Application and Ontology in Mathematics:

A Defence of Fictionalism

David Michael Price

PhD

University of York

Philosophy

July 2017

Abstract

The aim of this thesis is to defend fictionalism as a response to the mathematical placement problem. As we will see, against the backdrop of philosophical naturalism, it is difficult to see how to fit mathematical objects into our best total scientific theory. On the other hand, the indispensability argument seems to suggest that science itself mandates ontological commitment to mathematical entities. My goal is to undermine the indispensability argument by presenting an account of applied mathematics as a kind of revolutionary prop-oriented make-believe, the content of which is given by a mapping account of mathematical applications.

This kind of fictionalism faces a number of challenges from various quarters. To begin with, we will have to face the challenge of a different kind of indispensability argument, one that draws ontological conclusions from the role of mathematical objects in scientific explanations. We will then examine one recent theory of mathematical scientific representation, and discover that the resulting position is Platonistic. At this point we will introduce our fictionalist account, and see that it defuses the Platonist consequences of mathematical representation.

The closing chapters of the thesis then take a metaphilosophical turn. The legitmacy of a fictionalist response to the mathematical placement problem is open to challenge from a metaphilosophical perspective in two different ways: on the one hand, some modern pragmatists have argued that this kind of metaphysics relies on questionable assumptions about how langauge works. On the other, some modern philosophers have developed forms of metaontological anti-realism that they believe undermine the legitimacy of philosophical work in metaphysics. In the final two chapters I defend the fictionalist account developed here against these sceptical claims.

I conclude that the fictionalist account of applied mathematics offered here is our best hope for coping with the mathematical placement problem.

Contents

Declaration

I declare that this thesis is a presentation of original work and I am the sole author. This work has not previously been presented for an award at this, or any other, University. All sources are acknowledged as References.

Introduction

The aim of this thesis is to defend fictionalism as a response to the mathematical placement problem. A placement problem arises when we have committed ourselves to a synoptic philosophical project (such as naturalism) that places restrictions on the kinds of things we can admit into our ontology (Price, 2011, pp.184-189). We are then faced with a number of discourses (for instance, modal, mathematical, moral) for which it is difficult to find a home under this new, austere regime (Price, 2011, p.187). That is, it is difficult to find a place for the kinds of things apparently spoken of in these discourses, given our picture of human beings as thoroughly natural creatures, knowers and speakers embedded in the natural world.

The main line of argument in this thesis takes place against a broadly Quinean backdrop, in which naturalism is assumed, and in which ontological commitment is determined by establishing what objects there must be in order to account for the success of our best scientific theories (Quine, 1980). That is, we are assuming that our best scientific theories are our most reliable guide to what the world is like and what kinds of things it contains. Further, it is my belief that Quine is correct to see philosophy as essentially continuous with the natural sciences, and that the philosopher's job is to determine what precisely our best scientific theories are telling us about the world, by establishing what must be true, and what kinds of things must exist, in order to make those theories successful (Quine, 1980, pp.45-46).

As we will see, against this Quinean backdrop the mathematical placement problem becomes especially stark. On the one hand, we have a picture of human beings as thoroughly natural beings that makes the development of a satisfying epistemology for mathematics extremely challenging. On the other, we are faced with an argument, the indispensability argument, that seems to establish mathematical Platonism on the basis of scientific practice itself. This makes the challenge to the naturalist to find a place for mathematical discourse in our total picture of the world especially pressing.

Huw Price (Price, 2011, pp.6-8) sees a number of possible responses to this kind of situation. As he sees it, we can reduce the terms of the suspect discourse to the terms of a discourse more acceptable to our naturalistic scruples (reducing the terms of folk psychology to those of a mature science of the brain, for instance). We can eliminate the suspect discourse all together – this strategy can be carried out in one of three ways: we might recommend replacing the naturalistically suspect discourse with some new, scientific way of speaking (for instance, replacing mental terms with the terms of a mature science of the brain); we might adopt some kind of fictionalist response; or else we might drop the discourse all together, agreeing that, after all, we were not talking about anything (perhaps this is what we did with witch discourse). Or finally, we might adopt non-cognitivism, that is, we might claim that the purpose of the discourse was not to make truth-apt statements about the world, and so the claims of this discourse should not be seen as conflicting with the claims of best science (which does try to make truth-apt claims about the world). Price himself goes on to defend a fourth approach, a global non-cognitivism tied to linguistic functional pluralism, that has the advantage of dissolving *all* the placement problems (Price, 2011).

As Price sees it, then, fictionalism is related to eliminativism (Price, 2011, p.7). That is, the fictionalist about some discourse agrees with the eliminativists that the relevant region of discourse is representational (it has the usual semantics of fact stating portions of our language), and that it is false. But the fictionalist parts company with the other eliminativists at this point, by denying that we need to abandon or replace the questionable discourse. Instead, we can keep the suspect discourse on the books, by recognising that the sentences of a discourse need not be true to be ‘good’. Obviously, the fictionalist will need to tell us what is ‘good’ about the relevant sentences, but so long as she can substantiate the claim that they play a vital role in our cognitive or practical lives, then we have reason *not* to abandon the relevant region of discourse.

The Platonist, of course, faces a particular challenge from the placement problem, for she holds that mathematical entities exist, and that further, these mathematical entities are *abstract objects*. But at first blush it is difficult to see how to fit our knowledge and talk of abstracta into our picture of ourselves as natural creatures in a natural world. For instance, Hartry Field (Field, 1989, 25-30) has challenged the mathematical Platonist to say how it is that we arrive at our knowledge of mathematical objects, given that they are abstract objects, outside of spacetime, and causally isolated from us. Given that our best scientific theories locate human beings within the spacetime world, with only evolved cognitive abilities appropriate to that environment, it is difficult to see what kind of epistemological account the Platonist can offer that will show how mathematicians are able to acquire reliably true beliefs about the abstract mathematical realm. This seems to place a particular pressure on the naturalist Platonist, who is willing to follow scientific theory as her guide to what the world is like, but also wishes to find a space for abstract objects in her ontology. (It is important to note that this challenge does not depend on any particular epistemological theory. For instance, the claim is not that knowledge is causal, and that therefore we cannot have knowledge of causally isolated abstracta (Benacerraf, 1983b). Nor, it seems to me, does Field's challenge depend upon a reliabilist epistemology, despite being framed in terms of reliably true belief. Instead, the challenge to the Platonist is to give a naturalistically satisfying account of how mathematicians are able to arrive at reliably true beliefs about mathematical objects, on the plausible assumption that failure to do even this much will vitiate any attempt to provide a plausible Platonist epistemology).

I suggest that, in the light of Field's challenge, the best bet for the Platonist is to establish her Platonism on the foundation of the indispensability argument. By doing so, she is able to respond to the epistemological and semantic worries for Platonism (the main source of placement difficulties) by insisting that we come to know about abstracta in much the same way that we come to know about theoretical objects, that is, via their role in our best scientific theories. Of course, this tells us very little about how the connection to either abstract objects or theoretical ones *is* established, but it makes it look more likely that worries like those giving rise to naturalist placement problems are based on an a priori commitment to semantic and epistemological theories that may also struggle with the entities of mature science.

Insisting on the indispensability argument also puts pressure on the fictionalist. After all, the fictionalist differs from the other eliminativists by insisting that false discourses, like mathematics, play an indispensable role in our cognitive and practical lives, *even though they are false*. But the indispensability argument suggests that, instead, we should conclude that these discourses are not false, they are true (confirmed in just the way in which all the other posits of our scientific discourse are confirmed: by playing an indispensable role in best scientific theory).

In the first main chapter, I will review some of the responses to this kind of indispensability argument, showing how concerns over some of its central assumptions (especially confirmational holism) create space for the fictionalist to deny that we should believe in the posits of a theory (and the truth of its sentences) *just because* they appear in our best confirmed scientific theories. I will then consider attempts to press the indispensability argument that do not depend on the assumption of confirmational holism. In particular, we will consider the explanatory indispensability argument. This is the claim that because mathematical entities appear in our best *explanations* of scientific facts, we should conclude that they exist (Baker, 2005; Baker and Colyvan, 2011). I will argue that attempts to substantiate mathematical truth or mathematical existence on the basis of the putative existence of mathematical explanations of physical facts must fail, because the facts to be explained were not and could not be identified independently of mathematical theory. I suggest that we should recognise a general constraint on theories of explanation, to the effect that explanations do not license ontological commitments when the theory doing the explaining is required to identify the phenomena to be explained.

This suggests that the Platonist's best bet was, all along, to insist on the indispensability of mathematics to scientific representation not scientific explanation. So in the second chapter I turn to the topic of mathematics' role in scientific representation. I begin in chapter 2 by outlining one particular response to the question of how mathematical-scientific representation works, the so-called mapping account (Pincock, 2004). According to this theory, the contents of mathematical-scientific representations are given by the existence of structure preserving mappings between the empirical domain under investigation and a mathematical structure employed in the representation. I will consider a number of objections to this account and argue that an enhanced version of the theory, drawing on some of the machinery of the inferential conception of applications presented by Bueno and Colyvan (2011), can withstand these objections.

But the resulting theory seems thoroughly Platonistic: it seems to commit us to the existence of mathematical structures and mappings at the least. This suggests a variant of the indispensability argument: suppose we are naturalists, then we are likely to think that philosophy is continuous with science. But that suggests that we should see the project of investigating scientific representation as *itself a scientific enquiry into the workings of natural science*. But then, as naturalists, it behoves us to believe in the posits of our best scientific theory of scientific representation. So we should believe in morphisms and mathematical structures and the like, as they are part of our best scientific theory of representation.

In the third chapter I give the fictionalist response to this argument. In short, the fictionalist should claim that when we set up a mathematical-scientific representation, we speak *as if* there are mappings of the relevant sort and *as if* there is a mathematical structure of the required kind for the representation to work (drawing on the work of Leng (2010), who in turn draws on the ideas of Walton (1990; 1993), we will see how set-theory with ur-elements forms the basis of this pretence). In other words, we should see the mapping account as providing *the content of a fiction*, the fiction of applied mathematics. It is this fiction that does the job of representing how things are with the natural world, but the nature of the fiction is settled by the mapping account: the mapping account is part of the story we tell when we engage in the fiction of applied mathematics. This modification will also enable us to respond to some lingering worries with how empirical structure is identified prior to mathematical application. In effect, the attribution of empirical structure is itself a part of the fictional pretence, albeit a part of the pretence that we hope to be getting approximately right.

In the preceding sections of chapter 3 I discuss the kind of fictionalism employed in this fictionalist account of applied mathematics. I begin by suggesting that Kendall Walton's theory of fictions as prop-oriented make-believe (Walton, 1993) provides us with the kind of account the mathematical fictionalist needs (though other kinds of fictionalism may do the job just as well). Walton's theory enables us to evade ontological commitment to abstracta while preserving the idea that in engaging in the applied mathematical fiction, we learn important information about the real world. The rest of chapter 3 is taken up with the discussion of revolutionary and hermeneutic fictionalism. I conclude that, for conservative reasons, having to do with hermeneutic fictionalism's susceptibility to empirical falsification, we should be revolutionary fictionalists. I conclude the chapter by claiming that the resulting form of revolutionary, prop-oriented, fictionalist mapping account undermines the indispensability argument from our best theory of mathematical applications. I conclude that if the original holist indispensability argument didn't work, the explanatory indispensability argument doesn't work, and the argument from our best theory of mathematical-scientific representation doesn't work, then the prospects for the indispensability argument look dim. In the light of this, fictionalism appears an attractive response to the placement problem.

Chapter 4 takes a more metaphilosophical stance. Recall that Price defends a global non-cognitivism as the appropriate response to the placement problems, suggesting that the naturalist should reject the assumption of 'representationalism' (Price, 2011, pp.10-11), the assumption that the function of language is to tell us how things are with the world. I argue that this poses a serious threat to the project in this thesis – that we have, in effect, been assuming representationalism throughout. Should this assumption prove faulty, then the whole idea of finding a place for mathematical entities within the scientific order would be misguided. It would not be the job of mathematical language to *tell us anything* about mathematical objects out in the external world (nor would this be the job of scientific language) and so the ontological problem lapses. In its place Price recommends an anthropological inquiry into the role and functions of this kind of discourse in our cognitive and practical lives (Price, 2011, p.199).

Now one consequence of combining anti-representationalism, as Price does, with functional pluralism about language is a kind of metaphysical pluralism or metaphysical quietism (Price, 2011, pp.34-53). There is no more to the existence of some entities than their being spoken of in some region of discourse, but no region of discourse is getting things more or less right about the world than any other. The result is that any discourse, so long at it serves some function in our cognitive and practical lives will be as good as any other, at least so far as its existential commitments go and at least as far as its descriptions of the world go. But this seems to sit uncomfortably with Price's insistence that he is a naturalist. Price's naturalism amounts to the claim that we should see human beings as entirely natural creatures existing in an entirely natural world and enjoying only natural relations with things in that world. But surely our only (naturalist) reason for thinking that is that it is the picture of ourselves and our world delivered by our best scientific theories. The worry is that Price's pluralism has relegated science to the status of just one more perspective, its commitments and descriptions no more authoritative for philosophy than those of any other discourse (Price, 2011, pp.30-32). But, then, why must we philosophers be constrained to view humans as entirely natural creatures existing in an entirely natural world and enjoying only natural relations with things in that world? Price seems to have undermined his own claim that science is authoritative for philosophy.

In the remainder of chapter 4 I suggest further replies to Price's anti-representational anti-metaphysics. Firstly, I consider one way in which an anti-representationalist concerned by Price's pluralist tolerance might resist metaphysical pluralism or quietism by rejecting functional pluralism. By insisting that all language serves one function (just not a representational one), the anti-representationalist can preserve the appearance of discord among different regions of discourse. The main purpose of this discussion is to bring out the important role the questionable premise of functional pluralism plays for Price in defending his anti-metaphysical stance. I conclude by suggesting that Price's position faces a number of challenges for further explanation that have plausible representationalist answers, but about which Price himself has little to say. I conclude that the advantage is with the representationalist, pending further elaboration of the theory by Price.

Chapter 5 continues the metaphilosophical themes turning to a discussion of anti-metaphysical stances arising out of the metaontology literature. I believe that a number of these kinds of attempts to substantiate anti-metaphysical intuitions, for instance, those tying metaphysical pluralism to differences in the meaning of certain words across languages face grave difficulties of formulation. Nonetheless, at least two of these kinds of metametaphysical deflationism, the epistemicism of Karen Bennett (2009), and Yablo's figuralism (1998) do seem to pose an especial challenge to the debate over mathematical Platonism and fictionalism.

We begin with an attempt to draft fictionalism itself into the services of metaontological anti-realism, namely Stephen Yablo's contention that fictionalism undermines the Quinean ontological project (1998). As Yablo sees it, the project of Quinean ontology, that is, looking to best science to determine our ontological commitments, depends upon a distinction between the literal and non-literal that, like Carnap's distinction between analytic and synthetic, cannot be maintained in the light of actual scientific practice (Yablo, 1998, pp.240-258). That is, when we look to the kinds of languages, like that of natural science, that the Quinean ontologist investigates to determine their ontological commitments, we notice the presence of large amounts of non-literal language. Yablo suggests that in some cases, it will be impossible to eliminate this non-literal language, and that, even worse, in some cases it may be difficult to establish whether a piece of language is intended literally or non-literally (Yablo, 1998, pp.257-258). Against Yablo I argue that the presence of non-literal language in science is no threat to Quinean ontology, and that the divide between literal and non-literal is sufficiently robust in scientific language to enable us to continue with the Quinean ontological project.

In the second half of chapter 5 we turn to Karen Bennett's epistemicism. This is the claim that metaphysical questions, like that at issue in the debate over mathematical ontology, are trivial, not for linguistic reasons, but because no evidence is ever available that will settle them one way or the other (Bennett, 2009, p.42). Bennett argues that certain structural features of these debates (what she calls 'difference-minimization') mean that the proponents of the two sides of the debate will do everything possible to move their theory as close as possible in ontology and expressive power to the theory of their opponents (Bennett, 2009, p.46). But, Bennett suggests, this will leave every objection to and strength of the opposing views applying equally to both (Bennett, 2009, pp.65-71). Against Bennett I argue that in the mathematical case at least, the distance between the rival views will remain sufficiently great to enable the existence of objections that apply to one and not the other. In particular I suggest that in the absence of a workable indispensability argument, Field's challenge remains a serious problem for the Platonist, and not for the fictionalist.

I conclude by drawing these strands of argument together. I suggest that what we have established is an attractive theory of mathematical application, that draws on Walton's prop-oriented make-believe and the mapping account to give us a satisfyingly detailed theory of applications, without Platonist commitments. As this theory does not seem to face any sort of epistemological challenge equivalent to that of Field's challenge to the Platonist, and as the Platonist's best hope, the indispensability argument, seems to have failed them, I argue that the theory developed in this thesis provides our best account of mathematical applications, and a satisfying response to the mathematical placement problem. I end by sketching some avenues of further research.

Chapter 1. Indispensability and Mathematics' Role in Scientific Explanation

1.1 Preliminaries

In the introduction we saw how the question of mathematical ontology can be seen as a species of what Huw Price (2011) calls a 'placement problem'. That is, the challenge is to find a way of reconciling our ordinary mathematical (and mathematized scientific) discourse, with an appropriately naturalistic view of human knowers and speakers. In particular, a realist must find some way of 'placing' the objects apparently spoken of within mathematical (and mathematized scientific discourse) within our best naturalistic account of humans and their relation to the world.

In the introduction I suggested, on epistemological grounds, that the best bet for the mathematical realist is to do this via the indispensability argument, which links our knowledge of mathematicalia to our ordinary scientific knowledge of reality. If this can be accomplished then the mathematical realist can respond to any epistemological worries by pointing out that our knowledge of mathematical objects is established in much the same way as our knowledge of the theoretical posits of natural science, that is, via their presence in our best scientific theories.

In this chapter I begin to look at ways in which a mathematical anti-realist might challenge this indispensabilist position. In particular, my aim is to establish the viability of a fictionalist response to the indispensability argument. I begin by briefly sketching some ways in which we might challenge the premises of the indispensability argument, and suggest that if we reject confirmational holism this leaves open the possibility of a fictionalist answer to the mathematical placement problem. Much of this work has been accomplished by others, and the significance of the failure of the original Quine-Putnam indispensability argument for the fictionalist line in mathematical ontology has been adequately treated of by Leng (2010), so my main focus in this chapter is a more recent variant of the indispensability argument, one that seeks to do without the controversial holist premise. This is the so called explanatory indispensability argument, which seeks to establish mathematical Platonism on the basis of the role of mathematics in scientific explanations. My preliminary aim, then, is to show how and why this new indispensabilist strategy fails to establish mathematical Platonism.

But the preliminary aim of defeating the explanatory indispensability argument is itself only a contribution to the wider dialectical strategy of the thesis. If I am correct, then no attempt to draw ontological conclusions from the role of mathematics in scientific explanations will be successful. (To be more precise: the fact that mathematical claims or mathematical objects themselves seem to do explanatory work will not establish mathematical realism. Mathematics may still play a *non*-explanatory role in mathematical explanations, such as indexing or some other representational role that *might* license ontological commitment to mathematicalia, even if the argument of this chapter is successful) I will take this to show that the recent focus of Platonists on the role of mathematics in explanation is misguided, and that the indispensabilist was better off focusing on mathematics' representational role. The next two chapters will follow up on this, presenting a novel naturalist argument for mathematical realism from the role that mathematics plays in theories of mathematical representation in science. The third chapter then attempts to deploy a fictionalist theory of representation in order to evade this realist challenge.

1.2 Resisting the Quine-Putnam Indispensability Argument

The aim of this section is to briefly present and discuss the influential Quine-Putnam version of the indispensability argument. (Much of the following material is influenced by Mark Colyvan's discussion of this topic (Colyvan, 2001a), and so it might be appropriate to label this the Quine-Putnam-Colyvan indispensability argument). We begin by examining the various 'moving parts' of the argument, its explicit and supplementary premises. The main focus of this dicussion will be on bringing out the role of confirmational holism in establishing the Platonist conclusion. I then move on to present some arguments drawn from the work of Penelope Maddy (1992) that suggest that the holist premise might be the weak link in the Quine-Putnam indispensabilist's argumentative chain. One aim of this is to display the way in which the failure of the confirmational holist premise of the Quine-Putnam indispensability argument is suggestive of a potential fictionalist response to the mathematical placement problem. But my main focus in this chapter will not be on following up this suggestion (we will return to the issue of fictionalism in chapter 3); instead my aim is to illustrate the motivation that some Platonist philosophers have had for developing a new kind of explanatory indispensability argument, one that eschews reliance on confirmational holism.

1.2.1 The Quine-Putnam Argument: Premises and Supplementary Premises

The original Quine-Putnam indispensability argument was an attempt to draw positive ontological conclusions from the application of mathematics within natural science. In brief, (1) our best scientific theories are laced with existential commitments to mathematical objects, and there is no hope of paraphrasing away those commitments so as to render our scientific theories nominalistically acceptable. Call this the *indispensability premise*. (2) But we ought to be *ontologically committed* to all and only those existential commitments of our best scientific theories. Call this *our criterion for ontological commitment*. (3) Therefore, we ought to be ontologically committed to the existence of mathematical objects. This, of course, is the Platonist conclusion.

In order to strengthen the argument we will add two further supplementary premises, labelled (2b) and (2c), in order to indicate the role they play in supporting our criterion for ontological commitment.

> (2b) There is no higher court of appeal in settling philosophical disputes than that provided by our best scientific theories. Call this *naturalism*.

About the *naturalism* premise I will have little to say, except to note that its role in the indispensability argument is to secure the "only" in "all and only those existential commitments" of premise (2).

Nonetheless, we might still suspect that we are entitled to disregard some of the existential commitments of our best science when it comes to settling ontological matters. Of course, we would not be so entitled if our only reasons for so doing originated in some prior first philosophy (a corollary of naturalism), but perhaps we might find good scientific reasons for taking a differential attitude to the commitments of our theories. The purpose of premise (2c) is to scotch any hope of picking and choosing our commitments in this way; in other words, (2c) establishes the “all” in (2)’s “all and only”.

> (2c) Confirming or infirming evidence does not attach to individual hypotheses of our scientific theories; instead, our theories face the tribunal of evidence as a corporate body.

Let us call premise (2c) *confirmational holism*. *Confirmational holism* prevents us taking a differential attitude to the commitments of our scientific theories by ensuring that evidence for one part of a successful theory (one posit of the theory) is equally evidence for another part of that theory (another posit of the theory). In the particular case of mathematics, *confirmational holism* prevents us from isolating the physical objects and mathematical objects that appear within the existential commitments of our best science and saying, “The evidence which confirms our theory goes this far (physical objects) and no farther (mathematical objects).”

As we have presented it then, the indispensability argument has just these working parts:

> (1) The Indispensability Premise
>
> (2) Our Criterion for Ontological Commitment (with supporting premises (2b) Naturalism and (2c) Confirmational Holism)
>
> (3) Conclusion: Platonism (or realism in ontology)

Now any one of premises (1), (2), (2b) and (2c) might be (and has been) challenged. A challenge to *the indispensability thesis* is presented in Field’s (1980). Field’s ambitious project involves the presentation of a nominalized version of Newtonian gravitational theory along with metamathematical results establishing the conservativeness of classical mathematics over Field’s nominalized physics. An assessment of Field’s project is beyond the scope of this chapter, but it is often thought that the choice of Newtonian physics as a starting point was somewhat fortuitous, and that Field’s nominalization project may well stall when we move beyond spacetime theories to phase space theories such as quantum mechanics (cf. (Malament, 1982). But see also (Balaguer, 1996a) for a more hopeful appraisal of the chances for a nominalized quantum theory).

However that may be, my reason for introducing Field's efforts here is that they are the paradigm of what Mark Colyvan (2010) has called a *hard road* to nominalism (hard for the sort of reasons just alluded to). That is, Field's is an example of a nominalization strategy that seeks to challenge the indispensability premise and restructure science in such a way as to make it nominalistically acceptable. Theories that endorse indispensability and provide no nominalistic reconstrual of mathematized science, which nonetheless refuse to countenance mathematical objects, are lumped together as *easy road* strategies. As we shall see, Colyvan thinks that explanation in science makes it difficult to keep these roads from converging (Colyvan, 2010).

1.2.2 Indispensability and Confirmational Holism

Of the routes left open to us, if we wish to slum it on the easy road and avoid the narrow way of nominalizing science, one especially has seemed attractive to philosophers in recent years. This is the challenge to (2c), *confirmational holism*. The hope is that if we can resist the charge to spread our evidence evenly over all the posits of our best science, then we can take a differential attitude to some of those posits, without having to abandon the thought that it is within science that we address ontological issues. That is, if we can show that something is wrong with *confirmational holism*, then we can still be naturalists, and endorse a modified version of premise (2), perhaps something like "we ought to be *ontologically committed* to only those existential commitments of our best scientific theories (but not necessarily all)". And the hope is that once we have adopted this modified, non-holist naturalism, the unwelcome Platonist conclusion will no longer follow.

We will not here review the various way in which confirmational holism might be challenged, but one promising strategy, introduced by Penelope Maddy (1992), is to note the prevalence in our (current) best scientific theories of *explicitly false hypotheses*. For instance, we might work in physics with pendulums on frictionless pivots or waves in an infinitely deep sea. The idea that these idealizations might be dispensed with in our best science is itself often nothing more than an idealization. Quine seems to have believed that any idealization involving explicitly false hypotheses would be paraphrased away in our 'best science', where 'best science' seems to have meant something like 'science *at the limit of enquiry*'. But there are at least two reasons for thinking that this is unlikely and that some scientific fictions may prove resistant to paraphrase. The first is that 'best science', even the explicit idealization 'best science *at the limit of enquiry*' is still best science for creatures like us, human beings with our cognitive capacities and our cognitive limitations. If the resultant paraphrase is unmanageably complex – if we cannot calculate, cannot predict nor explain with it – then the theory will surely not be part of *our* best science. But it was *our* best science that was to determine *our* ontological commitments, not some idealized science available only at the limit of enquiry and useful only for superhuman intellects (Maddy, 1992, pp.281- 282).

Secondly, it may well be that there are theories for which we know of no procedure for paraphrasing away our fictional commitments. The idea of paraphrase gets traction only when we know of some background theory, more accurate than our explicitly false one. But what if the falsehood is part of our *most fundamental theory* (Maddy, 1992, p.282)? Newtonian mechanics, in its day, was surely such a fundamental theory. But Newtonian mechanics treats of forces acting on point masses. What are point masses? Certainly not physical things we might bump into, out there in the world. It is at least not obvious that point masses should ever have been considered part of scientific ontology, even if there was no more accurate theory than Newton's within which they could be dispensed with. Of course, one could object that Newtonian mechanics is no longer our *best* theory. But can we be certain that no explicit falsehoods have crept into those theories we now *do* regard as our best? And should we discover any, then we would have no more basic theory to appeal to in order to translate away our explicit falsehoods (Maddy, 1992, pp.181-282). We would simply have to mark them as false and withhold commitment, and the only *consistent* way of doing that would be to reject confirmational holism.

I will say little more on this issue here. For further elaboration I direct the interested reader to (Maddy, 1992) and for a defence of the idea that mathematical objects may well function like fictions to (Leng, 2010). For now we simply note the result for the original indispensability argument. We have seen reason (the presence of explicitly false hypotheses in our best science) to reject premise (2c) of the indispensability argument. With *confirmational holism* out of the way, we have opened up the possibility that not all the hypotheses of our best science feature in our theories *because they are true*, and that therefore we need not conclude that some entity exists merely because it is quantified over in such a hypothesis. In particular, we have opened up the easy road of fictionalism, the claim that the mathematics featuring in our best science need not be true to be useful, but that it may serve some other function (such as enlarging our descriptive resources), for which its truth and the existence of its posits is a matter of indifference.

1.3 The Explanatory Indispensability Argument

In this section we begin our investigation of the explanatory version of the indispensability argument. The strategy behind this dialectical shift, from the more generic indispensability claim to the particular role that mathematics plays in explanation, has been, on the one hand, to find a version of the indispensability argument that is independent of the controversial holist premise that we saw challenged in the previous section, and on the other, to press against the nominalist a kind of abductive argument form that she is supposed to see as part of good scientific method (and, therefore, of good philosophical method too).

We begin by reviewing the motivations for this shift to the explanatory version of the indispensability argument. I then present the argument in a form similar to the original Quine-Putnam argument examined above. In the next section we begin the evaluation of this new Platonist strategy.

1.3.1 Motivating the Explanatory Strategy

It is against the background of challenges to *confirmational holism* that the explanatory indispensability argument is introduced.[1] If *confirmational holism* is the weak premise, then defenders of mathematical Platonism will have to find an alternative that does without it. The explanatory indispensability argument is precisely such an alternative, non-holist argument for Platonism. It avoids the reliance on holism by focusing specifically on the role that mathematics plays in scientific explanation. In short, the claim is that even if it is not true that favourable empirical evidence confirms all the parts of our theories equally, it does confirm a theory's explanatory portions. And so, if a mathematical entity is playing an explanatory role (an *indispensable* explanatory role) in a scientific theory, then that entity must exist.

The explanatory indispensability argument has seemed an especially attractive move for the Platonist, because it gets most traction when directed at those nominalists who wish to remain scientific realists (or else realists about the non-mathematical parts of science). The strategy here is to point out to the scientific-realist-naturalist nominalist that they are ordinarily happy to endorse inference to the best explanation. Constructive empiricists and other scientific anti-realists might be more suspicious of IBE as an argument form (cf. Van Fraassen, 1980), but the indispensability argument is, anyway, directed at naturalists, who tend to be realists about science and its methods. Supposing IBE to be part of the normal methods of best science, the nominalist scientific-realist surely ought to endorse IBE in general.

But now, the Platonist hopes, if it can be shown that mathematical claims play an indispensable *explanatory* role in scientific explanations of some phenomenon/phenomena, then the nominalist will be forced onto one of the horns of the following dilemma: either reject the idea that an explanation must be *true* to be good (and run the risk of abandoning scientific realism as a consequence) or else argue that the examples offered in the literature of mathematical explanations in

[1] We have considered only Maddy's critique of this doctrine, as being the most congenial to the fictionalist position defended in this thesis. Other anti-holisms are available. For instance, Elliott Sober (1993) has used his contrastive empiricism to motivate a challenge to the doctrine of confirmational holism, arguing that unless there is some alternative kind of mathematics that might be deployed in scientific practice, then mathematized science cannot be tested against an opposing hypothesis. This is because the mathematics made use of in an empirically tested scientific theory, being common to all scientific theories, will also be a part of the theory it is tested against. This, according to Sober rules out seeing the empirical test of a scientific theory as confirming or disconfirming its mathematical content.

science are not good explanations after all (and so run the risk of abandoning naturalism, by questioning the deliverances of our best scientific theories). If the nominalist wishes to choose neither horn, then her only option seems to be to abandon her nominalism.

Our focus in this thesis, though, is not nominalism in general, but fictionalism in particular. And, of course, fictionalism, strictly speaking, is itself a brand of anti-realism about science. The official line is that the fictionalist does not believe her best scientific theories, but she does believe them to them be nominalistically adequate, where nominalistic adequacy is, in a rough and ready fashion, getting it right about the concrete stuff (Balaguer, 1998, p.131; Leng, 2010, p.180). So the fictionalist cannot accept unrestricted application of the principle that we should believe our best explanations to be true, because she is already committed to the existence of very many explanatory theories that are not true (they are merely nominalistically adequate), but which are nonetheless good explanatory theories. But the argument just sketched will prove to present a challenge to the fictionalist just as surely as it does to the nominalist-realist. In short, the challenge is to say what licenses the fictionalist's differential treatment of explanations. We will return to this in a later section.

1.3.2 The Explanatory Indispensability Argument: Premises and Conclusion

Now that we have seen the motivation for it, we should turn to the argument itself (see Baker, 2009, p.613). One premise of the argument we have encountered already, in the form: an explanation must be true to be good. That is, if a claim explains some phenomenon, and if that explanation is the best available, then we should believe that that claim is true. And of course, if such an explanatory claim requires for its truth the existence of some object, then that object must exist. Putting this in language which brings out the relation of this first premise to the project of establishing our ontological commitments, premise one will read: (1) We ought to be ontologically committed to those objects appearing in our best scientific explanations.

Premise two then seeks to build on this, by establishing the existence of mathematical explanations of scientific phenomena: (2) There exist mathematical explanations (requiring existent mathematical objects for their truth) of phenomena of interest to our current best science. This claim will be the topic of the next section.

Therefore, (3) we ought to be committed to the existence of mathematical objects (Baker, 2009, p.613).

And so we see that the explanatory indispensability argument has just these working parts.

> (1) We ought to be ontologically committed to those objects appearing in our best scientific explanations.
>
> (2) There exist mathematical explanations (requiring existent mathematical objects for their truth) of phenomena of interest to our current best science.
>
> (3) We ought to be committed to the existence of mathematical objects.

Once more we have a choice of premises to challenge, and perhaps neither one is *obviously* true. Nonetheless, proponents of the explanatory indispensability argument have tended to take it as given that (1) holds and that therefore it is premise (2) that requires defence and supplementation. In order to establish premise (2) what is required are real world examples of mathematical explanations of physical phenomena. And so it is to case studies of apparent mathematical explanations of physical phenomena that defenders of the argument have tended to turn.

1.4 A Case Study: Baker Among the Cicadas

We saw in the last section that defenders of the explanatory indispensability argument have tended to assume that it is premise (2) of their argument that will be found controversial. This has motivated them to find and display examples from contemporary science of mathematical explanations of biological, chemical, physical, and social phenomena. In this section we introduce one such example, the life-cycle periods of two sub-species of periodic cicada, the explanation of which is supposed to make indispensable use of certain theorems from number theory. It is important to note in advance that it is the number theoretic results themselves (relating to some characteristics of prime numbers) that are taken to do the explaining; the number theoretic theorems are not, according to Baker, just playing a descriptive/representational/indexing role (Baker, 2005). As we saw in the previous section, if it is the case that these number theoretic results are pulling the explanatory weight, then we ought to believe that they are true. And because their truth would require the existence of prime numbers, we seem to have here a compelling argument for mathematical Platonism. The remaining sections of this chapter will provide reasons for thinking that examples like these *will not*, contrary to appearances, support mathematical realist conclusions, and we will examine the consequences of this for the continuing debate over the issue over mathematical ontology.

1.4.1 A Mathematical Explanation for the Life-Cycle Periods of Periodic Cicadas?

In this section I present an example of a supposed mathematical explanation of a physical phenomenon. The example is taken from evolutionary biology, and was introduced to philosophers by Alan Baker in his paper *Are there Genuine Mathematical Explanations of Physical Phenomena?* (2005, pp.229 -233).

Baker's case study involves what appears to be a number theoretic explanation of a biological fact, the prime-numbered life-cycle of the North American *Magicicada* cicada. These are insects, most of whose lives are spent in a nymphal stage within the soil, emerging periodically after 13 and 17 years (depending on the species) as adults. The emergence of the adults is synchronized, with all the adult insects emerging within a few days. The adult stage then lasts only a few weeks (during which time the insects mate, and lay their eggs), after which the adults die off, the emerging nymphs burrow back into the soil, and the cycle starts over. There are, of course, many questions biologists might ask about these curious creatures, but one seems especially relevant for our purposes: *why life-cycles of 13 and 17 years?* In other words: *why the prime numbered life cycles?* Baker points out that this is a genuine question biologists *are* interested in, and that the best answer (the best explanation of the phenomenon) makes essential use of number theoretic results.

Our presentation of Baker's case study must needs be somewhat hasty and for a fuller presentation I direct the reader to (Baker, 2005). Here I will give the structure of the argument, as presented by Baker and then try to fill in some of the details. Here is the argument:

> **(1)** Having a life-cycle period which minimizes intersection with other (nearby/lower) periods is evolutionarily advantageous. **[biological law]**
>
> **(2)** Prime periods minimize intersection (compared to non-prime periods) **[number theoretic theorem]**
>
> **(3)** Hence organisms with periodic life-cycles are likely to evolve periods that are prime. **['mixed' biological/mathematical law]**
>
> (Baker, 2005, p.233)

Premise (1) as it stands requires some explanation; what are the nearby/lower periods the cicadas are supposed to be avoiding? Baker notes two different hypotheses making use of this biological law, one based on the hypothetical presence of predators in the *Magicicada's* evolutionary past, the other on the existence of similar species of insects with which the emerging adults might have hybridized. If, in either case, the hypothesized species itself had a periodic life-cycle, its emergence from the soil may well have coincided with that of the adult cicadas. Now, the adult stage of the cicada life-cycle is very short (compared with the nymphal stage) and opportunities to mate are correspondingly restricted. Given these biological restrictions, avoiding mating with a similar species could well have conferred an evolutionary advantage. If the hybrid offspring of coinciding species had life-cycle periods differing from those of their parents, then the emergence of the future adult stage might no longer synchronize with the other cicadas, thus reducing the likelihood of future mating. The evolutionary advantage of not getting eaten, for a creature with such limited opportunities to mate, hardly needs elaborating on.

Premise (2) depends upon two results from elementary number theory given by Baker as two lemmas.

Lemma 1: The lowest common multiple of m and n is maximal iff m and n are coprime.

Lemma 2: A number, m, is coprime with each number $n < 2m$, for $n \neq m$ iff m is prime. (Baker, 2005, p.232)

Now suppose m and n are lengths in years of the life-cycle of some periodic species, M and N respectively. Then, further supposing the emergence of M and N at some point of their life-cycle to have coincided in some year, the next year in which they will coincide is given by the lowest common multiple of m and n (that is, the lowest number such that both m and n will divide into that number). Now, this taken together with lemma 1 implies that the period of intersection will be greatest when m and n are coprime. Lemma 2, together with the assumption that the cycle periods of the hypothesized species, n, are low, generates the primeness result we wanted: prime life cycles will tend to reduce intersection with other periodic species.[2]

The argument presented so far establishes the general conclusion that animals with a periodic life-cycle are likely to develop life-cycle periods that are prime. A further premise (4), together with the preceding argument establishes the particular conclusion that the cicadas are likely to develop 17 year life-cycle periods:

> **(4)** Cicadas in ecosystem-type, E, are limited by biological and environmental constraints to periods from 14 to 18 years. **[ecological constraint]**
>
> **(5)** Hence cicadas in ecosystem-type, E, are likely to evolve 17-year periods.
>
> (Baker, 2005, p.233)

With (5), the conclusion, following from (3), the 'mixed' biological/mathematical law, and (4), the ecological constraint. (It should be clear how the preceding argument can be varied so as to establish the corresponding result for the 13 year species).

What we have then is a prediction that cicadas in a certain geographical location, with certain biological and ecological constraints, will tend to evolve life-cycle periods of certain lengths, in part *because* those lengths are prime (Baker, 2005,

[2] This argument will not work for the hybridization case, because the cycle periods for similar species are likely to have been similar, But the underlying number theory is the same. For further details of how the argument must be modified see (Baker, 2005).

p.233). Had we not known the fact in advance, we could have used the above argument to predict the existence of such species. In other words, the argument shows how the phenomenon was to be expected. This *to be expectedness* of the phenomenon, once we have seen the putatively explanatory argument, is often taken to be a mark of scientific explanations. So this would appear to be a successful scientific explanation. And, moreover, it seems to be a scientific explanation making indispensable use of mathematical facts (the two number theoretic lemmas); indispensable not in the sense that they are essential representational aids, but in the sense that they themselves do the explanatory work.

My aim in this chapter is not to challenge the cogency of Baker's example. For now I am willing to grant that this is a good explanation of a genuinely puzzling biological phenomenon.[3] Nonetheless, it remains to be seen whether this explanation or anything similar commits us to the existence of prime numbers. This is the question I address in the remaining sections of this paper.

1.5 Resisting the Explanatory Indispensability Argument

In this section we turn to the evaluation of the explanatory indispensability argument, using Baker's cicadas example as a foil. While my focus will be on the way in which Baker's argument fails to establish Platonism, the arguments of this section are supposed to be more general. The point is not that Baker's particular example fails (which, after all, would only serve as an invitation to Platonistic philosophers to provide new examples), but that no attempt to establish Platonism via the role of mathematics in scientific explanation is likely to be successful. Here I draw on an argument of Bangu (2008, pp.16-17), who points out that the explanatory indispensability argument assumes the truth of 'mixed' *explananda* (explananda with

[3] It has been pointed out to me in discussion that the argument has one suspicious feature. Remember that the general conclusion of the argument, independent of its application to the magicicada species was: "organisms with periodic life-cycles are likely to evolve periods that are prime." In other words, prime life-cycles are evolutionarily advantageous *for all periodic species*. If that is correct, then prime numbered life-cycles ought to be common across all periodic species, not just these two species of magicicada. But the phenomenon of the periodic cicadas with prime life-cycles was taken to demand explanation just because it was so striking, so unexpected. And it surely would not be striking or unexpected if it was found in *all* periodic species.

both mathematical and scientific components, like "the life-cycles of these and those species of cicada are *prime numbered*") and so presupposes the truth of the mathematics whose truth was supposed to be established by its appearance in the *explanans*. The argument I hope to sustain will, for reasons given in their proper place, be slightly different to Bangu's, but the essential drift of my response is similar: mathematical explanation of mathematised scientific descriptions of the world will always evince a kind of circularity that rules out drawing ontological conclusions, however good the explanation seems to be. In the concluding section of this chapter I argue that as a result of this, those seeking to defend Platonism via the indispensability argument should return their focus to the role that mathematics plays in scientific representation.

1.5.1 But Why Worry?

So far we have seen that under pressure from nominalist challenges, those wishing to defend a Platonistic construal of the indispensability of mathematics have sought to reach their conclusions via the use of mathematics in scientific explanations. In particular Platonists like Colyvan and Baker have argued that some uses of mathematics in science are explanatory *in and of themselves*. As good scientific realists, with a healthy respect for inference to the best explanation, it then behoves us to treat such explanatorily essential mathematics as true and, in particular, to regard the existence claims made by such explanatorily essential mathematics to be true.

How might we resist this argument? Well, we have seen that premise 2 is unlikely to be at fault (at least we are conceding this much for the present). We good scientific realists are very often also right minded naturalists, and as such we are unlikely to want to challenge the considered judgements of scientific practitioners about which explanations are and which are not good.[4] If the arguments of the preceding section

[4] It is a ticklish matter just how far naturalists are permitted to challenge the consensus judgements of the broader scientific community. Of course, one does not wish to claim that scientists are infallible; presumably methodological mistakes can be (and are) made. Nonetheless I take it as an article of good philosophical practice that explanations and theories taken to be good by the rest of the scientific community are not to be challenged by philosophers *just because* they seem to have unwelcome ontological consequences.

are sound, then we seem to have at least one good example of an explanatory application of mathematics in science. So Premise 2 of the explanatory indispensability argument looks to be comparatively robust.

But what of premise (1)? Remember that (1) claimed that "We ought to be ontologically committed to those objects appearing in our best scientific explanations." Have we been given any reason to think that this is true? Superficially it might seem no more difficult to resist this claim than the corresponding premise of the Quine-Putnam indispensability argument. Remember that (1) was found to rest on the assumption that any explanation must be true in order to be good. Well then, we might simply ask the Platonist, *Why* must good explanations always be true explanations? Couldn't the explanatory role of the mathematics deployed in scientific explanations be underwritten by some feature of the mathematics other than its supposed truth?

Now it might be thought that the naturalist, as a scientific realist, requires a blanket commitment to inference to the best explanation in order to independently motivate some kind of abductive 'no miracles' argument for scientific realism. I do not believe (though I cannot argue for this claim here) that naturalists need any kind of argument for scientific realism, so the naturalist nominalist will not acquire a blanket commitment to the claim that all good explanations are true explanations from this direction. Nonetheless, what is needed is some kind of argument that the naturalist cannot distinguish between kinds of explanation in a way that would enable her to say that some explanations can be good without being true, while the goodness of some other explanations does establish their truth. The aim of this section is to examine what reasons there might be for thinking this.

Perhaps the most explicit statement of the idea that explanations *must* be true in order to be good has come from Mark Colyvan. In his (2010) Colyvan argues that any explanation in science must be literally true or else eliminable in favour of a literally true explanation. Of course, we often use metaphor when we provide explanations, but, Colyvan contends, such metaphorical explanations go proxy for a literally true alternative (Colyvan, 2010, pp.300-301). Colyvan is willing to concede that we may not always know how to provide such a literally true replacement in practise. For instance, if we say that such and such an individual lost his job because the stock market crashed, we do not necessarily know how to provide a literally true alternative explanation that makes no use of the metaphorical term 'crashed'. Stock market crashes are very complex events and it may just be that we have no idea how to translate 'the stock market crashed' into literally true economic theory. But, Colyvan insists, we must at least have a *partial* translation, in terms of economic theory, which we can substitute for our metaphorical explanation in terms of crashes (Colyvan, 2010, p.301).

At this point one might wonder whether such partial translations are actually available (and indeed how the possession of a partial translation of a non-literal explanation relates to what seems to be Colyvan's belief that, *in principle*, fully literal explanations are always available, even if in practice we cannot find them. Surely having slightly less metaphorical explanations doesn't in itself provide evidence that fully de-metaphored explanations exist?) The only suggestion Colyvan offers for a partial literal translation of the stock market example talks of "many industry sectors being placed under financial stress and that this was the motivation for the change of career of the person in question."(Colyvan 2010, p.301) It is at least not obvious that this talk of "financial stresses" is less metaphorical than the original talk of crashes. So we seem to have no reason at this point to believe that such translations are even *possible* in most cases.

For now, I will leave this last point hanging. Colyvan has given us no reason to believe that literal replacements for every metaphorical explanation *can* be found. But for the sake of argument, I will grant Colyvan this claim. Nonetheless, we can continue to press Colyvan. Even supposing that Colyvan is correct that literal replacements for our metaphor-involving explanations are available, why *must* we provide them? Not, presumably, just because it can be done? Colyvan seems to be working with the assumption that the truer explanation will always be the better explanation. And if that is the case, then when we are confronted with some apparently metaphorical explanation, we have two options available: either we can paraphrase away the putatively metaphorical portions of our explanation, replacing them with a fully or partially de-metaphored alternative; or else, we simply conclude that the metaphorical portions of the explanation were not metaphorical at all, but were in fact true and literal claims all along. But what reason is there for thinking this? I believe that an answer to this question will emerge from an examination of the debate between Colyvan and Leng over structural explanations in science.

1.5.2 Structural Explanations and Colyvan's Slippery Slope

In this subsection I want to bring out what I think is the best reason for thinking that an explanation must be true in order to be good. By examining Colyvan's response to Leng's nominalist line on mathematical explanations, I think we can develop a challenge for the fictionalist: the challenge to say what it is about the distinctively mathematical explanations that rules them out as candidates for IBE style arguments like the explanatory indispensability argument. When we return to the Baker case study, we will see a way of responding to this challenge that should enable us to disarm the explanatory indispensability argument.

In her (2012) Leng concedes that there may well be explanatorily essential uses of mathematics and that, moreover, the mathematics itself may be bearing a significant portion of the explanatory burden. This is because, in certain cases, the mathematics enables us to provide a "structural explanation" (Leng, 2012, p.988). Structural explanations work by showing how the phenomenon under investigation results from general structural features of the physical/natural situation, and these structural features are, in the interesting cases, mathematical. So, for instance, in the cicadas case we see how general structural features of the natural numbers (the lemmas concerning coprimeness) account for the observed life-cycle periods. In brief, Leng's suggestion is that we link the explanatory mathematical structure with the physical domain under investigation by providing an interpretation of the axioms of the mathematical structure in terms of the physical stuff. We then see how the relevant explanatory result follows from the axioms characterizing the mathematical structure and the interpretation we have given to those axioms (Leng, 2012, pp.988-989).

Leng goes on to suggest that the explanatory role played by mathematics in these kinds of structural explanations may well be an indispensable one. We can perhaps see this best if we consider the result of trying to provide a nominalistic reconstrual of a particular structural explanation. We can, for instance, provide explanations of the life-cycle periods of *each* of the *magicicada* species in turn that are nominalistically unobjectionable (cf. Saatsi, 2011, p.150), but what we lose in the switch to the nominalistic alternative is the ability to provide one single explanation of why *both* species have the life-cycles they have. In other words, by taking the mathematics out, we lose the ability to see how the phenomenon in question results from perfectly general structural features of the empirical situation. And, further, by demathematizing the explanation we lose the ability to see how other similar phenomena result from *exactly the same* general structural features. What the nominalistic explanation lacks is generality and generalizability; in other words, it explains less.

But Leng goes on to argue that none of this requires us to assume the existence of the mathematical entities featuring in the structural explanations (Leng, 2012, p.991). In a structural explanation we have, on the one hand, a set of axioms presumed to correctly characterize some mathematical structure *and* an interpretation of that structure in terms of some non-mathematical stuff. That is, we claim that the axioms of our mathematical structure are (approximately) true of the non-mathematical domain – the lemmas concerning comprimeness are true of the life-cycle periods of cicadas when numbers are years, for instance. But none of this seems to require numbers themselves. It requires years and it requires that years exhibit some of the structure of numbers (or some of the structure numbers would have, were there any), but the numbers themselves do not seem to be playing an essential role in the explanation. It is the (presumably false) structure characterizing axioms given an (approximately) true interpretation in terms of (presumably nominalistically kosher) years that ultimately do the explaining. No need then for mathematical objects (Leng, 2012, pp.990-992).

Now if this is correct, then we cannot draw positive ontological conclusions from the role of mathematics in scientific explanation. But Colyvan presents the fictionalist attracted by this line of reasoning with a challenge. In essence, Colyvan's challenge is to say what motivates the fictionalist's scepticism about mathematical explanation, given that she is not a thoroughgoing scientific anti-realist. Inference to the best explanation appears to be ubiquitous in science and if we are going to start doubting its efficacy when it delivers unwanted mathematical objects into our ontology, why trust it when its deliverances are more favoured unobservable physical things? As Colyvan puts it

> Leng seems to have mathematical explanations singled out for special treatment. When an explanation invokes mathematical objects, Leng advises us not take the ontological commitments of that claim seriously, just read it as a claim about the structural properties of the physical system we are interested in. (Colyvan, 2012, p.1037)

He then asks, "But why stop there? Why not follow van Fraassen (1980) in reassessing apparent explanations involving unobservables?" (Colyvan, 2012, p.1037) His point is that we are in the vicinity of a slippery slope:

> If the aim is to advance a nominalist account, while remaining a scientific realist, more needs to be said about why mathematics is singled out for such special treatment and why the special treatment stops there. (Colyvan, 2012, p.1037)

The line of argument is clear: if we are going to start rejecting inference to the best explanation in some cases, then we need a principled reason for drawing the distinction where we do. In the absence of such a reason, our scepticism is unmotivated.

It is important to recognise that this is not, strictly speaking, an objection to Leng's position. It may well be that there are features of structural explanations involving mathematics to which Leng could point that will not carry over to explanations involving unobservables. After all, mathematical structural explanations make no essential appeal to causal processes or mechanisms, whereas many explanations invoking unobservables do. However that may be, it is clear that Colyvan is on to something: the mathematical fictionalist does need to say *something* about why it is that she privileges inference to the best unobservable explanation above inference to the best abstract explanation. If the fictionalist does not have a principled way of drawing this distinction, then it looks as though she is operating with unacceptable double standards.

More explicitly, Colyvan's challenge is to find some principled way of carving up the domain of explanation such that some explanations (hopefully the explanations in terms of abstracta) end up without ontological import, without abandoning inference to the best explanation entirely. In the absence of any principled way of achieving this, we would be forced to accept all (scientifically) respectable explanations as having the same kind of epistemic authority. And as I read him, Colyvan is betting that the nominalist scientific-realist will not be able to make good on the challenge to say why abstract explanation is not ontologically committing while explanation in terms of unobservables is, that is, she will be unable to find a reasonable principle of demarcation. If all that is correct then it looks as though the fictionalist cannot reject the particular instance of abductive reasoning embodied in the explanatory indispensability argument without endangering her scientific realism. And if Colyvan is correct in his presumption that the nominalist will not be able to meet his challenge, then we can see why he believes that only true explanations can be good explanations (or that all good explanations must be true).

1.5.3 Bangu's 'Question-Begging' Objection

What we are after, then, if we are interested in denying premise (1) of the explanatory indispensability argument, is some principled way of carving up the domain of explanations such that mathematical explanations fall on the ontologically harmless side. In this section I discuss an argument of Sorin Bangu (2008), which I believe suggests a route to this goal.

Bangu begins his objection to the explanatory indispensability argument by noting two essential presuppositions of what he calls the "inference to the best explanation strategy". The first of these presuppositions is that the explanandum be true (Bangu, 2008, p.16). If the explanandum featuring in some explanation is not true, then there is no reason to demand a true explanans for it. If we were to explain some false or fictional explanandum in terms of some equally false or fictional explanans, we would have committed no sin against the canons of proper explanation. So, for instance, if I explain Sherlock's torpor in terms of his drug addiction and his lack of an interesting case, I am not committed to Sherlock's drug addiction nor his lack of an interesting case. And I am not so committed because I was never committed to Sherlock or his torpor in the first place.

The second presupposition of the IBE strategy that Bangu notes is that the explanandum be independent of mathematics.[5] Inference to the best explanation based on a mathematical explanation of a mathematical explanandum would simply beg the question against the nominalist (Bangu 2008, p.16). The nominalist doesn't believe in the truth of any mathematics. And as we have already noted, a false or fictional explanandum doesn't require a true explanans. So the explanandum had better not just be another piece of mathematics, on pain of begging the question against the nominalist.

Now Bangu goes on to clarify what exactly it is that Baker's explanation explains. He notes an ambiguity in Baker's presentation. Baker begins by asking the question, "Why are the life-cycle periods prime?" (Baker, 2005, p.230). But Baker also suggests that the phenomenon to be explained is just "the period length of cicadas." (Baker, 2005, p.233). Now Bangu argues that of these two explananda, it is the former that is more basic.

> …once we know that the period has to be prime – i.e., once we have answered the first question – the number 13 [the life-cycle period in years of one of the species of cicada] comes out as the only acceptable answer. So, the first question "Why are the life-cycle periods prime?" is more basic and the central question to answer. (Bangu, 2008, p.17)

I certainly do not disagree with this, but I believe there is a stronger argument for the claim that it is the phenomenon of the *primeness* of the life-cycles that demands explanation. Were the phenomenon in question just the period length (either 13 or 17), it is difficult to see why anyone would have bothered looking for an explanation at all. Primeness aside, these seem like fairly unremarkable numbers and not at all in need of the kind of sophisticated explanatory attention they have received. So I

[5] Baker himself insists on this as a requirement of the explanatory indispensability argument, and his reasons for doing so are much the same as Bangu's.

contend that we should follow Bangu in regarding the most basic fact in need of explanation as the *primeness* of the life-cycle periods.

But if this reasoning is correct, the rot immediately sets in. As Bangu points out, the fact we are now explaining is that a certain number (that associated with the life-cycle period of a certain species of insect) has a certain mathematical property, namely, the property of primeness. Is this claim, this ascription of a mathematical property to a number (itself associated with some natural phenomenon), really independent of mathematics in the way that Baker requires? It would seem not; attributing primeness to some number looks to be a mathematical claim if ever there was one. But now remember the first presupposition Bangu identifies: in order to require a true explanans, the explanandum must be true. But if the explanandum is not sufficiently independent of mathematics, there is no need for the fictionalist to regard it as true, given her antecedent scepticism about mathematical claims. And so, in particular, there is no need to regard Baker's number theoretic explanation as true (and no need to adopt its existential commitments as our own) (Bangu, 2008, pp.17-19).

In other words, Bangu is objecting that Baker's particular version of the explanatory indispensability argument begs the question against the nominalist, by simply assuming the truth of precisely those kinds of mathematical claims that the argument is supposed to establish. And it is not at all clear how the explanatory indispensabilist might evade this challenge. As we have already noted, he must assume the truth of the explanandum or else there is nothing requiring a true explanans. And that means that the only option left for the indispensabilist is to find a scientific phenomenon whose literal truth would not require the existence of mathematical objects, but which can be explained by some piece of mathematics. But there are at least two reasons for thinking this challenge will not be easily met. In the first place, the (regular, non-explanatory) indispensability of mathematics to natural science makes it difficult to imagine from what branch of science a non-mathematized explanandum might be drawn. In the second, it is difficult to see how an explanandum making no mention of mathematics could essentially require mathematics for its explanation.

Bangu ends his paper by suggesting that this may be one instance of a much more general failing in the IBE strategy (Bangu, 2008, p.19). That is, all attempts to employ the explanatory indispensability argument are going to require presupposing the truth of the explanandum, and if every example the Platonist can come up with involves mathematics featuring in the explanandum in just the same way as Baker's example, then it looks as though no attempts to employ the explanatory indispensability argument are going to be effective. In other words, every instance of an explanatory indispensability argument is going to beg the question against the nominalist, whatever examples are deployed in support of it.

1.5.4 The No Circularity Condition

In this section I want to suggest a way in which I believe Bangu's argument can be adapted to our purpose of finding a principled reason to distinguish between the ordinarily ontologically committing explanations of our best science, and the ontologically harmless explanations appealed to in the explanatory indispensability argument. In effect, I will claim that Bangu's question begging argument has uncovered a circularity in the kinds of explanations appealed to in the explanatory indispensability argument that gives us good *scientific* reason to be suspicious of their ontological credentials.

Consider, to begin with, the following example. Quark theory includes a posit called a gluon. The gluons play a role in Quantum Chromodynamics (the beguilingly named theory of the strong force governing interactions within hadrons) similar to that of the photon in Quantum Electrodynamics; that is, they are gauge bosons, or 'force carrying' particles, mediating the strong force in interactions between quarks. Now the existence of quarks was, for a while anyway, a matter of debate within the scientific community, because the behaviour of the strong force (it increases very rapidly as a function of distance) made the detection of free quarks (quarks outside of a proton or neutron) impossible. Now suppose at this time some enterprising particle physicist had had an unfortunate bout of philosophy and argued thusly: "Quarks must exist! They are part of our best possible explanation of the behaviour and properties of gluons!" I cannot imagine that the scientific community would have been terribly impressed. And the reason they would not have been impressed seems relatively clear: gluons are a posit of the very theory taken to do the explaining. The fact that they are well explained by that theory gives us no reason to believe it, because we would only be interested in the explanation if we had independent grounds for believing in gluons. That is, the explanandum is not independent of the explanans.

Consider another example. Tarot must be true, because it correctly assigns the appropriate interpretation to certain arrays of playing cards. Again, the interpretation of arrays of playing cards just is part of the theory (if I may call it that) of Tarot. We would only accept the explanans (It gets the correct interpretations of arrays of cards) if we had a prior reason for thinking that Tarot was true. The explanans just isn't sufficiently independent of the explanandum to license inference to the best explanation.

What I think these two examples suggest is that the weak link in the explanatory indispensability argument is the requirement that the explanandum be independent of the explanans. In the two examples, the problem was that the entities required for the truth of the explanans were not independent of the theory from which the explanandum was drawn. In other words, there is a kind of circularity here. We conclude that a theory is true, because it provides the best explanation of a phenomenon whose existence could only have been established within the very theory doing the explaining. Clearly there is something just a little suspicious about reasoning of this kind.

So this suggests the following line of argument: In the Baker case, the phenomenon to be explained is the prime numbered life-cycles of the cicadas. And the theory doing the explanatory work is number theory. Therefore, we conclude that some number theoretic theorems are true from the role they play in explaining the natural phenomenon of the prime cycled cicadas. But the property of primeness, the attribution of which plays such a vital role in getting the explanatory ball rolling, is itself a number theoretic property. So we conclude the truth of number theoretic theorems from the role they play in explaining a fact that could only have been identified and characterised within number theory itself. Here we seem to have an example of just the kind of circularity that vitiated the quark theoretic and Tarot theoretic explanations, and if that is correct, Baker's instance of an explanatory indispensability argument looks unsuccessful. Moreover, for reasons very similar to those given by Bangu, we can expect this result to generalize. It seems likely that all mathematical explanations will suffer from this same defect: they will attempt to derive the truth of a mathematical theory from the fact that it explains phenomena that could only have been identified by the self-same theory.[6]

What all this suggests is a more general principle, a principle that will enable us to meet Colyvan's challenge and divide up explanations into those that license and those that do not license ontological commitment. The principle is this: an explanation is only ontologically committing when the kinds of circularity identified above are absent. That is, the explanandum must not be part of the same theory as the explanans, in the following sense: it must have been possible to identify the phenomena in the explanandum independently of the theory from which the explanans is taken.[7] In cases where this condition is not met, the use of the posits of a theory in some apparently successful explanation can give us no more reason to believe in the truth of a theory than we already had. And if we had no reason to believe in the truth of the theory in the first place, then its appearance in a circular explanation cannot give us one.

If all this is correct, then we have seen, in particular, how to resist Baker's indispensability argument. We need not conclude that numbers exist from their role in the explanation of the life-cycles of periodic cicadas, because the phenomenon in question only becomes explanatorily relevant in the context of the number theory taken to do the explaining. We have also seen, at a more general level, how to meet

[6] For instance: Mark Colyvan gives as an example of a mathematical explanation in science, the explanation in terms of the Borsuk-Ulam theorem of the existence of antipodal points on the Earth's surface with identical temperature and barometric pressure (Colyvan, 2001a, pp.49-50). But it is difficult to see how this could have been identified as a fact requiring explanation independently of the topological theories of which the Borsuk-Ulam theorem is a part.

[7] This qualification is important. We cannot simply insist that the explanandum not be part of the same theory as the explanans, because the applicability of theories seems to leave the question of what is and isn't a part of our theories somewhat open ended. For instance, Newtonian gravitational theory is applicable to the motions of heavenly bodies. And this explanatory success (the explanation, for instance, of the elliptical orbits of planets) is taken to be a major success of the theory, and evidence for its truth. So are facts about heavenly bodies a part of the theory of Newtonian gravitation? Perhaps not (they are not *required* for the truth of the theory), but if we did wish to extend the notion of 'part of a theory' to include phenomena the theory is applicable to, heavenly phenomena *would* qualify as part of Newton's theory. And in that case the success of Newton's theory, given the crude requirement of independence of explanandum and explanans, would *not* count as evidence for its truth.

Colyvan's challenge; we have discovered a principle (which we might call *the no-circularity condition*) which enables us to divide up explanations in a principled way into those that license and those that do not license ontological commitment, without succumbing to a thoroughgoing anti-realist disavowal of IBE.

1.6 Conclusion

The principal focus of this chapter has been on the explanatory indispensability argument. My aim has been to establish a principle governing responsible use of explanatory arguments, the no-circularity condition, which tells us when we are and are not licensed to conclude that a statement is true from the role it plays in scientific explanation. The suggestion is that a claim or statement gains nothing from its appearance in the explanans of an explanation when the explanandum is not sufficiently independent of the theory from which the explanans is taken. As we saw, the requisite independence criterion is that it must have been possible to identify the explanandum independently of the theory from which the explanans is taken. If this criterion is not satisfied, then we have a kind of circularity in which, in a sense, a theory is being invoked to explain itself. In this case, it seems that the theory's deployment in an explanation gives us no more reason to believe that it is true than we already had for believing the theory in the first place. And if we have no more reason than we already had to think that the theory is true, we have no more reason than we already had to conclude that any objects required for the truth of its sentences must exist.

We further saw how this argument undermines Baker's attempt to motivate the explanatory indispensability argument on the basis of the example of the periodic cicadas. The fact that number theoretic theorems play a vital role in the explanation of the life-cycles of periodic cicada is no evidence for number theory (or no more evidence than we already had for number theory), because the fact to be explained is the unusual *prime numbered* life-cycles, and this seems to be a partly number theoretic explanandum. If we are attracted by the fictionalist response to the original indispensability argument, then we are likely to think that we had very little initial reason to believe in number theory; and Baker's number theoretic explanation of the cicada life-cycle periods does not seem to give us any more reason to believe that number theory is true. So we have no more reason than we already had to believe in the abstract objects necessary for Baker's number theoretic explanations to be true.

I further suggested that this is likely to be a general feature of attempts to motivate an explanatory indispensability argument. Indeed, the very indispensability of mathematics to natural science suggests that Platonists will struggle to find examples of mathematical explanations in science where the entities required for the truth of the explanation are independent in the right way from the theory within which the phenomenon being explained is identified. Of course, it remains open to those attracted by the explanatory indispensability argument to prove me wrong, and to produce examples of apparent mathematical explanation where the explanandum is sufficiently independent (or else to undermine the no-circularity condition by evincing respectable examples from the natural sciences in which it is violated). Until and unless such a refutation is provided, I submit that we have here a convincing reason to reject the explanatory indispensability argument.

But while my focus has been on rebutting the explanatory indispensability argument (and while this is a welcome result for the fictionalist), this chapter serves a somewhat different role within the dialectic of the thesis. Part of my aim in resisting the Platonists' explanatory gambit has been to sure up the fictionalist position, but I also believe that the failure of the explanatory indispensability argument suggests something more. What this result implies is that a Platonist wishing to take a naturalist line by defending an indispensability argument was always better served by focussing on the role of mathematics in scientific representation. Explanation is a dead end for the Platonist, and so, the worries about confirmational holism notwithstanding, anyone wishing to defend a naturalist Platonism must return their attention to the role of mathematics in scientific representation. That means that if we are to defend a nominalist account of mathematics, we too should turn our attention to scientific representation, in an effort to pre-empt the kinds of arguments that Platonists might devise.

In the next two chapters I begin this enquiry into the role of mathematics in scientific representation, and whether we can continue to defend a fictionalist ontology in light of results established during the course of this investigation. In chapter 2, I present a recent attempt to develop a systematic account of the representational role that mathematics plays in scientific applications, the so-called ‘mapping account’. In the course of this discussion we see a number of ways in which this theory must be modified if it is to be successful, but what emerges is a plausible and powerful tool for thinking about how mathematics serves to enhance and even enable the scientific representation of phenomena. In chapter 3 I then turn to the ontological consequences of this theory. As we see, the theory seems, prima facie, to require for its truth the existence of very many abstracta. This suggests an alternative indispensability argument that starts, not from the role of mathematics in scientific explanation, nor from the role of mathematics in science more generally, but from the particular role that mathematics seems to play in our best theories of its own applicability. The remainder of chapter 3 focuses on how we can deploy fictionalism to undercut this line of reasoning, and further suggests ways in which a fictionalized mapping account may have advantages over its realist cousin.

Chapter 2. The Mapping Account of Applied Mathematics

2.1 Introduction

In the previous chapter we saw that it is to the role of mathematics in representation that we must look if we are to settle questions of mathematical ontology. Accordingly the purpose of this chapter is to outline and defend one approach to the issue of mathematical representation: the mapping account of how mixed mathematical-scientific representations are assigned content (Pincock, 2004). This is the claim that for our mathematized scientific representations to have the right kind of content (the right kind of representational content to be useful in applications), there must exist a structure preserving mapping between the empirical domain under investigation and the mathematical structure used to model it.

This primitive mapping account, due to Pincock, faces challenges from the existence of certain uses of mathematics in science that do not appear to be entirely or solely representational. In particular, the existence of idealized representations and the purported existence of mathematical explanations of physical phenomena seem to suggest that the mapping account outlined in (Pincock, 2004) is at best incomplete. To plug these gaps I take over some of the machinery of Bueno and Colyvan's Inferential Conception of applications of mathematics (Bueno and Colyvan, 2011), but I aim to do so while resisting their claims that an account of applied mathematics *must* be given in terms of mathematics' inferential role. What results is an enhanced mapping account that should be able to handle all cases of applied mathematics at least as well as its rivals.

Unfortunately, as we will see, the resulting theory is highly ontologically inflationary. Bearing in mind the results of the previous chapter, this seems a deeply unwelcome result. In the next chapter we will see how to resolve our difficulties with the mapping account by embedding it within a fictionalist approach to ontology. The result is that an attractive and detailed account of mathematical applications can be rendered compatible with an anti-realist position about mathematical ontology. This should serve to strengthen the case for mathematical fictionalism as a response to the placement problem.

2.2 Some Preliminaries

2.2.1 Unreasonable Effectiveness

Before we begin in earnest, it will be helpful to get clear on the exact nature of the problem facing us. There are several ways in which the applicability of mathematics might seem philosophically puzzling, but in this section I just want to separate out two issues, confusion of which could potentially infect the rest of our discussion.

On the one hand, we have the so-called "unreasonable effectiveness of mathematics", first addressed by Eugene Wigner (Wigner, 1960). Wigner's own concerns are somewhat amorphous, and it is not entirely clear what is meant by "unreasonable" in this context. One suggestion (Mark Colyvan's) is that some kind of formalist or anti-realist philosophy of mathematics might lie behind Wigner's worry (Colyvan, 2001b, p.266), in which mathematical concepts are chosen for their aesthetic value and their amenability to clever logical manipulation. At no point does the mathematician develop his subject with a view to future applications in physics. It is then taken to be mysterious why such a logical game, whose standards of evaluation are purely aesthetic, should play a useful role in our investigations of the physical world. As Wigner puts it: "Most more advanced mathematical concepts…were so devised that they are apt subjects on which the mathematician can demonstrate his ingenuity and sense of formal beauty." (Wigner, 1960, p.5) If that is the subject matter of mathematics, concepts chosen for their aesthetic qualities, then it may well appear as though mathematics must be irrelevant to the natural sciences, a subject whose concepts are beholden to more objective standards of appraisal (empirical adequacy, perhaps, or representational accuracy).

A second way to approach Wigner's worry is through the work of Mark Steiner (Steiner, 1998). Like Wigner, Steiner claims that the introduction of mathematical concepts is subject to aesthetic evaluative criteria. Even worse, for Steiner these are *species specific* aesthetic criteria, so we cannot even claim that our mathematical aesthetics tracks objective aesthetic standards. Steiner then claims that mathematics has played an essential role in the discovery of new scientific theories. In the more recent history of science, scientists have come more and more to rely on mathematical analogies in the discovery of new theories of new physical phenomena, and in some cases it seems as though these mathematical analogies between mathematical models are *not* underwritten by any corresponding physical analogy between the physical domains represented by the models (Steiner, 1998, pp.48-115). These kinds of mathematical analogies Steiner calls Pythagorean (Steiner, 1998, p.3) and he suggests that they pose a serious problem for naturalists: the incredible success of the theories generated by these analogies, together with the fact that the analogies are generated by species specific aesthetic judgements (Steiner, 1998, pp.65-66), suggests a kind of anthropocentrism. That is, Steiner concludes that the structure of the world mirrors the structure of human cognitive activity, and that this kind of anthropocentrism must be anathema to any naturalist (Steiner, 1998, p.10).

Now it is important to be clear: neither of these is the problem to be discussed in this chapter. I will not here address the "unreasonableness" worry in either of the two forms just outlined. Instead, the following chapter addresses itself to a rather more homely topic: how do our mathematical scientific representations acquire their content, or how can we use mathematized models to represent the physical world. Another way to put all this is to ask: how are the truth conditions of mixed mathematical-scientific statements (statements like "The mass of object A is 40 grams" that contain both physical and mathematical terminology) fixed?

Having an answer to this question may or may not help with the unreasonableness puzzles addressed above. Alternatively, we might wish to challenge the assumptions underpinning these concerns more directly, arguing against the formalist view of mathematics that seems to underpin Wigner's worry (according to Colyvan) or questioning the role or existence of Steiner's Pythagorean analogies. However that may be, at present none of this is our concern. This chapter we will be solely concerned with the problem of isolating the representational contents of mathematized scientific models. Our interest is entirely in the role mathematics plays in scientific representation, and the consequences of that role for mathematical ontology. As our wider concern is with defending fictionalism as a response to the mathematical placement problem, these further puzzles about mathematics' ‘surprising’ role in science, even if they can be given a substantive formulation, are at best tangential to our purpose.

2.2.2 Syntactic and Semantic Views of Theories

So far I have talked rather loosely of “the contents of mathematical-scientific representations”, and before we can make our first pass at a solution to the puzzle of how these contents are assigned, we will do well to get clearer about what we mean by “representation” in this context. Because, it will have been observed, I have also mentioned in passing that one way to approach our problem is by asking how truth-conditions of mixed mathematical-scientific sentences are ascertained, and it is not immediately clear that these are the same problem at all.

The problem here is that there are two ways of approaching the question of what a scientific representation is, corresponding to two views of the nature of scientific theories. On the syntactic view, a scientific theory is a collection of sentences. More precisely, following van Fraassen, the syntactic view holds that a theory is to be identified “with a body of theorems, stated in one particular language chosen for the expression of that theory.” (van Fraassen, 1980, p.44) The semantic view of theories, on the other hand, has it that a theory is to be identified with a “class of structures [mathematical structures] as its models.” (van Fraassen, 1980, p.44) So the difference here is between an identification of theories with some linguistic entities, and identification of theories with some model theoretic entities.

Now there are questions here about how much of a theory is supposed to be representational, and about whether every scientific representation needs to find a home in some scientific theory (some philosophers of science have argued that at least some of the models employed by practicing scientists are largely independent of any fundamental scientific theory (cf. Morgan & Morrison, 1999)). But it should at least be plausible that the kind of answer one gives to the question of what a scientific theory is will influence the kind of view one takes of the status of scientific representations. For instance, on the plausible (moderate realist) assumption that at least some parts of some of our scientific theories are intended to represent the world, the adoption of the syntactic view of theories will deliver the result that some of our scientific representations are bodies of sentences, while the proponent of the semantic view will conclude instead that at least some our scientific representations are model theoretic entities, mathematical structures. And for the syntactic view, it would perhaps be more natural to talk in terms of truth-conditions for the sentences comprising our representations, while for the semantic view some other way of talking might feel more congenial.

Now as it happens, I believe that the account of mathematical applications to be presented below will work just as well for either view of scientific representations. For that reason, I believe we can elide over this distinction for now, and persist in our loose talk. In what follows I will mostly talk in terms of truth-conditions for mathematical-scientific sentences, as this will allow us to focus on some relatively simple (and perhaps unrealistic) examples of scientific representations. But this should not be interpreted as an endorsement of a syntactic view of scientific representations and theories more generally. Once the machinery of Pincock's mapping account is in place, it should be clear enough how to rejig it to fit with a more semantic conception (and all this should hold, *mutatis mutandis*, for the enhanced mapping account we end up endorsing).

2.3 Pincock's Mapping Account

With all these preliminaries out of the way, we can turn to Pincock's presentation of his mapping account. Consider, to begin with, an extremely primitive kind of application of mathematics: the use of positive integers to enumerate some collection of "medium sized dry goods" - fingers will do for our purposes. How is it that we can apply the natural numbers to the business of sorting out how many fingers we have? Remember that in our case, this question comes to: how can the truth conditions of "The number of my fingers is *n*" be fixed? Let's suppose that I have five fingers. Then, for Pincock, the truth-conditions of the sentence "The number of my fingers is five" are fixed by the existence of a structure preserving mapping that associates with each one of my digits one of the numbers 1 through 5 (Pincock does not discuss this example, but the general approach presented here is based on (Pincock, 2004, pp.145-150)). And in this case, the right kind of structure preserving mapping will be an isomorphism between my fingers and the initial segment of the positive integers terminating with 5.

More generally, Pincock's proposal can be summed up as: the truth conditions of mixed mathematical-scientific sentences are fixed by the existence of a structure preserving mapping between the empirical domain of application and some appropriate mathematical structure. If an appropriate mapping can be found, then the mathematical structure in question can be used to represent our empirical domain; if not, then not.

What kinds of mappings will do the job? Well, in the very simple case just reviewed, we saw that an isomorphism between fingers and numbers enabled us to isolate the truth-condition of our bit of applied mathematics. But Pincock notes that more complicated applications of mathematics may well require other kinds of homomorphisms (Pincock, 2004, pp.149-150), and that most actual applications of mathematics in the sciences will indeed be very much more complicated than our finger counting problem. In actual, real-world scientific representations we may not always have a one-one correspondence between the domain of application and every object in the mathematical structure (indeed, it is very unlikely that we will), so we cannot just rely on bijective homomorphisms to generate the truth conditions of our bits of applied mathematics. Unfortunately, this is about all Pincock says in his initial presentation of the mapping account, and as we will see when we come to review Bueno and Colyvan's criticisms of Pincock's approach (and their proposed alternative), it is this lack of specificity about the available mappings that forces the move to a slightly more elaborate mapping account.

Of course, it is not quite accurate to say that this is all Pincock tells us about the relevant mappings: he does insist that constraints need to be placed on the mappings, and for this purpose he employs a modification of measurement theory (Pincock, 2004, pp.147-149). Now I do not want to get bogged down in the specifics of measurement theory, but the basic idea is that measurement theory provides a set of constraints on a physical domain for a mathematical description of that domain to be possible. Or, as Pincock puts it: "what sort of structure must a physical domain have if the right kind of mapping between the physical domain and a mathematical domain is to exist?" (Pincock, 2004, p.147) Pincock's idea is to reverse the usual direction of explanation in measurement theory, and to use the results of measurement theory to place restrictions on the kinds of *mappings* that can be used to fix mathematical-scientific truth-conditions. That is, instead of characterizing the physical domain of application, the results of measurement theory are instead employed to constrain the choice of mapping between mathematics and world. Using this kind of approach, Pincock hopes to generate necessary and sufficient conditions that a mapping must satisfy, if it is to be able to provide our mathematical-scientific sentences with their truth-conditions.

It is this kind of account, then, an account which says that the representational content of mathematics is given by structure preserving mappings, that I hope to make use of in the fictionalist approach to applied mathematics developed in the next chapter. There I will suggest that we view the mapping account as giving us the specific fictional content of a fictional representation, a kind of prop-oriented make-believe taking the empirical world as prop. In the remaining sections of this chapter, I will defend the mapping account from objections levelled at it by Bueno and Colyvan and Robert Batterman. But, as we will see, this defence will require us to move beyond Pincock's mapping account, to an enhanced mapping account incorporating some of Bueno and Colyvan's suggestions.

2.4 The Inferential Conception and the Enhanced Mapping Account

In this section we introduce and discuss Bueno and Colyvan's alternative "Inferential Conception" of the application of mathematics (Bueno & Colyvan, 2011). Our aim in this section is twofold: on the one hand, we need to get clear the kinds of problems facing Pincock's approach that motivate Bueno and Colyvan to seek an alternative. As we will see, these kinds of challenges do require that we take over some of the machinery of the inferential conception into the mapping account. On the other hand, we will need to assess whether this kind of move on our part will also require us to adopt Bueno and Colyvan's entire approach to applications of mathematics. Here I want to make two claims: firstly, whatever inferential role mathematics plays in science is underwritten by its representational functions, so it is wrong to claim that the *fundamental* role of mathematics is inferential. Secondly, Bueno and Colyvan need to do much more to establish that the kinds of applications they have in mind – prediction, unification, explanation – depend upon establishing inferential relations within our scientific representations.

If the remaining subsections are successful then we will have a much fuller picture of the kind of mapping account that will be used in the next chapter as part of our fictional account of mathematics. We will have presented a mapping account, supplemented by Bueno and Colyvan's machinery of immersion and interpretation maps, without finding ourselves committed to their further claims about mathematics' specifically inferential role. It is this theory that I will employ as the content of the prop-oriented make-believe to be developed in chapter 3.

2.4.1 Bueno and Colyvan's Objections

Before we turn to the kind of approach to applications favoured by Bueno and Colyvan, it will be helpful to see what kind of problems they identify with Pincock's initial presentation of his mapping account. We will postpone discussion of their solutions until a later subsection. Our primary aim here is simply to see why the mapping account presented above cannot be the whole story about applications of mathematics.

The first difficulty noted by Bueno and Colyvan is with the somewhat loose talk we have so far employed concerning "the structure of the world." They begin by characterising a structure as "a set of objects (or nodes or positions) and a set of relations on these" (Bueno & Colyvan, 2011, p.347). They then claim that "the world", presumably meaning by this the empirical domain of application, is not pre-theoretically divided into nodes/objects and relations between these in the way the mapping account seems to suppose. In order to establish our structure preserving map between the empirical domain and the mathematical structure used in modelling it, we will need to be able to identify both the objects in the empirical domain *and* the relations between these objects that we want our mapping to preserve. But, claim Bueno and Colyvan, the world does not come pre-theoretically carved up in this way; it is our scientific theories that identify "the structure of the world." So until we apply our mathematical-scientific theories there is no reason to expect to discover any structure in the empirical world at all (Bueno & Colyvan, 2011, p.347).

The second difficulty they identify with Pincock's approach has to do with the mappings themselves. As we noted above, Pincock says very little about the kinds of mappings that the applied mathematician will have at her disposal. As Bueno and Colyvan see it, one reason for this is that there is very little to be said: the kinds of mappings available will be very limited. They begin by agreeing with Pincock that for most actual applications of mathematics, isomorphisms will be unsuitable. As they put it "the mapping employed will depend on the richness of the two structures in question, *W* and *M* ["World" and "Mathematics"]." (Bueno & Colyvan, 2011, p.348) Should either the structure attributed to the world or the structure of the mathematical domain outstrip the other, isomorphisms will no longer be appropriate for our application. In such a case we will need to employ some other kind of mapping.

Bueno and Colyvan identify three types of structure preserving maps we might try to employ here: homomorphisms, epimorphisms, and monomorphisms (Bueno & Colyvan, 2011, p.348). Of the three though, it seems only monomorphisms will do the job. This is because we want to be able to "move back and forth" between the empirical domain and the mathematics used in modelling it, rather as we do when using a street map. That is, we compare the map with our surroundings in order to discover where we are or where we are supposed to be. In a similar sort of way, we want the mapping between our empirical structure and our mathematical structure to be invertible, so that we can compare the mathematical-scientific representation with its domain of application (Bueno & Colyvan, 2011, pp.348-349). If our mapping is to be invertible, that leaves us only with monomorphisms , but this in effect rules out the possibility of the world having more structure than the mathematics used to model it, and this seems a hopelessly unrealistic picture of scientific applications (Bueno & Colyvan, 2011, p.349).

A further objection raised by Bueno and Colyvan is that the mapping approach cannot by itself account for how we assign physical content to some parts of a mathematical structure and withhold it from others (Bueno & Colyvan, 2011, pp.349-250). In cases where the mathematical structure is richer than the empirical domain we will have these sorts of decisions to make, but the kind of contextual information we use to guide these kinds of decision making cannot be accounted for in terms of mappings. They consider the case of "a typical projectile problem in which one needs to calculate where a projectile (of known initial velocity and position whose only acceleration is due to gravity) will land." (Bueno & Colyvan, 2011, p.349) They remark that "the displacement function for such a projectile is a quadratic with two real solutions—only one of which is physically significant." (Bueno & Colyvan, 2011, p.349) So the decision we have to make is which of these solutions is the physically significant one. Of course, in this situation it is clear which of the two solutions is physically significant "projectiles do not land in more than one place and they typically land forward of their launch site" (Bueno & Colyvan, 2011, p.349). The problem, as Bueno and Colyvan see it, is that the guidance provided by these kinds of physical intuitions cannot be incorporated into the mapping account. We use our knowledge of past physical systems, resembling the one we are investigating, to decide which of our solutions is physically significant. But this sort of information is not coded in the mappings used by the mapping account.

A further worry for the mapping account is presented by cases of idealization. As noted above, it is unrealistic to think that in actual applications of mathematics there are no cases in which the structure of the empirical domain outstrips that of the mathematics used to represent it. As Bueno and Colyvan see it, we get these sorts of cases whenever we encounter idealization in science:

> "Here there seems to be no mapping between the empirical structures in question and the mathematical structures. If anything, there would appear to be a mapping between the mathematical structure and some possible, but non-actual empirical structure." (Bueno & Colyvan, 2011, p.351)

This creates a problem for Pincock, because if there can be no structure preserving mapping between the (real) empirical structure and the mathematics used in representing it, then it seems we are forced to conclude that the truth-conditions of the relevant mixed sentences are undetermined. But given the widespread use of idealisation in natural and social science, and the plausible assumption that idealisations can be ranked in terms of their accuracy, this seems like an implausibly rigid line to take on idealised mathematical-scientific representations.

The final worry Bueno and Colyvan raise for the mapping approach concerns explanation. As they see it, the *only* role the mapping account allows for mathematics in applications is as a representational or descriptive device. But if, as some philosophers believe, mathematics plays an *explanatory* role in our best scientific theories, then the purely descriptive role allowed for on the mapping approach will be at best an incomplete account of mathematical applications (Bueno & Colyvan, 2011, pp.351-352). If these philosophers can substantiate their claims about the explanatory role of mathematics in science, then it seems that the kind of indexing strategy employed by the mapping account cannot be successful as a complete account of applications of mathematics within the sciences (Bueno & Colyvan, 2011, p.352).

These, then, are the kind of problems facing Pincock's mapping account that suggest that we must at least revise his approach to mathematical applicability. As Bueno and Colyvan see it, these kinds of worries require an entirely new approach to the problem of applied mathematics. In the next subsection we present their "inferential conception." We will then see how they employ this alternative account to solve the kinds of problems they have outlined for Pincock's approach. In the process we will discover that much of their added machinery is either compatible with an enhanced mapping account of applications or else unnecessary. Before closing this section, we will turn to the evaluation of their claim that the primary role of mathematics in applications is inferential.

2.4.2 The Inferential Conception

Bueno and Colyvan's alternative account of mathematical applicability starts from the claim that the role of mathematics in applications is inferential: "by embedding certain features of the empirical world into a mathematical structure, it is possible to obtain inferences that would otherwise be extraordinarily hard (if not impossible) to obtain." (Bueno & Colyvan, 2011, p.352) As they point out, this does not rule out other uses of mathematics, in explanation or unification for instance. It is just that these further uses depend on using mathematics to establish inferential relations between empirical phenomena (Bueno & Colyvan, 2011, p.352).

It is interesting to note at this point that the inferentialist claim is not necessarily unwelcome to the anti-realist. After all, Hartry Field in *Science without Numbers* (Field, 1980) suggested that the principal role of mathematics in science was inferential, as part of his nominalist strategy to disarm the indispensability argument. For Field, mathematics is just a "theoretical juice extractor" (Hempel, 1945, p.554), a device for facilitating inferences that would be much more difficult using a purely nominalistic theory. So, if anything, the fictionalist ought to see the claim that the fundamental role of mathematics is inferential as grist to her mill, lending further support to the idea that mathematics' role in science does not support the Platonist's ontological commitments. Unfortunately, as we will see, I feel that Bueno and Colyvan have done far too little to establish their claim that the mathematics used in science has a fundamentally inferential role.

However that may be, Bueno and Colyvan suggest that in order to establish these inferential relations, we need to have in place some mappings between the mathematics and the empirical domain under investigation (Bueno & Colyvan, 2011, p.353). Bearing in mind their criticisms of the original mapping account they offer a three stage approach to the application of mathematics.

In the first step, *immersion*, we set up a mapping between the empirical domain and some appropriate mathematical structure. We will have a choice of mappings at this stage and our choice will be guided by contextual factors having to do with the details of the application. Nor need this be just a one-step process. Bueno and Colyvan allow that we may map the empirical structure onto some mathematical domain, and then proceed to map this mathematical structure straight on to some new mathematical structure (Bueno & Colyvan, 2011, p.353).

The next step is *derivation*. This consists in drawing consequences from the mathematical formalism by purely mathematical means. Bueno and Colyvan take this to be the key step in the application process (Bueno & Colyvan, 2011, p.353).

The third and final step is *interpretation*. Here we interpret the mathematical consequences derived in the derivation step in terms of our original empirical set-up.

This is done by establishing a mapping between these mathematical results and the empirical domain. These mappings need not be the inverse of our immersion map, but can be completely new mappings between the two domains. (And so we see how Bueno and Colyvan propose to increase the stock of available mappings. Because our immersion maps no longer need to be invertible, we are free to use any structure-preserving mapping at the immersion stage and any structure-preserving mapping at the interpretation stage. We are no longer limited only to monomorphisms). Once again, there is no need for this to be a one-step process. We may interpret the results of our mathematical derivations by first mapping them onto a new mathematical structure, and only then mapping the new structure back on to the empirical domain (Bueno & Colyvan, 2011, pp.353-354).

2.4.3 Bueno and Colyvan's Solutions

Now let us turn our attention to how Bueno and Colyvan propose to use their new account to deal with the problems they outlined for Pincock's mapping approach. In the first place, we saw that for Bueno and Colyvan the mapping account looked as though it was going to have trouble locating an empirical structure for the mapping to latch on to. Now one way they suggest the mapping account might get around this is for the applied mathematician to posit the existence of some "assumed structure", and use this as the basis for their application of mathematics to the empirical domain. The applied mathematician can then "treat this initial assumed structure as defeasible and let the resulting mathematical model help inform refinements or revisions to the initial assumed structure." (Bueno & Colyvan, 2011, p.357)

The advantage that Bueno and Colyvan see with their proposal is that this process of assuming and revising empirical structure will go much more smoothly given their more flexible account of the *immersion* mappings (Bueno & Colyvan, 2011, p.357). Remember that during the immersion process, we were permitted to map our (assumed) empirical structure onto some mathematical structure, *and then* map that structure onto a further mathematical structure. In principle there is no limit to the number of times we can do this, and this allows us to see the revision of our assumed structure as the interpolation of further steps in this immersion process. In effect, this frees the applied mathematician from having to scrap her representations every time she revises the initial assumed structure, and this increased flexibility appears as a considerable advance over the original mapping account (Bueno & Colyvan, 2011, p.357).

In the next chapter we will return to the issue of assumed empirical structure, and we will note some further worries we might have about the identification of empirical structure independent of or prior to the application of mathematics, further worries that it seems to me Bueno and Colyvan's response fails to address. There we will see how a fictionalized version of the mapping account gives us a particularly attractive solution to these remaining problems, lending further support to this version of the theory.

The second difficulty we noted was with the impoverished range of mappings available to the mapping account. The inferential conception is able to deliver a richer supply of mappings, because by separating the *immersion* and *interpretation* stages, it has effectively removed the need for the mappings to be invertible. Because the mappings no longer need to be invertible, we are free to employ any of the structure preserving mappings Bueno and Colyvan identify at both the immersion and interpretation stages. And so we are no longer limited just to monomorphisms and no longer faced with the problem that we cannot account for the case in which the world has surplus structure over the mathematics used to represent it (Bueno & Colyvan, 2011, pp.354-355).

I will return to the problem of idealisation in the next section. Bueno and Colyvan's solution to this issue is somewhat more technical than their other solutions, and it will save time if we discuss their suggestions in tandem with our evaluation of them.

The third problem Bueno and Colyvan identify for the mapping account is with the case in which the mathematical structure outstrips the empirical domain of application. In particular, we saw that when some parts of the mathematical structure did not receive a physical interpretation whilst others did, we need to say something about what distinguished the two pieces of mathematics. And, recall, we saw that the relevant distinctions would depend upon contextual factors which the mapping approach was poorly situated to account for. Now we simply note that Bueno and

Colyvan build into their account of the immersion stage the existence of contextual features determining the choice of appropriate mapping (Bueno & Colyvan, 2011, pp.356-357).

We will return to the problem of explanation, and how Bueno and Colyvan propose to address this, in a later section on the inferential portion of their inferential account. For now, though, we note that the existence of a derivation stage in their account allows for the use of mathematics in more than an indexing capacity, and so it is within this stage that Bueno and Colyvan believe their inferential account can accommodate other applications of mathematics beyond mere representation (Bueno & Colyvan, 2011, pp.363-366). As we will see, it is not at all clear how this is supposed to work, and so it is not entirely clear that the case for a derivation stage has been established. Nonetheless, at this point we should note that there is nothing about this derivation stage itself that cannot be accommodated within an enhanced mapping account. It is the claim that the inferential relationships established at this stage are the fundamental or most basic use of mathematics in scientific applications that seems to make the difference between the inferential approach and the mapping account. Absent the former claim, and the mere existence of a derivation stage seems perfectly compatible with an enhanced mapping account of mathematical-scientific representation.

2.4.4 Evaluating the Solutions and an Enhanced Mapping Account

In this section we turn to the evaluation of these various responses, and we begin by noting that our response to each will be fairly similar. As we will see, because the inferential conception seems to make little use of mathematically established inferential relationships in accounting for the problems we have discussed so far (despite this supposedly been its raison d'etre), and because the existence of mappings plays such a central role in the machinery Bueno and Colyvan employ, an enhanced mapping account should be able to help itself to much of the inferential conception's elaborations without feeling a great deal of compunction.

In response to their solution of the first problem, we note that Bueno and Colyvan have done little to establish that the mapping account can only incorporate a defeasible assumed structure “via more ad hoc and trial-and-error methods” (Bueno & Colyvan, 2011, p.357) than their own two-step immersion approach. But perhaps more importantly, there is nothing in this two-step mapping account that cannot be taken over by a more elaborate mapping account than Pincock’s. Indeed, there is nothing to stop Pincock adding to his mapping account the claim that we can map empirical structure onto mathematical structure onto new mathematical structure in just the same way Bueno and Colyvan do. Clearly this device is not present in the original presentation of the mapping account, but nor is it incompatible with it. I conclude that the mapping account can deal with this challenge, then, in just the same way as the inferential conception.

Again, with respect to the role of contextual features in settling the choice of mapping, I propose that the mapping account simply be supplemented to allow for the existence of contextual decision making. Nothing about the mapping account is inconsistent with this; and as the use of context to settle these kinds of decisions does not seem to depend on any inferential role of the mathematics used in applications, I conclude that if the inferential conception is entitled to this contextual supplement, so is the mapping account.

The problem of the impoverished range of mappings available to the mapping account is more of a challenge to Pincock’s approach. There was no place in Pincock’s original presentation of the mapping account for the two stages of immersion and interpretation, and it is clear that incorporating these two features would be a significant structural change in the mapping approach to applications. Nonetheless, I take it that the upshot of Bueno and Colyvan’s discussion is that this is precisely what the mapping theorist should do. Once more, I see nothing precluding a mapping account from incorporating these two stages of mapping, and so nothing requiring the mapping theorist to limit herself only to monomorphisms. And once again, I see no especially inferential role for mathematics in Bueno and Colyvan’s solution to this problem.

Indeed, this is a very general feature of Bueno and Colyvan’s discussion of their alternative account of applications. With the *possible* exception of the problem of explanatory uses of mathematics, none of the problems identified by Bueno and Colyvan for the mapping account requires the existence of an inferential relationship facilitated by the mathematics. This is true also of the problem of idealisation we are about to discuss. And because the inferential conception is in other respects so closely related to the mapping account, there is nothing to prevent the creation of an enhanced mapping account utilizing the same machinery of immersion and interpretation stages as the inferential conception.

The problem of idealisation and its proposed solution is somewhat more complicated than those we have so far discussed, and would require a greater degree of technical exposition to discuss fully. As I do not want to become bogged down in a morass of technical details, this brief discussion will unfortunately be extremely condensed. For a fuller presentation, I direct the reader to (Bueno & Colyvan, 2011, pp.357-363) In short: Bueno and Colyvan propose to account for idealisations in terms of the theory of partial structures. That is, because there can be no full mapping in these cases between the empirical and mathematical structures, we need to make use of *partial* mappings instead, in which some "features of the empirical set up – although not all – can be mapped into appropriate mathematical structures." (Bueno and Colyvan 2011, p.358) These partial mappings are then analysed in terms of the formal framework of partial structures (I will not attempt a formal presentation of this theory here, which is adequately discussed in the paper already referenced). The idea, then, is that in cases of idealisation, when our mappings must perforce be partial, "certain aspects of the actual empirical situation can still be mapped to relevant features of the mathematical structures invoked." (Bueno & Colyvan, 2011, p.359)

Now there are two things that could be said in response to this solution. The first is to note again that nothing here *requires* the existence of inferential relationships facilitated by mathematics, and so nothing seems to prevent the mapping account from being supplemented with these partial mappings. Indeed, as partial mappings could be seen as just a species of mapping, it might simply look like an oversight on Pincock's part not to make use of them in the first place.

But I believe that this extra machinery is in fact unnecessary (or, at least, that it has not shown to be necessary). I believe that with the flexibility of the two stage immersion and interpretation mappings, there will be no need to make this extra machinery available. To see this, remember that for Bueno and Colyvan, we had a case of idealisation when we had a case of the world possessing greater structure than the mathematics used to represent it. This was a problem for the mapping account, because the original Pincockian approach was limited to the use of monomorphisms. But as we saw above, once the two stage approach is adopted, we are no longer limited to monomorphisms, and so we are no longer stymied when we attempt to account for those cases, like idealisation, in which world outstrips mathematics.

Bueno and Colyvan's final objection to the mapping account was based around the concept of mathematical explanation. As we saw above, the mapping account is largely a theory of the descriptive use of mathematics in science. But, as we saw in the previous chapter, in recent years some philosophers have urged that mathematics plays a more than descriptive role in applications; some uses of mathematics, they have suggested, are genuinely explanatory (cf. Baker, 2005). Bueno and Colyvan suggest that this threatens the completeness of the mapping account's theory of applied mathematics: because explanatory power is not simply a function of descriptive adequacy, and the mapping account is only a theory of the latter, there is no way to account for explanations using the mapping approach to applicability.

How might we respond to this line of objection? Well, in part, a fully developed response to Bueno and Colyvan will have to await the next two sections, in which we discuss the inferential portion of the inferential conception and a related worry raised by Robert Batterman (Batterman, 2010); but there are some things we can say right now. Firstly, Bueno and Colyvan's paper gives us little reason to believe that mathematical explanations exist within science. This is a controversial matter within the philosophy of science, and it is certainly not an established fact. So one way out might be to challenge the claims made about mathematical explanations in science, ie. to counterclaim: "there aren't any". This is not an option I want to pursue at present; as we saw in the previous chapter, there are no ontological pressures motivating this line of argument, so if we are to reject the existence of mathematical explanation in science we will have to do so on the basis of independent methodological considerations I cannot, for reasons of space, address here. Nonetheless, I do not believe the matter of mathematical-scientific explanation has been definitively settled, and so I believe this response may be a live one.

Another way out of the quagmire would be to establish some way in which the mapping account *can* account for explanations. One way of doing this might be to incorporate the derivation stage of the inferential approach into the mapping account. But for now we might as well observe, anticipating some of our later arguments, that while explanatory power is not a function of descriptive adequacy, it is a plausible claim that some descriptive machinery must be in place before any explanation can be possible. This makes some form of descriptive adequacy at least a necessary condition on explanation, and so the mapping account is not totally silent on how explanatory mathematical applications might be possible.

I think it is also important to note, before we move on to a more thorough discussion of these matters, that the outcome of this debate is likely to have little bearing on the issue of mathematical ontology. We have already established, in the previous chapter, that mathematical explanation, if there is any, is unlikely to commit us to the existence of mathematical objects. It is description or representation that fixes our ontological commitments in this case, the descriptive role mathematics plays in our best theories; that is the only reason we have found ourselves investigating the nature of mathematical representation in science at all.

Now, should this objection to the mapping account stick, that would certainly be unfortunate: I would be forced to alter the account I hope to give of the nature of applied mathematics, adopting a different theory of how mathematics relates to the empirical world, and embedding that new theory within my fictionalist account. Nonetheless, the basic structure of that fictionalist account would remain intact. Once again, it is important to bear in mind that mathematical explanation (if there is any) is not a threat to mathematical anti-realism.

The final point (and this will become much more important in the next section): Bueno and Colyvan offer no characterisation of explanatory power. In particular, they offer no real reason to think that explanatory power *is* a function of inferential relationships, and so we are given no real reason to think that the inferential conception is any better placed to deal with explanatory uses of mathematics than the mapping account. Even worse, as will become apparent in the next section, it is not at all clear that a theory of representation is required to also be a theory of explanation. One reason that both the inferential and mapping accounts are *equally* silent on this issue might be that neither is really the kind of theory that would have anything to say about explanation.

2.4.5 The Inferential Conception: How Inferential?

So far we have seen ways in which an enhanced mapping account can take over much of the machinery of Bueno and Colyvan's conception of applied mathematics, and use it to solve the problems faced by Pincock's initial presentation of a mapping theory. But it remains that all our hard work might be undone, unless we can resist the claim that the *primary* role of mathematics in science is inferential. With that in mind we turn to an evaluation of Bueno and Colyvan's *inferential* component of their inferential conception.

We saw above that the main difference between Bueno and Colyvan's proposal, and Pincock's mapping account was that the former takes the *fundamental* role of mathematics to be *inferential*: "by embedding certain features of the empirical world into a mathematical structure, it is possible to obtain inferences that would otherwise be extraordinarily hard (if not impossible) to obtain." (Bueno & Colyvan, 2011, p.352) It is these inferential relationships that Bueno and Colyvan believe will underpin the use of mathematics in generating predictions, unifying theories, and providing explanations. And it is the supposed failure of the mapping account to deal with these kinds of applications that requires the move to the richer inferential conception.

The problem with this line of argument is that Bueno and Colyvan say very little about *how* it is that inferential relationships underpin these further uses of mathematics. We are simply told that the establishing of inferential relationships is essential to these three features of scientific activity. Now, it would clearly be a large project to review every extant approach to the unification of scientific theories, prediction and confirmation, and scientific explanation. And it is equally clear that this chapter is not the place for sorting out *all* of that. But without having some account of these aspects of scientific methodology, it is very difficult to know what to say about the role of inference, and so very difficult to know what to say in response to Bueno and Colyvan's claim that the *fundamental* role of mathematics in applications is inferential.

In order to make some progress here, I suggest we shelve for the moment the question of mathematics' role in generating predictions and unifying theories. It is here that Bueno and Colyvan's claims about the importance of inference looks most plausible, and this may well provide some evidence of the need to incorporate mathematics' inferential role into our enhanced mapping account. If this is correct, we may have here some (tentative) evidence of the need for a derivation stage in between the immersion and interpretation steps.

But what of explanation? Here the waters are less clear, and it will help to very briefly consider the kinds of accounts of explanation for which an inferential role might be essential and those for which inference might be less important. It is clear that a deductive-nomological model would make explanation a function of inferential relationships. On this kind of account of explanation, giving an explanation depends upon providing an argument, at least one of whose premises is a law of nature, and whose conclusion is the explanandum (Hempel and Oppenheim, 1948). This is about as clear an instance of the establishing of an inferential relationship as you could ask, but sadly the D-N account of explanation cannot, for very familiar reasons, which I will not review, be the correct account of explanation.

So what about the alternatives? Well, once again, without some clear statement of the theory of explanatory power preferred by Bueno and Colyvan, it is difficult to know what to say. We begin by noting that there is at least one theory of explanatory power for which inferential relationships might not be of fundamental importance. This is van Fraassen's pragmatic account, according to which the giving of explanations consists in an answer to a "why question" (van Fraassen, 1980, 126-129). Because the kind of answer appropriate will depend upon contextual and pragmatic features, it is unlikely that *every* explanation will consist in evincing some inferential relationships. It is reasonable to think that for some audiences, and for some "why? questions", a complex inference might be a truly useless explanation.

The remaining alternatives are causal accounts, statistical relevance accounts, or some kind of unificationist account. It would take far too long to review all of these different approaches to explanation, but for now we can note one more problem Bueno and Colyvan's proposal faces: we are told very little about the *kinds of inference* they believe mathematics aids. For some of the accounts of explanation just named (statistical relevance, and unificationist) it is plausible that inferential relationships will play an important role. But what about the causal mechanical account? It may be possible for Bueno and Colyvan to argue that the identification of causal mechanisms involves inference, but it is not clear at all that this will be the same kind of inference employed in establishing statistical relevance or unifying two theories. Here the challenge for Bueno and Colyvan is to say what *mathematics* in particular can contribute to these kinds of causal inferences.

This ambiguity about explanations on the part of Bueno and Colyvan ultimately makes the task of knowing how to respond to the objection canvassed in the previous section unmanageably difficult. That is, it is just not clear how the mapping theorist should respond to the objection that mathematical explanations cannot be accounted for within the mapping approach. As we saw in the previous section, one way in which the mapping theorist might want to go is to adopt the derivation stage into her mapping account, while resisting Bueno and Colyvan's further claim that this inferential machinery is the whole point of mathematical applications in science (something we will turn to in the remainder of this section). The problem here is that it is just not clear how the derivation stage is supposed to help in providing explanations, nor how drawing inferences within a mathematical model could possibly amount to explaining anything. Nor is it obvious that it is within a mathematical representation that explanation ought to take place. Explanation looks like something we do with representations, not something we do within them. For these kinds of reasons, I am chary about adopting Bueno and Colyvan's solution to this particular problem (the derivation stage). In short, I am not convinced that mathematical explanation even is a problem to be solved by a theory of mathematical representation (and this explains why even Bueno and Colyvan finally seem uncertain about what exactly to say about it).

The two main conclusions to take from all this are: firstly, that at the very least, Bueno and Colyvan need to say much more about what they take explanatory power to consist in, and what kinds of inference they believe mathematics assists with, before they can establish their conclusion that the fundamental role for mathematics in applications is inferential. And secondly, because they do not address these issues, it is unclear how their own solution to the challenge from mathematical explanation is supposed to work, and even unclear whether a theory of mathematical representation is the appropriate place to address issues of explanation.

The previous line of argument, if it has been successful, has established the existence of a lacuna in Bueno and Colyvan's presentation of their inferential conception. Before closing this section, I want to present a more direct argument against the claim that facilitating inference is the primary or fundamental role of applied mathematics. I propose that the fundamental role of mathematics in applications is in facilitating the *representation* of scientific phenomena, and, moreover, that whatever inferential role mathematics plays in science is underwritten by this representational function (It is important to note, before continuing, that this should not be taken as ruling out an inferentialist account of representation (cf. Brandom, 2000). The claim is just that whatever the fundamental philosophical character of the representation relation, representational uses of mathematics are more fundamental than specifically inferential ones, such as facilitating otherwise complex or impossible inferences).

In essence, the claim I want to make is very simple: before we can apply our mathematical-scientific representations inferentially, we need to have in place a mapping establishing the representational adequacy of our representations. If this mapping is not in place, and our mathematical model is not a description or representation of some scientific structure, then it is unclear what relevance our inferences or derivations employing that mathematics could have for the empirical phenomena being investigated. Here we would have only a mathematical formalism, floating free of any interpretation, and so floating free of any actual application.

If the above reasoning is correct, then it seems that the existence of representational applications of mathematics is prior to and necessary for the specifically inferential applications of mathematics. Or to put all this another way: every inferential application is already, of necessity, a representational application. That is, the set of inferential applications is a subset of the set of descriptive applications, and if that is correct, then there is no way inference could be *fundamental*; it is representation that is fundamental, and the primary use of mathematics in science *must be* to facilitate these representations.

To conclude this evaluative section: we have seen that Bueno and Colyvan fail to sustain their suggestion that the principle role of mathematics is inferential. On the one hand, they must say more about the role of inference in explanation, before they can establish their claim that all the applications of mathematics they identify depend upon mathematics' role in facilitating inference. On the other, we have seen good reason to think that even if they can establish this vital role for mathematical inference, such inferences must, of necessity, be underwritten by mathematics' representational role, and so it is representation not inference that turns out to be essential for understanding the applicability of mathematics. We have seen further that without some good reason to think that explanatory uses of mathematics must be addressed by a theory of mathematical representation, the objection Bueno and Colyvan raise to the mapping account from the existence of mathematical explanations of physical phenomena lapses.

2.5 Batterman on Explanation

In this section I want to consider one final challenge for the mapping theorist, introduced by Robert Batterman in his *On the Explanatory Role of Mathematics in Empirical Science* (2010). Like Bueno and Colyvan, Batterman believes that the mapping account will struggle with both idealisation and explanation, but Batterman's unique focus is on the intersection of these two kinds of application. That is, Batterman raises a worry for the mapping account from the direction of *idealised* explanations, especially insofar as these involve the taking of limits and the generation of singularities that these limiting operations can give rise to.

As Batterman sees it, the problem with mapping accounts is that they focus on static relationships between mathematical structures and the physical domain (Batterman, 2010, p.11). But in many cases the explanatorily relevant features of some application of mathematics involve the use of limiting *operations*, and these cannot be understood in terms of a structural relation between the mathematics and the physical world. This is because, if "the limits are not regular, then they yield various types of divergences and singularities for which there are no physical analogs." (Batterman, 2010, pp.21-22) Batterman then argues that these singularities are essential to the explanatory role of mathematics. Having the limiting idealisation in place allows us to discard or ignore aspects of the physical system that are irrelevant to the features we are interested in explaining (Batterman 2010, p.23). If this is correct, and if we cannot find physical analogues for these limiting operations and the singularities they yield, then it is hard to see how the mapping account could accommodate explanatory applications of mathematics.

As an example of the explanatory use of limiting operations, Batterman considers the explanation offered by condensed matter physics for "the existence of phase transitions and of the universality of critical phenomena" (Batterman, 2010, pp.5-8). We will not go into the details of this example here, but the upshot of Batterman's discussion is that the explanation presented by condensed matter physicists involves taking the *thermodynamic limit*. This is the limit in which (roughly speaking) the number of particles of the system, for example, the number of water molecules in a kettle, approaches infinity. This is a fairly clear instance of idealisation: tea kettles can only hold a finite number of water molecules. But nonetheless, this idealization is an essential part of the explanation of phase transitions. "The existence of a phase transition requires an infinite system. No phase transitions occur in systems with a finite number of degrees of freedom." (Kadanoff, 2000) (quoted at (Batterman, 2010, p.8)) So it seems as though we have an explanatorily essential use of a limiting operation, where the limiting operation has no physical analogue. And it is Batterman's contention that the majority of idealising explanations in physics should be viewed as instances of the use of limiting operations in this way (Batterman, 2010, p.8).

Batterman goes on to suggest that the best line for a mapping theorist to take with respect to idealisation is to adopt what he calls the Galilean conception (Batterman, 2010, p.16). The idea here is to claim that what makes an idealisation useful is the (in principle) possibility of removing it by telling some de-idealising story. That is, idealisations are useful in science to the extent that they can be eliminated by filling in the details ignored and distorted by the original idealised model.

But, Batterman goes on to argue (Batterman, 2010, pp.17-19), the kinds of idealised *explanations* he has in mind cannot be accounted for in this way. The existence of explanatorily *essential* idealisations, like that of the thermodynamic limit in the explanation of phase transitions, rules out this Galilean approach to idealisation. The idealisation cannot be removed by filling in the details glossed over in the original model, because, if the model is "corrected" in this way, the explanation is no longer possible. In these cases, adding more details counts as "explanatory noise" (Batterman, 2010, p.17), drowning out the features of the system that are actually of interest to the investigator.

Batterman then claims that what makes the provision of such a "promissory back-ground story" (Batterman, 2010, p.17) impossible is the fact that the limits involved in the explanations are singular (a singular limit is one in which the behaviour as one approaches the limit is qualitatively different from the behaviour at the limit). So adding more detail in order to remove the limiting operation makes a qualitative change in the behaviour of the model. In most cases this will lead to the explanatory features of the model being lost.

Now if Batterman is correct about all this, then we have one more piece of evidence of the need to incorporate a "derivation" stage into our enriched mapping account. Note that the use of limiting operations in the kind of examples Batterman has in mind is exactly the kind of mathematical transformation for which Bueno and Colyvan make room in the derivation stage of their application procedure. But two things need to be said about this move.

First, contra Bueno and Colyvan, while this does involve an important role for mathematics in facilitating inferences (in this case, inferences about what will happen when some physical feature is ignored or deliberately simplified), this inferential role is still underpinned by the vital representational relations established at the immersion and interpretation stages. So even if we must accommodate an inferential role for mathematics, we still have no reason to think that this role is *fundamental*.

Secondly, contra Batterman, none of this gives us a reason for abandoning the mapping approach altogether. If the immersion and interpretation stages were removed entirely, we would be left with a piece of uninterpreted formalism, and the kinds of mathematical operations Batterman is so keen to focus on would not amount to *explanations* at all. As Pincock puts it: "Batterman's proposal risks turning his explanations into purely mathematical derivations." (Pincock, 2011, p.216)

But, of course, all of this is only hypothetical: "If Batterman is correct, then…" Can we find any reasons to doubt the antecedent? Well, first off, we might worry about Batterman's claim that *most* idealised explanatory applications of mathematics will involve limiting operations. One reason for hesitation here is that we might think of *apparently* explanatory uses of idealised mathematical representations that *do not* involve the taking of limits or the appearance of singularities. For instance, some instances of what Pincock (Pincock, 2012, p.6) calls "abstract acausal" representations (a representation in which mathematics is used to abstract away from particular causal details of the empirical situation under investigation) seem to be used explanatorily, but do not rely on some parameter tending to a limit.

Consider, for instance, Euler's proof that there is no way of walking over each of the bridges of Konigsberg just once and returning to one's starting point. In brief, the explanation here is that the bridges form a non-Eulerian graph, for which there is no such path. Here the explanation depends upon the existence of a map between the physical arrangement of bridges, and a certain mathematical structure; no limiting operation is involved. And yet the usefulness of this explanation perfectly fits Batterman's diagnosis of the explanatory power of asymptotic techniques. That is, the explanation is perfectly general—it ignores completely the causal and material features of the particular bridges of Konigsberg, and focuses solely on their *abstract structure*. And by focussing on this abstract structure, it enables us to see how the same result will hold for *any* system of bridges with that structure, regardless of material constitution or any particular causal details about the kind of walk attempted.

Of course, it would be a large task (and an empirical matter) to decide whether the majority of idealised explanations in science fit Pincock's or Batterman's model. But there is another, more fundamental reason to worry about Batterman's focus on the use of singularity generating limiting operations. In short, Batterman seems to think that the kinds of asymptotic techniques he discusses will take us from a representation in which *every* parameter is physically interpreted to one in which some features of the representation can no longer receive a physical interpretation, and that that is how idealisations are generated. But in many actual scientific applications, we may not have such a fully physically interpreted model in the first place. That is, our use of limiting operations may well take us from one idealised model to another idealised model. And if the representations to which asymptotic techniques are applied are *already themselves idealisations*, the use of asymptotic techniques cannot be the key to idealisation in quite the way Batterman assumes.

I think this point important, because it undermines one of Batterman's central claims in his argument against the mapping approach. Remember that for Batterman, any mapping approach will be committed to a Galilean view of idealisation, in which every idealisation is, in principle, eliminable in favour of a completely accurate representation of the empirical scenario. But in the light of the previous paragraph, this seems an extremely implausible requirement for the mapping account to meet. In many cases, the mapping account will be attempting to describe a situation in which we already know that the empirical domain is unfaithfully represented by the mathematical model we map onto. In other words, it is beginning to look as though Batterman's objection is just that the mapping account cannot handle any cases of idealisation, because it can never allow that the empirical domain might have more structure than its mathematical counterpart. But this is just the objection from idealisation that we addressed when discussing Bueno and Colyvan's inferential conception, and we have already seen how to solve this difficultly by taking over some of their modifications.

A final worry about Batterman's objection involves paying closer attention to the way the explanatory payoffs are generated. As we saw above, having the idealisation in place allows us to discard or ignore aspects of the physical system that are irrelevant to the features we are interested in explaining. Or in other words, if we tried to make our representation of the empirical phenomena physically complete, we would only be adding "explanatory noise", obscuring those features of the empirical structure that are actually responsible for the phenomena of interest. It is *this* feature of the explanatory uses of idealised mathematical representations that is actually pulling the explanatory weight, and it is *this* feature that makes the telling of a de-idealising “Galilean” story impossible. It is the loss of explanatory clarity, not the use of limiting operations that renders the Galilean conception an implausible account of idealisation. But now note that this makes the usefulness of idealised mathematical-scientific representations a function of their impoverished content, and we have already seen above that both the enhanced mapping account *and* Bueno and Colyvan’s partial structures approach can handle impoverished representation at the immersion stage.

Once again, then, we have seen reason to regard whatever evidence there is of the need for a derivation stage as inconclusive. It is important to note that I am not ruling against incorporating a place for mathematical derivations into the enhanced mapping account once and for all: I believe that as long as we bear in mind the primacy of representation, there is nothing to stop us from doing so. Indeed, it may well remain that predictive and unificatory uses of mathematics in science require us to make a place for mathematical derivation in our model. But pending a detailed investigation of the way in which mathematics functions in generating scientific predictions and unifying scientific theories, I hold that we have so far been given no convincing reason for regarding derivation as an essential part of our model of applied mathematics.

2.6 Conclusion

Before closing it will be helpful to recap the material of this chapter and discuss some of the further challenges facing the mapping account.

We have seen that by incorporating the immersion and interpretation stages of Bueno and Colyvan’s inferential conception into an enhanced mapping account, we have been able to meet some of the challenges facing a mapping theory of applied mathematics. Moreover, we have argued that it is the representational role of mathematics, and *not* its inferential role that is fundamental for understanding how mathematics is applied within the sciences. If this is correct, then we can help ourselves to the more sophisticated machinery introduced by Bueno and Colyvan, without endorsing their full inferential conception of mathematical applicability. We then saw ways in which we might resist Batterman’s claims that asymptotic techniques cannot be handled by the mapping theorist, either by following Bueno and Colyvan and introducing a derivation stage into our model, or else by challenging Batterman's claim that it is the generation of singularities at the limit that is doing the explanatory work.

The eventual picture of how the contents of mathematical-scientific representations are fixed looks like this: the contents of mathematical-scientific representations (the truth-conditions of mixed mathematical-scientific sentences) are given by the existence of a structure preserving mapping between the empirical domain under investigation and a suitable mathematical structure, *and* the existence of another mapping (not necessarily the inverse of the first) from the mathematical model back onto the empirical domain—remembering, of course, that the number of mappings at both the earlier immersion and later interpretation stages can be as large as we require to get the representation to function. At this stage of the investigation I want to leave open the question of the need for a derivation stage, noting only that we have not so far been required to make use of one in our model of applied mathematics, and that only a detailed examination of mathematics' role in prediction, confirmation, unification, and explanation will settle the matter finally.

In the next chapter I discuss a number of outstanding challenges to this kind of account of applications, and suggest that by incorporating the mapping account into a kind of ontological fictionalism we can solve these worries. In particular, and perhaps most disturbingly for the mathematical anti-realist, the mapping account as developed here seems highly ontologically inflationary. As we will see, this initial impression of ontological profligacy can be bulked up into a novel version of the indispensability argument for mathematical platonism.

It is here that fictionalism will prove useful, allowing us to move the story developed in this chapter into a kind of make-believe setting, within which its unpalatable ontological commitments can be contained. As we will see, the fictionalised version of the mapping account helps with some further worries we might have about the theory, so the resulting package of fictionalism plus the mapping account is attractive for more than just its ontological parsimony.

Chapter 3. Fictionalism and the Mapping Account

3.1 Introduction

In the previous chapter we saw how the mapping account of mathematical applications proposes to explain how mathematical-scientific representations get their content. Drawing on Pincock's work, I suggested that when mathematics is applied to some phenomenon of scientific interest, a structure preserving mapping is established between the empirical domain and some suitable mathematical structure; it is this mathematical structure that is then employed to represent the structure of the empirical domain. I then went on to argue that Pincock's approach already possesses the resources to adequately respond to a number of concerns raised for mapping account by Bueno and Colyvan, and that even if this were not the case, there is nothing to prevent the development of an enhanced mapping account, incorporating the elaborations suggested by Bueno and Colyvan. The conclusion, then, was that little reason had so far been found for abandoning Pincock's attractive and informative approach to scientific representation.

In this chapter we will look at the prospects for a fictionalised version of the mapping theory, one in which the dominant representational role is played by an account of pretence. At first this might seem like something of a change in direction, so before outlining the pretence theory I propose to adopt, I will briefly say something by way of motivation.

The first thing to note is that the mapping account is, at least prima facie, a platonistic theory. It talks of structure preserving mappings, and mathematical structures, and seems to quantify over these things. As we will see in the final section of this chapter, this raises the worrying possibility that adopting the mapping account may open us up to a variant of the indispensability argument in which the indispensable appearance of mathematical posits in our theory of mathematical applications argues for their inclusion in our ontology. If we are attracted to something like the mapping account of scientific representation, but independently suspicious of mathematical objects, this creates an unwelcome conflict.

Secondly, as we will see when we come to discuss the fictionalised mapping account, this pretence based approach solves a number of outstanding worries with the Pincock's version of the theory. In particular, we will see how an account of mathematical-scientific representation in terms of *imagined* structure preserving mappings can solve problems with the empirical side of the relation – in particular, how some non-mathematical stuff can be related by a mathematical function, and what we should say about the existence of the requisite structure within the empirical domain.

Finally, by incorporating the mapping account into a pretence based theory of mathematical-scientific representation we get an attractively detailed picture of the *content* of the applied-mathematical fiction. In this case, when mathematics is applied we are invited to pretend that there exists an appropriate structure-preserving mapping between the empirical domain and a suitable mathematical structure. In essence, then, applied mathematics is the story we tell about such mappings and mathematical structures.

The topics I have just discussed by way of motivation will be more fully treated of in the final section of this chapter. Before that, I need to introduce and motivate a pretence based account of mathematics that can do the job of incorporating the mapping account and drawing its platonistic sting. In the first section I will introduce the pretence theory of Kendall Walton (Walton, 1993), and suggest that his account of prop-oriented make-believe can supply the kind of anti-realist account of fictionalism a nominalist needs for applied mathematics. Much of this ground has already been covered adequately elsewhere and by others (Yablo, 2005; Leng, 2010), so I do not intend to spend very long discussing this aspect of my fictionalist account. It should also be noted that I am not wedded to a Waltonian pretence theory of fiction. Should it turn out that there are insuperable difficulties with the Waltonian account, I am happy to switch to some other pretence theory, that is, *so long as the alternative theory is suitably anti-realist.* The exact details of the workings of the pretence are not of the first importance for the mathematical fictionalist. What matters is that the resulting theory allows one to speak speak as if there are some things or entities, without being committed to the existence of those things. One way of doing that is via pretence, and it is Walton’s pretence theory with which I am most familiar. But should it transpire that there is a better theory out there, then I reserve the right to help myself to that.

Nor should it be supposed that the mathematical fictionalist is a hostage to the fortunes of debates in the philosophy of literature about the correct account of fiction and its ontology. The question of how best to account for mathematical ontology is independent of the question of how best to account for the ontology of fiction. Should it transpire that an anti-realist account of fiction was, for some reason local to the philosophy of fiction, untenable, then that might be a setback for the general project of defending a nominalist account of metaphysics across the board, but it would not show that mathematical objects exist. If pretence is not how fiction works, it might still be how applied mathematics works.

Nor, finally, am I particularly wedded to a pretence theory at all. The fictionalist claim is that mathematics is like fiction, in that it can be good without being true. This claim seems to be compatible with *any* anti-realist account of fiction, not only with a pretence theory. So I further reserve the right to help myself to *any* anti-realist account of fiction that will work, that is, that will underwrite the claim that mathematics is good without being true.

In the second section of this chapter, we will discuss the question of whether the mathematical fictionalist should be a hermeneutic fictionalist (mathematics, correctly interpreted, already *is* like a fiction; mathematicians already *are* engaged in something like pretence) or a revolutionary fictionalist (mathematics *should* be treated as a fiction; whatever mathematicians think they are up to, philosophers should treat mathematics as pretence).

This issue matters for two reasons. Firstly, the revolutionary fictionalist makes claims that, according to some, contravene the Naturalist's obligation to take seriously our best scientific theories. In other words, they are offering corrections to best science that no Naturalist philosopher is licensed to make. Secondly, the hermeneutic fictionalist makes claims that have empirical consequences. Hermeneutic fictionalism is a theory about the *actual* behaviour of agents engaged in applied mathematical discourse, and as such it makes claims about the best interpretation of that behaviour. But these empirical claims *could* be false, and some have argued that evidence exists to show that they *are* false. This means that we cannot just prevaricate between revolutionary and hermeneutic fictionalism, as we can with accounts of fiction, just helping ourselves to whatever works or whatever we are most familiar with. There are arguments to the effect that neither approach will work, and these arguments need to be countered if we are to defend the legitimacy of either way of being a fictionalist.

So in the second section we will investigate and respond to these arguments against revolutionary and hermeneutic fictionalism. The upshot of all this will be to favour, very slightly, the revolutionary variety. As we will see in the course of our discussion, hermeneutic fictionalism is just much more provisional than the revolutionary fictionalism, and there are simply more ways for it to fail. There may currently be no knock-down argument against it, but there are suggestive lines of enquiry that make a positive outcome for hermeneutic fictionalism seem unlikely, and should any of those lines of enquiry present negative results, we will have very good reasons to abandon fictionalism. The safe money, then, is on revolutionary fictionalism.

In brief, then, what I propose to defend in this chapter is a revolutionary fictionalist account of applied mathematics, whose content is given by a pretence involving the existence of structure preserving mappings between the empirical domain and some suitable mathematical structure. It is a kind of prop-oriented make-believe (to use Walton's terminology (1993, p.39)), with the empirical domain serving as the prop, and so an anti-realist account of the things (mappings, structures) we talk about within the pretence. We begin, then, with prop-oriented make-believe.

3.2 Walton and Prop-Oriented Make-Believe

Consider the following children's game. Some children are playing in the garden a game called *Avoid the Bears* (the example is Walton's (Walton, 1993, p.53); the name is mine). Clearly, at least if they are playing the game in an ordinary garden, their game does not involve escaping from the clutches of *actual* bears. Instead the children run around, avoiding tree stumps. Wherever they find a tree stump, they are moved to exclaim, "Oh, no! A bear!" Often they will then change direction, and seek to place some distance between themselves and the tree stump. This odd behaviour may be accompanied by cries of, “Run away from the Bear!” Upon stumbling onto a tree stump hidden within some weeds, Timmy is declared by the other children to have been “eaten by the bear”. Upon examining Timmy we find no tooth marks, claw marks, or any other signs of recent ursine ingestion. Moreover, it was a tree stump, and not a bear he stumbled into.

How are we to make sense of the childrens’ strange performance? We might consider the children insane or the victims of some febrile hallucination, and seek medical advice. We might check the medicine chest or the drinks cupboard to see if anything is missing. But most likely, at least if we are ordinary adults, we will regard the children as engaged in *make-believe*. The children are playing a game, one feature of which is that participants in the game are invited to make-believe that the tree stumps are bears. Wherever there are tree stumps, there are bears. The bears are to be imagined as large, and dangerous bears, and, moreover, to touch a tree stump is to be invited to make-believe that one has been mauled or that one has been eaten by the bear. It is this kind of activity, making-believe, that Kendall Walton makes the basis of his anti-realist theory of fiction (Walton, 1990), and more importantly for our purposes, his account of metaphor as prop-oriented make-believe (Walton, 1993).

We can see make-believe clearly in such children’s games as *Avoid the Bear*. But Walton suggests that this mechanism of pretence also plays a role in more adult activities of artistic engagement. In particular, for our purposes, Walton argues that make-believe is central to our activity of creating fictional representations. A fictional representation, for Walton, is one that represents its objects by prescribing imaginings concerning those objects (Leng, 2010, p.158). So, for instance, the game *Avoid the Bears* represents the tree stumps as bears by prescribing participants in the game to imagine of the tree stumps that they are bears.

How is this prescription to imagine, of the tree stumps that they are bears, established? It is at this point in his theory that Walton introduces the technical vocabulary of *props* and *principles of generation* (Walton, 1990, pp.35-43). Principles of generation are the often implicit rules of our games of make-believe that tell us when we are to imagine that something is the case, and what we are to imagine. So, for instance, in the game *Avoid the Bears*, we have the following principles of generation: (i) wherever there is a tree stump, there is a bear; (ii) if you step on a tree stump, you get eaten by a bear; (iii) if you are eaten by a bear, then you are dead; and so on. Notice that the first principle, (i), establishes that whenever we encounter a tree stump, we are to imagine of it that it is a bear. By so doing the principle ensures that our game *represents* tree stumps *as* bears.

In the children's game the rules of generation are largely explicit; indeed, they may have been agreed upon in advance, with the children stipulating, vocally, that this is how the game is to be played. But principles of generation need not be explicit. So it is a convention of novel reading that, other things being equal, one is to imagine that the sentences of the novel express truths. In so doing, we generate a prescription to imagine that what the sentences relate has actually happened. In other words, in our games of make-believe with novels, we represent the sentences of the novel as true, and thereby represent their contents as true, and the events they relate as having actually occurred. But notice that in this case, the principle of generation, that we are to imagine of the sentences in novels that they are true, is not explicit. We do not open a novel to a reminder that the sentences therein are to be imagined as relating real events. We already, implicitly, understand how to engage with novels, and how to use them as fictional representations. This is an important point, because very many games of make-believe work on the basis of principles of generation that are implicit. In particular, if our theory of applied mathematics is to be given a fictionalized treatment, it will likely be in terms of principles of generation of which the users of those mathematical-scientific representations are only implicitly aware.

Notice that in the case of principle (i) of the children's game, the children are instructed to imagine of the *tree stumps* that they are bears. The tree stump here is functioning as a *prop* in the game of make-believe. Again, in the case of novel reading, we are instructed to imagine of the sentences contained in the text of the novel that they are true. The text of the novel here is the prop. What is a prop? A prop is something like an occasion for the application of the principles of generation of our games of make-believe (Leng, 2010, pp.161-163). A prop is anything, a sentence, a physical object, a suitable situation or event, which, in concert with the principles of generation, generates a prescription to imagine that something is the case. Together, then, the principles of generation and the props of our games of make-believe prescribe certain imaginings, and it is on the basis of these prescribed imaginings that they represent things as being thus-and-so.

So far, our discussion has focussed on the way in which fictional representations represent certain real objects in certain ways. In particular, we have focussed on the way in which fictional representations *misrepresent* certain real objects, representing them as thus-and-so when, in fact, they are not thus-and-so. So the children's game, *Avoid the Bears*, misrepresents real tree stumps as bears. And in reading a novel, we misrepresent real sentences in the text of the novel as true, when in reality they are false (or mostly false). But fictional representation is not limited just to representing real world objects. Novel reading is instructive here, because by representing the sentences of the novel as true, the novel can further represent certain events *that may never have happened*, and certain people *who have never existed*. Consider the Sherlock Holmes stories of Conan Doyle. On the basis of the implicit rule of generation that the sentences of the stories are to be imagined as true (established, in part, by the presentation of those stories as factual reports authored by Dr James Watson), and the text of the stories functioning as a prop, we represent the career of a consulting detective, Sherlock Holmes, even though such a consulting detective never existed. This is an important point to bear in mind, for, when we come to discuss a fictionalized version of the mapping account, our fictional mathematical-scientific representations will involve us representing the physical world as standing in certain relations to a world of mathematical objects *we do not believe exist*. It is this feature of Walton's account that enables us to evade commitment to a world of morphisms, and mathematical structures.

Now for the most part, when we are engaged with a fictional representation like a Sherlock Holmes story or a game of *Avoid the Bears*, we are interested in the content of the fiction itself. We play the game of make-believe for its own sake, because we find it fun, or exciting, or aesthetically pleasing, or intellectually challenging. But this will clearly not do if we are looking to adapt this kind of approach to fiction to the case of mathematical-scientific representation. We do not build mathematical-scientific theories of the world just for the sheer fun of doing so. We expect them, at the very least, to be empirically adequate, and most of us sincerely hope that they are accurate representations of the world itself. Now fortunately, because the props of our make-believe games exist in the real world, there are forms of engagement with fictional representations that are not what Walton calls *content-oriented* (Walton, 1993, p.39); that is, there are ways of engaging with make-believe that focus on the props themselves, not on the content of the fictional representation. In this sense, fictional representations are Janus-faced: they can face inward, from the props, via the principles of generation, to the content of the fictional representation, which is what we are really interested in learning about; but they can also face outward, from the content of the representation, back via the principles of generation, to the props. In this way we can learn about the props, from the fictional representation. Walton calls this kind of make-believe *prop-oriented* (Walton, 1993, p.39), and it is this kind of prop-oriented fictional representation that we will make the basis of our account of applied mathematics.

To see how prop-oriented make-believe works, consider a case of Walton's chosen example of prop-oriented fiction, metaphor. So, for instance, suppose I say to you, "There are butterflies in my stomach." Now, unless you suppose me to have some very outré dining habits, you are likely to regard a literal reading of this sentence as totally false. I do not, and never have had, a belly full of butterflies. But nonetheless, you are unlikely to dismiss my utterance of this sentence as a bare-faced lie. Instead, you are likely to suppose that, far from telling you about the contents of my digestive tract, I am actually trying to convey some information about my mental state and associated physical sensations. I am, in fact, telling you I am nervous, and what that nervousness feels like.

How do I achieve this by uttering the false sentence "There are butterflies in my stomach"? Here is how Walton thinks prop-oriented make-believe can help give an account of metaphors like this (Walton, 1993, pp.45-46). If you hear me utter the sentence "There are butterflies in my stomach", you are entitled to treat this as a move in a game of make-believe. What kind of game of make-believe? One governed by the following rule, "when you are feeling nervous and experience a certain sensation linked to that nervousness, then you have butterflies in your stomach." In this game, the sensation and associated nervousness are the props; the principle of generation tells you that on the occasion of the symptoms of nervousness, you are to imagine that there are butterflies in your stomach. By uttering the sentence “There are butterflies in my stomach”, I invite you to join in the game of make-believe according to which there are butterflies in my stomach. And because you are familiar (implicitly) with the principle of generation for this game, you are able to work backwards from the content of the game (the pretend butterflies in my stomach) to learn about the props, namely, to learn that I am nervous and experiencing a concomitant digestive discomfort.

What we are doing in the case of metaphor, according to Walton (Walton, 1993, p.43), is indirectly representing, via a fictional (mis)representation, that the props are such as to make our pretences appropriate. As Yablo puts it: ‘A metaphor on this view is an utterance that represents its objects as being *like so*: the way that they *need* to be to make the utterance pretence-worthy in a game that it [the metaphor] itself suggests’ (Yablo, 1998, p.247) So, by uttering the sentence “There are butterflies in my stomach”, and inviting you to participate in the ensuing game of make-believe, I am indirectly representing my psychological state and physical sensations to be the way they need to be to make this pretence appropriate in the ensuing game.

Notice that, on this account, we may not be able to say very much about how the props need to be situated in order to make our pretence appropriate (indeed, that might be the very reason we are using a metaphor and not a literal declarative in the first place). So, the hypothesized, implicit principle of generation we were supposing to govern the game of make-believe suggested by the stomach butterflies metaphor says that, when we are experiencing a certain nervous sensation, we have butterflies in our stomach. But we may not be able to be very exact in our description of this sensation; its phenomenology may defeat our powers of expression. Indeed, we might not be able to say any more about it than that it is a nervous sensation or that feeling you get when there are butterflies in your stomach. But this doesn’t mean we don’t know how to work with the metaphor. We know that whatever the feeling is like exactly, it’s the one that licenses the game involving stomach butterflies.

Again, this is a point we will need to bear in mind when we come to the fictionalised account of applied mathematics below. We will probably not be able to say very much about the nominalistic portion of our theories that licenses the make-believe involving mathematics, precisely because mathematics is indispensable to our scientific representations. But this doesn't mean we cannot work with those representations, any more than it means we cannot work with metaphorical representations. The nominalistic content we are trying to express will just be what our theory says about the physical stuff that licenses the make-believe that that physical stuff is related to some imaginary mathematical stuff in the way that our make-believe says it is. The nominalistic content of our theories will turn out to be that things are such with the physical props to make appropriate our use of the mathematical metaphor. Applied mathematics, then, is just more stomach butterflies.

Hopefully the preceding material will have made clear what kind of use I intend to make of Walton's theory of make-believe. In particular, the account of applied mathematics given here will make use of the concept of prop-oriented make-believe to argue that mathematical-scientific representations are best seen as analogous to metaphorical representations, with the fictional content of the metaphor given by the prescription to imagine that there exists a suitable morphism between the physical stuff on the one hand, and a suitable mathematical structure on the other. The real, nominalistic content of the metaphor, on the other hand (analogous to the emotional state and concomitant sensations in the stomach butterflies metaphor), is that things are such with the physical props as to make our use of the mathematical-scientific make-believe appropriate. We will return to the discussion of this material in the final section of this chapter. But first, we turn to the discussion of revolutionary and hermeneutic fictionalisms, in order to precisify our account of applied mathematics still further. We have tentatively adopted Walton's account of fictional representation as make-believe. Can we say anything more definite about whether our adoption of Walton's theory should be undertaken in a revolutionary or more conservative, hermeneutic spirit?

3.3 The Revolution Will be Fictionalised

3.3.1 Revolutionary and Hermeneutic Fictionalism

John Burgess (2004) has raised the following worry for mathematical fictionalists: either the fictionalist must claim that mathematicians *actually* intend their utterances of mathematical sentences as contributing to a developing communal fiction or else they must claim that that is how mathematical utterances *ought* to be regarded. In the former case the fictionalist is committing herself to hermeneutic fictionalism and in the latter she is endorsing a revolutionary fictionalism. The problem, as Burgess sees it (Burgess, 2004, pp.23-30), is that neither of these positions is tenable.

The problem for the hermeneutic fictionalist is that she has endorsed a controversial empirical claim about the actual linguistic intentions of practising mathematicians. Not only is this claim controversial, it looks, prima facie (but more on this later), downright implausible (Burgess, 2004, pp.25-28). It just seems incredibly unlikely that mathematicians intend their utterances of mathematical sentences as parts of a developing communal fiction. The problem for the revolutionary fictionalist on the other hand is that it looks as though she is contradicting mathematicians on mathematical matters (Burgess, 2004, p.30). All mathematicians think that 7 is a prime number, but the fictionalist doesn't. She doesn't believe that there are any prime numbers, so a fortiori she doesn't believe that 7 is one of them. But offering philosophical correctives of this sort to actual mathematical practice just seems misguided. Good (naturalist) methodology frown upon this sort of philosophical interference. Even if we are not naturalists, we might wonder how appropriate it is for philosophers to correct mathematicians on just those matters mathematicians are supposed to be expert about. Indeed, given the respective track records of the two disciplines, such an approach seems "comically immodest." (Burgess, 2004, p.30)

In what follows I will attempt to defend a version of revolutionary fictionalism from the claim that it is comically immodest. My final conclusion will be similar to that of Mark Balaguer (Balaguer, 2009), that is, I will try to defend the claim that the revolution in question is limited in its ambitions. The revolution in question is limited to the ontological consequences of mathematical discourse; we are not interested in correcting or revising mathematical practice. Further, we should not expect mathematicians to be interested in the ontological consequences of their mathematical utterances (indeed, I will argue that we should not expect the average mathematician to have *any* attitudes towards the ontological consequences of his utterances) and so we should not expert mathematicians to be expert in these matters at all. In the division of theoretical labour, it is the philosophers who are (or at least ought to be) experts in ontological matters. So there is no comic immodesty in their correcting or advising mathematicians on these matters.

I have said that my position resembles that of Mark Balaguer, but it is not quite identical to his view. In particular, I motivate revolutionary fictionalism via a slightly different argument from that employed by Balaguer and the result, I believe, is a position that more closely fits with actual mathematical practice. If this is correct then we have one plausible version of fictionalism, revolutionary fictionalism, which we can then employ in our account of mathematical applications.

3.3.2 Mark Balaguer on Revolutionary Fictionalism

In his (2009) Mark Balaguer attempts a defence of mathematical fictionalism on two fronts. On the one hand, he endorses a variety of revolutionary fictionalism that he believes is immune to Burgess' "comic immodesty" objection (Balaguer, 2009, pp.153-157). On the other, he resists Burgess' claim that mathematical fictionalist is forced to choose between revolutionary fictionalism and hermeneutic fictionalism. He does this by outlining a third option for the fictionalist, Hermeneutic Non-Assertivism (HNA), that he believes is neither a revolutionary fictionalism nor a hermeneutic fictionalism. While his official position is revolutionary, he suggests that the fictionalist could endorse HNA instead (Balaguer, 2009, pp.157-161).

While this section primarily aims to defend revolutionary fictionalism, we will actually spend a great deal of time discussing HNA. Hopefully the reason for this dialectic strategy will become clear as we proceed. Before that can happen, though, I must first briefly sketch Balaguer's defence of revolutionary mathematical fictionalism.

In short, Balaguer's defence of revolutionary mathematical fictionalism rests on the claim that the revolution in question is not *mathematically* interesting and so not something we should expect mathematicians to be experts about. In Balaguer's words:

> (i) the fictionalist thesis—or if you would rather, the fictionalist revolution—is mathematically unimportant and uninteresting (it is only philosophically interesting, and the main target of fictionalism is not mathematicians or mathematical theory, but a certain philosophical view, namely, platonism); and (ii) the mathematically uninteresting issue that fictionalists raise—the one that leads to their disagreement with mathematicians—is not something that most mathematicians think or care very much about, and it is not something that they have any substantive expertise or training in. (Balaguer, 2009, p.154)

Balaguer has two arguments for (i). Firstly, no corrective is being offered to actual mathematical practice and no revisions or rejections are being suggested to established mathematical results (Balaguer, 2009, p.154). In this sense, the revolution the fictionalist agitates for is very different from the revolution that the intuitionists advocated (Brouwer, 1983; Heyting, 1983). Had mathematicians been convinced by the philosophical arguments of the intuitionists, then that would have necessitated a wholesale revision both of mathematical methods and mathematical results. *That* certainly would have been a mathematically interesting revolution. But the mathematical fictionalist agrees with the mathematician on which mathematical results are correct (in the sense of mathematically acceptable) and which methods are likely to produce correct results. The only place the mathematician and the philosopher are supposed to disagree is on the existence or otherwise of abstract mathematical objects and the existence or otherwise of abstract mathematical objects makes no substantive difference to mathematical practice.

Now I certainly think that Balaguer is on to something here, but it is not clear that it is enough to establish the innocuousness of revolutionary mathematical fictionalism. It does seem very plausible that the existence of mathematical objects as abstracta makes no real difference to actual mathematical practice, but the problem is that this just doesn't establish (i). What we need is to establish that mathematicians do not regard ontological matters as important (or as mathematically important) and pointing out that mathematical practice would be unaffected by their existence or non-existence doesn't achieve yet this. *Even* if mathematical objects would make no difference to mathematical practice, it might still be the case that mathematicians regarded speculating on their existence as important and important *qua* mathematics. So what we need to establish is that mathematicians do not believe this, that they do not regard the issue of mathematical ontology as important qua mathematics.

Balaguer's second argument for (i) attempts to establish this. Balaguer notes that mathematical proofs and arguments in mathematics do not seem to settle the question of the existence of abstracta one way or the other. But this is prima facie suspicious: if mathematicians thought the issue of mathematical ontology important qua mathematics, then it would seem that there is a lacuna in all existing mathematical proofs. But, of course, there is no such lacuna (or no such lacuna is perceived to exist), mathematical arguments and proofs are all right as they are. This suggests that the fact that mathematical methods do not settle ontological questions is down to the fact that these questions just are not important mathematically (Balaguer, 2009, pp.154-159).

Now Balaguer goes on to gloss this by saying that mathematical methods provide no evidence for the existence of abstract mathematical objects, because "they essentially assume that there exist mathematical objects." (Balaguer, 2009, p.55) It is here that I demur. I think that Balaguer has identified one important phenomenon, namely the absence of mathematical evidence for the existence of abstract mathematical objects, but I believe he has misdiagnosed the reasons for this absence. In short, I will go on to argue that the reason ontological matters are absent from mathematical practice is that mathematicians have no attitude towards them whatsoever. They do not assume the existence of mathematical objects, they simply don't know or understand what that existence would involve, and so have no beliefs (or any propositional attitudes) towards mathematical existence at all. The defence of this claim though will have to await an evaluation of Balaguer's HNA (hence the rather circuitous dialectic).

Balaguer doesn't really attempt to argue for (ii) at all, just noting that it "is entirely obvious." (Balaguer, 2009, p.55) I certainly think that it is obvious that mathematicians do not think or care about ontology, but again I hope that my defence of revolutionary fictionalism will show *why* this is. In short, I want to suggest that the reason mathematicians do not think or care about ontology is because they have no attitudes towards ontological matters at all. They do not need to possess such attitudes in order to understand and do mathematics and so (in general) they fail to develop them. Again, though, the defence of this claim must wait until later in the chapter. For now I merely want to indicate my points of agreement and disagreement with Balaguer. I agree with Balaguer that mathematical existence (in the ontologically robust sense: existence as abstracta) is mathematically unimportant, but I disagree that this is down to mathematicians simply assuming in their proofs and arguments the existence of mathematical objects (one way to see this is to consider the plausibility of hermeneutic realism, the thesis that mathematicians intend their utterances as literal truths about a realm of acausal entities outside of space and time. Put so baldly, this thesis seems no more plausible than hermeneutic fictionalism). And I agree with Balaguer that mathematicians are simply not interested (or most mathematicians are not often interested) in ontological matters. But again, I think that we can say a great deal more than Balaguer does about why this should be so. Sadly, the elaboration of these points must await the resolution of our discussion of Hermeneutic Non-Assertivism.

3.3.3 Balaguer on Hermeneutic Non-Assertivism

Balaguer agrees with Burgess about the viability of hermeneutic fictionalism (Balaguer, 2009, p.152), that is, he believes the claim that mathematicians actually intend their utterances to be parts of a work of fiction is hopelessly implausible. One might worry that this is a very swift dismissal. After all, hermeneutic fictionalism is an *empirical* claim, a claim about the actual linguistic behaviours and intentions of actual real world language users. One would expect the rejection of such a claim to be backed up by actual empirical evidence about what those language users really are up to. Unfortunately Balaguer (and Burgess) fail to provide such evidence; they simply assume that were the relevant tests to be carried out, the results would be unfavourable to the hermeneutic fictionalist.

We will return to this issue in the next section of the chapter, so for now we can set it aside. Let us assume, for the sake of argument, that Burgess and Balaguer are correct that hermeneutic fictionalism is a non-starter. As we have seen, one way in which we can nonetheless endorse fictionalism, despite Burgess' worry is by going revolutionary. But Balaguer does not leave things here; he thinks, further, that

Burgess is wrong that these are the only items on the menu. Fictionalists, contra Burgess, do not have to choose between hermeneutic and revolutionary fictionalism, they might try Hermeneutic Non-Assertivism instead (Balaguer, 2009, p.159).

Here is how Balaguer defines HNA:

> Hermeneutic non-assertivism: When typical mathematicians utter sentences like ‘Every number has a successor’ and ‘4 is even’, they should not be interpreted as saying what these sentences say, and indeed, they should not be interpreted as saying anything, i.e., as asserting propositions at all. (Balaguer, 2009, p.158)

As he sees it, given the failure of hermeneutic fictionalism, this is the only viable option left to the fictionalist whose conservative instincts baulk at revolution. That is, the conservative fictionalist doesn’t want to regard the mathematician as saying something untrue, but if hermeneutic fictionalism is off the table and we cannot regard what the mathematician is saying as part of a fiction, the only option left is to claim that mathematicians are not saying anything at all. In other words “one might claim that ordinary mathematical discourse is a “language game” in which the “players” typically do not assert what their sentences actually say.” (Balaguer, 2009, p.159)

Now Balaguer does not claim that HNA is true. He doesn’t even attempt to defend its plausibility. What he does argue is that HNA is not obviously false in the way that he takes hermeneutic fictionalism to be (Balaguer, 2009, p.159). In order to establish this he defends HNA against one objection (presumably he believes that this is the kind of objection that is most likely to present itself to a philosopher upon first learning of Hermeneutic Non-Assertivism). The objection Balaguer responds to is that mathematicians intend their mathematical utterances as assertions of propositions, because they think that the sentences they utter are *true*. Balaguer responds to this by asking whether a mathematician is likely to have the *right* sense of true in mind (Balaguer, 2009, pp.160-161). He does not dispute that mathematicians *in some sense* believe that their mathematical utterances are true, but “it is not clear they think their utterances are true in the way that fictionalists have in mind when they claim that our mathematical theories are not true.” (Balaguer, 2009, p.160) And Balaguer thinks that the sense of true that fictionalists have in mind is "a kind of truth that, for the sentences in question, requires accurate description of actually existing objects". (Balaguer, 2009, p.160)

Now it is contentious whether this is the kind of truth fictionalists have in mind; as Armour-Garb points out (Armour-Garb, 2011a, p.343), this seems to rule out any combination of fictionalism with a deflationary account of truth. Nonetheless, I think Balaguer's point can be made without the questionable metaphysics of truth. The point, in short, is that whatever the correct metaphysics of truth, the chances are that when mathematicians say that their utterances are true, they probably don't have that metaphysics in mind. What they actually mean is something more like: this is a mathematically correct thing to utter. And this latter construal of the claim that mathematical utterances are true is consistent with HNA.[8]

This, then, is the position Balaguer suggests as an alternative to revolutionary fictionalism: mathematicians do not intend to assert propositions when they utter mathematical sentences (they do not say what those sentences say), and they do not have a philosophically interesting sense of true in mind when they claim that those sentences are true. In the next section we will examine an objection to this position, due to Armour-Garb. My primary interest, though, is not in the question of whether Balaguer or Armour-Garb is correct about HNA, but in what Armour-Garb's account of mathematical understanding tells us about revolutionary fictionalism.

3.3.4 Armour-Garb on Mathematical Understanding

As we noted above, Balaguer does not really offer a defence of HNA and what he does say in its favour is intended only to head off a certain prima facie plausible objection that he believes is likely to prejudice philosophers against the view. But as

[8] It may be thought that mathematicians intend their uses of the truth predicate to be continuous with their uses of that predicate in other, non-mathematical contexts, and hence, that even if they have no specific metaphysics of truth in mind, they do intend their mathematical utterances as true in the same way as their more quotidian utterances are true. My suspicion is that this kind of argument is susceptible to a similar kind of objection to the one presented above: is it actually *obvious* that this is how mathematicians intend their deployment of the truth predicate? I suspect the question, if asked of mathematicians, would be more likely to elicit frustration or confusion than any firm avowal.

Armour-Garb reads Balaguer (Armour-Garb, 2011a), there are certain remarks that suggest one reason why we *might* take mathematicians not to be asserting what it is their utterances say. As Armour-Garb sees it, Balaguer is working with a presupposition about assertion, namely "The presupposition...that there are conditions that have to be met in order for an assertoric utterance to count as an assertion and that mathematicians do not satisfy those conditions." (Armour-Garb, 2011a, p.339) In particular, Armour-Garb thinks that Balaguer is committing himself to the claim that if mathematicians understand (in a certain technical sense) the mathematical sentences they assertively utter, then they should be interpreted as asserting those sentences. A defence of HNA must then show that mathematicians do not understand the sentences they assertively utter.

Armour-Garb's idea here is that when mathematicians deploy mathematical concepts, they do so in virtue of their possessing what Balaguer calls a Full Conception (Balaguer, 2009, pp.141-144). So, for instance, when mathematicians deploy the concept of a natural number, they do so in virtue of their Full Conception of the Natural Numbers (FCNN). The FCNN is essentially a set of sentences, certainly including the axioms and (perhaps some) theorems of standard arithmetic, but also perhaps containing sentences expressing mathematicians' intuitions about the natural numbers. But, Armour-Garb notes (Armour-Garb, 2011a, p.140), the existence (or otherwise) of mathematical objects as abstracta does not seem to be a part of the Full Conception of any mathematical domain. When, for example, mathematicians deploy the concept of Natural Number, they do so in virtue of a FCNN that says nothing (contains no sentences) about the metaphysical status of natural numbers.[9]

What Armour-Garb seems to be getting at here is that anything not part of the Full Conception of some mathematical domain need not be understood in order to do successful mathematics within that domain (Armour-Garb, 2011a, p.140). And, presumably, if something need not be understood by mathematicians in order to do

[9] Obviously the FCNN will contain sentences like "There exists a number," and "There are an infinite number of primes," that on their surface seem to claim that, at a bare minimum, some numbers exist. What Armour-Garb seems to have in mind here is that these sentences say nothing about the metaphysical nature of the numbers, their existence *as* abstracta, and that this is this what matters in the debate between the fictionalist and the Platonist.

mathematics, then it is unlikely that they will (in general) understand it. So if the metaphysical status of numbers is not part of the FCNN and the FCNN is all that is necessary to do arithmetic, then it is unlikely that mathematicians will understand the metaphysical consequences of their arithmetical utterances. And given that understanding of one's utterances was a necessary condition for assertive utterance, it seems that mathematicians' utterances of mathematical sentences cannot be assertive. This leads Armour-Garb to formulate the following revised (or precisified) version of HNA:

> HNA∗: When typical mathematicians utter M sentences, they should not be interpreted as either understanding or believing what those sentences say about the world and should not be interpreted as saying – that is, as asserting – any such propositions at all about the world. (Armour-Garb, 2011a, p.339)

This, then, is the position that Armour-Garb goes on to criticise.

The gist of Armour-Garb's criticism is that HNA* incorporates a false picture of mathematical understanding. In short, Armour-Garb endorses the presupposition that assertion involves understanding, but he disagrees with Balaguer (or, at least, with his reading of Balaguer) on just what it takes to understand mathematical sentences. As Armour-Garb reads Balaguer (Armour-Garb, 2011a, p.340) he is committed to the view that in order to understand our mathematical utterances, we must understand what the sentences we utter *say,* in the particular sense of *what they say about the world*. But, Armour-Garb argues, that just isn't a sensible way of construing the verb 'say' here. Instead, Armour-Garb suggests that we should construe 'say' in the above account of understanding as requiring that we know what the sentences we utter *mean* (Armour-Garb, 2011a, p.341). And, as Armour-Garb sees it, knowing what the sentences we utter *mean* can, on occasion, require much less than knowing what our sentences say about the world and its denizens (Armour-Garb, 2011a, p.341).

In order to motivate this claim, Armour-Garb considers the case of truth talk (Armour-Garb, 2011a, pp.341-342). Most ordinary speakers, it seems, understand truth talk perfectly well. We can have true beliefs about which sentences are true and we can understand what others mean when they say that certain sentences are true and others false. But it seems very unlikely that ordinary speakers of English have any views or attitudes about the correct philosophical interpretation of truth talk. As Armour-Garb puts it "...understanding 'true' does not require that one have any attitude at all about what truth is." (Armour-Garb, 2011a, p.341) In other words, knowing what the word true means (in the sense of being a competent participant in truth-discourse) does not require knowing what one's truth-sentences say about the world. We do not need to know what property the word "true" stands for, nor even that there is such a property (if there is) in order successfully to communicate using the word "true". And if we need not know or have any attitude at all about the metaphysical status of the truth predicate in order to understand sentences containing it, then it seems we need not have any knowledge of or attitude about the metaphysical status of the truth predicate to *assert* sentences containing it.

The case (or so Armour-Garb claims (Armour-Garb, 2011a, p.342)) is the same for mathematical sentences. It is not necessary for mathematicians to understand the sentences that they utter that they know what those sentences entail about the metaphysical feng-shui of the world. For mathematicians to understand the mathematical sentences they utter, they need only know what those sentences mean (in the sense of being able to communicate successfully using those sentences). And, Armour-Garb contends (Armour-Garb, 2011a, pp.342-343), there is no more to this latter accomplishment than possessing the Full Conception of whatever mathematical concepts are involved in the sentences in question.

Returning once more to the arithmetical case, the FCNN contained both the axioms of arithmetic and mathematicians' informed intuitions about the natural numbers. But what more could be required to do mathematics? And what more could be required to communicate successfully with other mathematicians? It seems nothing. And so it seems as though the FCNN is all mathematicians would need to know in order to know what their arithmetical sentences meant. But the metaphysical status of numbers was no part of the FCNN, and that strongly suggests that knowing the metaphysical status of the natural numbers is not required in order to understand what arithmetical sentences *say*. And if all that is true, then it seems mathematicians do not need to understand the metaphysical consequences of their assertive utterances in order to be interpreted as *actually* asserting the sentences those utterances say. As Armour-Garb concludes, “if understanding one’s M [mathematical] sentences is sufficient for us to interpret one as asserting, when one (assertorically, sincerely) utters the sentences that one does, then it seems to follow, contra HNA, that mathematicians, when they utter the M sentences, are asserting.” (Armour-Garb, 2011a, p.343)

3.3.5 Balaguer vs. Armour-Garb

It is not my intention to adjudicate between Balaguer and Armour-Garb here. Instead I would like to put Armour-Garb’s account of mathematical understanding to work in a defence of revolutionary fictionalism. But before turning to that I suppose I should say something about the arguments we have just reviewed. The first thing to note is that it is not clear that the position Armour-Garb is attacking is the same as the one Balaguer defends. Balaguer says nothing about what mathematical understanding involves and certainly doesn’t motivate Hermeneutic Non-Assertivism on the grounds that mathematicians do not understand what the sentences they utter mean. What Balaguer does say on this score is:

> it seems to me that the most plausible versions of HNA involve the idea that when typical mathematicians utter mathematical sentences, they are doing something that differs from asserting (or W-asserting [W-assertion is just ordinary assertion, assertion of propositions]) in a pretty subtle way, so that the difference between W-asserting and this other kind of speech act is not obvious. (Balaguer, 2011, pp.346-347)

This strongly suggests that Balaguer is not interested in whether mathematicians understand the sentences they utter, but in a difference between apparent assertoric utterances that is much more subtle and insidious than the difference between understanding and ignorance. Indeed Balaguer says as much. He characterises Armour-Garb’s argument thus:

> 1. Mathematicians have the concept number, and they accept various number sentences, e.g., ‘3 is prime’, ‘2+2=4’, and so on. Therefore,

2. Mathematicians understand their mathematical sentences. But

3. If mathematicians understand these sentences, then the best account of what they are doing when they utter them is that they are making assertions. Thus,

4.When mathematicians utter mathematical sentences, they are making assertions, and so HNA is false. (Balaguer, 2011, p.346)

And, after noting that Armour-Garb spends most of his time arguing for 2, he suggests that his own target is actually 3 (Balaguer, 2011, p.346). That is, what Balaguer wants to establish is that *even if* mathematicians understand the sentences they utter, they still need not be regarded as actually asserting. In short, what Balaguer rejects is Armour-Garb's presupposition that understanding an apparent (sincere) assertoric utterance is sufficient for assertion. The distinction between assertion and what Balaguer thinks mathematicians are up to is just too subtle for that.

Now this, of course, raises further difficult questions for Balaguer. If the distinction between assertion and whatever it is mathematicians are up to is as subtle as all that, then how on earth are we tell them apart? It seems a plausible constraint on interpretation that when someone sincerely, assertorically utters a sentence that they understand, we should treat them as actually asserting what it is their utterance says. After all, if understanding and assertoric utterance combined are not evidence of assertion, what is? How could we ever tell, if Balaguer was correct, whether anybody was asserting the sentences their utterances expressed? I think these are certainly huge challenges for Hermeneutic Non-Assertivism, but it is worth noting that nothing Armour-Garb says either suggests or develops them. I suggest, then, that the case on HNA is still open.

3.3.6 Understanding and Revolutionary Fictionalism

So far, so inconclusive. But, after all, our interest was not really in HNA, but in revolutionary fictionalism and I believe that Armour-Garb has given us the tools necessary to respond to Burgess' argument. Remember that what we needed was some way of showing that, even if fictionalists are committed to a mathematical revolution of sorts, the revolution in question is not objectionable since it concerns only parts of mathematics that mathematicians themselves are not interested in. Now Balaguer attempted to argue for this conclusion by suggesting that the revolution is mathematically innocuous because mathematical methods simply presuppose the existence of mathematical objects, they do not establish them. Now I think this is troubling and one reason that I think this is troubling is that it is not clear that mathematical methods *do presuppose* the existence of abstract mathematical objects. Indeed, one of the motivations for nominalism (one of the psychological motivations, perhaps; I am not sure it could be bulked up into a substantial philosophical objection to Platonism) is that it seems to make no difference to mathematical practice *whether or not* abstracta exist. And if mathematical methods would remain unchanged, even if the world were devoid (as it is) of all non-concrete things, that strongly suggests that mathematical existence (in the Platonic sense) *is not* presupposed in mathematical practice.

We also saw Balaguer defend revolutionary fictionalism on the grounds that mathematicians (in general) do not understand ontological issues. They are not experts on whether abstract objects exist or how one might go about establishing this. But Balaguer was content to leave things here, noting the lack of understanding without explaining it. Now I believe we can deploy Armour-Garb's ideas about mathematical understanding to clear up these two issues; that is, we can use Armour-Garb's account of mathematical understanding to show why mathematical methods are not regarded as faulty, even though they say nothing about Platonic mathematical existence (and we can do so without claiming that mathematical methods simply assume there are Platonic mathematical objects). *And* we can explain why it is that, in general, mathematicians fail to have any understanding of or attitudes towards ontological matters. That would establish Balaguer's thesis that the revolution in question is not a mathematically objectionable one. But I believe we may be able to do even more: we may be able to say that the revolution in question is not a mathematical revolution at all.

We will start with the question of mathematicians' understanding of ontological matters. Let us agree with Armour-Garb that all there is to understanding mathematical sentences is possession of the Full Conception of whatever mathematical domain is under discussion. That is, let's agree with Armour-Garb that all there is to knowing what mathematical sentences *mean* is possessing the relevant Full Conception. Now we recall that the Full Conception of a mathematical domain never seems to include sentences about the metaphysical status of the objects of that domain. That is, mathematical understanding *never* involves knowing about the metaphysical status of mathematical objects. Nor does it involve taking any attitude towards the metaphysical status of mathematical objects whatsoever. Because one can understand mathematical sentences perfectly well without knowing or believing or having any attitude at all about the metaphysics of mathematics (precisely because this is not part of the one thing, the Full Conception, upon which mathematical understanding depends), it seems one can do mathematics, one can even become quite expert about mathematics, without knowing or believing or having any attitude about the metaphysics of mathematics at all.

Why then should we expect mathematicians to know or believe or have attitudes about the metaphysical status of mathematical objects? At best, such a proceeding would count only as a hobby, and a somewhat outré one at that. Certainly there seems no reason to expect mathematicians to become *experts* about something so utterly nugatory from the point of view of their professional interests. So we cannot be treading on mathematicians' toes if we presume to correct them on something they may know, believe, etc. nothing about.

What about the first point, the point about mathematical methodology? Well, it certainly cannot be that mathematical methods fail to decide ontological questions because they presuppose the existence of abstracta. Suppose, in order to see this, that we enlarge the full conception so that it includes, not only axioms and informed intuitions, but also however much proof theory is required to understand how to construct and evaluate proofs, and perhaps practising mathematicians' mature hunches about which kinds of strategies are likely to be fruitful in the domain in question.

Once again, this seems like all that is necessary to do mathematics successfully. And once again, our Full Conception, even engorged with proof theory and conventional wisdom, says nothing about the metaphysical status of its own posits. Platonic existence is not so much assumed as ignored. But this suggests an alternative reason why we feel there is no lacuna in our mathematical methods: understanding of mathematics and application of its methods can be achieved perfectly successfully without our having *any attitudes whatsoever*, not knowledge, not belief, not ardent desire, not anything, towards abstract mathematical objects. We do not presuppose their existence; we just don't care. Nor need we care. And now we can see why it is that we feel the existence and non-existence of abstract mathematical objects makes no difference to mathematical practice: there is nothing in the Full Conception, the only thing, we are assuming, that matters for mathematical understanding, that could settle the matter either way.

If the preceding arguments have been anywhere near the mark, we have now established the conclusion that the revolution the fictionalist recommends is not a mathematically interesting one (and so, not a philosophically troubling one); and it is not a mathematically interesting revolution precisely because no part of mathematical understanding is involved in or corrected by it. But now I want to attempt to go further. As Balaguer points out, one way in which we might try to defend revolutionary fictionalism is by arguing that the revolution in question is not a mathematical revolution at all (Balaguer, 2009, p.156). Leaving mathematical practice untouched and concerning itself only with the ontological issue of the existence of abstract mathematical objects, the revolution the fictionalist desires is a paradigmatically *philosophical* one. As we have seen, this is not the position that Balaguer himself defends, partly on the grounds that he can see no way of separating the philosophical sentences from the mathematical ones (is "Prime numbers exist in Platonic heaven" mathematical or philosophical? It seems a bit of both) (Balaguer, 2011, p.346). What I want to do before concluding is to (tentatively) suggest that Armour-Garb's notion of mathematical understanding may help us here as well.

First, let us say that something counts as a mathematical claim *only if* it is part of the Full Conception of some recognised part of mathematics (perhaps we will have to enlarge our Full Conception, as we did previously, so as to include methods and methodological hunches). Now one reason we might argue that this is all there is to being a mathematical claim (I say *might*; I don't claim to be certain of this) is that this is all it takes to understand mathematical sentences, all it takes to communicate with other mathematicians using mathematical sentences, and all it takes to do mathematical work successfully. Now, of course, beyond this there are the text books, the professional associations, the journals, and conferences and so on and so forth that go to make up Mathematics as a discipline. I do not mean to claim that all there is to Mathematics is what is included in all the Full Conceptions of all the various mathematical domains. But it might be that inclusion in some Full Conception is what it takes to counts as a mathematical *claim*.

If that is the case, if the mark of a distinctively mathematical claim is its presence in some full conception, then we can at least attempt a defence of the stronger position that, not only is the fictionalist revolution not mathematically interesting, it is not a mathematical revolution at all. And we might argue that it is not a mathematical revolution because it is a revolution in the way we interpret certain sentences (from a realist interpretation to a non-realist one). And, of course, questions of the correct interpretation of sentences *from* the full conception will not be be part *of* the full conception of any mathematical domain. Hence, claims made about the correct interpretation of mathematical sentences, whether, that is, we construe them realistically or nominalistically, are better thought of as philosophical claims, not as mathematical claims at all. And if that is correct, then, because the fictionalist revolution only affects this question of the correct interpretation of mathematical sentences, it is a philosophical, and not a mathematical revolution. We cannot, therefore, object to the revolutionary fictionalist that she is trespassing on mathematical territory.

3.3.7 Concluding Remarks

In this section I have attempted to defend revolutionary fictionalism from the objection that it is "comically immodest". In doing so I have taken over some ideas about mathematical understanding developed by Armour-Garb in response to Mark Balaguer's Hermeneutic Non-Assertivism. In short, the idea is that all that is involved in understanding mathematical sentences (and to some extent in doing mathematical work) is what is contained in the Full Conception of the mathematical domain in question. If this is true, then we can see why we should expect the fictionalist's revolution to be so mathematically uninteresting: the Full Conception of a mathematical domain *never* contains sentences about the metaphysical status of the entities in that domain. And because the Full Conception is all there is to mathematical understanding, we should not expect mathematicians to develop attitudes towards issues (like the ontological question) that contribute little to understanding and developing their professional work.

I have also attempted to defend the stronger claim that, not only is the fictionalist revolution not mathematically interesting, it is not really a mathematical revolution at all. I have done so on the grounds that the fictionalist revolution involves the reinterpretation of sentences from the Full Conception of some mathematical domain, and that claims about the correct interpretation of sentences within the Full Conception are not themselves part of the Full Conception. And because all there is to being a mathematical claim is being part of some Full Conception, the fictionalist revolution does not affect any mathematical claims at all.

If either of these strategies is successful, then revolutionary fictionalism is vindicated.

In the next section of this chapter we turn to the evaluation of hermeneutic fictionalism. Can we discover any reasons for preferring a revolutionary to a hermeneutic fictionalism (or vice versa)? Once we have an answer to this question, we will have a better idea of the shape of the fictionalist account of mathematical practice to be developed in the final section of this chapter, and within which we will embed the mapping approach to applications.

3.4 Hermeneutic Mathematical Fictionalism

In this section, we turn to the evaluation of hermeneutic fictionalism. Hermeneutic mathematical fictionalism, by contrast with the revolutionary variety we discussed previously, is the view that, while a face value reading of mathematical sentences makes them come out false, nonetheless everything is all right with mathematical discourse. And everything is all right with mathematical discourse, because sincere utterance of mathematical sentences is *not* assertion of the contents of those sentences, and *acceptance* of mathematical sentences need not be belief (Kalderon, 2005a, p.112). The sentences are already put forward in a make believe spirit, or it is merely pretended that they are true, or else we hedge with fictionalist prefixes or prefaces our utterances of them. And this *is* supposed to be an interpretation of what mathematicians are up to; making up mathematical fictions is not just something we (philosophers) can do, it is a game in which mathematicians can and already do indulge.

We saw in the last section Burgess arguing that the revolutionary fictionalist is committed to revising mathematicians' considered judgments about the truth and falsity of mathematical sentences. This was taken to involve the fictionalist in a criminal trespass on the mathematician's territory. It is the mathematician who is expert in which mathematical sentences are true, not the philosopher. And no philosophical argument or prejudice should be permitted to overturn mathematicians' considered judgments about which mathematical sentences are true or false. To this we objected that facts about mathematical understanding suggest that the revolution the fictionalist recommends is less radical than it appears. There is no need to recapitulate this defense here.

So what does Burgess think is wrong with hermeneutic mathematical fictionalism? In contrast with the revolutionary fictionalist, the hermeneuticist's problems are not philosophical – involving a transgression of the proper boundaries of philosophy – but scientific. In short, Burgess suggests (Burgess, 2004, pp.25-28), there simply is no evidence to back up the hermeneutic fictionalist's claims. Hermeneutic fictionalism is an empirical thesis about the pragmatics of mathematicians' utterances of mathematical sentences. It says that those sentences are not assertions, and the attitude of mathematicians towards those sentences is not belief. But surely this should be backed up by empirical evidence? And the evidence has not been provided (we will return to this point below. More accurately, I think that Burgess would argue that the limited evidence hermeneutic fictionalists have offered does not actually support their fictionalist reading of mathematical discourse (Burgess, 2004, p.28).

In this section I want to draw on some arguments of Mark Eli Kalderon (Kalderon, 2005a) to defend the hermeneutic mathematical fictionalist from Burgess' arguments. In short, I will argue that Burgess' attempts to undermine the evidence provided for hermeneutic fictionalism fail, and that they fail because Burgess thinks that evidence for Hermeneutic fictionalism must be evidence about what mathematicians *intend* by their utterances (that they intend them to be non-literal, for instance). On the contrary, hermeneutic fictionalism proposes that mathematicians *actually* engage in a fiction when uttering mathematical sentences, regardless of their intentions. I will conclude by noting a striking similarity between Kalderon's defense of hermeneutic fictionalism and various defenses of revolutionary fictionalism, and suggest a way forward for deciding between these two kinds of anti-realist approach.

3.4.1 Against Hermeneutic Fictionalism

Consider the following argument. There are no numbers, therefore there are no prime numbers, therefore there are not an infinite number of prime numbers, therefore Euclid's theorem is wrong. Consider submitting the preceding argument to a mathematics journal. Then consider one's chagrin when the submission is found not even to warrant a revise and resubmit.

Or consider the following philosophical argument. I have two hands, therefore the number of my hands is two. But two is an abstract object, therefore abstract objects exist, Consider submitting the preceding argument to a philosophical journal. Well, perhaps the submission would not be found quite so ridiculous as the attempted disproof of Euclid's theorem, but it is hard to imagine the philosophy of maths community converting, en masse, to mathematical Platonism.

What might we say about these examples? Well, we might, following Stephen Yablo (Yablo, 2005, p.88), take them to support hermeneutic fictionalism. That is, when we consider mathematical practice, we notice that it is curiously immune to arguments like these about mathematical ontology. Euclid's theorem is not a hostage to the fortunes of mathematical Platonism, and facts about our ordinary counting practices seem insufficient to establish mathematical Platonism. But this suggests that mathematicians are, on the whole, indifferent to the existence of mathematical objects. As Burgess puts it:

> On the one hand, objections to what mathematicians and others would ordinarily say about mathematical entities on the grounds of their alleged non-existence are regarded with scorn, while on the other hand, purported proofs of their existence are viewed with suspicion. (Burgess, 2004, p.25)

But this curious indifference to mathematical ontology on the part of mathematicians can be explained if we take utterance of mathematical sentences to be non-literal, part of some fictional or metaphorical practice. In that case, attempts to prove the literal truth of mathematical sentences or attempts to prove their literal untruth are both misguided, in the same way that attempts to prove the literal truth or untruth of a Sherlock Holmes story would be misguided. In both cases, questions about the truth of the participants' apparent assertions are in fact just silly questions, because the apparent assertions were never genuine assertions in the first place.

Or suppose we were to question mathematicians about their ontological beliefs, and were to discover that, on the whole, they don't have any. Perhaps they express puzzlement at our question, or offer confused or incoherent answers, or repudiate commitment to any ontology, or are willing to accept what philosophers tell them is the correct ontological view. We might think that *this* too suggests that mathematicians are engaged in a fictional practice, that their utterances of mathematical sentences are not assertions, that their attitude towards their sentences is not belief. And we might think that this is evidence for hermeneutic fictionalism, for much the same reason that we thought that the arguments considered above were arguments for fictionalism: namely, that it evinces a lack of interest on the part of mathematicians, qua mathematicians, in mathematical ontology, a lack of interest that would be adequately explained if they didn't really mean what they were saying (Burgess attributes the preceding argument to Charles Chihara (Burgess, 2004, p.24), though it is worth noting that Chihara is not himself a fictionalist).

Burgess objects to both lines of argument that they are founded on a mistake about the meanings of the words ‘commitment’ and ‘literal’ (Burgess, 2004, pp.25-26). In the case of the commitments of a discourse, for instance, those commitments are signaled by what the sentences of the discourse quantify over (once they have been translated into the idiom of first order logic with identity). Commitments are not signaled by what the participants of the discourse *intend* to refer to. A participant in mathematical discourse incurs a commitment to numbers just if she actually asserts ‘There are an infinite number of prime numbers.’ She incurs this commitment because she asserts the sentence, and because the sentence quantifies over prime numbers. What her further beliefs or intentions about the matter might be are taken by Burgess to be irrelevant (Burgess, 2004, p.25).

In the case of literalness, Burgess (Burgess, 2004, pp.25-26) thinks that both Chihara and Yablo require use of the word ‘literal’ (qualifying a particular utterance) to indicate the presence of something positive, something extra we add to our utterance of a sentence, like a particular kind of emphasis – the ontological equivalent of raising one’s voice. The fact that this positive feature is absent in mathematical discourse is then taken to be evidence that mathematicians do not intend their utterances literally. But Burgess (Burgess, 2004, pp.25-26) suggests instead that the word signals the absence of something negative, the absence of a particular kind of emphasis – the ontological equivalent of the upwards inflection. In that case, the literal reading of the sentence is the default, a non-literal reading requires finding evidence for, as Burgess puts it, 'mental reservation and purpose of evasion' (Burgess, 2004, p.25). That is, the hermeneutic fictionalist must find evidence that mathematicians actually intend their utterances to have a non-literal reading. And, Burgess, contends, such evidence has not been provided, hermeneutic fictionalists have not shown that mathematicians positively intend their utterances to be taken in a non-literal spirit.

3.4.2 Kalderon’s Defence of Hermeneutic Fictionalism

In this section I will argue that some comments due to Mark Kalderon suggest that Burgess’ objections fall flat. In particular, I want to suggest that how mathematicians intend their utterances to be taken, whether they intend utterance of mathematical sentences to be assertion of the contents of those sentences, even whether they intend their attitude to the sentences in question to be belief, may be irrelevant to the assessment of hermeneutic fictionalism. If Kalderon is correct, then mathematicians could mistakenly believe their utterances of mathematical sentences to be assertions, when in fact they are not. Again, they may mistakenly think that they believe the sentences in question, when they do not. In that case, the absence of a negative psychological feature underwriting our ascriptions of non-literalness to the sentences of mathematical discourse, the inability to locate the 'mental reservation and purpose of evasion', may only be a feature of mathematicians' own mistakes about the character of their discourse, and not evidence for the literalness of that discourse. In other words, mathematicians could be engaged in a gigantic game of make believe *without intending to be*, indeed, without even realising that that is what they are doing.

I think we can be pretty brief with Burgess' comments about commitments. Of course, we are committed to those objects whose existence is required to make true the quantificational claims we seriously assert. But what is at issue between the hermeneutic fictionalist and the realist is whether the sentences of mathematical discourse are *actually* asserted. I cannot see that anything that Burgess says about ontological commitment undermines the fictionalist's claim that the sentences of mathematical discourse are not in fact asserted. And if Burgess is just supposing that they are, then he is begging the question against the hermeneutic fictionalist.

Perhaps Burgess intends his remarks on commitment to link with his remarks on literalness: that is, both sides *ought* to agree that commitment is signalled by quantification within *asserted* sentences. What the hermeneutic fictionalist contends is that mathematical utterances, being non-literal, are not asserted. So, if the fictionalist is wrong about the non-literalness of mathematical discourse, then, given this account of ontological commitment, she must also be wrong about the commitments of mathematical discourse. However that may be, the important point is Burgess' claim that non-literalness requires evidence about the intentions of mathematicians in uttering mathematical sentences.

As we saw above, Burgess objects to the hermeneutic fictionalist that she must find some positive evidence for the existence of a certain kind of mental state, a unique kind of reservation or diffidence that marks the non-literalness of an utterance. We cannot assume that an utterance of a mathematical sentence is intended non-literally just because we cannot plausibly attribute to the utterer a positive intention to speak literally. Assumption of literalness is the default in interpretation. Nonetheless, I believe that this focus on the intentions and mental states of mathematicians is a distraction, and that by examining Kalderon's defense of his moral hermeneutic fictionalism we can see why.

The first thing to note is that Kalderon is responding to a somewhat different objection to hermeneutic fictionalism to the one we are considering here. Kalderon imagines a critic of his moral hermeneutic fictionalism offering the following challenge: "We, as speakers of moral language typically take our utterances of moral sentences to be assertions of the contents of those sentences. And typically we take ourselves to believe the moral propositions so expressed. Therefore, your fictionalism is not, after all, a hermeneutic one, for it requires making revisions to our moral beliefs and our attitudes towards our moral sentences." (Kalderon, 2005a, pp.140-142)

Kalderon responds by noting that, in the first place, first order moral practice will remain very much as it was (Kalderon 2005a, p.142). The moral sentences we accepted before we discovered the fictional nature of our moral practice, we will continue to accept. What will change is a set of what we might call second order beliefs about the practice, namely, our belief that acceptance of moral sentences is belief, and our belief that utterance of moral sentences is assertion. But, Kalderon claims, it is the first order stuff that matters for the fictionalist (Kalderon 2005a, p.142). Moral fictionalism would be revolutionary if it required us to revise our attitude towards, for instance, the sentence 'stealing is wrong', if we should go from accepting this sentence to rejecting it. But, Kalderon maintains (Kalderon, 2005a, p.141), this will never happen, even if we come to believe that moral acceptance does not involve belief. Moral fictionalism is hermeneutic because it leaves unrevised our attitudes towards our moral sentences.

Our second order beliefs about the practice will have to change, though (Kalderon, 2005a, pp.141-142). That is, while we may well believe that moral acceptance is belief, it is not in fact belief. So our beliefs about our moral attitudes require revision. Again, we may well believe that moral utterances are assertions, but, again, they are not. So we should cease to believe that moral utterance is assertion. But this need not trouble the hermeneutic fictionalist. First order moral practice doesn't really require us to have any beliefs about these second order matters (as is indicated by the independence of patterns of first order moral acceptance from the discovery that fictionalism is true of that practice). Nor should we expect ordinary users of moral language to have sophisticated beliefs about these second order matters, given that they seem to lie in the domain of linguistics and psychology. In short, hermeneutic fictionalism is not revolutionary fictionalism in disguise, because the revisions it makes are not really revisions to moral practice at all: they are revisions to beliefs about the pragmatics and psychology of moral practice. Furthermore, these are not implausible revisions to make, as we should not expect ordinary moral practitioners to be experts in linguistics and psychology (Kalderon, 2005a, p.154).

As I have remarked, this is a defense of hermeneutic fictionalism against a somewhat different objection to the one we are considering here. Our problem is to defend hermeneutic fictionalism from the criticism that it has failed to locate, in the kinds of evidence offered in its favour, a suitable mental reservation or ontological diffidence marking the fictional character of mathematical utterances. But I believe that what Kalderon says in defense of moral hermeneutic fictionalism will carry over to the mathematical case. Note, to begin with, that Kalderon is, in effect, attributing to ordinary moral practitioners systematic error in their beliefs about their own attitudes and actions. Speakers of moral language *think* they are asserting the sentences they utter, when they are in fact performing some quite different speech act with those sentences. And speakers of moral language *think* that they believe the moral sentences they accept, when in fact their attitude towards those sentences is some quite different attitude. This might initially strike us as implausible. How could we be so radically mistaken about our actions and attitudes? Kalderon attempts to mitigate the prima facie implausibility in two ways (Kalderon, 2005a, pp.154-156).

First, he notes that we can readily imagine situations in which a person is mistaken about her attitudes and mistaken about the character of her actions (Kalderon, 2005a, p.154). Imagine a philosopher who is, unbeknownst to herself, jealous of a successful colleague. She is rude to the colleague in meetings, unjustly critical in department colloquia, she makes sarcastic remarks behind her colleague's back about her professionalism, intellectual attainments, alma mater, personal hygiene, etc. Nonetheless, when called out about her unprofessional conduct by a (different) colleague, she denies that she is jealous at all. Her attitude towards her colleague, she insists, is one of indifferent appraisal of the colleague's myriad personal failings. Her actions are not in fact instances of unjustifiable rudeness, but are in fact just and constructive criticism aimed, not at destroying the colleague's confidence and departmental standing, but at reform of her deplorable character. Such instances of self-deception are, I suspect, somewhat less rare than we would like them to be.

So it is not that rare for our attitudes and actions to fail to be transparent to us. But, further, Kalderon wants to motivate the specific claim that our actions and attitudes in the moral case might not be transparent to us. As he notes, most speakers of moral language are not experts in linguistics, nor are they likely to have thought deeply and reflectively about moral psychology (Kalderon, 2005a, p.142). So it is perhaps not that surprising to discover that they make routine mistakes about these matters. Kalderon also notes that the semantics of moral discourse is apt to lead us astray (Kalderon, 2005a, pp.155-156). Because moral sentences are declarative sentences, and moral semantics is just Tarskian semantics, moral sentences seem to attribute moral properties to worldly inhabitants. But because moral sentences seem to represent facts about the instantiation of moral properties, it is very easy to assume, when we utter those sentences, that is what we *are* doing: just representing facts about the instantiation of moral properties (Kalderon, 2005a, pp.155-156). In other words, the representational semantics of moral discourse blinds us to its non-representational pragmatics.

Now I think that both of these points will carry over to the mathematical case. Suppose we are assessing mathematicians' utterances in order to establish whether they intend these utterances in a make-believe or fictional spirit. That is, we are looking for some kind of mental state on the part of mathematicians that would indicate the presence of an intention *not to mean what they say*. To our dismay, we are unable to find one. Mathematicians, it seems, when we ask them whether they believe in numbers, reply that they *really* do believe in numbers. Or else they return conflicting responses, both individually and as a community, but these conflicting responses seem explicable without attributing to the mathematicians an intention not to mean what they say. Should we conclude that hermeneutic mathematical fictionalism has been defeated?

Well, no. After all, what Burgess has demanded is that the hermeneutic fictionalist locate *a certain kind of attitude* on the part of mathematicians towards their sentences, and to extract this attitude from mathematicians' utterances. But as we have just discovered, our attitudes need not be completely transparent to us. Mathematicians may well give every indication in their explicit statements that they believe in the existence of numbers, and intend their utterances literally. But they might just be mistaken. We cannot expect mathematicians to be experts on mathematical psychology any more than we can expect day to day moralists to be experts on moral psychology. Again, we should not expect mathematicians to be experts in the pragmatics of mathematical discourse, any more than we should expect ordinary speakers of moral language to be experts in the pragmatics of moral discourse. And just as in the moral case, the very semantics of mathematical language, which is just as representational, just as Tarskian as in the moral case, may well conspire to lead us astray. We may just assume that a representational semantics entails an intention to represent, when, in fact, the two can come apart.

It may well be, then, that any intention on the part of mathematicians not to mean what they say is hidden from them, in such a way that it is unlikely to come out in the kinds of explicit evidence for ontological commitment that Yablo and Chihara consider. This might suggest that simply polling mathematicians on their ontological beliefs is not the way to proceed in the assessment of hermeneutic fictionalism, and to a certain extent I would concur. But I think that it also suggests something much stronger. I think that what the hiddenness of mathematicians' attitudes suggests is that looking for an intention to make-believe or to speak non-literally is just the wrong way to proceed entirely. That is, I believe that what Kalderon shows is that hermeneutic fictionalism may well be the best account of a practice even if it conflicts with the psychological states of the participants in the discourse. In other words, the participants in the discourse may well have no intention to speak non-literally, they may *intend* to believe what they say, they may *intend* assertion and not quasi-assertion when they utter sentences of the discourse. Nonetheless, when we come to interpret what it is that participants in the discourse are up to, we may discover that what makes best sense of their behaviour, both linguistic and non-linguistic, is that they do not really believe and do not really assert. To cut a long story short, even an *intention* to believe doesn't guarantee that one is not in fact pretending to believe: between the intention and the act falls the shadow of attitudinal opacity.

To put all this another way, I believe that we should follow Gabor Forrai (Forrai, 2010) in recognizing that hermeneutic mathematical fictionalism is part of an interpretative exercise, an attempt to make the best possible sense of all aspects of mathematical practice. And as Forrai points out (Forrai, 2010, p.200), sometimes, in making sense of what participants in a discourse are up to, we need to disregard what the participants themselves think they are up to. We may even need to ignore (or at least confer less weight upon) evidence about what they *intend* to be up to.

Consider, for instance, an anthropological interpretation of some religious practice. The practitioners themselves may believe that they are in communication with the spirits of their ancestors. They almost certainly *intend* to be in communication with the spirits of their ancestors. The anthropologist, though, is likely to be sceptical of the existence of spirits and otherworldly communications, and when offering an explanation that makes best sense of the community's religious practice it is unlikely that the anthropologist will take the practitioners' beliefs and intentions at face value. Instead, the explanation is likely to appeal to purely naturalistic features of the community's situation, naturalistic features that the community may well be unaware of, naturalistic features that they may not intend to play a role in their practice at all. Nonetheless, we would hardly take this psychological unfaithfulness to be a deficiency in the anthropologist's explanation, if in other respects she had provided the best interpretation of the practice, that is, the explanation that makes best sense of what the community is up to.

I believe that the case is the same for the hermeneutic mathematical fictionalist's interpretation of mathematical practice. Given the opacity of our attitudes and given that we are sometimes mistaken about the kind of speech acts that we are performing, these can only form *part* of the evidence for an interpretation of mathematical practice. Burgess is simply placing too stringent a limitation on the kinds of evidence that are available to the hermeneutic fictionalist in defense of her view. Instead, we should consider the *whole* practice, both the explicit avowals of mathematicians of their own ontological beliefs (if any), and the kinds of utterances made by mathematicians in the prosecution of their day to day mathematical business (and any other evidence that might be relevant). And if we find that, over all, hermeneutic fictionalism makes the best possible sense of this practice, then it will not do to object that mathematicians do not intend to speak fictionally or to make-believe, any more than it would do to object to the anthropologist that because the members of a community do not intend to enhance social cohesiveness or reinforce social status, that they must really be in communication with the mighty dead.

3.4.3 The Way Forward

So what should we conclude about the debate between revolutionary and hermeneutic fictionalism? Is mathematical discourse already a pretense or should we revise our attitudes towards mathematical sentences so as to treat them in a make-believe spirit? Unfortunately, I believe the outcome of our previous discussion has been to call into question the availability of a quick answer to this question. That is, we cannot appeal to the *prima facie* implausibility of hermeneutic fictionalism in order to discount it, precisely because such *prima facie* implausibility is poor evidence of its viability as an interpretative theory. So we seem to be faced with viable defenses of *both* hermeneutic and revolutionary fictionalism.

Indeed, it might even seem that we are forced in the direction of scepticism about the very possibility of deciding between revolutionary and hermeneutic fictionalism. To see this, note an apparent symmetry between the kind of defense of revolutionary fictionalism developed earlier in this chapter, and the kind of defense of hermeneutic fictionalism offered by Kalderon. In both cases, what is appealed to is the independence of mathematical practice from certain kinds of seemingly philosophical considerations. In the case of revolutionary fictionalism, we saw that facts about mathematical understanding make it unlikely that mathematicians will need to have any kind of attitude at all towards the abstract objects their sentences apparently quantify over. And because understanding of ontological matters is unnecessary for mathematical practice to go smoothly, we should not expect revisions to our ontological beliefs to have any effect on first order mathematical practice. Analogously, Kalderon argues that because moral practice is independent of the kinds of sophisticated second order beliefs about moral psychology and pragmatics hermeneutic fictionalism needs to revise, his view is not in fact revisionary in any important way.

Now this might suggest that if the preceding defense of hermeneutic mathematical fictionalism works – that is, if we should expect mathematicians' attitudes to be opaque to them in a way that undermines Burgess' suggestion that evidence about their intentions is the only admissible evidence for hermeneutic fictionalism – then the analogous defense of revolutionary fictionalism will work. In other words, if mathematical practice is independent of one's beliefs about one's own attitudes in the way the hermeneutic fictionalist requires, then it is plausible to think that mathematical practice will be independent of mathematicians' ontological beliefs in the way the revolutionary fictionalist requires, and, of course, vice versa. And that would leave us unable to distinguish between revolutionary and hermeneutic fictionalism.

In the next section, I want to turn to a way in which we can resist this sceptical result. I will begin by introducing the so called autism objection to hermeneutic fictionalism, which suggests that empirical results about the mathematical capabilities of autistic children are incompatible with hermeneutic fictionalism's interpretative claims. As we will see, drawing on work by David Liggins, this argument is not entirely successful (Liggins, 2010). Nonetheless, I believe that the very vulnerability of hermeneutic fictionalism to this kind of empirical evidence gives us good reason to favour the revolutionary variety. In short, revolutionary fictionalism is, ironically enough, the more conservative option: it leaves fewer hostages to scientific fortune. And as philosophers do not control the winds of empirical research, they would be well advised to endorse revolutionary and not hermeneutic fictionalism. In the final section of the chapter, we will apply this revolutionary fictionalism to solve our outstanding concerns with the mapping account.

3.4.4 Jason Stanley's Autism Objection

The previous section left us at an apparent impasse. We saw there how some considerations from Kalderon's *Moral Fictionalism* could be used to resist the objection that hermeneutic mathematical fictionalism gives a hopelessly implausible account of the behaviour of mathematicians. We saw further a symmetry between the kind of response Kalderon develops, and our defence of revolutionary fictionalism, which led to an apparently sceptical outcome. In this section I want to suggest a way in which we might break that impasse.

In his 2001 paper *Hermeneutic Fictionalism*, Jason Stanley puts forward an argument that he believes provides empirical refutation of the hermeneutic fictionalist's core interpretative claim. According to the hermeneutic fictionalist, when uttering mathematical sentences, we do not really believe the propositions our sentences express, and we are not really asserting the propositions our sentences say. As we saw in the introduction to this chapter, one way of fleshing out this proposal is to suggest that our attitude to mathematical propositions is the kind of attitude we adopt to explicitly fictional propositions, that is, it is some kind of make-believe or pretence. But, Stanley argues (Stanley, 2001, pp.47-49), people with autism are not able to engage in pretence/make-believe, and so, if the hermeneutic fictionalist is correct, we should expect their arithmetical ability to be similarly impaired. But, people with autism display no such arithmetical impairment. Therefore, hermeneutic fictionalism is false.[10]

[10] It is worth noting that, as formulated, this argument applies only to pretence based varieties of hermeneutic fictionalism. As our focus in this chapter is on pretence theories, this need not concern us.

Following Liggins (2010, p.769) we can formalize this argument in the following manner. Where M is some mathematical discourse,

Suppose M is in fact fictional, then:

1. Engagement with M involves pretence/make-believe.

2. People with autism are capable of engaging in M.

3. People who lack the capacity to pretend/make-believe are incapable of engaging in M.

4. People with autism lack the capacity to pretend.

From 1, 3, 4:

5. Therefore people with autism are incapable of engaging with M.

But 5 contradicts 2, hence our supposition that M is in fact fictional must be abandoned: hermeneutic fictionalism is false.

3.4.5 Liggins' Response to Stanley's Autism Objection

If our interest is in breaking the deadlock between hermeneutic and revolutionary fictionalism, then Stanley's argument seems like exactly what we were hoping for. If successful, then we have convincing empirical reasons for thinking that the hermeneutic interpretative strategy is unlikely to work. And that means that if we still wish to defend a fictionalist account of applied mathematics, then revolutionary fictionalism is our only option. We have broken the impasse, and can move on.

Unfortunately, however, things are not quite so simple. After presenting the previous formalization of Stanley's argument, Liggins goes on to consider four ways in which the hermeneutic fictionalist might respond to the autism objection. We will begin by briefly setting out the four possible lines of response Liggins considers, before going on to present his more detailed comments on each objection:

> "(a) Reject (4): refuse to assert that people with autism are incapable of pretending; argue that the evidence that is supposed to show this does not succeed in showing it." (Liggins, 2010, p.769)
>
> "(b) Reject (2): refuse to admit that people with autism are capable of engaging in the discourse in question." (Liggins, 2010, p.770)
>
> "(c) Withdraw (1): replace (1) with the claim that discourse D involves something similar to, but distinct from, pretence." (ibid.)
>
> "(d) Restrict (1): rather than claiming that everyone who engages in D is pretending, claim instead that most of the people who engage in D are pretending, or that people who engage in D are pretending unless they have autism." (ibid.)

In short, Liggins sees the possible lines of response as the denial of 4, or 2, a reworking of 1, or the restriction of 1 to non-autistic people. He does not consider a line of response that denies 3, and in this we will follow him. Even if it is possible to defend the view that M involves pretence/make-believe, but doesn't require it, so that people incapable of pretence/make-believe could still engage with M, my suspicion is that this would not alter the general line of argument I wish to pursue in this section. We can, then, safely disregard this option, trusting that what I will go on to say will apply, mutatis mutandis, should this be a live option for the hermeneutic fictionalist.

Liggins begins his more detailed examination of these responses with (a), the denial of premise 4 (Liggins, 2010, p.770). According to this defence of hermeneutic fictionalism, it is simply incorrect to say that people with autism cannot pretend. The evidence presented for this claim is insufficient to establish the conclusion that autistic people lack a capacity for pretence, or else the evidence has been misinterpreted.

If this is meant as a straight denial of the scientific evidence, then it is clearly an unattractive response. If the evidence from psychological studies is that autistic people lack a capacity, then it is surely overstepping our professional boundaries to contradict this evidence merely because it sits uncomfortably with a chosen philosophical theory. But, of course, this is not the only way to read the claim. Instead, we might argue that Stanley has drawn an over-strong philosophical conclusion from the actual scientific evidence. The evidence *as it is* does not support the interpretation Stanley places upon it.

Drawing on work presented in (Jarrold, 2003) Liggins goes on to do just this. He concedes to Stanley that psychological studies have shown that autistic children "exhibit a striking lack of spontaneous make-believe play" (Liggins, 2010, p.770), but he notes that this is consistent with autistic children possessing a capacity that they simply do not *spontaneously* exercise. It is going beyond the evidence to conclude that autistic children lack this capacity altogether.

Indeed, Liggins goes on to present a number of results from Jarrold's review paper that suggest that, at the very least, the question of whether autistic children can engage in acts of make-believe remains open. As Liggins reports, "Some children with autism do spontaneously behave in ways that at least resemble make-believe play." (Liggins, 2010, p.770) Furthermore, some studies seem to suggest that children with autism, when prompted by an adult to engage in activities involving pretence, perform these activities at least as well as non-autistic children (Liggins, 2010, pp.770-771). He notes that some researchers have suggested that in these cases autistic children might not be really pretending, but were merely guessing how they were meant to act. But at the very least, the question seems an open one whether autistic children in these kinds of studies are pretending or not. And if that is correct, premise 4 of Stanley's argument seems questionable; we are well within our rights to deny that people with autism cannot pretend/make-believe, at least until more decisive evidence has been presented.

We can be fairly brief with discussion of (b). Taking this line against the autism objection in the specifically mathematical case of interest to us, would involve the claim that people with autism cannot in fact engage in mathematical discourse. But this is clearly implausible: Stanley is surely correct in his claim that the mathematical abilities of people with autism are not impaired by their condition in the way that this response would require. Indeed, Liggins suggests that this response is likely to be implausible for most of the discourses of interest to the hermeneutic fictionalist (Liggins 2010, p.772). He identifies only one case where this response might be an attractive reply to the autism objection, Egan's account of idiomatic expressions. As our interests lie elsewhere, there is no need to discuss this response further.

Response (c) involves retracting 1, and replacing it with a new claim 1*. According to 1*:

> 1*. Engagement with M involves something like/analogous to pretence/make-believe.

We then go on to claim that this new mental state and speech act that is *like*/*analogous to* pretence or make-believe is one that autistic people can be in or engage in. As Liggins puts it, we "water-down" (Liggins, 2010, p.773) the original fictionalist claim that mathematics involves pretence/make-believe, conceding to the anti-fictionalist that autistic people cannot engage with discourses involving this, but claim that the discourse nonetheless involves something similar to make-believe/pretence that autistic people can engage in.

To this, Stanley objects that watering down premise 1 correspondingly waters down the motivation for fictionalism (Stanley, 2001, p.50). If our reason for being a fictionalist was the existence of apparent analogies between mathematics and more explicitly fictional discourse, then this motivation is weakened if it turns out that the psychological mechanism at work is not the same in each case (Stanley, 2001, p.50). In response, we might object that so long as the psychological mechanism at work in each case is *sufficiently* similar, then we still have an adequate explanation of the analogies between mathematical and explicitly fictional discourse.

Liggins responds differently (Liggins, 2010, p.773), noting that the range of evidence for a fictionalist account of some discourse could well go beyond apparent analogies between that discourse and explicit fictions. In the mathematical case, we might cite our pre-theoretical intuition that the existence of mathematical entities makes no difference to the world, or the Balaguer-Field epistemological challenge, or the advantages of a simplified ontology (though, as we will see later, it is unclear that these kinds of evidence would distinguish revolutionary from hermeneutic fictionalism). Stanley just fails to appreciate the range of evidence the fictionalist can adduce (Liggins, 2010, p.773).

Liggins then goes on to raise a worry for this approach: that it requires finding a mental state sufficiently similar to, but distinct from make-believe/pretence, that autistic people are capable of engaging in, even though they cannot (ex hypothesi) engage in make-believe/pretence. But, Liggins argues, the chances of finding such a mental state are slim (liggins 2010, p.773). How could the state in question be similar enough to pretence to serve the fictionalist's needs, but also distinct enough from pretence that autistic people could engage with it (assuming, for the sake of argument, that they cannot engage in pretence)? If the fictionalist cannot find such a mental state, then their position seems empirically under-motivated.

Response (d) involves restricting 1 to the case of people without autism. In that case we would have 1':

> 1'. Engagement with M involves pretence/make-believe in the case of non-autistic people. In the case of autistic people, it involves belief.

Most people are pretending when they engage with mathematics, but autistic people are not. They really believe the propositions their mathematical sentences express, and their utterances of those sentences are assertions.

Stanley objects to this line of reasoning in two ways (Stanley, 2001, pp.49-50), but as his responses are so similar (both depend on the fact that, as he sees it, the fictionalist who takes this line will have to claim that autistic and non-autistic users of mathematical discourse will be behaviourally indistinguishable) I will limit attention to his first response. According to Stanley,

> Someone who cannot engage in the make-believe we engage in when discussing arithmetic will nevertheless be able to add, subtract, and multiply. Though such a person will be operating under the misapprehension that she is adding and subtracting things that really exist, she will nevertheless be behaviourally no different from those of us who do engage in the make-believe. (Stanley, 2001, p.49)

If that is the case, there will be nothing to distinguish between the autistic users of mathematical discourse, and the non-autistic users of mathematical discourse, at the level of behaviour. And if there is nothing to choose between them at the level of behaviour, then we should say that either all are pretending or none are. And as the default assumption when someone behaves as if they are not pretending is that they are not pretending (Stanley, 2001, p.50), we should say that none of the participants in mathematical discourse is pretending.[11]

Liggins responds to this objection in two ways. We will begin with his second response, as it is less useful for our purposes. According to Liggins (2010, p.774), hermeneutic fictionalists are not limited solely to behavioural evidence for their theories. So even if it should turn out that there are no behavioural differences between autistic and non-autistic users of mathematical discourse, that needn't undermine the motivation for fictionalism. We might instead motivate mathematical fictionalism by appeal to its ontological simplicity, epistemological considerations, etc.

But it seems to me that this kind of response will not help us decide between hermeneutic and revolutionary fictionalism (our current focus), because both kinds of fictionalist seem capable of appealing to the same sorts of evidence to motivate their fictionalism. In other words, if this response entails that there is nothing to choose between realist and non-realist construals of the discourse at the level of behaviour, nothing at all that would separate them, then we have no means of choosing between hermeneutic and revolutionary fictionalism.

Liggins' second objection is that the hermeneutic fictionalist is under no obligation to say that there is no behavioural difference between autistic and non-autistic users of mathematical discourse (Liggins 2010, p.774). As he goes on to note in the particular case of Yablo's arithmetical fictionalism (Liggins 2010, p.776), it is open to Yablo to respond to Stanley by claiming that, in fact, there are behavioural differences between autistic and non-autistic participants in arithmetic discourse. The fact that autistic people believe the arithmetical sentences they utter, and the rest of

[11] Stanley's second objection rests on the contention that the kind of speech act performed by a person has consequences for their subsequent behaviour. So if autistic people are really asserting their mathematical sentences, then this should have some detectable behavioural consequences. But the fictionalist must claim there are no such behavioural consequences. Ergo, hermeneutic fictionalism is false (Liggins, 2010, p775).

us do not predicts that, under certain circumstances, autistic people will behave differently to non-autistic people.

For example, Liggins takes the case of Yablo's philosophical oracle (Liggins, 2010, p.778), who is omniscient, and who has just informed us that there are no natural numbers. Upon learning about the non-existence of the apparent referents of our arithmetical discourse, what do we do? Yablo imagines that most of us would be unperturbed (Yablo, 2005, p.88). We would go on with our ordinary counting practices, go on adding and subtracting, unconcerned by the philosophical bombshell the oracle has landed us with. But perhaps the intuitions of autistic people differ here (Liggins, 2010, p.779). Perhaps if we were to ask them about their intuitions in this thought experiment, they would tell us that they would feel forced to abandon all their ordinary arithmetical practices, and give up mathematical discourse for good. In that case, we would have a behavioural difference that we could detect, and that would argue in favour of hermeneutic mathematical fictionalism.

Indeed, Liggins goes on to suggest that so far from being a problem for the hermeneutic fictionalist, this is in fact an advantage of her view. If the hermeneutic fictionalist takes this line, then she is in a position to claim that there are empirically detectable traces of the non-literalness of our ordinary mathematical discourse in the unusual behaviour of autistic people (Liggins, 2010, p.779). It is open to the hermeneutic fictionalist to claim that, because autistic participants in the discourse actually believe the arithmetical propositions they assert, they behave in detectably different ways from ordinary participants in the discourse. And if this *were* the case, then it would be evidence that the rest of us do not believe the propositions we assert. As Liggins puts it "Far from refuting Yablo's position, therefore, Stanley's argument suggests where one might look for evidence to support it." (Liggins, 2010, p.779)

3.4.6 How to Break the Impasse

What can we conclude from the arguments presented above? Well on the face of it, it seems we are right back where we started. If Liggins is correct then it seems like there are a number of attractive options for the hermeneutic mathematical fictionalist to consider in replying to the autism objection. Any one of (a), (c), or (d) seems to be a live option, with (a) and (d) seeming particularly attractive (Liggins, 2010, p.782). That is, the hermeneutic mathematical fictionalist seems perfectly entitled to deny premise 4, and claim that autistic people can engage in pretence. She seems equally entitled to restrict premise 1 to 1', and claim that autistic people actually believe the mathematical sentences they utter, while the rest of us don't. And if she takes (d) as her response to Stanley's objection, she gets the added bonus of suggesting possible empirical corroboration of her view.

On the surface, Stanley's autism objection seemed like exactly what we needed to break the deadlock between hermeneutic and revolutionary fictionalism. It was exactly the kind of empirical dis-confirmation of hermeneutic mathematical fictionalism that would have left us sitting pretty, with revolutionary fictionalism our only viable option. Is there, then, given the failure of the autism objection, any way forward?

Well, it seems unlikely that we will be able to rely on any argument that straightforwardly empirically dis-confirms hermeneutic fictionalism, for the simple reason that the evidence is not yet in. But perhaps we can find a reason to favour revolutionary fictionalism in this very openness of the hermeneutic variety to empirical dis-confirmation. In short, perhaps the fact that hermeneutic fictionalism leaves so many hostages to fortune is itself a reason to favour its more tentative revolutionary cousin.

Let us begin elaborating this line of thought by looking again at response (a). We saw in Liggins' discussion of (a) that the issue of whether autistic children are capable of pretence is an open question among psychological researchers, and he concluded from this that the hermeneutic fictionalist is perfectly entitled to deny premise 4. But as he goes on to note, this leaves significant hostages to scientific fortune (Liggins, 2010, p.771). If the winds of psychological research change, then it could turn out that the only viable interpretation of the data is that autistic children cannot engage in pretence, and the autism objection would seem to be a compelling response to hermeneutic fictionalism once more. As Liggins notes: "psychology has not refuted (4); indeed, it is not unlikely that it will eventually confirm (4)." (Liggins, 2010, p.771) And if psychology were to confirm premise 4, then hermeneutic mathematical fictionalism would once more be in trouble.

Alternatively, it leaves Stanley or some other anti-fictionalist philosopher perfectly free to return to the psychological literature and find some other psychological condition that really *does* prohibit make-believe or pretence, but which does not impair mathematical ability (Liggins, 2010, p.771). In that case the objection could be run again, with this new condition in place of autism. Once again, response (a) to the autism objection would be blocked.

But revolutionary fictionalism leaves neither of these kinds of routes open, because as a claim about the correct philosophical approach to mathematical discourse, it is not open to empirical dis-confirmation in the way that hermeneutic fictionalism is. Because hermeneutic fictionalism makes interpretative claims about actual real world language users, it is open to empirical challenge. Ironically, in this respect at least, revolutionary fictionalism is in fact *more conservative* than the hermeneutic variety: revolutionary fictionalism can stand whichever way the empirical winds are blowing.

Or consider response (d). According to Liggins, far from being a problem for the hermeneutic fictionalist, the potential existence of behavioural differences between autistic and non-autistic participants in mathematical discourse gives us a route to the empirical confirmation of hermeneutic fictionalism (Liggins, 2010, p.779). But now suppose that we present an autistic person with the Oracle thought experiment, and his reaction is no different from that of any other person. That is, suppose that our hypothetical autistic person possesses the same intuition as the rest of us (assuming that we do all possess this intuition), that upon being told by the Oracle of Philosophy that there are no natural numbers, he would just carry on with all his usual arithmetical practices unfazed by his new found ontological scepticism. In that case, the hermeneutic mathematical fictionalist seems to have made a prediction about the behaviour of autistic participants in arithmetical discourse that is not borne out by the empirical evidence. Autistic people behave just like the rest of us who (ex hypothesi) do not believe that numbers exist. It seems reasonable, then, to conclude that either: (1) neither autistic nor non-autistic people believe in the existence of numbers; or (2) both autistic and non-autistic people do. And given that the default is that when people say they mean what they say, they mean what they say, we should conclude that (2) is the case: hermeneutic fictionalism has been refuted by the evidence.

Once again, revolutionary fictionalism is just not open to this kind of empirical challenge. The revolutionary fictionalist is perfectly happy to concede that the semantics of mathematical discourse is perfectly ordinary Tarskian semantics, and that participants in that discourse assert the sentences they appear to be asserting. Therefore, the revolutionary fictionalist will not be troubled by evidence that suggests that a fictionalist interpretation of mathematical discourse is untenable. The revolutionary fictionalist is not defending an empirical thesis about what mathematical discourse actually means, but instead is defending a philosophical thesis about what philosophers, qua philosophers, should make of that discourse.

So perhaps this gives us one reason to prefer revolutionary over hermeneutic fictionalism. The hermeneutic fictionalist leaves a number of hostages to empirical fortune, precisely because her doctrine is an empirical claim about what mathematical discourse really means or what mathematicians are really up to. But this means that hermeneutic fictionalism is only ever provisional, in permanent danger of being withdrawn should the evidence from psychology or linguistics prove uncongenial. This makes the entire project of defending a fictionalist account of mathematical ontology contingent upon a hospitable empirical environment; should that environment change, the hermeneutic fictionalist will either have to give up on fictionalism or switch to a revolutionary variety anyway. To put it bluntly, revolutionary fictionalism is the safer bet.

Of course, this is not a knock-down argument for favouring revolutionary fictionalism. Those who feel certain that the empirical evidence will go the way the hermeneutic fictionalist needs it to, or who are competent to perform the relevant scientific studies themselves, may well feel themselves capable of mounting a defence of hermeneutic fictionalism. But for the purposes of this thesis, we need only defend the claim that there is a viable fictionalism about mathematics, within which we can embed the mapping account of applied mathematics outlined in the previous chapter. And if that is our only interest, then it seems unnecessary to involve ourselves in empirical complexities that can only render our fictionalism about mathematical ontology contingent upon scientific studies that could go either way. For that reason, in the remainder of this chapter, we will work within a revolutionary variety of fictionalism about mathematics.

In the next and final section of this chapter, we will set to work applying this variety of fictionalism to the mapping account of the previous chapter. Before doing this, though, it will be helpful to remind ourselves what has been achieved so far. In the first section of this chapter we suggested that a pretence theory of mathematical discourse, based on Walton's prop-oriented make-believe, would do the job we needed in deflating the apparent Platonism of the mapping account, though we argued that nothing terribly important hangs on this particular way of doing things. If some other brand of fictionalism works better, we reserved the right to cross the floor, and switch our allegiance to a new variety of fictionalism. We then outlined two defences of revolutionary and hermeneutic fictionalism respectively, and turned to the autism objection to give us a way to decide between them. Unfortunately, even this apparent empirical refutation failed to settle the issue, and we have eventually settled on a revolutionary approach for the simple reason that it leaves less to chance, and insulates our fictionalist project from empirical dis-confirmation. In the remainder of the thesis, then, it is to be understood that this is the variety of fictionalism we are defending: a revolutionary prop-oriented make-believe about applied mathematics.

3.5 Fictionalism and the Mapping Account

In this final section of this chapter, we will deploy the account of fictionalism already developed, in order to solve some outstanding concerns with the mapping account of mathematical-scientific representation. In particular, our focus will be on evading the apparent Platonistic commitments of the mapping account. To that end, we will begin by discussing a variant of the familiar indispensability argument for Platonism, focusing specifically on the role of mathematical posits in our best account of mathematical applications.

Following this, we will then consider two more worries that remain for the mapping account presented in chapter 2, before moving on to present a fictionalist solution to all three problems. The basic idea is that we can treat the mapping account itself as a kind of prop-oriented make-believe, taking the empirical world as its prop, and so evade the apparent Platonic ontological commitments of the original mapping account.

We conclude by discussing the purpose of the pretense, and how it can be useful to pretend that there are structure-preserving mappings between some empirical domain and a suitable mathematical structure.

3.5.1 The Mapping Account and The Indispensability Argument

The account of the applications of mathematics developed in the previous chapter seemed, prima facie, to bring with it a swarm of abstract entities. We saw there that the representational content of a mathematical-scientific representation was secured by the existence of a suitable mapping between the empirical domain being represented and the mathematical structure doing the representing. But this seems to leave us committed to the existence of, at least, structures and mappings. And given that structures are themselves usually regarded as sets of object with relations defined over them, we seem to be landed with ordinary mathematical objects aplenty: objects, relations, mappings.

What I want to do in this short section is develop this intuitive argument into a kind of indispensability argument, an argument from our best theory of mathematical applications in science to the existence of abstract objects. I make no claims of novelty for the argument about to be presented. I suspect that it is just a special case of the original indispensability argument, a case in which, instead of focusing on the presence of mathematics in our best *total* theory of the world, we focus on the specific case of our best theory of mathematics' applications to the world. However that may be, my aim is simply to bring out more clearly the worry expressed in the previous paragraph.

With that in mind, I offer the following as an attempt to render the above worry into a more formal indispensability argument.

1. Our best theory of applications of mathematics within the natural sciences seems to be committed to the existence of mathematical objects.

2. We should believe our best theory of applications of mathematics in the natural sciences to be true.

3. Therefore we should believe in the existence of mathematical objects.

Some comments on the first premise first. When I initially presented the worry that the mapping account would bring with it a host of abstracta, I suggested that we could find ourselves committed to mappings, relations, and as many mathematical objects as found employment in representational structures within the natural sciences. We can now see exactly why the mapping account seems to be committed to such things: in short, it quantifies over them. If we accept that theory then we believe that something like the following schema characterizes mathematical applications within natural science:

(*) Mathematical-scientific theory A represents empirical domain B iff *there exists* a suitable mapping between *some* mathematical structure C (employed within theory A) and the empirical domain B.

And schema (*), as it stands, quantifies over both mappings and structures.

The second premise is where the action is. This tells us that we are ontologically committed to whatever is quantified over by our best theory of applications of mathematics in the natural sciences. But should we believe this? After all, unlike physical theory, philosophical theories about mathematical applications do not on the face of it appear to be part of our best scientific theory of the world. But the second premise of the original indispensability argument got its traction from the plausible naturalist assumption that our best scientific theory is our best guide to what there is. Can the same kind of plausibility be claimed for this variant of the indispensability argument?

I want to suggest that it can. If we are naturalists, then we are likely to see our philosophizing as taking place *within* natural science (Quine, 1980). And if we see our philosophizing as taking place within natural science, then we are likely to think that our best attempts to develop theories about the mechanisms of application are themselves a part of our overall best scientific theory of the world. In short, if we are naturalists and if we are attracted to the mapping account as an account of the representation relation at work in scientific applications of mathematics, then we will probably want to say that the mapping account, appearances notwithstanding, is a part of our best *total* scientific theory. And if we say *that*, then it seems we have all the reasons a naturalist inherits in the case of physics for taking the posits of our theory seriously.

If the above is correct, then the conclusion follows as a matter of course: we have every reason to take the posits of our best theory of applications seriously and, mathematical entities being among those posits, we have every reason to believe in the existence of mathematical entities. And if mathematical entities are, as is generally thought, abstract objects, then we have every reason to believe in the existence of abstract objects. The account of applications developed in the previous chapter, then, is unavailable for the committed nominalist.

3.5.2 Two More Worries

Before returning to the ontological issue, and presenting a solution drawing on the groundwork already laid down in this chapter, I want to develop two related worries facing the mapping account. I introduce these here because I believe that our solution to the ontological problem will also provide us with the resources to resolve these worries also. The first worry we have, in part, already encountered. In the previous chapter we noted that Bueno and Colyvan objected to Pincock's mapping account on the grounds that it presupposed the existence of a suitable structure in the empirical domain (Bueno & Colyvan, 2011, p.347). But this assumption is itself a controversial metaphysical hypothesis: what if the world doesn't come pre-carved at the joints in such a way as to make it a suitable input into our mappings? Even if we are unconvinced by this metaphysical possibility, it seems likely that whatever structure the world does have is identified, at least in part, by the scientific theories we employ in its investigation. And that would require that we apply our mathematical representations in order to secure the necessary structure in order to apply those representations (Bueno & Colyvan, 2011, p.347). Clearly that is not an acceptable result.

Bueno and Colyvan suggested that one way out of the difficulty might be to *assume* the existence of a suitable structure in the empirical domain, a structure that may even be characterized mathematically (Bueno & Colyvan, 2011, p.347). As we saw, this assumption is a defeasible one, required to get our representations off the ground, but in principal revisable if further investigation demands it. And in the last chapter we let this suggestion slide, noting only that the mapping account could be modified to include a similar mechanism. But, in fact, this suggestion introduces a fatal fault into our account of mathematical applications. If we assume the existence of some mathematically characterized structure in the empirical domain, then it seems we must already have some way of applying mathematics. We were supposed to be accounting for the relationship whereby a piece of mathematics comes to be employed in a representation of the empirical world, but it seems that in order for this relationship to be established, we must already have some way of employing mathematics to represent the empirical world. In that case, it would appear that it is not in fact the mappings that do the representing: there must already exist a representational relationship between the empirical world and a mathematical structure. And we haven’t accounted for that at all.

Our second worry also concerns the relationship between the mapping account's maps and the empirical domain. This time we note that mappings are mathematical entities, functions or morphisms, and they hold only between mathematical structures. But the mapping account maps *empirical* structures onto mathematical structures. Ordinary mathematical maps cannot do this. So, at the very least, we are forced to conclude that the maps talked about in the mapping account cannot be ordinary mathematical ones. In that case, what kinds of mappings are they? What kind of entity is it that takes an empirical structure as input and produces a mathematical one as output? As it stands, the mapping account doesn’t answer this question and so it leaves us totally in the dark as to the status of its central representational machinery.

Now one solution to this worry might be to claim that the maps in question are some kind of sui generis entity: a structure preserving relation between an empirical structure and a mathematical structure. They are, in short, not mathematical maps at all, they just behave like them. Let us call these new, sui generis entities *ur-maps*.

We can then amend our account of applications so that it claims that: we have an application of some piece of mathematics within natural science iff there exists a suitable ur-map between some mathematical structure and the empirical domain under investigation.

One downside to the ur-map proposal is that it introduces a new kind of entity into our ontology, a sui generis entity. This is especially problematic when we consider that we were hoping to explain how applications of mathematics function, and we did so by employing a previously understood concept: that of a mapping. We now seem to have retreated to explaining applications in terms of a previously unknown concept: an ur-map. This seems, prima facie, to be a step back for our account of applications.[12]

I do not believe that these worries about the ur-map proposal need be fatal; our solution to the ontological problem should provide us with the resources to resuscitate ur-maps also. But for now, I want to suggest one other possible solution: this is, in short, Bueno and Colyvan's suggestion of assumed empirical structure. In particular, we can take over their idea that our assumed empirical structure can itself be mathematically characterized in order to solve the worry about mappings. If the assumed structure is itself already mathematized, then there is no problem about its entering as input into an ordinary, run of the mill, mathematical map. Our worry was only about non-mathematical, empirical structures: mathematically described structures are fair game for mappings.

But, of course, this solution is no solution at all. As we saw above, this brings with it the fatal fault, the worry that our mappings don't seem to be doing the representational work at all. If, then, it came down to a fair fight between ur-maps and Bueno and Colyvan's assumed structures, it seems that ur-maps would win the day. But as I hope to show, our solution to the ontological problem will level the

[12] One possible way of making sense of this might be to identify ur-maps with certain sets of ur-elements in a set theory with ur-elements (Leng, 2010, pp.176-179). In that case this problem simply collapses back into that of the mapping account's Platonistic ontology.

playing field between these two proposed amendments to the mapping account and, in the end, there will be little to choose between them.

3.5.3 How to Kill Three Birds With One Stone

We have these three worries: One the one hand, the mapping account seems to bring with it a host of ontological commitments we do not share. On the other, Bueno and Colyvan's amended mapping account seems to suffer from a fatal fault that suggests that the mappings are not doing the representing after all. On the third hand (so great are our difficulties, we require three hands to hold them), we seem to be talking about entities that behave in ways similar to ones with which we are antecedently familiar and which are yet not entities with which we are antecedently familiar. How are we to resolve our difficulties?

Well, at least two of our difficulties suggest their own solution. Our first worry was that we were speaking about things we do not believe to exist. But, as we have already seen, one way in which we often speak about things we do not believe to exist is to engage in a pretense. We pretend that the suspect entities exist, even though we know that really they do not. For instance, when reading the Hobbit, we engage in the pretense that there is a dragon called Smaug, though, of course, we are well aware that there is no such dragon (nor any dragons at all). So perhaps we might evade our ontic responsibilities by claiming that, after all, we were only pretending.

What about our second worry? There we noted that, if we follow Bueno and Colyvan, we seem to land ourselves with the result that the mappings are not doing the representational work we thought they were. But as it happens, we can deploy a fictionalist account of the kind countenanced in the last paragraph to resolve this difficulty as well. That is, we can say that instead of providing the representation relation that makes applications possible, mappings and assumed empirical structures are themselves part of the content of a pretense, and it is that pretense that provides us with our representation.

What about our third worry? Will a fictionalized mapping account assist here? Well, the details will, of course, depend on the particular solution we prefer (ur-maps or assumed structures), but I believe that worries about both approaches can be assuaged by a healthy dose of pretense. I will return to this particular issue later when a general fictionalist strategy has been introduced.

With all that in mind, let's try to develop a fictionalist version of the mapping account developed in the previous chapter, drawing on material already presented in this chapter. Here is how that goes: when we apply a piece of mathematics, let us say that we engage in a pretense or make-believe. There is, of course nothing new about this suggestion; fictionalist accounts of applications have been defended before, and we are drawing here on work already discussed early in this chapter. What is novel here is the content of the pretense. In short, let us say that when we apply a piece of mathematics within natural science, we engage in the pretense or make-believe that there is a mathematical structure and a suitable mapping between that structure and the empirical domain. This certainly seems to help with our first worry. We were concerned that by endorsing a mapping account we would land ourselves with anti-nominalist commitments to all sorts of undesirable entities. But now we note that all the existential commitments that had been giving us grief have ended up within the scope of a pretense. And surely we need not be committed to the existence of anything we merely *pretend* to quantify over!

Let us try to be a bit more precise about this. Following Leng (2010, pp.177-179), we can say that when we apply mathematics to the empirical world we engage in a pretense according to which the non-mathematical objects in the world can be collected together to form sets. That is, we conduct our applied mathematics against the background make-believe game characterised by the axioms of set-theory with ur-elements. These axioms will serve as our principles of generation.

According to the principles of generation of this game (the axioms of set-theory with ur-elements), there is a set of all the non-mathematical things, and (according the subset axiom), for any predicate, there will be a subset of the set of all ur-elements such that all its members satisfy that predicate (Leng, 2010, p.177). In particular, we note that for any property exemplified by non-mathematical things (provided we can find a predicate for it), there will be a subset of the set of all ur-elements with just those things as members. As we will see, this gives us our assumed structure at the base of mathematical-scientific representation.

Once we have our sets of worldly props, we can go on to form further sets of these, and sets of sets of these. But once we start doing this it becomes interesting to ask which sets it is appropriate to make-believe in given the nature of the worldly props. That is, as Leng notes (2010, pp.177-178), the nature of the worldly props, the non-mathematical ur-elements, will constrain our further set formation, and make it appropriate to make-believe that there are some sets of ur-elements, and inappropriate to make-believe that there are others. In particular, I want to suggest that the nature of the worldly props will make it the case that in cases of mathematical application, we will be required to establish the existence of a structure preserving map, a morphism, between some mathematical structure (or to be precise, some set-theoretic surrogate for this structure) and the original set of ur-elements with which we started.

Let us see how this works in a simple case. We start by noticing that, according to the background make-believe of set-theory with ur-elements, my fingers are part of the set of all ur-elements. So, via the subset axiom, I can form the set L of all those fingers that satisfy the property predicate "...is on my left hand." In other words, we can make-believe there is a set of fingers on my left hand. Now suppose I wanted to count my fingers, how would I go about doing so? According to the theory of application presented here, you would begin by selecting an appropriate structure – in this case the set N of natural numbers would probably do. You then establish the existence of a structure preserving map between the set of fingers on my left hand, and the set of natural numbers, that is, you prove the existence of a certain kind of function with domain consisting of the objects in the set L, and range consisting of some subset of N (most usually the ordered set beginning with one and ending with five). In more complicated cases, and especially if we are convinced of the need to allow a role for derivation in our account of mathematical applications, we will also need to establish the existence of a similar (but not necessarily identical) function taking us back from N to L.

Notice that this is all taking place within the pretense according to which there are sets of ur-elements, so this is all taking place within a make-believe game, the game of applied mathematics. And because it is all taking place within a make-believe game, there is no need for us to be ontologically committed to the things we quantify over while engaging in the pretense. That is, we are not committed to the existence of mathematical structures, we are not committed to the existence of structure preserving maps, and we are not even committed to the real world existence of sets of ur-elements. It is perfectly plausible to claim that the only things we need be committed to are the ur-elements themselves, that is, the worldly props that constrain our mathematical-scientific theorizing.

We have seen how to solve our ontological concerns, but what about our second and third worries? These, remember, were related issues and our solution to the second worry will be identical to our solution of the third if we endorse the "assumed structures" approach. Let us see how that works. Suppose that we want to claim that we need not worry about the existence of empirical structure or its discovery independent of our scientific theories, because we can simply assume the existence of some antecedently identified empirical structure. Suppose, even, that we go further and claim that this antecedently identified empirical structure is described mathematically. One way we might make sense of this is by claiming that what we are in fact doing is *engaging in the pretense* that the world is so structured. That is, when we apply some piece of mathematics, we engage in the pretense that there is a set of ur-elements made up of just those worldly things satisfying some structural predicate, in other words, we suppose there is a set of worldly, non-mathematical objects, and a relation on these, that can serve as the base of our mathematical-scientific application.

How does this help us? Well, our problem was that the mathematically characterized empirical structure seemed to employ some representation relation between mathematics and the empirical domain that remained unaccounted for. But that was exactly the kind of thing our theory was supposed to be accounting for! But once our fictionalized account is in place, we can note that our assumed structure is only part of the content of a pretense and it is the pretense that does the representing. And we already have antecedently worked out theories of how pretend representations work. In short, then: We solve the worry that the world might not be suitably structured by simply pretending that it is; we solve the worry that our account was circular by acknowledging that it is not the mappings that do the representational leg-work after all. Instead they form the content of a pretense that really accounts for the representational role of mathematics.

And this same kind of solution will work if we adopted the assumed structures approach because we needed mathematically characterized empirical structures as inputs to our mathematical maps. We will still be able to say that we are engaged in the *pretense* that such mathematically characterized structures are available and everything that was said in the previous paragraph will carry over nicely.

Now if we adopted the ur-map solution of our third worry, then it seems that we will not require any fictionalizing. And in so far as we are only interested in the problem of how the empirical world can be an input to a mathematical map, this certainly seems to be the case. Remember that we earlier identified one concern for that approach, namely that it seemed to introduce a new kind of sui generis entity into our ontology. Now one way around this, without employing fictions, is to identify ur-maps with some already well-known entity: perhaps ur-maps are themselves just sets in a set theory with ur-elements, for instance. That is why I remarked when I introduced the ur-map approach that the worry about their sui generis status was unlikely to be fatal. But, as nominalists, we are presumably worried not only by the problem of empirical inputs to mathematical maps, but by the very existence of mathematical maps in the first place. And we may well remain worried by Bueno and Colyvan's concerns about the existence of empirical structure at all or its identification prior to our applications of mathematics. And as we saw above, one way of dealing with these challenges is to go fictionalist. But once we have prior reasons for adopting a fictionalized version of the mapping account, we can see our way to a quite different resolution of the worry about the ontological status of ur-mappings. In short, they do not have one. When we apply mathematics, we merely engage in the pretense that there are suitable ur-mappings between some empirical domain and a mathematical structure, or to be more precise, we simply identify these ur-maps with some suitable function in a set-theory with ur-elements. We are not committed to a sui generis kind of entity at all, nor are we committed to sets in a set theory with ur-elements; we merely pretend to be so committed, and pretend commitment is ontologically innocuous.

Let us bring all this together. Let us say that when we apply some mathematics, we engage in a pretense such that there is a mathematical structure, and there is a suitable map (or ur-map), and the empirical domain is suitably structured so as to make it appropriate to map (or ur-map) the empirical domain onto the mathematical structure. That is, let us say that we make-believe that set-theory with ur-elements is true, that some of these ur-elements will be the non-mathematical objects of our scientific theories, and that the nature of the non-mathematical objects constrains our pretense in line with the mapping account developed in the previous chapter. As far as I can see, this solves our three worries. We are not committed to any unwelcome entities, because all quantification occurs within the pretense. We need not assume any prior mathematically-characterized empirical structure exists, we merely pretend that it does. And we can either pretend in the existence of ur-maps or else pretend in the prior mathematical characterization of the empirical domain, and so solve our worry that mathematical maps do not take empirical inputs.

3.5.4 The Point of the Pretence

If the above arguments are correct, then we have a neat solution to our three main concerns about the mapping account. Perhaps most importantly, we have shown how to incorporate one plausible, but seemingly platonistic account of mathematical applications within a nominalist framework. But before we can rest on our laurels, we must say something about why we engage in all this pretense. In order for our account to be successful, it cannot be the case that viewing applied mathematics as pretense makes it a mystery how our mathematical-scientific theories could have been successful.

Now I do not want to say a great deal about this; others have discussed the role of fiction in the natural sciences and there are a number of defenses in the literature of the idea that literally false or fictional theories can nonetheless be useful in applications (Balaguer, 1996b; Field, 1980; Leng, 2010; van Fraassen, 1980). But here is one thing we might say: the pretense we engage in when we apply mathematics (the pretense that there are suitable mappings and so forth) will be useful so long as the world holds up its end of the representational bargain, that is, so long as the world is such (however that may be) as to make it pretense worthy that it is so structured as to be an appropriate input to a mapping onto some mathematical structure. In other words, we note that the world itself will constrain our pretense making.

One way in which this constraining of our pretense is manifested is in experiment. Perhaps the need to provide explanations also places limits on what we can pretend. It could be that our pretenses will only be appropriate if they (indirectly) allow us to express the nominalistic facts. However that may be, we note only that our pretense will be a species of what, following Walton (1993), we have called "prop-oriented make-believe" with the world as a prop. And the world as prop will make certain pretenses admissible and rule others out. Experiment and further investigation will (so long as the world cooperates) enable us to sort the wheat from the chaff; so we see that even if we regard applied mathematics as mostly pretense, there is ample room for the exercise of the scientific method and ample opportunity for scientific success.

3.6 Concluding Remarks

In these final remarks I want to recapitulate what has been achieved over the course of this somewhat lengthy chapter. To begin with, we have adopted, following Leng (2010) and Yablo (2001; 2005), the idea that we can use Walton's (1993) account of prop-oriented make-believe to support a fictionalist theory of mathematics generally, and applied mathematics in particular. We then moved on to a lengthy discussion of revolutionary and hermeneutic fictionalism, concluding that it was safest (though by no means obligatory) for the fictionalist to adopt the former. In this final section we developed the idea that treating the mapping account of chapter 2 as a prop-oriented make-believe, taking the world as a prop enabled us, among other things, to evade its apparent, and unwelcome ontological commitment to abstract mathematical objects.

In the final two chapters of this thesis, prior to the conclusion, our focus broadens somewhat, as we move away from a narrow focus on issues of mathematical representation and explanation, into the realm of metaphilosophy. In the next chapter we will discuss how the fictionalist can defend her position against a certain kind of

Pragmatist objection that sees all metaphysical dispute as traceable to confusions about how language works. In the chapter after that, we discuss some recent ideas from metaontology that seem especially threatening to the fictionalist about abstracta. In the conclusion I draw these various strands together in order to argue that fictionalism about mathematics offers both a coherent and naturalistically satisfying account of applications of mathematics within the natural sciences and a viable response to the placement problem with which we began.

Chapter 4. Fictionalism and Meta-philosophy, Part 1: Anti-Representationalism

4.1 Introduction

So far we have been considering the consequences one theory of applications of mathematics in scientific representation has for the ontology of mathematics. What we have discovered is that a revolutionary, prop-oriented pretence theory, a kind of mathematical fictionalism, offers one way to defuse the apparently inflationary ontology of the mapping account of mathematical applications. But at this point we may begin to feel disquiet at the ease with which Platonist ontology has been dismissed. Perhaps, after all, there is less to this ontology business than we first supposed.

There are, I suspect, two ways in which this initial disquiet might be developed into a full blown assault on the foundations of ontology. On the one hand, we may suspect that the ease with which the local (mathematical) fictionalist deflates inflationary metaphysics holds out the promise of using a global fictionalism to deflate metaphysics entirely. In that case, our successes at the local level will prove entirely Pyrrhic: we will have defused the Platonist menace, only at the cost undermining nominalism in turn.

The strategy just considered invokes a global fictionalism in the defense of a deflationary metaontology. In the next chapter I will consider some attempts to substantiate a kind of *metaontological* anti-realism (distinct from the first order ontological anti-realism, of which genus local fictionalism is a species). But in this chapter I want to consider a different way in which our disquietude might be employed in the services of metaontological anti-realism: perhaps we drew negative conclusions from our investigation of scientific representation *simply because representation doesn't work the way we thought it did*. Perhaps, after all, there are no conclusions to be drawn about ontology from the theory of representation.

In the following chapter, I will set out one recent attempt to develop this pragmatist line of thought about representation, that of Huw Price's Naturalism Without Mirrors (Price, 2011). As we will see, this kind of deflationary account of semantic relations poses a challenge to the naturalist ontologist, because it suggests that she has been working all along with an unnatural account of representation. I will offer some criticisms of Price's project (principally based around the claim that it is not as naturalistic as Price suggests), before moving on to more positive attempts to defend the ontologist's activities. I will initially suggest that there is scope for a kind of pragmatism that would nonetheless be resistant to Price's metaphysical pluralism (indeed, I suspect that something like this is the kind of thing Quine meant by naturalism (cf. Quine, 1951)). I will then go on to argue that however the debate over the correct pragmatist account of ontology turns out, Price himself may need some kind of word-world relations, of the kind the representationalist employs, in order to explain some of the features of his own account of linguistic practice. I will conclude by suggesting that whether or not my positive suggestions work out, Price has given us very little reason to favour his account of representation (and so, his metaphysical pluralism) over that of the naturalist ontologist.

4.2 Naturalism and the Placement Problems

Suppose we have committed ourselves to some broad philosophical program, empiricism say, or physicalism. Then there will be things about which we wish to talk, but which we may have trouble squeezing in to the procrustean bed of permissible ontology. For instance, an empiricist, whose ontology might be so austere as to allow only momentary sense data and constructions out of these, will have to find a place for ordinary middle-sized dry goods, theoretical entities, morals, mathematicalia and so forth. Obviously, this is not an easy task. A physicalist may have an easier time of it with middle-sized dry goods and theoretical entities, but may still struggle to find a place for morals and mathematics and minds and the modal. These difficult cases are what Price calls 'placement problems' (Price, 2011, pp.184-189): our philosophical program allows only entities of certain kinds, yet our everyday talk is full of apparent references to things of other kinds. We must either find a home for these other kinds, placing them among the kinds of things already allowed, or else do without them.

Naturalism, of course, is just such a broad philosophical program, and like empiricism and physicalism it brings with it a prohibition on the kinds of things we are allowed to talk about: in short, there are no non-natural things (Price, 2011, p.4). Another way of putting this is to say that for the naturalist, there will be no things not countenanced by the natural sciences, no things that violate our picture of ourselves and our talk as part of the natural world and revealed by our best scientific theories. But this commitment brings with it a similar set of placement problems faced by the physicalist (Price, 2011, pp.3-6). We allow only natural properties and natural relations between those properties and ourselves. In particular, there can be no non-natural relations between ourselves and the properties we know about and talk about; the only relations we can countenance are those that would be countenanced by our best science of ourselves as talkers and knowers. So how came we by our ability to talk about moral properties, if these should prove to be irreducible, non-natural properties? How did we learn about modal facts, if these involve knowledge of possibilia causally isolated from us? Just how do minds fit into our picture of the biological and physical world revealed by those sciences? Indeed, we have already encountered one of these placement problems. It has been the main focus of the thesis so far: just where do mathematical objects fit into the naturalist's picture of the world?

4.2.1 Placement Strategies

Price sees four responses to the placement problems in the extant literature (Price, 2011, pp.6-8). In the first place, we might conclude from the difficulty of fitting these important areas of discourse into our naturalistic picture of the world that naturalism is flawed. If naturalism is too austere to allow morals and modals, maths and minds, then naturalism should be abandoned. First philosophy is back, resurrected by our troubles with placement. But of course, such a pigheaded resistance to the tide of scientific success has proven justifiably unpopular. Canute hoped that his futile display before the waves would refute his fawning courtiers and prove his fallibility and piety. The non-naturalist has nothing comparable to gain by setting her face against the Tsunami of scientific accomplishment (Price, 2011, p.132).

Suppose, then, we have refused the retreat to first philosophy, what options are left for the naturalist trying to cope with the placement problems? One option is hard headed eliminativism (Price, 2011, p.7). The concepts we employ in mind talk or moral talk have no home in the conceptual landscape revealed by natural science, therefore they must go. There are, for instance, no minds and the concepts of folk psychology are irreparably faulty. In their place we should employ the concepts of neuroscience and experimental psychology. The problem with this response is that it seems to underestimate the importance of the concepts that would have to be jettisoned (Price, 2011, p.132). We can perhaps see this best when we consider that the eliminativist response has been less appealing to naturalists when dealing with modal talk and mathematical talk. The problem, of course, is that modal talk and mathematical talk themselves play a vital role in our scientific theorizing. To eliminate all mathematical talk from our discourse would cripple the natural sciences, indeed, would do most damage to the science traditionally of most interest to naturalists themselves, physics. We might, as enlightened naturalists do without morals, but we couldn't spare our functions, sets and numbers.

An alternative strategy is to find a home for the troublesome entities within the framework of naturalism (Price, 2011, p.6). We might, that is, attempt to reduce the suspect concepts to those with which naturalism has no difficulty: mental states just are brain states, for instance, or mathematics is megethology. The problem with this strategy is carrying it out. It is difficult to think of a single uncontroversial example of a naturalistic reduction of one concept to another. Indeed, in the moral case, the very intelligibility of the strategy has been repeatedly called in question. Given the difficulties of the reductionist strategy, we might, as naturalists hope for a better way.

Price identifies one last strategy that he believes the naturalist can make use of to deal with the placement problems: we might adopt a non-cognitive semantics for the region of discourse (Price, 2011, pp.7-8). That is, we could claim that the discourse is unthreatened by our scientific world-view, because it doesn't have the function of making truth-apt claims or representing how things are with world. The obvious case study here is in metaethics. There is no problem, claims the non-cognitivist, finding a home for moral properties in the world, because the function of moral property words is not to refer to moral properties after all (cf. Ayer, 2001). Moral sentences do not purport to tell us how things are with the world and so their claims do not conflict with scientific sentences.

Now the problem Price sees with this strategy is one of containment (Price, 2011, pp.239-242). For instance, it has been argued that the non-cognitivist faces difficulties if she marries her non-cognitivism to a deflationary theory of truth (Price, 2011, p.240). After all, if all it takes for ‘P’ to be true is p, then how can the non-cognitivist rule out the truth aptness of moral discourse? All it takes for ‘Stealing is wrong’ to be true, is that stealing is wrong. But presumably the non-cognitivist would be happy to say that stealing *is* wrong (if not, we would have to doubt her character as well as her theory). So the non-cognitivist seems to have difficulty drawing the needed distinction between truth-apt discourse and non-truth-apt discourse. But then, she seems to have no basis on which to found a distinction between cognitive and non-cognitive uses of language.

As Price sees it, this is to get things the wrong way round (Price, 2011, pp.240-241). To view the deflationary challenge as a threat to non-cognitivism is to ignore the possibility that what blurring the lines between cognitive and non-cognitive, truth-apt and non-truth-apt discourse undermines is the former and not the latter category. The challenge assumes that it is cognitive, fact stating discourse that is in good standing, with non-cognitive, non-fact stating discourse needing to be distinguished from it. But perhaps, Price suggests (Price, 2011, p.241), the lesson we should learn is that no discourse is ever fact stating in the way the cognitivist supposes, that all uses of language perform some function other than that of representing things in the world. After all, the *essence* of minimalism about semantic concepts like truth is that there are no substantial semantic properties and no interesting theories to be had about them. In particular, there will be no theory of the kind: the purpose of truth-apt discourse is to state how things are with world. The lesson of the deflationary challenge to non-cognitivism, then, seems to be that we should allow that *all* language serves some function besides that of representing the world and the things within it (Price, 2011, p.241).

Price goes on to suggest that this is an overlooked position in the literature on placements problems: instead of eliminativism, reductionism, non-naturalism and local non-cognitivism, we might adopt a global non-cognitivism (Price, 2011, pp.8-

11). We might then claim that the placement problems cause no problem for naturalists, because, after all, it is not the function of any discourse, even scientific discourse to represent how things are with the world and its inhabitants. It is this fifth strategy for the naturalist faced with placement problems, global non-cognitivism, on which Price founds his metaphysical pluralism. In the next section I will give further details of Price's theory and attempt some initial criticisms of its plausibility, but for now I want to say a few words about the challenge Price's position presents my project in this thesis.

So far I have been assuming that there is something to be learned about the ontology of mathematics from scientific representation. In particular, if it had turned out that reference to mathematical entities played an indispensable role in scientific representation, I suggested that we should have been forced to accede to Platonism. But throughout I have been working with the assumption that the purpose of a representation is to tell us how things are with the world. Should this turn out not to be the case, should scientific representation play some other role in our cognitive lives, some expressive or prescriptive role, then it is difficult to see what could be learned about ontology from its study. Science, we were supposing was our authoritative guide to how the world is and in particular to what the furniture of the world contains. If Price is correct, then even science is not a guide to how the world is; it may be that nothing in our linguistic practice fulfils that representational function. And if that is so, it could well be that ontology rests on a mistake.

4.3 Representing Doesn't Work Like That

We have already seen one strand of Price's pragmatist deflation of metaphysics at work: global non-cognitivism. It is not the purpose of *any* of our linguistic practices to represent how things are with world. Another way of putting this is to say that Price adopts a very radical form of semantic minimalism: there are no robust, big R representations, no word-world relations of the kind the representationalist presupposes (Price, 2011, p.25). But, of course, this is only part of the story. At present this doesn't seem to deliver metaphysical pluralism. After all, it may be that some parts of our linguistic practice conflict with others, even if this is not a matter of saying different things about the world and its contents. Consider, for instance, two regions of discourse, one moral and one scientific, both of which serve a prescriptive function in our cognitive lives, but both of which prescribe conflicting activities. In this case, it seems that simply denying big R representationalism might not automatically deliver ontological pluralism (a point that I hope will become clearer when we discuss a possible pragmatist response to Price).

Second, Price wants to present his position as a kind of naturalism (Price, 2011, pp.184-185). Non-naturalism, remember, seemed hopelessly antiquarian, a desperate attempt to deny the inevitable encroachment of science and its results into the territory of philosophy. So Price needs to substantiate his claim that his global anti-representationalism is naturalistically respectable. In what follows I hope to show that it is not.

To establish metaphysical pluralism, Price makes use of a claim about human language use, namely that it is functionally diverse (Price, 2011, pp.136-140). As Price sees things, this functional pluralism is linked to his rejection of representationalism (Price, 1997, p.253): instead of thinking of language as an homogeneous tool for representing how things are with the world, we need instead to concentrate on the variety of roles language plays in our cognitive and practical lives. Once robust word-world relations of the kind the representationalist envisages are out of the picture, this anthropological investigation of linguistic practices is really all that is left to the linguistically oriented philosopher (Price, 2011, p.199).

I believe – and will argue in the next section – that Price has overstated this link between global non-cognitivism and functional pluralism. But for now I just want to concentrate on the details of Price's pluralism and to draw out its consequences for metaphysics of the kind being practiced in this thesis. Firstly, then, it is a striking feature of what we have been calling Price's global *non-cognitivism* that he does not in fact deny that there may be a central linguistic activity of 'description'. It may well be that there is a core practice, which Price variously labels 'assertion' and 'description', which all linguistic practices have in common or which all uses of language derive from (Price, 2011, pp.138-139). For instance, when discussing Brandom, Price suggests that "It is open to us to say that the key similarity is precisely that various of the different language games all avail themselves of the same inferential machinery" (Price, 2011, p.310), where this inferential machinery is part of Brandom's inferentialist account of the linguistic 'downtown' of assertion. Again, in *Naturalism and the Fate of the M-Worlds* (Price, 1997, p.254), Price suggests that there may well be a central linguistic category, 'description', whose core properties are common to all uses of language.

Nonetheless, Price is quick to point out that this is a fairly superficial level of linguistic homogeneity (Price, 2011, p.138), akin to the common clothing that Wittgenstein believed masked the diversity of language games and forms of life underlying our linguistic practices (Price, 2011, p.42). So, for instance, taking as our example the category of description, Price argues that what we need is an investigation of the core properties of this linguistic category that enable it to play the roles that it does in our cognitive and practical lives. But we need not assume that there is just one property or relation, namely representation, that is of the essence of description. Nor need we assume that the core properties of the descriptive category of language use cannot be employed differently in different situations. "Descriptive utterances about aardvarks serve different purposes from descriptive utterances about zygotes," (Price, 2011, p.138) and they are able to do this because the core properties of description can serve different functions.

It is now easy to see where the representationalist goes wrong. She assumes that there is one core property of description or assertion, and one purpose for which we employ descriptions with this property: namely, all descriptions are representations (employing word-world relations) and all are used to tell us how things are with the world. A global non-cognitivism, seen as the rejection of the idea that the function of language is to represent the world, undermines the idea that this can be the one true purpose for which language is employed – but note that it is functional pluralism, the idea that even core linguistic practices may serve a variety of functionally diverse roles in our lives, that does all the work of undermining the idea that there is *any* one true purpose for which language is employed.

4.4 From Functional Pluralism to Metaphysical Pluralism

We have seen where the representationalist goes wrong, but what about the metaphysician, where did she lose her way? In adopting the misguided doctrine of representationalism, replies Price (Price, 2011, pp.12-14). Clearly, as our focus in this thesis is on the metaphysics of mathematics, it will be important to understand and evaluate this claim.

To begin with, let's return to the placement problems. We were faced, as naturalistic metaphysicians with the problem of fitting important areas of human discourse into our picture of ourselves as entirely natural beings. Where, for instance, do mathematicalia fit, given that such things are causally inert and our best scientific picture of humans makes it difficult to imagine how human beings can talk about or know about causally inert entities (Benacerraf, 1983b)? But why should the placement problems be a disease endemic to representationalism? Well, because the representationalist is working with the picture that it is the function of all areas of human discourse (to be more precise, of all descriptive or assertive discourses) to tell us how things are with the world and its furniture. And if that is the sole function of language, then discourses that give us conflicting accounts of the world and its furniture will be incompatible.

We are now in a position to see more fully how Price's global non-cognitivism empowers us to deal with this sort of placement problem. Firstly, we deny that it is the function of *any* area of human discourse to tell us how things are with the world. From this we move to the claim that human language is functionally diverse. The first move makes it difficult to see how the claims of the two discourses, the naturalist and mathematical for instance, could conflict (Price, 2011, pp.7-8). More precisely, because neither discourse is in the business of telling us how things are with the world and its inhabitants, it cannot be that one discourse tells us something about the world and its inhabitants that is contradicted by the other (Price, 2011, p.8).

But it is the second move, functional pluralism, that enables us to rescue the previously imperiled regions of discourse. To see this, recall the global prescriptivist considered above, that is, someone who believes that no area of linguistic practice serves the function of representing, instead contending that all discourse in some way aims to prescribe values to others. This is a kind of global non-cognitivism, but it is still functionally monist. And because of this it is still possible for two regions of discourse to conflict – when, for instance, one of the discourses prescribes something that is proscribed by the other. It is still conceivable, then, that even Price's global rejection of representationalism may still suffer with placement problems, if there is some one linguistic function served by both scientific and moral, modal, mathematical, and mental discourses.

It is the business of functional pluralism to rule out this sort of thing. There may well be some core practice common to all these language games, but it will be a core practice capable of serving in many different functional roles (Price, 2011, pp.138-139). And if all these areas of discourse have different roles and fulfill different purposes in our cognitive lives, then it will not be possible for them to conflict. In other words, there is no reason to do away with mathematical talk just because we struggle to make it square with our best scientific picture of ourselves as knowers and speakers. To do so would be to assume that both mathematical and scientific talk were in the same functional category, that both had the same role in our cognitive and practical lives. But this latter claim remains to be substantiated, and given functional pluralism it is at least not immediately obvious that it will be.

So attempts to settle placement problems are philosophically misguided. They rest on a functional monism about language that is at best questionable. The effect of all this, Price suggests, is a kind of Carnapian quietism about metaphysics (Price, 2007, pp.377-378). Recall that for Carnap, existence questions of the kind of interest to the ontologist can be separated into two kinds (Carnap, 1950). On the one hand, we have the internal questions, questions the answer to which will be settled by the linguistic rules definitive of a particular linguistic framework. On the other hand, we have the external existence questions, questions the answer to which would be independent of any linguistic rules (Carnap, 1950, pp.21-23).

Now internal existence questions will come in two kinds. Consider, for instance, the framework of the rational numbers augmented with a name for the ratio of the circumference of a circle to its diameter. We can now ask two kinds of internal question within this framework, a substantive kind and a trivial kind. As an example of a substantive internal question, consider whether there are numbers *p* and *q* for which *p/q* = pi. The rules of the framework tell us how to find the answer to this question, but the answer is not trivial, we will have to do some work to arrive at it.

Now consider the trivial internal question, are there numbers? Given that we are working entirely within the number framework, the rules of the framework will deliver the immediate answer: yes.

External questions, again, come in two varieties. In so far as they are substantive, then they are pragmatic questions about whether it serves our purposes to adopt a given set of rules, to work within a particular linguistic framework. But in so far as external existence questions are attempts to get, as it were, behind the linguistic rules that tell us how to go about answering them, then they are not even trivial, they are meaningless. So, for instance, "are there numbers?", if it is not a trivial internal question or a pragmatic question about whether or not talking about numbers serves our purposes, is a piece of meaningless metaphysics, an attempt to bypass the linguistic rules that give any questions their meaning by telling us how to answer them (Carnap, 1950, pp.22-23).

Now Price sees similarities between this Carnapian stance on metaphysics and his global non-cognitivism plus functional pluralism (Price, 2007, pp.377-378). To see this, note that there will be substantive internal existence questions within any region of discourse. Suppose that we settle on what there is by seeing whether we quantify over things of that nature in our talk. And suppose that we are at present wondering whether there are electrons. Settling this may not be an easy matter. It may require us to carry out sophisticated and sensitive experiments on oil drops, for instance, or involve us in calculating minimal electric charges. None of this need be easy to accomplish. Nonetheless, the correct procedures for deciding whether or not we quantify over electrons, and so whether there are any, will be a matter internal to the region of discourse (physics) for which their existence is an issue (again, there will be trivial internal questions for Price as well. Consider, "are there physical objects?" asked within physics. The answer, "Of course there are!" is as trivial as could be).

But what about metaphysical existence questions? How are they to be settled? Let's suppose we have asked the question, are there really electrons? That is, we acknowledge that electrons are quantified over in the region of discourse within which the existence of electrons is at issue, but we remain unsatisfied. We want to know really, Really really, whether there are electrons. Well, this might be a kind of pragmatic external question. An anthropological question about what purpose, if any, electron talk serves in our lives. Note that this need not be trivial for Price; nothing in the anthropological investigation of human linguistic practice rules out the discovery of a linguistic practice that is useless or even harmful. And nothing rules out the discovery of a linguistic practice whose purposes might perhaps be better served by talking differently. But however that may be, this external question is not, and for Price cannot be a question about the furniture of the world, about what is Really really there.

How about the other kind of external question, the kind that wants to get behind the linguistic framework to the world it represents? Well, like Carnap, this is where Price locates metaphysical questions (Price, 2011, pp.189-190). Metaphysics turns out, for Price, to be an attempt to move from the linguistic representation to the world represented, an attempt to ask, not just whether we quantify over something in our ordinary linguistic practice, but whether the things quantified over are Really really there. But for Price, as for Carnap, this kind of question is illegitimate, and it is illegitimate for Price precisely because it presupposes representationalism (Price, 2011, p.190). The mistake the metaphysician makes is to assume there is some question beyond the question of whether we speak within a discourse about some kinds of objects. It is the mistake of assuming that that discourse serves the one true purpose of representation, that there are robust relations between our words and the world. But this is false: there are many purposes served by language, many reasons we might say that something exists. And there is no route from semantics to the world, no path from what we say (for myriad reasons) exists and what *exists*.

Given this mixture of semantic minimalism and linguistic functional pluralism, metaphysical pluralism results. All that remains is for us to investigate *why* humans feel compelled to speak about the existence of certain kinds of things, what purpose such talk fulfills in their varied and complex cognitive and practical lives (Price, 2011, p.199). And semantic minimalism of the extreme variety espoused by Price, the denial of any kind of strong semantic relations, rules out our finding a way down from the ways we talk to the world we talk about. Metaphysics, in so far as it requires a descent from semantics to world, is out; in its place a suitably pluralist anthropology will teach us that being, and so existence, is said in as many ways as there are forms of life (Price, 2011, pp.189-199).

Clearly this is an unwelcome result for this thesis, which is, after all, intended as a contribution to one small part of metaphysics. The question of mathematical ontology of concern to us, whether numbers, sets, functions, etc. exist is an instance of a placement problem. If the combination of anti-representationalism, linguistic functional pluralism, and their corollary metaphysical pluralism render placement questions moot, then they render this thesis moot, an academic exercise of the worst kind: the naturalist equivalent of wondering how many angels might dance upon the head of a pin. It will be helpful, then, to develop some kind of response to Price's anti-metaphysical challenge.

4.5 Naturalism and Pluralism: an Uncomfortable Marriage

With functional pluralism comes metaphysical pluralism, and with metaphysical pluralism we lose the picture of philosophy as an investigation of the world and its furniture. At the end of the last section we were left with the unsettling conclusion that all there is to existence is what is quantified over within a particular region of discourse, and that will be a matter for participants in that discourse. So what is left to the philosopher? And what is left for the naturalist, who saw herself as working within the confines of the scientific project?

Well, we were left at the end of the last section with the idea that there remains a kind of external question about linguistic practices and linguistic frameworks, namely, the anthropological investigation of the reasons and purposes human beings have for talking in the many different ways that they do. And Price is keen to reassure the naturalist here (Price, 2011, p.199): there is nothing for the naturalist to balk at in the idea that her new role is as a kind of sociologist or anthropologist of language. After all, this is a perfectly naturalistically acceptable undertaking: it is the investigation of human linguistic practice as a natural phenomenon (Price, 2011, p.199).

But in this section I want to argue that the combination of metaphysical pluralism and naturalism is more difficult than Price makes it appear. And if that is so, then in so far as metaphysical pluralism results from functional pluralism and global non-cognitivism, it makes the combination of these views with naturalism less attractive than we might initially have supposed.

So why might metaphysical pluralism and naturalism make uncomfortable bedfellows? Well, in short, the problem is with the status of science. After all, naturalism is, if somewhat nebulously, nothing but the idea that science is in some sense authoritative for philosophy. As Price himself puts it "I take [naturalism] to be the view that the project of metaphysics can properly be conducted from the standpoint of natural science." (Price, 1997, p.247n.1) Indeed, I think that Price has understated the naturalist's demands significantly here. Naturalism is the view that *philosophy* and not just metaphysics, should be conducted from the standpoint of natural science.

And Price himself seems initially happy with this. After all, his project is to tell us how we can stop worrying and love the placement problems consistently with our naturalistic scruples. And he repeatedly adverts to the idea that what science tells us about human beings and the linguistic practices of human beings may well have deflationary consequences for metaphysics (Price, 2011, p.5; pp.184-199). The problem with all this is with what happens to science if metaphysical pluralism is true. After all, if the arguments of the preceding section are anywhere near the mark we are left with the result that no discourse, not even scientific discourse is in the business of telling us how things are with the world. And all discourses are in the same boat here. Science may well play a different function in our cognitive lives to, say, moral discourse, but not a better one, and not a more interesting one, and certainly not a *metaphysically fundamental* one (Price, 2011, p.199). The upshot of all this is that the existential claims of natural science turn out to be different to, but no better than, any other region of discourse. Put another way, in the world of the metaphysical pluralist, science is just another perspective, with no real claim to authority (Price, 2011, pp.30-32).

But this seems to fit rather poorly with the naturalist claim that science *is* authoritative. One reason for embracing functional pluralism is that it provides a naturalistically respectable way of diffusing the placement problems. But why should we be constrained to accept only *scientifically* acceptable solutions to the placement problems, if science is just another perspective, speaking with no metaphysically authoritative voice as to how things are?

Perhaps worse is the havoc that metaphysical pluralism plays with Price's claims about human language use. Given what we know of ourselves as revealed by natural science, and given the likely role that language will play in the lives of creatures like that, linguistic functional pluralism comes to look like an attractive story about semantics. At the very least, semantic monism of the kind embraced by the representationalist has a worryingly a priori flavour, if it is just the blanket rejection of pluralism prior to any scientific investigation. But now suppose that Price is correct about the link between global non-cognitivism and metaphysical pluralism. Suppose that science is no more authoritative about how things are with human language users than is any other discourse, because no discourse plays that representational role. Then why should we treat the deliverances of natural science as authoritative for philosophical semantics? And if metaphysical pluralism is true, and no discourse's existence claims are better (in the sense of more representative of how things Really really are) than any other's, why should philosophical semantics be constrained by the existential claims of natural science?

To put all this another way, we are supposing that human language users are natural creatures with only natural properties and natural relations to natural things, *only because this was the picture revealed by our best science!* (Price, 2011, p.186) We have since discovered that the picture of the world revealed by our best science is no more representative of how things Really really are with human beings than is the picture revealed by any other functionally distinct discourse. And yet we seem to be clinging to the idea that human beings Really really are natural creatures with only natural properties and natural relations to natural things, rather than, say, Cartesian egos with non-physical properties and extra-sensory relations to a realm of pure ideas. If we could find some role that talk of the latter kind plays in at least some areas of human life (and I suspect that we could), why should its existential claims and its descriptions of human language users not count as equally authoritative with those of science, at least given the pluralism Price wishes to defend?

We seem to be faced with an unpleasant choice, given metaphysical pluralism and the position to which it relegates scientific discourse. Either way we seem to have to abandon the claim that science is authoritative for philosophical semantics, but there are two ways in which we might take this. On the one hand, we could see this as undermining many of the foundations on which Price's pluralism rests. That is, we no longer have reason to believe that human language users are natural creatures and so we should abandon this claim and the idea that it somehow supports functional pluralism or underwrites the naturalistic acceptability of Price's project. But this is just to retreat to first philosophy, to treat functional pluralism and anti-representationalism as a priori commitments unresponsive to scientific theory.

On the other hand, we could retain the idea that human beings are natural creatures, and that this constrains proper philosophical theorizing about semantics. But now we are faced with two further problems. The first is that the claim about human language users – that they are natural creatures, in a natural world, entering into only natural relations – was originally derived from scientific theories we now regard as just one more perspective among many equally good perspectives. If that is so, then it looks worryingly like our claim that human language users are natural creatures, in a natural world, entering only natural relations, is an a priori commitment. Once again we have beaten a retreat to first philosophy. But this response looks even less promising when we consider in what sense we are to take the claim that humans are natural creatures, in a natural world, entering only natural relations. After all, if this is not a descriptive claim, a claim about how things Really really are with the world, then what kind of claim is it? What other role could it be playing and how would that role ensure that this claim *actually* did constrain philosophical speculation about semantics?

I suspect that these kinds of worries pose a major challenge to the kind of pluralism Price envisages. That is, I see no way to be a naturalist, that is, someone for whom science is authoritative, without embracing monism of some kind. In the next section I want to spend some time discussing ways in which a naturalist might embrace pragmatism *without* ensnaring themselves in pluralism. My aim is not to defend this position, but to show how much of Price's project depends upon the unargued assumption of functional pluralism (and a kind of evaluative equality as well). But whether or not the suggestions I make succeed in undermining the idea that a pragmatist must or should be a pluralist, I suspect the objections raised in this section force us to an important choice point: we can be pluralist or we can be naturalist, but we cannot be both.

4.6 A Non-Pluralist Pragmatism

In this section I want to suggest two ways in which a pragmatist might resist Price's metaphysical pluralism. In short, this requires rejection of either of two unargued assumptions Price makes (one explicit and one implicit in Price's presentation). On the one hand, we can accept that the function of language is not to mirror the world, without accepting that language is functionally diverse. Alternatively, we might accept both of these claims, but deny a premise that I believe is implicit in Price's presentation of his project, namely, *evaluative equality*; in other words: of all the different linguistic functions, none is better than any other.

It is not my intention to defend the resulting position. Indeed, I suspect that such a position will face a challenge from those who believe that it is a kind of metaphysical trivialism. My aim is only to exhibit a kind of philosopher for whom the denial that language is a mirror of the world would not involve the view that almost any way of talking is potentially as good as any other. In doing so, I hope to exhibit the central role that the unargued premises of functional diversity and evaluative equality play in establishing metaphysical pluralism. Hopefully this should further strengthen my claim that metaphysical pluralism is not mandated for a naturalist, and further bolster the defence of the variety of naturalist metaphysics undertaken in this thesis.

4.6.1 More Problems with Pluralism

Why might a philosopher who has rejected representationalism be interested in denying Price's pluralist thesis? Well, recall that for Price existence is a thin notion. There is no more to ask about the existence of some entities than whether they are quantified over (or referred to or whatever) in a particular region of human discourse. The only remaining questions are not metaphysical ones (sure, so and sos 'exist', but do they Really really *exist*?), but instead the anthropological question of why creatures like us find it useful or important to speak a language in which such existence claims are warranted.

The resulting position, as we saw, was a kind of Carnapian metaphysical pluralism, a pluralism in which many different kinds of ontological commitments – those of mathematical discourse, for instance, as well as natural scientific discourse – can be tolerated. But this tolerance was bought at the cost of deflating the very idea of ontological commitment, by making ontological commitment no more than a matter of whether entities of a particular kind are quantified over in some region of discourse (or something to that effect. It is, in fact, none too clear what notion of ontological commitment Price is working with). The obvious worry here is that this makes Price's position *far too* tolerant. There will be many kinds of existential commitments, in many regions of human discourse, that we do not want to inherit. There are the gods of various kinds of religious discourse, the planetary influences of astrological discourse, the 'memory' that homeopaths attribute to water, and so on and so forth. Even if we allow our naturalistic scruples to turn a blind eye to moral talk and modal talk, there is no way that we can tolerate the existence claims of theology and crystal healing.

The issue here is that Price seems to lack the resources to rule out these less respectable areas of human language use. Certainly, some people sometimes for some reasons find it profitable to speak about supramundane causal influences or transubstantiation. And given that it is not the function of *any* discourse to represent how things are with the world, there is no further question about whether there *really* are supramundane causal influences or whether transubstantiation ever actually occurs. Instead, all we as philosophers are left with is the rather dry task of understanding why creatures of the kind we believe human beings to be would go in for talk of this kind.

But this seems deeply troubling. The only way the kind of inquiry Price envisages could rule out these kinds of talk is by discovering some anthropological reason to reject them. Perhaps, after all, they do not serve any function. Or perhaps the role they play in our cognitive and practical lives would have been better served by some other kind of linguistic practice. But this doesn't seem nearly enough. There are two worries here: the first is that anthropological enquiry is *not guaranteed* to throw up some fault in the relevant kind of discourse. But, as naturalists, we do seem justified in saying that the existence claims of homeopathy and astrology are guaranteed to be wrong.

The second worry is that this just seems to locate the problem with these kinds of linguistic practices in the wrong place. Intuitively, the worry with homeopathy is not that creatures like us, situated as we are, should refrain from homeopathic discourse or that our practical needs or whatever would be better served by, say, scientific medical discourse. Or to put this another way, these are problems with homeopathic discourse, *but they have a deeper foundation*. What is wrong with homeopathic discourse and the reason we should go in for some other kind of discourse is that homeopathy just gets things wrong about the world.

Now anybody who has committed themselves to the rejection of representationalism cannot really salvage the second point. Indeed, anyone committed to the idea that there are no robust word-world relations presumably would not feel the force of this objection. Nonetheless, the scrupulous naturalist may well feel that we still need to do something to guarantee that our repudiation of the existential claims of quack discourses will not depend on the whims of an anthropological enquiry that could go either way. That is, even if we reject representationalism, we may still want to preserve the authority of natural science. If the natural sciences say something doesn't exist, or make the existence of something unlikely, then whether that something is morals, numbers, or the holy trinity, we should not allow functional pluralism to overrule their authority.

4.6.2 Pragmatism Sans Pluralism

Let's suppose that we are willing to concede to Price his anti-representationalist stance (for the sake of argument), but as naturalists we are concerned about even *thin* ontological commitment to the kind of nonsense foisted onto us by the quacks – how might we nonetheless resist ontological pluralism? Well I see two ways of achieving this. Remember that Price's pluralist position has two working parts: the rejection of representationalism *and* the endorsement of functional pluralism. So suppose we were to reject functional pluralism. That is, we reject the idea that different regions of language serve different functions. We are now in the position of the global prescriptivist we considered earlier, that is, we have conceded that all language is non-cognitive, but we have denied the crucial premise that different regions of discourse may serve different functions (or that there may be a central activity of asserting or describing, but that this central activity is itself applicable in a variety of different cognitive and practical projects). And we saw above that the claims of different regions of discourse *can* conflict for someone who has adopted this mixture of anti-representationalism and functional monism.

Now the kind of naturalistic pragmatism I have in mind here doesn't want to claim that *all* language serves a prescriptive function. Instead consider an anti-representationalist who says something like this: "no region of language serves the function of representing how things are in an external world. Instead, all language serves a much more practical function: it enables us to cope with incoming experience, it enables us to predict future experience on the basis of past experience." Now, clearly, if all language has this practical function, different regions of discourse, employing different ontological posits *can* conflict. They can conflict at the level of *how well* they enable us to predict and control our experience.

Suppose, then, that we have two regions of discourse, a region of discourse that predicts the flow of future experience using a theory of elementary particles and laws governing their interaction, and a region of discourse that attempts to meet the same requirement using a theory of the Homeric gods. The elementary particle theory (well, really other theories in what we might broadly think of as 'scientific discourse') make the existence of Homeric gods unlikely (in the thin sense, that existential quantification over them would play havoc with the theory), and the elementary particle theory does a much better job of predicting and controlling our future experience. Surely this gives us very good reason to reject the thin posits of the Homeric theory in favour of those of the elementary particle theory.

Again, consider a community of speakers who make use of a theory of viruses and another community of speakers who make use of a theory in which all illness is the result of demonic possession. Ex hyposthesi, both communities speak the languages they do in order to predict and control their future experience (in this case their experiences of bodily health). But clearly the speakers who make use of the existential commitments of the virus theory will, all things being equal, do a much better job of this than the community who make use of the demon theory. Again, we have a reason to adopt the thin ontological commitments of the virus theory that is, at the same time, a reason to reject the thin ontological commitments of the demon theory.

Now a position *like* this will sometimes be available to Price. So long as our anthropological enquiry discovers that two apparently distinct regions of discourse *actually* serve the same function in our cognitive or practical lives, then they can come into conflict in this way. And perhaps in the case of the demon theory and the virus theory this will be the case. Both, even for the functional pluralist, will be employed in the pursuit of health and the treatment of illness. But is every quack discourse employed in this way, in pursuit of some goal that is also the goal of a more successful scientific enquiry? Consider, for instance astrology. Astrology is surely bunk, from the perspective of natural science. Its ontological commitments are just straightforwardly naturalistically unacceptable, functional pluralism notwithstanding. But just which scientific toes does astrology tread on? what functional role is served by *both* scientific talk and astrological talk? Astrology, after all, is not weather forecasting. Nor is it experiment or engineering or any of the other scientifically respectable activities where some knowledge of future contingencies is useful.

The worry here is that Price's anthropology of language use just will not be able to find some function that both scientific and astrological discourse both serve, and so some way in which they conflict with one another. And given Price's thin notion of ontological commitment, it looks as though the posits of an astrological theory will turn out to be naturalistically respectable after all.

But this is just monstrous: any naturalism that cannot banish astrology is in dire straits indeed. By denying the functional pluralism that underwrites Price's metaphysical pluralism, we have seen one way in which the anti-representationalist and the pragmatist can banish the quack discourses. If we insist that all language has the non-representational, pragmatic function of predicting and controlling future experience, then this enables us to recognize that some discourses do this better than others. And once we have recognized this we can limit our ontological commitment, even our *thin* ontological commitment, to just those discourses that are the best at fulfilling this functional role.

There is another way, I think, of achieving a similar result, and consideration of this second road to a non-representationalist monism will enable us to bring to view a premise that I believe is implicit in Price's presentation of his project. Suppose, then, we have conceded global non-cognitivism. And suppose further that we have granted Price's functional pluralism. Is metaphysical pluralism the inevitable result? I think not. To see this, consider what would happen if we were to allow that different language games have different roles in our cognitive lives, but that, notwithstanding this, some one of these 'forms of life' was the preeminent one. That is, we humans go in for many different kinds of activities, subserved by many different linguistic practices, but only one of these activities (and so only one of these language games) is *really* useful or valuable. Suppose, for instance, we were to suggest that the only really valuable human activity was to discover what would happen to us in the future and to control future occurrences. That is, the *really* valuable activity is prediction and control of incoming experience. We could then limit our *thin* ontological commitments to just the posits employed in that region of discourse that best serves the one really valuable human activity.

To put all this another way, we can see that Price's metaphysical pluralism rests on the assumption that all the different forms of life are equally valuable. In other words, he has tied his functional pluralism to a kind of equality of evaluation: all the different regions of discourse, all the different language games, are all *equally good*. But it seems perfectly possible to deny this claim, and if we do so, and tie ontological commitment to what we regard as the most valuable linguistic practice, then it doesn't seem that metaphysical pluralism results. Many posits, of many regions of discourse will be discarded, simply because they are not among the posits of a truly valuable discourse.

Now either of these ontologically monist positions seems to deal better with the quack discourses than does Price's. In the second case, we free ourselves to discard the existential commitments of astrology, homeopathy and the rest, by tying a thin notion of ontological commitment to the idea that some activities and so some linguistic practices are better (not better at representing, but perhaps just better at realising important human values) than others. In the first case, we saw that by insisting on the functional unity of human language use, we could deny representationalism, but still banish the quack discourses and their commitments from a naturalistically respectable ontology. Given that these two positions solve Price's problems with tolerance, I suggest they are better bets for a naturalistically inclined pragmatist.

But it has not been my intention in this section to defend either variety of anti-representationalism. I can see a number of issues with both the strategies outlined above. Firstly, it is open to Price to insist that this is not much of a rescue of metaphysics. After all, ontological commitment is still a thin notion, nothing more than being quantified over in the discourse or discourses that best serve the one function of all language (or appear in the discourse that best serves the linguistic role required by the most valuable human activity). This is not a matter of how things are in the world; it is still illegitimate to ask, given that there are electrons, whether there Really really are electrons? Indeed, metaphysics seems to have become nothing more than the trivial business of reading off the existential commitments that appear in our best scientific theories.

I can see other issues with the two proposals outlined in this section.[13] But as I have said, it has not been my intention to defend either way for an anti-representationalist to resist metaphysical pluralism. Instead, I hope to have shown the essential role that functional pluralism plays in establishing metaphysical pluralism, that it is, in effect, the sine qua non of Price's deflationary metaphysics. Without the claim that language plays many different roles in our practical and cognitive lives (and without the equally essential claim that all of these activities and discourses are equally good), Price cannot claim to have established metaphysical pluralism. And as far as I can see, neither claim has been sufficiently argued for. Neither functional pluralism nor evaluative equality is straightforwardly a result of denying representationalism. Merely because language does not have the one function semantic monists have tended to think it does, that doesn't establish that it has no *one* function after all. And while the claim that language is a multi-purpose tool might at least be established by

[13] For instance, with respect to the second strategy, the claim that the only, or even the most, valuable human activity is prediction and control of experience seems hard to swallow.

the kind of anthropology Price recommends, it is difficult to see how any similar kind of enquiry could establish the claim of evaluative equality. Even if we were to discover that, as a matter of contingent fact, descriptive or assertive language use had many functions, that wouldn't establish that all of these functions were equally good.

There is, then, at the heart of Price's project an essential premise for which it is difficult to uncover any argument. And given that, as we saw at the end of the last section, together with anti-representationalism this premise threatens to undermine the very naturalism that motivates Price's response to the placement problems, it seems that we have strong prima facie reason to resist, if not functional pluralism, then at least its combination with anti-representationalism to produce the kind of view Price recommends. Furthermore, if the arguments of this section are correct, then we have at least two kinds of naturalistic pragmatism for which the ontological question addressed in this thesis would be a perfectly substantial one. I conclude that anti-representationalism by itself is no threat to the mathematical fictionalist.

4.7 A Defence of Representationalism and a Vindication of the Placement Problems

In the previous section we considered a possible kind of anti-representationalism which was resistant to the kind of ontological pluralism Price defends. If we accept that it is not the function of any discourse to 'represent' how things are with the world, but insist that, nonetheless, language is functionally monist, then we may well discover that there can be the kinds of ontological conflicts we have been considering under the name of placement problems. But I do not want to suggest that this is the best that an opponent of Price can do. Instead, in this section, I want to consider a number of issues faced by Price's program that seem to have natural responses if we endorse representationalism, but look more difficult to account for from the anti-representationalist perspective.

To begin with, we need to consider just exactly what the anti-representationalist thinks language is for. Given that language does not serve the function of representing how things are with the external world and its inhabitants, just what is its function? The first thing to note is that clearly we cannot expect an entirely uniform answer to our question; the raison d'etre of Price's Naturalism Without

Mirrors is the denial of the idea that language has just one function. To a certain extent, then, our question will be answered by the piecemeal anthropological enquiry that tells us why human creatures have developed the kinds of linguistic practices they have, and what role these practices play in their everyday practical and cognitive lives.

But this will not be all that can be said. Remember that, for Price, there is a certain amount of uniformity to human linguistic practices, a kind of common clothing that they all wear: namely, assertion. Assertion and assertoric language form a kind of core linguistic practice, language's 'downtown', upon which all our other linguistic activities depend. So we can rephrase our original question about the function of language so that it becomes instead a question about the function of assertoric language: for what purpose do we have practices of uttering and evaluating assertoric sentences?

Here is what Price says about this:

> The suggestion is that the core function of assertoric language is to give voice to speaker's mental states and behavioural dispositions, *in a way which invites criticism by speakers who hold conflicting mental states.* (Price, 2011, p.139)

In other words, Price believes that the reason so much of our language comes in assertoric form is that it invites criticism from our interlocutors. When we utter sentences (or write them) asserting something, we are in effect making ourselves answerable for what we have asserted, opening ourselves to the challenge to give reasons for our utterance. Or in other words:

> The key thing about assertoric discourse is thus that it embodies a normative idea of answerability to an external standard, the effect of which is to place an onus on speakers to be prepared to defend their views in the case of disagreements. (Price, 2011, p.139)

But this external standard is not the external standard imposed by the requirement to say only things that accurately represent the world, but is instead a standard imposed by our linguistic community, a case of the norms of evaluation for the relevant area of discourse (which may differ from discourse to discourse, and which, for the anti-representationalist, will presumably never be a matter of getting it right about the external world and its inhabitants) (Price, 2011, p.139-140).

Now Price is fairly tentative about putting forward this theory of the role of assertoric language, insisting that it is only one plausible hypothesis and that it doesn't ultimately matter (for his purposes, anyway) if it is, in fact, the correct one (Price, 2011, p.139). But it is interesting to note that it does parallel some of things that Price says about truth. Note, to begin with, that assertoric discourse is, as ordinarily construed, discourse that is truth apt. Price does not want to deny this, but he clearly cannot make use of any kind of theory of truth that would make truth a matter of a relation between a truth bearer (in this case, assertoric sentences) and the world. Such a relation would seem to be the kind of word-world relation the anti-representationalist is at pains to deny the existence of, and so, clearly, he will need to make use of some other kind of theory of truth. Price's suggestion (Price, 2011, p.47) is that the role of truth is to serve as a norm governing discourse, a norm that, in effect, facilitates agreement between speakers. When speakers agree that something is true, this will put an end to their disagreement and to any further discussion. Without some such norm it is difficult to see how discursive and argumentative activities could ever reach a satisfactory termination.

This is obviously just a sketch of the kind of idea Price has about truth, but even from the sketch it is clear that this kind of proposal meshes quite well with Price's tentative hypothesis about assertoric language. Assertoric, truth-apt language, invites challenge and invites discussion of reasons, but contains at the same time a discursive norm that will tend to settle discussion and promote agreement among interlocutors. But this neat picture of assertoric language is threatened when we start to wonder how disagreement and conflict could arise for speakers of the kinds of language Price envisages. That is, we need to ask how speakers can have *conflicting mental states* given Price's anti-representationalist picture of language.

We are now faced with the challenge of saying how mental states can conflict with, or contradict one another. Price says very little about this, but here is one kind of account of how this can happen. Beliefs, and mental states in general, have truth conditions. And these truth conditions can conflict. We might, for instance, be persuaded by Field of the need to attribute truth-conditions to mental states for explanatory purposes, and that, moreover, if these explanatory purposes are to be served, then these truth-conditions will have to be correspondence truth conditions (Field, 1978). This gives us an easy answer to how mental states can conflict, but it gives us this answer at the price of representationalism, in this case representationalism about mental states. Given this representationalist theory of truth conditions for mental states, we can say that two people's mental states conflict when they deliver conflicting representations of the world. But this would be, to say the least, a very odd position for Price to adopt. It would amount to the claim that the function of language is not to represent the world, but that the function of our mental states is. And given that, if assertoric language is meant to invite criticism about our mental states it will somehow have to give voice to those mental states, it is very difficult to see how these positions might be coherently combined.

Of course, it is open to Price and other anti-representationalists to deny representationalism about mental states, as well as representationalism about language. But at this point a second worry arises. The whole point of denying representationalism was to evade the kind of disagreements between apparent representations that we have been calling placement problems. The mathematical placement problem, for instance, is dissolved once we recognise that, because it is not the function of either mathematical or natural scientific discourse to represent how things are with the world, neither discourse in fact says conflicting things about that world. But this might cause us to fear that anti-representationalism about mental states might be a similarly powerful solvent of apparent disagreements. If no mental states serve to represent how things are with the world, then they will not be representing the world in conflicting ways. But this would be a disaster if the 'core function of assertoric language is to give voice to speaker's mental states…*in a way which invites criticism by speakers who hold conflicting mental states*' (Price, 2011, p.139), because it will turn out that there aren't any conflicting mental states.

I am not suggesting that anything I have said so far is a knock-down argument against Price's anti-representationalism. But at the very least, the onus is on Price to tell us, given his anti-representationalism about language, what kind of theory he has in mind for mental contents, and how disagreement and conflict work in that theory. The representationalist has an easy answer here, and it is not clear just what Price's answer is supposed to be. Given the role conflict between mental states is playing in Price's account of assertion, this is a troubling omission.

We might also worry about the level of tolerance that Price's theory of truth, sketched above, seems to enjoin. Remember from the last section that metaphysical pluralism seemed to have a problem ruling out kinds of discourse that we do not in fact regard as respectable. The price of reconciling naturalism and moral talk, for instance, seemed to be that we also had to tolerate homeopathic and astrological talk. A similar worry arises for Price's anti-representationalist theory of truth. We presumably want to give voice to our concerns about the quack discourses by saying that the problem with these discourses is that almost everything they say is false. But for Price, truth is just a matter of regulating agreement and disagreement *within* discourses, it is not a matter of getting things right or wrong about the external world. But then we might worry that, so long as there are evaluative standards within a discourse, marking some sentences as acceptable and some unacceptable, then that will be enough to have *truth* for that discourse. In other words, so long as there are recognised standards for settling disputes within a discursive practice, there will be truth for that practice. But that certainly seems to be the case for, say, homeopathy. Homeopaths will disagree with one another, and there will be recognised channels for settling such disagreements. But then there will be homeopathic truth, and so long as the standards for settling disagreement do not overlap with those of, say, medical science, then homeopathic truth will just be a different (not a worse) kind of truth to medical scientific truth. It seems that naturalists, at least if they adopt Price's theory of truth will have problems ruling out kinds of linguistic practice that even the most lenient are usually intolerant of.

Once again, it seems that the representationalist has an easy answer to this challenge. Let us take as an example of a representational theory of truth a correspondence theory. Then we can say that the problem with homeopathic sentences is that they fail to correspond to facts, or states of affairs (or we can tell a more complicated story if we have adopted something like Field's correspondence theory). In other words, the problem with homeopathic sentences is that they are very largely false. Once again, the representationalist has an easy answer to an issue that seems problematic for the anti-representationalist – which must surely serve to enhance the attractiveness of representationalist theories over anti-representationalist ones.

Another worry for Price's anti-representational stance comes out when we consider the topic of reduction. Clearly the pluralist wishes to avoid the kinds of reductions of mental or moral properties, for example, to natural or physical ones that certain kinds of naturalists or physicalists will believe necessary. But the *metaphysical* pluralist goes much further: reducing the entities of one discourse to another *functionally* distinct discourse is not only not necessary, it is something like a category mistake. In effect, reductions of, say, moral properties to natural ones look like attempts to settle the 'Julius Caesar' question of whether the number 2 is identical to the famous Roman emperor. The problem with both the reduction and the 'Julius Caesar' question is that they are attempts to settle the identity of objects across discourses or linguistic frameworks. As Price puts it, 'To ask whether an entity referred to in one framework is identical to an entity referred to in another framework seems to presuppose a framework-independent stance, from which the question can be raised.' (Price, 2011, p.146)

Now the worry with this is that it seems to prove too much. It looks as though all attempts at reduction *must*, for Price, be internal to a discourse. But this just doesn't look as though it will always be the case. Consider, for instance, the reduction of water to H_2O. Many people will want to claim that water is identical to the substance with that chemical composition, but this seems to be a case where we are attributing identity across discourses or frameworks. On the one hand, we have our linguistic practices concerning ordinary, common-sense objects and substances, and on the other we have the linguistic practices constitutive of mature chemical science. And it just doesn't seem like these are the same framework. If this is correct, then it looks as though the identification of water with H_2O is as problematic as the identification of mental states and brain states, goodness with utility, or Julius Caesar and the number 2. There is no framework independent stance, no position above and beyond our discursive practices from which these attributions of identity can be made.

The problem here is noted by Price (Price, 2011, p.147), but never adequately resolved. It is, in effect, the problem of individuating linguistic frameworks, and describing their relations to one another. For instance, we could solve the problem with water and H_2O, if we were able to say that the common-sense ontology discourse is somehow embeddable within the chemical scientific discourse. Or, perhaps, the objects and properties of chemistry are shared with the objects and properties of common-sense ontology. The problem with this is that Price says nothing at all to settle these issues. He raises, for instance, the questions of individuating linguistic frameworks, and of combining linguistic frameworks and nesting them, and how linguistic frameworks might relate to one another (Price, 2011, p.147); but he never actually answers these questions. He doesn't tell us if frameworks *can* be nested or *if* they relate to one another at all. He doesn't even provide a rudimentary sketch of identity conditions for frameworks. But this just leaves open the possibility that each region of human language use is an island, entire unto itself, and that all reductions, even apparently scientifically respectable ones are in fact illegitimate.

Once again, representationalism appears to have the upper hand here. If all discourses are in the business of representing how things are with the world, then the subject matters of two discourses will overlap just if they are describing the same bit of the world. In other words, the objects of two regions of discourse can be identified, because the objects of two regions of discourse can be shared. And they can be shared because both discourses are in the business of representing how things are with the world and its objects.

Obviously this doesn't make reduction an easy matter; there is still the tricky task of trying to discover *when* two apparently distinct regions of discourse are in fact describing the same bit of the world. But it does at least make reduction possible. We have left open the possibility that, for instance, our common-sense ontology discourse (at least when it talks about water) and our mature chemistry discourse (at least when it talks about H_2O) *are in fact talking about the same stuff*! We have, of course, also left open the possibility that mental states are identical with brain states (or that the number 2 may be identical to Julius Caesar). But this is surely how it should be: reduction may be difficult because it involves chemical investigation (water is H_2O), but it may also be difficult because it involves some other kind of investigation, metaphysical perhaps or conceptual (mental states are brain states). Reduction may even be impossible because it involves identifications that are, for local reasons, malformed (like, perhaps, the attempt to identify Julius Caesar with the number 2). But reductions should not be globally impossible because of the lack of an overarching framework or discourse in which to carry them out.

Once again, it is important to note that this is not intended as a coup de grace to the anti-representationalist theory; it is a request for further elaboration. Until the anti-representationalist enlightens us as to how linguistic frameworks or regions of discourse are to be individuated and how they relate to one another, she will leave open the unpalatable possibility that even scientifically respectable identifications of the objects of one discourse with those of another are in fact *naturalistically* illegitimate. Perhaps there are ways of filling this lacuna, but until this is accomplished (and in such a way as to rule out reductionist responses to placement problems), then the representationalist seems to have the upper hand.

So far the representationalist has had the upper hand in accounting for scientifically respectable reductions and in accounting for disagreement and conflict between mental states. I also believe the representationalist has the upper hand when it comes to an issue that is at the very heart of Price's project. Remember that for Price the core role of assertoric language is 'to give voice to speaker's mental states…*in a way which invites criticism by speakers who hold conflicting mental states*'. (Price, 2011, p.139) Now this explains (if we discount previous objections), why we have a *linguistic* practice of assertoric utterance. But it does little to explain why we have practices of disagreement and agreement *in the first place*. What role does agreeing with one another serve in our practical lives? Why do we bother to demand reasons of one another, when those reasons have nothing to do with how the world is?

Once again, I think the representationalist has an (admittedly speculative) answer to this question. Creatures like ourselves, existing in the kind of world we inhabit will have a very strong interest in *getting things right about how the world is*. Creatures, otherwise like ourselves, who are routinely mistaken about how things are with the external world would not tend to last very long in that world. But one important way in which we learn about the world is through discussion with others of our kind. We learn more from others than we do by ourselves. But discussions that continue interminably (or end in unresolved disagreement), will serve this function very badly. We learn nothing from one another if we can never agree.

We have, then, an account of how norms tending towards agreement (truth, for Price) can grow up for a representational species. What about the very practice of discussion, of agreeing and disagreeing, of demanding reasons? Well, here I think our answer will have to depend on the idea that after a process of giving reasons that are themselves beholden to the norm of getting things right about the world, any conclusions on which we all agree will be better representations of how things are with the world. Interlocutors who agree after they have reviewed all the reasons, will be agreeing about representations that are more likely to be correct about how things are with things in the world. And having representations like that must surely be a good thing for creatures like us.

Now I do not expect anything I have said to convert the confirmed sceptic. If you are convinced that, after a process of sustained examination of reasons beholden to the supposed norm of getting things right about the world, we could end up agreeing, and still be radically mistaken, then the above story will probably not be terribly convincing. But the representationalist has at least provided a story about how these practices grew up among creatures like ourselves situated as we are. Just what kind of story can the anti-representationalist give? Why do we bother agreeing and disagreeing with one another, when that has nothing to do with describing our shared (external) environment? I am not sure that Price says anything about this, and the concern is that, for the anti-representationalist, practices of agreement and disagreement *are* the bedrock on which all other linguistic practices are founded. That is, there is a worry that for the anti-representationalist, engaging in discussion becomes an end in itself, not explainable by any kind of more fundamental activity. But this is a deeply unattractive picture.

Once again, it is important to recognize that the anti-representationalist may well have responses available. Perhaps reaching agreement in conversation in some way facilitates social cohesion, for instance. But however that may be, the provision of some such story is essential. We cannot rest with practices of agreeing and disagreeing, as though they floated free of any requirement for further explanation. In so far as the representationalist offers such an explanation, she seems to have the advantage over Price's anti-representationalist.

In this section we have seen three points at which the anti-representationalist owes us an explanation that seems to be within easy reach of the representationalist. We need to be told how mental states can conflict, without that committing us to a representationalist view of mental content. We need to be told how discourses are to be individuated and identified (and how they relate to one another) in such a way as to rule out reductionism about placement problems, but to rule in reductionism about water and H_2O. And finally we need to be told what the point of all this linguistic behaviour is, all this discursive to-ing and fro-ing, if it is not to get things right about the world. To all three challenges the representationalist gives a similar answer: mental states represent the world, and conflict when they provide conflicting representations; discourses represent the world, and reductions are possible when they represent the same bit of the world; discourses represent the world, and reaching reasoned agreement in discussion with others tends to produce better representations. Until Price provides his anti-representationalist responses to these challenges the ball is in his court, and it is advantage representationalism.

4.8 Final Remarks – Back to Mathematical Fictionalism

The bulk of this chapter has concerned issues that may in some ways seem far removed from the topic of mathematical fictionalism. It will be well, then, to remind ourselves how all this bears upon the defence of the fictionalist account of mathematical applications developed in the previous chapter.

In chapter 3 we saw how a revolutionary, prop-oriented pretence-theory could be leveraged to downgrade the ontological commitments of the mapping account of mathematical applicability. But this involved us in claims about scientific representations involving mathematical posits. In effect, our claim was that mathematical-scientific representations are a kind of pretence or make-believe, something like an ongoing communal fiction whose content is given by the mapping account, and the focus of which is not on the content of the fiction so much as on the props it is used to describe. The idea was that because these are fictional representations, we need not accept ontological commitment to their posits in the way we would if those posits appeared in a realist discourse whose focus was on directly representing the way the world is.

From this perspective, Price's anti-representationalism presents an extremely worrying challenge. It threatens to undermine the very utility of this kind of project by calling into question the naturalistic respectability of the whole idea of a realist discourse, that is, a discourse which has the function of representing how things are with the world. If no discourse represents how things are with the world, so that we cannot distinguish between mathematical-scientific representations and supposedly *literal* representations in this way, then we might wonder what *is* the point of distinguishing them. Of course, it might be that we have good naturalist, anthropological reasons for distinguishing these kinds of uses of mathematical-scientific discourse from what we ordinarily call "literal" language. But we certainly shouldn't do so just because we are feeling ontological scruples.

In this chapter I have identified three different kinds of response we might make to Price's anti-metaphysical project. We noted, to begin with, that Price's anti-representationalism seems to struggle to vindicate the naturalist presupposition that science is in some sense authoritative for philosophical inquiry. By relegating science to just one more discourse among many equally good discourses, it threatened to undermine that authority, and we were left wondering just how coherent the combination of naturalism and anti-representationalism ended up being. We then considered two kinds of non-pluralist anti-representationalist: the anti-representationalist who is a monist about linguistic function, and the anti-representationalist who is a chauvinist about linguistic function. Both seem able to recover some version of the placement problem. In the penultimate section, we examined some reasons for thinking that representationalism offered better explanations of a number of features of Price's own account of linguistic practice, then Price's own anti-representationalism.

If any of these responses are successful, then we have done something to vindicate the kind of placement problems of which genus our own case of mathematical ontology is a species. But clearly some of these lines of argument are of more use for my purposes than others. In particular, it is difficult to see that the project of this thesis, defending a revolutionary, prop-oriented pretence theory of the mapping account of mathematical application, is greatly assisted by adopting a non-pluralist anti-representationalism. Both the varieties of this kind of view we considered seem to lead to a minimalist kind of metaphysics, in which we simply read off our ontological commitments from our favoured discourse. Such ontological minimalism sits uncomfortably with the approach taken in the rest of the thesis.

But the other arguments, that anti-representationalism is insufficiently naturalistic, and that representationalism offers better explanations of linguistic practice anyway, seem compatible with the fictionalist project outlined in the previous chapter. It would, of course, be nice to say a bit more about the kind of representationalism that might be adopted by a fictionalist about mathematics. In section 4.7 we drew on Hartry Field's (1972; 1978) early correspondence theory of truth, but it is not clear that a correspondence theory is the only one available to the fictionalist about mathematics. For instance, Leng (2010) combines fictionalism about mathematics with deflationism about truth, and it would be interesting to examine whether that combination of views was also resistant to Price's own extremist version of semantic minimalism. Unfortunately, that will have to await further work in the future.

For now, I want to suggest that Price has done too little to undermine the kinds of placement problem we have focused on in this thesis. At best, he has provided a pluralist alternative to representationalism, which awaits further empirical confirmation. If any of the objections raised in this chapter stick, he has not done even this much. I conclude that it is perfectly appropriate to defend anti-realism in the philosophy of mathematics (and perhaps elsewhere), without this leading to a global quietism about metaphysics.

In the next chapter we turn to metaontology, to see whether some recent metametaphysical arguments will establish a deflationary approach to metaphysics – and so threaten our fictionalist project – where Price's anti-representationalism could not.

Chapter 5. Fictionalism and Metaphilosophy, Part 2: Metaontology

5.1 Introduction

In this chapter we turn from the philosophy of representation to the methodology of metaphysics: what are the consequences for fictionalism of recent discussions in metaontology and metametaphysics? This will complete our defence of fictionalism against metaphilosophical objections. The first part of the chapter concerns itself with Stephen Yablo's (1998) attempts to motivate a deflationist metaontology using insights that will be familiar to the fictionalist. After dismissing Yablo's metaontological deflationism, I will turn to a sceptical suggestion of Karen Bennett (2009) and argue that, while fictionalism is a difference minimizing brand of metaphysics, it is unlikely to fall prey to similar objections to those that afflict the most odious brands of Platonism (in particular, there is a kind of epistemic challenge to Platonism that does not seem to carry over to fictionalism). I conclude that fictionalism seems well placed to defend itself against those forms of metaontological scepticism that seem, prima facie, the most threatening.

Before proceeding, it will be helpful to discuss why it is I have chosen to focus on these two kinds of metaontological scepticism. After all, there are many kinds of deflationary position in the metaontological literature, and it seems that a full defence of fictionalism from this direction would need to refute all of these or at least the most characteristic varieties of deflationism. To some extent this is a fair point, and a more systematic refutation of ontological deflationism would be welcome. Nonetheless, I believe there are two things we can say.

The first is to note that it is not clear that all forms of metaontological scepticism are meant to apply to the debate at issue in this thesis. For instance, as we will see below, Eli Hirsch, whose semantic variety of deflationism has been extremely influential, does not believe his position threatens the debate over mathematical ontology (Hirsch, 2011). On the other hand, it seems clear that the two positions discussed here would undermine the fictionalist's position, by undermining the very viability of the debate she is trying to contribute to.

And this leads into our second point, that these two varieties of metaontological scepticism seem especially threatening to the fictionalist. In the case of Yablo's deflationism it should be obvious why this is: because Yablo is drawing on the very same insights as the first-order fictionalist, it is initially difficult to see a way of resisting his argument that doesn't undermine the very fictionalism one is seeking to defend. In the case of Bennett's epistemicism, the way in which she identifies high-ontology and low-ontology sides in the debates that fall victim to her scepticism seems, prima facie, the perfect characterisation of the debate over mathematical ontology, with Platonists ranged with the high-ontologists, and fictionalists with the low.

Both these varieties of metaontological scepticism seem, then, to offer a very specific kind of challenge to the fictionalist position we have sought to defend in this thesis. If we can resist these kinds of scepticism, then this should strengthen fictionalism's claim as a theory of mathematical ontology and applications. In particular, it should make us confident that the fictionalist can resist those more general varieties of deflationism that seem to affect fictionalism no worse than any other branch of metaphysics.

5.2 Does Ontology Rest on a Mistake?

In his (1998) Stephen Yablo attempts to motivate a brand of metaontological deflationism that draws upon some of the central insights of fictionalism. In brief, the strategy outlined in *Does Ontology Rest on a Mistake?* is to call in question our ability to distinguish literal from non-literal uses of language in a way that makes it difficult to see how we could set about determining which of the existential commitments of our discourse we inherit as ontological commitments. The border between the literal and non-literal, he suggests, is simply too ill-defined, too porous, for us to determine in certain cases which of our utterances are intended literally and seriously, and which are only put forward in a make-believe spirit (Yablo, 1998, pp.257-258). This is, of course, consistent with there being large and settled regions of discourse, outside the vague border lands, where the question of literality and non-literality is perfectly determinate. It is just that the questions of interest to ontologists, the kinds of utterances about abstract objects and temporal parts and mereological sums and so on that he takes to be of central concern to metaphysics, are likely to fall outside these more stable regions, in the unsettled and contested marches between the literal and the figurative. And, supposing that we need not be committed to the posits of a merely make-believe utterance, this will make it indeterminate what our ontological commitments actually are.

5.2.1 Carnapian Deflationism

The starting point for Yablo's deflationism, as with so many others, is Carnap (1950). Carnap, he suggests gave us a theoretical framework in which to make good the suspicion, apparently so widely shared, that metaphysics is bunk (Yablo, 1998, pp.230-232). Many, it seems, feel there is just something wrong with questions like: are there temporal parts? When, if ever, do two things compose a third? Can a statue be located where its constitutive lump of clay is located? Do numbers, sets, functions and the like *actually* exist? These questions have something like a whiff of triviality about them, or else they strike one as malformed, uninteresting, or otherwise downright silly (Yablo, 1998, p.230). But a suspicion is not a theory, and without a theoretical framework in which to locate and explain our feeling that something is very wrong with metaphysics, it is hard to tell whether there is really anything of substance to the naysayers' naysaying. After all, impatience with philosophy and philosophers is a cross-cultural phenomenon and hardly limited to metaphysics and its practitioners. Perhaps what the anti-metaphysicians are feeling is just the near universal dissatisfaction felt when one is asked to think deeply and carefully about issues one would rather settle summarily and without cerebral travail.

It is this theoretical framework for making good anti-metaphysical sentiment that Carnap is supposed to have provided (Yablo, 1998, p.232). We have encountered Carnap's suggestions in previous chapters. The idea, as we saw in the previous chapter, is to distinguish between questions asked from within a linguistic framework and questions asked outside the framework. A linguistic framework is something like a set of rules that provide meaning for the sentences and constituents of sentences by specifying under what conditions a sentence is to be counted true, and under what conditions false (Carnap, 1950, pp.21-22). An internal question, that is a question asked within the linguistic framework, will be perfectly meaningful, the linguistic framework rules ensuring that there will be some procedure for settling it one way or the other. But questions like this will be of little philosophical interest. The framework will tell us what empirical results would settle the matter, if the framework is an empirical one (Carnap,1950, p.22). If the framework is a logical or mathematical one, then the issue will not be settled by collecting empirical evidence but by the kinds of logical manipulation licensed by the framework in which one is working (Carnap,1950, p.22). In either case, the philosopher, it seems, must simply accede to the judgements of the first order practitioner, the experimenter or the logician.

External questions will be another matter. Consider, for instance, the question “Are there material objects?” asked in the context of philosophical discussion over the so-called ‘thing language’ (Carnap, 1950, pp.22-23). There are three ways of reading this question. One might see it as a straight-forward internal question of the kind discussed in the previous paragraph. In that case, the question is perfectly meaningful (the linguistic rules of the thing language will provide a determinate procedure for settling it), but it is likely to be trivially analytic. Given the rules of the thing language, it is likely to follow from the linguistic rules alone that there are material objects. Alternatively, we might read the question in a way that makes it external to the linguistic framework (Carnap, 1950, p.22). In that case, being detached from the linguistic rules that give meaning to the constituent words (“material”, “object”, etc.), the question is cognitively empty, that is, it lacks meaning. But there are two ways to ask a cognitively empty external question. One might raise the question in a merely pragmatic spirit, that is, the question is in fact a request for the point of the thing language, an attempt to ascertain how advisable (given our aims) it is to go on speaking as if there are material objects (Carnap, 1950, p.23). On the other hand, we might ask the question in a theoretical spirit, that is, we might be wondering whether, quite aside from the linguistic framework for talking about things, there *really are* any things. The first kind of external question is harmless. The second is *strictly* meaningless, it is the attempt to ask a meaningful theoretical question without the theoretical framework that could confer meaning upon it (Carnap, 1950, pp.22-23).

Carnap's suggestion (Carnap, 1950, pp.31-32) is that the majority of metaphysical questions, the debate over the existence of universal properties, for instance, or numbers and sets, are *theoretical external questions,* that is, the wholly pernicious kind. They are attempts to raise the issue of the existence of some objects in a way external to the linguistic framework in which that question might have meaning. Were the questions asked in a pragmatic spirit, that is, were philosophers simply querying the advisability for scientific purposes of continuing to employ the linguistic framework in question, then they would be quite in order, though perhaps not terribly philosophically interesting. But this is not how philosophers have traditionally seen their metaphysical enquiries (Carnap, 1950, p.31). The traditional picture of metaphysics sees its questions as theoretical existence questions, but not as questions whose issue depends wholly on the relatively trivial matter of what the rules of our language are. As such, the traditional picture of metaphysics makes its questions cognitively empty external questions. Metaphysics is, for good logico-linguistic reasons, bunk.

But, of course, and as Yablo is well aware, Carnap's project could not work (Yablo,1998, pp.235-240). The idea of a linguistic framework, so the traditional history of analytic philosophy has it, is intimately bound up with the idea of the analytic-synthetic distinction. And the analytic-synthetic distinction, or at least any analytic-synthetic distinction sufficiently robust to underwrite Carnap's anti-metaphysical project, was decisively overturned by Quine's *Two Dogmas* (1951a). Yablo sees no reason to demur from the traditional verdict on the outcome of Carnap's tussle with Quine over the analytic-synthetic issue (Yablo, 1998, p.232). But, he suggests, the palm was too quickly awarded to Quine in the ontological contest, because the notion of a framework can be made useful without relying on the analytic-synthetic distinction (Yablo, 1998, p.240).

5.2.2 Linguistic Frameworks as Games of Make-Believe

The core of Yablo's deflationist proposal is that we can recover the concept of a linguistic framework, and so the distinction between internal and external questions, by seeing linguistic frameworks *not* as systems of rules giving meaning to the sentences of the language, but instead as sets of generation principles for Waltonian games of make-believe (this is the fictionalist element of Yablo's metaontology. Waltonian games of make-believe will of course be familiar from chapter 3's first order fictionalism, in which they functioned as a way of deflating existential consequence at the local level of mathematics or empirical science. Yablo is, in effect, deploying fictionalism as a way of deflating existential consequence across the board) (Yablo, 1998, pp.242-244 and pp.245-248). The rules of generation tell us what we are licensed to pretend is true when we engage in the game of make-believe generated by those rules. An internal question will now be a question asked within the game, that is, a question asked in a make-believe spirit. An external question becomes a question asked from outside the game generated by the rules of the linguistic framework, a question about whether the make-believe or fictional truths of the game are *really* true.

Now it is important to note that the new concept of a linguistic framework does not make external questions cognitively empty, nor does it relegate external questions to a merely pragmatic significance. External questions can be perfectly meaningful questions about the existence or otherwise of the posits introduced in a merely make-believe spirit within the linguistic framework. The problem for the project of settling our ontological commitments is that games of make-believe are to a certain extent autonomous of the external questions. Consider, for instance, the game of make-believe generated by the linguistic framework of Melville's Moby Dick. And consider an utterance of "Call me Ishmael". Now we are faced with a question: Is there anyone whom we are to call Ishmael? As an internal question this is settled positively (and immediately and trivially) by the rules of generation. So within the linguistic framework, it is perfectly proper to say "Yes. There is someone whom we are to call Ishmael." But we can also ask the question in an external spirit, and in that case the appropriate answer (ignoring for now disputes in the metaphysics of fictions) seems to be: "No. There is no-one whom we are to call Ishmael." But now note that our answer to the external question does not in any way affect the legitimacy of our continuing to talk, within the Moby Dick framework, as though there was someone called Ishmael. Our internal practices are unresponsive to our external deliberations. We do not abandon a make-believe framework just because its posits do not exist.

But nor, obviously enough, do we inherit its posits. The whole point of the make-believe approach to fictionalism is that we can continue to talk as if there are Xs (within the game of make-believe), even if there are no Xs. Not even Quine thinks that we are ontologically committed to things we only figuratively or fictionally say there are (cf. Quine, 1948). So it seems there will be cases in which we continue to talk as if there are some things (internal to a framework in Yablo's sense) even though we do not think there are any such things (external to the framework in Yablo's sense).

None of this immediately makes trouble for the ontologist. So long as we can continue to distinguish between posits introduced within a make-believe framework and posits introduced external to any make-believe, then we can continue to pursue the Quinean ontological project. But this is precisely what Yablo goes on to suggest we cannot do (Yablo, 1998, pp.248-255). For how are we to identify *when* it is we are talking in full ontological seriousness? When, says Quine, we are doing science. We are ontologically committed to the posits of science because it is there, and there alone that we talk in full literalness about what there is (indeed, I think Quine would concede to Yablo the porousness of the literal and non-literal distinction in our ordinary discourse. This is precisely why the man on the street has no fenced in ontology). But this claim would seem to be in danger of straight-forward empirical refutation. For, as we have noticed previously in this thesis, metaphor (a familiar example of figurative or non-literal language) seems to play an indispensable role in scientific discourse. Scientific discourse is not literal through and through; it is shot through with make-believe.

What is Quine to do about this? He needs to isolate some literal uses of language, if we are to have any ontological commitments at all (given that we do not inherit the commitments of our non-literal language games). But his chosen candidate, regimented best science, seems at present to be up to its ears in figurative language; in frictionless planes, in point masses, in thermodynamic limits, and so on. Here is one suggestion: best science at present is a work in progress. As the work progresses, the non-literal elements will drop out; we will clear the tropical jungle of its tropes, and in the long run, perhaps the very long run, science will be a completely literal enterprise. But Quine is in trouble here if the non-literal elements never do drop out, if metaphor turns out to be indispensable or ineliminable. And Yablo suggests this may well be the case (we have already seen reasons to agree, but it will be helpful to rehearse his arguments) (Yablo, 1998, pp.248-258).

5.2.3 Three Grades of Metaphorical Involvement

Yablo goes on to identify three grades of metaphorical involvement, each of which makes it progressively more likely that the metaphors at that grade will be ineliminable (Yablo, 1998, pp.250-255). At the first grade of metaphorical involvement are the representationally essential metaphors. These are metaphors whose content cannot be expressed by any strictly literal paraphrase, such that the only way to say what one wants to say is to employ the metaphor in question. For instance, "There are butterflies in my stomach." We all know what content is being expressed by utterances of that sentence, but it is doubtful whether there is any reductive paraphrase, employing only literally descriptive language that captures exactly what it is that the metaphor conveys (Yablo, 1998, pp.250-251).

At the second level of metaphorical involvement are the presentationally essential metaphors (Yablo, 1998, pp.251-253). These are metaphors whose content could perhaps be expressed by some entirely literal paraphrase, but where the metaphorical presentation is doing more work for us than just expressing some content. It is the way that the metaphor presents its content that is important in this case, the way in which the metaphor encourages us to see something as something else. The example given by Yablo is that of calling someone "A wagging lapdog of privilege." (Yablo, 1998, p.252) I do not just want to convey, by employing that unflattering epithet, that someone has a number of doglike properties, a fawning obsequiousness, perhaps, or a foolishly naïve and misplaced loyalty. It is not just by failing to go over into a paraphrase in which only the relevant properties are invoked that the metaphor can be essential. What I want is for you to see the victim of my insult *as* a fawning lapdog; I want to call to your attention the ways in which one might see someone as a dog; I want to reframe your view of the individual in such a way as to bring out his essential dogginess. I could not do any of that without employing the metaphor I did employ (or else another very like it).

The third level of metaphorical involvement makes the most difficulty for separating out the literal and non-literal aspects of our discourse. At this level we find the procedurally essential metaphors (Yablo, 1998, pp.253-255). These are metaphors that may not have *any* determinate metaphorical content at the time of utterance; or that may have several contents competing to be seen as the correct reading of the metaphor; or that, still worse, may have several different contents associated with them, but not in competition, all equally good readings of the metaphor. These are the metaphors put forward in a *make-the-most-of-it* spirit (Yablo, 1998, p.254), metaphors that are introduced with the aim that the reader or hearer of the metaphor will themselves read into the metaphor whatever available content there is. It is these metaphors that make the most havoc with our ordinary judgements of literality and non-literality, because one of the readings that may be up for grabs when the metaphor is introduced is the literal content! (Yablo, 1998, p.257) The idea here is that the metaphor is put forward with the intention that it is to be associated with the most suitable metaphorical content, if any and with its literal content if no figurative content is forthcoming.

Quine is in trouble, so Yablo contends (Yablo, 1998, p.255), if any of these grades of metaphorical involvement turns up in our best science. If there are essential metaphors at any of these grades of involvement in our best scientific theories, then even in the longest of long runs there is no hope of arriving at a science completely purged of ontologically innocent make-believe. But it is the final grade of metaphorical involvement, the procedurally essential metaphors, that cause the most difficulty for Quine, because it is these metaphors, put forward in a make-the-most-of-it spirit that blur the boundary between the literal and the non-literal (Yablo, 1998, p.257). Because one of the readings that is up for grabs in the case of a procedurally essential metaphor is its literal content, and because the business of settling on a correct reading for a metaphor is a matter of art and, to some extent, individual taste, we may not be able, at any point in the development of best science, be able to say whether some disputed sentence is non-literal or literal, a metaphor or the God honest truth. It is the procedurally essential metaphors, then, that threaten the entire project of distinguishing the literal and the non-literal, which is precisely what Quine needs if his project of determining our ontological commitments is ever to be accomplished (Yablo, 1998, p.258).

5.2.4 In Defence of Quinean Metaontology

Now clearly this is a disaster for the fictionalist: it turns out that the very ideas on which fictionalism is built, make-believe and pretence and talking as-if there are things when there are not, undermine the very ontological project to which it is supposed to be a contribution. After all, given the intractability of ontological disputes, isn't it likely that they will turn out to have been taking place within the vague and amorphous region of discourse introduced by the procedurally essential metaphors? Isn't it just possible that the problem with the existence or otherwise of numbers, sets, functions, and the like is just that we cannot be sure we were talking literally when we uttered them?

Fortunately, I think we can say a number of things in response to this challenge. The first is to note that Yablo is here discussing a kind of hermeneutic fictionalism (which I will henceforth, following Yablo, call figuralism (Yablo, 2001)). He is suggesting that some of our ontological utterances may well have been put forward in a kind of make-believe game, a make-believe game generated by the utterance of certain kinds of figurative language. And this brand of hermeneutic figuralism inherits the same challenges in the mathematical case as standard hermeneutic fictionalism did (cf. ch4.) Of course, given the arguments from chapter 3, we cannot claim that hermeneutic figuralism is implausible on the grounds that it attributes attitudes or linguistic intentions to speakers that they know they do not have (we cannot object: wouldn't people just *know* if they were putting forward existence claims in a make-the-most-of-it spirit?) Nonetheless, the worries we had about hermeneutic mathematical fictionalism's empirical openness, its susceptibility to challenges from future scientific discoveries, will carry over to the case of Yablo's figuralism as well. Should it turn out, for instance, that future psychological research suggests that some autistic people cannot competently handle certain kinds of metaphor, but that they show no mathematical impairment, that would tend to suggest that Yablo's figuralism just cannot be a correct account of our use of mathematics. Clearly this is not a knock-down argument against figuralism, but it should make us suspicious that it might not be the correct interpretative theory for mathematics.

In this connection, it will be noted that I give no examples of procedural metaphors in either science or ordinary language. This is because I cannot think of any. I am in fact deeply suspicious of this entire category of metaphor, and wonder whether we ever put forward an utterance without first deciding whether we mean it metaphorically or are actually asserting the literal content of our utterance. If there are no metaphors of this category, then that would further undermine the interpretative adequacy of Yablo's figuralism.

My second pair of responses begins by noting that Quinean ontology is not the ontology of ordinary discourse (Quine, 1948). As I have already remarked, the ordinary man on the street may well have no fenced in ontology and may not be able to distinguish between his literal and non-literal utterances. Quinean ontology is the ontology of best science, it is the ontology we find ourselves with when we have completed the project of regimenting our best scientific theories in the canonical notation of first order logic with identity. Now it seems to me that science just *is* more literal than ordinary language. Yes, Yablo is quite correct to point out that there are, nonetheless, substantial portions of non-literal discourse embedded within scientific discourse, but these regions will surely be smaller in extent than the non-literal regions of everyday discourse. It hardly seems likely that there will be huge areas of science where the metaphors roam wild, sufficient to underwrite a general scepticism about our existence claims. If there are areas of essential metaphor in scientific discourse, they are likely to be small regions, easily contained within the larger body of completely literal, completely serious science.

And this brings me to my second objection. Quine does not need to eliminate metaphor from science, he merely needs to isolate it. What the Quinean ontological project needs is some way to identify and ignore the metaphorical portions of discourse, so that we do not think that we are committed to some things that, in fact, we only pretend that there are. And the reason for switching to scientific discourse was not, surely, that it is completely literal, but because it is considerably more literal than everyday chatter. We turn to science to settle our ontological commitments, because, while there are still essential (that is, representationally essential and presentationally essential) metaphors within scientific discourse, there are fewer of them and we have a pretty good idea of which ones they are. We can be confident that we are not committed to anything we quantify over in these regions of our scientific discourse (and here we might use some kind of theory of prop-oriented make-believe to explain what it is we are actually doing in science with our non-literal utterances). I cannot see, then, that Yablo's first two grades of metaphorical involvement need greatly trouble the Quinean ontologist, so long as we have some way of isolating and discounting them from our ontological reckoning.

It is, then, as Yablo notes (Yablo, 1998, p.257), the final grade of metaphorical involvement that causes all the problems. As I have just argued, Quine will have no difficulties with essential metaphor, so long as he can distinguish the essential metaphors from the literal parts of scientific discourse. And there is no reason, in the case of presentationally and representationally essential metaphor, to think that this cannot be done. But procedurally essential metaphor threatens to evade this response altogether. What we need, in order to do Quinean ontology, is to separate out and ignore our merely make-believe commitments when trying to establish what there is. But if some of our existential commitments are embedded in games of make-believe constitutive of make-the-most-of-it metaphor, this cannot be done. And it cannot be done, because one of the readings of the metaphor that is potentially up for grabs is its literal reading; one way to legitimately engage in this kind of game of make-believe is not to make-believe at all, but just to believe. The problem, then, is that this kind of metaphor, with all its commitments, threatens to go literal and to become a piece of serious, ontologically loaded discourse – and there is no way of knowing, at any given time, if that might not be the correct way to receive the metaphorical utterance.

This would indeed be a problem for the Quinean ontologist. It would blur the boundaries of the literal and non-literal in such a way that the project of settling our ontological commitments would lapse. It would be a disaster for a great deal of late twentieth and early twenty-first century metaphysics. I say "would". It *would* be a disaster, but then it would only be a disaster if this kind of metaphor played any role in scientific theorizing. And I see absolutely no reason to think that it does. Indeed, I can think of very good reasons to say that it doesn't. Science is, among other things, an enterprise of prediction and explanation. But surely generating predictions and giving explanations requires our utterances and inscriptions, the utterances and inscriptions that are to play some role in those explanations and predictions, having some kind of determinate content. But that is what procedural metaphors lack, at the time of utterance/inscription, and possibly at all times until and including the completion of science! A metaphor that expresses some determinate content, but does so in such a way that only the metaphor can express that content – I can imagine metaphors like that playing a role in generating predictions and giving explanations. A metaphor that hovers indifferently between many contents, that even encompasses the possibility that it expresses no content whatsoever – such metaphors *may* be living in ordinary discourse, but they are surely dead to science.

An example. According to the DN model of scientific explanation (and it goes without saying that this is not an endorsement of the covering law model, but a sketch of a potential family of problems for procedural metaphor) a scientific explanation is a deduction at least one of whose premises states a natural law (Hempel and Oppenheim, 1948). But it follows from this that procedural metaphor cannot play a role in explanation, because contentless utterances or inscriptions (which are not candidates for truth or falsity) cannot play any role in deduction. It seems to me that a sentence can only be part of a deduction if it expresses some determinate content. Otherwise, not being a candidate for determinate truth or falsity, how can it play any role in a truth-preserving argument? Well, so much the worse for the DN model, right? But the example is intended to be perfectly generalizable. It is difficult to imagine any theory of explanation in which sentences expressing no determinate proposition could play a role.

I submit, then, that procedurally essential metaphor plays no role in scientific theorising. And as Quinean ontology is the ontology of our best scientific theory, I propose that procedurally essential metaphor is no threat to the project of settling our ontological commitments. Nor, for reasons already outlined, are the other kinds of essential metaphor obviously a threat to the ontological project. It is important to stress that this is not a defence of everything that passes for metaphysics in contemporary philosophy (indeed, throughout this chapter my concern will be to establish the substantiality of the dispute over mathematical Platonism and fictionalism. I will not be concerned at any point with defending metaphysics more broadly). I think that, the use of presentationally essential and representationally essential metaphor notwithstanding, it is perfectly intelligible to ask whether best science requires an ontology of abstract objects and theoretical entities or whether we can do without one or either. Where it leaves the debates over temporal parts or statues and lumps of clay I do not presume to judge.

5.2.5 Concluding Remarks

We have seen reason in this section to doubt that Yablo's project can succeed: at least in the way he has chosen to do it, there is no clear route from first-order ontological fictionalism, to a second order metaontological fictionalism with deflationary consequences. As we have seen, Quinean metaontology is not the ontology of ordinary language, it is the ontology of our best scientific theories, in which we are usually confident which parts of our theory are accurately representing the world, and which parts are meant non-literally. As far as concerns representationally and presentationally essential metaphor, therefore, we can be confident that we can isolate and discount the non-literal portions of our theories when doing Quinean ontology (of course, we will need to give some kind of account of what it is we are really doing with our non-literal utterances, and what we are really committed to). And we saw good reason to think that procedurally essential metaphor, if it even exists, can play no role in our scientific theorizing. As it was this kind of metaphor that blurred the boundaries between literal and non-literal, I submit that the Quinean metaontologist is perfectly entitled to a robust demarcation of the literal and non-literal. I therefore conclude that the kind of fictionalism defended in this thesis is not under threat from the brand of global fictionalist deflationism Yablo envisages. We are entitled to be both Quineans in metaontology, and fictionalists about mathematical ontology.

5.3 Difference Minimization and Epistemicism

In this section we consider an alternative way in which the deflationist intuition might be fleshed out, Karen Bennett's *epistemicism* (Bennett, 2009). According to the epistemicist, the problem with ontology is not that its questions have no correct answers, as some have suggested, but that we lack justification for believing one of the answers rather than another to be the correct one. This lack of justification, Bennett argues, can be traced to a strategy pursued by most *low-ontologists* (those with a preference for parsimony and desert landscapes) of 'up-playing' their expressive resources in order to minimize the difference between their own view and more inflationary ontological positions. But by placing themselves in a position to ape everything the high-ontologist says, the low-ontologist runs the risk of inheriting the very problems with less parsimonious metaphysical theories that motivated her to take the eliminativist route in the first place. The consequence of this difference minimization strategy is that the distance between the competing philosophical positions becomes so small that we have no way of choosing between them, and so no justification for believing one rather than the other (Bennett, 2009). Obviously this is of particular concern for the mathematical fictionalist, because her position is low ontology (it does without the abstract objects a Platonist is happy to countenance) *and* difference minimizing (both the revolutionary and the hermeneutic fictionalist want to preserve as much of mathematical practice as they can, in particular, they do not want to be outdone in this respect by Platonism).

5.3.1 Varieties of Metaontological Dismissivism

Bennett begins the presentation of her deflationary position by outlining three possible ways in which the deflationary intuition (what she calls 'dismissivism') might be made more concrete (Bennett, 2009, pp.39-42). According to the first, *anti-realist* dismissivist, the problem with ontological claims is that they lack a truth-value. So for instance, a claim of the form "There are Fs" is neither true nor false (Bennett, 2009, pp.39-40). Bennett doesn't really discuss this position in any detail, and nor will I. Indeed, it is not clear at this stage that there really is a position on the table. This is not to say that the suggestion that ontological claims lack truth value is obviously false or that its assertion is incoherent. But as things stand, anti-realism seems underspecified. *Why* do ontological statements lack a truth-value? The mere brute assertion that they do does not really amount to a philosophical theory. Until the anti-realist position can be sharpened, it does not seem any more philosophically substantial than the anti-metaphysical prejudice it seeks to flesh out.

A second kind of dismissivism is *semanticism*, the view that metaphysical disputes are trivial because they are merely verbal or because the disputants are simply talking past one another (Bennett, 2009, p.40). There are many ways in which this idea might be made more concrete, but perhaps the most familiar is Eli Hirsch's *quantifier variance* theory (Hirsch, 2011), according to which the disputants are talking past one another because they mean different things (or might be charitably interpreted as meaning different things) by their respective utterances of quantificational phrases like "there is", "there are", "there exists", etc. According to a dismissivist like Hirsch, there is no fact of the matter about who is right in an ontological dispute over the existence of Fs, because both parties to the dispute can be seen as talking their own language with its own meanings for the quantifier phrases, and because principles of interpretative charity demand that we interpret both parties to the dispute, both the F deniers and the F partisans, as speaking truly within their own language (Hirsch, 2002, pp.67-71). According to Hirsch, the only substantive issue in the area is that of whose language or whose use of the quantifier phrases best accords with ordinary English. But this is a matter to be settled, not by metaphysicians, but by philosophers of language (Hirsch, 2002, pp.64-71).

Now again, I will not say much about semanticism here. Partly, this is because I struggle to make sense of the idea of two languages sufficient to describe all the facts, but differing over the meaning of the quantifier terms. In particular, I find it obscure why some pieces of language should be called quantifiers if they differ in meaning. How can they *both* be existential quantifiers, if they do not both mean the same thing? Hirsch's suggestion is that we see the two phrases as sufficiently syntactically similar to license calling them quantifiers (Hirsch, 2011, p.xiv), but the collapse argument seems to threaten that if two words share the same syntactic profile (license the same inferences), then they will mean the same thing. There is a worry here that Hirsch's position is just the familiar point that we can imagine two orthographically identical words, and so two orthographically identical sentences containing those words, that nonetheless have completely different meanings. But no-one would find it ontologically significant that I can truly say "John is at the bank", and you can truly say "John is not at the bank", and neither of us need be disagreeing with the other because I meant that John is standing by the river and you meant that John is at the place where the money is routinely mishandled. The fact that two people may assign wildly different meanings to the word ‘exists’ is only ontologically significant, if it has already been conceded that there *is no correct or best meaning for that word* (cf. (Sider, 2011)). But that is surely what was at issue.

However that may be, Bennett says much more about semanticism than I have here, and so if you do not find my worries about the significance of the quantifier variance view satisfying, I direct you to her 2009. The gist of Bennett’s objection to semanticism is that its interpretative strategy requires an analytic equivalence between the sentence up for dispute and its meaning in the two respective languages of the disputants (Bennett, 2009, pp.50-57). But, Bennett argues (Bennett, 2009, pp.54-57), there is no reason to think that these analytic equivalences will hold in the respective metaphysicians’ languages. I think we can say more. If Hirsch’s view, and those like it, really do require analytic equivalences of this kind, then it is hard to see how they move us beyond the Carnapian deflationism from which they take their cue. If we are happy to concede that Quine won the debate with Carnap over the analytic synthetic distinction, then it is difficult to see how any view which takes on a commitment to this distinction can be advance over Carnap’s linguistic frameworks.[14]

The final version of dismissivism is the one that will concern us for the rest of this chapter. This is epistemicism. According to the epistemicist it may well be that an ontological claim such as “There are Fs” is true or false, and that the dispute over its truth or falsity is not merely a verbal dispute, but that, nonetheless, the dispute is still trivial (Bennett, 2009, p.42). And the dispute might still be trivial because we lack justification for believing one way or the other, that is, we lack justification for thinking that “There are Fs” is true rather than false, or false rather than true (Bennett, 2009, p.42). And if metaphysical disputes are like that, then clearly it is not worthwhile to engage in them, any more than it would be to engage in the dispute about how many grains of sand there are on Blackpool beach or how many quarks there are in the universe. Presumably questions like these are intelligible, and have

[14] It is also worth noting that at one point Hirsch seems to deny that his notion of a dispute’s being verbal will apply to the debate between the nominalist and the Platonist (Hirsch, 2011, p.192). That is, he sees no way in which the nominalist community (speaking a nominalist language) could charitably interpret Platonistic utterances in a way that makes them come out true.

answers with determinate truth values. But because we lack justification for believing one way rather than the other, dispute over these questions would be pointless. Metaphysical disputes, Bennett suggests (Bennett, 2009, p.42), are a little like that, intelligible questions with determinate answers, which we will never get the right answer to (at least if we stay at the level of first-order metaphysics).

5.3.2 Epistemicism and the Special Composition Question

Why might metaphysical disputes be trivial in the way the epistemicist envisages? It will help if we have a concrete example before us, and so, following Bennett (2009, pp.44-45), let us consider the dispute over the so called special composition question: When, if ever, do two things compose a third? Mereological believers (which group includes the mereological universalists) say sometimes, and possibly always, depending on which principles of composition they adopt. Mereological nihilists, by constrast, say never: two things never compose a third; there are only simples.

Here we have what Bennett calls a low ontology side, and a high ontology side (Bennett, 2009, p.46) (it is unclear to me whether this is a general feature of disputes threatened by epistemicism or just a feature of the kinds of examples Bennett considers. I see no necessary connection between difference minimizing metaphysics and ontology, and it is difference minimization that threatens to render metaphysical disputes trivial. Nonetheless, fictionalism fits the pattern Bennett identifies, and so we need not worry overmuch about what conditions a dispute needs to meet in order to be a candidate for epistemic deflation). The high ontology side are the believers, those who think there are tables, chairs, people, pigs, possibly simples, and possibly arbitrary mereological fusions as well. The low ontology side in this dispute are the nihilists, who countenance only simples.

As Bennett sees it, this division into high and low ontology sides is common to many different metaphysical disputes (Bennett, 2009, p.46). And clearly the distinction applies in the case of the dispute over mathematical ontology, with the nominalists of various stripes occupying the low ontological side, and the Platonists with their infinities of abstracta defiantly occupying the ontological high ground.

But both the high ontologists and low will, Bennett argues (Bennett, 2009, p.46), seek to minimize the distance between their respective theories. This is because neither side wishes to look crazy. Consider, for instance the mereological universalist. According to the mereological universalist, every arbitrary fusion of simples and non-simples is itself an object. So not only are atoms objects, so are chairs and ducks. And not only are chairs and ducks objects, but so are chairducks, where chairducks are objects composed of ducks and chairs. But there is a risk here, a risk that by endorsing such an expansive ontology one opens oneself up to the charge of ontological profligacy. So the high ontologist in this dispute will try to *downplay* her ontological commitments. She will say things like: “But really, there is *nothing more* to there being chairs and ducks than there being simples arranged tablewise and duckwise. And there is nothing more to there being chairducks than there being simples arranged chairduckwise. All it takes for an object to exist *is* for some simples to be arranged objectwise. Your mistake, my nihilist friend, is to suppose that the conditions on objecthood are more stringent than this.”

Now, clearly, if this kind of downplaying strategy can be made to work, it lessens the distance between the low and the high ontologist, but it is the other direction of difference minimization that Bennett sees as causing trouble for metaphysics (Bennet, 2009, pp.62-65). The low ontologist also wants to minimize the distance between herself and the high ontologist, because the low ontologist wants to be able to capture as much of our everyday discourse as possible within her ontologically austere framework. Consider the mereological nihilist this time. The nihilist denies the existence of all non-simple objects, but she does not want to condemn all of our ordinary speech about composites as useless falsehoods. Here the nihilist has two options to *up-play* her expressive resources and recover as much of everyday discourse as she can, both of which will be familiar from our earlier discussion of fictionalism: she can endorse a hermeneutic nihilism or a revolutionary nihilism. First the hermeneuticist: “Strictly speaking, there are no composite objects. But when, in the course of everyday chatter, we say things like ‘here is a table’, *we are not speaking strictly*! ‘Here is a table’, uttered in any common or garden conversational context is just a figurative or colourful way of saying ‘here are some simples arranged tablewise.’” The revolutionary nihilist, on the other hand, will say something like this: “Speaking strictly, there are no tables. So ordinary utterances of ‘here is a table’ are false. Nonetheless, ‘here is a table’ is suitably related to its literally true paraphrase ‘here are some simples arranged tablewise’, so that we – we philosophers, that is – know that we can always switch to the paraphrase whenever strict and literal speech is required, say, in the ontology room. And meanwhile, we can let the folk get on with their business unmolested, given that their strictly false utterances, by being suitably related to true ones, are perfectly innocuous.”

Now the danger here is that by up-playing her expressive resources in this way, the nihislist has moved her position so close to that of the high ontologist that there will be little to choose between them. By aiming to recover all those portions of our everyday discourse that the high ontologist is able to account for, the low ontologist risks collapsing the distinction between the two positions (Bennett, 2009, p.64) (it seems to me that the high ontologist’s strategy will have similar consequences. By downplaying the ontological weightiness of her commitments, the high ontologist looks to have available a line of response that will go something like this: “If your objections to my ontological profligacy stick, then they will be as much of a problem for you as for me. After all, there is no more to my ontology than…” So while Bennett focusses on the expressive up-playing route to epistemicism, it seems to me the ontological downplaying will take us in the same direction).

In order to make good on this suggestion, Bennett runs through a number of familiar objections to the high ontology side of the special composition issue (Bennett, 2009, pp.65-71). In each case she finds an analogous objection that can be raised against the low ontologist who has engaged in the kind of difference minimization (via expressive up-playing) we are considering here. Now we need not rehearse all these objections, but let us briefly consider one, in order to better appreciate how Bennett’s strategy proceeds. Let us take as our example the problem of the many (Bennett, 2009, pp.66-67). The mereological believer says “here is a table”. We point out that here is also an object containing all of the atoms of the table, minus one. But an object lacking only one atom possessed by a table would seem to meet every condition the mereological believer can offer for composing a table. So it too is a table. And so on for all of the table’s outlying atoms. We can imagine subtracting one at a time from the original table, giving us each time a new object that also seems to be a table. But then, where we thought there was only one table, there will be billions of tables.

The mereological nihilist might think that it is one advantage of her theory that she is able to avoid this unwelcome counterintuitive consequence. That is, because there are no tables located where we thought there was just one table, there are certainly not *many* tables where we thought there was just one. But things are not quite so clear cut as this suggests. Note that the difference minimizing nihilist thinks that when we say "here is a table", while there is in fact no table, there are simples arranged tablewise. But now we can simply run the problem of the many against these pluralities of simples. Subtract one atom from some simples arranged tablewise, and it would seem that you are still left with some simples arranged tablewise. So there are at least two groups of simples arranged tablewise. In fact, there are as many as there were tables in the original objection, because we can simply subtract one simple at a time from the original group of simples arranged tablewise to get another group of simples arranged tablewise. There are, then, billions of simples arranged tablewise where we were initially inclined to say "here is a (one) table". Because the difference minimizing nihilist wants to up-play her expressive resources to the point that "here is a table" is a legitimate thing to say, she seems to inherit the problem of the many in just the form that it afflicted the believer (Bennett, 2009, pp.66-67).[15]

Now, because the difference minimizing nihilist tries to recover all that the mereological believer says with respect to our ordinary quotidian speech about composites, she inherits all the objections that are traditionally urged against the believer (or so Bennett argues. For more details see her (2009)). But then we are left with no way to choose between the mereological nihilist's and the mereological believer's positions. If the objections to mereological belief stick, then they stick equally to expressively up-played mereological nihilism. If they don't stick, then they don't stick to either. Either way, we seem to have equal justification for both views, and so no justification for choosing between them. In that case, the dispute

[15] I do not want to suggest that one should find Bennett's reasoning convincing. This is meant only as an indication of her general strategy.

seems completely nugatory, no better, that is, than the idle dispute about how many grains of sand are on the beach: there is an answer, but we have no reason to think we know or will ever know what it is.

Now this is a fairly pessimistic conclusion to reach, but it is important to note that Bennett is not defending a kind of quietism about the debates in question. We cannot settle the dispute between the difference minimizing high and low ontologists, she suggests, by the usual first-order squabbling characteristic of analytic metaphysics. But it might be that the case before us is something like a case of underdetermination of theory by evidence, familiar from philosophy of science. Here the evidence that underdetermines the theories is the usual philosophical argumentation of first-order metaphysics. But perhaps, following the analogy with science, we can find some way to settle the issue by turning to the consideration of theoretical virtues (Bennett, 2009, pp.73-74). Maybe if we had a better idea of the relevant weightings of simplicity criteria, for instance, or fecundity, or elegance for metaphysical theories, then that would enable us to settle the issue in favour of the simpler, the more fecund, or the more elegant theory. Maybe the low ontologist out scores the high ontologist on ontological parsimony, for instance. And maybe idealogical parsimony is of less importance than ontological parsimony, or the high ontologist's promised ideological savings fail to materialize. In that case, we might have a route to deciding between the competing positions. In short, what Bennett suggests is that faced with an epistemicist impasse, we should switch to methodological considerations (Bennett, 2009, pp.73-74). We need to stop doing metaphysics, and start doing metaphilosophy.

5.3.3 Epistemicism and Fictionalism

Now I am fairly sympathetic to the above line of thinking, but before we throw in the towel, I think we should ask ourselves whether Bennett's epistemicism applies in the case of fictionalism about mathematics. On the face of it, both revolutionary and hermeneutic fictionalism would seem to be perfect candidates for Bennett's deflationism. The fictionalist is clearly on the low ontology side of the dispute and on the whole she is a difference minimizer: the mathematical fictionalist is usually unwilling to throw out the mathematical baby with the ontological bath water, she is happy to leave mathematical practice as it comes. How this is done will depend on the brand of fictionalism endorsed, whether hermeneutic (mathematical practice is already in good order. Strictly speaking, "2+2=4" is false, but, then, we were never speaking strictly) or revolutionary ("2+2=4" is strictly speaking false, but it is related to the true "In the mathematical fiction, 2+2=4" in such a way that mathematical practice need not be tampered with). But both the revolutionary and the hermeneutic fictionalist are keen to recover just as much of our everyday mathematical practice as the Platonist, and this leaves them open to the charge that they have moved their position so close to the Platonist's, there will be little reason to choose between them.

In order to make good this epistemicist objection, though, we would have to establish that every objection to mathematical Platonism will carry over to the expressively up-played nominalism we call fictionalism. By contrast, if we find even one objection to Platonism that does not carry over to the case of fictionalism, then it seems that Bennett's epistemicism cannot apply in the case of the debate over mathematical ontology. (In Bennett's defence at this point, we should note that she does not explicitly address the issue of the scope of her epistemicist deflationism, and so does not explicitly claim that issues of mathematical ontology are epistemically undecidable. Nor, it should be further noted, would the fact that epistemicism does not apply to the debate over mathematical ontology show that it doesn't apply anywhere else)

By far the most common objections to Platonism are epistemic ones. Let us then take, as our example of an objection to Platonism, Field's challenge (Field, 1989, 25-30), that is, the challenge to say how the existence of abstract, acausal entities located outside of spacetime is compatible with *any* plausibly naturalistic theory of knowledge. Should we discover that this afflicts both Platonism and expressively up-played fictionalism, then Bennett's epistemicism will have been partially vindicated (we would then have to complete the task for every other objection to Platonism). Should it turn out to afflict one and not the other, then epistemicism, in this case, will have been refuted.

To begin with, is Field's challenge a good one? Well, recall from the introduction that should the indispensability argument succeed, the Platonist would have a kind of response to this line of argument. She could say that we know about abstract, acausal entities located outside of spacetime in the same way that we know about theoretical entities: by their presence in our best well-confirmed theories of natural phenomena. In other words, if there is no plausibly naturalistic theory of knowledge for mathematical entities, then there will be no plausibly naturalistic theory of knowledge for whole swathes of our scientific knowledge of reality. But the indispensability argument, as we have repeatedly seen, is *not* successful. If we are correct about the status of the indispensability argument, then Field's challenge is still live. The Platonist still owes us an explanation of *how* we can come to know anything about the kinds of entities she takes the truth of mathematics to require.

Will anything like Field's epistemic challenge re-arise for the expressively up-playing fictionalist? Well, on the face of it, it is difficult to see how. Of course, there are all sorts of interesting philosophical issues surrounding the epistemology of fiction. "How do we (or can we) learn from fictions?" is one of particular concern for those of us who think that applied mathematics might be a sophisticated form of make-believe. But on the surface of it, there is little reason to think that answers to these questions will require the existence of a transfinite realm of acausal entities or a Platonic heaven. And if that is the case, there seems to be little reason to think that anything like Field's challenge will trouble the fictionalist. Of course, we may not have a fully worked out theory of knowledge for fictions, but the point is that there is no reason to suspect that our having one will require us to go beyond naturalism. In the case of Platonism, at least as traditionally conceived, it is difficult to see how we could have a theory of knowledge that did not go beyond naturalism.

There appears, then, to be a disanalogy between fictionalism and Platonism as traditionally conceived that seems to leave the epistemic balance in favour of fictionalism. Platonism is subject to an epistemic objection that simply does not seem to arise for fictionalism, even though the fictionalist can recover every part of our quotidian mathematical practice that the Platonist can. Putting this another way: fictionalism is difference minimizing, but only up to a point. The fictionalist wants to minimize the difference between herself and the Platonist with respect to mathematical practice, but with respect to mathematical ontology and epistemology she wishes to leave as much room between the views as is consistent with her difference minimizing aim. The difference here between the mathematical fictionalist and the mereological nihilist is that there is no way for the mereological nihilist to capture everything that we ordinarily say that does not close the gap with the mereological believer. Or in other words, there is no way to leave room between nihilism and mereological belief consistent with the difference minimizing aim. And here we might note the way in which the mereological believer downplays her ontology: when both parties to a dispute are moving towards one another, the distance is closed twice as fast.

In the case of mathematical ontology, the high ontologist, that is, the Platonist is willing to endorse the existence of a whole panoply of abstracta, and usually feels no obligation to suggest that in doing so she is really doing no more than ordinary folk do when talking of ordinary objects.[16] This means that the typical Platonist is just not very interested in downplaying her ontological commitments in a way that would move her position closer to the fictionalist. Meanwhile, of course, the fictionalist *is* concerned with reducing her ontological commitments, and so is concerned to position her theory, in this respect, as far from the Platonist as possible. It is this dual dialectic movement in the case of mathematical ontology, with the Platonist moving away from the fictionalist in the direction of greater ontological profligacy, and the fictionalist moving away from the Platonist in the direction of greater ontological parsimony, that leaves open the space between the two theories within which something like Field's challenge can decide against one in favour of the other. And it is this feature of the mathematical ontology debate, so different from Bennett's characterization of the debate between the mereological believer and mereological nihilist, that makes the former debate resistant to her epistemicism, whereas the latter

[16] Not all forms of Platonism do this. For Balaguer's Full Blooded Platonism (Balaguer, 1998b), according to which any consistent set of axioms defines an existent set of mathematical objects, this objection may not work. Similarly for Hale and Wright's abstractionist Platonism (Hale and Wright, 2009), according to which, mathematical existence is secured through abstraction principles definitive of our mathematical concepts. Nonetheless, our concern has primarily been with those forms of Platonism established on the basis of the indispensability argument, and these tend to be of the ontologically inflationary variety. Further, there are objections to both these kinds of Platonism, which I lack space to discuss, which make them independently unattractive as ontologies for mathematics.

was not. In other words, in the case of mathematical epistemology, we have found an objection that tells against mathematical Platonism, and that does not afflict mathematical fictionalism. I propose that in the case of mathematical ontology, epistemicism has been refuted.

5.4 Conclusion

In this chapter we have sought to respond to two kinds of metaontological challenges that seem especially threatening for the mathematical fictionalist. We began by considering Yablo's attempt to draft insights familiar to the fictionalist into the services of ontological deflationism. In particular, we saw that for Yablo the presence of certain kinds of metaphorical idioms in our linguistic practice threatens the divide between the literal and non-literal in a way that in turn threatens to undermine the whole ontological project. I argued, contra Yablo, that the presence of representationally and presentationally essential metaphors in science is no threat to the Quinean ontologist and that the trouble-making metaphor, the procedurally essential kind, is unlikely to play any role in science. The prospects, then, for a general fictionalist deflationism seem poor.

In the second half of the chapter we considered Bennett's epistemicism, according to which the way in which parties to metaphysical debates seek to minimize the difference between their respective positions, threatens our ability to discover the answers to metaphysical questions. I went on to argue that while fictionalism is a difference minimizing approach to ontology, in that it seeks to up-play its expressive resources and capture just as much of mathematical practice as the Platonist can, there is one kind of objection to Platonism, Field's epistemic challenge, that the fictionalist does not inherit. Because the Platonist is not interested in downplaying her ontological commitments, she is obliged to provide a naturalistic epistemology for those commitments, something that it seems difficult to imagine her succeeding with. No such challenge seems to face the fictionalist.

I conclude, then, that with respect to the most serious metaontological challenges, fictionalism is surprisingly well situated. There is no reason to think that the debate between the fictionalist and the Platonist cannot be decided, and no reason to think that fictionalist considerations favour dismissing ontological questions as ill-formed. This conclusion should serve to strengthen fictionalism's claims as a theory of mathematical ontology and applications.

Conclusion

In this thesis I have sought to outline and defend an anti-realist approach to the philosophy of mathematics based on Kendall Walton's theory of fictions as games of make-believe (Walton, 1990). The idea has been to combine this fictionalist approach to mathematical ontology with an attractive recent theory of mathematical applications, the mapping account (Pincock, 2004), to give us a detailed account of how mathematics works in application, without finding ourselves committed to any abstract objects. The remainder of the thesis addressed a number of objections to this kind of approach to metaphysics, and defended the legitimacy of pursuing an anti-realist project in ontology, within a realist approach to metaontology.

In the introduction we addressed the motivations for the fictionalist approach. We saw there that once we have adopted a synoptic program for philosophy, like naturalism, it becomes difficult to see how to accommodate certain entities within our conceptual scheme. In particular, drawing on a challenge to the mathematical platonist due to Hartry Field (Field, 1989) and Paul Benacerraf (1983b), we saw that there is a particular challenge in accounting for our knowledge of mathematical objects given what science tells us about the kinds of creatures we are, and the kinds of cognitive abilities we are likely to possess. Following Huw Price (Price, 2011) we have labelled this *the placement problem*, the problem of finding a place for those mathematical entities we routinely talk about, but which cause difficultly for a naturalist program in philosophy.

We saw further that the placement problem is exacerbated by the existence of the indispensability argument, which suggests that as good naturalists, who take our science seriously, we should be committed to the existence of mathematical objects because of the role they play in science (Putnam, 1971). This means that we cannot easily adopt an eliminativist response to the placement problem, because doing so seems to be incompatible with our naturalistic approach to philosophy.

In the first chapter we turned to the indispensability argument, in order to assess its cogency. I there argued that traditional indispensability arguments fail. The indispensability argument of Quine and Putnam, as formalized by Colyvan (2001a), includes as a premise a view about scientific confirmation, confirmational holism, that is questionable in the light of the recent close attention paid to scientific methodology. That is, philosophers such as Maddy (1992) and Sober (1993) have found reason to doubt that empirical evidence confirms every part of a successful theory equally, but that some parts of our theories are insulated from empirical confirmation or discomfirmation. This alone would be enough to show the unsoundness of the indispensability argument, but we saw further how Leng argues that mathematical portions of our theories may be more like the posits of idealized representations in science, which it is reasonable to treat as kinds of fiction (Leng, 2010, pp.155-181).

In the light of this failure we saw how some mathematical platonists have switched to a new and enhanced version of the indispensability argument, the explanatory indispensability argument (Baker 2005; Baker and Colyvan, 2011). This new argument does not depend on the truth of confirmational holism about science, but instead rests on the prima facie plausible assumption that whatever posits appear in our best explanations of some phenomena must exist (alternatively, theories that explain are true). The platonist pursuing this line of argument then presents evidence that seems to suggest that in some cases mathematical entities can explain physical phenomena. For the explanation to be true, the mathematical entities must exist. The naturalist believes the explanation is true, so she must be committed to the existence of the mathematical entities appearing in it. Once again, the naturalist seems to have to find a place for mathematical objects in her ontology.

I argued that this line of argument fails, because it fails to take account of a vicious circularity in the kinds of examples mathematical platonists have adduced. In every case the mathematical platonist can name, mathematical entities already appear in our characterization of the phenomenon under investigation. For instance, in Baker's cicadas example (Baker, 2005, pp.229-233) we must already have in place a numerical theory for the characterization of the cicada life-cycles, *before* we can appeal to number theoretic results to explain those life cycles. But then the explanatory pattern looks like this:

Number Theory + Biological Facts $\xrightarrow{\textbf{Explained By}}$ Number theory

But then what we have is a case of some number theoretic facts explaining something that is already in part number theoretic, and I argued that this introduces a circularity that vitiates the platonist's argument.

The upshot of this chapter was that explanatory indispensability arguments are unlikely to be successful, and so if there is a successful indispensability argument it will be found by examining scientific representation. That is, the failure of the explanatory indispensability argument suggests that the best bet for finding an argument for platonism is by turning to the role mathematics plays in facilitating representations of scientific phenomena. If it turns out that our best theory of mathematics' role in scientific representation requires a platonist ontology, then it seems we will have a new argument for a naturalistic platonism.

In the second chapter we turned to a contemporary theory of the role mathematics plays in facilitating scientific representations. According to Pincock's mapping account (Pincock, 2004), some piece of mathematics is playing a representational role in science when there exists a structure preserving map between the empirical domain under investigation and a suitable mathematical structure. This theory is attractive because it provides a detailed account of how mathematics functions in representations. Indeed, to me this seems a major advance over previous discussions of mathematical representation in science, which usually shy away from giving technical details about how representation might proceed.

We then saw a number of objections put forward by Bueno and Colyvan (2011, pp.348-352) that suggest that Pincock's mapping account might not be able to handle every instance of mathematical representation. In place of the mapping account they propose a three stage inferential conception of mathematical representation (Bueno and Colyvan, 2011, pp.352-356), with *immersion* mapping from the empirical domain to a mathematical structure, *derivation* allowing for inferential manipulation of the structure, and finally *interpretation* taking us back the other way, from mathematical structure to empirical phenomena.

We saw reason to doubt Bueno and Colyvan's claim that their inferential conception is a significant advance over the mapping account. I argued that the fundamental role of mathematics in its application to science is as a descriptive or representational device, not its role in facilitating inferences that we would otherwise struggle to make (though I mentioned in passing that a similar view is endorsed by Field (1980), and so is perfectly compatible with fictionalism). I also argued that there is no reason to expect a theory of mathematical representation to also be a theory of mathematical explanation or inference, and so there is little reason to think that a theory of mathematical representation will have to give central place to inferential uses of mathematics. Because the inferential conception is in other respects so similar to the mapping account, I then suggested that we could build an enhanced mapping account to avoid Bueno and Colyvan's objections, making use of their immersion and interpretation stages, but remaining agnostic about the derivation stage until a better argument could be produced for the central role of inference in applied mathematics (though I reserved the right to reincorporate this stage of application, if a convincing case could be made that this was necessary to accommodate uses of mathematics in science that are plausibly construed as inferential in nature. I do not believe that doing so need undermine the claim that the central role of mathematics in science is as a representational device).

But this theory, the enhanced mapping account, seems prima facie thoroughly platonistic. In chapter 3 I introduced a new version of the indispensability argument, drawing on the role mathematics plays in our best theory of scientific representation. There I argued that as naturalists, for whom philosophy is continuous with our best current science, we ought to be committed to any posits that play an indispensable role in our best *philosophical* theory of mathematical representation. If we are satisfied that the mapping account is a true account of mathematical representation, then we ought to be committed to the mathematical entities that are quantified over by that account. This leaves us ontologically committed to the existence of structure preserving maps and mathematical structures.

The bulk of chapter 3 was then spent outlining a particular kind of nominalistic approach to mathematics that would enable us to evade this commitment – a revolutionary pretence theory of mathematics based on Kendall Walton's view of fictions as games of make-believe (Walton 1990; Walton, 1993). The idea is to embed the mapping account's detailed story about mathematical applications within the kind of fictionalist approach to mathematics outlined by the likes of Leng (2010) and Yablo (2001; 2005). We start with principles of generation drawn from axiomatic set theory with (real world) ur-elements (that is, we say something like, "Let's pretend that ordinary things can form sets"), and then use the mapping account to constrain the kinds of sets that will be appropriate within mathematical applications. Because the relevant sets, mappings and mathematical structures are only encountered within a pretence, we do not inherit any ontological commitment to these kinds of abstract entities.

I further argued that it is appropriate to view this account as a variety of revolutionary fictionalism, that is, as a philosophical theory adopted by ontologists, and not as an interpretative account of what mathematicians and scientists themselves mean by their utterances (a hermeneutic fictionalism). The idea is that, while hermeneutic fictionalism cannot be dismissed as obviously false, and while objections like the autism objection do not at present seem sufficient to overturn it (Liggins, 2010), this very susceptibility to *possible* empirical discomfirmation makes hermeneutic fictionalism less attractive as an account of mathematical ontology. Once we have granted that a revolutionary fictionalism is acceptable from a naturalistic point of view (something I hope to have established in the material on Balaguer and Armour-Garb), adopting a revolutionary fictionalist perspective on applied mathematics looks like an attractive alternative to the hermeneutic option.

This theory of mathematics as a kind of make-believe game has a number of attractive features. Firstly, it has enabled us to evade the indispensability argument, even at the level of mathematical representation. If there is no hope for an indispensability argument from mathematical explanation, and no hope for an indispensability argument from our best theory of mathematical representation, and no hope for the Quine-Putnam holist version of the indispensability argument, then this leaves the naturalistically minded platonist, concerned to establish platonism on the basis of our best science, with very few options to pursue. If we are concerned about the placement problem of locating mathematics within our naturalistic program (say because of Field's epistemological challenge), then this is a considerable advantage.

Secondly, the fictionalized mapping account I have outlined here gives us a helpfully detailed story of how mathematical applications can be expected to proceed. We begin by pretending that we can gather the empirical stuff into sets as defined by the axioms of set theory with ur-elements. We then establish the existence of structure preserving mappings between these empirically grounded sets and a suitable mathematical structure, in line with the account of the immersion and interpretation stages outlined earlier. These mappings and the mathematical structures mapped onto will also just be further sets, and the whole procedure is taking place within the fiction generated by our choice of the axioms of set theory with ur-elements as principles of generation.

I believe that this theory can be leveraged to give us usefully detailed accounts of mathematical methodology. In this thesis, for reasons of space, I have shied away from giving detailed case studies, but one piece of future research might involve applying the above story to some particular scientific application of mathematics, showing how, in particular, the nature of the empirical stuff collected into our imaginary sets constrains our choice within the pretence of mappings and mathematical structures. Doing this should help illuminate the way in which practising scientists are constrained in their choice of mathematical structure to apply, and give us new insight into the way in which these choices proceed. I believe that there is, then, scope for applying this account in future research into the methodology of applied mathematics, and that this brand of fictionalism should be seen as a fruitful theory of mathematical applications in its own right, and not merely as device adopted to evade platonistic commitments.

In the remainder of the thesis we have sought to defend this brand of fictionalism about mathematical application from a number of challenges that call into question the very viability of this kind of approach to metaphysics. It will be remembered that the burden of chapters 1 through 3 was that it is to mathematics' role in scientific representation that we must look to establish what our ontological commitments are. But Price's (2011) pragmatism threatens this entire project, in effect calling into to question whether the notion of representation works in the way the realist/anti-realist debate in mathematical ontology needs it to. Price's contention is, that for good naturalistic reasons, we ought to adopt an anti-representational account of apparently descriptive language, according to which it is not the function of language to tell us what the world is like (Price, 2011, pp.184-199). The result of this change of focus is that questions about what kind of entities the world contains are likely to lapse (Price, 2011, pp.8-11): there is no reason to think that any one way of speaking (say the scientific way of speaking) is better than any other for establishing what there is (because no way of speaking will establish, in some metaphysically robust sense, what there is), and the naturalistic philosopher must just content herself with a kind of sociological or anthropological investigation into the reasons we have for speaking the languages we do (Price, 2011, p.199).

I argued against this that there are a number of reasons for favouring the traditional representational account of language over Price's alternative anti-representationalism. In particular, his metaphysical pluralism seems to sit uncomfortably with his claims to be a naturalist, the core commitments of his position are mostly unargued, and there are a number of outstanding challenges to the position that seem to have plausible answers from a representationalist point of view, but are more challenging to account for from the anti-representational point of view.

This opens a number of avenues for future research into related philosophical issues. Firstly, we must consider what kind of account of language *is* acceptable to a fictionalist. What kinds of theory of word-world relations are compatible with the realism and anti-realism debate in philosophy, and how plausible are those positions in the light of modern philosophy of language? Secondly, and relatedly, it would be useful to have some account of the kind of theory of truth the fictionalist ought to prefer, and whether there are any theories of truth that might undermine the kind of metaphysical project to which the fictionalist has committed herself. I hope to discuss both of these issues in future work.

Finally, in passing we noted that Quine might not have been totally resistant to some kind of pragmatist theory of word-world relations, but that his disagreement with Price would have centred on the role accorded to scientific discourse in building our total theory of the world (see Quine, 1951). This raises the important historical issue of identifying how it is that a debate that begins with Quine's pragmatist approach to issues in ontology, has since evolved into a debate that seems to involve robust metaphysical commitments and strong word-world relations.

In the final chapter we turned to the assessment of two objections drawn from the burgeoning subdiscipline of metametaphysics. We began with Yablo's (1998) attempts to motivate a deflationist metaontology on the basis of his figuralism. In particular, we saw how Yablo believes that the very same kind of theory of fictions as games of make-believe that we have employed to evade the indispensability argument could be employed to resuscitate the Carnapian idea of language frameworks (Yablo, 1998, pp.242-244). Once this is done, Yablo believes, the existence of differing grades of metaphorical involvement will tend to undermine the distinction between literal and non-literal uses of language that the Quinean ontologist requires (Yablo, 1998, pp.248-258). As the bulk of this thesis has been concerned with ontology in a largely Quinean mode (in which one inherits an ontological commitment to those entities one indispensability quantifies over in one's best theory of the world) Yablo's argument presents a very serious challenge to the work done previously.

Against Yablo I have argued that at least two of his forms of metaphorical involvement are harmless (representationally and presentationally essential metaphor). Metaphors of this grade may well be indispensable for our best science, but once we have recognised the falsity of confirmational holism (as we did back in chapter 1), the Quinean ontologist will have no problem regarding these as merely convenient fictions, with no ontological import. Further, the trouble making metaphors, the procedurally essential metaphors, seem to play no role in our best scientific theories (indeed, I raised the sceptical possibility that such a grade of metaphorical involvement might not exist even in everyday speech). Such metaphors, in which the metaphorical content is ambiguous or even undetermined, seem to thwart the aims of prediction and explanation for which we have scientific theories in the first place. I concluded that Yablo's fictionalist deflationism is unlikely to be successful.

But this raises the interesting question of whether there is *any* hope of marshalling the intuitions upon which the first-order fictionalist draws for the purpose of developing a second-order *meta*ontological scepticism or deflationism. In the modern field of metametaphysics, and in particular within metaontology, deflationary intuitions abound, and scepticism about metaphysics seems to be the dominant strain among modern metametaphysicians (of the 16 articles, not inclusive of the introduction, published in Chalmers, Manley, and Wasserman's 2009 volume *Metametaphysics*, over half develop a sceptical position with respect to metaphysics). Clearly, then, attempts to develop a deflationary metametaphysics on the basis of some kind of fictionalism would be both attractive against the backdrop of the current revival of anti-metaphysical sentiment, and a serious threat to the project pursued in this thesis. So far, Yablo's is the only developed attempt to do this in the literature, but being aware of the possible ways in which fictionalism might be employed in the service of metaontological deflationism would certainly be helpful for a defender of mathematical fictionalism, and one avenue of further research that ought to be undertaken in future.

In the final section of the thesis we turned to Bennett's epistemicism (Bennett, 2009), a kind of scepticism about our ability to settle metaphysical questions one way or the other. As we saw there, Bennett believes that in a significant number of metaphysical disputes, the two sides to the dispute (what Bennett calls the high-ontology and low-ontology sides) will seek respectively to downplay their ontological commitments or to up-play (that is increase) their expressive resources (Bennett, 2009, p.46). This is because, in most cases, the disputants wish to capture as much of our quotidian practice and speech as possible: they wish, in short, to avoid looking crazy. But this has disastrous consequences for the epistemology of metaphysics, because it means that in most, perhaps all of the disputes characterized by this difference-minimizing tendency, there will end up being nothing to choose between the two positions, no evidence that would settle the debate one way or the other (Bennett, 2009, pp.65-71).

This seemed to be a particular threat to the dispute over mathematical ontology, because in at least two ways this dispute seemed to fit the pattern identified by Bennett: there is a high and a low ontology side (the platonist and the nominalist respectively), and, at least in the case of revolutionary and hermeneutic fictionalism, serious attempts are made to up-play the expressive resources the nominalist can draw upon so that the resulting nominalism can capture every part of mathematical practice that Platonism can. It looks then, at first blush, as though the kind of fictionalism defended in this thesis will end up being a contribution to an undecidable debate; no evidence will be available to settle the issue either way.

Against Bennett I argued that, because the Platonist is not typically committed to downplaying her ontological commitments, there will be at least one piece of evidence – the Benacerraf-Field epistemic challenge – that will have purchase in the one case, and not in the other. But this raises the question of whether there might not be kinds of platonism that evade the epistemic challenge, platonisms for which the downplaying of ontological commitment is an option. In this thesis I have largely been concerned with those platonisms that are established on the basis of the indispensability argument, and for these kinds of platonisms, mathematicalia are usually abstract objects, causally inefficacious and located outside of space and time. But in a footnote to the main text I noted the existence of at least two kinds of platonism that do not fit this mould: Balaguer's Full Blooded Platonism, according to which every consistent mathematical structure exists (Balaguer, 1998b); and Hale and Wright's abstractionist platonism, according to which mathematical existence is merely a consequence of abstraction principles definitive of mathematical concepts (Hale and Wright, 2009). In the footnote I mentioned the existence of reasons to be suspicious of these kinds of platonism (the bad company objection, for one), but it would be helpful in future research to conduct a systematic study of the various kinds of options for epistemically downplayed platonism, and to examine any common weaknesses that might be exploited by the fictionalist seeking to defend the epistemic robustness of her position.

As a final line of future research, it would be helpful to conduct a thorough review of the modern positions in metametaphysics and to examine which, if any, pose a threat to fictionalism about mathematical ontology (and to develop responses to these). As a related line of future research, it would be helpful to examine the various kinds of metaontological realism (according to which ontological questions are substantive and decidable), and especially to seek out kinds of metaontological realism that fit with the broadly naturalist and Quinean framework within which the research presented here takes place.

Before closing, I will recap the results established in this thesis. The final position which I hope to have established is this: we should see mathematics in its applications to natural science as functioning something like a sophisticated game of make-believe. We begin with the axioms of set-theory with ur-elements, according to which we are instructed to pretend that we can gather together ordinary, nominalistically acceptable items of the world into sets (and sets of sets of these, and so on). These axioms will then serve as our principles of generation, in line with Walton's account of games of make-believe. On the basis of this we are then able to form further new sets of these ur-elements that will serve as our tentative assumed structure for our application of mathematics. This assumed structure will constrain our choice of further set formation, in particular, if the picture presented by Pincock is correct, we will be forced to establish the existence of a structure preserving mapping between this assumed structure and some appropriate mathematical structure, which will serve as our final representation of the empirical domain in our mathematical-scientific theory. Working back from this mathematical structure, via (possibly new) structure preserving mappings, we can interpret this mathematical structure in terms of our original assumed structure, the set of ur-elements with which we started. In this way we can see directly how the empirical stuff we are actually interested in constrains the kind of story we are able to tell within the mathematical-scientific fiction.

I have argued that we should see this position as a kind of revolutionary fictionalism, not as a hermeneutic variety, on the grounds that hermeneutic fictionalism leaves far too much to empirical chance. The result, I believe, is an ontologically minimal account of mathematical practice, that enables us to evade the challenge of the indispensability argument, while maintaining a nominalist position in metaphysics. It gives us an attractively detailed story about how mathematical applications proceed, that shows exactly *how* it is that the empirical world constrains our fictional story-telling about the mathematical portions of our scientific theories, and I believe the variety of fictionalism I have defended here has shown itself robust in the face of a number of recent challenges. On these grounds I conclude that this revolutionary pretence-theory of the mapping account of applied mathematics is our best account of mathematical practice, and our best available response to the mathematical placement problem.

Bibliography

Armour-Garb, B. (2011a). Understanding and mathematical fictionalism. *Philosophia Mathematica*, 19(3), 335-344.

Armour-Garb, B. (2011b). The implausibility of hermeneutic non-assertivism. *Philosophia Mathematica*, 19(3), 349-353.

Armstrong, D.M. (1989). *Universals: an opinionated introduction*. Boulder: Westview Press.

Ayer, A.J. (2001). *Language, truth and logic.* London: Penguin Books.

Azzouni, J. (1998). On 'On what there is'. *Pacific Philosophical Quarterly*, 79(1), 1-18.

Azzouni, J. (2012). Taking the easy road out of dodge. *Mind*, 121(484), 951-965.

Baker, A. (2005). Are there genuine mathematical explanations of physical phenomena? *Mind*, 114(454), 223-238.

Baker, A. (2009). Mathematical explanation in science. *The British Journal for the Philosophy of Science*, 60(3), 611-633.

Baker, A. and Colyvan, M. (2011). Indexing and mathematical explanation. *Philosophia Mathematica*, 19(3), 323-334.

Balaguer, M. (1996a). Towards a nominalization of quantum mechanics. *Mind*, 105(418), 209-226

Balaguer, M. (1996b). A fictionalist account of the indispensable applications of mathematics. *Philosophical Studies*, 83(3), 291-314.

Balaguer, M. (1998a). Attitudes without propositions. *Philosophy and Phenomenological Research*, 58(4), 805-826.

Balaguer, M. (1998b). *Platonism and anti-platonism in mathematics*. New York: Oxford University Press.

Balaguer, M. (2009). Fictionalism, theft, and the story of mathematics. *Philosopia Mathematica*, 17(2), 131-162.

Balaguer, M. (2011). Reply to Armour-Garb. *Philosophia Mathematica*, 19(3), 345-348.

Balaguer, M. (2015). *Fictionalism in the philosophy of mathematics*. In Zalta, E.N. (ed.) The Stanford Encyclopedia of Philosophy (Summer 2015 Edition). https://plato.stanford.edu/archives/sum2015/entries/fictionalism-mathematics/

Bangu, S.I. (2008). Inference to the best explanation and mathematical realism. *Synthese* 160(1), 13-20.

Batterman, R. (2010). On the explanatory role of mathematics in empirical science. *The British Journal for the Philosophy of Science*, 61(1), 1-25.

Benacerraf, P. (1983a). What numbers could not be. In Benacerraf, P. and Putnam, H. (eds.) *Philosophy of mathematics: selected readings*. 2nd edition. Oxford: Basil Blackwell, pp. 272-294.

Benacerraf, P. (1983b). Mathematical truth. In Benacerraf, P. and Putnam, H. (eds.) *Philosophy of mathematics: selected readings*. 2nd edition. Oxford: Basil Blackwell, pp. 403-420.

Benacerraf, P. and Putnam, H. (eds.) (1983). *Philosophy of mathematics: selected readings*. 2nd edition. Oxford: Basil Blackwell.

Bennett, K. (2009). Composition, colocation, and metaontology. In Chalmers, D.J., Manley, D. and Wasserman, R. (eds.) (2009) *Metametaphysics: new essays on the foundations of ontology.* Oxford: Clarendon Press, pp. 38-76.

Berto, F. and Plebani, M. (2015). *Ontology and metaontology: a contemporary guide*. London: Bloomsbury Academic.

Boolos, G. (1971). The iterative conception of set. *The Journal of Philosophy*, 68(8), 215-231. Reprinted in Benacerraf, P. and Putnam, H. (eds.) (1983). *Philosophy of mathematics: selected readings*. 2nd edition. Oxford: Basil Blackwell, pp. 486-502.

Brandom, R. (2000). *Articulating reasons: an introduction to inferentialism*. Cambridge, MA: Harvard University Press.

Brouwer, L.E.J. (1983). Consciousness, philosophy, and mathematics. In Benacerraf, P. and Putnam, H. (eds.) *Philosophy of mathematics: selected readings*. 2nd edition. Oxford: Basil Blackwell, pp.90-96.

Bueno, O. (2009). Mathematical Fictionalism. In Bueno, O. and Linnebo, O. (eds.) *New waves in philosophy of mathematics.* Basingstoke: Palgrave-Macmillan, pp.59-79.

Bueno, O. (2012). An easy road to nominalism. *Mind*, 121(484), 967-982.

Bueno, O. and Colyvan, M. (2011). An inferential conception of the application of mathematics. *Nous*, 45(2), 345-374.

Bueno, O. (2014). *Nominalism in the philosophy of mathematics*. In In Zalta, E.N. (ed.) The Stanford Encyclopedia of Philosophy (Spring 2014 Edition). https://plato.stanford.edu/archives/spr2014/entries/nominalism-mathematics/

Bueno, O. and Linnebo, O. (eds.) (2009). *New waves in philosophy of mathematics.* Basingstoke: Palgrave-Macmillan.

Burgess, J.P. (2004). Mathematics and Bleak House. *Philosophia Mathematica*, 12(1), 18-36.

Burgess, J.P. and Rosen, G. (1997). *A subject with no object.* Oxford: Clarendon Press.

Carnap, R. (1950). Empiricism, semantics, and ontology. *Revue Internationale de Philosophie*, 4(11), 20-40. Reprinted in Benacerraf, P. and Putnam, H. (eds.) (1983). *Philosophy of mathematics: selected readings*. 2nd edition. Oxford: Basil Blackwell, pp. 241-257.

Cartwright, N. (1983). *How the laws of physics lie.* Oxford: Clarendon Press.

Chalmers, D.J., Manley, D. and Wasserman, R. (eds.) (2009) *Metametaphysics: new essays on the foundations of ontology.* Oxford: Clarendon Press.

Colyvan, M. (1998). Can the eleatic principle be justified? *The Canadian Journal of Philosophy*, 28(3), 313-336.

Colyvan, M. (2001a). *The indispensability of mathematics.* |New York: Oxford University Press.

Colyvan, M. (2001b). The miracle of applied mathematics. *Synthese*, 127(3), 265-278.

Colyvan, M. (2002). Mathematics and aesthetic considerations in science. *Mind*, 111(441), 69-74.

Colyvan, M. (2010). There is no easy road to nominalism. *Mind*, 119(474), 285-306.

Colyvan, M. (2012). Road work ahead: heavy machinery on the easy road. *Mind*, 121(484), 1031-1046.

Colyvan, M. (2015). *Indispensability arguments in the philosophy of mathematics*. In Zalta, E.N. (ed.) The Stanford Encyclopedia of Philosophy (Spring 2015 Edition). https://plato.stanford.edu/archives/spr2015/entries/mathphil-indis/

Colyvan, M. and Lyon, A. (2008). The explanatory power of phase spaces. *Philosophia Mathematica*, 16(2), 227-243.

Daly, C. (2006). Mathematical fictionalism: no comedy of errors. *Analysis*, 66(3), 208-216.

Daly, C. and Langford, S. (2009). Mathematical explanation and indispensability arguments. *The Philosophical Quarterly*, 59(237), 641-658.

Damnjanovic, N. and Stoljar, D. (2014). *The deflationary theory of truth*. In Zalta, E.N. (ed.) The Stanford Encyclopedia of Philosophy (Fall 2014 Edition). https://plato.stanford.edu/archives/fall2014/entries/truth-deflationary/

Dieterle, J.M. (1999). Mathematical, astrological, and theological naturalism. *Philosophia Mathematica*, 7(2), 129-135.

Eklund, M. (2006). Metaontology. *Philosophy Compass*, 1(3), 317-334.

Eklund, M. (2009). Carnap and ontological pluralism. In Chalmers, D.J., Manley, D. and Wasserman, R. (eds.) (2009) *Metametaphysics: new essays on the foundations of ontology*. Oxford: Clarendon Press, pp. 130-156.

Eklund, M. (2015). *Fictionalism*. In Zalta, E.N. (ed.) The Stanford Encyclopedia of Philosophy (Winter 2015 Edition). https://plato.stanford.edu/archives/win2015/entries/fictionalism/

Feynman, R. (1985). *QED: the strange theory of light and matter*. London: Penguin Books.

Field, H. (1972). Tarski's theory of truth. *The Journal of Philosophy*, 69(13), 347-375.

Field, H. (1978). Mental Representation. *Erkenntnis*, 13(1), 9-61.

Field, H. (1980). *Science without numbers*. Oxford: Basil Blackwell.

Field, H. (1989). *Realism, mathematics and modality*. Oxford: Basil Blackwell.

Fine, A. (1993). Fictionalism. *Midwest Studies in Philosophy*, 18(1), 1-18.

Fine, A. (1996). The natural ontological attitude. In Papineau, D. (ed.) *The philosophy of science*. Oxford: Oxford University Press, pp. 21-44.

Forrai, G. (2010) What mathematicians' claims mean: in defence of hermeneutic fictionalism. *Hungarian Philosophical Review*, 54(4), 191-203.

French, S. (2000). The reasonable effectiveness of mathematics: partial structures and the application of group theory to physics. *Synthese*, 125(1-2), 103-120.

Gödel, K. (1983). What is Cantor's continuum problem? In Benacerraf, P. and Putnam, H. (eds.) *Philosophy of mathematics: selected readings*. 2nd edition. Oxford: Basil Blackwell, pp. 470-485.

Goldman, A. (1967). A causal theory of knowing. *The Journal of Philosophy*, 64(12), 357-372.

Goodman, N. and Quine, W.V. (1947). Steps toward a constructive nominalism. *The Journal of Symbolic Logic*, 12(4), 105-122.

Grice, H.P. and Strawson, P.F. (1956). In defence of a dogma. *The Philosophical Review*, 65(2), 141-158.

Hacking, I. (1983). *Representing and intervening*. Cambridge: Cambridge University Press.

Hale, B. and Wright, C. (2009). The metaontology of abstraction. In Chalmers, D.J., Manley, D. and Wasserman, R. (eds.) *Metametaphysics: new essays on the foundations of ontology.* Oxford: Clarendon Press, pp.178-212.

Hellman, G. (1989). *Mathematics without numbers.* Oxford: Clarendon Press.

Hempel, C.G. (1945). On the nature of mathematical truth. *The American Mathematical Monthly*, 52(10), 543-556.

Hempel, C.G. and Oppenheim, P. (1948). Studies in the logic of explanation. *Philosophy of Science*, 15(2), 135-175.

Heyting, A. (1983). The intuitionist foundations of mathematics. In Benacerraf, P. and Putnam, H. (eds.) *Philosophy of mathematics: selected readings*. 2nd edition. Oxford: Basil Blackwell, pp. 52-61.

Hirsch, E. (2002). Quantifier variance and realism. *Philosophical Issues*, 12(1), 51-73. Reprinted in Hirsch, E. (2011). *Quantifier variance and realism*. New York: Oxford University Press, pp. 68-95.

Hirsch, E. (2011). *Quantifier variance and realism*. New York: Oxford University Press.

Hoffman, S. (2004). Kitcher, ideal agents, and fictionalism. *Philosophia Mathematica*, 12(1), 3-17.

Horsten, L. (2016). *Philosophy of mathematics*. In Zalta, E.N. (ed.) The Stanford Encyclopedia of Philosophy (Winter 2016 Edition). https://plato.stanford.edu/archives/win2016/entries/philosophy-mathematics/

Jarrold, C. (2003). A review of research into pretend play in autism. *Autism*, 7(4), 379-390.

Joyce, R. (2005). Moral fictionalism. In Kalderon, M.E. (ed.) *Fictionalism in metaphysics*. Oxford: Clarendon Press, pp. 287-313.

Kadanoff, L. (2000). *Statistical physics: statics, dynamics, and renormalization.* Singapore: World Scientific Publishing Co.

Kalderon, M.E. (2005a). *Moral fictionalism*. Oxford: Oxford University Press.

Kalderon, M.E. (ed.) (2005b) *Fictionalism in metaphysics*. Oxford: Clarendon Press.

Kalderon, M.E. (2005c). Introduction. In Kalderon, M.E. (ed.) *Fictionalism in metaphysics*. Oxford: Clarendon Press, pp. 1-13.

Ladyman, J. and Ross, D. (2007). *Every thing must go: metaphysics naturalized.* Oxford: Oxford University Press.

Leng, M. (2002). What's wrong with indispensability? *Synthese*, 131(3), 395-417.

Leng, M. (2005). Revolutionary fictionalism: a call to arms. *Philosophia Mathematica*, 13(3), 277-293.

Leng, M. (2010). *Mathematics and reality*. Oxford: Oxford University Press.

Leng, M. (2012). Taking it easy: a response to Colyvan. *Mind*, 121(484), 983-995.

Lewis, D. (1978). Truth in fiction. *American Philosophical Quarterly*, 15(1), 37-46.

Lewis, D. (1986). *On the plurality of worlds*. Oxford: Blackwell Publishing.

Lewis, D. (1993). Mathematics as megethology. *Philosophia Mathematica*, 1(1), 3-23.

Liggins, D. (2010). The autism objection to pretence theories. *The Philosophical Quarterly*, 60(241), 764-782.

Liggins, D. (2012). Weaseling and the content of science. *Mind*, 121(484), 997-1005.

Lowe, E.J. (2002). *A survey of metaphysics*. Oxford: Oxford University Press.

Maddy, P. (1990). *Realism in mathematics.* Oxford: Clarendon Press.

Maddy, P. (1992). Indispensability and Practice. *The Journal of Philosophy*, 89(6), 275-289.

Maddy, P. (1997). *Naturalism in mathematics*. Oxford: Clarendon Press.

Maddy, P. (2011). *Defending the axioms*. Oxford: Oxford University Press.

Malament, D. (1982). Book review: *Science without Numbers: A Defence of Nominalism*. Hartry H. Field. *The Journal of Philosophy*, 79(9), 523-534.

Mancosu, P. (2015). *Explanation in mathematics*. In Zalta, E.N. (ed.) The Stanford Encyclopedia of Philosophy (Summer 2015 Edition). https://plato.stanford.edu/archives/sum2015/entries/mathematics-explanation/

Manley, D. (2009). Introduction: a guided tour of metametaphysics. In Chalmers, D.J., Manley, D. and Wasserman, R. (eds.) *Metametaphysics: new essays on the foundations of ontology*. Oxford: Clarendon Press, pp.1-37.

Melia, J. (2000). Weaseling away the indispensability argument. *Mind*, 109(435), 455-480.

Morgan, M.S. and Morrison, M. (eds.) (1999). *Models as mediators: perspectives on natural and social science*. Cambridge: Cambridge University Press.

Papineau, D. (ed.) (1996). *The philosophy of science*. Oxford: Oxford University Press.

Papineau, D. (2016). *Naturalism*. In Zalta, E.N. (ed.) The Stanford Encyclopedia of Philosophy (Winter 2016 Edition). https://plato.stanford.edu/archives/win2016/entries/naturalism/

Parsons, C. (1980). Mathematical Intuition. *Proceedings of the Aristotelian Society*, 80, 145-168.

Pincock, C. (2004). A new perspective on the problem of applying mathematics. *Philosophia Mathematica*, 12(2), 135-161.

Pincock, C. (2009). Towards a philosophy of applied mathematics. In Bueno, O. and Linnebo, O. (eds.) *New waves in philosophy of mathematics.* Basingstoke: Palgrave-Macmillan, pp.173-194.

Pincock, C. (2011). On Batterman's 'On the explanatory role of mathematics in empirical science.' *The British Journal for the Philosophy of Science,* 62(1), 211-217.

Pincock, C. (2012). *Mathematics and scientific representation.* New York: Oxford University Press.

Pitt, D. (2013). *Mental representation*. In Zalta, E.N. (ed.) The Stanford Encyclopedia of Philosophy (Fall 2013 Edition). https://plato.stanford.edu/archives/fall2013/entries/mental-representation/

Price, H. (1997). Naturalism and the fate of the M-worlds. *Aristotelian Society Supplementary Volume*, 71(1), 247-268. Reprinted in Price, H. (2011). *Naturalism without mirrors*. New York: Oxford University Press, pp. 132-147.

Price, H. (2007). Quining naturalism. *The Journal of Philosophy*, 104(8), 375-402.

Price, H. (2009). Metaphysics after Carnap: the ghost who walks? In Chalmers, D.J., Manley, D. and Wasserman, R. (eds.) *Metametaphysics: new essays on the foundations of ontology*. Oxford: Clarendon Press, pp.320-246.

Price, H. (2011). *Naturalism without mirrors*. New York: Oxford University Press.

Putnam, H. (1971). *Philosophy of logic*. New York: Harper & Row.

Putnam, H. (1983). Mathematics without foundations. In Benacerraf, P. and Putnam, H. (eds.) *Philosophy of mathematics: selected readings*. 2nd edition. Oxford: Basil Blackwell, pp. 295-311.

Quine, W.V.O. (1948). On what there is. *The Review of Metaphysics*, 2(5), 21-38. Reprinted in Quine (1980), 1-19.

Quine, W.V.O. (1951). Two dogmas of empiricism. *The Philosophical Review*, 60(1), 20-43. Reprinted and revised in in Quine (1980), 20-46.

Quine, W.V.O. (1960). Carnap and logical truth. *Synthese*, 12(4), 350-374.

Quine, W.V.O. (1969). *Ontological relativity and other essays*. New York: Columbia University Press.

Quine, W.V.O. (1980). *From a logical point of view*. 2nd edition, revised. Cambridge, MA: Harvard University Press.

Quine, W.V.O. (1992). *Pursuit of truth*. Revised edition. Cambridge, MA: Harvard University Press.

Resnik, M. (1981). Mathematics as a science of patterns: ontology and reference. *Nous*, 15(4), 529-550.

Resnik, M. (1982). Mathematics as a science of patterns: epistemology. *Nous*, 16(1), 95-105.

Resnik, M. (1985). How nominalist is Hartry Field's nominalism? *Philosophical Studies*, 47(2), 163-181.

Resnik, M. (1995). Scientific vs. mathematical realism: the indispensability argument. *Philosophia Mathematica*, 3(2), 166-174.

Resnik, M. (1997). *Mathematics as a science of patterns*. Oxford: Clarendon Press.

Rodriguez-Pereyra, G. (2006). Truthmakers. *Philosophy Compass*, 1(2), 186-200.

Rosen, G. (2005). Problems in the history of fictionalism. In Kalderon, M.E. (ed.) *Fictionalism in metaphysics*. Oxford: Clarendon Press, pp. 14-64.

Saatsi, J. (2011). The enhanced indispensability argument: representational versus explanatory role of mathematics in science. *The British Journal for the Philosophy of Science*, 62(1), 143-154.

Sainsbury, R.M. (2010). *Fiction and fictionalism*. London: Routledge.

Schmitt, F.F. (1995). *Truth: a primer*. Boulder: Westview Press.

Shapiro, S. (1997). *Philosophy of mathematics: structure and ontology.* New York: Oxford University Press.

Shapiro, S. (2000). *Thinking about mathematics: the philosophy of mathematics*. Oxford: Oxford University Press.

Sider, T. (2009). Ontological realism. In Chalmers, D.J., Manley, D. and Wasserman, R. (eds.) *Metametaphysics: new essays on the foundations of ontology.* Oxford: Clarendon Press, pp.384-423.

Sider, T. (2011). *Writing the book of the world.* Oxford: Oxford University Press.

Sober, E. (1993). Mathematics and indispensability. *The Philosophical Review*, 102(1), 35-57.

Stanley, J. (2001). Hermeneutic fictionalism. *Midwest Studies in Philosophy*, 25(1), 36-71.

Steiner, M. (1989). The application of mathematics to natural science. *Journal of Philosophy*, 86(9), 449-480.

Steiner, M. (1998). *The applicability of mathematics as a philosophical problem*. Cambridge: Harvard University Press.

Tallant, J. (2013). Pretence, mathematics, and cognitive neuroscience. *The British Journal for the Philosophy of Science*, 64(4), 817-835.

Vaihinger, H. (1935). *The philosophy of 'as if': a system of the theoretical, practical and religious fictions of mankind*. London: Routledge & Kegan Paul ltd.

Van Fraassen, B.C. (1980). *The scientific image*. Oxford: Clarendon Press.

Van Inwagen, P. (1998). Meta-ontology. *Erkenntnis*, 48(2-3), 233-250.

Walton, K.L. (1990). *Mimesis as make-believe: on the foundations of the representational arts.* Cambridge: Harvard University Press.

Walton, K.L. (1993). Metaphor and prop oreinted make-believe. *European Journal of Philosophy*, 1(1), 39-57. Reprinted in Kalderon, M.E. (ed.) (2005) *Fictionalism in metaphysics*. Oxford: Clarendon Press, pp. 65-87.

Wigner, E. (1960). The unreasonable effectiveness of mathematics in the natural sciences. *Communications on Pure and Applied Mathematics,* 13(1), 1-14.

Woodward, R. (2012). A Yablovian dilemma. *Thought: A Journal of Philosophy*, 1(3), 200-209.

Yablo, S. (1998). Does ontology rest on a mistake? *Proceedings of the Aristotelian Society, Supplementary Volumes*, 72, 229-261.

Yablo, S. (2001). Go figure: a path through fictionalism. *Midwest Studies in Philosophy*, 25(1), 72-102.

Yablo, S. (2005). The myth of the seven. In Kalderon, M.E. (ed.) *Fictionalism in metaphysics*. Oxford: Clarendon Press, pp. 88-115.

Yablo, S. (2012). Explanation, extrapolation, and existence. *Mind,* 121(484), 1007-1029.