we are justified in accepting the truth of standard set theory; one needs to argue also that we are justified in accepting that this theory is true of a domain that is abstract and metaphysically independent of social practices. The true target of

[78]Burgess' and Rosen's use of simplicity as a scientific standard that is accepted almost universally is somewhat problematic. There is no doubt that this standard was one of several that Quine accepted, and that many Quinean naturalists have followed Quine is this respect. Yet another excellent feature of Penelope Maddy's *Naturalism in Mathematics* (Maddy, 1997) is that it provides convincing arguments that in fact this is not one of the standards that set theorists apply in deciding whether new set-theoretic axioms should be adopted. To apply the standard of simplicity to set theory is to illegitimately generalize the standards of the non-mathematical natural sciences. Thus, I contend, following the publication of (Maddy, 1997), simplicity is no longer a standard of justification accepted almost universally when applied to mathematical theories.

Yet this last point is rather minor, because Quinean naturalism should never have been committed to acceptance of justification by the standards identified by Quine as the standards of justification operative within science, but to the actual standards of justification operative within the science, or sciences, that are responsible for justifying the particular claims under consideration. In this case, the relevant standards would be those of set theorists. Thus, naturalism, properly understood, i.e., Egalitarian naturalism, does counsel that, barring some cogent set-theoretic argument to the contrary, if belief in the truth of the standard axioms of set theory is justified by the standards of set theorists, then we are justified in believing that standard set theory is true.

[79]As mentioned above, I shall undertake a more systematic appraisal of the mistake made by Fictionalist philosophers of mathematics in Part III of this dissertation.

the epistemological argument is not someone who merely believes that mathematical assertions are true, but rather someone who conceives of the mathematical domains represented by mathematical assertions and beliefs as being abstract and metaphysically independent of social practices. So Burgess' and Rosen's point against Field does not amount to their having successfully defended Platonism.[80]

While discussing the implications of an Egalitarian naturalism within the context of Field's version of the epistemological argument, there is one final issue that should be addressed. The heart of this concern is that scientists do not, in fact, ask for an explanation of the systematic truth of mathematicians' pure mathematical beliefs. Given this, the worry is that the challenger's request for such an explanation is a request for a justification of the truth of mathematical statements and beliefs that goes beyond the standards of justification adopted by mathematicians. In light of this, one might believe that the challenge is, itself, anti-naturalistic.

Drawing this conclusion would be a mistake, for there is a good reason why actual scientists do not call for an explanation of the type that Field's version of the epistemological challenge requests. It is that mathematicians, in general, do not make any claims about mathematical domains being abstract and metaphysically independent of social practices in the way that Platonists claim they are. And, given that they do not make such claims, it follows that there is no need for mathematicians to provide the kind of explanation that Field's version of the challenge demands.

I suggest that if the mathematical community were, as a whole, to adopt a policy of justifying their choices of axiom systems by means of an appeal to a direct epistemic grasp of mathematical domains that are abstract and metaphysically independent of

[80]Please recall my earlier disclaimer that Burgess and Rosen might never have been attempting to defend Platonism.

social practices, then natural scientists would start demanding the kind of explanation of the faculty underlying this grasp that is at the heart of Field's request. It is precisely because mathematicians do not make any claims — and thus, specifically, not the claims of Platonists — about the metaphysical nature of mathematical domains, that natural scientists do not offer the kind of challenge that is at the heart of Field's formulation of the epistemological challenge. As I have been emphasizing throughout this chapter, the epistemological challenge is not a challenge to the truth of our mathematical beliefs and assertions, or the methodology or standards of justification operative within mathematical practices. It is, rather, a challenge to a construal of mathematical domains as abstract and metaphysically independent of social practices. Given that mathematicians, in general, do not themselves claim that mathematical domains have this metaphysical nature, it is no wonder that the scientific community does not challenge them to account for their epistemic access to domains that are abstract and metaphysically independent of social practices.

3.5 Some Final Thoughts

So, what morals should we draw from my extended discussion of Field's epistemological challenge? First, if you want to be an Egalitarian naturalist, then you must maintain that because appropriate mathematical statements and beliefs, including ones that have ontological commitments, are true by the standards of mathematicians, we are justified in believing that they are true. Thus, you should develop a philosophy of mathematics according to which mathematical domains exist.

Second, if you accept CYF, then Field's version of the epistemological argument against Platonism stands as a good argument against Platonism. Consequently, it would be wise for an Egalitarian naturalist's philosophy of mathematics not to both take mathematical domains to be abstract and metaphysically independent of social practices, and accept CYF.

Third, let us suppose for a moment that the way out of this situation for an Egalitarian naturalist is not to reject CYF. In this case, the above arguments suggest that an Egalitarian naturalist should develop a philosophy that takes mathematical domains to be strongly metaphysically dependent on some variety of social practices. What more natural candidate could there be than the social practices of mathematicians? In other words, what is suggested by these arguments is that an Egalitarian naturalist should espouse PDR concerning mathematics, for this account of mathematics maintains that mathematical domains are strongly metaphysically dependent on mathematical practices.

With these conclusions in mind, it is clear that the next step in showing PDR to be preferable to Platonism as an interpretation of mathematical practices is to show that rejecting CYF incurs unwanted, and unacceptable, costs for a Platonist. That will be the project of Chapter 4.

CHAPTER 4

A NATURALISTIC REJECTION OF PLATONISTIC SOLUTIONS TO THE EPISTEMOLOGICAL CHALLENGE

4.1 Overview

Undermining the legitimacy of the epistemological argument against Platonism is one way to counter it. We saw in Chapter 3 that this is Burgess' and Rosen's primary strategy. They attempt to argue that it is illegitimate because it is anti-naturalistic. The other way to counter it is to provide a positive answer to it. This latter strategy has been popular in the literature. The attempts to provide positive accounts of our epistemic access to Platonistically construed mathematical domains are numerous and diverse. Fortunately, Mark Balaguer (cf. (Balaguer, 1998, Chapter 2)) has undertaken the task of collecting together the main accounts and providing reasonably compelling arguments that all such accounts either fail outright, or can only succeed by adopting a form of Platonism that embodies an assumption that the authors of these accounts never make explicit.[81]

The assumption that Balaguer identifies is that the mathematical realm is so large that any individually "consistent" mathematical theory will be true of some part of

[81] I shall provide a short discussion of these arguments in Section 4.2.

that realm.[82] I place scare-quotes around consistent here, because what Balaguer means by "consistent" is not, or at least should not be, "deductively consistent". He is never quite clear about what the relevant notion is, but, as I shall argue in Section 4.3, the appropriate notion is close to Shapiro's notion 'coherent' (cf. (Shapiro, 1997)). For most of this chapter I shall use "coherent" rather than "consistent" when discussing Balaguer's assumption. The exception to this will be Section 4.3, in which I shall justify my use of coherence rather than "consistency". With this terminological point in place, another way to put the assumption that Balaguer identifies is this: the mathematical realm is so large that it contains a domain that makes every coherent mathematical theory true. Balaguer calls any form of Platonism that embodies this assumption full-blooded, or plenitudinous, Platonism; **FBP** for short.

In fact, I believe that Platonisms that embody an assumption of the type characteristic of FBP are but one variety of conceivable Platonisms that have the potential for providing a response to the epistemological argument against Platonism. At a number of points throughout Chapter 3, I made explicit that the epistemological argument against Platonism relies on the thesis CYF — it is possible for a mathematical theory to be coherent yet false. The intuitive idea behind an FBPist's response to the epistemological argument is to undermine this assumption by taking the mathematical realm to be very large. Indeed, FBPists claim that the mathematical realm is so large that any coherent mathematical theory will be made true by some part of that realm, i.e., some domain contained within it. If two coherent mathematical theories have different ontological commitments, then they are about different parts of the mathematical realm — or perhaps the same part under a different interpretation.

[82] I shall say a little more about the content of this assumption in Section 4.3.

This is the case even if they are both theories of what might be thought of intuitively as the same type of entity, e.g., sets.

We shall return to a discussion of this way of rejecting CYF in Section 4.3. For now I want to raise the possibility of rejecting CYF in a second way. An FBPist's conception of mathematical domains implicitly takes them to be strongly metaphysically independent of all social practices. According to it, mathematical domains exist and have their ontological structure completely independently of mathematical practices. Our mathematical practices merely serve to identify which domain(s) we are talking about. Counterfactual variation in the ontological commitments of our mathematical practices is accommodated by taking different parts of the mathematical realm, i.e., different mathematical domains, to make the various mathematical theories accepted in those counterfactual situations true. Yet if we take Quine's metaphor concerning the extrinsic individuation of reality seriously — that is, we adopt a neo-fregean[83] Platonism — then we can see that there is no need for this level of metaphysical extravagancy. In so far as the ontological structure of a mathematical domain is determined by features of the mathematical practice that represents it, the very same mathematical domain can have the ontological structure that we take it to have as a result of *our* mathematical practices being a certain way, and have a different ontological structure in other possible situations as a result of the mathematical practices in those situations being different. Thus, for example, one could maintain that the set-theoretic hierarchy has an ontological structure that makes the axiom of choice true of it, but had it been the case that set theorists had chosen to reject the axiom

[83]Recall that a neo-fregean Platonism is one that takes mathematical domains to exist independently of social practices, yet have their ontological structures constitutively constructed by mathematical practices.

of choice, that very same domain would have had an ontological structure that made the axiom of choice false of it. In essence, what the above makes clear is that for CFY to be true, at least part of the mathematical realm must be strongly metaphysically independent of social practices.

To my knowledge, nobody has explicitly explored this second possible response to the epistemological argument against Platonism. Most probably this is because of the tremendous difficulties involved in making precise — indeed, intelligible — Quine's metaphor concerning the extrinsic individuation of reality. Yet, with enough ingenuity, it might be possible to make this metaphor precise, and thus use it as a basis for defending Platonism against Benacerraf-style arguments. In fact, I can't help but think that, at some level, something like this is motivating the Neo-Fregean logicists in their belief that they have provided a response to the epistemological argument against Platonism. Yet if this is so, then, at least to my knowledge, they are never explicit about this fact.

Before I undertake my argument that Egalitarian naturalists should not endorse Platonisms that reject CYF, it is incumbent upon me to say a little more about why other attempted Platonistic solutions to the epistemological challenge fail. Therefore, in Section 4.2, I shall provide an overview of the discussion found in Chapter 2 of (Balaguer, 1998). Following this, in Section 4.3, I shall articulate the content of FBP style Platonisms and discuss in more detail how they answer the epistemological challenge. In Section 4.4, I shall offer an argument that FBP style Platonisms are philosophies of mathematics that Egalitarian naturalists should not adopt given the availability of PDR as a legitimate alternative to these Platonisms. In Section 4.5, I shall extend the argument offered in Section 4.4 to cover neo-fregean Platonisms of the type just discussed. Finally, I shall provide an overview of my argument for the superiority of PDR to Platonism.

4.2 Balaguer's Rejection of Solutions to the Epistemological Challenge

In Chapter 2 of (Balaguer, 1998), Mark Balaguer outlines three potential strategies of defense against the epistemological argument against Platonism. From the perspective of an Egalitarian naturalist, the first and most easily rejected is the suggestion that the human mind is not located in the spatio-temporal world. Potentially, this suggestion has as a consequence that there is not the metaphysical gulf between human minds and mathematical domains that the challenge presupposes. This suggestion is in direct conflict with the best theories of natural scientists concerning human minds, and so must be rejected by an Egalitarian naturalist.

The second strategy that Balaguer identifies is to argue that our epistemic access to mathematical domains is provided by our spatio-temporal senses. The main proponent of this strategy has been, but is no longer, Penelope Maddy (cf. (Maddy, 1990)). This second strategy, Balaguer argues, is faced with a dilemma: either the position under discussion is not Platonistic because the domains in question are not abstract and/or metaphysically independent of social practices, and we are not concerned with it here, or the domains in question are abstract and metaphysically independent of social practices, and it is simply a mistake to claim that we can gain knowledge of them with our spatio-temporal senses.

The third, and most promising, strategy acknowledges that we have "no contact" with Platonistically construed mathematical domains, yet argues that we can know pure mathematical truths about Platonistically construed mathematical domains nonetheless. Several different suggestions fitting into this third category are distinguished by Balaguer. The first suggestion is the one offered by Bob Hale and Crispin Wright. Their Neo-Fregean logicism (cf. (Wright, 1983) and (Hale and Wright, 2001)) builds on Frege's insight (cf. (Frege, 1884)) that the truth conditions of a range of pure mathematical statements and beliefs can be stated in non-mathematical terms. They argue that since knowledge of truth conditions stated in non-mathematical terms obtaining is considered unproblematic, knowledge of the truth of the pure mathematical statements and beliefs in question is unproblematic. Balaguer correctly points out that knowledge of the non-mathematical truth conditions obtaining is only taken to be unproblematic precisely because it is not taken to yield knowledge of domains that have the metaphysical nature that Platonists take mathematical domains to have. He then offers Hale and Wright a dilemma: either the knowledge in question is unproblematic but not of Platonistically construed mathematical domains, or the knowledge is of such domains and is problematic after all, in which case their solution begs the question. Either way, Hale's and Wright's solution, Balaguer argues, is unsuccessful.

It should be clear that if underlying Hale's and Wright's explicit suggestion is a neo-fregean conception of mathematical domains, then they are in fact correct about mathematical knowledge being obtainable in an unproblematic way. In effect, this knowledge is obtained in a manner similar to the way in which an FBP style Platonist achieves mathematical knowledge, i.e., by means of a rejection of CYF. Yet in fairness to Balaguer, at least to my knowledge, Hale and Wright are never explicit about their solution to the epistemological challenge involving a rejection of CYF. So, while Balaguer has identified the assumption required to solve the epistemological challenge incorrectly — he takes it to be the assumption characteristic of FBP, when in fact all that is required is a rejection of CYF — he is correct that it is only Platonisms that embedded a certain type of thesis that their authors never make explicit, i.e., the rejection of CYF, that can solve the epistemological challenge.

Balaguer calls the second “no contact” suggestion the “no-contact theory of intuition”. He summarizes this suggestion as follows: “The view here is that we possess a psychological apparatus whose only ultimate sources of information are the naturalistic sources of perception and introspection, but that nevertheless generates intuitive beliefs and thoughts about mathematical objects (or structures or patterns)” (Balaguer, 1998, p. 37). He ascribes this view to Charles Parsons, Jerrold Katz, and Mark Steiner, among others. The essential problem with this suggestion is that if there is such a psychological mechanism, then its mere presence provides little assistance in a quest to answer the epistemological challenge. The mere presence of such a mechanism offers us no account of why the thoughts and beliefs generated by the mechanism in question constitute a form of knowledge. At a minimum, as Field’s challenge emphasizes, we need an account of why the thoughts and beliefs generated by this mechanism are systematically true of Platonistically construed domains. The suggestion under discussion offers us no such account, though, if combined with a Platonism that rejects CYF, such an account could be provided. Of course, in the latter case it would be the rejection of CYF that would be doing the main work in answering the epistemological challenge, not the “no-contact theory of intuition”.

The third “no contact” suggestion that Balaguer raises is the Quinean route to mathematical knowledge through holistic confirmation. As I have already mentioned, Quine, and any Quineans who follow him, are mistaken in believing that confirmational holism extends to pure mathematical statements. I have argued for this thesis in (Cole, 2001).[84]

The fourth "no contact" solution that Balaguer identifies is the solution relating to the fact that mathematics is necessary. This is the heart of the Katz-Lewis criticism of the epistemological argument against Platonism that I discussed, and rejected, in Chapter 3. The rejection that I offered in Chapter 3 relied on CYF. Thus, it should be no surprise that the Katz-Lewis criticism can also be combined with a Platonism that rejects CYF to generate a solution to the epistemological challenge. It should be noted, however, that the force of my discussion in Chapter 3 was to show that in this case it would, once again, be the rejection of CYF that would do the main epistemological work in providing an answer to the epistemological challenge, not the necessity of mathematics.

It is worth exploring this point a little further. By rejecting CYF we can ensure that all coherent mathematical theories are true, i.e., no matter what the mathematical theory, if it is coherent, then it is true. In light of this, it should be no surprise that Katz and Lewis would think that the necessity of mathematics is relevant to answering the epistemological challenge. In essence, realizing that mathematical theories need to be true so long as they are coherent, Katz and Lewis offer a metaphysical explanation of this fact: mathematical theories are true if coherent, because the domains that they refer to are necessary existents. Pointing to counterfactual variation in mathematicians' mathematical beliefs makes it clear that this explanation is not sufficient. The appropriate variation to cover the "no matter what" earlier in this

[84]Balaguer offers a different problem with the Quinean argument than the one that I endorse in (Cole, 2001).

paragraph is not variation across counterfactual worlds, but rather variation across ontological commitments. It is thus the rejection of CYF that is required to do the work needed to answer the epistemological challenge.

The final "no contact" solution that Balaguer identifies is the structuralist solution (cf. (Resnik, 1997) and (Shapiro, 1997)). The core idea behind this solution is that mathematical axiom systems serve to implicitly define the structures/domains to which they refer and of which we gain knowledge by investigating the axiom systems in question. The problem with this strategy, as Balaguer points out, is that it provides us with no reason to believe that the structures/domains so picked out exist and have the metaphysical nature ascribed to them by Platonists. Though, provided that it is combined with a rejection of CYF, it can be seen to be the basis of a Platonistic solution to the epistemological challenge.[85]

4.3 FBP Style Platonisms and Their Solution of the Epistemological Challenge

With this brief summary of the literature on Platonistic answers to the epistemological challenge behind us, let us return to our discussion of FBP style Platonisms. Recall that the assumption that is characteristic of FBP style Platonisms is that the mathematical realm is so large that any "consistent" (coherent) mathematical theory will be true of some part of that realm, i.e., some domain contained within it. Two

[85] As an historical note, Balaguer's claim that Shapiro doesn't reject CYF is false. Textual evidence for this claim is included below. In fact, it is precisely this aspect of his view that Shapiro uses to answer concerns about whether the structures implicitly defined by various collections of axioms actually exist.

notions in this definition call out for further commentary: the notion of a mathematical theory being "consistent" (coherent), and the notion of a mathematical theory being true of some part of the mathematical realm.

Let us begin with the first. *Prima facie*, there is disagreement between Balaguer and Shapiro, the two main expositors of FBPs, on how an FBPist should understand the notion of "consistency" (coherence). Balaguer's FBPist draws on the work of Hartry Field (cf. (Field, 1989)) to spell out what is meant by "consistency". She invokes the notion 'it is logically possible' as a modal primitive in her account in order to avoid mention of Platonistically construed ontology. She does this because she wants knowledge of the mathematical domain referred to by a mathematical theory to be obtained by means of knowledge of the "consistency" of that theory. Her desire is for this to be an epistemological gain. Yet this desire for an epistemological gain will not be realized if knowledge of the "consistency" of the mathematical theory in question itself requires contact with/knowledge of some Platonistically construed domain.

Shapiro questions an FBPist's ability to obtain such an epistemological gain using the strategy she invokes. He writes:

> . . . 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." It is misleading to put things this way, however, because the modality that we invoke here is nontrivial, about as problematic as the traditional matter of mathematical existence. . . . it is not obvious that a notion of "consistency" suitable for an antirealist program will work here.
>
> . . .
>
> The relevant formal rendering of "coherence" . . . is not "deductive consistency." A better analogue for coherence is something like "satisfiability."

> It will not do, of course, to *define* coherence as satisfiability. Normally, to say that a sentence Φ is satisfiable is to say that *there exists* a model of Φ.
>
> ...
>
> ...We cannot ground mathematics in any domain or theory that is more secure than mathematics itself. ...I take "coherence" to be a primitive, intuitive notion, not reduced to something formal, and so I do not venture a rigorous definition. (Shapiro, 1997, pp. 133-135)

While it might appear that there is an irreconcilable difference between Shapiro and Balaguer on what the relevant notion of "consistency"/coherence is, I don't believe that the dispute is as severe as it appears to be. Shapiro appears to believe that, in exploiting an anti-realist notion of "consistency", Balaguer is attempting to provide an epistemological grounding for mathematics, i.e., to ground mathematics epistemologically in something epistemologically more secure than mathematics itself — specifically, the anti-realist modal primitive that FBPists' explication of "consistency" invokes. In a number of places, Shapiro has offered convincing arguments against the possibility of providing such an epistemological grounding for mathematics (cf., e.g., (Shapiro, 1991) and Shapiro (1993)). In fact, (Shapiro, 1993) is specifically directed against anti-realist modal strategies for providing such an epistemological grounding for mathematics.

Yet Shapiro has not interpreted Balaguer correctly.[86] Or if he has, then there is no need for Balaguer to be attempting to provide an epistemological grounding for mathematics. I think that Balaguer has — or should have — a weaker epistemological goal in mind. His goal should merely be this: to show that obtaining knowledge of pure mathematical truths does not involve being in "contact with" Platonistically

[86]The quote at the end of this section will provide support for my interpretation being preferable to Shapiro's.

construed mathematical domains. This is something that Shapiro should be in a position to agree with Balaguer about. It is, after all, the heart of his own solution to the epistemological worries about Platonism. By formulating his position using an anti-realist notion of “consistency”, Balaguer is attempting to make it plausible that knowledge of pure mathematical truths does not involve being in “contact with” Platonistically construed mathematical domains. This situation is quite compatible with our knowledge of whether a theory is “consistent” being no more secure than our knowledge that the mathematical domain described by that theory exists. Indeed, Balaguer can agree with Shapiro that both pieces of knowledge are nontrivial and that the two are equally difficult to obtain. All that Balaguer need insist on is that neither involves being in “contact with” Platonistically construed mathematical domains.

So, despite appearances, Balaguer and Shapiro can agree on a notion of “consistency”/coherence. It is one that, as Shapiro explains above, is neither deductive consistency nor satisfiability, though satisfiability is a good “model” for it. Further, it is, as Shapiro insists, a primitive that is epistemologically no more secure than mathematical knowledge itself. It is a notion of this type that I have been using and will continue to use when I use the term ‘coherence’.

Let us turn to the second issue, i.e., what it is for a mathematical theory to be true of a part of the mathematical realm. Shapiro’s mention of satisfiability as an analogue for coherence is helpful here, because it points in the direction of the relationship under consideration. The idea seems to be that it is possible to construct a model — Balaguer would say a “natural” model — of the theory from the ontological resources present in the mathematical realm. It is no easy matter to be precise about what Balaguer means by “natural” here, yet the intuitive idea is that there is some part of the mathematical realm that contains a collection of objects with properties and relationships among them, that perfectly correlates in the model-theoretic sense to the mathematical theory under consideration. Shapiro's structuralist leanings result in his seeing no need for the model to be "natural" in this sense. I have no interest in articulating an optimal version of an FBP style Platonism, so I am not going to address this dispute. I believe that leaving this dispute unresolved will not obscure anything relevant to my discussion of FBP style Platonisms. It should be noted, though, that the objects, properties and relations used in the models talked about here are what I am calling the domain that the mathematical theory in question is about.

With this brief explication of FBP style Platonisms behind us, let me be explicit about how they answer the epistemological challenge. Suppose two things: 1) that $ZF + C$ and $ZF + \neg C$ are coherent;[87] and 2) that instead of choosing to accept the axiom of choice, set theorists had chosen to reject it. The idea behind the answer offered by FBP style Platonisms is the following: that under such circumstances the axiom of choice would not have been true as it in fact is, but would rather have been false. How could this be, you might legitimately ask. An FBPist's answer is that what makes the axiom of choice true is its being an axiom of our set theory combined with the fact that our set theory is true in virtue of there being sufficient ontological resources in the mathematical realm for there to be a ("natural") model,

[87]This assumption would likely be challenged by Shapiro. His commitments to a very strong logic result in there being little room for maneuvering when it comes to which theories are coherent. Yet, even for Shapiro, there are set-theoretic principles such that both standard set theory combined with one of those principles and standard set theory combined with its negation are coherent. In the arguments below that use the axiom of choice as the relevant example, it is possible simply to replace the axiom of choice with some such set-theoretic principle and generate all of the conclusions that I draw.

or perhaps collection of models,[88] of that set theory drawing on resources from the mathematical realm. Yet if set theorists had made different choices, they could have adopted a different set theory, and the mathematical realm is so large that it has sufficient ontological resources that there would be a ("natural") model, or perhaps collection of models, of that alternative set theory in it, thus allowing the alternate set theory to be true. Of course, if we identified the specific ontological resources used in these ("natural") models, then, in all likelihood, they would be different. Though for Shapiro, whose structuralism makes him less interested in the model being "natural", it could be that the model simply uses a different interpretation. To put this point metaphorically, we can say that our set theory is true of one part of the mathematical realm and the alternate set theory — in which the axiom of choice is false — is true of another part of the mathematical realm. Alternatively, to talk in terms of domains, our set theory is true of one mathematical domain. The alternate set theory is true of a different mathematical domain.

Here is Balaguer's statement of the intuitive line of thought behind an FBPist's solution to the epistemological challenge:

> If FBP is correct, then all [coherent] purely mathematical theories truly describe some collection of abstract mathematical objects. Thus, to acquire knowledge of mathematical objects, all we need to do is acquire knowledge that some purely mathematical theory is *[coherent]*. ...But knowledge of the [coherence] of a mathematical theory ...does not require any sort of contact with, or access to, the objects that the theory is about. Thus, the Benacerrafian objection[89] has been answered: we can acquire knowledge of abstract mathematical objects *without* the aid of any sort of contact with such objects. (Balaguer, 1998, pp. 48-9)

[88]Balaguer is explicit that such an eventuality needs to be taken into consideration. Shapiro, by contrast, because of his commitments to a very strong logic, most probably would be skeptical about this eventuality needing to be taken into consideration.

[89]Balaguer is here referring to the epistemological argument against Platonism more generally speaking, not simply Benacerrafs' version of it.

I hope that the above suffices to make clear both the content of FBP style Platonisms as intuitive philosophies of mathematics and how FBP style Platonists are able to answer the epistemological challenge. With these points behind us, we must now assess whether any FBP style Platonism is an attractive, appealing, or true philosophy of mathematics.

4.4 Why an Egalitarian Naturalist Should Not Find FBP Style Platonisms Appealing

One concern that one might have about FBP style Platonisms relates to the technical details required in order to turn any specific formulation of such a Platonism into a complete, well worked out philosophy of mathematics. For example, here is a quote from the abstract of a recent paper: "In this paper, I argue that Balaguer's attempts to characterize full-blooded platonism fail. They are either too strong, with untoward consequences we all reject, or too weak, not providing a distinctive brand of platonism strong enough to do the work Balaguer requires of it" (Restall, 2003). These concerns are legitimate and might indeed be fatal to FBP style Platonisms. Yet for the purposes of this discussion, I want to ignore these types of technical concerns and grant that it is possible to work out the details underwriting at least one FBP style Platonism. For the remainder of Chapter 4, let us work under the assumption that there is an FBP style philosophy of mathematics that a) sustains Platonism as characterized in Section 1.3, and b) has the resources to answer the epistemological challenge, as characterized in Chapter 3.

So, we suppose, there is a properly Platonistic philosophy of mathematics that can answer the epistemological challenge. If one's desire is to hold on to Platonism come what might, then espousing this FBP style Platonism is a way to do that. Yet surely the fact that this FBP style Platonism is a philosophy of mathematics that does this does not, by itself, make it an attractive, appealing (or true) philosophy of mathematics. Even those who are inclined towards Platonism, and so are likely to be tempted to embrace an FBP style Platonism because of its epistemological successes, have their reasons for being so inclined. And before they are going to adopt an FBP style Platonism as their philosophy of mathematics, they are going to want to confirm that the FBP style Platonism in question is consonant with those reasons (and, indeed, the philosophy of mathematics that best accommodates those reasons).

What are the reasons why people find Platonism appealing? In recent times, there can be little doubt that the possibility of a semantic account of mathematical discourses that is uniform, or continuous, with the best account of non-mathematical discourses has been emphasized as the primary such reason. This desideratum was clearly articulated by Paul Benacerraf in *Mathematical Truth* (Benacerraf, 1973) and, I suggest, plays a role in the very popular Quine-Putnam indispensability argument (cf., e.g., (Putnam, 1971) — see also (Colyvan, 2001)).

Undoubtedly, FBP style Platonisms can vindicate this reason. According to FBP style Platonisms, the appropriate semantics for mathematical discourses is close to a standard Tarskian one.[90] Yet FBP style Platonisms are not alone in this respect. Pure constitutive social constructs are among the items to which our non-mathematical discourses ontologically commit us. According to a PDRist, the semantics for mathematical discourses will be uniform or continuous with these parts of non-mathematical discourses. Certainly these parts of our non-mathematical discourses were not the

[90]It should be noted that there might be some wrinkles here. Specifically, it might be that certain mathematical theories do not manage to isolate the structure of the domain to which they are ontologically committed up to isomorphism. In this case, one must modify the standard Tarskian semantics.

parts that Benacerraf had in mind when formulating his constraint of semantic uniformity. Yet it is clear that by making the semantics of mathematical discourses uniform or continuous with these parts of non-mathematical discourses, PDR is satisfying his constraint as it should have been formulated. Certainly, the semantics of mathematical discourses offered by PDRists is not radically discontinuous with the semantics of non-mathematical discourses in the way that a proof-based semantics would be.

A second advantage that Platonists often offer in favor of their account of mathematical domains is its ability to account for the literal truth and falsity of mathematical statements. Indeed, not only this, but it is also able to account for why the truth-values of mathematical statements coincide with the ones that mathematicians assert them to have. Once again, however, a PDRist is also able to account for these facts about mathematical statements. According to her, mathematical truths are as literally true as the statement that "There is a political border between the U.S.A. and Canada." And because the truth-values of mathematical statements are determined by the standards of true assertion present in the practices of mathematicians, the truth-values of mathematical statements coincide with the ones that mathematicians assert them to have.[91]

Furthermore, it should be noted that a PDRist is able to offer the above-mentioned explanations while taking on much weaker ontological commitments than an FBP style Platonist. For while PDRists maintain that there are as many mathematical domains as mathematicians have coherently characterized, all of these domains are

[91]Ideally, I should provide more detailed arguments that PDR can account for these features of mathematics. Unfortunately there is only a finite amount of space in my dissertation. These arguments will follow in later work.

constitutively constructed by mathematical practices. It should be clear that any reasonable Egalitarian naturalist will take ontological commitment to practice-dependent domains constitutively constructed by practices already, and uncontroversially, in her ontology, to be preferable, all things being equal, to ontological commitment to the same number of Platonistically construed domains.

An immediate consequence of this observation is that an FBP style Platonist is going to have to provide further naturalistic reasons for adopting her Platonism if that Plationism is to be found preferable to PDR. Indeed, in light of the metaphysical extravagancy of FBP style Platonisms in comparison with PDR, if one of them is going to be found appealing or to be a theory that an Egalitarian naturalist will be justified in believing to be true, then a proponent of that FBP style Platonism is going to have to provide some explanatory, or perhaps justificatory, advantage for her Platonism over PDR. Otherwise her Platonistically construed domains are destined to strike an Egalitarian naturalist as "epiphenomenal" and be dismissed as unnecessary ontological encumbrances.

Of course, historically, abstract entities — like mathematical domains — have earned their keep in precisely this way. They have been invoked by their defenders in so-called metaphysical explanations. And it has been claimed that we are justified in our beliefs about them in virtue of how they really are. It is almost uniformly accepted that the presence of a spatio-temporal world that is at least weakly metaphysically independent of social practices should be involved in the best explanation of both the presence of, and to some extent the shape of, everyday, and perhaps theoretical, discourses concerning that world. In addition, it is widely, though by no means uniformly, accepted that our taking certain statements from such discourses to be true is, to some extent, justified by independently existing features of that world. Even the majority of those who reject this thesis, primarily because, following Wilfrid Sellars (cf., e.g., (Sellars, 1956)), they seek to emphasize the social-institutional nature of justification, acknowledge that the spatio-temporal world being the way that it is plays an important role in explaining, and constraining, the social-institutional practices of empirical justification. Thus, a spatio-temporal world that is at least weakly metaphysically independent of social practices concerning that world is taken to play both an explanatory and, at least to some extent, justificatory role for those practices.

Let us say that a realm or domain that is a) at least weakly metaphysically independent of the practices that serve to represent and explain it, and b) is invoked in either an explanatory or justificatory role with respect to those practices, in ways that are similar to the ways in which the spatio-temporal world is invoked in explaining and justifying the structure of discourses about the spatio-temporal world, serves as a **metaphysical grounding** for the practices in question. Historically, one reason why people have been inclined towards Platonism is that they have believed that there is a need for mathematical domains that are metaphysically independent of social practices to serve as a metaphysical grounding for mathematical practices.

It would likely be false, but at a minimum an exaggeration, to suggest that those who affirm some variety of Platonism in the contemporary literature take mathematical domains to perform any substantial amount of explanatory or justificatory work concerning mathematical practices. Most have had their Platonism tempered in one way or another. Yet as the above discussions make clear, even contemporary

Platonists do take the mathematical domains they countenance to do some explanatory or justificatory work. While the majority nowadays would restrict that work to that mentioned above, i.e., the availability of a uniform semantics and the ability to take mathematical statements to have the truth-value ascribed to them by mathematicians, individual Platonists are tempted to take them to do more work.[92]

For convenience, let us call any explanatory and/or justificatory work done by mathematical domains that are metaphysically independent of social practices beyond that mentioned above the **metaphysical work of the radical Platonist**, and those who believe that Platonistically construed mathematical domains do such work **radical Platonists**. It is important to note that *it is precisely because of the way in which FBP style Platonists solve the epistemological challenge that the mathematical domains countenanced by FBP style Platonists are unable to perform the kind of explanatory and justificatory work that motivates the radical Platonist.*

Consider, for example, the claim that it is because the set-theoretic hierarchy has the structure that it does that mathematicians believe, and are justified in believing, that the axiom of choice is true. This claim is one that could easily be made by a radical Platonist — think, for example, of Kurt Gödel. The idea behind it is that it is precisely because mathematicians have the kind of epistemic access to a Platonistically construed set-theoretic hierarchy that an FBP style Platonist's answer to the epistemological challenge takes them to fail to have, that they choose, and are justified in choosing, the set-theoretic axioms that they do.

In effect, the above claim about the axiom of choice is an expression of an old, and almost certainly incorrect, understanding of how discourses evolve so that the

[92]Examples include Bob Hale (cf. (Hale, 1987)) and Kurt Gödel (cf. (Gödel, 1947)).

statements made within them are true of domains that are strongly metaphysically independent of social practices. This understanding takes the domains in question as having determinate ontological structures and sees discourses that are intended to make true claims about those domains as molding themselves to capture those ontological structures accurately, where 'accurate capturing' amounts to there being the relationship between the statement/discourse and domain in question typified by truth in a model.

This traditional perspective emphasizes the primacy of the metaphysically independent ontological structure of the domain in question in the evolution of true statements/discourses. Following W.V.O. Quine's attack on the analytic-synthetic distinction (cf., e.g., (Quine, 1951)) and defense of methodological naturalism (cf., e.g., (Quine, 1969)), it has been typical to emphasize the importance of both the ontological structure of domains and linguistic intentions/conventions in the development of true discourses, and to claim that the roles of these two components cannot be separated.[93] Yet even according to this conception of the evolution of discourses towards making true claims, observation plays an important role in bringing about changes in truth-value assignments. Thus, since these observations are, at least indirectly, of the domains in question, features of those domains have a kind of primacy in the evolution of discourses that make true claims about those domains.

If one reflects on an FBP style Platonist's account of how it is that true mathematical discourses evolve, one will see that it inverts the order of explanation in that evolution from that embodied in both of the perspectives outlined in the previous paragraphs. According to the perspective of an FBP style Platonist, the language of

[93]Some draw the weaker conclusion from Quine's work that these two components cannot easily be separated.

mathematical discourses, and consequently their ontological commitments, can evolve free from any guidance/interference from mathematical domains that are metaphysically independent of social practices. This evolution is subject only to considerations internal to mathematical — and perhaps other scientific — practices, most importantly coherence. Yet once mathematical practices have solidified with respect to their ontological commitments, the mathematical realm that is abstract and metaphysically independent of social practices is so plenitudinous that, provided only that the theories that evolve are coherent, there are aspects of that realm of which those theories are true. The effect is that mathematical practices, and the linguistic intentions/conventions they embody, are primary in the evolution of mathematical theories — mathematical domains play only a secondary role. This is the exact opposite of what is envisaged by a radical Platonist.

Certainly an FBP style Platonist can legitimately affirm in a vacuous way that it is because the set-theoretic hierarchy has the structure that it does that mathematicians believe, and are justified in believing, that the axiom of choice is true. Yet the more informative explanation and justification of this fact, from the perspective of an FBP style Platonist, requires one to look at the historical development of set theory and ask how and why it was that set theorists came to accept the axiom of choice and take ZFC to be coherent. That history shows that it was because a rejection of the axiom of choice would have crippled many branches of mathematics. It turned out to be impossible to prove many of the central theorems of many important branches of mathematics without invoking the axiom of choice. In other words, using Penelope Maddy's terminology, the real justification, and consequent explanation, from the perspective of an FBP style Platonist, of why it is that set theorists adopted the axiom of choice, is internal to mathematics and almost entirely extrinsic in nature.

Shapiro explicitly acknowledges that his *ante rem* structures do not play the kind of explanatory (and, I believe he would agree, justificatory) role that *ante rem* universals have played in the history of philosophy. He writes,

> In the history of philosophy, *ante rem* universals are sometimes given an explanatory primacy. ... No such explanatory [primacy] is contemplated here on behalf of *ante rem* structures. I do not hold, for example, that a given system is a model of the natural numbers because it exemplifies the natural-number structure. If anything, it is the other way around. What makes the system exemplify the natural-number structure is that it has a one-to-one successor function with an initial object, and the system satisfies the induction principle. (Shapiro, 1997, pp. 89-90)

Balaguer is not explicit about this point. Yet there seems little doubt that he would agree with Shapiro's assessment. Indeed, Balaguer touts it as an advantage of FBP that it "reconciles the objectivity of mathematics (to which all platonists are committed) with the legitimacy of pragmatic modes of justification" (Balaguer, 1998, p. 69); that is, it reconciles the objectivity of mathematics with the extensive use of extrinsic justification — in Maddy's sense — within mathematics. Further, it is a "(related) advantage of FBP that it reconciles the objectivity of mathematics with the extreme *freedom* that mathematicians have" (Balaguer, 1998, p. 69).

So Balaguer certainly doesn't take the Platonistically construed domains countenanced by FBPists to be doing one sort of explanatory and/or justificatory work. We have just seen that he takes all Platonists to be committed to the objectivity of mathematics. Perhaps the Platonistically construed domains countenanced by FBPists can help explain and/or justify the objectivity of mathematics, another explanatory and justificatory purpose to which a radical Platonist might put the mathematical domains she countenances. Consideration of the following collection of claims from Balaguer's reply to the criticism that FBP gives up on the objectivity of mathematics, should make it clear that Balaguer does not take the mathematical domains countenanced by FBPists to do such explanatory and/or justificatory work:

> The claim that FBP-ists cannot salvage the objectivity of undecidable open questions is simply false. Most mathematical disputes can be interpreted as disputes about what is true in the standard model (or models). . . . When people argue about whether some axiom candidate that's supposed to settle the CH question is true, what they are really arguing about is whether the given axiom candidate is inherent in *our notion of set*. . . .
>
> Now, it *may* be that *our notion of set* is non-categorical, that is, that there are numerous models of set theory that are not isomorphic to one another but are, nonetheless, standard — or as standard as any other model. . . . If this is the case, then for *some* open set-theoretic questions, there is no objectively correct answer. . . .
>
> . . . FBP-ists can account for *more* of mathematical practice in this connection than traditional Platonists can. In particular, they can account for the existence of undecidable open questions with objectively and uniquely correct answers *and* undecidable open questions *without* objectively correct answers. Most philosophies of mathematics *dictate* that we take one stance or the other here with respect to *all* open questions. But FBP allows mathematicians to say whatever they *want* to say . . . This, I think, is an extremely appealing feature of FBP. . . . a good philosophy of mathematics should not dictate things like this to mathematicians; the point of the philosophy of mathematics is to *interpret* mathematical practice, not to place metaphysically based *restrictions* on it. . . .
>
> As for open *arithmetical* questions, I think we can safely say that *all* of these have unique, objectively correct answers. . . . because we're convinced that *our conception of the natural numbers* is categorical. . . . That, at any rate, is what *mathematicians* would say. (Balaguer, 1998, pp. 62 - 64)

It seems clear that an FBPist is committed to the idea that whether or not certain open questions have objectively true or false answers is a matter of whether or not *our conception* of the relevant domains delivers determinate answers to the relevant open questions. Yet a Fictionalist philosopher of mathematics can just as well appeal to *our conception* of the relevant domains to account for the "objectivity" of certain mathematical claims, as can an FBPist. So, the mathematical domains countenanced by FBPists play neither an explanatory nor a justificatory role in an FBPist's account of that in which the objectivity of mathematical statements consists.

Indeed, Balaguer's appeal to *our conception* of mathematical domains — as embodied in the notion of the (or a) standard model — in explaining and/or justifying aspects of mathematical practices is prominent in his whole discussion of FBP, as is his lack of invocation of mathematical domains that are abstract and metaphysically independent of social practices in such explanations and/or justifications. Consider, for example, Balaguer's response to the objection that we think of sentences like '2 + 2 = 5' as false in some *absolute* sense:

> FBP-ists can account for the intuition we have that sentences like '2 + 2 = 5' are false in some absolute sense. We could construct a consistent purely mathematical theory in which '2 + 2 = 5' was a theorem, but to do this, we would have to use at least one of the terms in this sentence in a non-standard way. . . . But if we interpret the terms of this sentence in a non-standard way, then it would not really be saying that 2 + 2 = 5. As long as we interpret '2 + 2 = 5' in the standard way, that is, according to *English*, it will be false. . . . I think that this way of putting the point explains why the fact that '2 + 2 = 5' is false in the standard model leads to the intuition that it is false absolutely. (Balaguer, 1998, p. 67)

Clearly, appeal is being made here only to how our conception of arithmetic, as embodied in the standard model of arithmetic, generates the intuition that '2 + 2 = 5' is false in an absolute sense. Thus, once again, the mathematical domains countenanced by FBPists are not invoked here as either explainers or justifiers.

If we return to Balaguer's discussion of the objectivity of mathematics, we can see that there is something more going on in it than the simple fact that Balaguer only ever appeals to features internal to mathematical practices and never to the mathematical domains countenanced by FBPists in providing explanations and justifications of mathematical practices. He is, in fact, critical of those who attempt to proceed in other ways, for to do so is — at least potentially — to place metaphysically based restrictions on the development of mathematical practices. Balaguer is clear that this is inappropriate.

One might be tempted to write off both Balaguer's and Shapiro's commitment to mathematical practices being primary in the order of explanation (and/or justification) as one that is separate from, and independent of, the commitments of an FBP style Platonist. Yet, in fact, there is a deep connection between the two, for if our epistemic access to mathematical domains is as an FBP style Platonist maintains it to be, then how could one proceed in any way other than from mathematical practices to conclusions about mathematical domains? It seems that one could not, because our only epistemic access to those domains is embodied in our knowledge of certain features of mathematical practices. Most importantly, one cannot invoke any knowledge of the metaphysical nature of mathematical domains that is not provided by knowledge of the mathematical practices that refer to those domains in explaining or justifying features of those practices. According to an FBPist's account of our epistemic access to mathematical domains, no such knowledge is available.

Further, if some feature of a mathematical practice does justify us in claiming that we do have knowledge of some feature of the metaphysical nature of the mathematical domain(s) it represents, then it isn't really that the metaphysical nature of the mathematical domain(s) is explaining or justifying that feature of the mathematical practice. It is rather that we are able to draw the metaphysical conclusion about the mathematical domain(s) on the basis of evidence from the relevant features of the mathematical practice. The bottom line is that an FBP style Platonist, given his/her account of our epistemic access to mathematical domains, cannot do what a radical Platonist wants to do.

It is worth emphasizing that, given an FBP style Platonist's solution to the epistemological challenge against Platonism, an FBP style Platonist has not provided herself with the kind of epistemic access to mathematical domains that could serve to allow certain features of those domains to provide naturalistically legitimate explanations and justifications of features of mathematical practices. The only way that a mathematical domain that is abstract and metaphysically independent of social practices could be invoked in a legitimate naturalistic explanation or justification of how, or why, mathematical practices are the way that they are would be for the individuals engaged in those practices to be influenced by that domain. The influence need not be causal, and so the point I am making is more general than that of the old epistemological argument, yet it does need to be achieved by some means that is naturalistically acceptable. Unfortunately for her, an FBP style Platonist's ability to solve the epistemological challenge is precisely built on there being no need for mathematical domains to influence human beings in any way in order for them to have mathematical knowledge. So, the epistemic access granted to Platonistically construed mathematical domains by means of adoption of an FBP style Platonism won't do the trick for a radical Platonist.

Perhaps an example will be useful in illustrating this general point. Consider for a moment the suggestion that the metaphysical nature of mathematical domains explains and justifies our belief that mathematical truth is necessary. How exactly could it do this without exerting some influence over us? It has to be admitted that it is a contingent fact about the philosophy of mathematics community that the majority of us believe that mathematical truth is necessary. In light of the influence of Quine on contemporary analytic philosophers of mathematics, specifically his naturalism and his indispensability argument, a number of philosophers have come to believe that mathematical truth is not necessary after all (cf., e.g., (Colyvan, 2001)). Given this, had someone with Quine's influence and understanding of the nature of mathematics played an earlier role in the history of philosophy, it seems perfectly possible that the consensus opinion could have been that mathematical truth is contingent rather than necessary. In light of the contingency of our belief in the necessity of mathematical truth, our general acceptance of its necessity, and the legitimacy of that acceptance, is in need of a naturalistic explanation and justification. Without being able to provide a substantive naturalistically acceptable account of how the metaphysical nature of mathematical domains could influence our beliefs, that metaphysical nature can play no role in any such naturalistically acceptable explanation or justification.

In light of FBP style Platonists' solution to the epistemological challenge, an FBP style Platonist might try to respond to this argument by making the same move with respect to metaphysical nature that she makes with respect to the number and variety of different ontological commitments that can be accommodated by the mathematical realm. That is, she could insist that the mathematical realm is so large that one can find not only domains in it that will make every coherent purely mathematical theory true (in a natural way), but also make true any metaphysical claims that we choose to make about the said mathematical domains.[94] But to do this is to give up on a

[94]It is not clear to me that one could do this for all such metaphysical claims, but perhaps one could do it for some. At any rate, given that this strategy is not ultimately of help to the radical Platonist, or FBP style Platonist, we need not investigate this matter.

radical Platonist's attempt to use the metaphysical nature of mathematical domains to explain and justify our mathematical practices. As with FBP style Platonist's explanations and justifications of features of our mathematical practices, such as the adoption of the axiom of choice or the objectivity of mathematics, the real explanatory, and perhaps justificatory, force is located in historical naturalistic explanations, and perhaps justifications, of why we choose to make the metaphysical claims about mathematical domains that we do. It is merely that the mathematical realm is now so large that there will be some domain in it that makes such metaphysical claims true.

The general lesson of this discussion is, I believe, the following. A radical Platonist has two options. First, provide a substantive naturalistically acceptable account of how a Platonistically construed mathematical domain being the way that it is can influence non-abstract beings' beliefs. Second, accept that the only strategy for making various metaphysical claims that she wants to have turn out true come out as ones that we are justified in believing to be true by the standards of an Egalitarian naturalist, denies mathematical domains the kind of explanatory or justificatory power that motivated her to her Platonism in the first place. In the latter situation, the real explanatory, and perhaps justificatory, power must be located in causal historical accounts of how and why our social practices developed in the way they did. There is little solace to be found for a radical Platonist in an FBP style Platonist's solution to the epistemological challenge, or the above suggested metaphysical extension of it. If a radical Platonist who is also committed to Egalitarian naturalism wants to hold on to Platonistically construed mathematical domains as a metaphysical grounding for mathematical practices, she must find a different, yet naturalistically acceptable, account of her knowledge of those domains.

In light of the general lesson above, I think that it is fair to say that FBP style Platonisms, and their metaphysical extensions, will not be as popular with radical Platonists as one might expect, in light of their epistemological successes. Yet, I believe that an Egalitarian naturalist can draw an even stronger lesson. That lesson is that, provided PDR is a legitimate alternative, we are not justified in believing any FBP style Platonism to be true, for, as I pointed out earlier, an FBP style Platonism should only be taken to be preferable to PDR by an Egalitarian naturalist if its mathematical domains can be shown to do the kind of explanatory and/or justificatory work that I have just argued they cannot.

4.5 Extending the Argument to Neo-Fregean Platonisms

The above arguments have all been aimed at FBP style Platonisms. Yet it should be clear that a Platonism embodying the Quinean suggestion that mathematical domains are extrinsically individuated would face exactly the same problems, from the perspective of an Egalitarian naturalist, that an FBP style Platonism faces. Specifically, given the meager ontological commitments of PDR — to mathematical domains that are strongly metaphysically dependent on mathematical practices — a neo-fregean Platonism's robust ontological commitments — to mathematical domains that are weakly metaphysically independent of social practices — would need to be offset by their yielding an explanatory or justificatory advantage over those of PDR. Yet, once again, unless some naturalistically acceptable explanation of these domains influencing mathematicians can be provided, they are incapable of yielding any such explanatory or justificatory advantage. Indeed, given that, according to neo-fregeans, these domains lack their ontological structure independently of mathematical practices, it isn't clear that they have any features that could influence mathematicians in the way in which a radical Platonist wanted them to. Furthermore, what is true of FBP style Platonisms and neo-fregean Platonisms is true of all forms of Platonism that reject CYF. Thus, what the above discussion has established is that *the availability of PDR as a legitimate philosophy of mathematics, together with the epistemological argument against Platonism, rules out all varieties of Platonism as philosophies of mathematics that should be endorsed by Egalitarian naturalists.*

The above discussions also point towards an independent advantage that PDR has over Platonism. According to a growing consensus among philosophers of mathematics, the real explanations of, and, if they are available, justifications of, our mathematical practices and beliefs, even with respect to the metaphysical status of mathematical domains, are to be found in causal historical details concerning the development of those practices and beliefs. This growing opinion was apparent in my earlier discussions of Maddy's work on set theory (cf. Section 3.4) and Balaguer's FBP (cf. Section 4.3), and is the result of a growing hermeneutic project in the philosophy of mathematics. Platonists seek to combine this recognition with a belief that pure mathematical statements and beliefs are about Platonistically construed domains. They thus take the ultimate ground for some group of explanations and justifications of some practices to reside in one realm, yet at the same time insist that that in virtue of which these statements and beliefs are true is another realm.

If correct, this would be a most peculiar state of affairs. In contrast to Platonists, PDRists take the realms responsible for these two features of mathematical practices and the beliefs embodied within those practices to coincide. This seems to me to be significantly preferable to the situation countenanced by Platonists. It thus provides independent evidence for the superiority of PDR to any form of Platonism.[95]

4.6 An Overview of My Primary Argument Against Platonism

The preceding discussion is long and intricate. Consequently, I think that it will be useful for me to provide a short overview of it in the form of a condensed argument for the superiority of PDR to Platonism. My argument for this thesis is actually very simple:

> If PDR is a legitimate alternative to Platonism, then Platonistically construed mathematical domains are explanatorily and justificationally superfluous.
>
> PDR is a legitimate alternative to Platonism.
>
> Therefore, Platonistically construed mathematical domains are explanatorily and justificationally superfluous.
>
> Therefore, we should not accept the existence of Platonistically construed mathematical domains.

Let me make some observations about this argument. First, the conclusion follows from the intermediate conclusion by means of an application of Occam's razor — don't multiply types of entities without necessity. The idea is that if Platonistically construed mathematical domains are explanatorily and justificationally superfluous, then they serve no necessary purpose.

[95]We shall see in Chapter 6 that PDR has further explanatory advantages over Platonism.

Those familiar with mathematical practices might be wary of applying this principle to mathematical domains. Mathematics is not governed by Occam's razor. Rather, it is an underlying methodological feature of many mathematical practices that one should seek maximal generality, which, particularly in foundational areas such as set theory and category theory, can result in the characterization of — and thus, according to the PDRist, constitution of — ever larger mathematical domains.

There is no problem here, however, because my application of Occam's razor is not internal to some mathematical practice, but rather takes place within the practice of naturalistic metaphysics, i.e., metaphysics guided by the methodological practices of natural scientists. Occam's razor is a legitimate tool within this practice, because it is a legitimate tool within the non-mathematical aspects of natural science.

Further, I take it to be a benefit of PDR that it predicts this methodological difference between the mathematical and non-mathematical aspects of natural science. If mathematical domains are pure constitutive social constructs, then Occam's razor governs mathematics if and only if it governs the practices that constitute pure constitutive social constructs. Are the practices that constituted pure constitutive social constructs governed by Occam's razor? No! Consider for a moment the collection of legal statutes of the United States of America. This collection most certainly lacks theoretical elegance and simplicity. Without doubt, the system of law embodied in this collection could be represented in a simpler and theoretically more elegant way by a collection of statutes with fewer members than there are in the actual collection. If Occam's razor governed legislative activities, then we would claim that there are exactly as many legal statutes in the USA as there are in the most theoretically elegant systematization of the laws of the USA. We make no such claim, however. Rather we claim that the number of legal statutes in existence in the USA is exactly the number of legal statutes constituted by legislative activities in the USA. That number is, at least roughly speaking, the number felt necessary in order for the legal statutes of the USA to serve the social functions for which they are constituted. So, the proposal that mathematical domains are pure constitutive social constructs should bring with it two predictions: first, that Occam's razor does not govern mathematical practices, and second, that the number of mathematical domains that in fact exist is linked with the purposes for which mathematical domains are constituted. Both predictions are accurate.

Let me now consider the premise of my argument, viz., if PDR is a legitimate alternative to Platonism, then Platonistically construed mathematical domains are explanatorily and justificationally superfluous. The thesis that there is no naturalistically acceptable sense in which Platonistically construed mathematical domains can be thought to influence mathematicians and their practices is central to the justification of this premise. In Chapter 3, I provided an extensive discussion of the epistemological argument against Platonism in order to make it clear that this thesis is true. Yet my premise requires further justification than is provided by this thesis, for it is perhaps possible for Platonistically construed mathematical domains to play some kind of explanatory or justificatory role without influencing mathematicians and their practices.

Indeed, this belief has been embedded in a number of recent arguments for Platonism. For example, the existence of Platonistically construed mathematical domains has been argued to be required in order for mathematical statements to have the truth-value ascribed to them by mathematicians. Also, the existence of Platonistically construed mathematical domains has been considered necessary for providing mathematics with a semantics that resembles the semantics of everyday discourses sufficiently closely to account for the way in which these two types of discourses are intermingled.[96]

Accepting the legitimacy of PDR seriously undermines both of these reasons for postulating Platonistically construed mathematical domains, however. First, PDR takes mathematical statements to have the truth-value ascribed to them by mathematicians. Second, since pure constitutive social constructs are among the entities talked about using everyday discourses, an adequate semantics for everyday discourses must be able to accommodate them.

Perhaps there are other explanatory or justificatory benefits that Platonistically construed mathematical domains might yield. The most natural suggestion would be that they are indispensable to an account of the objectivity of mathematics, but I do not believe that this is the case, for I believe that a PDRist has the resources to provide such an account.

In fact, it is difficult to see what work Platonistically construed mathematical domains can do that the mathematical domains countenanced by PDR cannot do. And, unless some such work can be found — indeed, a fairly significant amount of such work can be found — we should not countenance Platonistically construed mathematical domains, for to do so would be to multiply types of entities without necessity. This concludes my discussion of Platonism. Let us next turn to Fictionalism.

[96]See (Benacerraf, 1973) on both accounts.

PART III

EGALITARIAN NATURALISM VS. FICTIONALISM

Introduction and Overview

Recall from the Dissertation Preliminaries that, because of certain similarities between PDR and neo-fregeanism,[97] I am structuring this dissertation — at least to some extent — around natural questions that arise concerning neo-fregeanism. In Part I, I addressed questions relating to metaphysics. Specifically, I gave detailed metaphysical accounts of Platonism, neo-fregeanism, and PDR. And in Part II, I offered an extended argument that, by the standards of an Egalitarian naturalist,[98] practice-dependent metaphysics is superior to Platonistic metaphysics, whether of a traditional or neo-fregean nature.

The other natural group of questions that I identified in the Dissertation Preliminaries related to the explanatory inversion envisioned by neo-fregeanism and PDR. Traditionally, Platonistic accounts of mathematical domains have been taken to provide some sort of metaphysical grounding for mathematical practices. The explanatory inversion that is characteristic of PDR excludes mathematical domains from playing this kind of role. In light of this, it seems legitimate to ask questions like the following: if mathematical practices don't exist to represent accurately mathematical domains that are metaphysically independent of all social practices, then why do we

[97]Recall that neo-fregeanism is the metaphysical account of the relationship between discursive practices and metaphysical domains suggested — at least to Fraser MacBride (cf. (MacBride, 2003)) — by Bob Hale's and Crispin Wright's Neo-Fregean logicism. For further details concerning neo-fregeanism see Section 1.3.

[98]See Section 0.2 for a detailed discussion of Egalitarian naturalism. Two theses are particularly important to it: 1) the mathematical and non-mathematical sciences use different methodologies and accept different standards of justification; and 2) mathematical and non-mathematical scientists deserve equal methodological respect from a methodological naturalist because their methodological practices constitute a unified methodological approach to the natural scientific understanding of the world.

have such practices? Further, why do mathematical practices take the form that they do? And, what role do mathematical practices play in our cognitive economy?

(Mathematical) **Fictionalism** is the thesis that existential pure mathematical assertions should be understood in a fictional/metaphorical/figurative, i.e. some kind of non-literal, way. Throughout the remainder of this dissertation I shall simply use 'non-literal' to cover all interpretations in this group unless it is important for me to be explicit about which specific interpretation I have in mind. As mentioned in the Dissertation Preliminaries, it is to one particular variety of Fictionalism that I turn for the inspiration for my answers to many questions of the type just mentioned. Specifically, I turn to the Fictionalism that has recently been articulated and defended by Stephen Yablo (cf. (Yablo, 2002a), (Yablo, 2002b), and (Yablo, ToAp); yet see also (Yablo, 2000)).

There are two well-known forms of Fictionalism advocated in the contemporary philosophy of mathematics literature. The best known is Hartry Field's Fictionalism (cf. (Field, 1980) and (Field, 1989)). Field's Fictionalism is a variety of Mathematical Nominalism, from now on simply Nominalism. **Nominalists** maintain that *there are no mathematical domains*. The second popular form of Fictionalism advocated in the contemporary literature is Yablo's Hermeneutic Fictionalism. It is this Fictionalism that we shall be particularly interested in. While many take Yablo's Hermeneutic Fictionalism to be a variety of Nominalism, Yablo invariably draws a weaker conclusion from his arguments than the conclusion that mathematical domains do not exist. He concludes that mathematical domains *need not* exist in order for mathematical practices to play the role that they do in our cognitive economy. Thus, strictly speaking, Yablo remains neutral on the issue of whether mathematical domains in fact exist.

To my mind, Yablo's Fictionalism, though at present still in need of further explication, is superior to Field's. First, Field's earlier, and at least at present more influential, Fictionalism is premised on a commitment that the Quine-Putnam indispensability argument[99] would be a, and indeed the only, successful argument for Platonism were the major premise of that argument true. From this, Field infers that all that one needs to do in order to establish Nominalism is show that mathematics is dispensable to science. I take a commitment of this type to the indispensability argument to be a mistake. I have offered an argument for this claim in (Cole, 2001). Second, I take Yablo to be correct in his belief that representational usefulness is far more central to mathematics than deductive usefulness.[100] Further, in (Yablo, ToAp), Yablo offers convincing arguments that indispensability is a red-herring for Fictionalist philosophers of mathematics and offers an alternative, and far more convincing, argument in favor of Fictionalism.[101] Third, I believe that Yablo's arguments for his variety of Fictionalism are also the essence of the best argument available for Nominalistic Fictionalism. Of course, in order to establish Nominalistic Fictionalism, Yablo's arguments need to be combined with an argument for the conclusion that if mathematical domains need not exist in order for mathematical practices to be able to play the role that they do in our cognitive economy, then mathematical domains do not exist. Yet anyone who takes Nominalistic Fictionalism seriously should be able to provide such an argument.

Part III of this dissertation has two purposes: first, *to provide a reasonably detailed account of Yablo's answers to the above mentioned questions and several others, for*

[99]This argument can be found in (Putnam, 1971) and is discussed in (Colyvan, 2001).

[100]See Chapter 5 for more details concerning this claim.

[101]Indispensability arguments are also criticized by Yablo in (Yablo, 2000).

these, or slight modifications of these, are the answers that I want to offer in answer to these questions; second, *to argue that an Egalitarian naturalist should not advocate any form of Fictionalism.*

The argument that Fictionalism should not be advocated by an Egalitarian naturalist is somewhat easier to make than the argument that Platonism should not be advocated by an Egalitarian naturalist. In essence, it is the following: Fictionalists maintain that when mathematicians make existential pure mathematical assertions they are not speaking literally. By contrast, all Egalitarian naturalists should hold that mathematicians are speaking perfectly literally when they make such existential pure mathematical assertions.

I only wish that I were done. Yet there is considerable controversy surrounding the second of these claims, i.e., the claim that mathematicians are speaking perfectly literally when they make existential pure mathematical assertions. It is even more controversial to maintain that not only do they speak literally, but, in addition, they should be understood in a face-value way, i.e., as speaking about mathematical domains.[102] Yet this, too, is something that all Egalitarian naturalists should maintain.

I have now used two terminological distinctions: literal vs. non-literal and face-value vs. non-face-value. In order to clarify these distinctions, it is perhaps best to relate them to accounts of mathematics with which the reader should be familiar. Contemporary Nominalism has two modern faces. One of them promotes Nominalism on the basis of accepting the thesis that mathematicians' existential pure mathematical assertions should not be understood in a literal way. Nominalists of this variety suggest that if mathematicians were to be speaking literally when making existential

[102]See the next paragraph for details concerning the distinction between using expressions in face-value and literal ways.

pure mathematical assertions, they would be ontologically committed to *mathematical* domains, yet insist that mathematicians do not speak literally when making these assertions. These Nominalists insist that when mathematicians make existential pure mathematical assertions, they should be understood as speaking non-literally. Let us call this variety of Nominalism **non-literal Nominalism**. The best known proponent of non-literal Nominalism is, or more precisely was, Hartry Field (cf. (Field, 1980) and (Field, 1989)). The other variety of Nominalist accepts that mathematicians' assertions are, at least for the most part, literal assertions, but claims that that in virtue of which these literal assertions are (objectively) true or false is not mathematical domains. Rather, mathematicians' assertions are (objectively) true or false in virtue of certain modal facts — let us call this variety of Nominalism **Modal Nominalism**. The best known proponents of Modal Nominalism are Charles Chihara[103] (cf. (Chihara, 1990)) and Geoffrey Hellman (cf. (Hellman, 1989)).

Throughout the following, when I distinguish between literal and non-literal use of an expression, I shall be referring to the distinction, invoked by non-literal Nominalists, between speaking fictionally/metaphorically/figuratively vs. speaking austerely. If I want to distinguish between an expression being about what it looks on the face of it to be about, as opposed to some other domain, as the Modal Nominalist does, I shall use the distinction face-value vs. non-face-value. Thus, non-literal Nominalists (and, in general, Fictionalists) interpret mathematics in a face-value yet non-literal

[103]In fact, in (Chihara, 2004), Chihara claims that his account of mathematics should merely be interpreted as an answer to the Quine-Putnam indispensability argument, not as an interpretation of actual mathematical practices. Unfortunately, the fact that I am unable to provide a detailed discussion of Modal Nominalism in this dissertation prevents me from fully exploring this subtle point.

way, while Modal Nominalists interpret mathematics in a literal yet non-face-value way.

Nobody, from any camp, questions that mathematicians make a range of claims that, if taken literally and at face value, incur ontological commitments to mathematical domains. For example, any number theorist, and many well educated high-school students, will tell you that "there exist infinitely many prime numbers." The issue raised by Fictionalists and Modal Nominalists is whether or not mathematicians should be understood as speaking in a face-value and literal way when they make such existential claims. Fictionalists (and Modal Nominalists) maintain that they should not.

My main argument for the thesis that Fictionalism should not be advocated by an Egalitarian naturalist can be found in Section 6.3. There I argue that an Egalitarian naturalist should hold that mathematicians are speaking perfectly literally when they make existential pure mathematical assertions. Supplementary arguments for this thesis and the thesis that it is difficult to draw the distinction between speaking literally and non-literally when it comes to existential pure mathematical statements can be found in Sections 6.4 and 6.5. It should be noted that the argument of Section 6.3 will go some way towards establishing that Modal Nominalism should not be advocated by an Egalitarian naturalist. Unfortunately, I cannot provide Modal Nominalism with the detailed discussion that it deserves in this dissertation.

In Chapter 5, I shall provide a reasonably detailed exposition of Yablo's Hermeneutic Fictionalism. This exposition will serve two purposes. First, it will provide a statement of what I take to be the most plausible form of Fictionalism, which in turn will be a good foil for my criticisms of Fictionalism in Chapter 6. Yet the second, and perhaps more important, purpose is the one mentioned above. Yablo's Hermeneutic Fictionalism offers answers to a range of important questions, which PDRists can use in defense of their own interpretation of mathematics.

CHAPTER 5

YABLO'S HERMENEUTIC FICTIONALISM

5.1 Overview

In this chapter I shall discuss Stephen Yablo's recent work on Hermeneutic Fictionalism (cf. (Yablo, 2002a), (Yablo, 2002b), and (Yablo, ToAp)). I take this work to constitute the most plausible form of Fictionalism. Thus, this work will serve as a good foil to use in arguing that Fictionalism should not be advocated by an Egalitarian naturalist — the project of Chapter 6.

The specific structure of this chapter will be the following. In Section 5.2, I shall introduce Yablo's notion of a representational aid. In Section 5.3, I shall discuss how Yablo puts the notion of a representational aid to use in his Fictionalist philosophy of mathematics. In Section 5.4, I shall provide one part of Yablo's argument in favor of his Fictionalism. The other part of Yablo's argument in favor of his Fictionalism is its explanatory advantages over Platonism. In Section 5.6, I shall briefly outline these explanatory advantages and their availability to a PDRist. Yet, before doing this, in Section 5.5, I shall be explicit about Yablo's answers to the questions that Part III of this dissertation is designed to answer.

5.2 Representational Aids

Consider the everyday expression, “I’ve got butterflies in my stomach,” and the related expression, “I’ve got stomach-butterflies.” On nearly all occasions when these expressions get used in everyday life, the speaker is not speaking literally.[104] That is, it is not that the speaker has just swallowed some butterflies. And it is not that there is a special type of butterfly — a stomach-butterfly — that the speaker is related to in a special way. The speaker uses these expressions to express the content that her gastric system is in a particular type of state. Yablo would call this the real content of these expressions. Roughly speaking, at least for everyday non-literal expressions, the **real content** of an expression is the real world condition that makes it appropriate to assert the expression in question.

Undoubtedly, the real content of this pair of expressions could be represented in a literal way by most competent speakers of (American) English, e.g., “I am so nervous that I have sensations in my stomach that are like those that I imagine I would have were I to swallow a butterfly.” Yet this literal expression of the real content of this pair of expressions is cumbersome. Faced with a frequent desire to express this content, the community of (American) English speakers found an alternative — non-literal and far less cumbersome — way of expressing it; we talk about stomach-butterflies, or having butterflies in our stomachs. In Yablo’s terminology, the community of (American) English speakers invented the stomach-butterfly as a representational aid. In general, communities of speakers invent representational aids for a variety of reasons. For example, it could be that many members of the community are unfamiliar with the

[104]Yablo usually claims that they are speaking figuratively or metaphorically, rather than non-literally. As stated in the introduction to Part III, I shall use non-literally to cover this whole family of notions.

discourse that could be used to express the real content in question literally, it could be a matter of members of the community not having the time, or wanting to take the time, to express the real content in question literally, or it could be that the community lacks a way of expressing the real content in question literally. At one point, Yablo even suggests that it could be simply a matter of the literal expression being boring.

Here is one of Yablo's brief explications of the notion of a representational aid:

> Stomach-butterflies and the rest are *representational aids.* They are "things" that we advert to not (not at first, anyway) out of any interest in what they are like in themselves, but because of the help they give us in describing other things. Their importance lies in the way they boost the language's expressive power. (Yablo, 2002a, p. 229)

So, what is Yablo's notion of a representational aid? Let us suppose that there is a community that has in place a representational practice that has the ability to express a certain, perhaps limited, range of contents, e.g., our community, or an earlier time-slice of our community. Roughly speaking, a **representational aid** is a "thing", where this could be an "object", a "property", a "relation", or a whole "domain of objects, properties, and relations,"[105] such that the use of an expression that refers to, or denotes, that "thing" helps that community to represent contents that their present practice of representation is either unable to express or only able to express in a cumbersome manner. In his writings, Yablo places scare-quotes around the "things" that are representational aids, because he maintains that when a community uses terms referring to or denoting "things" that serve this purely representational purpose

[105]Yablo is primarily interested in "objects" rather than "properties" or "relations" when it comes to the representational aids that he uses as examples. I believe however, that this is an artifact of a common practice within the philosophy of mathematics of talking more about objects than properties and relations.

in their representational practice, they should be understood as speaking non-literally, where what this amounts to is that they need not take on the face-value ontological commitments of the use of the expressions in question. For convenience, I shall desist from following Yablo's use of scare-quotes. The reader should note that *what Yablo is claiming should be understood non-literally is our existential commitments to representational aids.*

Other examples of representational aids that the community of (American) English speakers uses are easy to find. Consider the following collection of expressions taken from Yablo's work — some of which I have modified:

- Pinpricks of conscience register less than pangs of conscience.
- The back-burner is where things are left to simmer.
- The invisible hand operates all by itself.
- He is attached to his mother by very short apron-strings.
- She's got a real chip on her shoulder.
- The real-estate bug doesn't sting, it bites.

Pinpricks of conscience, pangs of conscience, the back-burner, the simmering of things on the back-burner, the invisible hand, a mother's apron-strings, the chip on her shoulder and the real-estate bug are all representational aids. These representational aids were introduced by earlier time-slices of the community of (American) English speakers to provide a non-literal means for expressing the real content of these expressions. It seems clear that, for all of these examples, Yablo is correct; in normal situations, when individuals from the community of (American) English speakers use these expressions, they are speaking non-literally and do not need to be ontologically committed to the representational aids that they invoke.

5.3 Mathematics as a collection of Representational Aids

Fictionalism, and with it non-literal Nominalism, was introduced into the most recent round of the debate between Nominalists and Platonists by Hartry Field (cf. (Field, 1980)). The central conceptual breakthrough contained in Field's *Science Without Numbers* (Field, 1980) is the thought that mathematics could be useful for scientific purposes without being true,[106] specifically, without mathematical domains being "real existents".[107] This is the idea behind Field's development of the thesis that mathematical discourses are (semantically) conservative[108] over nominalistic discourses,[109] and his discussion of the deductive usefulness of adopting a scientific language that includes mathematical discourses in addition to nominalistic discourses. In (Field, 1980), Field considers the argument:

1. There are exactly twenty-one aardvarks;

2. On each aardvark there are exactly three bugs;

3. Each bug is on exactly one aardvark; so

[106]The only exceptions to this are mathematical statements that are vacuously true in virtue of their logical form.

[107]In the next section I'll address the issue of how this notion should be understood when used by Field and Yablo.

[108]Roughly speaking, discourse X is semantically conservative over discourse Y if adding X to Y does not result in new (semantic) consequences of sentences in Y.

[109]A nominalistic discourse is one that has no terms that refer to, or denote, abstract entities, properties or relations.

4. There are exactly sixty-three bugs.

The conclusion Field draws from this consideration is the following: if you reason within a first-order nominalistic language with numerical quantifiers, "the inference needed for getting from the premises to the conclusion is long and tedious. (Though not nearly as bad as it would have been if we hadn't introduced the numerical quantifiers!)" (Field, 1980, p. 22). Alternatively, if you include the pragmatic resources of a discourse that treats numbers as objects in your derivation, there is a relatively short proof that only requires you to avail yourself of three theorems of arithmetic. This feature of being able to significantly reduce the length of the proof required for an argument (particularly when all of its premises and its conclusion can be stated using the representational resources of a (first-order) nominalistic language) by introducing mathematical discourses that quantify over the constituents of mathematical domains is quite general. It is this feature that philosophers of mathematics refer to as the **deductive usefulness** of mathematics.

The insight that mathematics could be pragmatically useful without being true also plays a major role in Yablo's Hermeneutic Fictionalism. For Yablo, however, the central ingredient of this pragmatic usefulness is not deductive usefulness, but rather representational usefulness. The **central thesis of Yablo's Hermeneutic Fictionalism** is that *mathematical domains are representational aids.* Yablo illustrates this thesis with a variety of examples. Consider a representational practice with the expressive resources of a first-order nominalistic language containing numerical quantifiers. Suppose that participants in this practice wanted to express the content "there are exactly as many Fs as Gs," where F and G are taken from the nominalistic language. It is impossible for the participants of this practice to express this content with their present expressive resources. Yet if the domain of natural numbers is added to their practice as a representational aid, then participants in this practice can simply assert that, "the number of Fs is equal to the number of Gs." Of course, this is not their only option for increasing their expressive resources, they could also express this content by introducing a device that allowed them to express infinite disjunctions — "$(\exists_0 xFx \& \exists_0 xGx) \vee (\exists_1 xFx \& \exists_1 xGx) \vee$ etc." Or they could introduce second-order resources and capture this content as Frege did in his *Grundlagen* (Frege, 1884).[110]

A second illustration of Yablo's central thesis can be found in the discussion that Yablo uses to introduce the suggestion that mathematical domains are representational aids in (Yablo, ToAp):

> The psychiatrist need not believe in libido or ego strength to derive representational advantage from them. Why should the physicist have to believe in numbers to access new contents by couching her theory in numerical terms?
>
> Suppose that our physicist is studying escape velocity. She discovers the factors that determine escape velocity and wants to record her results. She knows a great many facts of the following form:
>
> > (A) a projectile fired at so many meters per second from the surface of a planetary sphere so many kilograms in mass and so many meters in diameter will (will not) escape its gravitational field
>
> There are problems if she tries to record these facts without quantifying over mathematical objects, that is, using just numerical adjectives. One is that, since velocities range along a continuum, she will have to write uncountably many sentences, employing an uncountable number of numerical adjectives. Second, almost all reals are "random" in the sense of encoding an irreducibly infinite amount of information. So, unless we think there is room in English for uncountably many semantic primitives, almost all of the uncountably many sentences will have to be infinite in length. At this point someone is likely to ask why we don't drop the numerical-adjective idea and say simply that:

[110]This example is discussed by Yablo in Section 9 of (Yablo, ToAp).

> (B) for all positive real numbers M and R, the escape velocity from a sphere of mass M and diameter 2R is the square root of 2GM/R, where G is the gravitational constant.
>
> Why not, indeed? To express the infinitely many facts in finite compass, we bring in numbers as representational aids. We do this despite the fact that what we are trying to get across has nothing to do with numbers, and could be expressed without them were it not for the requirements of a finitely based notation. (Yablo, ToAp, Section 6)

We saw in Section 5.2 that the purpose of using representational aids is to allow us to express real contents in a non-literal way. Given this, if mathematical domains are representational aids, then what real contents do they help us express when used in applied mathematical statements? The answer, of course, depends on which mathematical domain is invoked. Yet the general characterization of the real content of an applied mathematical statement is the following: it is the real world condition that makes the applied mathematical statement appropriate to assert. So, for instance, the real content of "the number of fish is even" is there are evenly many fish. The real content of "the number of planets is nine" is there are nine planets. The real content of "the number of Fs is the same as the number of Gs" is there are exactly as many Fs as Gs.

Given the central thesis of Yablo's Hermeneutic Fictionalism, one might believe that he maintains that all uses of mathematical items are uses of them as representational aids. This is not so. Yablo acknowledges that in addition to serving in a representational role, mathematical domains can serve as the things represented in a given assertion or belief. One of his examples of this is "the number of natural numbers is $\aleph_0$." In this example, the natural numbers serve as the things represented, $\aleph_0$ in the representational role characteristic of representational aids.

The recognition that not all uses of mathematical expressions are uses of mathematical items as representational aids is central to Yablo's account of the distinction between pure and applied mathematics (cf. Sections 9 and 10 of (Yablo, ToAp)). Having introduced a mathematical discourse including terms referring to, or denoting, items that serve as representational aids, Yablo suggests that it would only be natural for the community that introduced this discourse to take an interest in its domain quite independently of its ability to serve as a representational aid. Thus, its study as a domain of pure mathematics would be born. Why would it be natural for that community to take this interest? Well, precisely because of the important pragmatic purposes that that domain would then be playing for that community.

In fact, the situation is even more complex than the above indicates in that domains introduced at the cutting edge of contemporary pure mathematical research might not be introduced as representational aids at all. Yablo would maintain, however, that these mathematical domains do not serve as counter-examples to his thesis that the real nature of mathematical domains is that of being representational aids. The situation at the cutting edge of pure mathematics is rather a side-effect of the practice of mathematical representational aids serving as things represented, i.e., pure mathematics, having taken on a life of its own.[111]

While Yablo certainly emphasizes the representational usefulness of mathematical domains above other types of pragmatic usefulness, he most certainly does not want to deny that mathematical domains serve other useful pragmatic purposes. Among these he would acknowledge Field's claim that they are deductively useful. He also offers the following suggestion:

[111]If the reader is interested in other instances of representational aids, then note that "life of its own" serves as a non-mathematical representational aid in this sentence.

> Representational usefulness will be the focus in what follows. But I don't want to give the impression that the possibilities end there. Another way that numbers appear to "help" is by redistributing theoretical content in a way that streamlines theory revision. (Yablo, ToAp, Footnote 15)

The idea here seems to be that by representing certain contents using a discourse that includes extra representational resources in the form of mathematical domains, a scientist will be more readily able to explore various possible modes of theory revision.

Yablo illustrates his point about theory revision with the following example. Consider a community of individuals that uses a representational practice that only has the resources of a first-order nominalistic language with numerical quantifiers. Suppose that they wish to change their theory of Z-particles so that instead of that theory claiming that there are between two and three quarks in each Z-particle, it claims that there are between two and four quarks in each Z-particle.[112] First, note that both of these claims about the number of quarks in each Z-particle can be expressed in a first-order nominalistic language containing numerical quantifiers. Then compare the level of representational revision that you would be required to engage in to make this revision if you can express contents using the domain of natural numbers as a representational aid with the amount of representational revision required without this domain serving as a representational aid. With the numerical representational aids in place it is a simple matter of one correction. Without these representational aids you need a new quantifier, two new non-identities, and two new identities. Clearly, it is representationally much more convenient to have the numerical representational aids available. Yablo's suggestion seems to be that, in virtue of this fact, and presumably

[112] Yablo's discussion of this example can be found in Footnote 15 of (Yablo, ToAp).

some relationship between the ease with which you can think contents and can represent them in your representational practices, this representational convenience will actually translate into more streamlined theory revision.

5.4 Yablo's Argument for the Central Thesis of his Fictionalism

Yablo maintains that the types of useful function that numerical representational aids can serve all[113] provide us with "important non-evidential reason[s] for making as if to believe in numbers" (Yablo, ToAp, Section 5). In this quote, 'making as if' seems to amount to using them in the non-literal expression of real contents concerning the spatio-temporal world. The important question is: why only take them as reasons for 'making as if' to believe in numbers, rather than as reasons to believe in numbers? The straightforward answer from Yablo is that their serving these useful functions doesn't require natural numbers "really" to exist. As Yablo remarks:

> The deductive advantages that "real" Xs do, or would, confer are (Field tells us) equally conferred by Xs that are just "supposed" to exist. But the same would *appear* to apply to the *representational* advantages conferred by Xs; these advantages don't appear to depend on the Xs really existing either. (Yablo, ToAp, Section 6)

Before we can continue, we must determine what Yablo means by a "real" existent. I claim that the best interpretation of Yablo's (and Field's) talk of Xs being "real" existents is: Xs exist and the existence of Xs is metaphysically independent of all social practices. Recall from Chapter 1 that this means that Xs exist and, at a minimum, the presence of social practices is not a necessary condition on the existence of Xs.

[113] In fact, he is only explicit about representational and deductive usefulness. Yet I take it that he would extend this claim to pragmatic usefulness in theory revision.

The evidence in favor of this interpretation comes, primarily, from two sources. First, the mainstream tradition in philosophy of mathematics at the moment is one that takes the existence of mathematical domains to be the existence of domains that are metaphysically independent of all social practices. And, most likely, if Yablo (or Field) had had in mind something different from what the mainstream tradition would have in mind when hearing "real" existent, then he (they) would have been explicit about what he (they) meant by "real" existent.

It should be noted, however, that, at least in the philosophy of mathematics, the distinction between existence of domains that are metaphysically independent of social practices and existence of domains that are metaphysically dependent on social practices is not one that is common currency. Thus, this reading of the state of affairs in the philosophy of mathematics is controversial. I believe, however, that it can be justified by considering contemporary attitudes towards Brouwer-Heyting style intuitionism, according to which mathematical domains are mental constructions.

The second source of evidence for my interpretation of what "real" existence amounts to is, in light of the last paragraph, perhaps the more important. It is that charity demands that Yablo (and Field) be understood in this way, for understanding "real" existence as the existence of items that are metaphysically independent of social practices is the best way to have many of Yablo's (and Field's) claims come out true. Most importantly, it is required in order to make Yablo's (and Field's) claims that mathematical domains need not be (are not) "real" existents come out true. If an X gets to be a "real" existent simply by existing, where X's existence can be either metaphysically independent of social practices or constitutively socially constructed[114] by some social practice, then these claims turn out to be false. For if, as I have been arguing, PDR is true, then the use of a (coherent) representational practice including a mathematical domain as a representational aid to express real contents about the spatio-temporal world is enough to ensure the existence of the mathematical domain in question as an item constitutively socially constructed by that practice.

This observation is going to be very important in the remainder of Part III, so let me repeat it. If items whose existence is constitutively socially constructed by social practices were to be included as "real" existents, then all claims that mathematical items need not be "real" existents in order to serve pragmatic functions (such as deductive usefulness and representational usefulness) are false.

Any commitment to Fictionalism, and indeed to non-literal Nominalism, relies on it being possible to make sense of the distinction between using existential pure mathematical statements in a literal and in a non-literal way. Unfortunately, neither Yablo nor Field provides a clear characterization of this distinction. They both rely on our being able to make it in certain clear-cut cases. The last paragraph indicates that the best interpretation of Yablo and Field on how to draw this distinction takes them both to believe that for an existential pure mathematical statement to be (literally) true is for there to be a mathematical domain whose existence is metaphysically independent of social practices. By contrast, when an existential pure mathematical statement is used in a non-literal way, Yablo maintains that it need not have any such ontological implications, while Field maintains that it does not have any such ontological implications.

[114]See Section 1.1 for details about what constitutive social construction is.

Now that we are clear on what Yablo and Field take a "real" existent to be, let us return to Yablo's argument. The justification that Yablo provides for the claim that the deductive and representational advantages of using Xs as representational aids don't depend on the Xs "really" existing comes while Yablo is talking about claim (B), found in the long quote in Section 5.3. He tells us:

> That (B) succeeds in gathering together into a single content infinitely many facts of form (A) owes nothing whatever to the real existence of numbers. It is enough that *we understand what (B) asks of the non-numerical world*, the numerical world taken momentarily for granted. How the real existence of numbers could help or hinder that understanding is difficult to imagine. (Yablo, ToAp, Section 6)

The idea here is that all you need is a representational framework that will provide the resources to represent things about the spatio-temporal world that can't be represented using a representational practice with the expressive resources of a first-order nominalistic language containing only numerical quantifiers. Such a framework can both exist and be used as an aid to representation without any of the objects, properties, and relations that the system seems to invoke being "real"[115] existents, i.e., having their existence independently of social practices. After all, as the discussion in Section 5.2 illustrated, we constantly use representational aids to which we need have no ontological commitment to help us represent how the world is.

Yablo would, undoubtedly, extend his thought that mathematical domains need not exist to include their use in the redistribution of theoretical content in a way that is beneficial to theory revision. In particular, I suggest that he would maintain something like the following: because of the representational nature of the redistribution of theoretical content, the "real" existence of mathematical domains is not relevant

[115] This qualifier is important here. If it were just a matter of them existing rather than existing independently of social practices, then Yablo's claim would be false.

to their ability to serve the useful purpose of helping us to redistribute theoretical content in a way that is beneficial to theory revision. Essentially, the idea is that it is the representational resources made available by using mathematical domains as representational aids that are exploited in the redistribution of theoretical content, not the "really" existing domains themselves. Thus, these domains need not "really"[116] exist in order for them to serve this function.

5.5 Interlude

Before preceding to discuss the second half of Yablo's argument in favor of his Fictionalism, i.e., his Fictionalism's explanatory advantages over Platonism, let us briefly make explicit the answers that a defender of Yablo's Fictionalism or PDR can offer to the questions that motivate this part of this dissertation. Recall that those questions were questions of the following type: if mathematical practices don't exist to represent accurately mathematical domains that are metaphysically independent of all social practices, then why do we have such practices? Further, why do mathematical practices take the form that they do? And, what role do mathematical practices play in our cognitive economy?

Yablo's answer to the first question is that an earlier time-slice of many linguistic communities introduced mathematical discourses, and hence, according to PDRists, mathematical domains, for the purpose of more readily representing aspects of the non-mathematical world. Yablo's most detailed discussion of this answer can be found in (Yablo, ToAp, Section 12) in the form of his Myth of the Seven, which mimics Wilfrid Sellars' famous Myth of Jones (cf. (Sellars, 1956)).

[116]Once again, this qualifier is essential.

Let us consider a specific case. Perhaps the easiest case to consider is arithmetic. When looking around, it is easy to see that how the spatio-temporal world is determines a wide range of so-called cardinality facts. For example, the collection of planets in our solar system has the cardinality property of nine-ness, the collection of moons of Jupiter has the cardinality property of four-ness, and the collection of sisters of Julian Cole has the cardinality property of two-ness. Yablo suggests that earlier linguistic communities introduced the domain of natural numbers to help them represent these cardinality facts. They found it easier to represent these facts by associating an object with collections rather than a property. Most likely, it was easier for them, because they found it easier to reason with objects than with properties and relations.

Let us next consider the second question, i.e., why do mathematical practices take the form that they do? Well, suppose that we want to introduce a collection of objects to help us represent cardinality facts by means of associating an object with each collection that is such that the object represents the cardinality of the collection. Then, for each distinct cardinality property, we are going to need to have a distinct object. Further, given that it seems at least possible that there could be collections of arbitrarily large cardinality, we are going to need a potentially infinite collection of objects to do the job. Further, given our interest in how cardinality properties behave under collecting together and separating, we shall undoubtedly be interested in operations like addition and subtraction on these objects. In addition, the objects in question are going to have to respect logical relations between cardinality properties on pain of incoherence. So, serving the particular representational purpose that the domain of natural numbers was introduced to serve in the way in which that domain does serve that purpose places a wide range of constraints on our arithmetical discourses. In fact, in the case of arithmetic, logic alone forces us to accept nearly all of the truths that we do accept about natural numbers — this is, in essence, what Frege showed in his *Grundlagen* (Frege, 1884).

Further, our answers to the first two questions constitute at least a partial answer to the question concerning the role of mathematics in our cognitive economy. Mathematics plays a primarily representational role in our cognitive economy. Yet, as we saw earlier, Yablo — and PDRists — recognizes other purposes than the primary, representational purpose for mathematics.

5.6 The Explanatory Benefits of Yablo's Hermeneutic Fictionalism

With this brief interlude behind us, let us turn to the second half of Yablo's argument for his Fictionalism. In Section 12 of (Yablo, 2002b), Yablo discusses a number of explanatory benefits that his Hermeneutic Fictionalism has over Platonism. All concern the explanation of certain features of our, or mathematicians', practices. The list of explanatory benefits that he mentions includes insubstantiality, indeterminacy, representationality, necessity and apriority.[117] Let us (briefly) consider each of these in turn.

Mathematical items have a kind of thinness about them. There is nothing more to them than is entailed by the axioms and definitions that capture them. The best that any kind of Platonist can do in terms of explaining this insubstantiality is to attempt to provide some kind of metaphysical explanation of it. Yet, as I argued in Chapter 4, no form of Platonism espoused at present can, in a way acceptable to an

[117] A number of these are broached in a more general setting in (Yablo 2000).

Egalitarian naturalist, provide such explanations. Either the Platonism in question accepts CYF[118] and so owes an Egalitarian naturalist a naturalistically acceptable account of our epistemic access to the mathematical realm and its metaphysical features, or it rejects CYF and gains epistemic access to the mathematical realm at the cost of not being able to provide metaphysical explanations of features of mathematical practices. In contrast, if one recognizes that mathematical domains are invented or constructed for representational purposes, it is no surprise that we should not know more about them than is specified in the axioms that formalize the central elements of their construction. There is no reason to specify more than is needed for them to serve their representational purpose. Thus, the Fictionalist and PDRist can — at least potentially[119] — provide a fulfilling explanation of the insubstantiality of mathematical items.

Many contemporary philosophers of mathematics have devoted a lot of time to understanding why it is that identity statements involving mathematical singular terms are, or at least appear to be, less determinate than identity statements concerning everyday spatio-temporal objects. For instance, it is one of the major motivations behind (early) structuralism (see, e.g., (Benacerraf, 1965)), and it is at the heart of the so-called (Julius) Caesar problem (see, e.g., (MacBride, ToAp)). Yet specifying identity conditions between structures remains a sore spot for structuralists, and, at least according to (MacBride, ToAp), the Neo-Fregean logicists still have not provided a convincing answer to the (Julius) Caesar problem.

[118]See the opening of Part II.

[119]Obviously, the above only points in the direction of such an explanation rather than providing the explanation.

Once again, however, if mathematical domains are inventions or constructs, only as many of whose features are specified as are needed so that they can play their (primarily) representational role, then it is no surprise that identity conditions involving their constituents should be indeterminate. Thus, a Fictionalist or PDRist can — at least potentially — provide a fulfilling explanation of the indeterminacy of identity statements involving singular terms referring to mathematical entities.

A close investigation of the role of mathematics in science makes it clear that mathematical entities play a substantially different role in scientific explanations than do spatio-temporal entities. Spatio-temporal entities most frequently play what might be called a causal-explanatory role, while mathematical entities play what might be called a representational role. It is difficult to see what kind of explanation a Platonist can provide of this fact, while a Fictionalist or PDRist can — at least potentially — provide a fulfilling explanation of this fact. After all, according to Yablo's Hermeneutic Fictionalist, and (at least) this PDRist, aiding us in representing other things is the *raison d'etre* of mathematical discourses and hence, for a PDRist, of mathematical domains.

It should also be noted that the above is the start of an account of the applicability of mathematics, another problem that has plagued Platonists. If mathematical discourses play a primarily representation role in their applications within science, and mathematics' *raison d'etre* is to aid us in representing things, then it is no surprise that mathematical domains are applicable as representational tools within science.

Without doubt, the last two explanatory advantages listed above — necessity and apriority — are the most significant of the advantages mentioned by Yablo. Indeed, they are so important that (Yablo, 2002a) is almost entirely devoted to a discussion of them. We saw earlier that applied mathematical statements are taken by Yablo to express a real content in a non-literal way. Roughly speaking, they express the real-world condition that makes the statement in question appropriate to assert. The discussion in Section 5.5 implicitly suggested that Yablo's Fictionalist extends the notion of real content so that it, in addition, covers pure mathematical statements.[120] Sections 8 and 9 of (Yablo, 2002a) are devoted to arguing that the real contents of arithmetical statements and set-theoretic statements are logical truths. Arithmetic statements have as real contents logical truths about cardinality. Set-theoretic statements have as real contents logical truths of a combinatorial nature. Yablo conjectures that the real contents of all pure mathematical statements introduced for the representational purposes mentioned earlier are logical truths. He also argues that this is the reason why many pure mathematical statements feel necessary and a priori. Many pure mathematical statements feel necessary and a priori, because, at the level of real content, being logical truths, they are necessary and a priori. Once again, this is a much more fulfilling explanation than the, most likely unacceptable, metaphysical explanations that Platonists usually offer of these features of pure mathematical statements.[121]

5.7 Looking Forward

In this chapter, I have briefly outlined Stephen Yablo's Hermeneutic Fictionalism and its explanatory benefits over Platonism. This discussion is designed to serve two purposes. First, this discussion will provide a strong target for criticism in the next

[120] At least those pure mathematical statements referring to items initially introduced as representational aids.

[121] Recall from Part II that such explanations cannot be given or accepted by Egalitarian naturalists.

chapter. Second, this discussion points in the direction of explanatory benefits that PDR might have over Platonism. In the next chapter, I shall criticize, and consequently reject, Fictionalism as a philosophy of mathematics that should be espoused by an Egalitarian naturalist.

CHAPTER 6

A NATURALISTIC REJECTION OF MATHEMATICAL FICTIONALISM

6.1 Overview

Over fifty years ago, Rudolf Carnap asked a very important question: what exactly is at stake in debates between Platonists and Nominalists concerning some subject matter X? His answer was (cf. (Carnap, 1950)): nobody has made it clear. Sure, there are the slogans that we are all familiar with: Xs exist; Xs do not exist. Yet Carnap's investigation of the long history of debates between Platonists and Nominalists led him to believe that nobody on either side of these debates had made it clear what it would be for Xs to exist or to fail to exist.

In response to this situation, Carnap sought to distinguish internal and external varieties of the question "Do Xs exist?", and offered his infamous proposal that the best way to explicate the debate surrounding the external variety of this question is as a debate over the pragmatic value of adopting a certain discourse as a tool to use in navigating the world.

Whatever the reader's opinion of Carnap's positive proposal, I believe that (s)he should recognize at least this: Carnap asked an important question, a question that is still in need of a good answer. Despite this need, Carnap having asked this question has helped later generations of philosophers understand that they need to be precise in specifying the content of their various Platonistic/Realistic and Fictionalistic/Nominalistic claims. It was for this reason that I devoted Part I to addressing issues surrounding Platonistic and practice-dependent metaphysics, and it was for this reason that, in Chapter 5, I made the claim that any Fictionalist must be able to provide content to the distinction between speaking literally and speaking non-literally when making an existential pure mathematical assertion.

I don't believe that either Field or Yablo has done this. They have instead relied on our ability to easily classify certain paradigm cases as literal and non-literal. Yet, as I indicated in Chapter 5, their claims about "real" existence suggest that they would at least affirm the following about the distinction between speaking literally and speaking non-literally. To speak literally when you use an existential pure mathematical claim is to be committed to a Platonistically constructed mathematical domain, i.e., one that is abstract and metaphysically independent of social practices. To speak non-literally is, respectively, either to make no such ontological commitment or to accept that there is no need to make such a commitment.

I contend two things. First, there is no evidence that the majority of mathematicians' existential pure mathematical assertions should be understood non-literally. Indeed, it isn't clear to me that existential pure mathematical assertions have any systematic non-literal use. Second, when mathematicians make literal existential pure mathematical assertions — as they do in their everyday practices — there is no evidence that they are ontologically committed to Platonistically construed mathematical domains.

In Section 6.2, I shall point out that Yablo's methodology in his discussion of Hermeneutic Fictionalism seems to be the very methodology that Carnap sought to caution against in (Carnap, 1950). In Section 6.3, I shall argue that Egalitarian naturalists should take mathematicians to be speaking literally in their everyday practices of making existential pure mathematical assertions. Having established this conclusion, in Section 6.4, I shall briefly consider what sense can be made of the literal vs. non-literal distinction for existential pure mathematical assertions. In Section 6.5, I shall provide a diagnosis of the mistake made by Fictionalist philosophers of mathematics. Finally, in Section 6.6, I shall further the diagnostic work of Section 6.5 by bringing in considerations from Sally Haslanger's work on social construction (cf. (Haslanger, 1995)). My discussion of Haslanger should, in addition, aid the reader's understanding of PDR.

6.2 Yablo's Carnapian Mistake

While Carnap was not explicit about why it was that philosophers had failed to address the important question "*what would it be for Xs to exist?*", there seems little doubt that the heart of this lacuna is that for certain items we are very comfortable with classifying the items in question as existent or non-existent. For example, tables do exist, while human-made warp-drive spaceships do not. Julian Cole does exist, while Santa Claus does not. Tigers do exist, while hobbits do not. Further, there can be no doubt that if, for five of the six items mentioned above,[122] you asked a collection of typical human adults whether or not they exist, you would get the appropriate answers from all of them, or at least all of them who didn't have some

[122] I exclude Julian Cole because many do not know me.

non-standard beliefs about the items in question. Surely, the thought went, if we can make these kinds of classifications so easily and with such uniformity, then we must know what it is for an X to exist and what it is for an X not to exist. And, consequently, we must know how to apply this distinction to all Xs.

Carnap, of course, realized that no such thing followed. The fact that there is a large collection of things — roughly, things that either are, or according to the story surrounding them are taken to be, spatio-temporal items — that we can classify as existent or non-existent easily and uniformly, doesn't exclude there also being many items for which we don't know how to make this classification. Why? Well, because we aren't clear about what is at stake when we make the claims "X exists" and "X does not exist." We know merely that certain particular items fit on one side of this distinction and others on the other.

We saw in Chapter 5 that the heart of Yablo's Hermeneutic Fictionalism is the claim that mathematical items are representational aids. To motivate this thesis, Yablo provides a wide range of analogies between mathematical statements and statements that use non-mathematical representational aids to express contents non-literally. On the basis of these analogies and the arguments considered in Section 5.4, Yablo concludes that mathematical items are representational aids and that we use them in the non-literal expression of real contents concerning, at least at first, the spatio-temporal world. Specifically, he claims that the "exists" of "mathematical entity X exists" should be understood non-literally.

I noted in Section 5.4 that Yablo's arguments for this thesis rely implicitly on the thesis that to speak literally using existential pure mathematical expressions is to be committed to Platonistically construed ontology. Yet, at least to my knowledge, Yablo offers no arguments in favor of this thesis. Indeed, at least to my knowledge, Yablo never discusses the content of the distinction between speaking literally and non-literally in the context of existential pure mathematical expressions. All that he says is that if existential pure mathematical statements are taken literally, then mathematical domains are taken to be "real"[123] existents; while if existential pure mathematical statements are taken non-literally, there is no need to take mathematical domains to be "real" existents. In (Yablo, 2002a), (Yablo, 2002b), and (Yablo, ToAp), the larger part of the discussion of the distinction between literal and non-literal uses of expressions concerns representational aids of a non-mathematical variety. Further, even this discussion is minimal. In general, Yablo assumes that it is just obvious that the expressions used in the analogies mentioned above are used in a non-literal way.

Now, as I acknowledged in Section 5.2, there is no doubt in my mind that when the non-mathematical statements that Yablo uses in his analogies are used in most everyday situations, they are used in a non-literal way. Further, I'm prepared to admit that there would be a high level of agreement among ordinary speakers that these statements really are used to express real contents in a non-literal way. Well, more precisely, there would be such a high level of agreement if ordinary speakers had all of the concepts required to make the judgment in question.

This having been admitted, I want to point out three things. First, an examination will show that the non-mathematical representational aids that Yablo discusses are all parasitic on items with respect to which we are easily and uniformly able to draw the existent vs. non-existent distinction. Second, as Carnap's discussion of existence serves to remind us, the ability to draw the literal vs. non-literal distinction

[123]Recall from Chapter 5 that I maintain that the best interpretation of both Yablo's and Field's use of "real" existent is an existent whose existence is metaphysically independent of social practices.

easily and uniformly for this class of cases doesn't mean that we know what it is to speak literally (or non-literally) when making an existential claim within a discourse concerning Xs, for general Xs. Third, the ability to draw the literal vs. non-literal distinction easily and uniformly for this class of cases doesn't mean that it will be immediately apparent whether we are talking literally or non-literally when we make existential claims concerning Xs that are not included in this class of cases.

The bottom line is that Yablo has no right to assume that his examples provide us with a general distinction between literal and non-literal use of existential claims. To draw his Fictionalist conclusions legitimately, he owes us a well motivated account of this distinction (at least for the cases in which he wants to draw Fictionalist conclusions). And, as I observed above, at least to my knowledge, he fails to provide any such account.

6.3 Egalitarian Naturalism, Ontological Commitment, and Speaking Literally

So, Yablo has failed to address appropriately the issue of literal vs. non-literal use of mathematical expressions, most importantly existential pure mathematical expressions. This, of course, is still compatible with the thesis that his arguments implicitly rely on, i.e., that to use existential pure mathematical assertions literally is to be committed to the "real" existence of mathematical domains, and that to accept that these assertions are generally used non-literally is to accept that there is no need for mathematical domains be "real" existents. Yet, as I mentioned above, I believe that this thesis is false. Let us investigate to find the evidence against this thesis.

A good starting point is the observation that philosophy of mathematics is not the only place where it has been suggested that natural scientists are not speaking literally when they make existential claims. Instrumentalist philosophers of science have also made this claim with respect to theoretical discourses. Perhaps we can learn something by investigating how methodological naturalists have responded to instrumentalists in the philosophy of science.

So, how have methodological naturalists responded to instrumentalists? Primarily with the thesis that whether or not non-mathematical natural scientists should believe a particular class of entities to exist is a matter internal to natural science and, hence, a matter that should be assessed by the standards of natural science. To put this thesis a different way, non-mathematical natural scientists get to decide what the standards of literal existential assertion are for entities within the purview of the non-mathematical natural sciences.

Quine, the arch methodological naturalist, offers his own account of what these standards are, and hence of how existential matters should be assessed. Primarily, Quine maintains that these matters should be assessed by determining the simplicity, familiarity of principle, scope, fecundity, and empirical adequacy of the theory referring to these entities (cf. (Quine, 1955)). Maddy, the arch mathematical naturalist, offers her so-called 'argument from practice' (cf. (Maddy, 1997, Chapter 6)) as a challenge to Quine's criteria. The major part of this argument is an extended discussion of the debate over the existence of atoms. Maddy claims that, according to Quine's criteria, the physics community should have accepted the existence of atoms well before it in fact did. She draws the conclusion that Quine's criteria do not adequately represent natural scientific practice.

Despite these disagreements, Maddy and Quine agree that, for the methodological naturalist matters of ontology are continuous with matters of natural science. Consequently, they agree that matters relevant to ontological issues — such as what the standards are for literally asserting the existence of a class of entities — should be assessed by the standards of natural science. For an Egalitarian naturalist, this means that matters relevant to mathematical ontology should be assessed by the standards of mathematicians. So, for an Egalitarian naturalist, if we are to determine whether or not mathematicians speak literally while making existential pure mathematical assertions, we must look to the standards of mathematicians.

So, what do mathematical practices tell us is involved in the mathematical community claiming that a certain mathematical domain exists? I suggest the following: the contemporary classical mathematical community will claim that a certain mathematical domain exists if there is a coherent mathematical practice that represents that domain. This contemporary standard found its most explicit formulation in Hilbert's famous correspondence with Frege. Though, also famously, Gödel's work on incompleteness showed that the notion of "consistency" required was not the deductive one advocated by Hilbert. As discussed in Section 4.4, I take the relevant standard to be that of coherence.

Yet this standard is clearly an evolved form of suggestions made earlier in history. For a reasonable sketch of the historical development of this standard, the reader is referred to Chapter 5 of (Shapiro, 1997). I shall merely offer a couple of highlights here. One famous debate from the history of mathematics relating to the existence of a mathematical domain concerned the complex numbers. The victors in that debate offered the suggestion that the complex numbers can be "modeled" in the two-dimensional plane as an important piece of evidence in favor of the suggestion that this domain exists. A second important debate leading to the acceptance of new mathematical domains was that between Euclidean and non-Euclidean geometry. Once again, the demonstration that non-Euclidean geometries had "models" in other ontologically acceptable domains was a central plank in the argument of the victors.

In addition, Maddy, while talking about the foundational role of set theory, acknowledges that one of the roles that set theory is taken by set theorists[124] to fulfill is to provide a unifying domain in which questions of mathematical existence, if raised, can be settled. As she puts it, "the set theoretic point of view has allowed existence questions to be clearly posed and answered" (Maddy, 1997, p. 27). How exactly is set theory to fulfill this role? Well, by providing a uniform domain whose constituents can be used in the construction of "models" of questionable theories. Further, the reader should recall from Section 4.4 that satisfiability of a theory is a good model for coherence. In other words, the heart of coherence is the availability of a (classical) set-theoretic model for the theory in question. Models in the sense of model theory are merely the developed form of the types of "models" used by some at earlier points in the history of mathematics as the appropriate evidence (as least so far as the winners of various ontological debates were concerned) that the domain of a mathematical theory exists. Thus, all of the above examples point in the direction of the contemporary standard that the conditions under which the contemporary classical mathematical community is prepared to assert that a mathematical domain exists

[124]The qualification that it is taken by set theorists in this way is important, for there is much evidence that mathematicians in general do not view the foundational efforts of set theorists in the way in which set theorists do.

are the following: that the mathematical community believes that there is a coherent mathematical practice that characterizes that domain.[125]

These examples also illustrate that there have been times in the history of mathematics when mathematicians have been challenged to provide evidence that a particular mathematical domain exists and have settled such ontological matters in their own way. Further, this way is quite different to the way in which non-mathematical natural scientists have settled their ontological debates. In many such cases, an important feature of the evidence that mathematicians have presented in favor of the existence of a mathematical domain has been the demonstration that the practice characterizing that domain is coherent, because it has a "model" in some domain that was agreed to be ontologically innocent. For clarity, I want to be clear that I do not want to suggest, at least from a historical perspective, that this is the only piece of evidence that has been important. Certainly the domain having pragmatic benefits has also been very important in such debates.

So, from the above, we should conclude that just as non-mathematical natural scientists have their standards for determining whether or not entities within the purview of the non-mathematical natural sciences exist, so mathematicians have their standards for determining whether or not mathematical domains exist. If an Egalitarian naturalist is going to claim that a non-mathematical natural scientist is speaking literally when she asserts that an entity, or class of entities, exists on the basis of it, or them, satisfying the standards implicit in that scientist's practices, then, given

[125]The reader should note that this almost exactly matches with what the PDRist claims is involved in the existence of a mathematical domain. The other condition that PDRists place on the existence of mathematical domains is what I called the external constraint in Section 3.4. As a matter of historical fact, debates about the existence of mathematical domains have also considered issues about the usefulness of the domain in question. So this constraint is also supported by historical evidence.

an Egalitarian naturalist's belief in the equal treatment of mathematical and non-mathematical natural scientists, she should claim that a mathematician who asserts the existence of a mathematical domain on the basis of it satisfying the mathematical community's standards is also speaking literally.

While I believe that the standard that contemporary classical mathematicians use in deciding ontological matters is to assess whether the theory in question is coherent, that this is the standard is tangential to my main point here, which is that mathematicians do have their own standards, and those standards are different from the standards of contemporary non-mathematical natural scientists. Thus, *pace* Maddy,[126] Field, and Yablo, when the mathematical community agrees that a certain mathematical domain exists, the Egalitarian naturalist should conclude that they are asserting its existence in a perfectly literal sense.

I can imagine some philosophers objecting at this point: "Look, you are right that belief in the coherence of the practice representing a particular mathematical domain is the condition that mathematicians place on finding it acceptable to make existential pure mathematical assertions within that practice. But that doesn't mean that they are speaking literally when they make these assertions. After all, if you push them, and explain what is "really" involved in being ontologically committed to a mathematical domain, then don't you frequently find that they will withdraw their ontological commitments?"[127]

There are three points that I want to make in response to this objection. First, mathematicians are not philosophers, so if asking them whether they are "really"

[126]I refer here to Maddy's suggestion (cf. Section 0.2) that natural science will not support the ontological weight of its mathematical practices.

[127]I thank participants of the Fourth Annual Midwest Philosophy of Mathematics Workshop for raising this concern.

ontologically committed to a mathematical domain involves explaining to them what is involved in being "really" ontologically committed to a mathematical domain, then it is asking them for an opinion on a subject matter over which they are not experts.

Second, the extensive explanations required to make sure that mathematicians are answering the question that philosophers (at least think they) want answered brings with it the risk that philosophers will bias mathematicians concerning what is involved in being "really" ontologically committed to a mathematical domain. Given the reaction of contemporary philosophers of mathematics to Brouwer-Heyting style Intuitionism, it seems likely that this extensive explanation will result in metaphysical independence from social practices being built into what is involved in "real" ontological commitment to a mathematical domain. Yet it is no wonder, if this is the content of the question being asked, that some astute mathematicians will withdraw their ontological commitment to mathematical domains. After all, according to PDRists, if this is what is involved in "really" being ontologically committed to mathematical domains, then there are reasons to believe that mathematicians both aren't, and shouldn't be, "really" ontologically committed to mathematical domains.

Third, we should bear in mind the fact that, at least in my experience (and that of others whom I have spoken to), mathematicians often show a great deal of impatience with philosophers asking questions about the "real" existence of mathematical domains. While it would require an empirical study to investigate the source of this impatience properly, I conjecture that it is the result of a combination of two things. First, philosophers aren't always particularly clear about what is involved in "really" being ontologically committed to a mathematical domain. Second, as a consequence of this, mathematicians feel that philosophers are replacing a reasonably clear cut question, which they have reliable intuitions on how to answer, i.e., whether or not this aspect of mathematical practice is coherent, with a nebulous question that their intuitions don't provide them with any guidance on answering.

This last point is especially telling, for it underwrites the essential point that mathematicians have perfectly good standards by which they assess the appropriateness of making existential pure mathematical claims. Surely, an Egalitarian naturalist should maintain, it is these standards that are the standards of literal existential assertion in the everyday practices of mathematicians. And, since according to these standards certain mathematical domains exist, an Egalitarian naturalist should maintain that mathematicians are speaking quite literally when they assert the existence of certain mathematical domains. So, *pace* Maddy, Field and Yablo, when a mathematician asserts that a mathematical domain exists because there is a coherent mathematical practice that characterizes that domain, an Egalitarian naturalist should take her to be speaking literally.

It is worth noting, by the way, that mathematicians are quite literally asserting the existence of *mathematical* domains. Thus, this line of enquiry also points toward a significant defect of Modal Nominalism.

6.4 What would it be for Mathematicians to make Non-Literal Existential Assertions?

Perhaps the conclusion of the last section can be made more vivid to the reader by considering some examples. Consider such typically fictional and metaphorical items as Sherlock Holmes, elves, and the butterflies in my stomach. Any competent speaker of English should recognize the legitimacy of the following questions: what would it be for Sherlock Holmes *really* to exist, as opposed to his being a fictional character?

What would it be for elves *really* to exist, as opposed to their being made up in stories and legends? And, what would it be for there *literally* to be butterflies in my stomach, as opposed to there metaphorically being butterflies in my stomach?

Contrast the above questions with the following: what would it be for the political border between the USA and Canada *really* to exist, as oppose to its being true that it exists? What would it be for the US Supreme Court *really* to exist, as opposed to its being true that it exists? What would it be for major league baseball *really* to exist, as opposed to its being true that it exists? These questions should all strike a competent speaker of English as peculiar — they presuppose a contrast that isn't genuine. Specifically, they presuppose the contrast between a literal and non-literal usage of existence with regard to these constitutive social constructs. Indeed, it is precisely because such a contrast is genuine for fictional and metaphorical items that the three questions of the previous paragraph are legitimate.

Now consider the following questions: what would it be for the number two *really* to exist, as opposed to it being true that the number two exists? What would it be for infinitely many prime numbers *really* to exist, as opposed to it being true that infinitely many prime numbers exist? What would it be for an inaccessible cardinal *really* to exist, as opposed to it being true that an inaccessible cardinal exists? These questions strike me as peculiar in the same way that the questions about the political border between the USA and Canada, the US Supreme Court, and major league baseball strike me as peculiar. They, too, presuppose a contrast between literal and non-literal use of the existential quantifier for mathematical entities that isn't a feature of mathematical practices. Of course, some philosophers of mathematics have *thought* that they can make sense of this contrast, but, as the arguments of the last section show, they are mistaken. I shall offer a diagnosis of their mistake in Sections 6.5 and 6.6.

What the questions of the last paragraph suggest is that it is difficult to see what it would be for mathematicians to make non-literal existential pure mathematical assertions. This is so primarily because it is difficult to see what a (global) distinction between making literal and non-literal existential pure mathematical assertions could amount to. Thus, the above argument is consonant with one of the main theses of John Burgess' *Mathematics and* Bleak House (Burgess, 2004), in which Burgess argues that he cannot make out a (global) distinction between the literal and non-literal use of existential pure mathematical assertions.

The best that I can do in making out some kind of distinction is to note the following. Much of the development of both the mathematical and the non-mathematical sciences works on the basis of the use of analogies. In the non-mathematical case, these analogies are often formulated, at least initially, as metaphors. Thus, at early stages in theory development, there is some legitimacy to the claim that non-mathematical natural scientists are engaged in existential metaphor.

Perhaps the same kind of existential metaphor is used by mathematicians in their development of new mathematical concepts and theories. If so, and I do not claim to have surveyed mathematical practices with sufficient detail to know whether this is so, then it might be that there is a local sense in which mathematicians do use existential pure mathematical assertions non-literally. Yet if this is so, then it should be noted that these uses of existential pure mathematical assertions are not the standard cases on which the arguments of Section 6.3 rely.[128]

There is one point in Yablo's writing of which I am aware where he tries to deflect a concern to do with the distinction between speaking literally and non-literally. In light of this, we should consider the relevant passage. It begins with the identification of a test, which Yablo calls the felt-distance test, for metaphorical use of an expression:

> Of all the reasons people give for thinking that platonic metaphors couldn't have slipped in unnoticed, the most common is this. I speak metaphorically only if I speak in a way that is guided by, but somehow at odds with, my notion of what would be involved in a literal deployment of the same sentence. The literal meaning is not mine, but I have to be exploiting or making play with it – I have at any rate to set myself up in opposition to it – if I am to count as a metaphorist. This immediately suggests a negative test. Metaphors are, says Fowler, "offered and accepted with a consciousness of their nature as substitutes." So in the absence of any such consciousness – in the absence of a literal meaning the speaker can point to as exploited where it might instead have been expressed – there is no metaphor. Call this the "felt distance" test for metaphorical utterance. (Yablo, 2000, Section XV)

I take the essential point behind the felt-distance test to be that metaphorical/non-literal use can only make sense in contrast with non-metaphorical/literal use. After suggesting this test Yablo, uses it to formulate an objection to counting existential pure mathematical claims as metaphorical/non-literal. In essence, it is that such assertions do not pass the felt-distance test.

[128]Burgess suggests another possibility. Specifically, particular mathematicians, guided by philosophical reflection, could take themselves to be speaking metaphorically. If this is true, then, once again, as Burgess points out, this is the exception and not the rule.

Yablo responds to this objection in two ways. First, he spells out something that I noted earlier: he is not claiming that the mathematical terms in mathematical expression get used metaphorically; rather, he is claiming that the existential quantifier is being used metaphorically. He then tells us:

> The reason this matters is that the existential quantifier passes the felt-distance test. When I assume for metaphorical purposes that numbers exist, I am guided by, but at the same time (running the risk of) disrespecting, the literal meaning of "exists"; for using "exists" literally, numbers may well not exist, in which case "10 is the number of my toes," ie., "there is a number which numbers my toes and which is identical to all numbers of my toes and which is 10," is literally false. (Yablo, 2000, Section XV)

As I have emphasized above, if I am correct, then Yablo is simply wrong here. 'Exists' is used in a perfectly literal sense in these types of claims. In the next section I shall offer a diagnosis of what I take to be Yablo's mistake in believing what he asserts in this passage.

After offering this response, Yablo offers his second response:

> Secondly, though, the felt-distance test is wrong. It is true that if I am to use a sentence S metaphorically, there had better be conditions under which S is pretense-worthy, and conditions under which it is not pretense-worthy. But as we know from the example of fiction, this does not require that S possesses a literal meaning, as opposed to fictionally possessing one in the relevant make-believe game. Flann O'Brien in *The Third Policeman* tells of a substance called "gravid liquid," the tiniest drop of which weighs many tons, and whose subtle dissemination through the parts of material objects is all that prevents them from floating away. When I pretend in discussions of that book that gravid liquid cannot be held in a test tube, I am guided by my idea of what "gravid" is supposed in the game to mean; I have no concern at all about what it means in English, and for all I know it is not even an English word. An example more to the present point is this. "Smart" in my dictionary is an adjective, not a noun. How is it that we can say "she has a lot of smarts" and be understood? Well, it is part of the relevant make-believe game that there are these entities called

> "smarts" that are somehow the carriers of intelligence; the more of them you have, the smarter you are. The make-believe meaning of "smart" as a noun is of course informed by its literal meaning as an adjective. Who is to say it is not the same with "ten"; the meaning it is pretended to have qua noun is informed by its literal meaning qua adjective. (Yablo, 2000, Section XV)

I find this passage most peculiar. First, given Yablo's own comments on what is used metaphorically/non-literally, why are we concerned with the literal meaning of the sentence? Surely, what we should be assessing is the following: how are we able to assess that existence is being used in a non-literal way in the claims "gravid liquid exists" and "smarts exist"? Well, I take the answer to be the following. There is enough information in *The Third Policeman* linking gravid liquid to physical substances whose existence we know how to assess in a literal way for us both to know what it would be for there literally to be gravid liquid and to know that our physical theories tell us that there is no such liquid. In other words, *The Third Policeman* provides us with something like an implicit definition of gravid liquid, thus providing existence claims about it with a literal meaning. Similarly, the way in which the noun 'smarts' is linked with the adjective 'smart' informs us as to what it would be for there literally to be smarts, and our best theory of how the brain produces intelligent behavior tells us that, in a literal sense, there are no smarts.

What these two cases have in common is that the "story" surrounding these entities makes it clear that the literal existence of these entities should be assessed by the standards used to assess the existence of spatio-temporal entities, and we know from everyday life and natural science what these standards are. Thus, we know what it would be to assert literally the existence of these entities; and we know that they do not exist.

Given the informal way in which the felt-distance test is stated, I don't know whether to claim that Yablo is right that the felt-distance test is wrong but that there is a related test that can be used to test for non-literal use, or whether the felt-distance test is right after all. Whichever it might be, I hope that it is clear that Yablo has given us no reason to doubt that, in order to assess whether an existential claim is made non-literally, we have to know what it would be to make that existence claim in a literal way. In fact, I'm convinced that Yablo has a set of standards for the literal assertion of the existence of mathematical entities in mind when he makes his assertion that these claims are made non-literally. He is interested in assessing whether there is a Platonistically construed mathematical domain containing the said entities to make his claim true. Yet, as I have argued above, and will continue to argue below, these standards are not the appropriate standards to apply in assessing whether existential pure mathematical statements are made literally.

6.5 A Diagnosis

The purpose of this section is to investigate the mistake that Field, Yablo, and Maddy make in applying Platonistic standards of literal assertion to mathematics. Let us begin with some evidence that Maddy makes this mistake. According to Maddy, "mathematical practice itself gives us very little ontological guidance" (Maddy, 1997, p. 192). Methodological debates within mathematics are not "settled on the basis of . . . philosophical considerations" (Maddy, 1997, p. 191), and so no conclusions relating to ontological questions can be drawn from mathematicians' practices. Maddy claims that "nothing seems to preclude even Fictionalist or Formalist interpretations" (Maddy, 1997, p. 192) of mathematics. Indeed, in (Maddy, 2005), as I noted in Section 0.2, Maddy suggests that some metaphysically anti-realist interpretation is the best interpretation of mathematical practices. These are very strange theses to maintain while at the same time acknowledging that "the methods of mathematics ... tell us ... that certain mathematical objects exist" (Maddy, 1997, p. 192). This seems to be an ontological thesis *par excellence*.

How are we to reconcile these seemingly contradictory claims? I suggest that the best way is to conjecture that Maddy, like Field and Yablo, is under the impression that for mathematical practices to tell us something relevant to ontological questions is for them to tell us something that is relevant to whether or not mathematical domains have an existence that is (at least weakly) metaphysically independent of social practices. For, as I am arguing, if this is what it is for mathematical practices to tell us something relevant to ontological questions, then there is good reason to believe that Maddy is right in her claim that mathematical practices have essentially nothing to tell us that is relevant to ontological questions.

Here is the important point. Upon reflection, Maddy's belief that what is involved in mathematical practices informing us about ontological issues is their informing us about issues of metaphysical independence is easily explained and consistent with what she says. She tells us that the practices of non-mathematical natural scientists are "the final arbiter of what there is" (Maddy, 2005, p. 456). So, for Maddy, what it is to be literally committed to the existence of *any* item is determined on the basis of investigation of the considerations actually used by non-mathematical natural scientists in drawing existential conclusions. It is perfectly reasonable that Maddy could have observed the practices of non-mathematical natural scientists and drawn the conclusion that existence that is (at least weakly) metaphysically independent of social practices is, roughly speaking, what is at stake in debates between non-mathematical natural scientists about whether spatio-temporal entities exist. Indeed, I would maintain that she would be correct if she had made this observation.

Now, if it is reasonable that Maddy could have reached this conclusion in this way, then it is not so unlikely that Field and Yablo could also have been influenced to believe this same thesis by consideration of the standards of non-mathematical natural scientists. Indeed, this seems all the more plausible in light of the fact that there is no alternative to this thesis on offer at present, when applied to mathematical items, in the Anglo-American tradition within which Field, Yablo, and Maddy are working.

Further, an Egalitarian naturalist will, of course, agree with Maddy that investigation of the considerations actually used by non-mathematical natural scientists is the means by which one should determine what it is to be literally committed to the existence of an item within the purview of the non-mathematical natural sciences. Yet, at the same time, we can insist that the need to show equal respect for the practices of mathematical and non-mathematical scientists requires that, in determining what it is to be literally committed to the existence of a mathematical domain, the practices that need to be investigated are not those of non-mathematical natural scientists, but rather those of mathematical scientists. Further, as Maddy acknowledges, these practices are quite different from those of non-mathematical natural scientists.

For non-mathematical natural scientists, as I have suggested on a number of occasions, some reason to believe that an item exists independently of social practices is required in order to assert the existence of that item. But, as I made explicit in Chapter 3 and in Section 6.3, there are reasons to believe that mathematicians have no such interest in metaphysical independence from social practices in making existential pure mathematical claims. The coherence of the practice representing a domain is all that contemporary classical mathematicians are interested in. And the coherence of a practice is independent of whether or not there is a domain that is metaphysically independent of social practices that that practice represents in the strict model-theoretic sense. Further, in light of the above suggestions, we can see that Maddy's claim that mathematicians' practices do not inform us about ontological issues should be understood as her recognizing this to be true.

Yet if assessing whether or not a mathematical domain has an existence that is (at least weakly) metaphysically independent of social practices isn't what mathematicians assess before making literal existential pure mathematical assertions, then what do they assess? The answer to this question, I contend, is simple. It is whether or not that domain exists *per se*. The issue of metaphysical independence from social practices is moot for mathematicians. This is, of course, exactly what PDRists maintain. Further, PDR is an account of mathematics that can combine the thesis that mathematicians are speaking literally when they make existential pure mathematical claims with the ability to provide the fulfilling explanations of certain features of mathematical practices that Yablo's Hermeneutic Fictionalism is able to provide and Platonism unable to provide. Thus, it is ideal from an Egalitarian naturalists' perspective.

6.6 More on Haslanger's Account of Social Construction

In Part I, I used Sally Haslanger's discussion of the distinction between causal and constitutive social construction (cf. (Haslanger, 1995)) in explicating the metaphysics of Platonism and PDR. I believe that Haslanger's discussion of social construction can be of further assistance to us. This assistance takes two forms. First, it can help to make plausible the ascription of a mistake to Field, Yablo, and Maddy. Second, it can aid us in further understanding and providing motivation for PDR. Thus, in this section, I shall provide a few more details from Haslanger's discussion of social construction and discuss how they relate to mathematics. Further, I am going to discuss how they provide another potential diagnosis of Field's, Yablo's, and Maddy's mistake.

One mechanism of social construction that Haslanger is particularly interested in is construction due to the way in which things are described and classified. It is well known from psychology that the way in which human beings are described and classified can have a dramatic impact on them and their behavior. Here is a discussion of this phenomenon, with an illustration, due to Haslanger:

> At least in the case of human beings, the mere fact of how we are (even potentially) described or classified can have a direct impact on our self-understandings and our actions, because typically these descriptions and classifications bring with them normative expectations and evaluations. This works in several ways. Forms of description or classification provide for kinds of intentions; e.g., given the classification "cool", I can set out to become cool, or avoid being cool, etc. But also, such classifications can function in justifying behavior — e.g., "we didn't invite him, because he's not cool" — and such justifications, in turn, can reinforce the distinction between those who are cool and those who are uncool. (Haslanger, 1995, pp. 98-9)

In line with this illustration, Haslanger defines the following type of social construction:

> *Discursive construction*: Something is discursively constructed just in case it is the way that it is, to some substantial extent, because of what is attributed (and/or self-attributed) to it. (Haslanger, 1995, p. 99)

It should be noted that the type of attributes or classificatory schemes that Haslanger is interested in while discussing discursive construction can both (in some sense) reflect the "intrinsic nature" of items that exist independently of social practices, or themselves be the product of social construction. Here, once again, Haslanger motivates the idea that the classificatory schemes could be the product of social construction with a discussion of "coolness":

> ... let's go back to the example of "being cool": In considering our use of the distinction between those who are cool and those who are uncool, it is plausible to conclude that the distinction is not capturing intrinsic differences between people; rather it is a distinction marking certain social relations — i.e., it distinguishes status in the in-group — and the fact that it is employed in any given context is a reflection of the importance of in-group and out-group relations. For example, suppose that I need a way to establish a cohort, I do so by calling those I like "cool" and those I don't "uncool." The distinction does not capture a difference in the individuals so-called except insofar as they are related to me (based on my likes and dislikes), and its use in the context is determined not by the intrinsic or objective coolness of the individuals but by the social task of establishing a cohort. (Haslanger, 1995, pp. 99-100)

For this reason, Haslanger introduces the following notion of social construction:

> *Pragmatic construction*: A classificatory apparatus (be it a full-blown classification scheme or just a conceptual distinction or descriptive item) is socially constructed just in case its use is determined, at least in part, by social factors. (Haslanger, 1995, p. 100)

Haslanger is here considering the function of particular parts of discursive practices that play a role in the discursive construction of individuals. She acknowledges that one function that such discursive practices can play is that of representing — in the strict sense given to us by model theory — objective features of the world. Yet Haslanger is more interested in discursive practices that have other functions, functions that are social and pragmatic in nature.

What we must ask is: how might discursive and pragmatic construction be relevant to a constitutive variety of construction? Well, one thing that we should note is that if a discursive practice under consideration as one that is constitutively socially constructing some domain, were one that is in the business of representing (in the strict sense given to us by model theory) objective features of a world that is strongly metaphysically independent of social practices, then we would not have constitutive construction at all. What we would have is representation. Thus, all discursive practices involved in constitutive social construction fail to represent, in the strict sense given by model theory, features of a world that is strongly metaphysically independent of social practices. In light of this, it is reasonable to believe that they perform some other pragmatic function, or functions. This fits nicely with a PDRist's account of mathematics. According to it, the primary purpose of mathematical discursive practices is to provide aid in representing features of the spatio-temporal world, not mathematical domains that are metaphysically independent of social practices.

After introducing pragmatic construction, Haslanger completes her classification of types of social construction by distinguishing between two strengths of pragmatic construction:

> A distinction is *weakly* pragmatically constructed if social factors only partly determine our use of it.
>
> A distinction is *strongly* pragmatically constructed if social factors wholly determine our use of it, and it fails to represent accurately any "fact of the matter."
>
> (Haslanger, 1995, p. 100)

This distinction might not be immediately transparent to the reader — it was not to me when I first read it. Yet Haslanger's continued discussion of the example of "coolness" helped me, both with understanding what this distinction amounts to and with understanding what she means by "fact of the matter" in the second definition:

> In the example of "cool," I use the term to establish my cohort, and in doing so my ascriptions are guided by my likes and dislikes; so there may be a real social distinction (admittedly parochial) that corresponds to my use — I call Mary and George "cool," Susan and John "uncool," and the application of the terms corresponds to who I like and who I don't. But note that in attributing "coolness" to someone, I'm doing so with the background assumption in play that the "coolness" is an intrinsic feature of the individual and is not merely a matter of who I like. In calling Mary and George "cool," I'm suggesting that there is something cool *about them* that has nothing to do with me — supposedly, it's *their coolness* that warrants my use of the term. It is here that the question of fact arises: Insofar as I am attributing intrinsic coolness to someone, my attribution misfires since no one is, so to speak, cool *in themselves*. In such cases I want to say that my attributions of coolness are false — there is no fact about their coolness that I am accurately representing, even if my use of the terms corresponds to some other features of the individuals, e.g., whether or not I like them. So, *strong pragmatic constructions are, in an important sense, illusions projected onto the world; their use might nevertheless track — without accurately representing — a genuine distinction*. The main point is that in cases of strong pragmatic construction there are no available facts corresponding to the intended content — in the case at hand, about intrinsic coolness or uncoolness — that my attributions could be tracking, so instead, we might conclude, they must be functioning *wholly* as a means to a social goal. (Haslanger, 1995, pp. 100-1)

So, "coolness" is taken to be a strong pragmatic construction by Haslanger in virtue of the fact that there is no intrinsic and objective feature of people that attributions of "coolness" serve to represent in the strict sense provided to us by model theory. Further, it seems that "fact of the matter" roughly corresponds to the idea that there is a feature of the world there that would be there even if the social factors driving the construction were not there. That is, it relates to metaphysical independence from certain social practices.

Now, let us see how this discussion might be relevant to mathematics. PDRists claim that mathematical domains are "strong" constructions. This follows from the fact that what it would be for there to be something that Haslanger would describe as a "fact of the matter", which a mathematical discursive practice represents, is for there to be a mathematical domain that is strongly metaphysically independent of social practices. Of course, this is precisely what PDRists deny.

There is one further important feature of the above quote that I would like to draw to the reader's attention. Specifically, along with the thesis that mathematics is a "strong" construction comes a sense in which mathematics is, as Haslanger puts it, "an illusion projected onto the world." In particular, there are no mathematical domains that exist independently of social practices. In light of this, it is no wonder that so many should be so tempted by Fictionalism.

Yet, as we are about to see, this is not the bottom-line. There is another wrinkle to Haslanger's story concerning "coolness" that it is important for our purposes to understand:

> To debunk the belief that there is a special quality of coolness that warrants the designation "cool", we show that there is no such property of "coolness" (so understood) and, in fact, that the application of the term "cool" is determined wholly by the interests and concerns of the in-group. In other words, "coolness," when debunked, is revealed as a *constitutive construction*; i.e., the concept doing the work of determining when the term should be applied makes essential reference to social factors (i.e., in-group status).
>
> But we must be careful here: What counts as the concept "cool"? Once we have disrupted the coolness illusion, there seem to be two different concepts playing a role in our use of the term. On the one hand, there is the concept that actually determines how we apply the term to cases, i.e., (roughly) being such as to conform to the standards of the in-group. Let's call this the *operative* concept. On the other hand, there is the concept that users of the term typically take (or took) themselves to be applying, i.e., being intrinsically or objectively cool, where this is supposed to be

> the objective basis for the in-group standards. Let's call this the *manifest* concept. (Haslanger, 1995, p. 102)

Haslanger's distinction between operative and manifest concepts is very important for the purposes at hand. I take the operative concept of something to relate to the conditions actually governing the application of the term/terms concerning that thing, while I take the manifest concept of something to relate to the rhetoric that surrounds the application of the term/terms concerning that thing.

I suggest that, for one reason or another, philosophers of mathematics of both Platonist and Fictionalist persuasion have been working with a conception of mathematics that is parallel to what Haslanger describes as the manifest concept of "coolness". The manifest conception of coolness takes "coolness" to be a feature of individuals that is intrinsic and objective — roughly speaking, strongly metaphysically independent of certain social practices — and which at least justifies, if not explains, our practices of classifying individuals as cool/uncool. Similarly, both Platonist and Fictionalist philosophers of mathematics have taken mathematical discursive practices, if taken literally, to concern domains that are metaphysically independent of social practices. Further, these domains are taken, at least by some, to play some justificatory and explanatory role in grounding our mathematical practices. Let us call this conception of mathematical domains the **manifest conception of mathematical domains**.

Owing to the fact that so many use the manifest concept of "coolness", there is a need for a "debunking" project showing that coolness is not an intrinsic and objective feature of individuals. One of the purposes of this dissertation has been to begin a parallel "debunking" project with respect to the manifest conception of mathematical domains. The outcome of the "debunking" project with respect to "coolness" is the recognition that "coolness" is a constitutive social construction. PDRists promote a parallel conclusion with respect to mathematics. Mathematical domains are constitutive social constructs.

There *might* be a disanalogy between mathematics and coolness, however. Haslanger's definition of the manifest conception of "coolness" invokes the fact that the majority of people using "cool" take themselves to be describing something intrinsic to the nature of the people that they are describing. I'm not sure that this is in fact true. I'm inclined to believe that people are much less taken in by ascriptions of coolness than Haslanger is suggesting. What I am sure about is that, in the mathematical case, the ascription of the manifest conception of mathematical domains to ordinary folk is not warranted.

In general, it is only philosophers, and not even all of us, who have taken the literal use of mathematical statements to concern domains that are metaphysically independent of social practices. A growing hermeneutic interest in the activities of mathematicians on the part of philosophers of mathematics — Maddy is a prime example — is, as I have been arguing, promoting the view that mathematicians do not take their literal assertions to have the metaphysical implications characteristic of the manifest conception of mathematical domains. Thus, the experts concerning mathematics do not appear to have the manifest conception of mathematical domains, but rather the opposing view of mathematical domains — what I shall call the **operative conception of mathematical domains**. PDR is an attempt to explicate the operative conception of mathematical domains. The manifest conception of mathematical domains is, to a large extent, a conception only prevalent among (certain) philosophers of mathematics.

The disanalogy with respect to how prominent the manifest conception of coolness and the manifest conception of mathematical domains are is important. For, as Haslanger acknowledges (Haslanger, 1995, p. 102), there is a real question as to whether the manifest or the operative conception of cool is the concept "cool". Both are good candidates. Yet because the evidence suggests that mathematicians have the operative conception of mathematical domains, there is a much stronger argument to be made in the mathematical case that the operative conception *is* the appropriate conception of mathematical domains. Hence, there is a strong argument that philosophers of mathematics, who in many cases have the manifest conception of mathematical domains, are simply mistaken. Of course, this is an understandable mistake, for, as much work on social construction maintains, people do, in general, make the very mistake that philosophers of mathematics have made when it comes to certain social constructs, e.g., gender, race, and coolness. This mistake is obviously an intellectual hazard of thinking about certain social constructs.

6.7 Some Final Thoughts

Throughout Part III, I have taken Fictionalism to be a hermeneutic thesis concerning the correct interpretation of mathematical practices, i.e., existential pure mathematical statements within such practices should be interpreted in a non-literal way. One concern that I have heard voiced about my argument[129] is that this is not the correct way to characterize Fictionalism. Fictionalism should rather be understood as a metaphysical thesis about mathematical domains either not existing or not needing to exist in order for mathematical practices to play the role that they do in our lives.

What underlies this suggestion is that many Fictionalists do not, in the way that Yablo does, reach their Fictionalism by finding features of mathematical practices that warrant ascribing a non-literal interpretation to existential pure mathematical statements. Rather, they first become convinced of the above metaphysical thesis, i.e., that either mathematical domains do not exist or that they need not exist, and then decide — *because of their endorsement of this metaphysical thesis* — that mathematical discourses are something like fictions. The point is that the thesis about mathematical discourses being something like fictions is secondary to the primary metaphysical thesis.

The first thing to notice about this concern is that there are two routes that Fictionalists of the type just characterized use to reach their metaphysical thesis. The first route is a rejection of the Quine-Putnam indispensability argument. Using a rejection of the Quine-Putnam indispensability argument to establish Fictionalism is confused, because indispensability to non-mathematical natural science is not what should motivate our acceptance of the existence of mathematical domains. It is rather, as I have been arguing, our respect for mathematicians and our recognition that they are ontologically committed to mathematical domains that should motivate our acceptance of the existence of mathematical domains. Indispensability is a red-herring. The second argument that these Fictionalists use to reach their metaphysical

[129] Otávio Bueno has raised this concern with me in private communication.

thesis is the epistemological argument against Platonism. In Part II, I established two things about this argument. First, it does not undermine Platonism in the way in which these Fictionalists suppose it to. Second, for an Egalitarian naturalist, the interpretation of mathematical practices that this argument motivates is PDR, not Fictionalism. So, even if the concern under consideration is legitimate, I have presented enough arguments to undermine the arguments of these Fictionalists.

PART IV

CONCLUSION

The Way Forward

In this dissertation, I have begun the project of articulating and defending a new metaphysical interpretation of mathematical practices — Practice-Dependent Realism. In Part I, I outlined PDR, contrasting it with both traditional forms of Platonism and neo-fregean Platonism. In Part II, I argued that PDR is superior to all forms of Platonism as an interpretation of mathematical practices, while in Part III I argued that PDR is superior to all forms of Fictionalism as an interpretation of mathematical practices.

Despite all this, there is still much work to be done in articulating and defending PDR. Perhaps the most notable absence from this dissertation is a detailed argument that PDR offers a better interpretation of mathematical practices than all forms of Modal Nominalism. The outline of such an argument is easy to find. While some mathematicians — particularly those working in more foundational areas such as set theory or category theory — would recognize the important links between mathematical existence and logical modalities, there is little doubt that, in general, mathematicians take the subject matter of mathematics to be mathematical domains/structures. So, as noted in Section 6.4, it is natural — *and appropriate* — for an Egalitarian naturalist to interpret mathematicians as being ontologically committed to *mathematical* domains.

Given this interpretative point, it is a defect of Modal Nominalism that it takes that in virtue of which mathematical truths are true to be logical modal facts rather than mathematical domains/structures. If, despite this defect, Modal Nominalism is to be found preferable to PDR as an interpretation of mathematical practices, then it had better have some fairly significant advantages over PDR to outweigh this defect.

Yet Modal Nominalism has no such advantages over PDR. Consequently, PDR is superior to Modal Nominalism as an interpretation of mathematical practices.

While this argument sketch is helpful, turning it into a convincing argument for the superiority of PDR to Modal Nominalism as an interpretation of mathematical practices would call for a detailed defense of the major premise, i.e., that Modal Nominalism offers no advantages — or at least no advantages that would outweigh its defect — over PDR as an interpretation of mathematical practices. Establishing this thesis would require a significant amount of work, work which I do not have the time or the space to carry out in this dissertation. Not only would it involve a significant investigation of Modal Nominalism's merits as an interpretation of mathematical practice, but, more importantly, it would necessitate a variety of arguments to support contentions that I have hinted at above about the merits of PDR as an interpretation of mathematical practices.

As a beginning, I would need to fill in the details of my sketch that knowledge of abstract mathematical domains is possible if PDR is true, and show that according to this account of mathematical knowledge, there is a sense in which mathematical knowledge is *a priori*. Further, I would need to argue that there is a sense in which (at least some) mathematical truths are objective if PDR is true. And a sense in which (at least some) mathematical truths are necessary if PDR is true, despite the fact that mathematical entities are not necessary existents according to PDRists. In addition, I would need to provide a detailed account of the applicability of mathematics to the spatio-temporal world. All of these are aspects of mathematical practices that Modal Nominalists have argued they can account for.

Fortunately, I have already said something about most of these features of PDR. The exceptions to this are the objectivity and necessity of (at least some) mathematical truths. Let me say just a few words about these. Before I begin, however, perhaps I should note the following. It is a wholly objective truth about baseball that a batter with three strikes is out. And it is a wholly objective truth about the political border between the U.S.A. and Canada that it runs through Lake Superior. So, at least in some sense of the term, there are objective truths about constitutive social constructs. Consequently, if mathematical domains (structures) are constitutive social constructs, then, at least in some sense of the term, mathematical truths can be objective. The secret, of course, is to say something more about the sense in which (at least some) mathematical truths are objective (and necessary).

I want to begin by assuming that some account can be provided of the objectivity and necessity of logical consequence.[130] Let us also acknowledge the central role of logical consequence in mathematics. Then at least part of the objectivity and necessity of mathematical truth can be seen to be a matter of the objectivity and necessity of logical consequence. What remains to be accounted for — roughly speaking[131] — is the objectivity and necessity of the axioms of various branches of mathematics, for if these can be shown to be objective and necessary, then their logical consequences will be objective and necessary in virtue of logical consequence transmitting this objectivity and necessity.

[130]I understand that, given the links between set theory and logical consequence, this is a substantial assumption, yet I cannot provide an account of the objectivity and necessity of logical consequence in this Conclusion.

[131]This way of putting things simplifies tremendously, because it treats mathematics as wholly axiomatic. Of course, in reality, this is not true.

Now, recall Balaguer's suggestion that the objective truth or falsity of set-theoretic open questions turns on the determinateness of *our concept of set*. What Balaguer is recognizing in this suggestion is that the debate among mathematicians about whether Cantor's continuum hypothesis (CH) has an objective truth-value centers about the strength and determinateness of our concept of set, not whether a Platonistically construed set-theoretic hierarchy exists. Specifically, this debate focuses on whether our concept of set is strong enough and determinate enough to yield an axiom that has CH or $\neg CH$ as a consequence. This debate reflects a general phenomenon within mathematical practices. When mathematicians show an interest in the objectivity (and, in fact, necessity) of a mathematical truth, they are interested in what is objectively true of and necessitated by their conception of the subject matter of the truth in question.

While there is a significant dispute about what is so necessitated by our concept of set, there is little such dispute with regard to our concept of the natural numbers. Our concept of the natural numbers is — it is agreed by everybody — linked to cardinality properties. One way[132] to specify this link — in fact, Frege's way — is by means of what has come to be called **Hume's Principle (HP)** (cf. (Hale and Wright, 2001)):

> The number of Fs is equal to the number of Gs if and only if the Fs and the Gs are equinumerous.

In essence, Frege proved (cf. (Frege, 1884)) that if HP is true, then the Peano-Dedekind axioms are true of natural numbers.[133] Thus, if our concept of natural

[132]Another way to specify this link is by means of Tennant's Schema N: The number of Fs is n iff there are n Fs. Similar conclusions to those I draw below using HP can be drawn using Tennant's Schema N rather than HP (cf. (Tennant, 1987)).

[133]The Peano-Dedekind axioms are the (now) standard axioms used to characterize natural numbers. Yet they were not quite standard at the time when Frege wrote his *Grundlagen*. Frege, in

number demands the truth of HP — which it clearly does — then the Peano-Dedekind axioms *follow from our concept of natural number as a matter of logic*. Further, the Peano-Dedekind axioms are categorical, i.e., pick out a single mathematical domain (structure). Thus, HP specifies a unique subject matter for arithmetic. As a consequence, all truths of arithmetic are objectively necessitated by our concept of natural number, because that concept demands the truth of HP.

So it is a sufficient condition on arithmetical truths being objective and necessary that HP be demanded as true by our concept of natural number. Further, since some instances of the right hand side of HP are logical truths, the truth of HP forces us to accept that the domain of natural numbers exists. But nothing about the truth of HP requires us to accept that that domain exists independently of all social practices. HP can be true in virtue of a practice-dependent domain of natural numbers as easily as it can be true in virtue of a Platonistically construed domain of natural numbers. This account of the objectivity and necessity of arithmetical truths is thus as available to a PDRist as it is to a Platonist.

Extending the above account of the objectivity and necessity of arithmetical truths to cover other branches of mathematics is not straightforward, for our concept of many of these branches lacks a principle as clear as HP that can be used in the above way. Yet those mathematical truths that are objective and necessary are so as a result of the concept governing them objectively necessitating their truth. And, in general, this only requires the existence of the domain the truth in question is about, not that that domain exists independently of all social practices. Consequently, a PDRist is

fact, proved that a closely related collection of truths about natural numbers followed from HP. A sketch of a proof that the Peano-Dedekind axioms can be proved using only 2^{nd} order logic and HP can be found in (Wright, 1983).

as able to provide this — *the appropriate* — account of the objectivity and necessity of (at least some) mathematical truths as a Platonist.

The above is, of course, only the tip of the iceberg in terms of what remains to be done in carrying out the project that I have begun, and in investigating questions that it motivates. For a start, in Chapter 2, I left open the question of whether a PDRist should advocate the thesis that mathematical domains are (at least close to) paradigm cases of abstract entities. I left this question open, because I was concerned about the intelligibility of varieties of social constructivism that promote this thesis and the modal variety of social constructivism that I used to raise my concerns about such varieties of social constructivism. The intelligibility of these varieties of social constructivism deserves careful consideration. This type of consideration could also be valuable in further illuminating the constitution relationship between mathematical practices and mathematical domains, a metaphysical topic in need of further exploration by PDRists. Further, I believe that investigating the modal variety of social constructivism would help clarify the relationship between PDR and Modal Nominalism, a very important task for a PDRist.

A much broader issue that needs further attention is my advocacy of Egalitarian naturalism. Many of the arguments that I have offered in this dissertation rely on the thesis that mathematicians are equal citizens in the natural scientific community. Yet for many working within naturalized philosophy of mathematics, this, it would seem, is a controversial thesis. Ideally, I need to provide some further arguments in support of this thesis. It would also be useful for me to provide further discussions of how this distinguishes my philosophy of mathematics from those of many working within the mainstream. There are still many who take the Quine-Putnam indispensability argument to be an important argument in favor of mathematical Realism. For an Egalitarian naturalist, indispensability is a red-herring. An extensive discussion of this difference of opinion would be extremely beneficial to the philosophy of mathematics community in general.

In addition to these types of issues, there are a range of interpretative issues concerning mathematical practices that I have only treated in the most minimal way in this dissertation. Perhaps the most conspicuous are the issues surrounding what it is for a mathematical practice to be coherent. I indicated above that standards of coherence might vary between mathematical practices, with mathematicians of different logical persuasions using different standards of coherence. This situation naturally generates the following questions: What kind of interpretative principles should govern decisions as to which standards of coherence are relevant to a given mathematical practice? Are there well-motivated restrictions on the standards of coherence that can govern mathematical practices?

While discussing classical mathematical practices, I suggested, with minimal historical support, that something like the notion of coherence developed by Shapiro in (Shapiro, 1997) is the appropriate standard to apply to those practices. There are a number of worries that various individuals might have about this suggestion. Perhaps the most important are Shapiro's firm commitment to second-order logic and his lack of interest in natural models. An alternate — proof-theoretic — tradition concentrates on relative consistency proofs that can be offered within first-order logic or first-order logic supplemented by reflection principles justified by contemplation of the standard models of axioms. Debates between these traditions concerning how to think about coherence within classical mathematical practices are complex and worthy of careful consideration. It goes well beyond the scope of this Conclusion to offer any substantive advice on how to settle these types of debates.[134]

Yet this is only appropriate given the purpose of this dissertation. PDR is first and foremost a metaphysical interpretation of mathematical practices. Given this, it should be — and is — compatible with a variety of logical interpretations of mathematical practices. A PDRists can as well advocate a proof-theoretic standard of coherence as a model-theoretic one, provided that the proof-theoretic standard in question recognizes that mathematical practices incorporate logico-inferential resources strong enough to specify the ontological structure of the domains that those practices are about. And, of course, any practice that embodies the notion of a standard model of its subject matter must incorporate resources that are this strong.

A further interpretative issue in need of detailed investigation is the question that I raised in Chapter 2 about which aspects of mathematical practices are responsible for constitutively constructing mathematical domains and which ones are not. In my earlier discussion, I noted some activities that most certainly are involved in the constitutive construction of mathematical domains and some that are not. Yet there are many types of mathematical activities that I have said nothing about. An investigation of all mathematical activities in light of this issue would be profitable.

A final important issue that I have not addressed in this dissertation is the issue of how to individuate mathematical practices. In broad outline, I have assumed

[134]In light of my earlier suggestion that PDR is compatible with standards of coherence varying from mathematical practice to mathematical practice, I wonder whether these debates are symptoms of a division within classical mathematics between two distinct standards of coherence. Ultimately, only time will tell whether both standards become acceptable, or whether the debate will be settled one way or the other.

that mathematical practices should be individuated by their subject matter. One mathematical practice is the same as another, if the two practices characterize the same structure or structures. Adding resources to a practice turns it into a different practice, if those resources allow the practice in question to characterize a structure or structures that could not be characterized without those resources. Two questions arise immediately: Does this proposal concerning the individuation of mathematical practices accurately reflect the way in which mathematicians think about the individuation of mathematical practices? How does my proposal concerning the individuation of mathematical practices interact with the issues raised above about the use of different standards of coherence and characterization by mathematicians of different logical persuasions? These are important and intriguing questions that are worthy of investigation.

As the above should make clear, there is much work that remains to be done in carrying out my project. Yet it should not be forgotten that much has been achieved. I have, I believe, provided the reader with the heart of a new metaphysical interpretation of mathematical practices, and offered the reader a variety of reasons for wanting to endorse this interpretation of mathematical practices. This, I am convinced, is a genuine achievement.

BIBLIOGRAPHY

Balaguer, M. (1998). *Platonism and Anti-Platonism in Mathematics*. Oxford University Press, New York, NY.

Benacerraf, P. (1965). What numbers could not be. *Philosophical Review*, 74:47–73. Reprinted in (Benacerraf and Putnam, 1983), pages 272-294.

Benacerraf, P. (1973). Mathematical truth. *Journal of Philosophy*, 70:661–679.

Benacerraf, P. and Putnam, H., editors (1983). *Philosophy of Mathematics: Selected Readings*. Cambridge University Press, New York, NY, 2^{nd} edition.

Burgess, J. (2004). Mathematics and *Bleak House*. *Philosophia Mathematica*, 12:18–36.

Burgess, J. and Rosen, G. (1997). *A Subject With No Object: Strategies for Nominalistic Interpretation of Mathematics*. Oxford University Press, New York, NY.

Butler, J. (1993). *Bodies that Matter: On the Discursive Limits of 'Sex'*. Routledge, New York, NY.

Carnap, R. (1950). Empiricism, semantics, and ontology. *Revue Internationale de Philosophie*, 4:20–40. Reprinted in (Benacerraf and Putnam, 1983), pages 241-257.

Chihara, C. (1990). *Constructibility and Mathematical Existence*. Oxford University Press, New York, NY.

Chihara, C. (2004). *A Structural Account of Mathematics*. Oxford University Press, New York, NY.

Cohen, P. (1963). The independence of the continuum hypothesis. *Proceedings of the National Academy of Sciences of the U.S.A*, 50:1143–1148.

Cole, J. (2001). The Quine-Putnam indispensability argument. Unpublished Manuscript.

Colyvan, M. (2001). *The Indispensability of Mathematics*. Oxford University Press, New York, NY.

Dummett, M. (1973). *Frege: Philosophy of Language*. Duckworth, London, England.

Ernest, P. (1998). *Social Constructivism as a Philosophy of Mathematics*. State University of New York Press, Albany, NY.

Eves, H. (1976). *An Introduction to the History of Mathematics*. Holt, Rinehart and Winston Press, New York, NY, 4^{th} edition.

Field, H. (1980). *Science Without Numbers*. Princeton University Press, Princeton, NJ.

Field, H. (1989). *Realism, Mathematics, and Modality*. Basil Blackwell, Oxford, England.

Frege, G. (1884). *Die Grundlagen der Arithmetik*. Translated by Austin, J. (1960). *The Foundations of Mathematics*. Harper, New York, NY, 2^{nd} edition.

Frege, G. (1893). *Grundgesetze der Arithmetik*. Jena, Germany.

Gödel, K. (1940). *The Consistency of the Axiom of Choice and of the Generalized Continuum-Hypothesis with the Axioms of Set Theory*, volume 3 of *Annals of Mathematics Studies*. Princeton University Press, Princeton, NJ.

Gödel, K. (1947). What is Cantor's continuum problem. *American Mathematical Monthly*, 54:515–525. Reprinted in (Benacerraf and Putnam, 1983), pages 470-485.

Haggarty, R. (1989). *Fundamentals of Mathematical Analysis*. Adison-Wesley Publishing Company, New York, NY.

Hale, B. (1987). *Abstract Objects*. Basil Blackwell, Oxford, England.

Hale, B. (1994). Singular terms (2). In McGuinness, B. and Oliveri, G., editors, *The Philosophy of Michael Dummett*, pages 17 – 44. Kluwer, Dordrecht, Germany. Reprinted in (Hale and Wright, 2001), pages 48-71.

Hale, B. (1996). Singular terms (1). In Schirn, M., editor, *Frege: Importance and Legacy*. Walter de Gruyter, Berlin, Germany. Reprinted in (Hale and Wright, 2001), pages 31-47.

Hale, B. and Wright, C. (2001). *The Reason's Proper Study: Essays towards a Neo-Fregean Philosophy of Mathematics*. Oxford University Press, New York, NY.

Haslanger, S. (1995). Ontology and social construction. *Philosophical Topics*, 23(2):95–125.

Hellman, G. (1989). *Mathematics Without Numbers*. Oxford University Press, New York, NY.

Hersh, R. (1997). *What Is Mathematics, Really?* Oxford University Press, New York, NY.

Katz, J. (1981). *Language and Other Abstract Objects*. Rowman and Littlefield, Totowa, NJ.

Lewis, D. (1986). *On the Plurality of Worlds*. Oxford University Press, New York, NY.

MacBride, F. (2003). Speaking with shadows: A study of neo-logicism. *British Journal for the Philosophy of Science*, 54:103–163.

MacBride, F. (ToAp). More problematic than ever: The Julius Caesar objection. In MacBrider, F., editor, *Identity and Modality: New Essays in Metaphysics*. OUP.

Maddy, P. (1990). *Realism in Mathematics*. Oxford University Press, New York, NY.

Maddy, P. (1997). *Naturalism in Mathematics*. Oxford University Press, New York, NY.

Maddy, P. (2005). Three varieties of naturalism. In Shapiro, S., editor, *Oxford Handbook of Philosophy of Mathematics and Logic*. Oxford University Press, New York, NY.

Putnam, H. (1971). *Philosophy of Logic*. Harper Torch Books, New York, NY.

Putnam, H. (1981). *Reason, Truth, and History*. Cambridge University Press, New York, NY.

Quine, W. V. O. (1948). On what there is. *Review of Metaphysics*, 2:21–38.

Quine, W. V. O. (1951). Two dogmas of empiricism. *Philosophical Review*, 60:20–43. Reprinted in (Quine, 1980), pages 20-46.

Quine, W. V. O. (1955). Posits and reality. In Uyeda, S., editor, *Basis of the Contemporary Philosophy*, volume 5, pages 391–400. Reprinted in (Quine, 1976), pages 246-254.

Quine, W. V. O. (1969). Epistemology naturalized. In *Ontological Relativity and Other Essays*, pages 69–90. Columbia University Press, New York, NY.

Quine, W. V. O. (1972). Identity. Unpublished Manuscript.

Quine, W. V. O. (1975). Five milestones of empiricism. Reprinted in (Quine, 1981), pages 67-72.

Quine, W. V. O. (1980). *From a Logical Point of View.* Harvard University Press, Cambridge, MA, 2^{nd} edition.

Quine, W. V. O. (1981). *Theories and Things.* Harvard University Press, Cambridge, MA.

Rayo, A. (2002). Frege's unofficial arithmetic. *The Journal of Symbolic Logic*, 67(4):1623–1638.

Resnik, M. (1997). *Mathematics as a Science of Patterns.* Oxford University Press, New York, NY.

Restall, G. (2003). Just what *Is* full-blooded platonism? *Philosophia Mathematica*, 11:82–91.

Ricketts, T. (1986). Objectivity and objecthood: Frege's metaphysics of judgment. In Haaparanta, L. and Hintikka, J., editors, *Frege Synthesized: Essays on the Philosophical and Foundational Work of Gottlob Frege*, pages 65–95. D. Reidel Publishing Company, Hingham, MA.

Sellars, W. (1956). Empiricism and the philosophy of mind. In Feigl, H. and Scriven, M., editors, *The Foundations of Science and the Concepts of Psychology and Psychoanalysis*, volume 1 of *Minnesota Studies in the Philosophy of Science.* University of Minnesota Press, Minneapolis, MN. Reprinted with an introduction by Richard Rorty, and a study guide by Robert Brandom, Harvard University Press, Cambridge, MA, 1997.

Shapiro, S. (1991). *Foundations Without Foundationalism: A Case for Second Order Logic.* Oxford University Press, New York, NY.

Shapiro, S. (1993). Modality and ontology. *Mind*, 102:455–481.

Shapiro, S. (1997). *Philosophy of Mathematics: Structure and Ontology.* Oxford University Press, New York, NY.

Tennant, N. (1987). *Anti-Realism and Logic.* Oxford University Press, New York, NY.

Tennant, N. (1997). On the necessary existence of numbers. *Nous*, 3:307–336.

Tennant, N. (2000). Deductive versus expressive power: A pre-gödelian predicament. *The Journal of Philosophy*, XCVII (5):257–277.

Wright, C. (1983). *Frege's Conception of Numbers as Objects*, volume 2 of *Scots Philosophical Monograph.* Aberdeen University Press, Aberdeen, Scotland.

Wright, C. (1992). *Truth and Objectivity*. Harvard University Press, Cambridge, MA.

Yablo, S. (2000). A paradox of existence. In Everett, A. and Hofweber, T., editors, *Empty Names, Fiction and the Puzzles of Existence*, pages 275–312. CSLI Publications, Stanford, CA.

Yablo, S. (2002a). Abstract objects: A case study. *Nous*, 36(1):220–240.

Yablo, S. (2002b). Go figure: A path through fictionalism. *Midwest Studies in Philosophy*, 25(1):72–102.

Yablo, S. (ToAp). The myth of the seven. In Kalderon, M., editor, *Fictionalist Approaches to Metaphysics*. Oxford University Press, New York, NY.

Zermelo, E. (1908). Investigations in the foundations of set theory 1. *Mathematische Annalen*, 65:261–281.