

Towards a Fictionalist Philosophy of Mathematics

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Abstract

In this thesis, I aim to motivate a particular philosophy of mathematics characterised by the following three claims. First, mathematical sentences are generally speaking false because mathematical objects do not exist. Second, people typically use mathematical sentences to communicate content the truth of which does not require mathematical objects to exist. Finally, in using mathematical language in this way, speakers are not doing anything out of the ordinary: they are performing straightforward assertions. In Part I, I argue that the role played by mathematics in our scientific explanations is a purely expressive one, merely allowing us to say more than we otherwise would be able to about, or yielding a greater understanding of, the physical world. Mathematical objects do not need to exist for mathematical language to play this role. This proposal puts a normative constraint on our use of mathematical language: we *ought* to use mathematically presented theories to express belief only in the consequences they have for non-mathematical things. In Part II, I will argue that what the normative proposal recommends is in fact what people generally do in both pure and applied mathematical contexts. I motivate this claim by showing that it is predicted by our best general means of analysing natural language. I provide a semantic theory of applied arithmetical sentences and show that they do not purport to refer to numbers, as well as a pragmatic theory for pure mathematical language use which shows that pure mathematical utterances do not typically communicate content that implies the existence of mathematical objects. In conclusion, I show the hermeneutic fictionalist position that emerges is preferable to any alternative which interprets mathematical discourse as aimed at describing a domain of independently existing abstract mathematical objects.

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Introduction

Positing the existence of mathematical objects, such as numbers, sets and functions, yields apparent theoretical benefits. For instance, it seems to allow for a unified interpretation of mathematical and non-mathematical discourse. Much of non-mathematical discourse is best interpreted as describing a domain of independently existing objects. This goes for everyday language—‘There is some beer in the fridge’—as well as scientific language—‘There is a black hole in the centre of that galaxy’. These sentences mention beers, fridges, black holes and galaxies, and assertoric uses of these sentences are aimed at describing these objects. Moreover, we would like to think that such assertions are sometimes true.

It is natural and desirable to treat our mathematical language as continuous with our non-mathematical language, and so to interpret mathematical language along the same lines. This goes for pure mathematical language—‘There is an even integer between 10 and 20’—as well as applied mathematical language—‘The mass of the brick in kilograms is 10’. These sentences appear to mention numbers, and so it appears that assertoric uses of them are in part aimed at describing numbers. Moreover, we would like to think that many of our mathematical assertions are true. Believing in the existence numbers allows us to provide a unified interpretation of mathematical and non-mathematical discourse.

However, asking after the nature of mathematical objects raises serious difficulties. We appear to have a decent enough grasp of what fridges and beers are, and some of us have a good idea of what black holes and galaxies are; but what are mathematical objects? The common view is that they are non-causal and not spatially located. Call such objects *abstract* (see Rosen 2012 for discussion of the

abstract/concrete distinction); and call the view that there are abstract mathematical objects *platonism*. The view that mathematical objects have no location and do not causally interact with the world has initial but weak support from science and mathematics:

The scientific literature contains no reference to the location of numbers or to their causal efficacy in natural phenomenon or how one could go about creating or destroying a number. There is no mention of experiments to detect the presence of numbers or determine their mathematical properties.

Such talk would be patently absurd. (Shapiro 2000: 27)

The view is also supported by its apparent ability to account for widely held assumptions about mathematics. Most would agree that at least elementary mathematical sentences, such as ' $2+2=4$ ', are true and could not have been false. If the objects described by such sentences are independent of the contingent goings on of the physical world, then our intuitions here are veridical.

Platonism also appears to be able to account for what makes for good mathematics. Why do mathematicians accept some theories and reject others? The platonist answers: they accept those that are, *inter alia*, true and reject those that are false.

Our first difficulty arises here. The nature of mathematical objects precludes us from interacting with them, raising difficult questions: How can we gain knowledge of such entities? How can we be sure our beliefs about them are reliable? How do our words and thoughts manage to be about them in the first place? How do beings of flesh and blood study a realm that is non-causal and non-spatial? (See

Benacerraf 1973 for the classic epistemological objection to platonism.) Another difficulty arises from the fact that there is more than one sequence of mathematical objects suitable for identifying with the natural numbers, and there appears to be no way of deciding which sequence we are talking about when we talk about the natural numbers (cf. Benacerraf 1965).

Platonism appears to raise as many problems as it promises to solve. Rather than revealing that the view is untenable, however, it is better to see these problems as an explanatory debt taken on by the platonist; one that, for all we know, it may be possible to pay off. However, if there is an independently motivated alternative to platonism for which these issues do not arise, then these difficulties count in favour of abandoning platonism in favour of this alternative.

One alternative to platonism involves denying that mathematical objects exist. Call this view *nominalism*. If there are no mathematical objects, then there is no need for a philosophy of mathematics to explain how knowledge of abstracta is possible. In light of the considerations mentioned in platonism's favour, however, nominalism appears to throw the baby out with the bath water. We can no longer appeal to the existence of mathematical objects to secure an objective subject matter for mathematics, and if we want to retain a unified interpretation of language, we are forced to accept that the world is not how mathematical sentences say it is, so mathematical sentences are generally speaking false. This conclusion seems to fly in the face of our strong intuition that many mathematical sentences could not be false. It also threatens to make the practice of accepting and rejecting mathematical theories mysterious.

The most formidable objection to nominalism is the *indispensability argument for platonism*, which can informally be expressed as follows. Most of us

are willing to make the leap from believing in the medium-sized dry goods that populate our environment to believing in the unobservable physical objects that science tells us constitute our environment, such as quarks and electrons (henceforth: *unobservable concreta*). That is, most of us are willing to adopt some form of *scientific realism*, and there are good reasons for doing so: talk of such objects is indispensable to our best explanations of observable phenomena. However, talk of abstract mathematical objects is also indispensable to our best explanations, so most of us should be willing to make the further leap to believing that mathematical objects exist. It seems that nominalism faces challenges no less serious than those facing platonism.

Nevertheless, I aim to establish in this thesis that a particular brand of nominalism is preferable to platonism. The position I will argue for is a kind of *hermeneutic fictionalism*. Hermeneutic fictionalism is a descriptive proposal about how mathematical language is actually used. The central claims of my position can be summarised as follows. First, mathematical and non-mathematical language is best interpreted continuously. Second, non-trivial mathematical sentences are false because mathematical objects do not exist. Third, mathematical sentences are typically used to assert content the truth of which does not require mathematical objects to exist. Fourth, in using mathematical sentences in this way, speakers are not doing anything out of the ordinary; they are making straightforward assertions.

Hermeneutic fictionalism is naturally contrasted with *revolutionary fictionalism*, which is a proposal about what our attitude towards mathematics *should* be, or how we *should* use mathematical language. In this thesis, I will use the term ‘fictionalism’ to mean revolutionary fictionalism and refer to hermeneutic fictionalism with its full name.

There are two challenges facing specifically the kind of hermeneutic fictionalism I propose. First, a sufficient characterisation must be given of the content communicated by typical mathematical utterances. Second, an account must be given of how speakers use sentences that say one thing to mean another, without thereby performing some kind of special speech act.

To establish my proposed conclusion, I must at least show that the prospects for meeting the challenges facing my hermeneutic fictionalism are better than the prospects for meeting the challenges facing platonism. I will do this by pursuing two lines of argument, corresponding to the two parts of the thesis.

In Part I, I will argue that the best account of the role of mathematics in scientific explanations is that it plays a purely expressive one. On this view, the indispensability of mathematics is best explained by the fact that mathematics allows us to represent more features of the physical world than we would otherwise be able to. Mathematical objects do not need to exist for mathematics to play this role, so the indispensability argument fails. Importantly, this proposal implies a normative or revolutionary claim: given the role that mathematics plays in science, we *ought* to use mathematically formulated scientific theories to express belief only in what those theories say about the physical world. This claim is compatible with the view that people do not ordinarily use mathematics in this way.

In Part II, I will argue that there are good reasons for thinking that people generally do use mathematics in this way. Moreover, I argue for an analogous claim about utterances made in the context of pure mathematics. I do this by outlining and defending a general philosophy of language that prescribes a particular means of analysing natural language. According to this view, the meanings of the sentences we utter play a minimal role in our communicative life. Working out what is under

discussion in a given discourse therefore involves both semantic and pragmatic analysis. That is, it involves paying attention to both what the sentences people utter mean and the purposes for which they utter them. I go on provide an independently motivated semantic and pragmatic analysis of mathematical discourse. My analysis predicts that speakers typically use mathematical sentences to communicate content that does not imply the existence of mathematical objects.

In the conclusion, I show that the account of mathematical discourse developed in Part II, combined with nominalism, yields a hermeneutic fictionalist philosophy of mathematics that is preferable to platonism. The obvious means to contrast them is in terms of their respective ontologies and epistemologies: platonism is metaphysically baroque and must be supplemented with an account of how we can know about abstract objects, while the hermeneutic fictionalist posits fewer entities and is not required to give an epistemology of mathematical objects at all.

More surprising is how they compare with respect to the analysis of language. The linguistic considerations brought to light in Part II reveal that the analysis of mathematical and non-mathematical discourse presupposed by my hermeneutic fictionalism is in fact more unified than that offered by platonism. Moreover, despite first appearances, it is harder for platonism to explain both the process by which mathematicians choose theories and our intuitions about the truth of elementary mathematical sentences. I finish by suggesting further applications of the arguments presented in this thesis.

I

The Applications of Mathematics

1. Introduction to Part I: Two Roads to Nominalism

1.1 The indispensability argument

In this first part of the thesis, I will argue that the indispensability argument for platonism fails. To this end, it will be helpful to have a specific formulation of the argument to refer back to. Presenting and justifying a particular formulation is the task of this section. In §1.2, I outline Hartry Field's proposed methodology for responding to the argument, and explain why it is not a promising approach, given the aims of this thesis. In §1.3, I describe the structure of Part I.

The indispensability argument is typically attributed to Quine (1948) and Putnam (1979). (Those who attribute the argument to Putnam include: Field 1980: 107 fn. 4; Maddy 1990: 29-30; Melia 2000: 455; and Colyvan 2001: 10. Those who attribute it to Quine include: Maddy 1990: 29; Papineau 1993: 191-2; and Colyvan 2001: 10.) Hence, it is sometimes called the *Quine-Putnam indispensability argument*. However, Michael Resnik (1997: 45 fn. 3) and David Liggins (2008) cast doubt on whether the argument attributed to Quine and Putnam is the argument they intended to convey. The argument often but perhaps mistakenly attributed to these authors is the argument I am interested in, so to avoid the need for Quine and Putnam scholarship, I shall continue to refer to it with its more neutral name.

Before presenting the argument, I should first say more about whom the argument is for. The indispensability argument is aimed at those who are already convinced by a certain class of arguments in favour of scientific realism. These arguments have a common thread: they highlight that talk of unobservable concreta is indispensable to successful scientific practice, and then argue that the best explanation of this fact is that unobservable concreta exist. Perhaps the most famous

of these arguments is the *no-miracles argument*, which says that scientific realism ‘is the only philosophy that doesn’t make the success of science a miracle’ (Putnam 1979: 73). The indispensability argument is supposed to show that these considerations equally support platonism.

I should also make explicit in what sense mathematics is supposed to be ‘indispensable to science’. This means that mathematics is indispensable to certain ontologically relevant scientific purposes, such as explanation and prediction. This specification is important. Talking about Elves and Dwarves is indispensable to the purpose of describing the plot of *The Lord of the Rings*, but this purpose is not relevant to ontology. In contrast, the thought is that, for a description of how the physical world is made up to make such accurate predictions and to explain such a wide range of seemingly disparate observable phenomena, it must in some sense reflect how things really are. Assuming that prediction and explanation are among the central purposes of scientific practice, I will henceforth abbreviate the claim that mathematics is indispensable to scientific explanation and prediction by saying that mathematics is indispensable to science. What does it mean for mathematics to be indispensable in this way? The following characterisation will suffice. It means that reformulating science so that it is free of mathematical language results in less predictively successful or less explanatory theories. Despite Hartry Field’s (1980; 1989) brilliant and insightful attempts to show otherwise, mathematics does indeed appear to be indispensable to science. I will discuss Field’s work in some detail in §1.2.

Another important part of the argument that I should make explicit is what aspects of mathematical language it appeals to. Since the existence of such things is what is at issue, a formulation of the indispensability argument that appeals to the

indispensability of mathematical objects begs the question against the nominalist. On the other hand, merely saying that mathematical language is indispensable to science does not reveal why this would be relevant to ontology. For these reasons, the indispensability argument should appeal to the indispensability of *apparent reference to mathematical objects*. Importantly, this assumes that mathematical language contains expressions that purport to refer to mathematical objects. In Chapter 6, I show that the amount of mathematical language used in science that contains such expressions has been over-estimated. Nevertheless, there is still a substantial fragment of mathematical language used in science that does purport to be about mathematical objects.

Finally, I should discuss the vagaries of *ontological commitment*. In the formulation of the indispensability argument often attributed to Quine, ontological commitment is something enjoyed by theories. To find out what a theory is ontologically committed to, Quine proposes the following process (see Liggins 2008: 116-117 for an accessible outline of Quine's ontological method). First, re-formulate the theory using a precise canonical language (Quine recommends first-order predicate logic). Second, draw out all of the existentially quantified entailments of the form ' $\exists x Fx$ '. Finally, work out which objects must serve as values of the variables of these formulas for them to be true. These objects are what must exist for the canonical re-formulation of the theory as a whole to be true, and so they constitute the ontological commitments of the theory.

It should be noted that, because Quine was suspicious of the notions of analyticity and synonymy, he did not regard the canonical re-formulations of scientific theories as meaning-preserving translations of the original theories. Instead, they are considered philosophically useful re-formulations because they

render obvious which objects must exist for them to be true. On this view, our scientific theories themselves do not have ontological commitments—only their re-formulations do. Theories provide us with reasons to believe in these commitments only insofar as they admit of the relevant re-formulation. Theorists are generally no longer sympathetic to Quine’s arguments against analyticity and synonymy (see Russell 2008 for a defence of the analytic/synthetic distinction in light of developments in contemporary philosophy of language). So, most contemporary presentations of the indispensability argument assume that our scientific theories have ontological commitments directly by including expressions that purport to refer to mathematical objects. I will follow suit. If a theory or sentence contains expressions which purport to refer to mathematical objects, I will say the theory or sentence is committed to mathematical objects. Equivalently, I will say that the theory or sentence entails the existence of mathematical objects, since it cannot be true without some mathematical object or objects existing.

The notion of ontological commitment is also sometimes applied to people. People take on ontological commitments when they endorse theories, the most straightforward assumption being that people take on all of the ontological commitments of the theories they endorse.

To present my own formulation of the indispensability argument, it will be useful to contrast it with Mark Colyvan’s influential formulation:

Colyvan’s argument

CA1. We ought to have ontological commitment to all and only those entities that are indispensable to our best scientific theories;

CA2. Mathematical entities are indispensable to our best scientific theories.

Therefore:

CA3. We ought to have ontological commitment to mathematical entities.

(Colyvan 2001: 10)

There are several aspects of this formulation of the argument I wish to avoid in my own. The most salient is that it is framed in terms of objects being indispensable to science. Colyvan is presumably abbreviating for the more longwinded version in terms of apparent reference, but to avoid any misunderstanding, my own formulation will be framed more carefully.

Another feature I wish to avoid is the deviant use of ‘ontological commitment’. Colyvan is employing a normative notion of ontological commitment that applies to people, according to which we choose what we are committed to, but we *should* be committed to all and only those entities apparent reference to which is indispensable to science. This strikes me as a confusing use of the term. It would mean that ontological commitment is descriptive with respect to theories and normative with respect to people.

However, I do like the normative dimension of the argument, since it highlights that the argument is supposed to force the scientific realist to adopt platonism on pain of irrationality. A better way of capturing this is to highlight the normative constraints that ontological commitment places on our beliefs: we ought to believe in all of those entities we are ontologically committed to. That is, we ought to believe in all those entities to which the theories we endorse are ontologically committed. It might be argued that the relationship between the commitments of the theories we endorse and rational belief is more complex than this. If someone were to endorse an inconsistent set of theories, then they would

perhaps be rationally required not to believe in all the commitments of those theories. For simplicity's sake, I will avoid this complication in my formulation of the indispensability argument. Nothing I argue in this thesis rests on this choice.

The final aspect of Colyvan's argument I wish to avoid is the strength of the first premise, CA1. The premise has two parts. One says that we are committed to all those entities apparent reference to which is indispensable to science (the 'all' part); the other says that we are committed to only those entities (the 'only' part'). I want to avoid both these parts in favour of a weaker and more plausible premise.

As with other theorists who defend a Quinean formulation of the argument (e.g. Bangu 2012: 16; Resnik 1995, 1997: 44-5), Colyvan invokes two Quinean doctrines in support of premise one: *Quinean naturalism* and *confirmational holism*. The first of these is supposed to justify the 'only' part of the first premise, and the latter is supposed to be what justifies the 'all' part. Quinean naturalism is sometimes stated innocuously as the claim that philosophy is continuous with science, a sentiment I doubt many philosophers today would disagree with. However, the strength of Quinean naturalism is better captured by the slogan that science is the 'ultimate arbiter of truth and existence' (Resnik 1995: 166; 1997: 45). That is, the only good reasons for thinking something is true, or that something exists, are scientific reasons. I do not want to discuss the plausibility of this claim; I only want to point out that it is very strong and not necessary for the formulation of the indispensability argument.

Confirmational holism is the claim that evidence confirms whole theories, not just parts of them. It is supposed to be what justifies the 'all' part of CA1. Many of the most important objections to the indispensability argument have been attacks on conformational holism (see Sober 1993 and Maddy 2005; Glymour 1980 attacks

conformational holism from within the philosophy of science). More recently, Joe Morrison (2012) has argued that, whilst some other forms of confirmational holism are plausible, the form that is required to support the indispensability argument is not plausible and has never been established by argument. For the above reasons, I will avoid the whole of CA1 in Colyvan's argument.

Thankfully, there is a far weaker and more plausible premise that can take its place. Recall that the argument is supposed to force a scientific realist into adopting platonism by using her own reasoning against her. Clearly the scientific realist is impressed with the role that talk of unobservable concreta plays in our scientific theories: she believes unobservable concreta exist because it plays that role. So, for the indispensability argument to do its job, it need only appeal to this role, whatever role it is. Scientific realists believe that all objects apparent reference to which is indispensable to, and plays the same role as talk of unobservable concreta in, science exist. If apparent reference to mathematical language plays the same role, then scientific realists are committed to the existence of mathematical objects.

I am now in a position to present the formulation of the indispensability argument that I will use for the remainder of this thesis.

IA

IA1: We (scientific realists) are committed to all those entities apparent reference to which is indispensable to, and plays the same role as apparent reference to unobservable concreta in, the scientific theories we endorse.

IA2: Apparent reference to mathematical entities is indispensable to, and plays the same role as apparent reference to unobservable concreta in, the scientific theories we endorse.

IA3: We are committed to mathematical entities. (From IA1 and IA2)

IA4: We should believe in all those entities to which we are committed.

IA5: We should believe in mathematical entities. (From IA3 and IA4)

I will finish this section with a few comments about this argument. First, it suffers none of the drawbacks of Colyvan's argument: it relies neither on Quinean naturalism nor confirmational holism; it imbues 'committed to' with its more intuitive descriptive sense; it retains the normative nature of the argument, implying that it is irrational to be a scientific realist who rejects platonism; and it doesn't beg the question by talking about mathematical objects being indispensable to science. Second, the meaning I have given to 'committed to' ensures that, once IA3 is established, the day is won for the platonist. The heavy lifting in this argument therefore occurs in IA1 and IA2. Rejecting IA1 or IA2 therefore amount to the only means of replying to IA. Field (1980; 1989) famously tried to undermine the indispensability argument by attempting to show that mathematics is in fact dispensable to science. If he were successful, the first conjunct of IA2 would be false, so IA would be unsound. In the following section, I explain why Field's response is not a promising one, if it is to serve as the foundation for a philosophy of mathematics.

1.2 The hard road to nominalism

Field (1980; 1989) attempts to achieve two tasks that are similar to those I aim to achieve in this thesis: to demonstrate that the indispensability argument fails; and to defend a fictionalist philosophy of mathematics. The only difference at this level of detail is that I intend to defend a kind of hermeneutic fictionalism. To better motivate

my own means of doing this, it will be instructive to outline Field's response to the indispensability argument and his defence of fictionalism so as to explain why Field's approach is not a promising one.

First: a reminder about fictionalism. The kind of fictionalism Field defends is *revolutionary fictionalism*. Where hermeneutic fictionalism is a descriptive thesis about the way in which people actually use mathematics, revolutionary fictionalism is a normative thesis about how we *should* use mathematics. For the remainder of this thesis, I will use 'fictionalism' to refer to revolutionary fictionalism, giving hermeneutic fictionalism its full name.

Any formulation of the indispensability argument worthy of the name involves the crucial claim that mathematics is indispensable to science. It features in IA as the first conjunct of IA2. It is this claim that Field (1980) tries to undermine. He demonstrates techniques with which he hopes we can rewrite our scientific theories without mathematics while suffering no loss of explanatory power or predictive success. The programme he thereby recommends for those wishing to reject the indispensability argument is a long and arduous one: a theory-by-theory reformulation of science. Because of this, Colyvan (2010) calls this means of replying to the indispensability argument the 'hard road to nominalism'. Much of the philosophy of mathematics published in the decades following Field (1980; 1989) is preoccupied with the prospects for completing Field's programme. The consensus now is that it is host to numerous insurmountable technical difficulties, and so is unlikely to succeed (see MacBride 1999 for a good survey).

Aside from the indispensability argument, Field claims that one of the main obstacles facing fictionalism is the fact that it looks unable to explain the applicability of mathematics in science (1989: 3-4). It seems as though the platonist

can appeal to the fact that mathematics is true to explain its applicability. On this view, mathematics is applicable in science because it accurately describes a part of reality that is crucial to understanding and explaining the physical world. Since the fictionalist believes that no mathematical sentences are non-vacuously true, she cannot appeal to the same explanation, and so needs to motivate an alternative.

Field's proposed alternative crucially involves two claims. The first claim is that mathematics is dispensable to science. The second claim is that mathematics is conservative over theories that only mention concreta (nominalistic theories). This second claim needs unpacking.

A theory T' is conservative over another theory T if and only if any consequence of T' that can be expressed in the language of T is also a consequence of T . Two important but tricky qualifications are needed here: that the nominalistic theories Field has in mind are second-order theories, and that the notion of consequence Field has in mind is a semantic one, as opposed to a deductive one (Field 1980: 115 fn. 30 is explicit that this is how his claim is intended). The difference between semantic and deductive consequence is as follows: a sentence S is a semantic consequence of a theory T if and only if it is not logically possible for T to be true while S is false; a sentence S is a deductive consequence of a theory T if and only if S is provable in T . The reason Field needs these qualifications is that he takes there to be a nominalistic theory in which the axioms of Peano's Arithmetic (PA) can be proved. Call this nominalistic theory 'NPA'. Kurt Gödel's (1931) incompleteness theorems show that any theory embodying the axioms of PA is incomplete, meaning there is a sentence which is expressible in it (e.g. the sentence that says it is consistent) that cannot be proved within it. This sentence will be provable in set theory, however. So, Field is committed to there being a sentence

expressible in NPA that cannot be proved within NPA, but whose mathematical equivalent can be proved using set theory. If we call the notion of conservativeness which assumes a deductive notion of consequence *deductive conservativeness*, we can conclude that mathematics is not deductively conservative over nominalistic theories.

However, the incompleteness theorems also show that, for second-order languages, the notions of semantic and syntactic consequence do not extensionally coincide, such that there are some semantic consequences of second-order theories that are not also deductive consequences of them. So, if NPA is a second-order theory, then the fact that there is a sentence expressible in NPA that cannot be proved without mathematics does not entail that this sentence is not a semantic consequence of NPA. If we call the notion of conservativeness which assumes a semantic notion of consequence *semantic conservativeness*, Field's second claim on which he bases his alternative explanation of the success of science is that mathematics is semantically conservative over second-order nominalistic theories (see MacBride 1999: 448-449 for more detail; see also Leng 2010: 48-50, 75 fn. 2).

If these two claims are true, then for each scientific theory, there is a nominalistic version that is just as explanatory and predictively successful. Applying mathematics to each of these nominalistic theories would merely facilitate a more efficient means of drawing out the consequences of these nominalistic theories. So, on this view, mathematics is useful in science because it acts as a kind of inference facilitator.

Mathematics could be conservative over nominalistic theories without being dispensable to science. It may be that there are no nominalistic premises sufficient for deriving enough scientifically interesting conclusions for mathematics to count as

dispensable to science. The tasks of demonstrating the conservativeness and the dispensability of mathematics to science are therefore separate (cf. MacBride 1999: 435).

To demonstrate that mathematics is semantically conservative Field provides a set-theoretic proof and a philosophical argument (1980: 16-19). I do not intend to discuss the formal details of Field's arguments, so I will omit discussion of Field's proof. (See Melia 2006: 205-208 for an argument that Field's proof does not have the philosophical import that Field attributes to it.) However, it is worth considering the philosophical argument. It takes the form of a *reductio ad absurdum*. Take S to be a body of mathematics, N to be a nominalistic theory, and A to be a nominalistic sentence. If S and N together entail A , and N alone does not entail A , the truth of S would hinge on $N \& \neg A$ not being true. This would mean that the truth of mathematics would depend on a contingent body of truths, which runs counter to our intuitions about mathematics as compatible with any possible state of the physical world.

Here is an example based on Field's (1989: 57). Imagine a form of Zermelo-Fraenkel set theory that, instead of the axiom of infinity, contains its negation. Call it ZF' . ZF' is consistent and rich enough in its consequences to be of mathematical interest. However, it has consequences that are at odds with how the physical world is or might have been (Field 1989: 57). There are theories in physics that postulate infinitely many non-mathematical objects between two points. Call one such theory P , and let us say it postulates an infinite number of non-mathematical objects between points p and q . If ZF' is applied to P , then a function will be definable that maps the infinitely many non-mathematical objects between p and q onto sets. The axiom of comprehension will ensure that these sets will form the members of a set.

So, the application of ZF' to P yields a definition of a set containing infinitely many sets, contradicting one of the axioms of ZF' , namely the negation of the axiom of infinity. Even if physical theories that posit infinite sequences of objects are not true, they might have been. That a mathematical theory can have consequences for the physical world that might have been false runs counter to our intuitions (Field 1989: 57).

To argue that mathematics is dispensable to science, Field (1980) provides nominalized versions of Newtonian Gravitational Theory (NGT) and Euclidian Geometry (EG). This choice may seem odd, since NGT and EG are inaccurate and outdated theories. However, Field intends only to demonstrate techniques that he hopes will be applicable to contemporary scientific theories. Field is under no illusion that this is enough to demonstrate that mathematics is dispensable; it is the first step in a long and difficult programme that is only complete once we have nominalized all of our best scientific theories, and are sufficiently reassured that no future scientific theory may resist such treatment.

There are two respects in which the tenability of fictionalism is supposed to rest on the completion of Field's programme. First, by showing that mathematics is dispensable to science, it would allow the fictionalist to reject IA. Second, along with the claim that mathematics is conservative over nominalistic theories, it allows the fictionalist to explain the applicability of mathematics in science. Recall that according to Field these are the two main obstacles facing fictionalism.

The hard road is extremely difficult and it looks as though there is no guarantee that it will succeed. And yet, Field recommends we attempt to take the hard road. What motivation does Field have for this?

Field argues that explanations involving reference to numbers are not as satisfying as those that do not (see for example Field 1989: 18-19). His reasons are that such explanations make reference to something that is not causally responsible for the phenomenon being explained, and, relatedly, which numbers we invoke in such explanations appear to be based on an arbitrary, conventional choice of scale. He calls these kinds of explanations *extrinsic*. According to Field, explanations that only make reference to causally relevant entities are more satisfying, and are *intrinsic*. Field takes these considerations justify the following principle:

EP: Underlying every good extrinsic explanation, there is an intrinsic explanation (1980: 44-45; 1989: 18)

He appeals to this principle to argue that there are nominalistic versions of our best scientific theories available (1980: 43-46; 1989: 18-19, 19-193). According to Field, then, the hard part of the hard road is just finding them. (Milne 1986 argues that the considerations Field appeals to do not support EP because the nominalistic explanations Field endorses as ‘intrinsic’ are also the result of arbitrary and conventional decisions about the way distance and magnitude are to be measured.)

The key aspects of Field’s philosophy of mathematics are now in place. To summarise, Field defends fictionalism against what he sees as its two main obstacles. By far the most serious obstacle is that mathematics appears to be indispensable to science. He tries to overcome this by demonstrating techniques that he argues will be sufficient for nominalizing the whole of science. To justify such a demanding programme for defending fictionalism, Field appeals to EP, the claim that underlying every good extrinsic explanation, there is an intrinsic explanation. The search for

nominalistic scientific theories can then be regarded as the search for intrinsic versions of our best scientific explanations. The second obstacle is to explain the applicability of mathematics in science without appealing to its truth. Field attempts to avoid this obstacle by appealing to both the dispensability of mathematics to science, and its being conservative over the nominalistic versions of our scientific theories. This characterises the utility of mathematics as that of facilitating the drawing out of consequences of our nominalistic theories.

As I have mentioned, the programme of nominalization recommended by Field is host to a variety of apparently insurmountable technical difficulties, and so the consensus is that it is not promising. For the purposes of this thesis, there would be little point in recounting these difficulties (again, I will merely point in the direction of a good survey: MacBride 1999). However, I will present two objections to Field that are more relevant, since they suggest that a different approach should be taken to undermining the indispensability argument and defending fictionalism. The first questions Field's motivation for thinking that taking the hard road is possible; the second suggests that even if it were possible, the kind of fictionalism Field would thereby have defended is deficient.

Recall that Field's motivation for thinking that it is possible to take the hard road rests on EP. My first objection falls out of a distinction between two notions of explanation. One concerns the explanations that we possess and are able to formulate. This notion concerns our theories. Explanations in this sense are interpreted sentences that we take to be illuminating with respect to some phenomenon. The question of what makes for a good explanation in this sense is a difficult and controversial one, yet it is clear that Field is assuming a view on this. His claim that extrinsic explanations are less satisfying than intrinsic explanations

indicates that he assumes that explanations should aim to mention only what is causally responsible for the phenomenon to be explained. (We shall see in chapter 4 (§4.2) that this assumption is false.)

The second notion of explanation concerns what is ultimately responsible for the production of the phenomenon. This notion concerns reality. There is a principle, seemingly similar to that which Field endorses, except that it concerns the present notion of explanation as opposed to the notion concerning our theories:

RP: Underlying every explanation, there is some causal process that is ultimately responsible for the phenomenon being explained. (cf. Kim 1993)

This is a plausible claim. I suspect that Field is conflating EP with RP. The truth of RP does not ensure that underlying every causal explanation there is an explanation that we can formulate which only mentions that which is causally responsible for the phenomenon. It therefore doesn't guarantee that mathematics is dispensable to science. Rather, the apparent indispensability of mathematics gives us reason to think that 'our intuitions concerning what it takes for an explanation to be satisfying are not a reliable guide to what explanations there are' (MacBride 1999: 435-6). What explanations there are is partly determined by facts about our theories and our mental and linguistic capacities. Similarly, whether or not mathematics is dispensable to our explanations is a fact about how it is possible for us to express explanations. Both of these are dependent on accidental mental and linguistic capabilities of humans. There is no guarantee that our expectations about what explanations are satisfying will be met by these capabilities.

My second objection is inspired by Stephen Yablo's (2005) discussion of Field. Yablo alludes to a fundamental problem about Field's motivation for pursuing his project. He begins by describing Field's project on the basis of what he calls a 'deliberate misunderstanding' (2005: 225). The supposed misunderstanding is to view the project as that of trying to provide a satisfactory explanation of the applicability of mathematics. On this view, Field's project can be seen as a response to the following question about applicability:

(d-app) How are actual applications of mathematics to be understood, be it indispensable or not? (adapted from Yablo 2005: 226)

According to Yablo, this is not what Field is concerned with: his arguments are aimed at undermining the indispensability argument by showing that mathematics is dispensable. The 'indispensable or not' part of (d-app) clearly renders it distinct from Field's worries. According to Yablo, the question Field is primarily concerned with is the following:

(d-ind) How can applications be conceived so that talk of mathematical objects comes out dispensable? (adapted from Yablo 2005: 226)

An answer to this question need say nothing at all in answer (d-app). Imagine for a moment that Field is right. Suppose there really are nominalistic versions of all of our scientific theories waiting to be discovered and that mathematics is conservative over these nominalistic theories. This would present us with an explanation of how mathematics is applicable outside of its domain when applied to these nominalized

theories: it facilitates the drawing out of nominalistic consequences from our nominalistic theories.

However, this is not how mathematics is standardly applied. In science, we make inferences from mathematically stated premises to mathematically stated conclusions. Field's explanation of the utility of mathematics when applied to nominalistic theories does not help us understand how mathematics works in actual applications.

Yablo's initial objection, based on the misunderstanding, is that Field fails to answer (d-app); but if Field only intended to answer (d-ind), then this is no objection at all. So, Yablo revises his objection. He claims that (d-app) is the more important question, and that Field neglects to say anything about (d-app) (2005: 226). The philosophy of mathematics should be concerned with how we actually use mathematics, not how we might use it, were our practices different. If we reflect on the reasons behind Field's confidence in the success of his project, we see that Yablo's revised objection is very astute.

Above, I argued that Field's confidence in his project stems from a lack of confidence in platonism. Platonism has certain explanatory deficits that fictionalism promises to fill; but now we have identified a devastating explanatory deficit that Fieldian fictionalism suffers. It appears unable to explain the utility of mathematics *as it is actually used*. This is something that any plausible philosophy of mathematics should explain.

It would be uncharitable to assume that Field has nothing to say about actual applications. If it turns out that mathematics is dispensable to science, then the question we are concerned with will change:

(d-app') How are actual applications of mathematics to be understood, given that it is dispensable?

Field could argue that (d-app') is a more tractable question than (d-app), and so accepting fictionalism is an important step on the way to our best philosophy of mathematics. If so, we are owed an explanation. We have on one hand a theory such as NGT, and on the other we have a Fieldian reformulation of it. In order for the explanation of the applicability of mathematics to the latter to bear on our understanding of the way mathematics is applied to the former, the two theories must be shown to be related in some appropriate way. For example, Field might argue that real applications and his artificial applications of mathematics are analogous. If this is what Field had in mind, we would expect him to have been more forthcoming about it, and to have made an attempt at explicating the nature of the analogy. Moreover, it is hard to see what the motivation would be for thinking that these two kinds of application are analogous.

However, let us give Field the benefit of the doubt and assume that his fictionalist philosophy of mathematics, once complete, would provide an adequate answer to (d-app'). If so, Field's claim that mathematics is dispensable to science is arguably irrelevant to the success of the resulting philosophy of mathematics. If we can explain the actual applications of mathematics in science without appealing to its truth, then why should we bother reformulating science without mathematics? (cf. Yablo 2005: 226).

In conclusion, Field's confidence in his programme was based on confusion between two notions of explanation, EP and RP. EP is implausible, but guarantees the success of Field's programme; RP is plausible, but does not. So, we have no

good reason for thinking that the completion of Field's long and arduous programme is possible, aside from the various technical difficulties it is subject to. This suggests that it is unwise to take the hard road, and wise to look for another way. Moreover, by Field's own lights fictionalism is only defensible if it provides an explanation of the applicability of mathematics to science; but showing that mathematics is dispensable to science and reconstructing applications does not promise to be relevant to the question of how actual applications of mathematics work. This suggests that a more direct route to a defensible fictionalist philosophy of mathematics is to relinquish the claim that mathematics is dispensable to science, and focus more directly on the actual applications of mathematics in science. This is not yet an appeal for hermeneutic fictionalism, as opposed to the revolutionary kind. In this first part of the thesis, I will argue that we *should* treat mathematics as a purely expressive device because of how actual applications of mathematics work. I save the motivation of hermeneutic fictionalism for Part II.

1.3 The (easy) road ahead

Recall my formulation of the indispensability argument:

IA

IA1: We (scientific realists) are committed to all those entities apparent reference to which is indispensable to, and plays the same role as apparent reference to unobservable concreta in, the scientific theories we endorse.

IA2: Apparent reference to mathematical entities is indispensable to, and plays the same role as apparent reference to unobservable concreta in, the scientific theories we endorse.

IA3: We are committed to mathematical entities. (From IA1 and IA2)

IA4: We should believe in all those entities to which we are committed.

IA5: We should believe in mathematical entities. (From IA3 and IA4)

The alternative means of undermining the IA I will pursue in the first part of this thesis is to reject IA2 by arguing that we should view the role played by mathematics in science as different to the role played by talk of unobservable concreta. This is an initially plausible claim: unobservable concreta are supposed to constitute the physical world in some way, while mathematical objects are supposed to exist independently of the contingent goings on of the physical universe. We would therefore expect that our theories assign unobservable concreta the role of constituting or causing observable phenomena. For instance, the physical interaction between the waves that constitute sunlight and the molecules that constitute the atmosphere is supposed to be what explains why the sky is blue. The role assigned to mathematical objects cannot be exactly like this. The explanation of why the sky is blue makes indispensable mention of the wavelength of blue light, which, in nanometres, is around 475; but it is not tempting to say that the number 475 is part of the sky or that it is in any way causally responsible for the behaviour of sunlight.

More specifically, the means by which I will undermine IA2 is a development of Joseph Melia's distinctive response to the indispensability argument (1995; 1998; 2000; 2002; 2003; 2008; 2011). Melia's arguments appeal to the claim that mathematics plays only a representational role in science, a role it can play whether or not mathematical objects exist. Call this claim *abstract expressionism*. (See Liggins 2014: 600; Melia 2000; Yablo 2001; and Leng 2010 for other defenders of abstract expressionism. See also Balaguer 1998: 19-89 for a sympathetic

presentation, though not quite endorsement of the view.) This is supposed to contrast with the role played by talk of unobservable concreta, which is that of describing the constitutional and causal aspects of the physical world responsible for observable phenomena.

I can now describe the structure of the first part of this thesis. In Chapter 2, I present Melia's attack on the indispensability argument, and endorse a modified version of one of Melia's arguments. The argument rests on two key premises: (i) that taking mathematics to play the same role as talk of unobservable concreta in science is tantamount to endorsing the view that physical magnitudes are, fundamentally, relations between physical objects and numbers—*heavy duty platonism* (HDP); (ii) that abstract expressionism is at least as plausible as HDP. Chapters 3 and 4 are concerned with justifying these two premises.

One way of justifying (ii) is to show that HDP is untenable. In Chapter 3, I show that this means of supporting Melia's argument is not promising, since HDP survives all objections to it stated and alluded to in the literature.

In Chapter 4, I outline three challenges that the platonist must meet to provide adequate support for the indispensability argument, and argue that the platonist cannot meet all three. This establishes that both (i) and (ii) are well-supported, so the modified version of Melia's argument is sound, and the indispensability argument fails.

2. The Weasely Way Round

2.1 Introduction

In conversation, we often say things like the following:

(ST) All the odd-numbered Star Trek films are excellent. Except *Nemesis*—
that film is terrible.

On the face of it, an utterance of (ST) is inconsistent. The speaker makes a generalisation and then immediately provides a counter-example; but it would be uncharitable to interpret speakers in this way. We should instead interpret speakers as consistent, if possible. According to Melia (2000: 467-468), such utterances are cases of a widespread phenomenon whereby we clarify what we meant in uttering a sentence by taking back some unwanted entailments of it. He calls this practice *weaseling*.

Melia claims that ‘almost all’ scientists would deny that there are abstract mathematical objects (2000: 469). That is, most scientists are *nominalists*. So, when scientists articulate theories which imply the existence of mathematical objects, they appear to be engaging in inconsistent behaviour. Again, Melia argues that it would be more charitable to assume that scientists are weaseling when they articulate their theories.

Why do scientists weasel if they can avoid it? It seems that weaseling is often unnecessary. For instance, instead of uttering (ST), we might instead list the good Star Trek films without mentioning the bad ones. Why can’t scientists similarly state their theories in a more straightforward way? Melia argues that sometimes we lack

the capacity to communicate the things we want to without weaseling. Mathematics allows us to say more things about the physical world than we would otherwise be able to, so scientists make use of it for this purpose, without thereby meaning to commit themselves to its ontological entailments. In other words, Melia endorses abstract expressionism.

Melia presents this position as part of an attack on the indispensability argument, and the relevance is clear. If Melia is right to suggest that scientists do not mean to commit themselves to the existence of mathematical objects, then mathematics and talk of unobservable concreta are being used in very different ways in science. By talking about unobservable concreta, one thing scientists intend to do is describe the constitution of matter and thereby explain the observable phenomena it gives rise to. By talking about mathematical objects, scientists intend only to express more about the physical world. If Melia is right about this, then the content of science does not entail the existence of mathematical objects. Hence, the second premise of IA is false, and so IA is unsound.

In this chapter, I do three things. First, I clarify Melia's position and defend it against some recent misunderstandings that have emerged in the literature (§2.2). Second, I evaluate the two arguments Melia presents in favour of his position (§2.3). The first argument is weak, and, to the extent that Melia's position depends on it, his position is weak, too; but I show that there are modified versions of Melia's position that do not depend on this argument. I show that a modified version of Melia's second argument is promising, but incomplete. In particular, two crucial premises require justification: (i) that taking mathematics to play the same role in science as talk of unobservable concreta is tantamount to endorsing heavy duty platonism (HDP); and (ii) that abstract expressionism is at least as plausible as HDP. Third, I

outline challenges that have been put to Melia, and claim that they need not be met to successfully undermine IA (§3.3).

2.2 What is weaseling?

By interpreting the Star Trek example (ST) as a case of weaseling, we are supposed to be interpreting the speaker as not contradicting herself. So we know what Melia proposes the speaker is *not* doing, but what does Melia propose the speaker *is* doing? What is weaseling?

In this section, I will endorse a particular interpretation of Melia, recently presented by David Liggins and myself (Knowles and Liggins forthcoming), which I argue makes the best sense of everything that Melia has to say about weaseling. In doing so, I will clear up some misunderstandings of Melia's position that have recently emerged in the literature.

On the characterisation of weaseling I endorse, someone asserting the sentences in (ST) should be understood as making two assertions, though the speaker should not be understood as having asserted anything that implies that *Nemesis* is an excellent film. Similarly, a scientist who articulates some platonistic theory and then goes on to assert that there are no mathematical objects should be understood as making two distinct assertions, but not understood as having asserted anything that implies the existence of mathematical objects.

This characterisation of weaseling may appear to be at odds with Melia's claim that '[s]ometimes it is legitimate to assert a collection of sentences whilst denying some of the logical consequences of this collection!' (2000: 456). Here, Melia appears to be saying that it is legitimate to make inconsistent assertions. But this claim would be puzzling on any interpretation of Melia. Melia denies (2000:

469) that he advocates inconsistent beliefs, so, since one should only assert what one believes to be true, we should expect him not to advocate inconsistent assertions, either. Knowles and Liggins make sense of this quotation as follows:

Some conceptual hygiene suggests a charitable explanation of this puzzling claim. The objects of assertion are propositions. Sentences can be uttered to make assertions, but the proposition asserted need not be the proposition expressed by the sentence uttered. For instance, ‘There are four eggs’ can be used to assert that there are *exactly* four eggs. (See Stalnaker 1978 for a popular and plausible account of assertion in this vein.) In light of this, we interpret Melia’s quoted claim as follows: ‘sometimes it is legitimate to *use a collection of sentences to assert something* whilst denying some of the logical consequences of this collection’. (Knowles and Liggins forthcoming)

Rather than taking Melia to be ascribing inconsistent behaviour to speakers, the above interpretation suggests that Melia was merely being careless or misleading with his terminology. Moreover, the claim this interpretation attributes to Melia—that we can use sentences that say one thing to mean another—is a sensible one.

Recently, some misunderstandings of Melia’s position have emerged in the literature. Diagnosing them will help bring the present characterisation of weaseling further into relief.

A response to Melia’s view that is proving popular in the literature is to claim that Melia’s strategy is only plausible if he can state exactly what is asserted when scientists engage in weaseling (Azzouni 2009: 157-9; Colyvan 2010: 295-6; Pincock 2007: 265-273, 2012: 252-6; Turner 2011). Call this the *content challenge*. Raley

(2012) is the most recent proponent of the content challenge, claiming that it reveals Melia's position to be incoherent. Her argument takes the form of a dilemma: if Melia can tell us exactly what has been asserted, then his view is just an example of the hard road strategy for responding to the indispensability argument. If Melia cannot tell us exactly what is asserted, then we should interpret the speaker as being inconsistent (Raley 2012: 343).

As Knowles and Liggins note (forthcoming), Raley's conclusion that Melia's position is incoherent is unwarranted. If Melia's view collapses into an example of the hard road strategy, it does not follow that it is incoherent, and if Melia is attributing inconsistent behaviour to speakers, it still does not follow that Melia himself is being inconsistent. Neither horn of Raley's dilemma establishes the conclusion to her argument.

Moreover, all of the instances of the content challenge presented so far are based on a misunderstanding of Melia's view. With respect to Raley's dilemma, the interpretation of Melia I have endorsed blunts the second horn. In general, we shouldn't interpret speakers as contradicting themselves if a more charitable interpretation is available. So, we should interpret the scientist instead as using a platonistic scientific theory in order to make an assertion that does not entail that mathematical objects exist. Her second assertion that there are no mathematical objects then clarifies the original assertion by ruling out some entailments of the theory as being part of what was originally asserted.

Raley anticipates this means of avoiding her dilemma by pointing out that paraphrases of the sentences used to make such assertions are not always going to be available; and, if they are not available, then we cannot know which parts of our theories to endorse and which to reject (Raley 2012: 343). This objection hits its

target only if all cases of weaseling are not necessary for communicating the intended content, as with the Star Trek example (ST). But Melia explicitly argues that weaseling is often necessary because our language lacks the resources to express the intended content directly. According to Melia, our best theories about the physical world are only communicable via weaseling, which is why mathematics is indispensable to science. Raley's response is therefore straight-forwardly question-begging (cf. Knowles and Liggins forthcoming).

Another misunderstanding of Melia's view is evident in Michael Scott and Philip Brown's objection that Melia is committed to an implausible account of speaker meaning (Scott and Brown 2012: 353-354; 357-359). Scott and Brown sketch their own account of scientific discourse that they claim avoids this difficulty (2012: 358-359). Again following Knowles and Liggins (forthcoming), I will argue that Scott and Brown's interpretation of Melia is wrong, and that their own account of scientific discourse in fact suggests one possible elaboration of the correct interpretation.

Scott and Brown label Melia's position a form of *figuralist fictionalism*, by which they mean the following:

Utterances of indicative [sentences of discourse D] are truth-apt but quasi-assertoric. A quasi-assertion is a speech act with the outward appearance of an assertion where the speaker does not endorse the uttered sentence but presents it as adhering to some norm other than truth. (2012: 352).

It is clear from this passage that Scott and Brown assume a notion of assertion that is different from that which I have endorsed. Scott and Brown assume that the objects

of assertion are sentences. On this view, asserting a sentence involves endorsing that sentence as true. As noted by Knowles and Liggins (forthcoming), this notion rules out the possibility of using a sentence that says one thing to assert another, so it forces Scott and Brown into endorsing an interpretation of Melia according to which scientists do not assert anything, but perform some other linguistic act instead.

On Scott and Brown's interpretation, Melia takes scientists to be routinely engaged in doing something comparable to the act of telling a fictional story. When I read from *The Hound of the Baskervilles*, I perform what seem like assertions, but I do not intend anyone to take my utterances as true. Scott and Brown are right that attributing such behaviour to scientists would involve commitment to an implausible account of speaker meaning because it would mean that scientists are unaware of the speech acts they are performing (2012: 353-354). As Jason Stanley says (2001: 46-47), we should expect competent speakers to deny that they are doing anything like engaging in fiction when using mathematical language. The problem, according to Scott and Brown, 'is not that the sentences... have truth-conditions of which speakers are unaware... but that *utterances* should have or lack content in [a] way that is opaque to speakers' (2012: 354).

In place of this implausible view, Scott and Brown offer their own account of scientific discourse (2012: 358-359). They claim that the sentences uttered by scientists do not mean what traditional semantic theory tells us they mean, but instead express some pragmatically enriched (or diminished) content. They suggest a pragmatic mechanism to account for this. 'The fridge is empty' is not often used to communicate that there is an absence of matter in the fridge. It is instead used to mean that there is no *food* in the fridge. But, to echo Stanley, we should expect speakers to deny that they are engaging in any non-standard speech act when they

talk in this way. Pragmatic theory posits a process called *loosening* to account for this: the meaning of the word ‘empty’ is contextually ‘loosened’ so that it is applicable to things that merely have no food in them. Scott and Brown suggest that some analogous mechanism is at work in scientific discourse: scientists assert the sentences of their theories which are pragmatically enriched by the context in such a way that they only express content concerning the physical world.

This account does not attribute ignorance to scientists about the speech acts they are performing. However, the idea that the words we use radically change meaning from context to context in this way is independently problematic, since it implies that semantic meaning is heavily determined by rich contextual features, such as the intentions of speakers. Clearly, some expressions change meaning from context to context. Indexical expressions, such as ‘I’ and ‘here’ are context sensitive, but the context-sensitivity of such expressions is built into their syntactic and semantic profile, and hence the shift in meaning is always lexically mandated. The view that Scott and Brown assume is stronger. They require that the context-sensitivity enjoyed by indexical expressions, for instance, extends to all expressions, at least by analogy, including names for mathematical objects. After all, they assume that expressions which purport to refer to mathematical objects in one context do not purport to refer to mathematical objects in another. I will not go into it now, but in Part II of this thesis (§5.2-§5.5), I provide powerful reasons for rejecting this view in favour of the more traditional division between pragmatics and semantics presupposed by my favoured account of assertion. According the traditional view, aside from ostensibly context-sensitive expressions, the semantic content of a sentence remains constant, though what one pragmatically communicates by uttering a sentence may vary widely from context to context.

More important for present purposes is the fact that assuming Scott and Brown's account of assertion results in a misunderstanding of Melia. Following Knowles and Liggins (forthcoming), there are four points to make: first, the evidence Scott and Brown cite in favour of their interpretation of Melia is weak; second, their interpretation contradicts what Melia says about weaseling; third, once weaseling is properly understood, Scott and Brown's objection fails; fourth, the pragmatic apparatus appealed to in support of Scott and Brown's own account of scientific discourse is available on the traditional view of assertion, and so is available to Melia as one possible elaboration of the view he intended.

Scott and Brown cite two pieces of evidence in favour of their interpretation of Melia as a figuralist fictionalist. First, they appeal to Melia's claim that scientists who appear to assert content implying the existence of mathematical objects, but who do not believe in such things, should be interpreted as being consistent (2012: 358). It is clear that this datum does not tell in favour of Scott and Brown's interpretation, since it is perfectly consistent with the interpretation I endorse.

The second piece of evidence is that Melia apparently compares weaseling to story-telling (Scott and Brown 2012: 358). The passage Scott and Brown are referring to is the following:

My thesis is that, just as in telling a story about the world, we are allowed to add details that we omitted earlier in our narrative, so we should also be allowed to go on to take back details that we included earlier in our narrative.
(Melia 2000: 470)

However, ‘telling a story about the world’ is often used not to mean telling a *fictional* story about the world, but instead to mean presenting a theory, or an explanation, or merely saying something about the world (cf. Knowles and Liggins forthcoming). In this passage, Melia is claiming that, when presenting information, taking back unwanted entailments is as legitimate as making explicit unarticulated assumptions. There is no evidence that Melia means to compare the behaviour of scientists to fiction making.

Scott and Brown’s interpretation has Melia claiming that scientists do not assert anything. This contradicts the claim made in the troubling quotation from Melia: ‘[s]ometimes it is legitimate to assert a collection of sentences whilst denying some of the logical consequences of this collection!’ On the interpretation I endorse, Melia is guilty of being sloppy or misleading with his terminology; according to Scott and Brown, Melia is guilty of self-contradiction (cf. Knowles and Liggins forthcoming).

On my interpretation of Melia, when scientists weasel, they do not assert anything that implies the existence of mathematical objects. They use sentences which imply some mathematical content to assert propositions that concern only the physical world. Clearly, Melia owes us an account of how this communicative feat works. He is silent on this issue, so his philosophical account of mathematical discourse is incomplete. However, one cannot accuse him of endorsing an implausible account of speaker meaning. Claiming that scientists indulge in fiction making is not a promising means of developing Melia’s account of mathematical discourse further, for just the reasons Scott and Brown allude to. For a speaker to tell a fictional story she must at least be aware that she is doing so; and yet we should

expect scientists to deny that they are telling fictional stories when they articulate their theories. Melia is right not to have endorsed this option.

A more promising means of developing Melia's view is to draw on pre-existing pragmatic theory to show that, in applied contexts, speakers pragmatically communicate content concerning only the physical world by uttering mathematical sentences. There is no reason why Melia could not appeal to the same apparatus endorsed by Scott and Brown to achieve this. The only difference being that, whichever pragmatic mechanism is appealed to, it would have to be posited as a post-semantic mechanism. That is, uncovering what a speaker means in uttering a mathematical sentence in an applied context will involve a prior interpretation of the semantic content of the sentence used (see §5.2-§5.5 for a more detailed account of this view). I demonstrate the success of this approach in chapter 8 (§8.4-§8.5) by drawing on well-established pragmatic theory in developing an account of mathematical discourse.

In conclusion, once weaseling is properly understood, many of the objections to Melia presented in the literature miss their mark; but this does not establish that Melia's attack on the indispensability argument is successful, since we have not yet been provided with a good argument in favour of Melia's position. In the following section, I evaluate the two arguments Melia presents in favour of his position.

2.3 Melia's arguments: charity and simplicity

To see again how weaseling is supposed to undermine the indispensability argument, imagine that a scientist articulates some platonistic theory. Melia recommends that we do not interpret this act at face value, as an assertion of the semantic content of that theory; rather, he recommends that we interpret it as a case of weaseling,

whereby the content the scientist means to communicate is only what the theory says about the physical world. On this view, the mathematical components of the theory are used only for expressive purposes, to make more things sayable about the physical world. If the scientist were philosophically rigorous, she would go on to deny that she believes the mathematical entailments of the theory. According to Melia, then, premise IA2 of IA, one conjunct of which says that mathematics plays the same role as talk of unobservable concreta in science, is false, and so IA is unsound.

Why should we interpret the scientist in this way? Why shouldn't we take her behaviour at face value, as asserting the propositions expressed by the sentences she utters? Melia offers two arguments, which I will call the *charity argument* and the *simplicity argument*. In this section, I will show that the first of these is weak, and, insofar as Melia's position relies on it, his position is weak, too. However, we shall see that there are altered versions of Melia's position that are not subject to this difficulty. I will show that the simplicity argument is more promising, though it requires further development to yield a successful response to the indispensability argument.

We are already familiar with Melia's charity argument: interpreting scientists as weaseling is more charitable than interpreting them as inconsistent, so scientists weasel. Melia claims that, since most scientists do not believe there are abstract mathematical objects, interpreting their utterances at face value would be uncharitable because it would mean interpreting them as asserting something that contradicts their beliefs. The more charitable interpretation therefore has them not asserting anything that entails the existence of mathematical objects.

The charity argument crucially rests on the claim that most scientists are nominalists. But how does Melia know this? This claim is a substantive sociological one, yet Melia provides no appropriate evidence for it. As Knowles and Liggins (forthcoming) point out, he does provide the following amusing anecdote:

In a set-theory class, the lecturer told me that I shouldn't go as far as to *believe* anything that he said, as I would end up like Gödel... (2008: 104)

... On further questioning, after the class, I made sure that the teacher meant 'mad' rather than 'brilliant'. (2008: 104, fn 3)

But anecdotal evidence does not establish generalisations. At best, Melia has singled out one lecturer who does not recommend platonism. In response, Knowles and Liggins note (forthcoming) that the lecturer in Melia's anecdote is a working mathematician rather than a scientist, and, further, single out a scientist who explicitly endorses platonism (see Penrose 1990: 123-128). Neither datum warrants any conclusions about the scientific community. Since Melia offers no justification for his claim about scientists, there is no reason to look for a charitable interpretation of what scientists say. In light of this, it is surely more sensible to take the assertions that scientists make at face value, as intended to communicate the propositions expressed by the sentences they use.

Knowles and Liggins (forthcoming) anticipate a response Melia might make: he might point to evidence that suggests an implicit commitment to nominalism in the scientific community. For example, it might be considered puzzling that scientists are so willing to utter sentences which imply the existence of mathematical

objects even though the existence of mathematical objects has not been established by normal scientific methods. Perhaps Melia's claim could be offered as the best explanation of this. However, as Knowles and Liggins note (forthcoming), there are other explanations that appear just as successful. For instance, scientists may be ignorant of the fact that their theories entail the existence of mathematical objects. Or, perhaps scientists think it is obvious that mathematical objects exist. Perhaps different scientists have different views on the matter. This move cannot supply the justification Melia needs.

The burden of proof lies with Melia here. To motivate his account of scientific discourse, he must provide evidence that the majority of the scientific community are nominalists. Because no evidence is currently available, there is no reason to think that scientists engage in weaseling. The charity argument is weak, and to the extent that Melia's position relies on it, Melia's position is weak, too. There are two things Melia might do in light of this conclusion. For the purpose of resisting the indispensability argument, however, only the second is successful.

Melia's descriptive proposal that scientists, who are interested in accounting for the nature of the physical world, use mathematical language merely to better express content concerning only the physical world is a plausible one. After all, the physical world is what they are interested in. The absence of relevant sociological evidence therefore does not mean Melia's descriptive proposal is hopeless; for all we know it might accurately describe how scientists actually put forward mathematical sentences. To pursue this descriptive proposal, however, one clearly cannot rely on Melia's claims about the scientific community as evidence. In the absence of any relevant sociological evidence, one would have to provide different independent evidence for the claim that mathematical language is generally used in this way. In

Part II of this thesis, I develop Melia's descriptive proposal and motivate it by providing general arguments from the philosophy of language. Hence, the first way Melia might respond to the above objection is to provide independent evidence for his view. Melia's descriptive proposal can still be developed, so long as the charity argument is not relied upon in doing so.

However, developing Melia's descriptive proposal is not appropriate for the purpose of undermining the indispensability argument, for even if the descriptive proposal is well motivated, there is a danger that it is not enough on its own to yield ontological conclusions. Suppose that scientists do not in general mean to express belief in the mathematical components of their theories. This does not guarantee that they are right to behave this way. If the role mathematics plays in our scientific theories is appropriately similar to that played by talk of unobservable concreta, then whether or not scientists mean to express belief in the mathematical components of their theories, they certainly *ought* to.

This brings me to the second means by which Melia might respond to the failure of his charity argument: remain agnostic about whether the descriptive proposal is accurate, treating it instead it as a description of how scientists *ought* to put forward mathematical sentences. When trying to settle questions of ontology, we should focus on scientific theories and scientific practices, not on what scientists happen to believe or say (cf. Leng 2010: 110; see also Bangu 2012: 21). While we are engaging with the indispensability argument, we are engaging in a distinctly ontological question: does our scientific practice require us to believe in abstract mathematical objects? Whether Melia is right about what scientists believe and say, the question remains as to what their beliefs *ought to be*, given their scientific practice and the theories they use. On this matter, Melia has something he can say.

He claims that mathematics plays a purely representational role in science. Given that mathematical objects need not exist for mathematics to play this role, Melia could argue that whether or not scientists actually do, they *should* articulate their theories while only meaning to express belief in the nominalistic content of those theories (cf. Knowles and Liggins forthcoming). If this is right, it still warrants the conclusion that the indispensability of mathematics does not require scientific realists to believe in mathematical objects. As Knowles and Liggins note (forthcoming), the suggested alteration of Melia's position parallels Gideon Rosen's (1994) interpretation of Bas van Fraassen's (1980) constructive empiricism. On face value, constructive empiricism implies that scientists do not believe their theories to be true, but rather accept them as accurate with respect to the observable part of the world. Rosen suggests that we might instead interpret constructive empiricism as a thesis about what it is rationally permissible to believe, as a practicing scientist. (See Leng 2010 for a fictionalist philosophy of mathematics put forward in a normative spirit.)

It seems that, so long as abstract expressionism is right, Melia's attack on the indispensability argument is sound; but why should we think that abstract expressionism is right? Melia's second argument addresses this question.

Melia notes that the role played by talk of unobservable concreta in science in virtue of which we believe in unobservable concreta is an explanatory one that involves simplifying our worldview (2000: 474). According to science, the physical world is made up of unobservable concreta, such as quarks and electrons, and observable physical phenomena occur in virtue of the ways in which entities are combined. By simplifying our picture of the physical world into one in which the physical world consists of various combinations of a handful of kinds of

unobservable concreta, talk of unobservable concreta plays an explanatory role in science.

The proponent of IA is committed to the claim that mathematical language plays the same role in science as that played by talk of unobservable concreta. So, it appears the platonist is committed to the claim that mathematical language plays a simplifying role. And it appears that mathematics does play a simplifying role, after a fashion. To take Melia's example (2000: 472-3), if we were to adopt a theory devoid of mathematics, then expressing the myriad distance relations that hold between physical objects will involve taking as primitive an infinite number of dyadic predicates of the form 'x is-5-metres-from y', 'x is-6-metres-from y', and so on. Once mathematical language is permitted, however, we can express all of these distance relations by taking as primitive just one triadic predicate of the form 'x is r metres-from y', where 'r' stands for a real number.

However, Melia argues that this is a 'misapplication of simplicity', because the way talk of numbers simplifies is importantly different to the way in which talk of unobservable concreta does (2000: 473). He claims that mentioning numbers in the present example simplifies the means by which we express distance relations, by 'indexing' them with numbers; but from this fact we cannot draw any conclusions about whether these relations are fundamental.

The distinction between kinds of simplicity Melia has in mind here is that of *ideological simplicity* vs. *ontological simplicity*. Ontological simplicity concerns how simple a theory says the world is, while ideological simplicity concerns the number of extra-logical terms that a theory takes as primitive. There is a further distinction to be made when it comes to ontological simplicity: *qualitative simplicity* concerns the number of types of things taken as fundamental, while *quantitative*

simplicity concerns the number of instances of each type posited. (See Daly 2010: 133 for a good discussion of the kinds of simplicity exhibited by theories; see also Lewis 1973: 87 for the distinction between qualitative and quantitative simplicity.)

Melia thinks that mathematics merely serves to help us better express facts about the physical world. That is, he endorses abstract expressionism. So he thinks that including mathematical language in our theories contributes to their ideological simplicity alone. If this is right, then mathematics really does play a different role in science than talk of unobservable concreta, which means IA is unsound.

The problem is that ideological simplicity and ontological simplicity appear to be very closely related. Intuitively, for every new primitive predicate introduced into a theory, a new fundamental type of thing is posited by that theory. This relationship can be stated as a biconditional: a theory posits a new basic type of thing *K* if and only if the theory introduces a primitive predicate ‘K’ (cf. Daly 2010: 134). If this is right, the platonistic theory of distance relations does indeed seem simpler. For each particular distance relation, the nominalistic theory introduces a new primitive predicate. Since there are infinitely many distance relations, it seems as though this theory must posit an infinite number of different kinds of relation to account for all the distances, where the platonistic theory must posit only one. It is worth noting that the cost of this simplicity is the positing of infinitely many numbers, but this is a case of positing one new kind of thing: *Number*. The cost of not including this one kind is the positing of infinitely many new kinds of relation corresponding to each distance relation. This view about the relation between ideology and ontology appears to imply that the platonistic theory is the simpler theory after all.

Melia must provide us with a good reason for thinking that, in the case of mathematics, the above biconditional does not hold. He must convince us that the ideological simplicity brought about by mentioning mathematical objects does not allow us to conclude that there are fewer kinds in the world. To do this, Melia cannot rely on the claim that the role played by mathematics is one of indexing physical properties, since that just amounts to a re-statement of abstract expressionism, the very position that he needs to argue for.

The reason Melia provides is as follows. He claims that, if we take mathematical language to play an explanatory role via playing a simplifying role, we are committed to the ‘wholly implausible’ view that physical magnitude properties, including distance, temperature and mass, ‘*are* really fundamental relations holding between concrete objects and abstract numbers’ (1995: 228-9). Call this view *heavy duty platonism* (HDP) (cf. Field 1989: 186-9). There are two claims here. The first is that taking mathematics to play an explanatory role via a simplifying role involves endorsing HDP. The second is that HDP is implausible. If Melia is right on both accounts, then he has provided good reason for thinking that mathematics does not play an explanatory role via a simplifying role in science. In doing so, he has provided some support for abstract expressionism by ruling out one respect in which the role played in science by mathematics and talk of unobservable concreta are alike. However, this does not go far enough to vindicate Melia’s attack on the indispensability argument, for there are two ways in which the platonist might respond.

First, the platonist could argue that HDP is not implausible after all. Melia does not provide any argument for the claim that it is. Objections to HDP do turn up in the literature, and we can be sure that Melia is aware of at least two of these

objections, since they feature in a paper by Tim Crane to which Melia replies (see Crane 1990 and Melia 1992). It is therefore charitable to assume that Melia has in mind some of these objections when he rejects HDP as implausible (cf. Knowles and Liggins forthcoming). For Melia's simplicity argument to go through, however, some objection to HDP must be shown to successfully undermine the view.

In light of this, one way in which Melia might strengthen his argument is to change this premise to the following weaker claim: that abstract expressionism is at least as plausible as HDP. Since IA is supposed to force the scientific realist into adopting platonism on pains of irrationality, it must be that platonism is the only option rationally available to the scientific realist. However, if abstract expressionism were equally as plausible as platonism, then being a scientific realist while denying the existence of mathematical objects looks like a rationally available option, in which case IA would fail. Melia would still have to motivate this claim, but the platonist could not undermine the simplicity argument merely by showing that HDP survives the available objections to it—she would have to show that HDP is *more* plausible than abstract expressionism.

Second, the platonist could agree with Melia that mathematics does not play an explanatory role in science via playing a simplifying role, but claim that mathematics plays some other kind of explanatory role. Talk of unobservable concreta in science is explanatory because the handful of entities it postulates, along with their various modes of combination, are supposed to make up the environment and thereby give rise to all the disparate phenomena we observe. HDP attributes a similar role to mathematical objects by implying that relations between physical objects and mathematical objects give rise to the detectable physical qualities those objects have. If mathematics plays a different explanatory role, however, then the

platonist could appeal to this role in support of IA without thereby committing herself to HDP. On this view, mathematics would still play the same role in science as talk of unobservable concreta, but only insofar as they both play an explanatory role of some kind. However, this move places the burden of proof on the platonist to show that the explanatory role she attributes to mathematics is one that requires mathematical objects to exist.

The above discussion suggests some modifications of Melia's argument that make explicit its assumptions, and forsake unnecessarily strong claims.

SA

SA1. The indispensability argument is successful if and only if HDP is more plausible than abstract expressionism.

SA2. Abstract expressionism is at least as plausible as HDP.

SA3. The indispensability argument is not successful.

This argument is certainly more promising than the charity argument, but its premises still lack motivation. The two means by which the platonist can respond, mentioned above, correspond to the rejection of premises SA1 and SA2. The platonist must either show that HDP is more plausible than abstract expressionism, or show that mathematics plays some other explanatory role in science that requires mathematical objects to exist.

2.4 Two challenges: content and communication

In this section, I want to outline two challenges for Melia's view put forward in the literature. I will argue that one challenge need only be met by someone developing

Melia's descriptive proposal, which I have already argued is not necessary for the purpose of undermining IA, while the other has been partially met already and need not be fully met to render SA successful.

The first challenge was suggested by my discussion of Scott and Brown's objection to Melia. I said that it was legitimate to claim that Melia's account of mathematical discourse is incomplete, since he offers no account of how scientists use mathematical sentences to communicate non-mathematical content. The *communication challenge* says that such an account must be provided (see Liggins 2012). This challenge must be met by someone seeking to develop Melia's descriptive proposal. This involves claiming that scientist's actually do use mathematical sentences to communicate non-mathematical content, so it is fair to demand that an account be provided of how this communicative feat is executed. I develop this proposal in Part II of this thesis; chapter 8 is dedicated to meeting the communication challenge.

However, for present purposes this challenge need not be met. In the previous section, I argued that the descriptive proposal on its own does not address the ontological issue raised by IA; I therefore recommended that we adopt a normative version of Melia's view. According to the normative version, since the mathematical assumptions of our scientific theories play only a representational role, scientists *ought* to only mean to communicate the content of their theories concerning the physical world. Since this proposal is compatible with the claim that this is in fact not what scientists typically do, there is no requirement that the proponent of the normative version explain how people perform this communicative feat. Might the proponent of the normative view need to explain how scientists might perform this communicative feat, were they to take her advice? If scientists were to take seriously

the normative version of Melia's view, it seems to me that they are free to choose whichever quick and dirty means they have at their disposal to get the right content across. One means of doing this would be to explicitly weasel, as follows: 'Apart from the implication that there are mathematical objects, T', where T is some mathematically expressed physical theory. Another, more positive means might be to utter 'T is true of the physical world'. So long as we are endorsing the normative view with an eye to undermining IA, the communication challenge need not be met.

The second challenge is a variant of the content challenge. We saw in §2.2 that the content challenge is question-begging. It is part of Melia's view that mathematics is necessary for the expression of certain facts about the physical world, so demanding that he directly characterise the purely physical content of science is 'like responding to the claim that some gases are invisible by demanding to see them all' (Liggins 2012: 999).

There is a weaker form of the content challenge that I think Melia should be expected to meet if his view is to be successful, but it seems as though Melia already has the means to meet it, at least to an extent sufficient for supporting SA. The weakened form of the challenge merely asks for an appropriate characterisation of the physical content of science, rather than a sentence-by-sentence paraphrase of our scientific theories. Colyvan argues that, unless Melia tells us something more about what the content of science is, apart from its being consistent with their being no mathematical objects, he will have rendered many scientific claims 'incomprehensible' (2012: 1039).

This challenge takes on a different dimension depending on whether one is defending the normative or descriptive version of Melia's view. On the descriptive view, characterising the content of science will be about trying to adequately capture

what speakers take themselves to be talking about when they articulate mathematically expressed theories. Meeting the content challenge for this view will not only involve providing a characterisation of the content of science, it will also require that this characterisation matches up with certain intuitions. As I noted above, however, it is not necessary to adopt the descriptive view in order to support SA and therefore undermine IA. On the normative view, meeting this challenge will instead involve providing an indication of which aspects of the content of science we are justified in believing in. It is this version of the challenge we are required to address for the purpose of undermining IA.

As Knowles and Liggins point out (forthcoming), Melia does say something which addresses this challenge: he suggests that the content of science worthy of belief is that concerning physical magnitudes, such as mass and temperature, which are physical properties and relations that enter into causal interactions (see Melia 1995: 228, 2003: 58). In other words, we should believe what science tells us about physical objects, as opposed to mathematical objects. It seems easy enough for the most part to identify the physical objects that scientific sentences concern; it is identifying what those sentences say *about* physical objects that is difficult to do without using mathematical vocabulary. Nevertheless, we can often still grasp what scientific sentences say about the physical world even if we can't directly express this content. For example, consider the sentence 'There is a one-to-one differentiable function from space-time points to quadruples of real numbers'. If this sentence were true, it would be true in part because of the existence and nature of real numbers. However, it would also be true in part because of the existence and nature of space-time points. We can grasp immediately that the physical content of this sentence concerns only space-time points. Further, we can grasp what this sentence says *about*

space-time points by thinking about what restrictions the truth of this sentence would place on them. Mark Balaguer puts it as follows: ‘The nominalistic content of a theory T is just that the physical world holds up its end of the “ T bargain”, that is, does its part in making T true’ (1998: 135).

However, some scientific sentences and theories are more problematic. For some areas of science, the distinctively physical subject matter is not so easy to delineate. For example, Margaret Morrison suggests that the property of spin, which plays a crucial role in quantum mechanics, is ‘perhaps best viewed as a curious hybrid of the mathematical and the physical’ (2007: 552). The platonist might therefore appeal to quantum mechanics to argue that the abstract expressionist cannot meet the content challenge, so HDP is the more plausible view. However, as Knowles and Liggins (forthcoming) note, this way of blocking Melia’s arguments is not promising, since it will involve appealing to areas of science whose interpretation is a controversial matter. Indeed, the interpretation of quantum mechanics is a murky area of philosophy, so it would be extremely difficult to appeal to it in response to Melia without begging the question, or without unwanted controversy.

In Part II of this thesis, I will develop the descriptive interpretation of Melia’s view. I will therefore be required to meet the communication challenge and the relevant version of the content challenge. However, meeting these challenges is not necessary for the purpose of undermining IA. If taking mathematics to play an explanatory role in science that supports IA is tantamount to adopting HDP, and if abstract expressionism is at least as plausible as HDP, SA will be sound, and IA will have failed. Meeting the content challenge and communication challenge is not

necessary for supporting SA, so they will no longer concern me in this part of the thesis.

2.5 Conclusions

In this chapter, I clarified Melia's position and defended it against recent misunderstandings that have arisen in the literature. I then analysed the two arguments that Melia presents in favour of his view. First, I showed that the charity argument fails, and insofar as Melia's position depends on this argument, his position is weak. However, we saw that a modified version of Melia's position does not depend on this argument: the normative version. According to the normative version, because mathematics plays only a representational role in science, scientists can and should articulate their theories while only meaning to assert what those theories say about the physical world. According to this view, scientists are advised to weasel out of the platonistic commitments of their theories.

Second, I showed that a modified version of the simplicity argument, SA, is promising, but that its premises still require justification. We saw that it rests on two claims: first, that the indispensability argument is successful if and only if HDP is more plausible than abstract expressionism; second, that abstract expressionism is at least as plausible as HDP. I argued that there is room for the platonist to respond to both of these claims.

Finally, I argued that two challenges that have been levelled against Melia in the literature do not have to be met for SA to be successful.

How might the claims on which SA rests be justified? One way is to appeal to the existing objections to HDP to show that it is untenable. In the following

chapter, I will show that this is not a promising means of supporting SA: we shall see that HDP survives the objections to it presented in the literature.

3. Heavy Duty Platonism

3.1 Introduction

Most of this chapter has been published in Knowles (forthcoming(a)). Heavy duty platonism (HDP) is the view that physical magnitudes, such as mass and temperature, are cases of physical objects being related to numbers. To my knowledge, no philosophers have openly endorsed HDP, but if Melia is right that endorsing IA is tantamount to endorsing HDP, then proponents of IA (e.g. Colyvan 2001; Baker 2005; Lyon 2012) are committed to the view. It is also notable that Pincock's (forthcoming) account of mathematical explanation in science looks very much like HDP. Moreover, many other philosophers have assumed that HDP is false as a crucial premise in important arguments. For example: we've already seen that Melia's response to IA relies on the claim that HDP is implausible; Hartry Field (1989: 186-200) rejects one theory of space-time in favour of another because it apparently implies HDP; Paul Churchland (1979: 105) rejects the view that intentional states are relations between thinkers and propositions because it is supposedly analogous to HDP; and Tim Crane, who rejects this analogy, agrees that HDP cannot be true (1990: 227).

These conclusions are justified only if HDP is shown to be untenable, but arguments to that effect are surprisingly hard to find in the literature. Most are content just to assert that HDP is false, while some go only as far as to point to what they take to be counterintuitive implications of the view. In this chapter, I organise these intuitions into five arguments against HDP and show that they each fail. I thereby establish two related truths: HDP has been too quickly dismissed in the literature, and the arguments mentioned above that take the falsity of HDP as a key

premise should be re-assessed. More importantly for the purposes of this thesis, I will show that Melia's attack on the indispensability argument cannot be supported by appealing to the objections to HDP stated and alluded to in the literature.

In §3.2, I formulate one argument based on the Lewisian distinction between intrinsic and extrinsic properties. The argument is that HDP wrongly categorises all physical magnitude properties as extrinsic. I show that a plausible and popular analysis of intrinsicity implies that HDP's physical magnitudes are intrinsic. I then consider the reply that a better analysis would characterise HDP's physical magnitudes as extrinsic. In response, I provide evidence that our intuitions about which physical magnitudes are intrinsic are misleading, so HDP need not honour them.

In §3.3, I formulate two arguments from Crane's discussion of HDP. The first is that HDP must accept one of two untenable theses: that there is a metaphysically privileged measurement scale; or that an object's physical magnitude consists in its being related to all the numbers the magnitude property is measurable with. I show that both are defensible. The second argument is that HDP entails that physical objects have some of their causal powers by being related to non-causal objects, which is incoherent. I show that this is coherent and outline a theory of explanation that shows why.

In §3.4, I undermine two arguments offered by Field using arguments outlined in §2 and §3, at which time I will have successfully defended HDP from all objections alluded to in the literature and so vindicated the above conclusions. However, before all this, more needs to be said about what HDP is.

The most direct discussion of HDP is provided by Field (1989: 186-9). He says that all platonists believe there are 'relations of physical magnitude that relate

physical things and numbers' (1989: 186). I should note that, by this, Field means platonists are 'willing to employ relational predicates that [they assume] to be instantiated by pairs of physical things and numbers' (1989: 186, fn.23). This understanding of relations in terms of predicates is part of a broader concern in Field (1989) to avoid muddying certain issues about the nature of space-time (see Field 1989: 181, fn.16). Nothing I say in this thesis presupposes this understanding, or any particular understanding, of relations.

For example, a 10kg bag of sand bears the mass in kilograms relation to the number 10. Call these relations of physical magnitude 'platonic relations'. What separates different kinds of platonism is what they tell us about these relations: weaker forms tell us they are derivative of more fundamental properties or relations that hold of physical objects alone, while HDP says these relations are fundamental and 'not explainable in other terms' (1989: 186). (Field presumably took 'not explainable in other terms' and 'fundamental' to be synonymous; but I don't want to commit to this.)

Another way of describing the difference is as follows. Weaker forms of platonism imply that there is a purely physical fact about a 10kg bag of sand that makes it the case that it bears the mass in kilograms relation to the number 10, while HDP implies that there isn't.

HDP allows more fundamental properties in virtue of which a given physical magnitude holds. For example, the heavy duty platonist can explain the mass relation between a brick and the number 10 in terms of the mass relations between the particles composing the brick and the relevant numbers, in which case the relation between the brick and the number 10 would not be absolutely fundamental, but fundamental relative to the brick. HDP does not allow that the mass of the brick is

explained by a property instantiated by the brick alone. Relative to the brick, HDP has it that the relations it stands in to numbers are the fundamental facts concerning its physical magnitudes.

3.2 Arguments from Lewis

Here I formulate one argument against HDP based on the Lewisian distinction between intrinsic and extrinsic properties. These notions are typically introduced by stating various platitudes about each. Here is a list from Lewis:

A sentence or statement or proposition that ascribes intrinsic properties to something is entirely about that thing; whereas an ascription of extrinsic properties to something is not entirely about that thing, though it may well be about some larger whole which includes that thing as part. A thing has its intrinsic properties in virtue of the way that thing itself, and nothing else, is. Not so for extrinsic properties, though a thing may well have these in virtue of the way some larger whole is. The intrinsic properties of something depend only on that thing; whereas the extrinsic properties of something may depend, wholly or partly, on something else. If something has an intrinsic property, then so does any perfect duplicate of that thing; whereas duplicates situated in different surroundings will differ in their extrinsic properties.

(1983: 197)

There are generally thought to be clear-cut examples of each. The property *being a stone* is thought to be intrinsic, for something's being a stone does not appear to

involve anything distinct from itself. *Being 20 miles away from a pig*, however, is extrinsic: it involves something other than its bearer – a pig.

Philosophers have claimed that some physical magnitude properties are clear-cut cases of intrinsic properties (see Stalnaker 1987: 9; Crane 1990: 227; Mumford 2006: 471–80; Molnar 2003: 131–7; and Ellis 2001: 114–5). Intuitively, an object's mass involves only that object, but HDP implies that an object has its mass by being related to a number. So HDP implies mass is extrinsic. The argument we might formulate from these intuitions runs as follows:

I1. Some physical magnitudes are intrinsic properties.

I2. According to HDP, all physical magnitudes are extrinsic properties.

I3: HDP is false.

Call this the *intrinsic argument*. I will present three responses to the intrinsic argument. The first is not satisfying, but helps clarify the argument and highlight important features of HDP. I will discuss this response first. The second and third responses involve rejecting I2 and I1, respectively, and are more satisfying.

I2 says that HDP's physical magnitudes are extrinsic properties. This is misleading: they are not properties at all, but *relations*. Granted, associated with each relation are *relational properties* that hold of each relatum. For instance, the relation holding between x and y just in case x is taller than y has the associated relational properties *being taller than y* , which holds of x alone, and *being shorter than x* , which holds of y alone. The relation is not identical with these properties, but it is intimately related: the properties are instantiated just in case the relation obtains.

Intuitively, the relational properties hold in virtue of the relation's obtaining, rather than the other way round. The relation is more fundamental.

The same goes for HDP's physical magnitudes: if *o* is related to 10 by the mass in kilograms relation, there is a relational property that holds of *o* alone: *bearing the mass in kilograms relation to 10*. But this property holds in virtue of the relation's obtaining. Moreover, the relation appears only to involve the physical object and the number, and nothing else. Far from implying that physical magnitudes are extrinsic relations, HDP implies they are *intrinsic relations*, where the notion of an intrinsic relation is a generalisation of the notion of an intrinsic property, and is introduced in a similar manner:

An n-place intrinsic relation is an n-place relation that n things stand in virtue of how they are and how they are related to each other, as opposed to how they are related to things outside of them and how things outside of them are.
(Weatherson & Marshall 2013: §1.3)

Hence, I2 is false, and the intrinsic argument is unsound. This response attacks the letter, rather than the spirit, of the argument. A more charitable interpretation has the argument concerned with the properties that HDP attributes to physical objects alone. Say *o* has mass 10kg. Intuitively, this partly involves *o*'s instantiating an intrinsic property. According to HDP, however, the only thing this implies about *o* alone is that it instantiates the relational property *bearing the mass in kilograms relation to 10*, which looks extrinsic. So HDP does mischaracterise the properties of the physical objects.

This is too quick. The property of having bigger quadriceps than biceps is a property that most people instantiate. It is relational: for it to be instantiated by Jill, the *bigger than* relation must hold between Jill's quadriceps and her biceps. Nevertheless, there is still some sense in which this property involves only Jill, so it is intuitively intrinsic. It is clear that relational properties are not automatically extrinsic (cf. Weatherson & Marshall 2013: §2.1).

Further, some relational properties are instantiated because a relation holds between an object and some necessary state of affairs. For instance, Bill instantiates the property of being such that Jill exists or Jill does not exist. One might think that such properties are extrinsic because they appear to depend on states of affairs distinct from the bearer. However, one might also think that they are intrinsic because objects have these properties no matter how the rest of the world turns out. Because it is necessary that Jill exists or Jill does not exist, Bill is necessarily such that Jill exists or Jill does not exist, so Bill's having these properties does not depend on how things go with other objects. Our intuitions can lead either way. (I should note that, depending on one's account of propositions, the claim that <Jill exists or Jill does not exist> is necessary will have to be cashed out in different ways. For example, some would argue that <Jill is alive or Jill is not alive> is a singular proposition, and so has Jill as a constituent. Salmon 1989 addresses such problems. On such a view, the proposition will not exist in worlds where Jill does not exist. On other accounts, propositions are mind-dependent, the proposition will not exist in worlds where there are no minds. On such accounts, a proposition is necessary iff it is true *at* all worlds, as opposed to *in* all worlds. See King 2007 and Soames 2010 for recent proposals in this vein. Nothing I say here depends on a particular account of propositions. In fact, as I say in §9, I intend to extend the fictionalism developed in

this thesis to talk of other problematic entities, such as propositions and possible worlds.)

When a theoretical distinction faces problematic cases, it must be made more precise. We need an analysis of the intrinsic/extrinsic distinction that pins down what it is for a property to only involve its bearer. Such an analysis should be plausible, decisive with respect to the problematic cases, and track our intuitions about the unproblematic cases.

Rae Langton and Lewis provide a plausible and popular analysis: ‘a property is *intrinsic*... iff whenever two things (actual or possible) are duplicates, either both of them have the property or both of them lack it’ (1998: 337). Call this the *Lewisian analysis*. This is a plausible account of what it means to say a property involves only its bearer and it tracks our intuitions concerning the unproblematic cases. Moreover, the analysis is decisive with respect to the aforementioned problematic cases. In all worlds where there is a perfect duplicate of Bill, Jill exists or Jill does not exist, so in all those worlds Bill will be such that Jill is alive or not alive. According to the Lewisian analysis, these properties are intrinsic.

The same goes for HDP’s relational properties. The standard assumption is that, if numbers exist, they exist necessarily. On this view, HDP implies that *o* will instantiate the property *bearing the mass in kilograms relation to 10* in all worlds in which there is a perfect duplicate of *o*. Even if we reject the standard assumption, we cannot claim there is a numberless world containing a perfect duplicate of *o* without assuming that the number 10 does not help make it the case that *o* has mass 10kg. This would beg the question. According to the Lewisian analysis, HDP’s relational properties are intrinsic.

This is the second response to the intrinsic argument. Our intuitions about the intrinsic/extrinsic distinction are not robust enough to classify certain cases, so we need an analysis to make it more precise. According to a plausible and popular analysis, HDP's relational properties are intrinsic. This is a principled reason for rejecting I2 and the intrinsic argument.

Some may have a strong intuition that HDP's relational properties are extrinsic, and take the fact that the Lewisian analysis classifies them otherwise to be a good reason to reject the analysis. This brings me to the third means of defending HDP from the intrinsic argument.

Suppose that the best available analysis of the intrinsic/extrinsic distinction implies that any property involving something other than the bearer, even if that something is a number, is extrinsic. On this analysis, HDP clashes with the intuition that physical magnitude properties are intrinsic. However, if it can be shown that these intuitions are unreliable, then HDP's failure to honour them will not count against it.

Mass is often taken by philosophers to be a clear-cut case of an intrinsic property (see Mumford 2006: 471-80; Molnar 2003: 131-7; and Ellis 2001: 114-15). However, as William A. Bauer (2011: 89-93) argues, contemporary science appears to falsify this intuition. According to our best theory of fundamental particles, mass is extrinsic.

The Standard Model is our best physical theory of fundamental particles. The predicted discovery of the W^+ , W^- and Z bosons in 1983 at CERN, and more recently that of the Higgs Boson in 2013, have given the theory substantial empirical support. The Standard Model tells us that the mass of a particle is not just a result of

the way that particle is, but also of the particle's interaction with a certain scalar field:

[The mechanism by which a particle gains its mass] is based on the assumption of the existence of a scalar field, the "Higgs Field", which permeates all of space. By coupling with this field a massless particle acquires a certain amount of potential energy and, hence, according to the mass-energy relation, a certain mass. The stronger the coupling, the more massive the particle. (Jammer 2000: 162-3)

The mass of particles depends not only on the properties of the particles, but also on the properties of the Higgs Field. The Higgs Field is entirely distinct from any particle interacting with it. For any particle p , there is a world in which p does not exist while the Higgs Field does. Science tells us that the mass of a particle depends on the properties of something distinct from the particle, so, on the present view, mass is extrinsic. According to HDP, then, mass is a three-place relation holding between an object, a number, and the Higgs Field.

It is not surprising that our intuitions about which physical properties are intrinsic can go awry, since they appear to be based on naïve observation. We do not observe anything distinct on which an object's mass depends, so we take it to be intrinsic. Our judgements about which properties are intrinsic should instead be informed by our theories about their nature. Physics reveals that our initial judgements were wrong and that mass is extrinsic, and there's no reason why metaphysics can't be informative in this way, too. For example, Ted Sider (2003) shows that many go-to examples of intrinsic properties, such as the aforementioned

being a stone, are in fact extrinsic. Whether or not something is a stone partly depends on whether it is part of a larger stone or not, so it depends on something distinct from itself. Along with the scientific example above, this provides a strong reason for rejecting arguments or theories that appeal to the intuitive intrinsicity of everyday properties (cf. Weatherson & Marshall 2013: §1.1).

HDP is a theory about the nature of physical magnitude properties. If correct, it should inform our judgements about which side of the intrinsic/extrinsic divide they fall. HDP has not been shown to be correct, but it has not been shown to be incorrect, either. Until it has, given that our intuitions on the matter are unreliable, the heavy duty platonist has no good reason to accept that some physical magnitudes are intrinsic, so can reject I1.

The heavy duty platonist has good reasons for rejecting either I1 or I2, so the intrinsic argument fails.

3.3 Arguments from Crane

I now formulate two arguments against HDP alluded to by Crane. The first is that HDP must be coupled with one of the following unpalatable options: that there is a metaphysically privileged measurement scale; or that a physical magnitude is a case of an object being related to all numbers the magnitude property is measurable by. I show that both options are defensible. The second argument is that HDP implies the following alleged contradiction: physical objects have some of their causal powers by being related to non-causal objects. I show that this is not contradictory, and outline a plausible theory of explanation that reveals why.

Crane argues that physical magnitude ascriptions involve an arbitrary choice of units that determines which number the physical object is said to be related to. In

the case of temperature, the choice between using the relation *degrees Celsius* or the relation *kelvin* changes which number boiling water is said to be related to: it bears the *degrees Celsius* relation to 100 or the *kelvin* relation to 373.15. The number mentioned is determined by an arbitrary decision. This is taken to suggest that these predicates are just convenient ways of picking out the physical properties of physical objects (Crane 1990: 227).

Call this the ‘arbitrary argument’ (see Daly and Langford 2009: 643 for a similar argument):

A1: Some physical magnitude properties are such that the number a physical object is said to be related to by an ascription of the magnitude rests on an arbitrary choice of scale.

A2. From A1: Some physical magnitude properties are such that no scale by which they can be measured specifies a fundamental relation.

A3: No physical magnitude properties are such that they are instantiated in virtue of a physical object being related to all of the numbers with which the magnitude can be measured.

A4: From A2 and A3: Some physical magnitude properties are such that they are not instantiated in virtue of a physical object being related to any of the numbers with which the magnitude can be measured.

A5: If HDP is true, all physical magnitude properties are such that an object has them in virtue of being related to a number or a collection of numbers.

A6: From A4 and A5: HDP is false.

There are two ways to respond to this argument. The first is to undermine A2 by claiming that there is a metaphysically privileged measurement scale and that all physical magnitudes can be measured with, or reduced to magnitudes that are measurable with that scale. There are two paths to take here: the bold and the cautious. The former is to give reasons why an existing scale of measurement is privileged in the way just mentioned; the latter is to claim that there is a privileged unit of measurement for each magnitude, though we may never be in a position to know which one it is. I will show that both paths are defensible.

For the bold path, there will have to be some virtue of using a certain scale that implies it has metaphysical import. Surprisingly, a candidate suggests itself: Planck units. As John Baez points out, our current physical worldview is ‘deeply schizophrenic’ (2001: 177). On the one hand we have the theory of general relativity, which recognises that space-time is curved while ignoring the uncertainty principle. Two constants appear throughout: the speed of light c and Newton’s gravitational constant G , which determines how much the geometry of space-time is affected by other fields. On the other hand, we have quantum field theory, which takes the uncertainty principle seriously, but assumes that space-time is flat. The two constants here are c and Planck’s constant h , which defines the limitations governing our ability to measure simultaneously two different quantities precisely. On the face of it, these theories are incompatible, and some kind of reconciliation is needed if we are to fulfil science’s goal of explaining all the phenomena in its domain.

Planck discovered a way to use the previously mentioned constants to define unique units of length, mass and time. Planck length (ℓ_p) is defined as follows

(where $\hbar = \frac{h}{2\pi}$):

$$\ell_p = \sqrt{\frac{\hbar G}{c^3}}$$

This is very small at about 1.616199×10^{-35} metres. There are several reasons for thinking that Planck length will play a significant role in our unified theory of everything, when we discover it. I will outline just one (see Wilczec 2001a; 2001b; 2002 for many more).

According to quantum field theory, associated with every particle of mass m is its ‘Compton wavelength’ such that determining the position of the particle to within this length requires enough energy to create another particle of mass m . Thus, the Compton wavelength is the length at which quantum field theory becomes crucial for describing the behaviour of particles of a certain mass (Baez 2001: 179). According to general relativity, associated with any mass m there is its ‘Schwarzschild radius’ such that compressing an object of mass m to a size smaller than this results in the formation of a black hole. Thus, the Schwarzschild radius is the length at which general relativity becomes crucial for describing the behaviour of particles of a certain mass (Baez 2001: 180). The Compton wavelength and Schwarzschild radius are equal when m is the Planck mass; the point at which they both equal the Planck length. As Baez says: ‘At least naïvely, we thus expect that both general relativity and quantum field theory would be needed to understand the behaviour of an object whose mass is about the Planck mass and whose radius is about the Planck length’ (2001: 180). This suggests that a unified physical theory will take Planck-sized chunks as its fundamental quanta. (See Garay 1995 for

arguments that a minimum length is a model-independent feature of all approaches to formulating a theory of quantum gravity.)

To claim that this scientific significance implies that Planck units are metaphysically privileged in the way that HDP requires, only two more highly plausible claims are required: First, that the unified physical theory will take Planck-sized quanta as fundamental because the world is carved up into Planck-sized chunks. If it turns out that the unified physical theory represents Planck-sized quanta as fundamental, which looks likely, then there would be unique numerical values assigned to physical objects for their length, mass and temporal extension. The second claim is that any other physical magnitudes will reduce to planck units. This also seems plausible to me. Take temperature, for example. The temperature of an object is a measure of the mean kinetic energy of its constituents. Kinetic energy is given as follows:

$$K = \frac{1}{2}mv^2$$

So the Kinetic energy of a particle is the result of its mass and its velocity, and its velocity is in turn a measurement of the distance travelled and the time taken to do so. So, the temperature of an object is a measurement of the mass, distance travelled, and time taken to do so, of its constituents. All of these magnitudes are measurable with Planck units. Of course, it might turn out that there are magnitudes that are not measurable or reducible to Planck units, but the burden clearly lies with Crane here to point them out. The heavy duty platonist can happily reject A2 and the arbitrary argument on that basis.

Some may worry that the plausibility of HDP should not depend on the deliverances of as yet undeveloped physical theories. In which case, the cautious

path should be taken. First, maintain that the physical world is made up of discrete quanta, and that physical properties can only take on certain discrete magnitudes that are functions of some smallest magnitude. Assuming that these smallest magnitudes are relations to the number one, each physical magnitude then has a unique numerical value, implying that some privileged scale of measurement accurately reflects the way the physical world is carved up. Second, accept that we may never be in a position to know which scale is privileged in this way.

One might object to the claim that the way world is carved up is potentially unknowable, but it is hard to see why this potentially unknowable truth should be objectionable. Unless, of course, one rejects the general claim that there is a determinate way the world is independently of whether we can know it, and so subscribes to some form of verificationism. I don't find this general claim implausible, and suspect that it is ubiquitous. Verificationism is no longer a widely held view, and for good reason. (One problem with verificationism concerns the status of the verification principle itself: it appears unverifiable and so meaningless by its own lights. Another is that knowing the meaning of a sentence appears to be conceptually prior to knowing what would verify or falsify it. See Lycan 2000: 115-28 for a good survey of the problems facing verificationism.)

The second way of responding involves altering HDP so that accepting that physical magnitudes are had in virtue of being related to every number the magnitude is measureable by is not absurd. For this to be successful, it must be shown that this is a defensible position. I will now demonstrate that it is.

What exactly is absurd about the claim that physical magnitudes are relations to all numbers? One worry is that individuating different magnitudes would be impossible. If mass related physical objects to just one number, different magnitudes

of mass would be individuated by the number the objects are related to. According to the present view, however, every case of an object having mass is a case of its being related to every number. The other worry is that positing an infinite number of fundamental relations to explain a single property is metaphysically baroque. I will address each of these in turn.

Which number an object is related to does not tell us what physical magnitude property it has. However, which relations relate the object to which numbers does. Once a scale is set, it is not arbitrary which number an object is related to by the specified relation. This is how we individuate physical magnitudes on the present view.

To illustrate, we can represent each different mass with a function that takes all possible mass scales as its domain, and the real numbers as its range. The function representing one mass cannot be individuated from one representing another by its domain and range. However, which values of the range are assigned to which values in the domain will be different for each. For example, for a 5kg and a 10kg mass, the function representing the former maps *mass in kilograms* to 5, while the function representing the latter maps *mass in kilograms* to 10. These functions are distinct, and the mass properties they represent are distinct.

For the second worry, a stronger claim is required: physical objects are related to all numbers, but only indirectly; they are primarily related to the functions just described. Call this relation between physical objects and functions ‘embodiment’. Mass scales can be described as relations assigning numerical values to each object with mass. On this account, what it is for *o* to have mass 10kg is for it to embody a certain function *f* that maps *mass in kilograms* to 10. If this is a tenable view, then the arbitrary argument fails. Though the physical object is related to all

numbers, this is only in virtue of a more fundamental relation holding between the physical object and a function. Thus there is no absurdity, since the explanation for each property bottoms out on one fundamental relation.

Some objections come to mind. The first is a charge of obscurity: we have not been told what embodiment amounts to. To this, the heavy duty platonist has a few things to say. The relations posited by HDP are supposed to be fundamental and not explainable in any other terms. The complaint that no further explanation has been given is therefore misguided. Nevertheless, a somewhat metaphorical gloss can be given of what the embodiment relation involves: an object's embodying f involves the object pairing up numbers and measurement scales in accordance with f .

Another objection is that, in its current form, HDP is too far removed from the naïve view described in §3.1. In responding to the arbitrary argument, the heavy duty platonist has given up on HDP. This worry is neutralised by recognising the following. First, the present view still characterizes physical magnitudes as fundamental relations between physical objects and mathematical objects. Second, it still characterises physical magnitudes as involving relations to numbers; it merely posits an intermediary relation. The present view is very much in the spirit of HDP.

The first argument from Crane was that HDP must adopt one of two incoherent claims: that there is a metaphysically privileged measurement scale; or that physical objects are related to all numbers by physical magnitude relations. I have shown that both of these claims are defensible. The first by either taking the scientific importance of Planck units as evidence of their being metaphysically privileged, or claiming that, though we may never be able to find out which one it is, there is nevertheless a privileged scale of measurement. I showed that the second

claim can be defended by altering HDP slightly so that physical magnitudes are relations to certain unique functions from measurement scales to numbers.

I turn to the second argument from Crane. It is a commonplace view in the philosophy of mind that intentional states are relations holding between thinkers and propositions. It is also commonplace to suppose that intentional states are causally relevant to behaviour. But the combination of these views implies that a relation to an abstract object can endow a thinker with causal powers. Some philosophers find this problematic and have tried to undermine the view that intentional states are relational by appeal to analogy. First, it is assumed that the role numbers play in physical magnitude ascriptions is merely to index purely physical properties. Second, it is argued that this role is analogous to that played by propositions in intentional state ascriptions. The conclusion is that intentional states are not relational after all (see Churchland 1979: 105 and Stalnaker 1987: 8 for examples of this reasoning).

If HDP is assumed instead, the same analogical reasoning supports the view that intentional states are relations to propositions. But Crane rejects HDP for the same reasons mentioned above: ‘How could the state of something’s having a certain temperature have effects, if it is really a relation to an abstract object?’ (1990: 225-6). This suggests a *reductio* argument against HDP:

Assume for reductio:

C1. Physical objects instantiate the physical magnitudes they do by bearing certain relations to numbers.

So, given that:

C2. Some physical objects have some of their causal powers in virtue of their physical magnitudes.

C3. Numbers are non-causal.

C4. If an object has causal powers in virtue of standing in a certain relation, then the thing the object is related to must be causal.

C5. From C1, C2 and C3: Some physical objects have some of the causal powers they do by bearing relations to non-causal objects.

C5 contradicts C4, and C2-C4 are undeniable, so C1 is false and so is HDP. Call this the *causal argument*. To properly establish the reductio, it must be shown that the only way to avoid contradiction is to reject C1. C2 is well-motivated by some relatively non-contentious causal explanations in science that make indispensable reference to physical magnitudes. The glass beaker breaks because the water in it is at 100 degrees Celsius (boiling point). It appears the temperature of the water is causally efficacious in breaking the glass. I will also assume the truth of C3: numbers are typically thought to be abstract objects, and abstract objects are characterised as non-spatial and non-causal. Therefore, the only option for defending HDP lies in rejecting C4. As it happens, this is a poorly motivated premise. I will show that it is coherent and even quite plausible to assume that its relations to abstract objects can determine the causal powers of a physical object.

To illustrate, I will focus on the example of boiling water in a glass flask, assuming that it is paradigmatic of scientific causal explanations, and so can be easily generalised. According to Frank Jackson and Philip Pettit (1990: 109), there are two explanations available for why the glass breaks. The one given above mentions the temperature property. Jackson and Pettit call this the ‘program

explanation'. The other is called the 'process explanation': the glass breaks because certain water molecules strike the glass with sufficient momentum to break it. The fact that there are two explanations available poses a problem. If the behaviour of certain water molecules is sufficient to break the glass, what use are the program explanation and the temperature property it mentions? It appears we either have to get comfortable with causal over-determination, or accept that the temperature property isn't causal. Neither seems desirable.

Any account of this explanation will have to reconcile these two competing explanations plausibly. It poses the following three challenges for HDP's account: (i) explain how the temperature property, understood as a relation to a number, is relevant to the breaking of the glass; (ii) explain how the momentum properties of the water molecules, understood as relations to a numbers, are relevant to the breaking of the glass; (iii) explain how both the temperature property and the momentum properties can be relevant, while avoiding causal over-determination.

Jackson and Pettit claim the threat of causal over-determination disappears once we appreciate the distinction between a causally relevant property and a causally efficacious one (1990: 114-7). Pettit explains: 'a higher-order property is causally relevant to something when its instantiation ensures... in a non-causal way, that there are lower-order properties present which produce it' (Pettit 1993: 37). The 'ensuring' relation is to be understood as a modal relation between properties: the causally relevant (program) property must always be accompanied by some lower-order property. The temperature property is a higher-order property that is a measure of the mean kinetic energy of the water molecules, and thus is determined by the distribution of momentum properties among the molecules. It is also multiply realisable: different distributions of momentum properties are also sufficient for its

instantiation, so long as they produce the same average energy. Therefore, whenever this temperature property is instantiated by some body of water in a glass flask, there will always be some molecules that strikes the glass with sufficient momentum to break it. Though the temperature property is not causally efficacious, it is causally relevant to the breaking of the glass because it ensures the instantiation of appropriate momentum properties.

It is tempting to read ‘ensures’ as a metaphysically loaded term, such as ‘determines’. This must be resisted. Though it hasn’t been explicitly said in the literature, the ensuring relation cannot be interpreted as a dependence relation. Because the temperature property is a higher-order property, multiply realisable by properties of molecules, it is clear that metaphysical determination runs in the opposite way to the ensuring relation here (see Rosen 2010 and Audi 2012 for more on metaphysical dependence). The temperature property is instantiated in virtue of the distribution of momentum properties among the water molecules. The ensuring relation must therefore be read epistemically: if we know that the temperature property is instantiated, we can be sure that one of the many ways it can be realised must obtain, and so we can be sure that the glass will break. It is the dependence of the temperature property on its realiser properties that explains why we can be sure of this.

It might initially seem that HDP cannot adopt this explanation of the relevance of the temperature property to the breaking of the glass. This is because, according to HDP, the relation between the water and the number is fundamental. To claim that it is also a multiply realisable higher-level property that ensures that some lower-level properties are instantiated may seem inconsistent. But this appearance disappears once the following point is appreciated. The more fundamental

momentum properties appealed to are properties of objects distinct from the body of water instantiating the temperature property: they are properties of water molecules, the constituents of the body of water. As stated at the end of §3.1, HDP implies that the relation between the water and a number is only fundamental *relative to the water*. This is perfectly consistent with the claim that the momentum properties of the molecules are ultimately more fundamental. Jackson and Pettit's story about the relationship between the two explanations is therefore available to the heavy duty platonist: the relation between a number and the water ensures that some water molecules instantiate the appropriate momentum properties.

The heavy duty platonist can therefore meet (i) above, but cannot yet meet (ii) or (iii). Unlike Jackson and Pettit, the heavy duty platonist cannot claim that the momentum properties programmed for by the temperature property are causally efficacious because she considers these properties to be relations holding between water molecules and numbers. Thus HDP faces (ii) mentioned above: explain how the momentum properties of the water molecules are relevant to the breaking of the glass. The model offered by Pettit and Jackson will be of no help here, for there are no properties of the constituents of the molecule to appeal to; and even if there were, appealing to them would be pushing back the problem. Sooner or later the heavy duty platonist will have to explain how the properties of some objects are relevant to the causal production an effect in terms of the properties of those objects. Thankfully, there is such an explanation.

The heavy duty platonist must identify some property instantiated by the relevant water molecules, the instantiation of which causes the glass to break. She will have to provide an account rendering it plausible that, for each molecule, the instantiation of their momentum property is metaphysically responsible for the

instantiation of the property that helps cause the glass to break. Jackson and Pettit's theory cannot help because the ensuring relation runs from higher-level derivative properties to more fundamental properties. According to HDP, the momentum property of a molecule is fundamental with respect to the water molecule, so the property which causes the glass to break must be understood as metaphysically derivative from it. My suggestion is that the causally efficacious property is the disposition *being prone to break glass*, instantiated by the water molecules that strike the glass. It is plausible that dispositional properties are causally efficacious. (McKittrick 2005 has shown that, on the most plausible accounts of causality, dispositions are causal; see also Mumford and Anjum 2011 for a theory of causation based on a metaphysics of dispositions.) And now we have a straightforward and plausible story to tell about why the instantiation of this disposition is dependent on the instantiation of the momentum properties of the molecules: a collection of molecules is prone to break glass because they have one of many distributions of momentum properties sufficient to break glass when they strike it. Moreover, the threat of causal over-determination disappears because there is only one property that causes the glass to break, namely the disposition; the momentum properties non-causally determine that the molecules have the disposition, while the temperature property non-causally ensures the instantiation of the momentum properties.

This account does not attribute any causal powers to the numbers the molecules are related to. The role they play is to help determine, non-causally, that the physical objects they are related to have a certain causally efficacious disposition. The thought that a relation a physical object stands in can endow that object with certain dispositions is not incoherent, even if the other relatum is an abstract object. Indeed, it is familiar: believing a proposition can dispose the believer to behave in

certain ways. The apparent inconsistency of HDP was appealed to in order to undermine this commonplace view of intentional states. Having shown that HDP is not inconsistent, the heavy duty platonist is free to appeal to philosophy of mind to demonstrate that non-causal determination of effects by abstracta is widespread. For example, believing that red kidney beans contain a high concentration of toxin disposes one to boil them for ten minutes before using them in cooking. Frege puts the point as follows:

How does a thought act? By being apprehended and taken to be true. This is a process in the inner world of a thinker which can have further consequences in this inner world of a thinker which can have further consequences in this inner world and which, encroaching on the sphere of the will, can also make itself noticeable in the outer world. If, for example, I grasp the thought which we express by the theorem of Pythagoras, the consequence may be that I recognise it to be true and, further, that I apply it, making a decision which brings about the acceleration of masses. Thus our actions are usually prepared by thinking and judgement. And so thought can have an indirect influence on the motion of masses. (Frege 1956: 310)

It is quite plausible to hold that some physical objects have some of their causal powers by instantiating relations to non-causal entities. The heavy duty platonist has no reason to reject C1 and every reason to reject C4; and this renders C5 harmless. The causal argument fails.

3.4 Arguments from Field

Field (1989: 171-226) relies on the falsity of HDP to vindicate a substantivalist view of space-time, and undermine a relationalist view. He claims that the latter implies HDP and so is untenable. Why does Field think that HDP is so poisonous?

One argument Field offers is aimed at showing that HDP is inconsistent with both the letter and spirit of relationalism. The letter demands that only relations between aggregates of matter be posited in our fundamental theory. Yet HDP involves relations between aggregates of matter and numbers. The spirit of relationalism is expressed as the thought that ‘only quite unproblematic relations will suffice’ (1989: 192). Yet Field (1989: 192) claims relations between physical objects and numbers are ‘extremely problematic if not somehow demystified’, and adopting HDP involves claiming that such relations have no explanation.

This does not yet amount to an argument against adopting HDP, but only questions the coherence of a relationalist doing so. However, the spirit of relationalism suggests a plausible assumption about what our physical theories should be like from which a more general argument against HDP can be constructed.

The assumption we can take from the spirit of relationalism is this. Our physical theories should take only unproblematic relations as fundamental. Along with the premise that HDP employs problematic relations, this would count against HDP. Call this the ‘problematic argument’.

The premises are not yet well motivated. Why is a physical object’s being related to a number via a magnitude relation something that should puzzle us and require further explanation? Say an object has a mass of 10kg. We have knowledge of each relatum, albeit of a different sort, and we know what it is that relates them, namely *mass in kilograms*. We also know what the conditions are in which we can

reliably say when the object stands in such a relation, and know what behaviour this determines in the object. What exactly is it that needs explaining here? We need to know what the unproblematic relations are, and why they do not require further explanation. Take the causal relation. This is often considered innocuous by nominalists such as Field, but I fail to see that our understanding of the causal relation goes any further than what I have just said about *mass in kilograms*. The problematic argument is not successful.

Field motivates one further objection to HDP by drawing attention to the fact that it must explain the behaviour of physical systems in terms of relations between physical objects and numbers. Such explanations, he claims, are extrinsic ‘because the role of numbers is simply to serve as labels for some of the features of the physical system’ (1989: 193), and undesirable because, whenever one has such an explanation, ‘one wants an intrinsic explanation that underlies it: one wants to be able to explain the behaviour of the physical system in terms of the intrinsic features of that system’ (1989:193). Call this the ‘explanatory argument’.

Elsewhere, Field specifies that, by ‘intrinsic features’ of a physical system, he means those features that are causally relevant to its behaviour (1989: 18). The response to the causal argument in §3.3 is enough to see off this objection: though HDP has physical objects related to non-causal objects, those relations are nevertheless relevant to the causal behaviour of physical objects.

One could suggest a different interpretation of ‘intrinsic explanation’: an explanation that only mentions intrinsic properties of the system, or the objects that comprise it. In which case, the response to the arguments from Lewis outlined in §3.2 will be enough to avoid this objection. According to a plausible and popular analysis of intrinsicality, the properties of physical objects posited by HDP are

intrinsic. If the proponent of the explanatory argument is loath to accept the Lewisian analysis, the heavy duty platonist can point out that our intuitions regarding which properties are intrinsic are misleading anyway. The proponent of the explanatory argument will surely accept explanations of physical behaviour in terms of mass; but we have seen that an object's mass involves something distinct from that object.

Whichever interpretation is endorsed, the failure of the explanatory argument is ultimately due to the same error. The view that the role of mathematics in explanations is simply to label physical features is assumed on behalf of the heavy duty platonist; but the heavy duty platonist would not, and must not, assume this view. Rather, she has it that there is a robust metaphysical connection between physical objects and numbers that renders the latter explanatorily relevant to physical phenomena.

3.5 Conclusions

I have shown that all objections to HDP alluded to in the literature fail. In 3.1, I showed that HDP is not threatened by the intuition that some physical magnitudes are intrinsic properties. In 3.2, I showed that there are good reasons from science to think that there is one metaphysically privileged unit of measurement for each magnitude. But even if this appeal to science fails, the heavy duty platonist could maintain that one unit of measurement more accurately represents the way the world is made up, despite its not making any difference to the success of science. I also showed that HDP's physical magnitudes being partly constituted by causally inert entities does not stop them from being causally relevant to physical phenomena.

Some of my responses involved embracing what initially look like strong theses. The theses that spring to mind are that there is a metaphysically privileged

unit of measurement, and that causal properties can depend on non-causal relations. This appearance is what explains why HDP has been assumed false in the literature. However, I have shown that this appearance is misleading. There are coherent and plausible versions of HDP that imply these theses. As it stands, then, HDP is a live option in the philosophy of mathematics. The philosophical ramifications of this conclusion are significant. As I have mentioned, many arguments rest on the assumption that HDP is untenable. This assumption is unwarranted, so the arguments that rest on it should be re-assessed.

Importantly for the purposes of this thesis, this means that Melia's attack on the indispensability argument cannot be supported by appealing to existing objections against HDP.

4. The Indispensability Argument Dispensed With

4.1 Introduction: three challenges

Recall from §2.3 that Melia's attack on IA relied on two claims: first, that taking mathematics to play an explanatory role in science is tantamount to endorsing HDP; second, that HDP is implausible. We saw in the previous chapter that supporting the second claim is an unpromising means of supporting Melia's attack. In §2.3, I suggested that there is at least room for the platonist to respond to the first claim. In fact, we shall see in this chapter that the platonist can also claim that mathematics plays an explanatory role in science without thereby endorsing HDP.

In light of this, it may appear that Melia's attack on the indispensability argument is in trouble. However, in this chapter, I will show that the modified version Melia's simplicity argument (SA) presented in §2.3 is successful. I will do this by arguing for the following two claims: that taking mathematics to play an explanatory role that is sufficient for supporting IA is tantamount to adopting HDP; and that HDP promises to provide a worse account of certain kinds of mathematical explanation than abstract expressionism. To do this, I have to make clear what exactly the platonist would have to do to render IA successful.

Platonists wishing to support IA in light of Melia's arguments have sought to show that mathematics plays more than a representational role in science by pointing to examples of scientific explanations in which they claim the mathematics plays a genuine explanatory role (see for example Baker 2005; Lyon & Colyvan 2008; and Lyon 2012). Unfortunately, no full account of 'genuinely explanatory role' that supports IA has been provided (Saatsi forthcoming offers several accounts, but argues they do not help support platonism). Without such an account, these putative

cases of mathematical explanation are not conclusive. Indeed, talk of some entities appears to be explanatory only insofar as it allows us to better understand the target explanandum. Call such a role *epistemically explanatory* (cf. Marcus 2014: 348). For example, talking about frictionless planes and bottomless oceans in science facilitates a better understanding of real planes and oceans, but no one is tempted to say such things exist (see Maddy 1997: 143-152). If the platonist is going to appeal to the explanatory role of mathematics, it must be an explanatory role in which mathematical objects are required to exist for the target explanandum to come about. Call this kind of role *metaphysically explanatory* (cf. Marcus 2014: 348). (Saatsi forthcoming makes the same distinction between metaphysical and epistemic explanatory roles, though he uses the terms ‘thick explanatory role’ and ‘thin explanatory role’.)

Aidan Lyon (2012) heads in the right direction by claiming that the explanatory role of mathematics can be understood in terms of Frank Jackson and Philip Pettit’s (1990) theory of program explanation, according to which the instantiation of non-causal properties can ‘program for’ the instantiation of the causal properties that produce the target explanandum. However, Juha Saatsi (2012) rightly holds Lyon to account for not doing enough to show that the examples appealed to are program explanations, and for not providing a metaphysical account of how mathematical properties can play a programming role. Platonists must show that the putative cases of mathematical explanation fit with the invoked theory of scientific explanation, and provide a metaphysical account that explains how mathematical objects can play the role that the theory ascribes to them.

In light of these developments, to render IA successful, the platonist must meet the following two challenges:

Challenge 1: Find scientific explanations in which mathematics plays an indispensable explanatory role.

Challenge 2: Provide a metaphysical account of the nature of mathematical explanation that requires the mathematical objects to exist.

However, there is one final challenge that is seldom made explicit (although see Melia 2000: 1995: 229 and Knowles & Liggins forthcoming). Even if the platonist meets challenges 1 and 2, she must show that the explanations appealed to cannot be better accounted for by the abstract expressionist. Merely providing an account of mathematical explanation according to which mathematical objects must exist is therefore not enough to establish platonism. We must add one final challenge:

Challenge 3: Show that an expressionistic account of the nature of mathematical explanation is unavailable for the relevant examples; otherwise, show that the platonistic account is preferable.

In what follows, I argue that the platonist can meet challenges 1 and 2 fine; but meeting challenge 3 would render the indispensability argument redundant. In §4.1, I show how challenge 1 can be met. I outline three explanations from evolutionary biology that are paradigmatic of a kind of explanation that promises to be widespread in science—optimality explanations. I show that in optimality explanations mathematics plays an indispensable explanatory role. In §4.2, I show how challenge 2 can be met. I show that optimality explanations are naturally understood as program explanations, and that, given some background metaphysical

assumptions, this leads naturally to a metaphysical account of the nature of mathematical explanation that requires mathematical objects to exist. In §4.3, I do two things. First, I show that challenge 3 cannot be met by appealing to optimality explanations understood as program explanations because wherever the platonistic account of the nature of mathematical explanation is available, there will always be an expressionistic account available that is preferable. Second, I argue that the platonist cannot meet challenge 3 without thereby undermining IA. I conclude that, the modified version of Melia's simplicity argument, SA, is sound, and so IA fails.

4.2 Optimality and robustness (challenge 1)

To those who wish to account for scientific explanation in causal-mechanical terms, the use of mathematical models to explain physical phenomena poses a challenge. Models are often strictly speaking false in virtue of containing idealised and fictitious elements, and their relevance to the causal-mechanical goings on in the target system is often unclear. Causal-mechanical explanations should account for the elements that make up the physical system and the causal interactions between these elements that give rise to the explanandum. However, explanations that appeal to models do not obviously cite any particular causes or mechanisms. In each case, there is plausibly a causal-mechanical process that gives rise to the explanandum, but the mathematical model does not obviously attempt to describe it.

Some philosophers have nevertheless attempted to show how the explanatory role of models can be accounted for in causal-mechanical terms. Some see models as abstract descriptions of the function of causal mechanisms, descriptions of their input and output, useful only because they play the role of standing in for poorly understood causal mechanisms and the heuristic role of guiding research towards a

better understanding (see for example Craver 2013: 149-151). Others claim that such descriptions are useful because there is a mapping between the model and the target mechanism which allows the model to represent particular causal relations, or patterns of causal connectivity in the mechanism that are particularly important for the production of the explanandum (see for example Levy and Bechtel 2013).

These attempts to account for the use of models in science in causal-mechanical terms all involve treating the model as some edited representation of the underlying causal mechanism. In each, knowledge of the causal mechanism is required, or missing to the detriment of the explanation (cf. Irvine forthcoming). The problem with this kind of view is that models are often very useful for explaining the behaviour of systems about whose underlying causal mechanisms very little is known, and also appear to be explanatory *in virtue* of their disregard for any causal-mechanical details. In these cases, the methodology of using models to explain is reversed. Instead of abstracting away from known or poorly understood causal-mechanical details, scientists start with an abstract syntactic schema, or *template*, and generate models by specifying how parts of the template are to be interpreted to fit the target system. Importantly, this process generally draws on background knowledge about the emergent behaviour of the target system, not its underlying mechanisms.

This process of modelling is particularly useful for explaining the behaviour of systems in evolutionary biology and in cognitive science. This is because, though the emergent behaviour of these systems is well understood, very little is known about the underlying mechanisms. In particular, this form of modelling is very useful for providing explanations of systems which reliably converge towards some kind of optimal state across a variety of alterations to the system. Models are used in these

cases to provide *optimality explanations*. In optimality explanations, models are generated from templates so that a certain currency is maximised while taking into account certain constraints and trade-offs. The examples I will consider in this section are from evolutionary biology, where the currency is the reproductive success of organisms or phenotypes (fitness). The trade-offs include the effect that the fitness of one phenotype has on the fitness of another, and the effect that two attributes of a single phenotype have on the fitness of that phenotype, etc.

The fact that optimality models make no attempt to represent the mechanisms underpinning the target phenomena, the fact that they accurately predict the emergence of the same behaviour or attribute across a wide range of differences (in phenotypes, in environment, in methods and patterns of reproduction, etc.), and the fact that what they do appear to represent are largely structural features of the target systems, has led many theorists to argue that their use amounts to a certain kind of non-causal explanation of physical phenomena (see for example Pincock 2012: 58-67; Batterman 2002a, 2002b; Rice 2012, 2013). Optimality explanations are supposed to show that, no matter what the particular causal-mechanical goings on in the target system, so long as certain structural features are instantiated, a particular behaviour or attribute emerges. Importantly, optimality explanations are good explanations in virtue of their neglecting causal-mechanical information; they explain the emergent behaviour in terms of structural features of the target system. I will now present three examples of optimality explanations and argue that the mathematics in them plays an indispensable explanatory role.

Honeybees

The first example was first highlighted by Colyvan and Lyon (2008). Honeybees make honeycomb with hexagonal cells. Why? There are two components to the explanation. First, the biological component which highlights the evolutionary advantages of creating honeycomb in a way that uses the least resources possible, given certain background constraints and trade-offs. Second, the mathematical component which consists of a proof that the most efficient way of dividing up a two-dimensional plane into partitions of equal area with the least total perimeter is to divide it into a partition of hexagonal cells. These two components together provide a model that tells us fitness is maximised by organisms that build hexagonal honeycomb across a wide range of differences in the target system. Hence, the fact that honeybees evolved to do so is explained.

Cicadas

The second example was first introduced by Alan Baker (2005). Periodical North American cicadas spend most of their life as larva underground. After a period of either 13 or 17 years, depending on the sub-species, they emerge for a few weeks to eat breed and die. Why do cicadas have prime-numbered life-cycles? Again, there are two components to the model. The biological component which highlights that it is advantageous for cicadas to minimise the chance of emerging when there are predators in the area or when there are other species with which the cicadas should not mate, given certain background constraints and trade-offs. The cicadas can achieve this by evolving to have a life-cycle that minimizes overlap between their time above ground with the time above ground enjoyed by other periodical organisms in

the area. The mathematical component is a number-theoretic proof that prime numbers, such as 13 and 17, maximize their lowest common multiple (LCM) with other nearby integers. For example, the LCM of 14 and 12 is 84, while the LCM of 14 and 13 is 182. If there are two organisms with periodical life-cycles of a similar length, the number of years between each overlap of their time above ground corresponds to the LCM of the numbers corresponding to their life-cycles in years. Hence, organisms with prime-numbered life-cycles minimise their overlap with other periodical predators and incompatible mates. The two components together provide a model which shows that the fitness is maximized by organisms with prime-numbered life-cycles across a wide range of differences in the system, and thus explains why periodical North-American cicadas evolved in this way.

Marine mammals

The final example was recently introduced to the debate by Sam Baron (2014). The explanandum is the fact that when marine mammals forage in prey-scarce environments without knowing where the prey is, they move in accordance with a particular mathematical representation of super-diffusive random motion called a *Lévy walk*. In non-mathematical terms, movement in accordance with a Lévy walk involves ‘a random sequence of larger jumps, interspersed with several smaller jumps with frequent reorientation’ (Baron 2014: 476). Again, there is a non-mathematical component to the explanation which highlights the fact that it is advantageous for organisms to increase their chance of finding prey, given certain background constraints and trade-offs. The mathematical component is a proof that Lévy walks maximize the

chance of hitting targets that are randomly distributed and scarce. Both components together yield a model according to which fitness is maximised by organisms that forage in accordance with a Lévy walk in prey scarce environments across a wide range of changes in the system, and so explains why marine mammals have evolved to employ this foraging tactic.

In each of these examples, no mention is made of the causal-mechanical process of the development of the relevant behaviours or attributes, though one surely exists in each; only important structural information is provided about the organisms and their environment. Moreover, it is the mathematical component that provides the core of each explanation. It demonstrates that, in systems with a particular structure, a particular behaviour will emerge regardless of whatever else goes on in that system. These are non-causal explanations in which mathematics appears to be of central importance. (Raz 2013 argues that *Honeybees* greatly simplifies the structure of honeycomb, and may turn out to be irrelevant once the actual structure is accounted for; if this is right, consider *Honeybees* a mere illustration of an optimality explanation.)

To understand better the role mathematics is playing in these explanations, we might compare one of the above explanations with the corresponding possible causal-mechanical explanation. Though it may be impossible to describe, either because we currently do not understand the mechanisms enough or because we do not have the required time or resources, we can be sure that there was some causal-mechanical process that led to the foraging behaviour of marine mammals. So, let us pretend that we are in possession of a complete description of this process, in which

case we would have a causal-mechanical explanation of the target behaviour. However, there are two important features such an explanation would lack.

The first is important modal information. If the causal-mechanical process had gone slightly differently, then the relevant behaviour would still have emerged. Yet, our causal-mechanical explanation tells us nothing about what would happen had things gone differently. We might enhance this causal-mechanical explanation by adding to it descriptions of all the possible causal-mechanical processes that can possibly lead to the relevant behaviour in the target system. Again, do not worry for now about the plausibility of finite beings achieving such a feat; this is just an illustration. In this scenario, we would be in possession of a causal-mechanical explanation that implies all the desired modal information. However, there would still be something lacking. This explanation describes how things go in nearby worlds, and explains in each world why things turn out the way they do. However, it fails to explain what these worlds have in common in virtue of which they are the closest possible worlds and in virtue of which the emergent phenomenon is the same. Though it describes the conditions sufficient for the truth of the relevant counterfactuals, it fails to explain why these counterfactuals are true. Another way of putting it is that it describes the structure of modal space by describing what goes on in nearby possible worlds, but does not explain why the modal space exhibits this structure.

Contrast these possible explanations with the optimality explanation *Marine mammals*. This explanation implies all the relevant modal information by implying that, no matter how things go with the underlying mechanisms, the system will still converge to the optimum behaviour. This implies that, in nearby worlds where the relevant structural constraints are met, marine mammals will still evolve to forage in

accordance with Lévy walks. Moreover, it explains what these worlds have in common in virtue of which these counterfactual claims are true. As well as the background environmental constraints and trade-offs, it is the soundness of the relevant mathematical proof that is constant across these worlds. That is, in all these worlds, Lévy walks are the optimum means of coming across randomly distributed and scarce targets. Hence, in all these nearby worlds, the optimal means for marine mammals to forage for prey is to move in accordance with Lévy walks. It is clearly the mathematics that contributes this modal robustness to *Marine mammals*, and the same goes for the other two optimality explanations, *Honeybees* and *Cicadas*.

Optimality explanations are modally robust non-causal explanations. Moreover, if the mathematical components are removed, the resulting explanations are not modally robust. (Colyvan and Baker 2011 highlight that nominalistic alternatives to mathematical explanations are not modally robust.) This warrants the conclusion that mathematics plays an indispensable explanatory role in optimality explanations. Moreover, it is becoming standard practice in biology to provide optimality explanations of the emergence of traits and behaviours in organisms (see Sutherland 2005), so examples of mathematics playing this role promise to be widespread. It appears that challenge 1 has been met.

It must be emphasised that this does not yet tell us anything about the role that mathematical objects play in explaining the relevant phenomena. We have seen that talking about the properties of two-dimensional planes, hexagons, prime numbers and Lévy walks allows us to provide modally robust explanations of physical phenomena. However, we do not yet know why talking about mathematical objects is useful in this way. Importantly, we do not yet know whether the role these objects are assigned by the above explanations is an epistemically explanatory one or

a metaphysically explanatory one. For the platonist to meet challenge 2, she must provide a metaphysical account of the explanatory role of mathematics according to which mathematical objects are metaphysically responsible for the target phenomena.

4.3 Mathematical realisation (challenge 2)

The only promising attempt so far of providing an account of the nature of mathematical explanation in support of the indispensability argument is Lyon's (2012). (Colyvan 2002 highlights the unifying power of mathematical explanation and Baker 2005 seems to assume something like the deductive-nomological account of explanation. However, I do not see how these accounts can have any ontological implications.) Lyon develops Colyvan's (2010: 51-53) suggestion that mathematical explanation might be understood in terms of Jackson and Pettit's (1990) program explanation, introduced in §3.1. For convenience, I will again present Jackson and Pettit's (1990: 110) example of program explanation.

Temperature

Water filling a closed glass container reaches boiling point and the glass breaks. Why does the glass break? There are two competing explanations. The first mentions the temperature property of the water as sufficient for breaking the glass—the 'program explanation'. The other mentions the particular water molecules that strike the glass and the momentum properties sufficient for breaking it—the 'process explanation'. Because the temperature property must be realised by an arrangement of molecules with momentum sufficient for breaking glass, the instantiation of the temperature property

non-causally ensures the instantiation of the momentum property that breaks the glass. Jackson and Pettit say the higher-level property *programs* for the instantiation of the lower-level ones. The instantiation of the temperature property is causally relevant to the breaking of the glass, though it is the instantiation of momentum properties that cause it.

In this example, the process explanation exhibits the same two limitations as those exhibited by the causal-mechanical explanations corresponding to optimality explanations. In the closest possible world in which the particular molecules mentioned take a slightly different path and do not strike the glass, the glass still breaks because some other molecules strike the glass. In all the closest possible worlds in which the water is at boiling point, the glass will break. The description of the actual causal-mechanical process leading up to the breaking of the glass says nothing about what happens in these worlds.

Again, the process explanation can be expanded so that it describes the possible causal-mechanical processes leading up to the breaking of the glass in these worlds. However, this explanation still fails to explain what these worlds have in common in virtue of which they are the closest possible worlds. Though it describes the conditions sufficient for the truth of the relevant counterfactuals, it fails to explain why these counterfactuals are true. It describes the structure of modal space by describing what goes on in nearby worlds, but does not explain why modal space exhibits this structure.

Contrast this with the program explanation. Because the temperature property must be realised by some arrangement of molecules sufficient for the breaking of the glass, the temperature property *programs* for the breaking of the glass. This

immediately carries with it all of the relevant modal information: had the particular molecules taken a different path, the glass would still have broken because the temperature of the water would ensure some other molecules hit the glass with sufficient momentum. It also explains why modal space is structured in the way it is: each of the nearby possible worlds are worlds in which the water is at boiling point, ensuring that some molecules will strike the glass with momentum sufficient to break the glass. For the same reasons mentioned with respect to optimality explanations, programming properties are indispensably explanatory: mentioning them makes our explanations modally robust.

Though he doesn't identify them as such, most of the examples Lyon appeals to are optimality explanations, including *Honeybees* and *Cicadas* (2012: 560-564). He appeals to the modally robust nature of his examples to argue that they are program explanations in which mathematics plays a programming role (2012: 565-568). However, Saatsi rightly points out that Lyon must do more show that the examples he appeals to fit the program explanation theory and provide a metaphysical account of how mathematical properties can play a programming role (2012: 580-582). Saatsi argues that the examples Lyon appeals to are not easily understood as program explanations because they are explanations of regularities. He claims that there cannot be a causal history leading up to a regularity, so there is no candidate process explanation in each of Lyon's examples (2012: 580-581). He also claims that the prospects for a suitable metaphysical account of how mathematical properties can play a programming role are dim (2012: 582-583).

In the remainder of this section, I will argue that Saatsi is wrong on both counts. Optimality explanations are naturally interpreted as explaining events and so in each there is a corresponding process explanation. Moreover, along with some

background metaphysical assumptions, this leads naturally to a metaphysical account of how mathematical properties can play a programming role, and according to which mathematical objects must exist.

I hope that the way I have introduced optimality explanations and program explanations has made their similarities salient. The paradigmatic program explanation *Temperature* and the paradigmatic optimality explanation *Marine mammals* are both explanatory in the same way. They both mention a non-causal property and in doing so provide a modally robust explanation of the behaviour of some physical system. Perhaps optimality explanations can be presented as explanations of regularities. *Marine mammals* may be presented as an explanation of the regularity that marine mammals tend to forage in accordance with Lévy walks. However, it is also naturally presented as an explanation of the event of marine mammals evolving to behave this way. This event has a causal-mechanical process leading up to it, so the explanandum admits of a process explanation. The process is probably too large and complex for us to ever properly account for, but the same holds true of *Temperature*. It is unlikely that we will ever know or care which particular water molecule struck which glass molecule. Both the program explanation in *Temperature* and *Marine mammals* omit causal information and provide good explanations of the target phenomena in virtue of this fact rather than in spite of it. Contra Saatsi, optimality explanations are naturally understood as program explanations.

How are we to account for the programming role of mathematical properties? In *Temperature*, the metaphysics of programming is straightforward: the higher-level temperature property is multiply realisable by molecules in motion. Thus, in each world in which the water instantiates the temperature property, the molecules must

be in such a state that ensures some molecules will have sufficient momentum to shatter the glass. In this case, it is the instantiation of the realisation relation that is doing the metaphysical work. In *Marine mammals*, however, the properties apparently doing the explanatory work are instantiated by an abstract mathematical structure—a Lévy walk.

Saatsi rules out various ways the realist might account for the programming role of mathematical properties. He rules out any platonist account because it will imply that the properties of mathematical objects are instantiated independently of any causal properties (2012: 583). The former could not therefore ensure the latter. However, Saatsi considers another account that is compatible with platonism, whereby mathematics plays a programming role ‘not in and of itself, but as an indispensable part of some kind of physical-cum-mathematical property complex’ (2012: 583). Saatsi confesses to having ‘no idea’ what such a complex might be like and how it can play a programming role (2012: 583). Here is one.

Platonists believe that there are certain non-causal relations that hold between mathematical objects and physical objects. These relations are typically, and most plausibly understood as structure preserving mappings. (This notion is central to Pincock’s 2004 mapping account of the applicability of mathematics; see Bueno and Colyvan 2011 for developments). For convenience, I will say that when there is a structure preserving mapping from one thing to another, they stand in the *shares structure with* relation.

When a relation obtains, each relata instantiates a relational property. For example, I stand in the *smaller than* relation to the sun, so I instantiate the property *being smaller than the sun*. A particular foraging of a marine mammal bears the *shares structure with* relation to a Lévy walk, so the foraging instantiates the higher-

level relational property *shares structure with LW*, where LW is a particular Lévy walk. This property can be realised by any number of different physical systems, so the same metaphysics underlying program explanation can be appealed to in this case. By mentioning the properties of Lévy walks, *Marine mammals* characterises higher-level relational properties instantiated by foragings of marine mammals. The instantiation of these properties non-causally ensures that one of the many causal-mechanical processes that may have led to the mammals evolving in this way occurred.

This account can be generalised to the other optimality explanations. Each characterises a class of higher-level relational properties of the form *has structure M*, where M is to be filled in with one of a particular class of mathematical structures. These higher-level properties must be realised by some physical state which is advantageous to the organism in question, and ensures that one of the many causal-mechanical processes leading up to this state occurred. Hence, this ‘physical-cum-mathematical property complex’ non-causally ensures the causal production of the explanandum. This is a plausible metaphysical account of how mathematical properties can play a programming role. For reasons that will become clear, I call this account *mathematical realisation plus* (MR+).

What MR+ tells us about the relevant mathematical objects is not yet clear. On the present story, by mentioning the properties of prime numbers, *Cicadas* characterises a certain higher-level properties of cicadas’ life-cycles. Crucially, it is not the properties of prime numbers that are doing the programming here, but rather the higher-level relational properties of life-cycles, though the latter are characterised via talk of the former. So, does MR+ require that mathematical objects exist? The following argument suggests that it does. Relational properties are instantiated only

if a relation obtains between the instantiating object and the other object(s) the property concerns. According to MR+, optimality explanations reveal that physical systems instantiate relational properties which concern mathematical objects. So, according to MR+, optimality explanations reveal that relations obtain between physical systems and mathematical objects. Since, a relation obtains only if its relata exist, we can conclude that, according to MR+, optimality explanations reveal that physical systems and mathematical objects exist.

Things are looking good for the platonist. We saw in §4.2 mathematics plays an indispensable explanatory role in optimality explanations, and that optimality explanations promise to be widespread in science. In this section, we have seen that optimality explanations are program explanations. Along with some background metaphysical assumptions, this leads naturally to a metaphysical account of the programming role of mathematical properties according to which mathematical objects must exist. Challenge 1 and challenge 2 have been met.

4.4 Expressionistic vs. heavy duty explanation (challenge 3)

To meet challenge 3, the platonist must show either that there are no expressionistic accounts of optimality explanations available, or that some platonistic interpretation is preferable. Several expressionistic accounts of particular optimality explanations have been offered. For example, *Honeybees* need not be interpreted as appealing to geometrical properties of hexagons; the explanation works just as well if we interpret the explanation as concerning the geometrical properties of space-time (see Saatsi 2011: 146-152 for discussion). Similar accounts of *Cicadas* have been offered (see Daly and Langford 2009: 657 and Saatsi 2011: 150-154; and Rizza 2011).

Might the indispensability argument be undermined on a case-by-case basis in this way? Two features of optimality explanations highlighted by Baron (2014: 469-476) suggest not. First, we have seen that optimality explanations promise to be widespread in science, meaning that the abstract expressionist is going to have a lot of work to do. Second, the mathematical explanatory core of each optimality explanation is typically quite different. In *Honeybees*, *Cicadas*, and *Marine mammals*, the explanatory cores are not only different proofs; they belong to different areas of mathematics. This suggests there is no guarantee that the methods used in the above expressionistic accounts will be successful across the board. This does not imply that MR+ is not universally applicable. Optimality explanations explain the emergence of physical behaviour by using mathematics to show that physical systems with certain structural features always produce this behaviour. The relevant mathematical objects must therefore exhibit the same structure as the physical system for it to be explanatorily relevant in this way, which strongly suggests that MR+ is universally applicable.

In this section, I will provide an argument that, wherever there is an MR+ account of an optimality explanation, there is an expressionistic alternative available. Since MR+ looks to be applicable across the board, so will this alternative. Moreover, I will argue that this alternative will always be simpler and so preferable. This shows that the platonist cannot meet challenge 3 by appeal to optimality explanations and program explanation. I then argue that the only means of meeting challenge 3 left for the platonist is to show that a radically different metaphysical account of the nature of mathematical explanation is preferable to the expressionistic one outlined below. In doing this, I will vindicate two claims that capture the spirit of the claims on which Melia's attack on the indispensability argument depend.

Central to the fact that MR+ enables the platonist to meet challenge 2 is the claim that structure-preserving mappings hold between physical systems and mathematical objects. It is this metaphysical assumption that implies that each physical system instantiates a physical-cum-mathematical property of the form *shares structure with M*, where M is the relevant mathematical object or objects. I will now show that, in each case, this property is instantiated in virtue of a multiply-realizable structural property instantiated by the physical system alone and a structural property instantiated by the mathematical system alone.

According to MR+, a property of the form *shares structure with RW*, where RW is a particular Lévy walk, is instantiated by each foraging F in virtue of a structure-preserving mapping holding between F and RW. A structure-preserving mapping obtains between two systems in virtue of the structures they have. So, the mapping between RW and F obtains in virtue of their structures. That is, there is a structural property of RW and a structural property of F in virtue of which they stand in the *shares structure with* relation. Since the *in virtue of* relation is transitive, we can conclude that the program property is instantiated by F in virtue of some structural property of F and some structural property of RW. Moreover, any number of physical systems in any number of configurations can stand in the same relation to RW, and each will do so in virtue of some physical structural property, so we can conclude that this physical structural property is higher-level and multiply realizable. Call this property *shares structure with F*. The same reasoning applies to any optimality explanation.

According to MR+, for each optimality explanation, there is a multiply-realizable structural property instantiated by the relevant physical system alone. It is not hard to see that in each case this property programs for the relevant lower-level

properties. Whenever *shares structure with F* is instantiated by a foraging, it must have one or other of the distribution of lower-level causal properties that instantiate it and one or other of the causal processes that might lead up to the organism evolving in this way must have occurred. Thus, a rival metaphysical account of optimality explanations is available according to which, in each case, the structural property instantiated by the physical system alone is the program property. The role of mathematical language according to this view is just to characterise this property by describing mathematical objects that exhibit the same structure. Importantly, this role does not depend on the obtaining of any real relation between these mathematical objects and the physical system. The mathematical objects might be imaginary; they are described only as a *means of representing* the relevant physical property which then does the explanatory work. For reasons I will give presently, I will call this expressionistic interpretation *mathematical realisation* (MR).

There are two reasons for preferring MR to MR+. The first is that MR is compatible with the view that mathematical objects do not exist. MR makes no appeal to the existence of mathematical objects and does not invoke relations between mathematical objects and physical systems. In this respect, MR is the simpler theory. The second reason is that MR+ has the unfortunate entailment that, for each optimality explanation, there are two program properties instantiated where there need be only one. Both MR and MR+ entail that there is a higher-level structural property instantiated by the target physical system alone which programs for the causal production of the explanandum. However, according to MR+ there is an extra physical-cum-mathematical property that also programs for the causal production of the explanandum. This is why I have reserved the name ‘MR’ for the expressionistic interpretation. MR is the simpler theory, since MR+ is just MR plus

the unnecessary duplication of program properties and the unnecessary invoking of mathematical objects and relations. In short, when compared with MR, MR+ is metaphysically baroque.

The above reasoning shows that wherever MR+ is applicable to an optimality explanation, MR provides a preferable interpretation. There is good reason for thinking that MR+ is applicable to all optimality explanations, so there is good reason for thinking that MR provides a preferable interpretation of all optimality explanations. We can conclude that the platonist cannot meet challenge 3 by appealing to optimality explanations and program explanation.

What went wrong for the platonist? Things seemed to be going so well at the end of §4.2. The problem is, as we saw in §3.3, the direction of program explanation runs in the opposite direction to metaphysical explanation (cf. Knowles forthcoming a). In *Temperature*, the higher-level temperature property is instantiated in virtue of the distribution of momentum properties among the water molecules. The momenta of the molecules give rise to the temperature. Metaphysical explanation is bottom-up. In contrast, it is the temperature property that programs for the instantiation of these momentum properties. Program explanation is top-down. For this reason, program explanations are plausibly only epistemically explanatory: once we know that the programming property is instantiated, we can be sure that one or other of the physical realisations of it must obtain. Providing a metaphysical account of programming according to which an object distinct from the physical system must exist for the programming relation to obtain is therefore bound to result in a metaphysically baroque theory. I will illustrate with *Temperature*. In ascribing the temperature property to the water, the number 100 is mentioned. We can provide a metaphysical account of *Temperature* according to which this number must exist in

order for the programming relation to obtain by positing a relation between the body of water and the number. A plausible candidate is the *degrees Celsius* relation. However, it is clear that we need not invoke this relation to make sense of *Temperature*. Invoking a multiply-realizable property of the water is enough.

At this point, it might seem that the best option for the platonist is to appeal to a different theory of scientific explanation. Interpreting optimality explanations as program explanations does not assign a metaphysically explanatory role to mathematical objects, but perhaps interpreting them in terms of another theory will. Saatsi (forthcoming) helpfully provides a list of the best contemporary accounts of scientific explanation and indicates how mathematical explanations can be understood in terms of them. However, Saatsi finds that the role assigned to mathematics by each theory is not a metaphysically explanatory one (though he uses the term ‘thick explanatory role’). I have no doubt that metaphysical accounts according to which mathematical objects must exist can be developed for each, but since the relevant mathematical objects will only be assigned an epistemically explanatory role, a simpler expressionistic alternative will likely always be available. After all, optimality explanations work by characterising the structure of the target physical system, so no matter which theory of explanation is invoked, a metaphysical account of how mathematical objects help to produce physical phenomena will have to give the relative structures exhibited by physical systems and mathematical objects centre stage. In other words, it will have to explain the role of mathematical objects in terms of structure-preserving mappings that hold between them and physical systems. Since a structure-preserving mapping holds between two systems in virtue of their structures, there will always remain a simpler expressionistic alternative which appeals only to the physical structure.

To avoid this pitfall, then, the platonist has no choice but to provide a radically different metaphysical account of the nature of mathematical explanation, one according to which the direction of explanation runs in the same direction as metaphysical explanation. On such an account, mathematics would be playing a metaphysically explanatory role, so rubbing out the mathematical parts would not yield a simpler expressionistic alternative.

We have already encountered this view of the relationship between the mathematical and the physical world—HDP. According to HDP, a ten kilogram mass has its mass in virtue of bearing the *mass in kilograms* relation to the number 10. Similarly, honeycomb has the structure it does in virtue of standing in a relation to hexagons, cicadas' life-cycles have the durations they do by standing in relations to the numbers 13 or 17, and the foragings of marine mammals exemplify the pattern they do by standing in relations to Lévy walks. On this view, mathematical objects clearly play a role in producing physical phenomena that requires them to exist. The relations they stand in to physical objects help determine the causal powers those objects have. On this view, the behaviours of the target systems of optimality explanations emerge because the systems are related in the right way to the relevant mathematical objects.

4.5 Conclusions

Recall again the modified version of Melia's simplicity argument:

SA

SA1. The indispensability argument is successful if only if HDP is more plausible than abstract expressionism.

SA2. Abstract expressionism is at least as plausible as HDP.

SA3. The indispensability argument is not successful.

In this chapter, we have seen that playing a simplifying role is not the only way in which mathematics can play an explanatory role in science. MR+ is an account of the explanatory role of mathematics according to which mathematical objects must exist that doesn't imply HDP. However, we have seen that the platonist cannot appeal to MR+, since there will always be a preferable account available according to which mathematical objects are not required to exist (MR). Hence, the platonist must appeal to HDP and try to show that it is preferable to abstract expressionism to support IA. SA1 is therefore true.

Now consider SA2. How might the platonist argue that HDP is more plausible than abstract expressionism? We saw in §2.3 that one way is to appeal to the ideological simplicity that mathematical language brings and argue that ideologically simpler theory represents a simpler world. Given that HDP stands up to existing objections, this move seems legitimate with simple cases, such as physical magnitudes. Our theory of distance relations seems ontologically simpler if we adopt HDP. However, in the case of optimality explanations, it is very difficult to see that HDP provides any advantages over abstract expressionism. In fact, it is very difficult to make sense of a heavy duty interpretation of such cases. Take *Marine mammals*, for example. It is easy to see the explanatory role mathematics plays here on the expressionistic interpretation: the mathematics characterises the structure of particular foragings, which ensure that one or other of the causal processes that might have led up to marine mammals evolving in this way occurred.

Now consider how the story would go according to HDP. HDP says that the foragings of marine mammals exhibit the pattern they do in virtue of their being related to Lévy walks. It is extremely difficult to make sense of the claim that foragings being related Lévy walks *metaphysically determines* that one or other of the causal processes that could lead up to marine mammals evolving in this way occurred. For a start, this seems to suggest that the relation foragings stand in at present metaphysically determine that certain things occurred in the past. It seems that the platonist must do a lot of work even to argue that HDP provides an account of the explanatory role of mathematics that is merely as good as that provided by abstract expressionism. This provides strong support for SA2, so SA appears to be a sound argument. The indispensability argument fails.

II

The Language of Mathematics

5. Introduction to Part II: The Study of Natural Language

5.1 *Why be descriptive?*

In Part I of this thesis, I motivated a normative view concerning the use of mathematics in science. Abstract expressionism says that mathematics plays a purely expressive role, allowing us to say more things about the physical world than we would otherwise be able to. So, we ought to use mathematical sentences to convey only what they say about the physical world. I used this to show, via a modified version Melia's simplicity argument (SA), that the indispensability argument fails. The role of mathematics in science therefore poses no problem for nominalism.

However, it would be nice if our nominalist philosophy of mathematics could say something about how mathematical sentences are actually put forward. Indeed, what our philosophy says about this will affect its plausibility. If it turns out that scientists typically treat the content of their scientific theories concerning mathematical objects with the same regard as the content concerning only physical objects, then the normative view I just described attributes widespread error to scientists. This arguably counts against the plausibility of the view.

Another reason we would want our nominalist philosophy of mathematics to say something about how mathematical language is used derives from our aim to provide a genuine alternative to platonism. We have seen that the role mathematics plays in science provides no support for platonism; but in the introduction to this thesis, we saw that there are other apparent benefits that come with adopting platonism. One is that it allows for a unified interpretation of language, according to which both mathematical and non-mathematical assertions are aimed at describing a domain of independently existing objects, and are often true of them. Another is that

taking mathematical sentences to describe abstract entities appears to explain why we think many of them could not have been false.

Finally, claiming that mathematical theories are true can help to explain why mathematicians choose to accept some over others. If a nominalist philosophy of mathematics is to be preferred to platonism, it must do a better job of honouring our desire to provide a unified interpretation of language, explaining our intuitions about mathematical sentences, and explaining pure mathematical practice. To do this, it seems we need to ask which analysis of mathematical language is the correct one, and whether it is available to the nominalist. My first port of call is to outline and defend what I think is the best general account of how language should be analysed, showing that the assumptions on which I will later rely fall out of it. The following four sections are dedicated to this (§5.2-§5.5). In §5.6, I outline the structure of the remainder of Part II.

5.2 The traditional view

The philosophy of language I wish to defend falls out of a particular view concerning the proper job description of semantic theory. I take the remit of natural language semantics to be that of providing the means of deriving the meaning of every declarative sentence of the target language. To put it another way, a semantic theory of natural language should aim to account for what a competent speaker knows. In semantic theory, the meaning of a declarative sentence is often given as a specification of the conditions under which the sentence is true—the sentence's *truth-conditions*. Underlying this approach is the principle that to know the meaning of a declarative sentence is to know what the world would have to be like for it to be true.

In what follows, I will slide between talking about the meaning of a sentence as its truth-conditions and talking about the meaning of a sentence as the *proposition it expresses*. There are different ways of thinking about the relationship between propositions and truth-conditions, but for my purposes, it will be enough to assume that propositions are the primary bearers of truth-conditions, while a sentence has truth-conditions by expressing a certain proposition. I will also talk about thoughts and concepts. Forming or entertaining a certain concept is required to grasp the meaning of an expression and forming or entertaining a certain thought is required to grasp the meaning of a sentence. The relevant thought/concept represents the meaning of the sentence/expression, but it is not itself the meaning of the sentence/expression.

The kind of semantic theory we can provide is constrained by the fact that speakers appear to be able to understand and produce tokens of indefinitely many novel sentences, despite their having only finite mental resources. This means that the knowledge of meaning ascribed to speakers by a semantic theory must be finite but able to facilitate the comprehension of an indefinite number of novel sentences. This is achieved by providing an account whereby the meanings of complex expressions of the language are derived from the meanings of the atomic expressions of the language, the *lexicon*, and the various rules governing their combination into complex expressions, the *syntax*. On this view, semantic comprehension is deductive, and is sensitive only to lexical and syntactic information, along with the limited features of context signalled by lexical or syntactic information. Importantly, this implies that appeals to features of context that are not linguistically mandated, what I will call appeals to *rich features of context*, are not needed to uncover semantic meaning. Given the claim that natural language semantics should be able to

account for the meaning of all the sentences of the target language, this approach assumes that all well-formed declarative sentences express propositions and have truth-conditions. It is important to note that, given the nature of semantic meaning according to this approach, the evidence that should inform semantic theorising is primarily syntactic data—data concerning how words can and cannot combine to form grammatical sentences—along with type-level intuitions about word meanings, sentence meanings, and meaning relations between expressions, such as entailment.

Following Emma Borg (2004; 2012), I will call this approach to semantics *minimal semantics* or the *minimal view*. The central tenets of this view can be summarised as follows (i, ii and iii adapted from Borg 2012: 4-5):

- i. Semantic content for well-formed declarative sentences is truth-evaluable content.
- ii. Semantic content for a sentence is fully determined by its syntactic structure and lexical content.
- iii. Recovery of semantic content is possible without access to rich features of context.
- iv. Semantic theorising should be informed only by syntactic data and type-level intuitions about expression meaning and meaning relations.

This familiar view of the job description of semantics has its roots in Frege (see [1892] 1948, for example), and has been subsequently developed in various ways by the likes of Bertrand Russell (1905), Rudolf Carnap (1947), Donald Davidson (1967) and David Kaplan (1977). I should note that some of these authors would not have agreed with one or more of (i)-(iv). For example, Russell's semantic theorising about

referring expressions was informed heavily by epistemic considerations (see 1911 for example). Russell would therefore reject (iv), the claim that only syntactic data and type-level intuitions about meaning are relevant to semantic theorising. Nevertheless, each would I think endorse the spirit of the view I have presented. Accordingly, (i-iv) should be considered the core of the most defensible version of the kind of approach these authors take to semantics. (See Borg 2004: 167-208 for convincing arguments against appealing to epistemic considerations when accounting for the semantics of referring expressions.)

Importantly for my purposes, the minimal view of semantics has been widely presupposed in most of the debates between platonists and nominalists in the philosophy of mathematics. Both sides accept that scientific and mathematical theories contain sentences which are true only if certain mathematical objects exist because certain mathematical expressions purport to refer to mathematical objects. On the one hand, those who do not justify these assumptions rely on our type-level intuitions about the meaning of mathematical expressions, such as the intuition that ‘7’ is a name for a number. On the other hand, those who have sought to justify or reject these assumptions, with regards to particular mathematical expressions, have appealed to syntactic evidence to do so (see Hale and Wright 2001: 31-47, for example; see also Felka 2014). (See §6.1 for more on the semantic assumptions of arguments in the philosophy of mathematics.) More generally, the minimal view was until recently the dominant view of semantics in philosophy.

The minimal view leads to a straightforward distinction between the domain of semantics and the domain of pragmatics. Semantics deals with the meanings of sentences in the way specified above, while pragmatics deals with the meanings of the linguistic acts. Paul Grice endorsed this view in his pioneering work in the

philosophy of language, distinguishing between ‘what is said’ and ‘what is implicated’, where the former is closely tied to the meaning of the sentence uttered, while the latter is tied to what is communicated by a person uttering that sentence in a particular context (see 1989: 22-40). Grice’s views on the matter can be summarised as follows.

Grice’s notion of ‘what is said’:

(a) What is said is very closely linked to the sentence uttered, and varies very little from one context to another (unlike what is conversationally implicated).

(b) The truth value a sentence is determined by what is said by the sentence.

If something is not relevant to the sentence’s truth value, it is not a part of what is said, even if it is a part of the meaning of the sentence uttered.

(Adapted from Saul 2012: 22-23)

What is said, according to Grice, is determined by the features of the sentence uttered that are relevant to the truth-value of the sentence, and varies very little from context to context. Clearly, Grice’s notion of what is said is very closely related, if not identical, with the notion of semantic content on the minimal view of semantics. What is implicated, on the other hand, is determined in part by what is said, but also by the rich features of the context in which the sentence is produced. To give a familiar example:

(1) John has good handwriting.

(1) is true if and only if John has good handwriting. According to Grice, then, (1) *says* that John has good handwriting. Suppose that John has been entered into a handwriting contest. In such a context, what is implicated by an utterance of (1) will be very close to what is said by (1). However, if we change the context appropriately, what is implicated by an utterance of (1) can change drastically. Suppose that John is applying for a job in philosophy, and (1) constitutes the body of the letter of reference sent by John's supervisor. In this context, though what is said remains unchanged from the previously considered context, this use of (1) implicates a great deal more: that John has no positive qualities relevant to the position applied for; or, more simply, that John is a terrible philosopher. Importantly, the appeals to features of context needed to uncover this interpretation are not linguistically mandated: there is nothing in the properties of the sentence (1) that suggests we should pay attention to the purpose for which (1) is written or spoken. (The handwriting example is based on one found in Grice 1989: 33).

How is pragmatics pursued in light of the present view of the semantics/pragmatics divide? Unlike semantics, pragmatics appeals to rich features of context to explain how speakers interpret particular utterances. Since these rich features of context come in a variety of forms (background beliefs, perceptual information, knowledge of the conventions governing the particular practice one is engaging in, etc.), and are not indicated by the properties of the sentence used, the uncovering of pragmatic meaning is an abductive process, whereby some kind of inference to best explanation is made by the listener to work out why the speaker would utter *that particular sentence in this particular context*.

Grice's (1989: 22-40) theory of conversational implicature was the first sustained effort to account for the principles that govern pragmatic interpretation. He

suggested that there are certain maxims that participants in conversation are expected to uphold, and that pragmatic meaning is uncovered by interpreting the speaker in such a way such that their utterance upholds these maxims. One such maxim is Quantity, which says that an utterance should be as informative as is appropriate, and no more. Another is Relevance, which says that an utterance should be relevant to the topic of conversation. So, the semantic meaning of (1) used in the context of a reference letter for a philosophy job to which John is applying drastically violates the maxims of Quantity and Relevance. So, we wonder why the letter writer would be reluctant to provide more information, and moreover, information relevant to the post. The obvious answer is that the relevant information does not reflect well on the applicant, and that the writer is unwilling to write a negative reference letter. This involves appealing to background knowledge of the conventions governing the writing of reference letters. In light of this, we search for an interpretation that does meet Quantity and Relevance. That the applicant has no positive qualities relevant to the position applied for, and that the applicant is a terrible philosopher, both meet the constraints imposed by Quantity and Relevance, so both count as adequate interpretations of the use of (1) in this context.

In the context of a handwriting competition, the semantic content (what (1) says) does not seem to flout any maxims, so the semantic content is the pragmatic content in this context. This is an example of how pragmatics should be carried out in light of the acceptance of the minimal view of semantics and the traditional view of the semantics/pragmatics divide. (We shall see in §8.2 that Grice's theory, though illuminating, has been replaced by a more explanatorily powerful theory, though a theory still very much Gricean in spirit.) I will call this approach to pragmatics the *post-semantic view*, since it assumes that semantic interpretation is prior to pragmatic

interpretation: the semantic content of a sentence *informs* or *constrains* the possible pragmatic interpretations.

I will call the combination of the minimal view of semantics and the post-semantic view of pragmatics the *traditional view*. The traditional view is a highly intuitive one. In recent years, however, this approach to studying natural language has been subject to savage and multi-faceted criticism from within the philosophy of language. There are many subtle ways in which theorists have disagreed with the traditional view, and the literature has become saturated with alternatives that differ in equally subtle ways. Unfortunately, there is not space here to assess each alternative. In the remainder of this section, I will give a brief outline of the main alternatives (see Borg 2012: 1-47 for an excellent survey).

One might wish to endorse the claims that semantic content is truth-evaluable, that it is wholly determined by lexical and syntactic features, and that semantic content can be reached without appeal to rich features of context (i-iii above), while rejecting the claim that semantics should only be informed by syntactic data and type-level meaning intuitions (iv above). In particular, one might maintain that it is part of the job of semantic theory to capture, as much as possible, intuitions about what is communicated by particular utterances, and the truth-values of particular utterances. Accounting for this kind of data is impossible with the resources available to the minimal semantic theorist, and this is supposed to be a serious failing of the view. To account for the data while retaining (i-iii) requires theorists to claim that there are many more context sensitive expressions operative in natural language than one might initially think. On this view, it is not just indexical expressions such as ‘I’ or ‘that’ which mandate appeals to context; other expressions contain hidden variables that require saturation from context (See Stanley 2002,

2005(a); Rothschild and Segal 2009, for example). The ability for such views to account for our intuitions about the truth-values of utterances and their communicated content is seen as reason enough for positing context sensitivity where there does not at first appear to be any.

Another means of departing from the minimal view is to go further than the view just described by also denying that semantic content is fully determined by lexical and syntactic features (ii). On this view, only allowing linguistically mandated appeals to context under-determines semantic content. To account for the propositions that sentences literally express on particular occasions of utterance, one must appeal to all kinds of rich features of context (see Carston 2002 and Recanati 2004, 2010). Again, the evidence for views of this kind (and against the traditional view) is their success in accounting for intuitions about what is communicated by particular utterances in particular contexts.

More radical still, some have argued that propositions only have a truth-value relative to parameters that include rich features of context, such as the mental states of agents (see MacFarlane 2007, for example). The evidence in favour of such relativist views (and against the traditional view) derives from their success in explaining intuitions about faultless disagreement. Perhaps the most radical departure from minimal semantics is the view that there just is no such thing as the meaning of an expression, independent from a particular use of it (see Travis 1989).

In light of the evidence presented by these theorists, the traditional view has fallen into disrepute. However, in the following three sections I will show that the traditional view is perfectly compatible with this evidence (§5.3-§5.4), and, moreover, the traditional view enjoys advantages that are not available to any view that takes semantic meaning to be partly determined by rich features of context

(§5.5). We shall see that the minimal view is to be preferred to its rivals, whatever form these rivals take.

5.3 Objections to the traditional view (I): communication and completeness

As described above, minimal semantics allows room for some context sensitivity in the semantic analysis of natural language sentences. However, given (i-iv), the input from context is severely constrained. First, minimal semantics only appeals to features of context that are signalled by the lexicon or the syntax of a sentence. Second, evidence for linguistically mandated context sensitivity must come from type-level intuitions about expression meaning or syntactic data. Third, uncovering semantic content never requires the listener to inductively reason about rich features of context, such as the intentions of speakers. The result of these commitments is a semantic theory that only attributes context sensitivity to a limited range of so-called *indexicals*, including at least those expressions listed at the beginning of Kaplan's seminal paper *Demonstratives*:

The group of words for which I propose a semantical theory includes the pronouns, 'I', 'my', 'you', 'he', 'his', 'she', 'it', the demonstrative pronouns 'that', 'this', the adverbs 'here', 'now', 'tomorrow', 'yesterday', the adjectives 'actual', 'present', and others. (Kaplan 1977: 489)

Again, the kind of data that motivates giving these expressions a contextually sensitive semantic analysis are, first, type-level intuitions about expression meaning, and second, syntactic evidence. Clearly, all of the expressions listed above have associated type-level intuitions that support their context sensitive analysis. No one

would deny that spelling out the meaning of the word ‘I’ would require taking into account the contexts in which the expression is used. As for the syntactic evidence appealed to, all of the above expressions have syntactic properties in common. For example, they all take wide-scope when embedded in propositional attitude reports:

(2a) John said that I am a swan.

(2b) Mary believes that John isn’t here.

Whenever an indexical expression is embedded in an attitude report or speech report, it scopes out and gets its meaning from features of the speaker’s context rather than the context of the person whose attitude or speech act is being reported. In (2a), ‘I’ refers to whoever utters (2a) rather than referring to John. In (2b) ‘here’ refers to the location of the utterer of (2a) rather than Mary’s location. In the absence of such syntactic evidence and type-level meaning intuitions, minimal semantics assigns a context-invariant meaning to expressions.

However, as noted in the previous section, there is evidence that suggests that an adequate theory of meaning should either posit widespread linguistically mandated context sensitivity, or make appeals to context that are not linguistically mandated. There are, broadly speaking, three kinds of evidence. The first kind concerns intuitions about what is communicated by specific utterances of sentences. The claim here is that minimal semantics systematically makes the wrong predictions about what speakers mean when they utter sentences. The second kind of evidence concerns intuitions about the semantic incompleteness of certain well-formed natural language sentences. The claim here is that there are certain well-formed sentences which intuitively do not express any truth-evaluable content, and

yet semantic minimalism is committed to saying that they do. The third and final kind of evidence concerns the analysis of indexicals. Most indexicals seem to require us to appeal to speaker intentions to fix their referent, and it appears that there is nothing in the lexicon or the syntax of a sentence that can provide access to speaker intentions. The following are examples typical of the first kind of evidence:

(3a) You won't die. [You won't die *in the near future from that injury*.]

(3b) Everyone is here. [Everyone *who is expected to be here* is here.]

(3c) Simon went to the top of the Empire State Building and jumped. [Simon went to the top of the Empire State Building and jumped *off the building*.]

(3d) I've eaten. [I've eaten *very recently*.]

To the right of each of (3a-d) I've written a sentence in square brackets which better captures what speakers typically communicate when they utter these sentences. For all the minimal semantic content tells us, (3a) expresses the proposition that the referent of 'You' is immortal. Imagine (3a) is uttered by the speaker after observing that her interlocutor has sustained a minor injury. The proposition communicated in such a context is likely to be one that is more straightforwardly expressed by the sentence 'You won't die in the near future from that injury'. A semantic theory that assigns the more anaemic content to (3a) therefore fails to account for what speakers mean when they utter (3a). The same goes from (3b-d). Minimal semantics, the objection goes, systematically makes the wrong predictions about speaker meaning.

I hope that the way in which I introduced minimal semantics makes clear that this objection is sorely misguided. At no point in §5.2 did I mention that semantic theory should aim to account for what speakers typically communicate when they

utter particular sentences. This was no careless omission; accounting for speaker meaning goes well beyond what semantic theory should hope to be able to do. It is the job of pragmatic theory to take the deliverances of semantics and appeal to the wider context to account for what speakers communicate.

Moreover, the objection can be turned on its head and used to criticise accounts that assign pragmatically enriched propositions as the semantic content of sentences. If it is not the job of semantics to account for the literal content of sentences, irrespective of whether people mean to communicate this content or not, then whose job is it? For it seems that the content is there and is recoverable by speakers. To see this, recognise that to an utterance of each of (3a-d), there is the possibility of what I will call a *facetious response*, which annoyingly picks up on the literal content of the sentence rather than the intended content of the utterance. So, in response to (3a), someone might answer ‘I will one day!’; and in response to (3b), someone might answer ‘Not *everyone* is here; the Dali Llama isn’t!’; and someone might respond to (3c) by saying, in rather bad taste, ‘Did he? How high did he jump?’; and, finally, someone might respond to (3d) by saying ‘Yes, but have you eaten this afternoon?’. Someone responding in this way is not being a very cooperative interlocutor, but neither are they plucking their interpretation of the utterances out of thin air. Part of what makes these responses so annoying is that they are responding to one meaningful feature of the communicative act (the sentence used), and feigning ignorance of the subtle aspects of communication (information about why the speaker might utter that sentence in this context).

The systematic availability of a facetious response in these contexts needs explaining, but theories which assign the enriched propositions as the semantic content of sentences such as (2a-d) will have a hard time doing so. Minimal

semantics, on the other hand, provides a simple explanation. The literal content of sentences such as (3a-d) is processed by listeners on the way to landing on the intended meaning of the utterance. As for the fact that listeners are able to, and generally do, uncover the enriched content of sentences such as (3a-d), this job is left for pragmatic theory, as Grice recommends.

The following are typical examples of the second kind of evidence, involving intuitions about semantic completeness. Recall that the key claim here is that there are certain well-formed sentences which intuitively do not express any truth-evaluable content, and yet semantic minimalism is committed to saying that they do. Consider:

(4a) Jane is late. [Jane is late *for school? For work? For her plane?*]

(4b) Faheem is ready. [Faheem is ready *for work? For the his close up?*]

(4c) Chloé has had enough. [Chloé has had enough *ice cream? Of the noise next door?*]

(4d) It is raining. [It is raining *in Manchester? In London? In New Deli?*]

Without the context providing answers for the questions in the square brackets on the right, and so pragmatically enriching or completing the content of the sentence, we would find it extremely difficult to evaluate (4a-d) or know what anyone uttering them was trying to convey. So, the objection goes, sentences such as (4a-d) do not express complete propositions until their meaning has been enriched or completed by context.

In response to the first part of this objection, the claim that we would find it extremely difficult to evaluate (4a-d), the semantic minimalist has the following to

say (see Cappelen and Lepore 2005: 116 for a response in this vein). All that is required for a sentence to have truth conditions is that there be conditions under which it is true; not that people should find it easy to find out whether or not it is true. Suppose Jane is late for work. In this circumstance, (4a) is true. In fact, (4a) is true in any circumstance in which Jane is late for something. Similarly, (4b) is true in any circumstance in which Faheem is ready for something and (4c) is true in any circumstance in which Chloé has had enough of something. It seems that (4b) and (4c) will be true in most if not all circumstances in which Faheem and Chloé exist. So, (4a-c) express propositions with truth-conditions just like any other well-formed sentence.

The second part of the objection was to appeal to the fact that, without a suitable context to enrich the content, it would be hard if not impossible to work out what a speaker of (4a-c) is trying to convey. The response to this mirrors our response to the objections concerning intuitions about what is communicated. The semantic minimalist can wholeheartedly agree that it would indeed be difficult to work out what was being conveyed by utterances of (4a-c), if the context were not suitably rich. Imagine receiving an anonymous and rather ominous letter through the letter box that reads ‘You are ready’. Supposing you correctly grasp that ‘You’ is supposed to refer to you, you would still be at a loss as to what the writer had intended to communicate. But you could still understand the sentence, and know that it is most likely true, since there are probably countless things you are ready for at any given moment of time. In this circumstance, all the semantic minimalist is forced to admit is that the attempt to pragmatically communicate something using the sentence ‘You are ready’ failed, though the semantic content was still there to be uncovered.

Once again, this objection can be turned on its head. It seems that there is an interpretation of (4a-d) available for facetious speakers to exploit. Suppose Faheem, Jane and Chloé are about to go on a camping trip. Chloé asks Jane whether Faheem is ready, meaning to ask whether Faheem is ready for the camping trip. Jane replies with (4b). Not trusting Jane, who is a philosopher with a bad sense of humour, Chloé decides to see for herself. She returns to Jane and says ‘Faheem is not ready; he’s still in bed!’ to which Jane replies ‘I said he was ready; I didn’t say what he was ready for!’ Again, giving this facetious response is not being a very cooperative conversational partner; but neither is it plucking an interpretation out of thin air. The facetious interpretation just is the semantic content of the sentence used, available for all the interlocutors to uncover, and that is partly what is so annoying about responses that exploit the facetious interpretation: they are, in one sense, legitimate.

In this section, I have shown that intuitions about the context sensitivity of what speakers typically communicate by uttering sentences are perfectly compatible with minimal semantics. Moreover, examples such (3a-4d) reveal an explanatory deficit in the rival theories to minimal semantics: they appear unable to comfortably explain the systematic availability of facetious responses.

5.4 Objections to the traditional view (II): indexicals

I now move on to the second way in which minimalism has come under attack with respect to context sensitivity. The referents of some indexical expressions seem to be determined completely independently of any rich features of context. ‘I’, for example, picks out the speaker of the utterance, which is an immediately available, objectively specifiable feature of context. Uncovering the referent of this expression does not appear to require any abductive reasoning about the mental states of the

speaker. Similarly, we can uncover the referent of ‘Tomorrow’, ‘Today’, ‘Yesterday’, etc., without appealing to such rich features of context.

However, other indexical expressions are more problematic. Take the indexical/demonstrative ‘here’. The first and most obvious thing to say is that this expression refers to the location of the speaker; but it is when we start asking where exactly the location of the speaker is that we start running into difficulty. Should we take ‘here’ to refer to the building occupied by the speaker? Should we take it to refer to the city occupied by the speaker? The country? The planet? It seems as though the correct answer is going to depend very much on the particular context of utterance. More specifically, it looks as though it is going to depend on what the speaker intends to refer to.

In an attempt to purge appeals to speaker intentions from our semantics, one could argue that objective features of context, such as the observable behaviour of speakers, is enough to determine the reference of utterances of indexicals. (There are different ways of spelling this proposal out; see Borg 2012: 115-134 for discussion.) For example, there are various conventions a speaker can exploit to communicate their referential intentions. Suppose a speaker were to point at a particular building and say ‘I used to work here’. In this circumstance, it seems the pointing is instrumental in fixing the referent of ‘here’. However, solutions of this kind raise many difficulties. One is related to Quinean sceptical worries about the indeterminacy of reference (see Quine 1960): it appears that the pointing itself is open to many different interpretations. Working out that the speaker intended to point at the building (rather than at a particular time-slice of the building, or at a particular brick, etc.) seems to be a paradigm case of reasoning about an agent’s intuitions (cf. Borg 2012: 121-122, 132-134). Another worry is that it appears there

are many different conventions people can take advantage of in trying to communicate their referential intentions. Working out which convention is operative on a given occasion seems again to involve reasoning about the intentions of the speaker (cf. Predelli 2002: 313-314). It seems inescapable that speaker intentions determine the reference of some indexicals. (It is notable that not all authors see a problem with incorporating rich features of context into formal semantics. See Cappelen and Lepore 2005 and Stokke 2010, for example; however, we shall see in §5.4 that one of the key advantages of minimal semantics relies on its being computationally tractable, which requires that semantic content be graspable without abductive reasoning about speaker intentions.)

Despite appearances, the fact that the referents of many indexicals are determined by speaker intentions is not devastating to the minimal view of semantics. To adequately explain why, I need to make two important distinctions and say something about what it means to grasp the content of an expression. (The below account of indexical reference draws heavily on Borg 2004: 186-208, 2012: 134-140. Borg notes similarities between her account and that found in Perry 2001.)

First, following Kaplan (1977), we need to distinguish between two levels of meaning. The first can be thought of as the part of the world that we take the expression to pick out in a particular context of use—its *content*. The second can be thought of as a linguistic rule or a function assigned to the expression which fixes its content for each possible context of use—its *character*. Knowing the meaning of a particular expression requires a competent speaker to know its character. To use the common fiction that speakers' minds are like dictionaries, a competent speaker's *lexical entry* for each expression contains information describing the character of the expression. Importantly, character does not feature as a part of propositional content,

though it fixes content and is the path by which a listener grasps content. Once the content of a sentence has been grasped by a listener, the character drops out, and the speaker is left with a direct representation of the truth-conditional significance of the expression.

This brings me to the second distinction. On the one hand, the reference of an expression can be *fixed* by a linguistic description. On the other hand, the reference of an expression can be *identified* by some other means (perceptually, for example). To see that these two come apart, consider the description ‘The first person to have made fire’. We are not in a position to identify non-linguistically to whom this expression refers. We cannot point them out, for example. The only epistemic access we have to the referent of this expression is via the description itself, or via reflexive descriptions suggested by it, such as ‘the referent of “The first person to have made fire”’. We might call this a *minimal* grasp of the content of the expression, for it is severely limited; but this limitation does not stop us from speaking about this person, or from saying true or false things about them. For example, ‘The first person to make fire lived before recorded history began’ is true, and we can grasp the truth-conditions of this sentence even though we have only a minimal grasp of the content of the expression in subject position.

Finally, it will be helpful to say a little more about what it is to grasp the content of an expression, especially a directly referring expression. Following Borg (2004: 168-174, 2012: 136-140), we can understand what grasping the content of a particular expression involves in terms of mental representation. To grasp the content of a directly referring expression, such as the name ‘Christopher Lee’, one must form a mental representation the content of which is completely exhausted by the reference of the expression, namely Christopher Lee. The character of ‘Christopher

Lee' can be thought of as a function that takes us from possible contexts of use to an individual. Since it is a directly referring expression, its character determines that the name refers to the same individual across all possible contexts of use. Hence, we interpret sentences such as 'If Christopher Lee really were Dracula, he would have lived forever' as tracking the same individual across different possible situations. Similarly, the mental representation we form when we grasp the content of 'Christopher Lee' will have associated information corresponding to the character of the expression, which fixes the content of the representation across imagined possible contexts. For simplicity, let us also call this information *character*. The character of a mental representation determines how we reason with thoughts that involve the representation. So, the thought *If Christopher Lee really were Dracula he would have lived forever* involves the concept *Christopher Lee* tracking the same individual across to a different imagined possible scenario. Call the kind of mental representation formed in grasping a directly referring expression a *singular concept*. Importantly, just as with the character of a directly referring expression, the character of a singular concept does not feature as part of its content. Though the character of the concept fixes its content and allows an agent to think appropriately with it, the content of a singular concept is completely exhausted by the object it represents.

Contrast this with a *general concept*: the kind of concept an agent must entertain to grasp the content of a general or descriptive expression. The concept an agent must entertain in order to grasp the expression 'The first person to have made fire' has a character such that the content of the concept can change across imagined possible situations. So, we can correctly reason with the thought *If fire were invented in 2001, the first person to have made fire would be famous*. Importantly, the concept *the first person to have made fire* does not always track the same individual across

imagined possible scenarios. This story about what it is to grasp the content of an expression provides a plausible criterion for distinguishing between directly referential expressions and other kinds of expression. Different kinds of expressions are distinguished by the kinds of concepts an agent must entertain to grasp their content, and different kinds of concepts are distinguished in terms of the inferential role they play in our thinking (cf. Borg 2004: 186-188).

Drawing the threads together, consider the indexical ‘that’. The referent of this expression in a given context of use is determined by the intentions of the speaker. Since it is the job of *character* to fix the content of an expression across possible contexts of use, the appeal to speaker intentions will have to come in at the level of character. So, we can understand the character of ‘that’ as a function that takes us from possible contexts of use to whichever object the speaker intends to refer to. Now consider an utterance of the sentence:

(5) That is red.

Assuming that demonstratives are directly referential expressions, for an agent to grasp the semantic content of this sentence she must form a singular concept whose content is completely exhausted by whatever the speaker intends to refer to. Drawing on the distinction between fixing reference and identifying reference, it seems that so long as an agent entertains a singular concept whose character *fixes* the right content, she can grasp the semantic content of the utterance whether or not she is able to *identify* the referent non-linguistically. This is possible, since the relevant singular concept will be available to the agent via knowledge the character of the expression. This character can be expressed as a reflexive description of the form ‘If the speaker

intends to refer with the utterance of “that” to α , then “that” refers to α . The singular concept grasped via this knowledge will have a corresponding character fixing the content of the concept as the speaker’s intended referent even in the absence of the ability to otherwise identify this referent. The ability to grasp the semantic content of an utterance of (5), then, runs via a minimal knowledge of its truth-conditions, which we might express as follows:

(5') If the speaker of ‘That is red’ intends to refer with the utterance of ‘that’ to x and nothing else, then ‘That is red’ is true iff x is red. (Adapted from Borg 2012: 136; see also Higginbotham 1994: 92-93)

In a particular context of utterance for (5), an agent who knows (5') will be able to form a mental representation whose content will be the truth-conditions of (5) relative to that context; and doing so will not require the agent to be able to non-linguistically identify x . Knowing that x is whatever the speaker intends to refer to in that context is enough. Where α is the singular concept triggered by the particular utterance of ‘that’ in (5), whose character fixes its content as the object the speaker intends to refer to, and RED is the general concept triggered by the utterance of ‘red’, then we might express the thought formed by the addressee in grasping the minimal semantic content of (5) as follows:

(5'') [α is RED]

It follows that uncovering the semantic content of this sentence does not require an agent to reason about the intentions of the speaker. The intentions of the speaker

determine the referent of ‘that’, but grasping the semantic content of ‘that’ does not require the listener to uncover those intentions. Moreover, the context-sensitivity exhibited by ‘that’ is linguistically mandated, since its reference across contexts is fixed by the character of the expression, and the character of an expression is part of its lexical/syntactic profile. Though the intentions of the speaker are part of the explanatory story, they feature in such a way that is compatible with the central tenets of semantic minimalism.

At this point, the opponent of semantic minimalism might object: what is the point in semantic meaning if it is so minimal? For the most part, understanding an utterance of (5) in a particular context will only be useful once we are able to identify non-linguistically which object is being talked about. Why posit a level of content that is so systematically useless? (Carston 2002: 177-181, 2008: 328 and Stanley 2005(b): 3 object in this spirit, claiming that minimal semantic content threatens to be an ‘idle wheel’ in the explanation of linguistic practice.)

The answer is that minimal semantic content yields considerable explanatory benefits. There are many examples where it is desirable to claim that agents understands a given utterance, even if they are not in a position to non-linguistically identify the objects spoken of. Imagine someone saying ‘There’s Armadillo over there!’ while looking through a window out of which her interlocutor cannot see. It seems counter-intuitive to suggest that the interlocutor does not grasp the meaning of this utterance until she has walked over to the window and perceptually identified the intended referent of ‘over there’. Examples where communicative acts go wrong are perhaps more convincing here. Suppose Fred utters (5), intending to describe *x*. However, given features of context unknown to Fred, his audience assume him to be talking about *y*. On any account that requires an appeal to rich features of context to

fix the semantic content of ‘that’, it looks like we are forced to accept one of two unattractive possibilities. The first is that Fred succeeds in referring to x , which means that his audience do not understand the sentence that he utters. The second is that Fred fails to refer to x and, without meaning to, describes y instead. Both possibilities seem implausible to me. It is far more plausible to say that Fred’s audience understand the sentence he utters, though they go on to identify the referent of ‘that’ with the wrong object in the environment (though, of course, the blame may ultimately lie with Fred for not being suitably aware of the context). Semantic minimalism allows us to say just this.

Another explanatory benefit comes from our ability to draw correct inferences from uttered sentences of which we only have a minimal grasp. For example, suppose someone utters truly ‘Christopher Lee was an incredibly versatile actor’ to an audience who are unaware of whom Christopher Lee was. Now suppose that the audience is a promoter who correctly infers that Christopher Lee would have made a superb Prospero in an upcoming performance of *The Tempest*. In making this inference, the member of the audience has tracked the individual, Christopher Lee, into an imagined possible situation in which Christopher Lee lived long enough to star in an upcoming performance of *The Tempest*. Explaining this inference would be awkward on any view according to which the promoter didn’t grasp the semantic content of the sentence uttered. In contrast, minimal semantics explains this easily by positing a level of content that an agent can grasp without being able to identify the objects referred to in any way other than via the reflexive descriptive content (the character) of the expressions uttered.

Before moving on to less defensive motivations for minimal semantics, and the traditional view of the semantics/pragmatics divide which falls out of it, it is

worth considering what opponents of minimal semantics have to posit in place of minimal propositional content. For they have to recognise that the properties of the sentence used in some way constrains or informs the content expressed by a particular utterance. After all, it is no accident that someone wanting to communicate the proposition that Chubb is a dog in a suitable context uses the words ‘He is a dog’; but opponents of minimal semantics would claim that the addressee does not grasp any propositional content until rich features of context are taken into account. So, in place of minimal propositional content, many claim that the purely linguistic content of a sentence comprises a level of content that is less than propositional. Unfortunately, what theorists tend to say about this level of content is somewhat metaphorical. Some call it a ‘fragment of a proposition’ (see for example Carston 2008: 326); others use the term ‘propositional radical’ (see, for example, Bach 1994: 269). There are two main points I wish to make here. First, the opponents of minimalism often like to make out that minimalists make a further and unjustified move in claiming that the purely linguistic content of a sentence is propositional (again, see Carston 2002: 177-181, 2008: 328 and Stanley 2005(b): 3). Yet it seems to me that both sides accept the existence of propositional content at some stage in their account of communicative behaviour; it is the opponents of the minimalist view that posit a further kind of content. It is therefore the opponents of the minimalist view that owe something. For one thing, they owe an account of what exactly a propositional radical is. The idea seems to be that propositional radicals are meanings that are not yet truth-apt, and, again to speak metaphorically, act as a guide to our pragmatic search for the communicatively intended proposition. However, we have seen from the examples above that we can often describe the conditions under which a sentence is true and draw valid inferences from it without being able to

identify non-linguistically the objects to which the speaker intends to refer. It is difficult to see how grasping meaning that is not truth-apt can allow for this; moreover, given that purely linguistic meaning does allow for this, the opponent of minimal semantics owes us an account of how exactly propositional radicals differ from propositions.

5.5 In support of the traditional view

We have already considered some reasons for favouring of the traditional view of the semantics/pragmatics divide. Recall that a facetious response is one that annoyingly picks up on the literal content of the sentence uttered, rather than what the speaker was trying to communicate in uttering the sentence. In §5.3, I suggested that the systematic availability of a facetious response wherever you have the speaker using a sentence whose linguistic meaning under-determines what is communicated is best explained by the traditional view. In §5.4, I suggested that our ability to understand and draw inferences from a sentence despite not being able to non-linguistically identify the objects that the speaker intends to refer to in uttering the sentence is best explained by the traditional view. In this section, I will provide further reasons for preferring the traditional view to any of its rivals. First, I will suggest that we need the traditional view to make sense of the intuitive distinction between lying and misleading and our practice of retreating to what is said by the sentence used. Second, I will argue that the minimal view of semantics is compatible with evidence that suggests that linguistic comprehension is highly modular, while its rivals appear not to be. Finally, I will argue that there are general theoretical reasons for preferring the minimal view of semantics.

We often want to distinguish between what was said by the sentence we have used, and what was communicated by our uttering the sentence. It is most tempting to do this when we are being held accountable to something we have communicated that is offensive or otherwise incorrect. For example, in response to the question of whether Bill Clinton had an affair with Monica Lewinsky, he famously replied ‘There is no improper relationship’ (note the present tense). If we are to interpret this utterance as addressing the question, it seems we need to uncover content that goes beyond the linguistic content of the sentence uttered. Accordingly, the natural interpretation has Clinton communicating that there never was any improper relationship. However, now that the relevant facts have come to light, Clinton has a defence to the accusation that he lied to the public. He can claim that what he *said* was that there *is* no improper relationship. He was being misleading by uttering a sentence that does not adequately address the question posed, but he did not lie. (See Saul 2012: 21-68 for discussion of this example, and many others, in the context of the lying/misleading distinction and the notion of what is said.) It seems that, in cases such as these, the distinction between what is said by the sentence we have uttered (minimal semantic content) and what is communicated or implied by our uttering it is a vital part of our communicative life (cf. Camp 2006: 207-208). By positing a purely linguistic level of semantic content, the traditional view of the semantics/pragmatics divide makes perfect sense of the lying/misleading distinction and our practice of retreating to what is said by the sentence we have uttered. In contrast, rivals to the traditional view of the semantics/pragmatics divide will have a hard time accounting for this practice, since they deny the existence of the minimal semantic content appealed to.

I now turn to evidence in favour specifically of the minimal view of semantics from which the traditional view falls out. There is evidence that suggests the faculty that is responsible for semantic interpretation, the *semantic faculty*, is modular. The minimal view is compatible with this evidence, while its rivals appear not to be. A modular faculty is one that has all of the following features (from Borg 2004: 86-108, which draws on Fodor 1983): first, a modular faculty is domain specific, which means it is only activated by a very specific kind of information; second, a modular faculty is informationally encapsulated, which means that information from outside of the range of information on which it operates does not affect its outcome; third, a modular faculty is mandatory, which means that whether or not it operates is out of our control; fourth, there is limited conscious access to the representations that a modular faculty operates over; fifth, a modular faculty is fast; and finally, a modular faculty is associated with a fixed neural structure, and exhibits telling patterns of failure and acquisition.

Visual perception is a modular faculty. Accordingly, it exhibits all six qualities mentioned above. It is domain specific in that it operates on visual information alone. For instance, sound does not typically trigger our faculty of visual perception. It is informationally encapsulated in that it only operates on visual information provided by our environment, and its output is unaffected by other information we may possess. For example, even if we know that the two lines in a token of the Müller-Lyer illusion have the same length, we cannot help but perceive them as having different lengths. It is mandatory, in that we cannot help but perceive things that are before our eyes. We have limited conscious access to the information that our visual perception operates over. For example, suppose someone perceives that there is a person in front of them without attending to the colour of the person's

hair. Once they no longer perceive the person in front of them, the information about the colour of the hair is lost to them, consciously, though the visual perception faculty will have picked up the information. The visual perception faculty is fast. There is very little delay between our receiving visual information and our producing a perceptual experience. Finally, the visual perception faculty is associated with a fixed neural structure, namely the visual cortex, which can fail while other parts of the brain function properly, and can function despite the failure of other parts of the brain. In virtue of its exhibiting all of these qualities, the visual perception faculty is considered a paradigm case of a modular faculty.

Our semantic faculty also exhibits all six qualities associated with modular faculties. It is only triggered by a very specific kind of information. For example, we do not typically hear the sound of rain as semantically significant. It is informationally encapsulated in that our beliefs and expectations do not affect our judgements about what sentences mean. For instance, suppose that someone utters a sentence that is completely irrelevant in the conversational context. Our expectation that the speaker must have meant to communicate something relevant does not affect our judgement of what the *sentence* uttered means, though it may affect our judgement of what the speaker means by uttering it. So, our beliefs and expectations do not affect the output of our semantic faculty. The operations of the semantic faculty are mandatory in that we cannot help but interpret meaningful phonemes and inscriptions. We have limited conscious access to the representations that the semantic faculty operates over. For instance, once we have interpreted the meaning of a sentence uttered, the information concerning the exact wording or exact syntactic profile of the sentence is often lost to us. We only retain the content of the sentence uttered, or perhaps the content of the utterance, not the specific

representations in virtue of which our semantic faculty interpreted the sentence. Semantic interpretation is also fast. There is also very little delay between the presentation of a meaningful expression to the semantic faculty and its production of a semantic interpretation. Finally, there is a fixed neural structure associated with semantic interpretation which can fail or continue working properly independently of the status of other neurological processes. There is evidence that the semantic faculty can continue working while the agent is unable to accurately interpret communicative acts. People with certain kinds of autism, for example, have great difficulty grasping the communicative intentions of speakers, though their ability to uncover the semantic significance of expressions is unaffected, and often above average. The fact that the semantic faculty exhibits *all* of the qualities in virtue of which the visual perception faculty is considered a paradigm highly modular faculty suggests that we should assume that the semantic faculty is also highly modular.

The reason the minimal view of semantics fits so nicely with the view that the semantic faculty is modular is that it assumes semantic processing to be a deductive affair, sensitive to a very limited and clearly defined domain of information. On the other hand, if we assume that working out the intentions of the speaker is necessary for semantic interpretation, this would be hard to reconcile with the modular nature of the semantic faculty. This is because there is no limit to the information that may be relevant to the kind of abductive reasoning necessary for working out the communicative intentions of a speaker. It seems implausible to suggest that this kind of reasoning could play a part in a process that exhibits the speed and efficiency associated with highly modular faculties.

The traditional view of the semantics/pragmatics divide also has many theoretical virtues. One is that it makes a stark distinction between the domains of

semantic inquiry and pragmatic inquiry: the former accounts for the meanings of sentences in terms of syntax, lexical meaning and lexically mandated features of context; the latter accounts for the meanings of communicative acts in terms of the deliverances of semantic theory and rich features of context. If we find ourselves appealing to features of context that are not linguistically mandated, we are no longer doing semantics. To see the benefits of this, consider an utterance of ‘You’ll live’, said to a child who is wailing over their grazed knee. This utterance has many possible interpretations. One is <You will not die>; another is <You will not die from this grazed knee>; yet another interpretation considers the utterance as a request that the child stop wailing. Theorists have argued incessantly about where to draw the line in such cases between the semantic interpretation and the pragmatic interpretation. If we allow the second interpretation, <You will not die from this grazed knee>, to be the semantic interpretation, this means that our semantics will have to be sensitive to non-linguistically mandated features of context—in this case, the contextually salient injury. However, once we allow such information to affect semantic interpretation, it becomes very difficult to decide where semantics ends and pragmatics begins. On the minimal approach to semantics, only the first interpretation may rightly be called semantic, since it makes no appeal to anything in the context that is not linguistically mandated. Such a clear demarcation keeps in sharp relief the respective aims of each discipline, and thereby makes pursuing them easier.

5.6 The (less easy) road ahead

Having laid the foundations in the philosophy of language, the remainder Part II is dedicated to motivating the descriptive proposal that speakers typically use

mathematical sentences to communicate only content concerning the non-mathematical world.

According to the traditional view of the semantics pragmatics divide, semantic meaning constrains pragmatic meaning. This means that, in working out what content is under discussion for a particular area of discourse, the first port of call is to provide a semantic theory for the relevant sentences. To this end, in Chapter 6 I survey a wealth of linguistic evidence and formulate a semantic theory of applied arithmetical language (physical magnitude ascriptions and cardinality ascriptions) that explains it (§6.2-§6.7). Surprisingly, applied arithmetical sentences do not contain any expressions that serve to stand for numbers. Such sentences therefore present no challenge to nominalism and the descriptive proposal about mathematical language I intend to defend. Pure arithmetical sentences (and pure mathematical sentences more generally) do contain expressions that serve to stand for mathematical objects, however (§6.8). The challenge facing me is therefore to show how pure mathematical sentences are used to communicate content concerning only the non-mathematical world.

In Chapter 7, I present Yablo's two attempts of meeting this challenge. The first, *figuralism*, involves the claim that mathematical discourse is metaphorical; the second, *presuppositionalism*, involves the claim that the mathematical content of a mathematical sentence is typically presupposed by utterances of the sentence. I show that both fail (§7.2-§7.5). However, Yablo's philosophy is extremely illuminating and useful for my purposes in three key respects: (i) some of the evidence Yablo presents in favour of figuralism suggests that pure mathematical language is non-literal, just in the sense that typical utterances are not aimed at communicating the semantic content of the sentences used; (ii) Yablo provides a strong account of the

content typically communicated by uses of pure mathematical language that fares better than literal interpretations of mathematical discourse with respect to our intuitions about mathematical language; (iii) the evidence Yablo uses to motivate presuppositionalism presents a means of independently motivating a pragmatic theory of mathematical discourse.

In Chapter 8, I develop the beginnings of a pragmatic theory of pure mathematical language, used in both pure and applied contexts (§8.2-§8.5). The theory is based on the plausible assumptions of a well-established theory of communication, *relevance theory*, along with consequences that fall out of the minimal view of semantics defended in this chapter. Finally, I show that the account of mathematical discourse I provide meets all the relevant desiderata for such an account (§8.6).

In the conclusion, I argue that the hermeneutic fictionalism which results from combining my pragmatic theory of mathematical language use and nominalism can do everything platonism can do, only better, and it also promises to do more. It is therefore the preferable view. I finish by outlining the prospects for extending this kind of fictionalism to account for other areas of discourse.

6. The Semantics of Number and Magnitude

6.1 Introduction

A complete philosophy of mathematics should provide an account of mathematical discourse as it actually is. This will require both a semantic theory of mathematical language and a pragmatic theory of its use, starting with the former because, according to the traditional view of the semantics/pragmatics distinction defended in the previous chapter (§5.2-§5.5), pragmatic theorizing should be informed by the deliverances of semantics. It is widely assumed that the sentences of mathematical theories contain expressions which purport to refer to or describe mathematical objects. It follows from this that, if the sentences of mathematical language are generally speaking true, then mathematical objects exist. To the extent that mathematical language is shot through with such expressions, the central challenge facing a nominalist philosophy of mathematics is therefore to motivate a pragmatic theory of mathematical language use that does not require mathematical sentences to be true.

Before this, we must first investigate to what extent the aforementioned semantic assumptions about mathematical language are true. That is, we must first answer the following question: which sentences of mathematical language contain expressions which purport to refer to mathematical objects? In this chapter, I will show that the answer to this question is: fewer than expected. I will provide a semantic theory of a considerable fragment of the mathematical language used in science: first, applied arithmetical sentences, including cardinality and magnitude ascriptions; and, second, pure arithmetical language. I will show that the latter do

contain expressions which purport to refer to numbers. Contra popular philosophical opinion, however, the former do not.

As well as identifying which sentences the nominalist's pragmatic theory should target, my findings in this chapter will reveal that certain arguments for platonism (and therefore against nominalism) are unsound. Some arguments for platonism involve indicating a class of mathematical sentences which strike us as obviously true. From the apparently innocuous premise that these sentences are true, along with the assumption that they contain expressions purporting to refer to mathematical objects, we can validly infer that mathematical objects exist. Consider:

EA

EA1: The following sentence is true:

(1a) The number of planets in the solar system is eight.

EA2: If (1a) is true, then the number eight exists.

EA3: Therefore, there are numbers.

EA is an example of an *easy argument* for platonism (Hale 1988). The operative assumption here is that expressions such as 'The number of planets in the solar system' (number-of expressions) and 'eight' (numerals) are referring expressions that stand for numbers, and that the copula expresses identity. (1a) is therefore assumed to be an *equative sentence*, one which expresses that the objects apparently mentioned by the pre- and post-copular expressions are numerically identical. This analysis has its roots in Frege ([1884] 1953: 69), so I will call it the *Fregean*

analysis. (I should note that Frege did not put forward his analysis as thesis about the semantics of natural language, but instead as part of his project of providing an ideal language for science and mathematics; however, EA assumes the Fregean analysis *put forward as a thesis about the semantics of natural language*, and it is this assumption that concerns me in this chapter.)

Other arguments do not rely on mere intuitions about the truth of a certain class of mathematical sentences, instead relying on the assumption that these sentences are confirmed by their use in science. (The indispensability argument I presented in §1.2 (IA) relies on the assumption that mathematical sentences are confirmed by their role in science, but there are two reasons why I will not discuss it in this chapter: first, I have already shown that IA is unsuccessful for independent reasons; second, IA does not target a particular class of mathematical sentences. The apparent strength of the arguments for platonism presented here is due to the apparent harmlessness of accepting the target sentences as true or confirmed; this appearance may diminish with sentences invoking more complex or less familiar mathematical concepts.) Liggins (2008: 125) presents two such arguments. Here is one (example sentence has been changed from original):

MA

MA1: We should believe the measurement claims made by well confirmed scientific theories. For instance, astronomy's claim:

(1b) The mass of Jupiter in kilograms is 1.8986×10^{27} .

MA2: If these measurement claims are true, then there are abstract mathematical entities.

MA3: So we should believe that there are abstract mathematical entities.

Call this the *measurement argument*. MA2 is clearly relying on the same semantic assumptions that were operative in the easy argument presented above. These arguments pose a serious challenge to nominalism. The strength of both EA and MA lies in the apparent harmlessness of taking the relevant sentences (1a-b) to be true or confirmed. Almost everyone would accept that these sentences are true. So long as she accepts the semantic assumptions that lend support to EA2 and MA2, it appears the nominalist must ascribe widespread error to speakers and provide an explanation as to why speakers are susceptible to such error. My findings in this chapter allow for a more attractive means of replying to these arguments: demonstrate that the Fregean analysis is false. In this chapter, I shall do just that. If my alternative analysis is correct, sentences of applied arithmetic (i.e. cardinality ascriptions and magnitude ascriptions) present no obstacle whatsoever for the nominalist.

Philosophy of mathematics aside, my findings in this chapter also have important consequences in the philosophy of language. As Thomas Hofweber (2005: 179-80) notes, the Fregean analysis gives rise to a linguistic puzzle. In (1a-b), the numerals ‘eight’ and ‘ 1.8986×10^{27} ’ appear to occur in the syntactic position reserved for referring expressions—*singular-term position*. However, associated with sentences such as (1a-b) are sentences which appear to have similar meanings, and yet contain the same expressions in different syntactic positions. Consider:

(2a) There are eight planets in the solar system.

(2b) Jupiter has a mass of 1.8986×10^{27} kilograms.

While in (1a-b) the numerals appear to be in singular-term position, and so are intuitively referring expressions, the same expressions in (2a-b) appear to occupy adjectival position and serve the distinct function of modifying the corresponding noun phrase.

There is a semantic and syntactic dimension to this puzzle. The syntactic dimension arises because, typically, the same expression cannot occur in both singular-term and adjectival position *salva congruitate*, while if the Fregean analysis is correct, numerals can. The semantic dimension arises from the fact that the adjectival occurrences of numerals do not serve their typical semantic function. The ‘eight’ in (2a) does not plausibly stand for an object, but instead serves to modify the noun phrase ‘planets in the solar system’. This is especially puzzling since sentences (1a-b) are at least very similar in meaning to sentences (2a-b), so it is not plausible to suggest that the two different occurrences of the numerals are merely homonyms. I follow Hofweber (2005: 180) in calling this syntactico-semantic puzzle *Frege’s Other Puzzle* (FOP).

The Fregean analysis gives rise to a linguistic puzzle and is relied upon to argue for controversial ontological conclusions. Whether or not this analysis is correct should therefore be of great concern to philosophers. It is surprising, then, that this question has attracted very little attention in the philosophical literature. However, recently several authors (Hofweber 2005; Moltmann forthcoming; Felka 2014) have each offered rival analyses of sentences such as (1a-b), claiming that their analysis is to be preferred to the Fregean analysis because it solves FOP. Throughout this chapter, I will agree that an analysis that solves FOP should be

adopted in place of the Fregean analysis, but only if it also provides the best explanation of all the other available linguistic data concerning such sentences. The analysis of sentences such as (1a-b) presented in this chapter does just that.

The structure of the chapter is as follows. In §6.2, I show that numerals in adjectival position are adjectives, so do not purport to stand for numbers; in §6.3, I show that sentences such as (1a-b) are *specificational sentences*, but I reject the standard analysis of specificational sentences as question-answer pairs; in §6.4, I present and motivate my own analysis of specificational sentences; in §6.5 I corroborate my analysis further with evidence directly concerning number-of expressions; in §6.6, I show how my analysis solves FOP while explaining all the relevant linguistic data. Importantly, my analysis shows that sentences such as (1a-b) do not contain expressions that stand for numbers.

Finally, in §6.7, I consider pure arithmetical language, and argue that despite some evidence recently appealed to by Hofweber, such sentences do contain expressions that purport to refer to numbers. In the conclusion (§6.8), I argue that MA and EA might be saved by having them instead appeal to pure arithmetical language; however, considerations from §6.8 motivate a pragmatic analysis of pure arithmetical language according to which they are typically used to express propositions that do not concern numbers. Hence, the thought that Melia's descriptive proposal might provide the basis for a hermeneutic fictionalist philosophy of mathematics is a promising one. (§6.2, §6.3, and §6.4 of this chapter feature in Knowles forthcoming(b)).

6.2 Adjectival numerals

Call numerals in adjectival position ‘numeral modifiers’. Hofweber (2005) argues that numeral modifiers are determiners, which compose with nouns to make quantifier expressions. In this section, I outline the case for this position. Though it is initially promising, I argue that the truth is more complicated: numeral modifiers are adjectives, though they can sometimes occur in unexpected contexts; but I provide an independently motivated explanation of their unusual distribution. The result is a plausible and unified semantic analysis of numeral modifiers that sets the agenda for solving FOP. But first I must outline the formal framework within which I will be working.

Analysing natural language using the first-order quantifiers is problematic. ‘Something’ is assigned the existential quantifier and ‘Everything’ the universal; these are the basic units of analysis. But in English there are more complex quantifier phrases: ‘Some apples’; ‘Every man’; ‘Most coins’ etc. This suggests ‘Something’ and ‘Everything’ are also complex, made up of ‘Some’, ‘Every’ and ‘Thing’. There are also quantifier phrases in natural language that resists analysis in terms of the existential and universal quantifiers (‘Most’, for example). An adequate analysis of natural language should provide a unified and systematic analysis of quantifier phrases (cf. Hofweber 2005: 196), and account for all quantifier phrases of the language (cf. Barwise and Cooper 1981: 156-61).

Generalized Quantifier Theory (GQT) is a plausible and widely-accepted means of providing such an account. (GQT is based on the work of Mostowski 1957 and Montague 1974; for an overview of its developments, see Keenan and Westerstahl 1997; for an excellent introduction, see Cann 1993.) According to GQT, sentences are typically composed of a noun phrase (NP) and a verb phrase (VP),

each of which are assigned semantic values in accordance with their semantic type. VPs are typically assigned type $\langle e, t \rangle$, or functions from individuals (type e) to truth-values (type t), and are assigned sets of individuals as their semantic values. NPs are either quantifier expressions of type $\langle \langle e, t \rangle, t \rangle$ (functions from sets to truth-values), or names of type e (individuals), and are assigned sets of sets of entities or individuals, respectively.

Sentences are of type t and their semantic values are truth-values. The truth-values of sentences are determined compositionally by functional application. When interpreting a sentence S containing a quantifier expression as NP, for example, the semantic value of the NP takes the semantic value of the VP as an argument and yields a truth-value. To illustrate, for ‘All red peppers grow quickly’ the set of things that grow quickly is the semantic value of the VP, and the function that yields ‘true’ for all and only sets that contain all of the red peppers is assigned to the quantifier expression. Thus, the sentence is true iff the set of things that grow quickly contains all the red peppers—iff all red peppers grow quickly.

The above outline of GQT concerns only the *extensional* semantic values assigned to expressions. Expressions are also assigned *intensions*—functions which take a possible world as argument and yield that expression’s extension in that world.

Before continuing, a quick note on the difference between denotation and reference. In formal semantics, set-theoretical objects are assigned to expressions as semantic values. It must be stressed that these semantic values are, in an important sense, arbitrary. Beyond their playing the right role in the formal system for generating precise statements of truth-conditions, they have little significance (cf. Oliver 1996: 68). I call the relation an expression bears to its semantic value

‘denotation’ and the relation an expression bears to the object in the world it conventionally stands for ‘reference’. To illustrate, for various reasons Montague treated proper names as quantifier expressions. On this view, ‘David Attenborough’ denotes the set of all the sets that have David Attenborough as a member, but the name does not refer to this set; it refers to the person. The referent of an expression must bear a suitable relation to the semantic value of the expression so that the truth-conditions come out correctly overall, but that relation need not be identity.

In quantifier expressions, the modifying elements are *determiners*. ‘Some’ in ‘Some apples’ modifies ‘apples’ to yield an expression that concerns one or more apples. Other determiners include ‘Most’, ‘Every’ and ‘Both’. The semantic type of a noun is $\langle e, t \rangle$ so its extension is a set. The semantic type of a determiner is $\langle \langle e, t \rangle, \langle e, t \rangle, t \rangle$ and its extension is a function from sets to functions from sets to truth-values—a function from sets to the extensions of quantifier phrases.

According to GQT, numerals in adjectival position are determiners. For example, ‘Six’ modifies ‘apples’ to produce the quantifier expression ‘Six apples’. GQT provides a plausible, unified and widely-accepted treatment of determiners and quantifier expressions in natural language, and its account of numeral modifiers makes some accurate predictions. Consider the following:

(3) Four pigs surrounded the house.

The NP here is clearly behaving as a quantifier expression, which suggests that the numeral modifier is behaving as a determiner. However, such examples do not exhaust the syntactic distribution of numeral modifiers. They can also occur as adjectives. The semantic type of an expression is supposed to determine the semantic

values assigned to it as well as which expressions it can be meaningfully combined with. Determiners are of type $\langle\langle e, t \rangle, \langle\langle e, t \rangle, t \rangle\rangle$, so they can only take expressions of type $\langle e, t \rangle$ as arguments. A determiner should not therefore be able to take a quantifier phrase as an argument. There are examples that verify this prediction (I use ‘?’ to signify ungrammaticality):

(4a) ? The all/some men in this room wore silk.

(4b) ? All some/every chicken was cooked.

However, there is a particular subclass of ‘determiners’ that combine with nouns to form expressions that can serve as arguments for determiners. Numeral modifiers in particular exhibit this pattern. Consider:

(5a) The few/many/four berries in the basket were squashed.

(5b) All four berries in the basket were squashed.

(5c) Some four students turned up today.

Here, ‘few/many/four berries’ cannot be quantifier phrases because they occur as arguments for ‘The’, ‘Some’ and ‘All’, which are determiners. The modifiers ‘four’, ‘many’ and ‘few’ cannot therefore be determiners in this context. They modify a noun to make something that might serve as an argument for a determiner, namely another noun. This suggests that the modifiers occur here as *adjectives* of type $\langle\langle e, t \rangle, \langle e, t \rangle\rangle$, and are assigned functions from sets to sets.

There are two further pieces of evidence suggesting that numeral modifiers can occur as adjectives. The first is that complex numerals appear to get their meaning compositionally. Consider:

(6a) Mary saw two hundred pigs.

(6b) Mary saw two thousand pigs.

Intuitively, ‘two’ makes the same contribution to the meanings of (6a-b), and the difference between them is down to the difference in meaning between ‘hundred’ and ‘thousand’ (cf. Ionin & Matushansky 2006: 317). These intuitions are honoured if we assume that numeral modifiers are adjectives. Following Godehard Link (1983), let us assume that the plural noun ‘pigs’ denotes a set whose members are all the pluralities of pigs. A plurality of pigs is an object that is divisible into non-overlapping parts, all of which are pigs. In (6a), ‘hundred’ modifies ‘pigs’ and yields the noun ‘hundred pigs’ which denotes a set containing all and only the pluralities of pigs that are divisible into pluralities of pigs that are divisible exactly into one hundred pigs (including the pluralities that are one hundred pigs exactly). Then, ‘two’ modifies this to yield a noun denoting a set containing only those pluralities divisible into two non-overlapping pluralities which are divisible exactly into one hundred pigs. The story for (6b) is exactly the same but the modifier ‘thousand’ yields a noun denoting a set containing pluralities divisible exactly into two pluralities which are divisible exactly into one thousand pigs. In both, ‘two’ makes exactly the same contribution, and the difference between (6a-b) is rightly attributed to the difference in meaning between ‘thousand’ and ‘hundred’.

In contrast, the traditional GQT story treats ‘Two hundred’ and ‘Two thousand’ as distinct determiners, the meanings of which are not compositionally determined. This neither captures the similarity between the contributions made by ‘two’ in (6a-b), nor the difference between the contributions made by ‘hundred’ and ‘thousand’. A compositional account of complex numerals requires they be analysed as adjectives (see Ionin & Matushansky 2006 for a developed analysis).

Another relevant datum comes from apparent adjectival uses of combinations of numerals and nouns. The verb ‘considered’ should only be followed by the attribution of a property to something (cf. Partee 1986: 361): one considers something *to be a certain way*. Moreover, the conjunction ‘and’ can only combine expressions of the same semantic type. Now consider:

(7) Mary considered her lunch (to be) two portions and cheap!

Here ‘two portions’ is both the complement of ‘considers’ and part of a conjunction in which one of the conjuncts is the noun ‘cheap’. This suggests that it too is serving as a noun, and so ‘two’ is serving as an adjective. There are two strong pieces of evidence suggesting that numeral modifiers can occur as adjectives.

There is evidence that numeral modifiers can occur both as determiners and as adjectives. It is implausible to suggest that they are systematically ambiguous. There must be a common thread of meaning that explains their use in examples such as (3) on the one hand, and their use in examples such as (5-7) on the other. But how else can we explain the evidence on the present framework, which assigns a single semantic type to each expression type? Before answering, I will demonstrate that the

puzzling syntactic distribution of numeral modifiers is not unique. Many adjectives admit of an identical pattern. Consider:

(8a) She considered them tiny insects and invertebrates.

(8b) Tiny insects surrounded the house.

The fact that ‘tiny insects’ can form a conjunction with the noun ‘invertebrates’ and occur as the complement of ‘considered’ in (8a) is good reason to take it to be a noun, too. In (8b), however, ‘Tiny insects’ is the subject of the sentence and forms a full NP with existential force. How are we to explain this?

There is a modification of GQT that is both intuitive and explanatory of the present data. The idea is to reject the one-to-one correspondence between expression types and semantic types, and instead assign to each expression, as well as its base semantic type, a family of other types that it can take on if the linguistic context demands it. This does not mean giving up on compositionality. Theorists have uncovered systematic rules that govern this kind of type-shifting (see Partee and Rooth 1983 and Partee 1986, for example). For instance, it is generally agreed that type-shifting occurs only as a last resort to avoid a type mismatch, and a plausible rule specific to plural nouns is that, when combined with a VP that implies spatial location, their type is raised to that of a quantifier expression. This explains why ‘Tiny insects’ (8b) has existential force.

Given the similarity in distribution between adjectives and numeral modifiers, I propose that they be understood in the same way: they standardly occur as adjectives, but, subject to specific linguistic contexts in which a type mismatch threatens, the nouns they form part of are type-shifted to form quantifier expressions.

This explains the otherwise puzzling syntactic distribution of numeral modifiers. On this view, cardinal modifiers do not themselves undergo type-shifting; only the nouns they form part of do. In ‘Four pigs surrounded my house’, the threat of type mismatch only looms when the noun ‘Four pigs’ and the VP ‘surrounded my house’ are merged. At this point, the whole noun ‘Four pigs’ is type-shifted to the type of a quantifier expression. The cardinal modifier never behaves as a determiner. Similarly, though ‘Tiny insects’ in (8b) carries existential force, it would be wildly implausible to suggest that ‘Tiny’ acts as a determiner in this sentence. This highlights a significant benefit of the present view: it provides a uniform semantics of numeral modifiers. They only ever behave as adjectives, and their unusual syntactic distribution is explained by independently motivated rules governing plural nouns in general.

Now we have the semantics of numeral modifiers, the agenda for solving FOP is set: the semantic and syntactic relation between numerals in adjectival position and numerals in singular-term position must be accounted for. If it can be shown that, despite appearances, numerals in contexts such as (1a-b) are adjectives, then FOP will have been solved. Pursuing this strategy involves understanding the syntactic and semantic profile of sentences such as (1a-b). In the following section, I argue that such sentences are *specificational sentences*, but show that the mainstream analysis of specificational sentences is subject to serious problems.

6.3 Specificational sentences

Some sentences are useful for emphasising certain aspects of the content they convey. Consider the following:

(9a) John swims quickly to shore.

(9b) The way John swims to shore is quickly.

Intuitively, the information conveyed by (9a-b) is the same, but (9a) presents the information neutrally, while (9b) emphasises the *way* John swims. To illustrate, (9a-b) are both appropriate answers to the question ‘How does John swim to shore?’, but to the question ‘Where does John swim to?’, only (9a) is. This is an example of *focus*. As Hofweber (2005: 210-1) notes, sentences such as (1a) exhibit focus. Consider again (1a) and (2a):

(1a) The number of planets in the solar system is eight.

(2a) There are eight planets in the solar system.

Though the information conveyed by (1a) and (2a) is intuitively very similar, (1a) emphasises how many planets there are. To the question ‘How many planets are there in the solar system?’ both are appropriate answers; to the question ‘What is in the solar system?’ only (2a) is.

Sentences that exhibit focus and appear to *specify* which thing is mentioned by the subject of the sentence are called *specification sentences*. To appreciate how specification sentences differ from subject-predicate sentences, compare the following from Mikkelsen (2011: 1806):

Predication:

(10a) The hat I bought for Harvey is big.

(10b) What I bought for Harvey is big.

Specification:

(11a) The director of *Anatomy of a Murder* is Otto Preminger.

(11b) Who I met was Otto Preminger.

In (10a-b), the predicate ‘is big’ describes the thing picked out by the subject of the sentences, rather than specifying which thing it is. In (11a-b), however, the post-copular term specifies which thing the pre-copular expression stands for.

Examples of predication and specification share syntactic features. (10b) and (11b) begin with an interrogative pronoun as part of a free relative, and both are *pseudoclefts* (Higgins 1979 is the classic text on pseudoclefts). There are predication and specification pseudoclefts. (10a) and (11a) begin with a headed relative clause and are plain predication and plain specification sentences, respectively. Linguists agree that the distinction between predication and specification is the more semantically important one. In the absence of any evidence that different kinds of specificational sentences determine their truth-conditions differently, it is desirable to have a semantics of predication sentences that unifies all their syntactic forms, including plain and pseudocleft, and a semantics of specification sentences that unifies all theirs (cf. Mikkelsen 2011: 1807; Felka 2014: 268).

The fact that sentences such as (1a-b) exhibit the characteristic features of specification sentences suggests that such sentences are specification sentences. Given that cardinal modifiers are adjectives, this is a promising avenue for solving FOP because many specification sentences appear to have adjectives in post-copular position:

(12) The colour of the squirrels is red.

To pursue this avenue, an analysis of specificational sentences must be provided. To begin with, there is good evidence for taking the post-copular expression of specificational sentences to be elliptical for a clause, because doing so solves an important syntactic puzzle. Consider:

(13a) John likes himself.

(13b) What John likes is himself.

In both, ‘himself’ borrows its referent from ‘John’. However, this is only possible if the two terms stand in a specific syntactic relationship called *c-commanding*. This relationship is represented in syntax trees in the following way. A node x dominates a node y iff x is above y and one can trace a line from x to y while only moving downwards. A phrase a c-commands for a phrase b iff the first branching node dominating a also dominates b , and neither a nor b dominate each other. See Figure 1 for the syntax tree assigned to (13a) below:

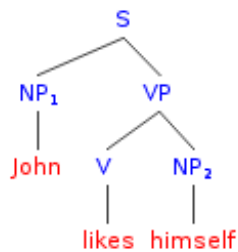


Figure 1: Syntax tree for ‘John likes himself’

According to figure 1, ‘John’ c-commands ‘himself’, so the former can lend its reference to the latter. Now compare figure 1 to figure 2, which gives the syntax tree for ‘What John likes is himself’:

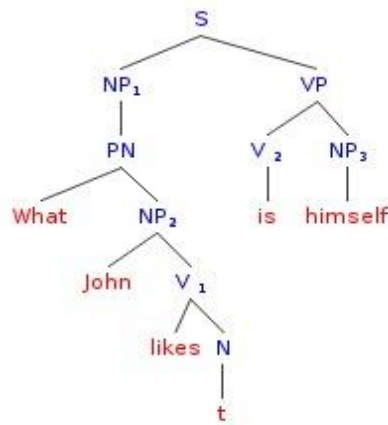


Figure 2: syntax tree for ‘What John likes is himself’

Here, ‘John’ does not c-command ‘himself’, so the former cannot lend its reference to the latter. However, in (13b) ‘John’ does lend its reference to ‘himself’.

If we accept that the post-copular expression is elliptical for a clause, the puzzle can be solved. Compare:

(13b) What John likes is himself.

(13c) What John likes is ~~John likes~~ himself.

Here the post-copular expression ‘himself’ is elliptical for ‘John likes himself’, which shares the syntactic structure of (13a) and therefore has ‘John’ c-commanding ‘himself’. The puzzle is thereby solved.

There is more evidence in support of the claim that the post-copular term in specification sentences is elliptical, the most impressive of which is that certain specification sentences allow for the post-copular clause to be stated in full:

(14a) What I did then was call the grocer.

(14b) What I did then was I called the grocer. (Ross 1972, (39a), (19b))

Here the ellipsis is optional, but sometimes it is not. There is a plausible explanation for this. Consider:

(15a) Who I called was the grocer.

(15b) Who I called was ~~I called~~ the grocer.

The repetition of the verb makes ellipsis obligatory, while there is no danger of repetition of the verb in (15b) (cf. Schlenker 2003: 14).

Specificational sentences exhibit two kinds of pre-copular clauses. The first kind is a relative clause headed by an interrogative pronoun—a free relative. The second is a definite description, sometimes in the form of a headed relative clause. As I mentioned above, in the absence of evidence suggesting they should be treated differently, it is desirable to have a unified semantics of these different syntactic forms. How can we analyse the pre-copular clauses of specificational sentences in such a way that meets this constraint? The following evidence suggests a promising means.

(16a) The murderer of Smith is John.

(16b) Who the murderer of Smith is is John.

Intuitively, (16b) provides an adequate paraphrase of (16a). Similar evidence has often been appealed to (in Grimshaw 1979, for example) in defence of the view that definite descriptions are disguised free relatives in embedded contexts:

(17a) I know the capital of Italy.

(17b) I know what the capital of Italy is.

Given these paraphrases, it is tempting to analyse definite descriptions in specificational contexts as free relatives in disguise. Free relatives are standardly classified as kinds of interrogative clause. To the *direct question* ‘Who is the murderer of Smith?’, there is the *indirect question* ‘Who the murderer of Smith is’. This suggests an analysis of specificational sentences according to which the pre-copular clause is a question to which the post-copular clause provides the answer in the form of an elided declarative sentence. Call this the *question-answer analysis* (QAA). This is a popular analysis, and a few different versions of it have been proposed (see Schlenker 2003, Felka 2014, and Moltmann forthcoming). With respect to analysing sentences such as (1a), the most compelling analysis is Felka’s (2014: 277):

(18) [~~What~~ the number of planets is] is [~~There are eight planets.~~]

According to Felka, what look like definite descriptions in contexts such as (1a-b) are elided free relatives that pose questions, and what look like singular terms in

post-copular position are elided declarative sentences that provide the relevant answer. This analysis promises to solve FOP because it takes the post-copular term to be elliptical for an expression in which the cardinal occurs in its typical syntactic position and contributes its typical semantic value.

However, there are two significant drawbacks to QAA that demand we look for an alternative. There is evidence to suggest that the semantics of free relatives and definite descriptions as they occur in specificational sentences is distinct, meaning that a unified account such as QAA is inappropriate.

The first drawback is that headed relatives and the definite descriptions they are supposed to elide are semantically distinct when they occur in contexts other than specificational sentences. In intensional contexts, free relatives can occur in both factive and non-factive contexts, while the corresponding definite descriptions can only occur in factive contexts. Consider:

Non-factive: (19a) I wonder/fear who the woman who John is seeing is.

(19b) *I wonder/fear the woman who John is seeing. (The latter sentence is grammatical, but it does not follow from the corresponding sentence in (19a).)

Factive: (20a) I know/remember what the capital city of Italy is.

(20b) I know/remember the capital city of Italy.

This suggests a difference in the semantics of free relatives and definite descriptions in intensional contexts. It is of course open to the proponent of QAA to claim that only definite descriptions occurring in specificational sentences are elided free

relatives, but with this QAA begins to feel *ad hoc*. The second drawback exacerbates this feeling. The problem is that the distribution of free relatives in specificational contexts outstrips that of definite descriptions in *specificational contexts*. One example is that free relatives can form specificational sentences that specify the qualities an individual has, while definite descriptions cannot. Consider:

(21a) What the woman who John is seeing is is small.

(21b) The woman who John is seeing is small.

The truth-conditions of these sentences are intuitively the same, yet (21a) is specificational while (21b) is predicational, so the latter cannot be elliptical for the former. Another difference in distribution is the following:

(22a) Where the woman who John is seeing is is Scotland.

(22b) *The woman who John is seeing is Scotland.

Despite the contextual prompting, the definite description in (22b) cannot be elliptical for the free relative in (22a). Yet, if QAA is true, there is no good reason why definite descriptions could not occur as elided ‘Where’-questions as well as ‘What’- or ‘Who’-questions (cf. Frana 2007). Again, the proponent of QAA could modify her position to the following: definite descriptions are elided free relatives only if they occur in subject position of specificational sentences that specify who or what an individual is. Now QAA looks unacceptably *ad hoc*.

In this section, I have argued that sentences such as (1a-b) are specificational. I showed that, though QAA is an initially plausible means of providing a unified

semantic account of the different syntactic forms of specificational sentences, the syntactic distribution of free relatives and definite descriptions in specificational and non-specificational contexts suggests that such a unified analysis is inappropriate. A better account of specificational sentences should attribute different semantics to free relatives and definite descriptions, but explain why their contribution in specificational contexts in which they both occur yields the same truth-conditions.

6.4 Free relatives and definite descriptions

In this section, I independently motivate accounts of both definite descriptions and free relatives in intensional and specificational contexts. I show that these accounts predict their differences in distribution, while still allowing a unified account of the truth-conditions of specificational sentences. I start by appealing to data concerning certain intensional transitive verbs. There is strong evidence to suggest that ‘need’ and ‘want’ take proposition-denoting terms as their object (see den Dikken, Larson & Ludlow 1997):

(23) The scientists need some new computers, but their budget won’t allow it.

What the scientists need is *that the scientists get some new computers*. The anaphoric ‘it’ is clearly referring to a proposition here; reference to the scientists would instead require ‘them’. This evidence is symptomatic of a general pattern. The impersonal pronoun ‘it’ is not appropriate for referring to people, and not always appropriate for referring to properties.

(24a) *Jane called Jim and asked it its opinion.

(24b) *Jane said Jim is reproachable, and he is it.

However, 'it' is routinely used to refer to propositions and facts:

(25a) It's true that there are aliens, but the government is covering it up.

(25b) The fact that $2+2=4$ is obvious; it can be proven.

It is therefore telling that specificational sentences concerning people only allow for the impersonal pronoun:

(26a) The winner of the competition is Anna, isn't it?

(26b) *The winner of the competition is Anna, isn't she?

(26c) Who the winner of the competition is is Anna, isn't it?

(26d) *Who the winner of the competition is is Anna, isn't she?

This suggests that the things referred to in specificational sentences are propositions or facts. But which is it? Consider:

(27a) What I love is a secret.

(27b) The thing I love is a secret.

Suppose I love mountain climbing. It doesn't make sense to say mountain climbing is a secret; it's the fact that I love mountain climbing that is secret. However, the same free relative and definite descriptions can also refer to a particular activity:

(28a) What I love is dangerous.

(28b) The thing I love is dangerous.

The fact that I love climbing isn't dangerous; the activity is. How can we explain these two readings? I will consider free relatives first.

To account for both readings of free relatives, I will consider more closely their syntax. The standard account is that they are syntactically derived from sentences by a process called 'movement'. In 'What I love', the pronoun 'what' has been moved out of its object position as the complement of 'love' into subject position. This is illustrated as follows:

(29) What₁ I love ____₁.

On the copy theory of movement, revived by Noam Chomsky (1993), moved terms leave behind a phonetically deleted copy of themselves in their original position. This view has a great many theoretical virtues. First, copying can be understood in terms of the procedure that combines two syntactic objects into a new syntactic object, *Merge*. A syntactic item is Merged for a second time in a different place—movement is *re-Merge*. Second, it does not posit any other objects over and above lexical items, bringing us closer to a theory of syntax as a recursive procedure that operates only on the lexicon.

More importantly, the copy theory of movement leaves open the possibility of interpreting the moved item in its surface position and/or its original position. The following demand the moved item be interpreted in different positions (cf. Bhatt 2002: 56-57):

(30a) The first book_i that John said Tolstoy ever wrote ______i was *Anna Karenina*.

(30b) The first book_i that John ever said Tolstoy wrote ______i was *Anna Karenina*.

The salient reading of (30b) is that John said the first book Tolstoy wrote was *Anna Karenina*, which is only possible if ‘first book’ is interpreted inside the scope of ‘said’. The copy theory of movement makes sense of this: the original copy of ‘first book’ is interpreted while the surface copy is not. The reading of (30b), however, is that John often attributes works to Tolstoy, but on the first occasion he attributed *Anna Karenina*. For this, ‘first book’ must be interpreted outside of the scope of ‘said’. A copy outside of the scope of ‘said’ is interpreted while the others are not. (I say ‘a copy’ because, according to many analyses, there is an intermediary copy between the surface position and the origin position; see Bhatt 2002, for example; and see Fox 2002 for a well-motivated account of how different copies are interpreted.)

The copy theory of movement explains the two readings of free relatives. In ‘What I love’, there is a choice as to which copy of the moved element ‘What’ we interpret. If the surface copy is interpreted, it is interpreted as an NP in subject position, and so plausibly refers to a person. The embedded sentence ‘I love ____’ then acts as a modifier, narrowing the referent of the pronoun to the thing loved by me. If we interpret the original copy, then its semantic function is that of an NP in object position. In this case, the semantic contributions of the terms in the free

relative are the same as in the sentence it is derived from, ‘I love what’. In this case, ‘What I love’ is a *nominalization* of ‘I love what’.

Nominalizations are referring expressions derived from non-referring expressions. They refer to something semantically assigned to the expression they are derived from. For example, the nominalization of the VP ‘is bad’ is ‘being bad’ which refers to the property expressed by the VP. Sentences nominalizations include ‘that’-clauses. In ‘John believed that there are aliens’, the ‘that’-clause nominalization ‘that there are aliens’ plausibly refers to the proposition that there are aliens. Propositions are semantically assigned to sentences as their intensions, so that nominalizations of sentences can refer to them is to be expected.

However, some sentence nominalizations appear to refer to facts or states of affairs. For example, the ‘that’-clause in ‘John remembered that it was Friday’ plausibly refers to the fact that it was Friday, and the imperfect nominal of ‘John’s walking to the park’ plausibly refers to the relevant state of affairs. In the present semantic framework, facts and states of affairs are not assigned to sentences, but it seems they should be if we want to give a systematic account of nominalizations.

Accounting for referents of certain nominalizations is one of many reasons for introducing facts or states of affairs into the semantics of natural language (see Kratzer 2014 and references therein for further reasons). There are many proposals, perhaps the most famous of which is *situation semantics* (Barwise and Perry 1983 is the classic text). There is not space here to motivate a particular account, so it will suffice to make a small extension of the present framework (inspired partly by Moffett 2003, though he extends a different, intensional framework).

Instead of truth-values, I will assign facts as the extensions of sentences. The role of truth-values in formal semantics is to have one or the other assigned to a

sentence, relative to a possible world, in virtue of the semantic values of the parts of the sentence and the way they are composed. When things go well, a sentence is assigned True; otherwise it is assigned False. The neutrality of this role is reflected in the fact that many use 1 and 0 as the semantic values of sentences. This role can be preserved by facts, so no harm is done to the formal framework. Instead of directly characterising the truth or falsity of sentences, I will indirectly characterise it in the following way. For each world w , there are two sets of facts, TRUE^w and FALSE^w . TRUE^w contains all the facts that are parts of w . These are the facts that *obtain*. FALSE^w contains all the facts that are not parts of w . These facts do not obtain. So, a sentence is true at w iff it denotes a member of TRUE^w , and false iff it denotes a member of FALSE^w . On this view, the intensions of sentences, or propositions, are functions from worlds to facts, or, equivalently, sets of facts. In §6.7 I will say more about the notion of correspondence.

Aside from complicating the statement of truth-conditions, this yields a systematic account of the referents of sentence nominalizations. Depending on the context and the kind of nominalization, they can refer either the extension of the relevant sentence (a fact), or the intension (a proposition). Now consider the following:

(31) What I love reflects badly on my character.

It makes no sense to say that mountain climbing reflects badly on my character. This forces the original copy of the pronoun ‘What’, and not the surface copy, to be interpreted. Hence, the free relative is interpreted as a nominalization of the sentence ‘I love what’ and refers to the fact denoted by this sentence.

We have seen that definite descriptions can also be used to refer to facts. The account I propose for them is inspired by a recent extension of Angelika Kratzer's (2002) *de re* account of knowledge ascriptions, proposed by Ilaria Frana (2006). Kratzer's analysis is that ascriptions of the form 'A knows that p' ascribe knowledge to an agent, *de re* of some particular fact, that it *exemplifies* the relevant proposition. Intuitively, a fact exemplifies a proposition just in case it involves only things that are relevant to the truth of the proposition. (I will use the word 'correspond' in what follows.) On Kratzer's view, 'that'-clauses play a dual role: they characterise a particular fact, and they characterise the content of the state of knowledge the agent has concerning that fact (Kratzer 2002: 659).

Frana's innovation is to recognise that view can be extended so that things other than facts are the *res* of knowledge ascriptions. Consider:

(32) Ben knows the capital of Indonesia.

The definite description here plays an analogous dual role. First, it characterise a particular object, namely Jakarta; second, it gives the description correctly ascribed to Jakarta by Ben. (32) says that Ben knows, of Jakarta, that it is the capital of Indonesia. Both of these roles are unquestionably played by definite descriptions in other contexts: they correspond to the *de re* and *de dicto* readings definite descriptions exhibit. The crucial claim here is that in some contexts they play both roles at the same time.

When a definite description plays a dual role, it can plausibly refer to a fact: the fact that the object characterized is the way the description says. Indeed, knowing of Jakarta that it is the capital of Indonesia is tantamount to knowing the fact that

Jakarta is the capital of Indonesia. This makes perfect sense of the fact-referring occurrence mentioned above:

(33) The thing I love is a secret.

It makes little sense to say that mountain climbing is a secret, so the dual role of the definite description is triggered, and the fact that I love climbing is referred to. This yields the desired interpretation.

The accounts of free relatives and definite descriptions I have developed can explain the difference in distribution between the two, demonstrated in the previous section. Recall that free relatives can occur in both factive and some non-factive contexts, while definite descriptions and headed relatives can only occur in factive contexts.

Non-factive: (19a) I wonder/fear who the woman who John is seeing is.

(19b) *I wonder/fear the woman who John is seeing.

Factive: (20a) I know/remember what the capital city of Italy is.

(20b) I know/remember the capital city of Italy.

On the present account, it is expected that definite descriptions cannot occur in non-factive contexts. Assuming they take on their dual role in such contexts, (19b) would read that I wonder/fear of a particular woman whether she is the one John is seeing. If this makes sense, it is the wrong interpretation. More generally, the dual role of definite descriptions, and the resulting *de re* reading, implies some kind of

acquaintance with the relevant object, whereas intensional verbs such as ‘wonder’ imply ignorance.

As for how free relatives can occur in non-factive contexts, unlike fact-referring definite descriptions, they do not presume acquaintance with anything. (19a) does not say that I wonder/fear *de re* of a particular woman whether she is the one John is seeing. Rather, it says that I wonder/fear what the fact of the matter is concerning who John is seeing.

Recall that definite descriptions cannot stand in for free relatives occurring in specificational sentences that specify qualities of an individual, including location and height:

(21a) What the woman who John is seeing is is small.

(21b) The woman who John is seeing is small.

(22a) Where the woman who John is seeing is is Scotland.

(22b) The woman who John is seeing is Scotland (does not follow from 22a).

Definite descriptions only refer to a particular fact. Let’s say John is seeing Susan. When ‘The woman who John is seeing’ takes on its dual role, it both picks out a person, Susan, and characterises the description that truly holds of her, namely that Susan is the woman who John is seeing. It thereby refers to the fact that Susan is the woman John is seeing. There is nothing in the content of the description that might permit reference to a fact concerning Susan’s whereabouts or her size, so the infelicity of (22b), and the fact that the definite description in (21b) does not refer to a fact at all, are to be expected.

The evidence at the beginning of the section suggests that specificational sentences concern facts or propositions. I have shown that and explained how definite descriptions and free relatives can be used to refer to facts across different contexts. I feel justified in assuming for the remainder of this paper that they refer to facts in specificational contexts.

6.5 Number-of expressions

In this section, I will show that number-of expressions are typically used to refer to facts. This supports my claim that sentences such as (1a-b) are specificational sentences, and my claim that the pre-copular clauses of specificational sentences refer to facts.

On the Fregean analysis, number-of terms stand for numbers. In this section, I examine evidence presented by Moltmann for the claim that number-of terms stand instead for number tropes. Tropes are typically understood as particular instances of properties. Typical examples include the beauty of a particular painting, or the fragility of a particular vase, and are the qualitative aspects of objects (see Williams 1953a; 1953b for the classic motivations for trope theory; see Campbell 1990 and Ehring 2011 for some contemporary applications). Moltmann claims that number tropes are the quantitative aspects of pluralities.

Moltmann presents two kinds of evidence. The first is meant to show that number-of terms are singular terms that do not stand for numbers. To see that ‘The number of pigs’ is a singular term, consider:

(34a) The number of pigs is small.

(34b) The number of pigs is surprising.

(34c) The number of pigs is the same as the number of chickens.

We have little choice but to interpret ‘The number of pigs’ as standing for an object that each predicate is describing.

Moltmann argues that number-of terms do not stand for numbers by demonstrating that constructions permissible with ‘The number four’ are not so with number-of terms, and vice/versa (2013a: 502-4; forthcoming: 2-1). Suppose the number of pigs is four, and consider:

(35a) * The number four is small.

(35b) * The number four is surprising.

(35c) * The number four is the same as the number four.

(35d) The number four is the number four.

Though (35d) may be permissible, it is less natural than (35c). (35a) and (35b), though grammatical, seem to exhibit a category mistake, attributing the wrong kind of property to the referent of their NPs. Similarly, when we rewrite (34c) as a straightforward identity statement, we reach:

(36e) * The number of pigs is the number of chickens.

This sounds odd, if it makes sense at all. If the number of pigs and the number of chickens were numbers, such an identity statement would sound fine.

This leads Moltmann to conclude that number-of terms do not stand for numbers. However, the evidence is not yet conclusive. The permissibility of (34a-c)

shows that, in those contexts, ‘The number of pigs’ doesn’t stand for a number: it is that there are four pigs that is surprising. It does not show that number-of terms do not stand for numbers in other contexts. Suppose that John typically wants things that are large and non-shiny, but now he wants a diamond. Compare:

(37a) What John wants is unusual.

(37b) What John wants is small and shiny.

In (37a), it is that John wants the diamond that is said to be unusual; the diamond itself need not be unusual for the sentence to be true. On the other hand, (37b) appears to describe the diamond. The same term, ‘What John wants’, stands for a different object depending on the context and the predicate used. In §6.4, I explained the ambiguity of such expressions. On the reading on which it stands for a diamond, the surface copy of the pronoun is interpreted, which explains why it stands for an individual. On the reading where it stands for a fact, the original copy is interpreted, so ‘What John wants’ is a nominalization of the sentence ‘John wants what’, and stands for the fact denoted by this sentence.

Might ‘The number of pigs’ also change referents in different contexts? One reason for thinking that it does not is that there would be no parallel explanation for how the different meanings arise in different contexts. The syntax of number-of expressions does not appear to be the result of any movement, so there is no referring expression that can be interpreted in different syntactic positions. The explanation for free relatives is therefore unavailable. However, some mathematical predicates are applicable to the referents of number-of expressions:

(38a) The number of pigs is even.

(38b) The number of pigs is prime.

Moltmann points out that certain mathematical predicates make unacceptable constructions:

(39b) * The number of pigs is rational.

(39c) * The number of pigs is real.

Moreover, she claims there is a uniting feature of the mathematical contexts that are permissible: they can be verified or falsified by performing operations on collections of objects (cf. Moltmann forthcoming 8-13). (38a) implies the pigs can be divided into two groups of equal number; (38b) implies they can't. Both can be verified or falsified by arranging the objects and counting them. The explanation for this is supposed to be that the referents of number-of terms are not numbers, but rather something more closely related to the relevant collection of individuals. I find this first set of evidence compelling.

Moltmann's second kind of evidence is supposed to show that the referents of number-of terms share properties with the relevant collections. Consider:

(41a) The number of women is unusual.

(41b) The women are unusual, in number.

The predicate 'is unusual' can be applied to both the number of women, and the collection of women, so long as the modifier 'in number' is added. Moltmann claims

this shows that the collection of women and the number of women share the property of being unusual. She claims that, because ‘in number’ must be added to preserve the meaning of (41a), it is something to do with the quantitative aspect of the women that is unusual.

This evidence is dubious. It is true that the number of women bears a special relation to the collection of women that allows the move from (41a) to (41b). But this does not mean that these two entities share the property of being unusual. Compare:

(42a) The font of the book is large.

(42b) The book is large, in terms of its font.

The book and the font stand in a relation that permits the move from (42a) to (42b), but the book and the font do not share the property of being large. (42a) and (42b) can be true when the book is small. Similarly, it is the number of women that is unusual; the women need not be.

Suppose these two entities do share properties. It does not immediately follow that number tropes are the ideal candidates for the referents of number-of terms. Moltmann argues that the kinds of properties shared by the referents of number-of terms and collections can only be instantiated by concrete entities. She concludes that the referents of these terms are number tropes, the concrete, quantitative aspects of collections (forthcoming).

Moltmann assumes the standard notion of concreteness, and so takes it that an entity is concrete if it can be a relatum of causal relations, is spatio-temporally

located, and can act as an object of perception. She points out that perceptual and causal predicates apply to the referents of number-of terms:

(43a) John noticed the number of women.

(43b) The number of women caused Mary consternation.

This is poor evidence. Examples attributing causal and perceptual properties to things that cannot enter into causal or perceptual relations abound:

(44a) John began to see Mary's point of view.

(44b) Fermat's Last Theorem caused Mary frustration.

Objection: 'see' is figurative in (44a). But that is precisely the point. Perceptual predicates are often used in this way to express that something is understood, and there is no evidence suggesting that 'noticed' in (43a) is not used in this capacity. Similarly in (44b), though a theorem is not causal, we would say that Fermat's Last Theorem causes frustration if attempts to prove it are frustrating. There is no evidence to rule out that 'caused' is used in this loose way in (43b).

Moltmann's criteria for concreteness include spatio-temporal location, and yet she admits that spatio-temporal predicates do not apply to the referents of number-of terms (2013a: 505; 2013b: 56-7). Consider:

(45a) * The number of cats is in the bedroom.

(45b) * The cats are in the bedroom, in number.

(45c) * The cats are no longer, in number.

(45d) * The number of cats is no longer.

If number tropes were the concrete quantitative aspects of collections, they would go wherever and whenever the collections go. Even if the number of cats and the collection of cats share properties, there is no evidence that they share concrete properties. There is no evidence that number tropes are the referents of number-of terms.

What can we conclude about number-of terms? The evidence presented in this section suggests that the referents of number-of terms are not numbers, but instead are entities that are more intimately related to the relevant collection of objects. We saw that Moltmann tried to motivate that these referents are kinds of tropes, but that her evidence was unconvincing. This means the claim that such expressions refer to facts is available. The following argument provides some positive evidence in favour of this claim.

(46a) Mary discovered an interesting fact.

(46b) The fact was the number of planets there are in the solar system.

(46c) Therefore, Mary discovered the number of planets in the solar system.

This argument strikes me as valid. On the view that number-of expressions and free relatives (in extensional contexts) refer to facts, the apparent validity of this argument is explained. If number-of expressions stood instead for numbers, it would be extremely difficult to account for the apparent validity of (46a-c).

The evidence presented in this section suggests that number-of terms stand for facts, and the same goes for magnitude-of terms. I am now in a position to

present my account of specificational sentence, and therefore sentences such as (1a-b).

6.6 Puzzles and solutions

The evidence presented in §6.3 suggests that the post-copular expression in a specificational sentence is an elided declarative sentence. The evidence in §6.4 and §6.5 suggests that the NPs of specificational sentences refer to facts. Consider:

(47a) The thing I love is mountain climbing.

(47b) [The thing I love] is [~~H~~love mountain climbing.]

Suppose I love mountain climbing. ‘The thing I love’ takes on its dual role and refers to the fact that I love mountain climbing. This is precisely the fact that is the extension of the sentence occurring in post-copular position. A natural way to interpret (47a) is as expressing the identity of the fact referred to by the pre-copular clause and the fact denoted by the post-copular clause. We can write the truth-conditions informally as follows (where $[[S]]$ is the extension of S , i.e. the fact it corresponds to):

$[[\text{The thing I love is mountain climbing}]] \in \text{TRUE}$ iff: the fact referred to by ‘The thing I love’ is identical to the fact that is the extension of ‘I love mountain climbing’.

Now consider a specificational sentence involving a free relative:

[[What I love is mountain climbing]] \in TRUE iff: the fact that is the extension of ‘I love what’ is identical to the fact that is the extension of ‘I love mountain climbing’.

This gets the truth-conditions right. Both are true only in worlds where I love mountain climbing. Call this the *fact-analysis* (FA). FA provides a unified account of specificational sentences in some sense, since the truth-conditions assigned to their different forms come out the same. However, as demonstrated in the previous section, the differences attributed to free relatives and definite descriptions mean FA is compatible with the differences in their syntactic distribution. Moreover, FA does all this without positing a new kind of copula. FA interprets the ‘is’ in specificational sentences as the familiar ‘is’ of identity. So FA suffers none of the drawbacks of QAA.

The claim that specificational sentences are kinds of equative sentences is not itself new. In fact, Philippe Schlenker’s (2003) own brand of QAA bears some similarity to my own. It will be instructive to contrast Schlenker’s account with FA because Schlenker’s analysis is subject to a problem that appears initially to plague FA. While Schlenker is forced to weaken the plausibility of his account, we shall see that FA has an elegant means of avoiding the problem.

Schlenker takes indirect questions to denote the proposition that is their own unique and exhaustive answer (as in Groenendijk and Stokhof 1997). He takes specificational sentences to equate the propositions denoted by the pre-copular clause with the proposition expressed by the post-copular clause. Suppose that the unique and exhaustive true answer to ‘What John loves?’ is <John loves only John>. ‘What John loves is himself’ is then understood as identifying this proposition with

the proposition expressed by the post-copular clause, $\langle \text{John loves John} \rangle$. The problem is that these propositions are not identical: $\langle \text{John loves John} \rangle$ can be true if John loves Mary as well. Implausibly, this means ‘What John loves is himself’ can never be true.

Schlenker’s suggested solution weakens the plausibility of his analysis. He claims that the post-copular clause pragmatically implicates the stronger proposition denoted by the pre-copular clause *before* the two compose into the equation (2003: 24-27). Even if we assume ‘John loves himself’ typically implicates that John loves only himself, that an utterance usually carries an implicature does not establish that the relevant pragmatic process is pre-semantic. We have seen that Schlenker is not on his own in thinking pre-semantic pragmatic processes are commonplace (he cites Chierchia 2000). However, in §5.2, I provided strong reasons for avoiding this sort of analysis.

Adopting FA yields a more elegant solution to this problem that stays safely within the boundaries of formal semantics. I have not said much about what facts are because the metaphysics of facts is beyond the scope of formal semantics. Here, facts are just the referents of certain sentence nominalizations. However, the role I have given them in my theory does tell us something about them. Along with Kratzer (2002), we might take them to be the parts of worlds that propositions true at that world correspond to, such that, if a fact f corresponds to a proposition p , then f should only involve things that are relevant to p ’s truth.

Now, let us suppose that John loves only himself and consider the propositions $\langle \text{John loves only John} \rangle$ and $\langle \text{John loves John} \rangle$. What part of the world is most relevant to the truth of the former? The part that involves John’s loving himself and no one else—the fact that John loves only himself. In this circumstance,

it is the very same part of the world that is relevant to the truth of $\langle \text{John loves John} \rangle$. In a world where John loves only himself, what difference could there be between the part where John loves himself and the part where John loves *only* himself? Despite the fact that they are different propositions, in worlds where they are both true they correspond to the same fact. Now suppose that John loves Mary as well as himself. In this scenario, there is no part of the world that involves John's only loving himself, so the fact that John loves only himself doesn't obtain and $\langle \text{John loves only John} \rangle$ is false. In this scenario, $\langle \text{John loves John} \rangle$ corresponds to the part of the world that involves John's loving John—the fact that John loves John. Since this obtains, the proposition is true. In worlds where John loves more than himself, the corresponding facts differ, so 'What John loves is himself' comes out false. FA gets the truth-conditions right with no need for suspect linguistic apparatus.

I am now in a position to give an informal formulation of the truth-conditions of (1a-b):

$[[\text{The number of planets in the solar system is eight}]] \in \text{TRUE}$ iff: the fact referred to by 'The number of planets in the solar system' = the fact that is the extension of 'There are eight planets in the solar system'.

$[[\text{The mass of Jupiter in kilograms is } 1.8986 \times 10^{27}]] \in \text{TRUE}$ iff: the fact referred to by 'The mass of Jupiter in kilograms' = the fact that is the extension of 'Jupiter has mass 1.8986×10^{27} kilograms'.

This analysis inherits the same explanation of FOP as the QAA analysis. FA assumes that the post-copular term of a specificational sentence is an elided

declarative sentence. In this case, the sentences are ‘There are eight planets in the solar system’ and ‘The mass of Jupiter in kilograms’, in which the cardinals ‘eight’ and ‘ 1.8986×10^{27} ’ occur in their typical syntactic position and contribute their typical semantic values.

6.7 Pure arithmetic

The evidence presented so far strongly suggests that cardinality and magnitude ascriptions do not contain any expressions which purport to refer to numbers. Such sentences therefore pose no problem for the nominalist. This has removed a considerable obstacle for nominalism, since applied arithmetical sentences comprise a substantial fragment of the mathematical language used in science. However, pure mathematical language comprises another substantial fragment of the mathematical language used in science, so we must now turn to the semantic analysis of pure mathematics.

Most areas of science draw on pure arithmetical language, in calculating quantities and magnitudes of objects for example. Consider the following formulae:

$$(48) 2 + 2 = 4$$

This is a theorem of pure arithmetic. The syntax and semantics of this sentence is clearly that of an equative sentence, whereby it expresses the numerical identity of the things the pre- and post-copular expressions stand for. In this case, the pre-copular expression ‘4’, and the post-copular expression ‘2+2’ are unquestionably names for numbers. The Dedekind-Peano axioms from which this sentence follows allow little room for argument on this front.

However, Hofweber has provided an alternative and nominalist friendly semantic analysis of pure arithmetical sentences. In this section, I will argue that, though it is inventive, Hofweber's analysis is problematic and unjustified. I will argue that the spirit of the proposal can be preserved in the form of a pragmatic theory of the use pure arithmetical sentences are put to. Moreover, I will show that the evidence Hofweber presents supports the kind of nominalist friendly pragmatic theory I intend to provide in this second part of the thesis, even where it fails to support his own semantic analysis.

Hofweber's alternative analysis of pure arithmetical sentences is motivated in part by its ability to explain another instance of FOP. Hofweber (2005: 194-195) points out that, when people are asked to read aloud sentences such as (48), they utter a variety of different sentences:

(49a) Two and two are four.

(49b) Two and two is four.

(49c) Two and two equal four.

(49d) Two and two equals four.

The important thing to recognise here is that two of these sentences are plural while the others are not. (To avoid confusion, I will avoid the expression 'singular' when describing non-plural sentences, since I will be using this expression for a different purpose in what follows.) The non-plural sentences appear to be about a particular object, namely the number four. The copula or 'equals' in these sentences is therefore interpreted straightforwardly as the identity predicate. However, because the other two sentences are plural, it seems they are instead about collections of

entities. In fact, it seems as though the numerals occurring in these sentences are not in singular-term position, but are instead occurring in their typical adjectival position, though absent an argument noun. I do not have the space to present a detailed syntactic and semantic account of such sentences, but the surface syntax and intuitions suggest that we should analyse the plural sentences above as expressing general propositions about collections of objects, whereby the lack of arguments for the cardinals indicates that no objects in particular are mentioned, and perhaps suggests the occurrence of hidden variables (see Hofweber 2005: 194-195; though, as mentioned in §6.2, Hofweber takes adjectival occurrences of numerals to be determiners). So, we again have two kinds of sentence with related meanings, one of which features numerals in singular-term position, while the other features numerals in adjectival position. FOP rears its ugly head once more.

Hofweber's alternative semantics is also motivated by its ability to solve another puzzle concerning the way in which children learn arithmetic (2005: 196-198). Children start by being taught to memorise the natural number sequence up to, say, 20; then they are asked to count objects; then they are asked to master the idea of taking away and adding to a collection of objects, and so on; then, finally, children are expected to learn and manipulate arithmetical formulae. Interestingly, children seem to find this last move particularly difficult. This is suggested by the fact that it takes them considerably longer than any of the other stages. The puzzle Hofweber points to is this: why does this particular stage of arithmetical learning give children so much trouble?

Hofweber presents his alternative semantic analysis as the best solution to both these puzzles. The proposal is that children undergo a demanding cognitive process called *cognitive type coercion* when they make the step to doing arithmetic

with arithmetical formulae (2005: 198-202). The cognitive backdrop of this claim is more or less the same as the one I provided in chapter 5 (see §5.4). The idea is that sentences do not bear semantic relations directly to objects in the world, but rather do so indirectly by demanding that an agent entertain a certain concept in order to grasp the content of the expression. We might say that expressions *trigger* or *express* concepts. It is the expressed concept that ultimately bears representational relations to the world. On this view, statements of truth-conditions can be thought of as descriptions of mental representations and reveal the formal and semantic properties of these representations. Recall that the formal properties of a concept are its character, while the semantic properties are its content. This view portrays thinking as mental operations over the form or syntax of these mental representations or thoughts. Again, this view is an endorsement of something like Fodor's (1975) language of thought hypothesis.

Recall that what distinguishes directly referential expressions from general expressions is the fact that grasping the former requires an agent to entertain a singular concept, while grasping the latter requires an agent to entertain a general concept. Singular concepts have distinctive character which helps determine our patterns of reasoning with thoughts that involve them, and fully determines the kind of content the concept can have. The content of a singular concept is exhausted by the object to which corresponding expression purports to refer, or would be if the object were to exist.

Hofweber's proposal is that sometimes we can undergo a cognitive process whereby we change the character of one kind of concept to that of another kind, but without thereby changing its content. This is cognitive type coercion. We have already come across semantic type-shifting in this chapter. This is analogous, except

where semantic type-shifting involves not only a change in syntactic properties but also a shift in semantic value, cognitive type coercion is supposed to keep the semantic properties of the coerced concept intact while changing its syntactic properties. Hofweber claims that this can explain the above puzzles. Children find it hard to learn to manipulate arithmetical symbols because it involves forcing their concepts of cardinality to take on a different character, a process which is presumably quite demanding.

Why do we force our children to undergo this process? Because reasoning with singular concepts is easier than reasoning with general concepts. To support this idea, Hofweber appeals to the respective semantic types of the natural language expressions corresponding to singular and general concepts (2005: 199-200). Expressions in adjectival position make a more complicated contribution to truth-conditions than expressions in singular-term position, and are accordingly assigned a higher semantic type. On my view, numerals in adjectival position have type $\langle\langle e, t \rangle, \langle e, t \rangle\rangle$, while names have type e . If linguistic expressions express concepts, then the relative complexity of these expressions will be preserved in their mental counterparts. More complexity in the semantic contribution a concept makes to thoughts would then coincide with more complexity in the rules governing the inferences we can make with thoughts involving the concept. Though thought is supposed to operate only on the syntax of mental representations, when things go well, such inferences will match up to those warranted by semantic relations holding between mental representations. So, the more complicated the semantic contribution, the more complicated the inferential rules associated with the syntax of the representation.

To summarise, Hofweber thinks that in the process of learning pure arithmetical language children must force their concepts of cardinality, which start out as general concepts, to take on the character of singular concepts without thereby changing their content. This is presumably a difficult process, which explains why children take so long to learn to manipulate symbolic arithmetical sentences. However, it brings with it considerable cognitive rewards, such as the ability to reason more easily about cardinality, and this explains why we go through this process in the first place. This is also supposed to solve the particular instance of FOP presented above by yielding the following consequence. Though their syntactic forms differ, grasping the semantic content of arithmetical formulae containing numerals in adjectival position and arithmetical formulae containing numerals in singular-term position requires speakers to entertain mental representations with the same content. If this is right, it is no wonder that speakers use sentences (49a-d) interchangeably, since grasping their semantic content involves entertaining a thought with the same content.

Unfortunately, Hofweber's account of pure arithmetical language is problematic. There are three reasons why it is not a promising means of defending nominalism. First, by invoking the process of cognitive type coercion, Hofweber is positing some heavy duty cognitive machinery to solve two puzzles for which there are no doubt other less extravagant solutions (I will present one later in this section). Second, Hofweber provides no independent motivation for the claim that cognitive type coercion is possible, and as far as I know, none is provided elsewhere either. Third, the independently motivated philosophy of language I defended in chapter 5 appears to rule out Hofweber's account as a legitimate means of solving the puzzles he sets out to.

To motivate this last point, there are two relevant considerations. The first is that the character of a concept *determines* the kind of content it can have as well as partly determining the kind of inferences we can make with thoughts that involve the concept. The character of a singular concept determines that its content is exhausted by a single object, and by doing so helps to determine that, for instance, reasoning counterfactually with thoughts that involve this concept tracks the same individual across imagined possible situations. On Hofweber's proposal, however, the character of a concept can come completely free from its content, since we can think with a concept which has the character of a singular concept and the content of a general concept. Given the role that the character of a concept is supposed to play in thought, it is difficult to make sense of Hofweber's proposal. On the one hand, he must reject the claim that concept character determines concept content, since he claims that we can entertain a concept that has the character of a singular concept and the content of a general concept; but on the other hand he has to claim that concept character in some way determines concept content, since he has to maintain that concepts with general content ordinarily have a general character, and it is only through sustained effort that we can force a general concept into the shape of a singular concept. There is a clear tension here, and I have no idea how one might go about resolving it.

The second consideration is that, as I argued in chapter 5, since semantic meaning is determined entirely by lexical and syntactic properties of expressions, the only evidence to which semantic theorizing should be sensitive is syntactic data and type-level intuitions about meaning. In claiming that arithmetical formulae have the same semantic content as sentences that have different syntactic features (their being plural, for instance) is illegitimate in the absence of strong evidence suggesting that we intuitively ascribe the same semantic content to these sentences. However, one

datum Hofweber appeals to is, by these standards, illegitimate, while the datum he appeals to is insufficient. One datum is that people appear to use these sentences interchangeably. This datum is on the right lines, since it could be suggestive of the fact that people have a type-level intuition that the sentences mean exactly the same thing; however, as it stands, this datum is insufficient. After all, people might use these sentences interchangeably because, despite the fact that they mean different things, for most practical purposes, it doesn't matter which one they use. Hence, as it stands, this datum can equally support a pragmatic proposal about the ways in which we use mathematical sentences. The second datum Hofweber appeals to is distinctively sociological: children take a long time to learn how to use arithmetical formulae. Since this datum has nothing to do with the syntactic properties of sentences and our intuitions about what sentences mean, it is illegitimate to draw any conclusions from it about the syntax and semantic content of sentences or the character and content of the thoughts they express. In contrast, pragmatic theory can in principle be sensitive to data of any kind, so a legitimate alternative explanation might run schematically as follows: it is hard to learn to use arithmetical formulae because what we learn to do with them is difficult, given their syntactic and semantic properties.

The above discussion suggests that a pragmatic explanation of the data Hofweber presents would be more appropriate. Does this mean a pragmatic solution to the first instance of FOP, outlined in §6.1, might also be more appropriate? I stand by my own solution because the syntactic and semantic apparatus I appealed to was independently motivated, as argued in §2.2-§2.6. Here, however, there appears to be no independent reason for adopting a more complicated semantics. Since the data concerns the way in which people appear to use certain sentences and some

sociological observations about children's arithmetical education, it seems a pragmatic solution is far more appropriate.

Since this chapter is dedicated to the semantics of mathematical language, I will not present in detail a pragmatic theory of arithmetical language use here, saving that for chapter 8 (see in particular §8.4-§8.5). I will, however, describe the proposal in outline, just to demonstrate that the evidence Hofweber appeals to motivates such a pragmatic proposal.

The pragmatic solution I propose starts with the claim that pure arithmetical formulae and the plural arithmetical sentences to which they correspond mean different things. Pure arithmetical formulae express propositions concerning relations between specific objects (numbers), while plural arithmetical sentences express propositions concerning relations between non-specific collections of objects. Nevertheless, speakers typically use arithmetical formulae to communicate the kinds of propositions expressed by plural arithmetical sentences. For example, speakers typically use the sentence ' $2+2=4$ ' to communicate the proposition directly expressed by the sentence 'Two and two are four'. Hence, people are willing to use both sentences when articulating the formula ' $2+2=4$ '.

The sociological datum can also be elegantly explained on this view. Children are taught to use sentences that concern particular objects to communicate and draw inferences about collections. That is, they are taught to use sentences that say one thing to mean another. When teachers introduce children to arithmetical formulae, they introduce them to sentences that contain, and behave inferentially as though they contain, referring expressions that stand for numbers. They are taught to manipulate these formulae, and reason with them, in accordance with their semantic and syntactic profile. However, they are taught to do this as the final stage in a

learning process that has up until that point been dedicated to collections of non-mathematical objects. The difficulty is due to trying to establish the relevance of the meanings of arithmetical formulae to facts about collections of objects. Semantically speaking, there is no relevance, which explains the difficulty. The process by which children learn to draw the appropriate inferences from pure arithmetical formulae is just to learn by rote to associate certain arithmetical formulae with particular propositions concerning collections of objects. Learning in this way takes time and can be confusing.

Why take this step in learning to reason about cardinality and magnitude? I find very plausible Hofweber's claim that it is easier to reason using directly referential expressions and the singular concepts they express, and my own account can exploit this aspect of his view. The syntax and semantics of pure arithmetic is simple and user-friendly, and facilitates a fast and efficient means of getting to conclusions about cardinality and quantity. Suppose John wanted to work out how many sweets to bring for eight friends, assuming that they would want ten sweets each. The informal plural formulation of the required thought would be 'Ten and ten and ten and ten and ten and ten and ten and ten are...', or perhaps 'Eight lots of ten sweets are...', neither of which are particularly useful for the purposes of finding the desired solution. Contrast this with the formal counterpart ' $8 \times 10 = \dots$ ', which is convenient, and also presents a shortcut for working the problem out: adding '0' to the end of '8'. So, John lands on the solution, ' $8 \times 10 = 80$ '. The proposition this sentence expresses has nothing to do with sweets and friends, or any physical objects whatsoever, but when John learnt to manipulate arithmetical formulae he learnt to associate them with particular propositions about physical objects. He therefore internalised a heuristic that can be expressed as follows: ' $8 \times 10 = 80$ iff eight lots of

ten are eighty'. (I say 'heuristic' because I do not want to commit to the claim that John *believes* this rule.) From the right hand side, John quickly infers that eight lots of ten sweets are eighty and correctly decides that he needs eighty sweets for her friends. Though associating seemingly irrelevant arithmetical formulae with the right plural propositions was difficult, it has clearly paid off, since John can now solve problems about cardinality and magnitude with relative ease.

To summarise, the pragmatic proposal I offer is that in the process of learning arithmetic, part of what we learn to do is use a sentence that means one thing to communicate another. So, when we utter ' $2 + 2 = 4$ ' in an applied context, what we typically mean to communicate is that two and two are four. If this is right, it is no wonder people read aloud (48) in the different ways shown in (49a-d), since the proposition they communicate by uttering all of these sentences is identical, though the sentences they use mean different things. This solution explains the data Hofweber has presented but without positing the implausible cognitive machinery of cognitive type coercion, and without altering our semantic theory on the basis of illegitimate and insufficient evidence. However, the spirit of Hofweber's proposal is retained in my reliance on the claim that using pure arithmetical formulae is useful because reasoning with singular concepts and directly referring expressions is significantly easier than reasoning with general concepts and general expressions. Though the sentences of pure arithmetic do contain expressions which purport to refer to mathematical objects, it seems that there is motivation for a nominalist friendly pragmatic theory of their use.

6.8 Conclusions

Recall the arguments presented in the introduction to this chapter (§6.1):

EA

EA1: The following sentence is true:

(1a) The number of planets in the solar system is eight.

EA2: If (1a) is true, then the number eight exists.

EA3: Therefore, there are numbers.

MA

MA1: We should believe the measurement claims made by well-confirmed scientific theories for instance, astronomy's claim:

(1b) The mass of Jupiter in kilograms is 1.8986×10^{27} .

MA2: If these measurement claims are true, then there are abstract mathematical entities.

MA3: So we should believe that there are abstract mathematical entities.

In this chapter, I have shown that the kinds of sentences appealed to by these arguments do not contain any expressions that purport to stand for numbers. The Fregean analysis is false, so both arguments fail. However, I showed that pure mathematical language does purport to be about mathematical objects, so might the platonist reformulate these arguments so that they appeal to pure mathematical claims?

With respect to MA, the resulting argument would be far less appealing. This is because MA only works if the sentences appealed to are ‘well-confirmed’ by science. While it is far more plausible to claim that our applied arithmetical sentences are well-confirmed, the same claim for pure arithmetical sentences is problematic. The reason is that it is not obvious that pure mathematical claims can be empirically supported. To appeal to well-confirmed pure mathematical language, the platonist must invoke and motivate to some form of confirmational holism or appeal to the claim that such sentences play the same role in science as our other well confirmed sentences, such as those that concern unobservable concreta. However, then MA would begin to resemble either IA, or other weaker forms of the indispensability argument (see §1.2 for discussion), and so inherit the weaknesses of these arguments made clear in Part I of this thesis.

On the other hand, EA does not suffer from the suggested alteration. The improved argument reads:

EA*

EA*1: The following sentence is true:

(48) $2+2=4$

EA*2: If (1a) is true, then the number four exists.

EA*3: Therefore, there are numbers.

No one will deny that sentences such as (48) strike us as true. Moreover, as saw in §6.7, such sentences do contain expressions that stand for numbers.

The upshot of all this is that applied arithmetical language poses no problem for the nominalist, while *prima facie* pure arithmetical language does. The nominalist will therefore have to engage with EA*. To do so, she will have to explain why, if (48) is false, it nevertheless strikes us as true. In §6.7, I considered evidence presented by Hofweber suggesting that speakers use pure singular arithmetical sentences and plural arithmetical sentences that do not concern numbers interchangeably when articulating arithmetical formulae. If speakers typically use arithmetical formulae to communicate the propositions expressed by these plural sentences, then this behaviour is unsurprising. I argued that this provides some motivation for thinking that speakers do use arithmetical formulae to express propositions that do not concern abstract numbers. This suggests a promising means of replying to EA*: claim that, though it is false, (48) strikes us as true because the non-mathematical proposition we typically use (48) to communicate is true.

At this point, it becomes crucial for the nominalist to meet the communication challenge and the content challenge, introduced in §2.4. The former says that the nominalist must provide an account of how pure arithmetical sentences are used to communicate propositions that do not concern numbers. The latter says that the nominalist must provide a characterisation of the content expressed by such uses.

7. The Pragmatics of Mathematics (I): Figuring Yablo

7.1 Introduction

In this chapter, I present and assess Yablo's two attempts at providing a pragmatic theory of mathematical language use. In his two accounts, he addresses directly the content and communication challenge. The first attempt he calls *figuralism* (2000; 2001; 2002; 2005), since it involves the claim that mathematical discourse is metaphorical. The second attempt he calls *presuppositionalism* (2006; 2009; 2012), since it involves the thesis that mathematical entailments of sentences are presupposed when those sentences are uttered.

In assessing these two positions, I will hold Yablo to account to the communication challenge and the relevant version of the content challenge, outlined in §2.4. (I will only consider pure mathematical language because, as I showed in the previous chapter, applied arithmetical language poses no problem for nominalism.) Before doing this, I should say more about what is involved in meeting these challenges. Meeting the communication challenge might be expressed as follows:

The communication challenge: (i) provide an independently motivated account of the mechanism by which mathematical language is used to communicate non-mathematical content; (ii) explain the relationship between the intuitions we have about mathematical language use and what the account says about mathematical language use.

For (i) to be met, the proposed account of mathematical communication must be motivated by considerations other than pursuing nominalism. For (ii) to be met, the

relationship between the theory and our intuitions must be explained. For example, if an account of mathematical communication implies that speakers typically engage in non-standard speech acts when uttering mathematical sentences, then it would have to explain why speakers are unaware that they are engaging in non-standard behaviour. The content challenge might be expressed as follows:

The content challenge: (i) provide an independently motivated characterisation of the intended content of mathematical language use (not to be understood as the demand that this content be explicitly stated across the board); (ii) explain the relationship between our intuitions concerning the content of mathematical utterances, and the content assigned to mathematical utterances by the theory.

In this chapter, I will argue that both figuralism and presuppositionalism fail to meet the communication challenge. In §7.2, I argue that the central claim of figuralism, that mathematical discourse is metaphorical, is *prima facie* implausible and makes meeting part (ii) of the communication challenge difficult. Moreover, I show that half of the data that Yablo appeals to in support of figuralism does not support a non-literal interpretation of mathematical discourse, let alone the claim that it is metaphorical. In §7.3, I show that the other half of the data Yablo appeals to does support a non-literal interpretation of mathematical discourse, though it still falls short of supporting the claim that it is metaphorical. In §7.4, I consider Yablo's account of the mechanism of communication independently of the claim that mathematics is metaphorical, and show that it fails to explain how purely non-mathematical content might be communicated by uttering mathematical sentences.

In §7.5, I show that presuppositionalism also fails to meet the communication challenge. It fails to meet part (i) because the independent motivation Yablo provides for the view fails to establish it. It fails to meet part (ii) because it is incompatible with the aforementioned evidence in favour of a non-literal interpretation of mathematical discourse.

Finally, in §7.6, I show that the account Yablo offers of the content of mathematical utterances successfully meets the content challenge. In conclusion, I outline what is required for a successful non-literal pragmatic account of mathematics. In the final chapter, I take steps towards meeting these requirements.

7.2 Mathematics as metaphor

When speaking metaphorically, we do not mean to convey the proposition expressed by the sentence we utter. Instead, we mean to communicate some other proposition that is often difficult to otherwise communicate. For instance, in uttering assertorically ‘Steam was blowing from Mary’s ears!’, I do not intend to report an unusual biological event. Instead, I communicate something that might be more directly conveyed with ‘Mary was very angry’; but the literal counterpart does little justice to just how angry Mary would have to be for the metaphor to be apt. We might therefore say this metaphor is indispensable to my communicative intentions in this case.

Yablo claims that we use mathematical language in this way (2000: 160, 2001: 191-3, 2002: 208-9, 2005: 232), so that when we use the sentences ‘ $3+3=6$ ’, we are not intending to describe mathematical objects, but instead trying to communicate something about the non-mathematical world that would otherwise be hard or even impossible to get across.

Even if this similarity between mathematical and metaphorical language holds, it does not immediately follow that mathematical language is metaphorical. We have seen that cases where speakers use a sentence that says one thing to mean another are not hard to find (see §5.2). Moreover, the claim that mathematical language is metaphorical is *prima facie* implausible. Speaking metaphorically plausibly involves performing a special kind of speech act, and it seems that performing such a speech act at least requires the speaker to be aware that she is doing so. Yet we should expect practitioners to deny that they are pretending to do anything when they utter mathematical sentences.

However, sometimes counter-intuitive theses turn out true, and Yablo provides an argument for his: figuralism is presented as the best explanation of a list of similarities between metaphorical and mathematical discourse. Unfortunately, the argument is weak. In this section, I will outline roughly half of the data Yablo invokes for his argument, and show that it provides no support for a non-literal account of mathematical discourse, let alone the claim that it is metaphorical. However, we shall see in §7.3 that the second half of the data Yablo appeals to does support a non-literal interpretation of mathematical discourse, though it still falls short of supporting figuralism.

Below is roughly half of the list of data Yablo appeals to. (It is based loosely on the list found in Yablo 2000: 172-175; I have changed the terminology for clarity and some of the examples for relevance.)

Indeterminacy

Metaphorical objects/mathematical objects can be ‘indeterminately identical’.

There is no fact of the matter as to the identity relations between the fuse I

blew last week and the one I blew today, and there is no fact of the matter as to the identity relations between the natural numbers and the various set-theoretic conceptions of them.

Insubstantiality

Metaphorical objects/mathematical objects tend to have not much more to them than what follows from our conception of them. For example, the green eyed monster has no ‘hidden substantial nature’ and all the really important facts about the numbers follow from the axioms of second-order arithmetic.

Expressiveness

Metaphorical objects/mathematical objects tend to boost the language’s power to express facts about other, more ordinary entities: ‘The average taxpayer saves more than the average home-owner’; ‘The area of a circle is π times the square of its radius’.

Disconnectedness

Metaphorical objects/mathematical objects have a tendency not to do much *other* than expressive work; they tend not to push things around.

Irrelevance

Metaphorical objects/mathematical objects are called in to explain phenomena that would not on reflection suffer by their absence: ‘Ben hates his boss because he’s lost patience with all this paper work.’ If we remove Ben’s reserve of patience, then Ben would still be angry with his boss;

similarly, if we annihilate all the bijective functions, there would still be as many left shoes in my closet as right.

Availability

Metaphorical objects'/mathematical objects' lack of naturalistic connections might seem to threaten epistemic access until we remember that 'their properties' are projected rather than detected.

These data are difficult to interpret. It is unclear whether the 'facts' appealed to are supposed to be metaphysical, sociological or semantic in nature. Unfortunately, however they are interpreted, they do not support a non-literal interpretation of mathematical discourse.

Indeterminacy says that, just as with certain metaphorical objects, there is no fact of the matter about the identity of certain mathematical objects. This seems to be a metaphysical datum: an appeal to the nature of the nature (or lack thereof) of mathematical objects. Yablo appeals to the familiar example that there is more than one adequate set-theoretic reduction of the natural numbers (first introduced to the philosophical literature by Benacerraf 1965). However, the fact that there is more than one adequate candidate does not entail that there is no fact of the matter. Yablo's insistence that there is no fact of the matter appears question-begging. Moreover, even if Yablo were right, it would not lend any support to a non-literal interpretation of mathematical discourse. Figuralism concerns how mathematical sentences are put forward, not what mathematical objects are like. Suppose that there are no mathematical objects and so there is no fact of the matter about whether the

natural numbers are sets. This in no way precludes speakers from wrongly believing that there are mathematical objects, and even that there is a fact of the matter about whether numbers are sets, and so speaking of them literally. This metaphysical datum does not bear on the question of whether mathematical language is literal.

There are two further interpretations of *Indeterminacy*. They are just as problematic as the first. One interpretation is to take it as a sociological datum, concerning what speakers believe about the entities about which they appear to be speaking. This would indeed be relevant to the question of whether mathematical language is metaphorical. It would be strange for people who believe they are engaged in the practice of literally describing numbers to simultaneously believe that there is no fact of the matter as to whether numbers are identical to some sequence of sets. However, just as with Melia's §2.3 claim about the beliefs of scientists, we have a right to ask how Yablo knows that practitioners believe there is no fact of the matter concerning the set-theoretic reduction of numbers? It bears repeating that unsubstantiated sociological claims should not be appealed to as data.

The final interpretation is that the datum is semantic. On this view, *Indeterminacy* says that, *for all mathematical language says*, there is no fact of the matter concerning whether numbers are sets. On this interpretation, *Indeterminacy* seems right. There is no mathematical proof that shows which sequence the natural numbers are identical to; nor any that shows that they are not identical to any of them. But why should this fact preclude speakers from believing that there is a fact of the matter here? Speakers might believe that natural numbers are not sets. Why should the fact that our mathematical theories do not explicitly decide the matter be relevant to the way in which mathematical sentences are put forward by speakers? The semantic interpretation of *Indeterminacy* renders it irrelevant.

Insubstantiality says that, just like metaphorical objects, there tends to be nothing more to the nature of mathematical objects than what flows from our conception of them. On a metaphysical interpretation, this datum is irrelevant. Perhaps the axioms of second-order arithmetic do exhaust the nature of numbers. This does not stop speakers from either believing that numbers do have more to them than our best theory captures, or believing that numbers are exactly how the axioms describe them but believing that they exist nonetheless, and so intending their mathematical utterances to be taken literally. The sociological interpretation is irrelevant, too. Speakers may believe that there is more to numbers because they believe that the axioms of second-order arithmetic are literally and exhaustively true with respect to numbers.

Expressiveness says that, just like talk about metaphorical objects, talking about mathematical objects allows us to say more about physical objects. This tells us nothing about how mathematical utterances are put forward. By describing the objects in her bedroom, I may be able to communicate more things about my friend's personality than I would be able to directly. I may also believe that describing these objects has this effect. This in no way suggests that I'm speaking metaphorically.

Perhaps *Expressiveness* is only supposed to get its bite when combined with *Disconnectedness*, which says that, just like talk of metaphorical objects, talk of mathematical objects do nothing *more* than expressive work. As a metaphysical datum this is irrelevant. Liggins (2014) shows how a literal interpretation of mathematical language use can be combined with an abstract expressionist account of why mathematical language is useful, naming the resulting view *belief expressionism*. On this view, speakers believe in the literal content of the bridge sentences connecting up mathematical claims to claims about the physical world,

such as ‘There is a bijective function from my right shoes to my left if and only if I have as many right shoes as left’. By making use of such bridge principles, speakers can express more about physical objects. Suppose that abstract expressionism is right, and suppose that numbers do not exist. Why should this alone preclude the possibility that speakers intend their uses of these sentences to be taken literally? As a sociological claim, *Disconnectedness* should not be taken seriously unless evidence is provided showing that speakers are abstract expressionists. There doesn’t appear to be a semantic interpretation of either *Expressiveness* or *Disconnectedness*.

Irrelevance seems to be a combination of a sociological claim and a metaphysical claim. The former is that practitioners use talk of mathematical objects to explain physical phenomena. The latter is that mathematical objects are irrelevant to the phenomena to be explained. We saw in Chapter 4 that both of these claims are true. However, this in no way suggests that practitioners are speaking metaphorically when they use mathematics; they may be mistaken about what kind of relevance mathematical language has to the physical world. This datum fares no better if it is interpreted as a sociological claim: how do we know what scientists think with respect to the role mathematical objects play in their explanations? The semantic interpretation of *Irrelevance* says that scientists talk about mathematical objects in the course of providing explanations in which, for all scientific language says, the mathematical objects are not relevant to the production of the explanandum. This does not seem obvious enough to me to be relied upon as a datum. More importantly, even if it is right, there are two possibilities that are perfectly compatible with a literal interpretation: first, practitioners wrongly believe that mathematical objects are explanatorily relevant to physical phenomena; second, practitioners do not

believe they are relevant, but think there is still some value in truly describing these entities.

The final datum in this list, *Availability*, suffers similar drawbacks. Interpreted metaphysically, it says that mathematical objects, if they exist, lack any naturalistic connections to the rest of the world. If by ‘naturalistic connections’ Yablo means causal and spatial relations, then this is certainly true, or at least it can be assumed true in the present debate. But again, what this tells us about the spirit with which practitioners utter mathematical sentences is unclear. What relations mathematical objects can or cannot stand in seems to have nothing to do with the spirit in which practitioners talk about them. Perhaps practitioners are mistaken about the nature of mathematical objects and assume they have naturalistic connections to the world; perhaps they see some other value in describing them. Either option is compatible with practitioners speaking literally when they utter mathematical sentences.

I cannot imagine what a semantic interpretation of this datum would be, or what its relevance would be. However, there is a sociological one, and it appears to fare better than the metaphysical one. The thought might be that, if scientist’s apparent assertions about mathematical objects are to be taken at face value, it is puzzling that scientists do not say anything about the relations these objects stand in in virtue of which scientists know about them. If mathematical language were metaphorical, however, we would expect scientists not to get side-tracked by such issues. It is certainly true that scientists do not discuss any naturalistic connections mathematical objects may stand in. In the introduction to this thesis, I provided a relevant quote from Shapiro:

The scientific literature contains no reference to the location of numbers or to their causal efficacy in natural phenomenon or how one could go about creating or destroying a number. There is no mention of experiments to detect the presence of number or determine their mathematical properties. Such talk would be patently absurd. (2000: 27)

Though I think this provides motivation for thinking that scientists have an attitude towards mathematical language that is different from their attitude towards talk of unobservable concreta, I do not think it singles out figuralism as the best way of spelling this out. For example, perhaps scientists view the investigation of mathematical knowledge as the domain of mathematics, and so leave it aside when they pursue physics or biology, etc. Perhaps once all the data is considered, the sociological interpretation of *Availability* will contribute.

7.3 Mathematics as non-literal

Most of the above data are at worst question-begging, and at best irrelevant to the conclusion Yablo seeks to draw. However, we have only considered one half of Yablo's list of data. The second half concerns the phenomenology of mathematical language use (cf. Liggins 2010: 767), and is difficult to accommodate on a literal interpretation of mathematical discourse. Though it doesn't go far enough to support Figuralism, I will argue that it does lend support the view that mathematical discourse is non-literal in some sense. (Again, the following is based loosely on the list found in Yablo 2000: 172-175.)

Paraphrasability

Metaphorical objects/mathematical objects are often paraphrasable away with no felt loss of subject matter. ‘That was her first encounter with the green-eyed monster’ can be paraphrased ‘That was her first time feeling jealous’; ‘There is a bijective function from the left shoes in my wardrobe to the right’ can be paraphrased as ‘There are as many left shoes in my wardrobe as there are right’.

Impatience

One is impatient with the meddling philosopher who worries about whether metaphorical objects/mathematical objects exist. ‘Well, say people *do* store up patience in internal reservoirs; then *my* patience is nearly exhausted’; ‘Say there *are* models; then *this* argument has a counter-model’.

Silliness

Metaphorical objects/mathematical objects invite ‘silly questions’ probing areas the metaphor/theory does not address: ‘You say you lost your nerve; has it been turned in?’; ‘What are the intrinsic properties of the empty set?’.

Reluctance

We are reluctant to infer that metaphorical objects/mathematical objects exist, though assertions made using sentences about them strike us as true: we are reluctant to infer that green eyed monsters exist from an utterance of ‘She had a visitation from the green-eyed monster’; similarly, we are reluctant to infer that functions exist from an assertion made with ‘There is a

bijjective function from the left shoes in my wardrobe to the right shoes in my wardrobe’.

Translucency

It is hard to hear an utterance of ‘What if there is no green-eyed monster?’ as communicating what the sentence literally expresses: one ‘sees through’ to the (bizarre) suggestion that no one is ever truly jealous. It is hard to hear an utterance of ‘What if there are no countermodels?’ as meaning what the sentence literally expresses; one ‘sees through’ to the (bizarre) suggestion that no arguments are invalid.

These data are easier to interpret: they all clearly concern some phenomenological aspect of mathematical language use. I will now show that, though each datum may individually be accommodated by a literal interpretation of mathematical discourse, accounting for all of them in this way will involve making some strong commitments. A non-literal interpretation has an easier time explaining the data, but this by no means takes us as far as the claim that mathematics is metaphorical.

Paraphrasability points to the fact that many sentences containing expressions that purport to stand for mathematical objects have adequate paraphrases that do not contain such expressions. Yablo takes this to show that when we use these mathematical sentences, we are not really talking about the mathematical objects. However, this reasoning can be reversed. The feeling that ‘There is a bijjective function from the left shoes in my wardrobe to the right’ means the same as ‘There are as many left shoes in my wardrobe as there are right’ could be down to the fact that we take an utterance of the latter to be about a function as well as an

utterance of the former. (For a classic discussion, see Alston 1958; see also Hofweber 2007 for more on ordinary sentences and their ‘metaphysically loaded counterparts’.) The commitment is clearly left implicit, but since those who use mathematical language literally will presumably believe that mathematical objects exist necessarily, they will believe that wherever there are two groups of objects with as many objects in each group, there is a bijective function that witnesses this (see Felka 2014: 28 for a proposal in this vein).

Impatience highlights the fact that, if someone were to object to our use of mathematical language on the grounds that mathematical objects do not exist, we would grow impatient. The feeling would be that the objector is missing the point. After all, by uttering ‘This argument has a counter-model’, we are not really meaning to say anything about models and whether or not they exist; we’re just trying to say that the relevant argument is invalid. This is closely related to the datum of *Silliness*, which says that there are certain questions about the mathematical objects we appear to be mentioning that strike us as silly. Asking after the intrinsic properties of the counter-model we mentioned strikes us as silly because our reasons for invoking the counter-model are exhausted by what it tells us about the argument we are considering. Asking after its intrinsic properties is again to miss the point.

Impatience can be accounted for on a literal interpretation by again appealing to the beliefs of the speaker. For example, if belief in mathematical objects is foundational to the project we are engaged in, then we might respond to anyone questioning those beliefs with impatience. A practicing physicist who takes all the mathematics she uses to be true of mind-independent mathematical objects would consider the question of whether such things exist as an orthogonal line of inquiry—a philosophical one—to the project she is engaged in and so she may just not be

interested. Or, if speakers typically consider the existence of mathematical objects to be obvious, then we would expect them to meet anyone questioning their existence with impatience. This move allows someone who endorses a literal interpretation of mathematical discourse to account for *Silliness*, too. Perhaps most people just think it is obvious that models and sets don't have any intrinsic properties, and it strikes them as silly to ask about such things.

Reluctance points to the supposed fact that we are more than happy to endorse certain mathematical sentences as being true, but less happy to endorse some of their immediate entailments as true. For example, we would have no problem with taking 'The number four is even'. Yet the immediate entailment 'Numbers exist' feels less innocuous and so harder to endorse. Stanley (2001: 50) states that the data Yablo presents in support of figuralism will only be compelling to those who are sympathetic to nominalism. He is certainly right about the metaphysical interpretations of the data presented in the previous section. Concerning the data presented in this section, however, I think he is mistaken. But Stanley's point seems particularly forceful when it comes to *Reluctance*. I certainly feel the reluctance Yablo is talking about, but I am all too aware that this could be down to my nominalistic tendencies. However, I still think Stanley is mistaken here. Those who think the question of whether mathematical objects exist is a substantial one, but haven't made up their mind on which side of the debate they sit, shouldn't want to *immediately* endorse the existence claims entailed by the mathematical sentences we typically use and endorse. As with *Silliness* and *Impatience*, the source of this feeling seems to be that familiar mathematical claims are not supposed to address the question of whether mathematical objects exist, while endorsing explicit existence claims concerning mathematical objects feels like taking a stance on this question.

In light of this discussion, it seems that the only way in which *Reluctance* can be accounted for by a literal interpretation of mathematical discourse is to deny it. This involves denying that speakers would feel any reluctance to endorse the existence claims entailed by our mathematical sentences. This means one of two things. First, speakers take the question of whether mathematical objects exist to be substantial, but are platonists. Second, speakers do not consider the question of whether mathematical objects exist to be a substantive one. Either way, this looks like attributing sophisticated philosophical positions to speakers.

Translucency is a strong piece of evidence and Yablo's example is a powerful one. It seems to me that there are two ways to interpret utterances of sentences such as 'There are no counter-models' and 'There are no bijective functions'. The easiest interpretation to come across seems to be one according to which the speaker means to say something about non-mathematical things alone: that no arguments are invalid, that there are no equinumerous groups of objects. The second interpretation, which is harder to come by, is one according to which the speaker is intending to communicate something about mathematical objects: that counter-models do not exist, and that bijective functions do not exist. Moreover, uncovering the second interpretation in each case has the same feeling as uncovering the literal content of a well-worn figure of speech. Suppose John is hosting a ping pong tournament, and he's doing particularly well. If someone were to utter 'John is cleaning up!' there are again two interpretations. One has the speaker talking exclusively about John's progress in the tournament—that he is consistently winning—while the other, which is harder to get at, has the speaker talking exclusively about John's domestic activities—that he's cleaning up some mess or other. Moving from the easier interpretation to the harder interpretation has a very

similar feel in each case. Again, the explanation figuralism offers of this is that speakers typically use mathematical language to convey content about non-mathematical things.

Burgess and Rosen explicitly discuss how a literal interpretation of mathematical discourse might accommodate *Reluctance* (2005: 531-2). They suggest that, on hearing an utterance of ‘There is no counter-model for this argument’, given enough questioning, the listener might be prompted to agree that the statement is about models and not just arguments. But this is exactly what we would expect if this sentence were being used non-literally. As the ping pong example above shows, the literal content of an utterance is recoverable, given the appropriate prompting. It is the fact that prompting is needed in the first place that needs explaining. If the content that concerns numbers really were a part of the content the speaker intended to communicate, it would be very strange if speakers required prompting to become aware of this. This would mean that, when speakers use such mathematical language, they are typically unaware of the content of their own assertions. It seems to me that speakers should at least be aware of the content they intend to communicate. The examples of well-worn figures of speech show that they need not be fully aware of the content of the sentences they use to communicate this content.

In light of this, it seems the only way to accommodate *Translucence* on a literal interpretation of mathematical discourse is to deny its status as a datum. This involves claiming that speakers are immediately aware of the literal content of their mathematical utterances.

Let us take stock. We saw that most of the evidence presented in the previous section is not worth taking seriously. In contrast, the evidence presented in this section is far more compelling. We have seen that there is room for the proponent of

a literal interpretation of mathematical discourse to reply to each datum individually. However, if she is to accommodate all of the data presented in this section, the proponent of a literal interpretation has to endorse a collection of theses concerning the beliefs of the participants in mathematical discourse that, collectively, seem implausible. The most straightforward account of *Paraphrasability* on a literal interpretation involves attributing to speakers not only the belief that mathematical objects exist, but also the belief that mathematical objects exist necessarily. Accounting for both *Impatience* and *Silliness* on a literal interpretation involves attributing to speakers the belief that the existence of mathematical objects is obvious. Finally, accounting for *Reluctance* and *Translucency* on a literal interpretation involves attributing to speakers a willingness to take a philosophical stance on the question of whether or not mathematical objects exist, or a meta-philosophical stance on whether or not the question is a substantive one.

Taken together, the phenomenological data presented above reveal that endorsing a literal interpretation of mathematical discourse involves endorsing an implausible collection of sociological claims about the beliefs of the participants in mathematical discourse. These data present a real problem for a literal interpretation of mathematical discourse.

In contrast, figuralism makes easy work of the data because it involves the claim that typical utterances of mathematical sentences are not intended to communicate the literal content of the sentence used. But figuralism makes the stronger claim that mathematical discourse is also *metaphorical*. Metaphorical utterances involve communicating something other than the literal content of the sentence used *and* they plausibly involve performing a special kind of speech act. This second claim plays no role in explaining the above data, so the data does not

support it. In §5.2-§5.5, I defended a view of the distinction between pragmatics and semantics according to which speakers can use a sentence that says one thing to mean another without thereby performing any special kind of speech act. I suggested that, far from being a special occurrence, much of our everyday talk is like this. If we hold on to the meaning of ‘literal’ according to which the literal content of an utterance is just the semantic content of the sentence used to make the utterance, then my view implies that much of our discourse is non-literal. In light of this, it is not implausible to think that mathematical discourse is also non-literal. Moreover, if I am right and the data presented in this section demands to be taken seriously, then this view of mathematical discourse fares better than a literal interpretation when it comes to explaining the phenomenology of mathematical language use.

In conclusion, Yablo has failed to establish figuralism. The view is *prima facie* implausible because it involves the claim that participants in mathematical discourse unknowingly but systematically engage in non-standard speech acts. Moreover, the argument Yablo offers in favour of figuralism fails to establish the intended conclusion. It does, however, lend considerable support to the weaker claim that mathematical discourse is non-literal, in the sense just specified. For this account to be workable, we still have to account for the mechanism by which speakers can use a sentence that says one thing to mean another.

7.4 Mathematics as make-believe

In an attempt to account for mathematical communication, Yablo (2001; 2002; 2005) appeals to Kendall Walton’s (1993) make-believe account of metaphor. Yablo fails to establish that mathematical discourse is metaphorical, but the weaker non-literal interpretation of mathematical discourse I endorse might also appeal to Walton’s

apparatus to meet the communication challenge. In this section, I will outline Walton's account, and show that it is not up to the task.

Games of make-believe are constituted by rules that connect descriptions of actual states of affairs with descriptions of imaginary states of affairs; the former's obtaining makes the latter *true according to game*. (Yablo uses *sayable* when it is true according to the make-believe.) For example, in a game of cops and robbers, my having a stick in my pocket makes it sayable that I have a gun in my holster. The appropriate rule of generation is '*I have a stick in my pocket* if and only if according to the game I have a gun in my pocket', where '*S*' means 'S is sayable'.

Importantly, the inferences we make with principles of generation can go both ways. *Content oriented make-believe* is the describing or bringing about of actual states of affairs in order to communicate something about the content of a game (Walton 1993: 39). Pulling the stick out of my pocket, pointing it at my friend, and jerking it back makes it clear that in the game I have drawn my weapon and shot whoever my friend is supposed to be. In contrast, *prop-oriented make believe* is describing imaginary states of affairs in order to communicate that certain actual states of affairs obtain (Walton 1993: 39). Suppose we are supposed to go indoors in reality when we have been killed in the game. My friend asks me where Bill is, and I know he wants real-life facts because in the game Bill's name is 'Marlowe'. I say: 'He's dead', and though this is a fact about the content of the game, my friend can immediately infer that Bill has gone indoors. The prop-oriented content, that Bill is indoors, is what makes 'He's dead' sayable in this case.

Walton (1993) suggests that metaphor might be understood as a species of prop-oriented make-believe. Suppose I utter 'Jim is on fire tonight!' meaning that

Jim is doing very well. According to Walton, in uttering this sentence, I am indicating a certain make-believe game partly constituted by the following principle of generation: ‘*Someone is on fire* iff they are doing very well’.

Yablo attempts to explain how we use mathematical sentences to communicate non-mathematical content in the same way. He claims that applied mathematics is an instance of prop-oriented make-believe:

...numbers as they figure in applied mathematics are creatures of existential metaphor. They are part of a realm that we play along with because the pretense affords a desirable – sometimes irreplaceable – mode of access to certain real-world conditions, viz. the conditions that make a pretense like that appropriate in the relevant game. Much as we make as if, e.g., people have associated with them stores of something called ‘luck’, so as to be able to describe some of them metaphorically as individuals whose luck is ‘running out’, we make as if pluralities have associated with them things called ‘numbers’, so as to be able to express an (otherwise hard to express because) infinitely disjunctive fact about relative cardinalities like so: The number of Fs is divisible by the number of Gs. (2005: 232)

How might this account be extended to pure mathematical language? We shall see in §7.6 that Yablo takes the non-mathematical contents of pure arithmetical utterances to be logical truths concerning cardinalities of objects. The make-believe game tells us that there are objects which measure cardinality. Hence, by pretending to assert ‘ $3+3=6$ ’, given the rules of the game, we can communicate the fact that combining two distinct groups of three distinct objects gives us a group of six distinct objects.

(Recall that this is the content I suggested pure arithmetical utterances communicate in §6.7.) A similar story can be told about set-theoretical statements, where the make-believe game tells us that, for every collection of entities, there is another distinct entity, namely the set of those entities. By pretending to assert certain sentences of set theory, we can communicate combinatorial facts about objects via the appropriate principles of generation.

However, some pure mathematical sentences cannot plausibly be used to communicate anything about non-mathematical objects. What does ‘There are infinitely many prime numbers?’ tell us about non-mathematical objects? On Yablo’s view, such sentences are used to describe ways we should conceive of mathematical objects. Yablo claims that utterances of such sentences are content-oriented make-believe because they are primarily about the content of the make-believe game of mathematics (2005: 232). With respect to such pure mathematical sentences, mathematical objects appear to be playing a dual role. ‘The number of primes’ appears to mention an element of the make-believe, while the ‘ \aleph_0 ’ is invoked to describe the mentioned part of the make-believe. Yablo says utterances of pure mathematical sentences ‘are to be understood as prop-oriented make-believe, with numbers etc. serving both as props and as representational aids’ (2005: 234; again, see §7.6 for more on Yablo’s proposal; I make a slightly different proposal about such mathematical utterances in §8.5.)

Here, a familiar objection arises. With metaphor, the claim that we make-believe that certain facts obtain to communicate something is easier to swallow, since with many metaphorical sentences, we are aware that we are using language in a special way. But it is *prima facie* plausible that we are pretending that certain mathematical facts are true in order to communicate other things. In order to pretend

or make-believe, it seems we must at least be aware that we are doing so. Yablo responds to this as follows:

Making believe is an amalgam of (i) being as if you believe, and (ii) being that way through your deliberate efforts. It is only (i) that the figuralist needs. Call it simulation. Someone is simulating belief that S if although things are in relevant respects as if they believed that S, when they reflect on the matter they find that they do not believe it; or at least are agnostic on the matter; or at least do not feel the propriety of their stance to depend on their belief that S if they have one. They do not believe that S except possibly per accidens. (2001: 90)

Clearly Yablo is using ‘make-believe’ in a very broad sense, covering cases where we appear to be consciously pretending that something is true, all the way to cases where we merely unconsciously simulate the belief that it is, where to simulate something is to merely behave linguistically as though one believes it. (Walton 1997 also claims that we could engage in make-believe without being consciously aware of it; he also uses the term ‘simulation’, but his use bears little resemblance to Yablo’s, so I have omitted discussion of it.) Appearing to assert mathematical sentences gives the appearance that the speaker believes in the literal content of those sentences. However, if the speaker is just simulating belief in the literal content, this appearance does not go deeper than this linguistic behaviour, and on reflection the speaker will find that she either does not believe in the literal content or is indifferent to it. So long as mathematical discourse is a case of simulating

belief, as opposed to consciously engaging in make-believe, then Yablo can avoid the above objection.

This notion of simulation can also help block a second objection to the make-believe account. According to the view, when I utter ‘No man is an island’, I use the sentence to invoke a game or collection of principles, one of which links the sentence with a description of some real state of affairs. The content communicated is that people are not isolated from one another. Getting to this content involves recognising two things: first, that the literal content is not what the speaker intends to communicate; second, recognising the right game, by recognising the principles of generation which constitute it. The first of these initially poses a problem. It is unclear how the speaker knows that the literal content is not being asserted. Catherine Wearing calls this the problem of ‘triggering the pretense’ (2012: 510-1).

To respond to this objection, Yablo could draw on the view I defended in §5.2-§5.5, according to which many of our ordinary utterances are non-literal in the sense that they are not aimed at the communication of the semantic content of the sentence used—a position he clearly endorses (2001: 85). This response is a good one. When combined with the make-believe account, it may at first appear far-fetched because it implies that communication in general involves make-believe, but so long as we take most cases of make-believe to be mere simulation, the view retains its plausibility. The problem of triggering the pretense can be solved by appeal to simulation.

However, there is another more serious objection. Recall the second thing that the make-believe account demands of the listener: that they be able to grasp the right game of make-believe. The problem is that it is unclear how this happens. There must be countless games that make ‘No man is an island’ sayable, and Walton

and Yablo are both silent on how it is we ensure the listener identifies the right one. The only plausible answer is that the circumstance in which the sentence is uttered makes it clear which principle of which game is being appealed to. If the actual state of affairs that makes it appropriate to utter ‘No man is an island’ is contextually salient, then the listener can grasp the content expressed by both sides of the principle of generation and thereby identify the right game. But, as Wearing neatly puts it: ‘the hearer cannot accomplish this task unless she already understands the metaphor’ (2012: 515). Picking out the right make-believe game involves knowing which principles of generation constitute it, and knowing which principles of generation are being indicated involves knowing what actual states of affairs are mentioned by them. In other words, the communicated content must already be known. Invoking principles of generation alone cannot explain how a speaker can use a sentence that says one thing to mean another.

Let us finish the discussion of figuralism and the make-believe account by removing from it all the implausible and impotent components and seeing what remains. In the previous two sections, we saw that there is no reason to think that mathematical discourse is metaphorical, but there is good reason to think that, along with much of language use, it is non-literal in the sense that typical utterances are not intended to communicate the literal content of the sentences used. In this section, we saw that the make-believe account of how this communicative feat works fails insofar as it relies on our consciously pretending anything, and insofar as it gives principles of generation a primary explanatory role. So, if we remove the claim that mathematical discourse is metaphorical, the claim that it involves performing non-standard speech acts, and the claims that communication involves calling to mind principles of generation, what do we have left? We have an account of mathematical

discourse according to which we simulate belief in the propositions expressed by mathematical sentences to communicate propositions about non-mathematical things. Keeping in mind that simulating belief in p is just behaving linguistically as though one believes p , this just amounts to the interpretation of Melia I endorsed in §2.2. In fact, Yablo recognises the similarity, saying that figuralism is

... apt to be misunderstood. ‘Pretense’ sounds like making as if you believe, when you do not—and all I meant by it is being as if you believe, without regard to whether the belief is true. The construction in those earlier papers can, however, be replayed in the key of weaseling. (2012: 1011)

This view is far more plausible than figuralism, but as I mentioned in §2.2, it is silent on the question of how we use mathematics to communicate non-mathematical propositions, so it does not on its own address the communication challenge.

7.5 Mathematics as presupposition

Yablo’s more recent account of mathematical communication is *presuppositionalism* (Yablo 2006; 2009; 2012). The central claims are: first, the purely mathematical content expressed by mathematical sentences is presupposed in mathematical utterances; second, speakers can rely on presuppositions that are false, or presuppositions the truth-value of which they are indifferent to, to communicate content that is evaluable independently of the presupposition.

Presuppositionalism is grounded in a pragmatic theory applicable to language use more generally. Yablo motivates the theory by arguing that it explains a widespread pragmatic phenomenon: sentences that semantics classifies as false or

truth-valueless for one reason sometimes strike us as true or false for another. This phenomenon is *felt truth-value*.

In this section, I show that presuppositionalism does not provide an adequate explanation of felt truth-value, and that its application to mathematical language has some serious drawbacks. First, I will show that Yablo underestimates the scope of felt truth-value and presuppositionalism has limited explanatory power.

Sometimes, sentences that semantic theories tell us should be false, or truth-valueless, strike us as true or false for reasons other than those semantic theory is sensitive to. Consider:

(1) The man drinking a martini is a philosopher.

If the man is drinking water from a martini glass, semantic theory tells us (1) is false or truth-valueless. However, if the drinking man were a famous philosopher, the utterance would strike the listener as in some sense true. This is an example of felt truth-value. By developing a general pragmatic theory to explain this phenomenon, Yablo provides independent support for his account of mathematical communication as an application of the theory.

Yablo's mechanism works as follows. Let π be the presupposition of the utterance, S the sentence uttered and X the content communicated over and above the presupposition—the *assertive content*. We recover X from S by asking what is demanded of a world, over and above that π holds, to make S true. That is, we look for S's π -free entailment (Yablo 2006: 283). π -freeness is defined as follows:

[π -f]: X is π -free iff: either (i) X is true and could be true for the same reason even if π were false; or (ii) X is false and could be false for the same reason even if π were true. (Adapted from Yablo 2006: 287)

Consider for example:

(2) *Pointing to an empty chair* The King of France is in *that* chair.

Semantics tells us an utterance of (2) is false or truth-valueless because there is no king of France. However, an utterance of (2) strikes us as false for reasons independent of France's constitution: because the chair is empty. Yablo's theory explains this as follows. When someone utters (2), the listener uncovers the π -free entailment <Someone is sitting in that chair>. Our sensitivity to this false proposition explains why the utterance strikes the listener as false.

Presuppositionalism does not do a good job of explaining felt truth-value. Our sensitivity to the truth-value of an entailment of (2) concerning the contents of the chair is supposed to explain the felt truth-value of the utterance; but what explains our sensitivity to the π -free entailment? Presuppositionalism does not offer a satisfactory explanation of this.

The answer presuppositionalism offers depends on how we interpret π -f. The stronger interpretation says it is an account of how we identify the π -free entailment, in which case, the following problem arises. Yablo is trying to explain how a case of presupposition failure can strike us as true or false for reasons independent of the presupposition. However, identifying the π -free entailment of (2) involves identifying the presupposition, recognising it as false, and knowing why it is false.

This gets things backwards. The recognition that something false has been said *because of the empty chair* is prior to the recognition that the presupposition is false. Someone who forgot for a moment that there is no king of France would still not hesitate in saying that (2) is false.

In fact, there are cases where the presupposition is true while we seem sensitive to the truth-value of something other than the truth-conditional content. For example, there is no relevant difference between the way (2) initially strikes us and the way (3) does:

(3) *Pointing to an empty chair* The president of the U.S. is in *that* chair.

Both involve recognising that the chair is empty and evaluating the utterance as false prior to any considerations about the presupposition. We are sensitive to the truth-value of <Someone is sitting in that chair> not because we consider it to have the right kind of independence from the presupposition; we are sensitive to its truth-value *independently* of our attitudes toward the presupposition. An adequate explanation of felt truth-value must reflect this.

The weaker interpretation of π -f is as a way of merely specifying which entailment we are sensitive to. If this is right, presuppositionalism does not explain how we identify the π -free entailment and so does not address the communication challenge.

The similarities between (2) and (3) make clear that Yablo has underestimated the scope of felt truth-value. In cases where the presupposition is true, it still seems as though we are sensitive to the truth-value of some content other

than the semantic content. Once this is appreciated, the range of cases Yablo is concerned with appears artificially limited to those that have false presuppositions.

There also seem to be cases where the proposition we are sensitive to is not independent of the presupposition, in the way specified by π -f. Suppose (4) is uttered in the UK:

(4) The King of France is in Spain.

This seems false because there is no King of France; considerations concerning who is in Spain do not enter into our initial evaluation. Presuppositionalism gets the wrong result here: the π -free entailment is that someone is in Spain, which is true. Yablo could appeal to a semantic theory which classifies (4) as false to account for the felt truth-value of this utterance, but this is problematic for several reasons. First, only a semantics with a broadly Russellian account of presupposition, whereby presupposition failure renders the utterance false, would classify (4) as false. This is controversial: rival theories in a Strawsonian spirit entail that presupposition failure renders the utterance truth-valueless. It is preferable to account for all cases of felt truth-value while remaining neutral about the semantics of presupposition. Second, the phenomena appealed to so far appear similar. They all involve making an initial evaluation of an utterance, based on salient information. It would therefore be desirable to provide a unified pragmatic account of felt truth-value, rather than a disjoint one. The benefit of appealing to non-mathematical language is to avoid the charge that presuppositionalism's application to mathematical language is *ad hoc*. If this appeal to non-mathematical language involves an *ad hoc* division between cases where the semantic content explains felt truth-value and where the assertive content

does, this benefit is undermined. An adequate account of the felt truth-value of (5) will perhaps identify an entailment that has the right kind of independence from propositions concerning Spain and has the right truth-value in this context.

There are other cases of felt truth-value where the content we are sensitive to changes with the context. Yablo's theory cannot explain this, either. Consider:

(5) The President of the U.S. is in Croatia.

Suppose (5) is uttered in the U.S, among people who are ignorant of the President's whereabouts. I take it that most would not make an initial evaluation. π -f is not applicable here because the presupposition <There is a unique president of the U.S.> is true. Now imagine the President is standing in the room. π -f is still not applicable here. However, this utterance would strike most as false because the President is present. We seem to be sensitive to the truth-value of <The President of the U.S. is elsewhere> in this context. An adequate account of felt truth-value should explain why the President's whereabouts changes our initial evaluation of the utterance.

We have seen that presuppositionalism does not plausibly explain why we sometimes seem to be sensitive to the truth-value of something other than the truth-conditional content in our initial evaluation of an utterance. It does not adequately explain the phenomenon of felt truth-value. We have also seen that the scope of the phenomenon has been underestimated: there are cases of felt truth-value where the presupposition is true; there are cases where the content we appear to be sensitive to is not independent of the presupposition; and there are cases where changes in context change which proposition we appear to be sensitive to. An adequate account

of felt truth-value must account for all of this. In the following chapter I develop an account that does.

Presuppositionalism is not a desirable means of developing fictionalism for two reasons. The first reason is that such a fictionalist account would clash with the compelling phenomenological data presented in §7.3. Recall that explaining these data on a literal interpretation of mathematical discourse involves endorsing a collection of implausible sociological claims about the beliefs of participants in mathematical discourse. A non-literal interpretation of mathematical discourse is preferable, with respect to the phenomenology of mathematical language use, since it is compatible with a wide range of attitudes that speakers might have. Speakers might be ignorant of the ontological implications of the sentences they utter, or perhaps undecided on the relevant questions of ontology, or perhaps indifferent to them. On the strong interpretation of π -f, presuppositionalism has the speaker recover the intended content of mathematical utterances by recognising the presuppositions of the sentences uttered, recognising the reasons they are true or false, and recognising that there is some content evaluable independently. It is hard to reconcile this account with the phenomenological data about mathematical language use, since according to the account the presuppositions recognised by the speaker are the controversial ontological entailments of the sentences uttered. On this view, it is hard to see how one could interpret mathematical utterances without paying attention to these entailments.

The second reason is as follows. In (1)-(5), it is the truth-conditional content that is ultimately communicated by ordinary utterances of these sentences. In the mathematical case, however, the semantic content is not supposed to be what is ultimately communicated. Why is the π -free entailment communicated in the case of

mathematical utterances, while in other areas of discourse the π -free entailment merely plays the role of explaining felt truth-value? Nothing in presuppositionalism answers this question.

7.6 The intended content of pure mathematics

In his presentation of figuralism, Yablo provides a detailed proposal of the intended content of mathematical utterances. If we ignore the fact that figuralism is supposed to be an account of the mechanism by which we communicate, we can see it only as perhaps one of many suitable means of characterising the intended content of mathematical utterances. Yablo gives no impression that his thoughts about what is communicated by mathematical utterances have changed with his move to presuppositionalism, and the theory would likely provide the same results. In this section, I will outline what Yablo takes to be the content of mathematical utterances and show that it can explain all of the relevant intuitions concerning the content of mathematics. This aspect of Yablo's philosophy is a boon to the nominalist.

Yablo (2002: 211-2) provides an account of what he takes the intended content of applied arithmetical utterances to be. In Chapter 6, I showed that the semantic content of applied arithmetical sentences is free of commitments to mathematical objects, so I will ignore this part of Yablo's account and focus on what he says about the content of pure mathematical utterances. Yablo claims the intended content of pure arithmetical utterances are logical truths. The following is adapted from Yablo (2002: 212-3):

$$(6) \quad 3+5=8$$

Numerical quantifiers are defined recursively as follows: $\exists_0 x Fx =_{\text{df}} \forall x \neg (Fx \rightarrow x \neq x)$, and $\exists_{n+1} x Fx =_{\text{df}} \exists y (Fy \& \exists_n x (Fx \& x \neq y))$. Yablo would use the following to express the non-mathematical content of (6):

$$(6') \exists_3 x (Fx \& \exists_5 y (Gy \& \forall x \neg (Fx \& Gx))) \rightarrow \exists_8 z (Fz \vee Gz)$$

‘F’ and ‘G’ here cannot be specific predicates because the truths expressed by arithmetical utterances are supposed to be general truths about combinations of any objects. So how are we supposed to read (6')? The following passage settles the matter of interpreting Yablo:

The view that is emerging takes something from Frege and something from Kant; one might call it “Kantian logicism.” The view is Kantian because it sees mathematics as arising out of our representations. Numbers and sets are “there” because they are inscribed on the spectacles through which we see other things. It is logicist because the facts that we see through our numerical spectacles are facts of first order logic. (2002: 218)

So Yablo takes (6') to be a formula of first-order logic. This means the predicates ‘F’ and ‘G’ must have an interpretation, but any interpretation would deprive the content of pure arithmetical utterances of its much needed generality. An alternative is to view (6') as a schema for forming logical truths concerning specific objects. However, schemas do not express propositions, so that would mean (6') would not serve its function of expressing the content communicated by an utterance of (6). Perhaps Yablo intends the schema (6') to stand in for a conjunction of each statement

of the same form, corresponding to each possible interpretation of the predicates. This would have the required generality, but if this is supposed to be the intended content of (6), we may as well represent it as a logical truth of second order logic, where F and G are variables in predicate position. Accordingly, (6') is better expressed as follows:

$$(6'') \forall F \forall G (\exists_3 x Fx \ \& \ \exists_5 y (Gy \ \& \ \forall x \neg (Fx \ \& \ Gx))) \rightarrow \exists_8 z (Fz \vee Gz))$$

Some may think this problematic. Assuming that quantification into predicate position is quantification over sets, Quine famously described second-order logic as 'set theory in sheep's clothing' (1986: 66-68). On this view, if the non-mathematical contents expressed by arithmetical utterances are truths of second-order logic, they imply that there are mathematical objects, so this account of the content of an utterance of (6) will be of no use to the nominalist.

However, there is no reason to think that quantifying into a syntactic position commits us to any entities over and above what expressions in that syntactic position typically commit us to. (See Wright 2007 and Rossberg and Cohnitz 2009:154-5 for arguments.) Moreover, Rayo and Yablo (2001) argue as much, so the present revision of Yablo is Yablonian in spirit, while allowing the content of pure arithmetical utterances to be logical truths of the required generality.

Yablo also takes the intended contents of set-theoretical utterances to be logical truths. Yablo (2002: 215) provides the following example:

$$(7) x = y \rightarrow (\{x, u\} = \{y, v\} \equiv u = v)$$

An utterance of (3) is assigned the following logical truth as its intended content:

$$(7') x = y \rightarrow (((x=y \vee x=v) \& (u=y \vee u=v) \& (y=x \vee y=v) \& (v=x \vee v=u)) \equiv u=v)$$

This may require some unpacking. The intended content of ‘ $\{x, u\} = \{y, v\}$ ’ is that the supposed members of $\{x, u\}$ are identical with the supposed members of $\{y, v\}$. The conjunction of disjunctions forming part of the consequent of (7') is just an exhaustive description of the ways in which this condition can be met. So, if $x = y$, then the supposed members of $\{x, u\}$ are identical with the supposed members of $\{y, v\}$ iff $u=y$. (7') is a truth of first-order logic.

According to Yablo, pure arithmetical sentences and set-theoretical sentences are useful for stating logical truths about cardinality and combinatorial logical truths, respectively (2002: 214-215).

With respect to pure mathematical utterances that do not plausibly communicate truths about combinations of objects, Yablo gives two possible interpretations. The first assigns straightforward descriptions of the entailments of mathematical theories (2005: 232-3). This is most plausible for utterances of sentences such as:

(8) Five is a prime number.

To which the following non-mathematical content would be assigned:

(8') $\langle \text{PA} \rightarrow \text{five is a prime number} \rangle$

Here PA stands for Peano Arithmetic, as characterised by the Dedekind-Peano axioms. However, this cannot capture the content of all pure mathematical utterances. To give one example, the axioms of mathematical theories are typically not themselves consequences of further axioms. Yablo offers another example:

(9) The number of prime numbers is \aleph_0 .

This requires a different interpretation because \aleph_0 is an object that lies outside of the domain of number theory. Its proper home is set theory, but it is being called upon to help describe numbers. (9) is therefore not strictly an entailment of number theory. Yablo recommends such utterances be interpreted as expressing how we should conceive of mathematical objects (2005: 233). Thus, the non-mathematical content communicated by an utterance of (9) is explicitly expressed as follows:

(9') We should conceive of prime numbers such that there are \aleph_0 -many of them.

This interpretation also appears appropriate for uses of the axioms of our mathematical theories. It is at first unclear whether (9') implies that there are prime numbers. If the intended content concerned only ways we can or do conceive of mathematical objects, then it does not: there are ways we can and do conceive of the Theseus's Minotaur, but this doesn't imply that it exists. However, the proposition expressed by (9') is potentially more problematic because of the normative force of 'should'. Without knowing what is responsible for this normative force, we cannot

know whether (9') implies there are prime numbers. If there really are \aleph_0 -many primes, we better conceive of them as such. Yablo needs to explain where the normative force comes from if it is not from mathematical reality.

Yablo does provide such an explanation that renders the intended content of pure mathematical utterances free of commitments to mathematical objects. It partly concerns our intuitions about the content of mathematical utterances, so it is to assessing Yablo's account with respect to such intuitions that I now turn.

It is plausible that when we utter pure arithmetical sentences we mean something about what happens when objects are grouped together. Recall:

$$(6) \ 3+5=8$$

Someone pressed to justify an assertion of (6) would likely begin trying to demonstrate its truth by counting groups of objects. Such demonstrations would only reveal the truth of the logical formula Yablo takes to express the content of an utterance of (6); it would not reveal the truth of the proposition expressed by (6). Moreover, we take pure arithmetical truths to be necessary truths, which is a feature of logical truths. The same goes for set-theoretical truths. Imagine trying to demonstrate the truth of (7) to someone. The easiest way would be to use real objects to demonstrate its truth. But this again would only demonstrate the truth of the logical truth Yablo takes to be the content of an utterance of (7). Again, that what is communicated by assertions using set-theoretical sentences comes out as necessary on this view does not run counter to our intuitions.

Recall one of the more straightforward pure mathematical sentences:

(8) Five is a prime number.

The non-mathematical content of (8) concerns what is entailed by number theory. The contents of such utterances are taken intuitively to be *a priori* necessary. That a logical entailment holds is certainly that.

It might be objected that people typically take themselves to be talking about numbers when uttering (8). Granted, speakers would likely answer ‘Numbers, of course!’, if asked what they were talking about. But this does not settle the question of whether they take themselves to be talking about numbers as independently existing mathematical entities. Let us think about the situations in which (8) is likely to be uttered. Ordinary speakers are likely to utter (8) in one of a few situations. One is an applied situation, in which perhaps someone wants to indicate that the number of sweets they have, five, is not going to share equally among their four friends. In this context, it seems plausible that by ‘Numbers, of course!’ the speaker would mean that they are talking about *numbers of things*. In this case, the intended content of an utterance of (8) might better be represented as another second-order logical truth. Another context might be someone recalling knowledge of arithmetic. In this context, it seems plausible that by ‘Numbers, of course!’ the speaker would mean *what arithmetic says about numbers*, or perhaps, *what arithmetic captures about our conception of numbers*. Finally, there is the context in which someone mathematically sophisticated utters (8) in order to articulate a theorem of arithmetic. In this context still, it strikes me as plausible that by ‘Numbers, of course!’ the mathematician would mean *what the axioms of arithmetic say about numbers*. (In §8.5, I show that well-established pragmatic theory predicts what I have argued for here.)

Utterances of more sophisticated mathematical sentences that cannot be interpreted as concerning the entailments of theories appear more problematic. Recall:

(9) The number of prime numbers is \aleph_0 .

And the content assigned to an utterance of (9):

(9') We should conceive of prime numbers such that there are \aleph_0 -many of them.

Yablo must explain what determines how we should conceive of mathematical objects, and why utterances that express this content appear to be necessarily true. Yablo's explanation is posed as an answer to the question

...what accounts for the feeling of a right and wrong way of proceeding when it comes to mathematical theory-development? I want to say that a proposed new axiom A strikes us as correct roughly to the extent that a theory incorporating A seems to us to make for an apter game—a game that lends itself to the expression of more [non-mathematical] truths—than a theory that omitted A, or incorporated its negation. To call A correct is to single it out as possessed of a great deal of 'cognitive promise'. (Yablo 2005: 236)

The idea is that we choose the axioms of our mathematical theories based on how conducive they are to communicating non-mathematical truths. This accounts for

where the normativity associated with the non-mathematical contents of pure mathematical sentences comes from: we should conceive of mathematical objects in such a way that maximises our theory's utility for expressing truths about non-mathematical things.

This account of the non-mathematical content of pure mathematical sentences does not require that mathematical objects exist, but it is not yet clear how it helps capture our intuitions about the content of utterances of these sentences. Take the intuition that we express necessary *a priori* truths with the relevant sentences. Which mathematical theory is more conducive to expressing more non-mathematical truths is arguably a contingent matter, partly depending on how the world is non-mathematically. Moreover, though sometimes pure mathematical theories are useful for expressing truths about the non-mathematical world, they are typically developed to express things about pre-existing mathematics. Indeed, Yablo is aware of this:

Suppose we are working with a theory T and are trying to decide whether to extend it to $T^* = T + [\text{axiom}] A$. An impression I do not want to leave is that T^* 's aptness is simply a matter of its expressive potential with regard... only concrete things. T^* may also be valued for the expressive assistance it provides in connection with the mathematical subject matter postulated by T .
(2005: 236)

Though mathematicians are sensitive to other considerations when developing theories, the ability of a theory to establish more connections between pre-existing mathematics is certainly an important one. Importantly, this ability is not contingent:

what a theory has to say about mathematical concepts x and y is not dependent on any contingent matters of fact. This suggests an argument for the claim that the expressive capabilities of a mathematical theory with regards the physical world is also non-contingent. It is plausible that the more mathematically expressive a theory is, the richer its structure, and so the more expressive it is in general. A generally more expressive theory will allow us to say more things about the physical world than a generally less expressive theory. So it is plausible that which mathematical theories allow us to say more about the physical world is a non-contingent matter.

Yablo could also appeal to natural necessity here. All the possible worlds in which the laws of nature are constant are worlds in which our most physically expressive mathematical theories will remain so. It does not strike me as *ad hoc* to claim that our intuitions about the content of mathematics are not sensitive to worlds where, for example, grouping two objects with another two objects causes a fifth object to appear. The above arguments strongly suggest that a nominalist-friendly non-literal interpretation of mathematical discourse can meet the content challenge.

It may appear that literal accounts of mathematical discourse honour them more easily. For example, the platonist might argue that, when we utter pure mathematical sentences, what we intuitively take ourselves to be talking about when we utter mathematical sentences really is what we are talking about. An utterance that appears to be ascribing a property to prime numbers is ascribing a property to prime numbers. Moreover, mathematical objects exist necessarily, so the intuition that utterances of pure mathematics are necessarily true is honoured.

However, if my arguments in this section are sound, this appearance is illusory. Granted, we take the content of pure mathematics to be necessarily true, but it is not clear where we take this necessity to stem from. A literal interpretation of

mathematical discourse honours our intuitions only if the reason we take the mathematical truths to be necessary is that we take the objects they describe to exist and have their properties necessarily. But it strikes me as implausible that non-philosophically minded speakers would have such beliefs. We saw in §7.3 that there is compelling evidence in favour of the view that typical mathematical utterances are not aimed at communicating the semantic content of the sentence used. The data favours a non-literal interpretation because it involves taking on fewer commitments about the beliefs of ordinary speakers. The same point applies here. If the platonist can only explain the intuitions we have about the content of mathematical utterances by attributing to speakers the belief that there are independently and necessarily existing mathematical objects, then so much the worse for platonism.

7.7 Conclusions

Recall the two challenges that a successful fictionalist philosophy of mathematics must meet:

The content challenge: (i) provide an independently motivated characterisation of the intended content of mathematical language use (not to be understood as the demand that this content be explicitly stated across the board); (ii) explain the relationship between our intuitions concerning the content of mathematical utterances, and the content assigned to mathematical utterances by the theory.

The communication challenge: (i) provide an independently motivated account of the mechanism by which mathematical language is used to

communicate non-mathematical content; (ii) explain the relationship between the intuitions we have about mathematical language use and what the account says about mathematical language use.

We have just seen that Yablo has provided an account of the content of pure mathematical utterances that meets the content challenge. It provides a characterisation of the content of pure mathematical utterances as logical truths or truths about how best to conceive of mathematical objects. Crucially, the content of mathematical utterances on this view does not entail the existence of mathematical objects. Moreover, the account yields explanations for the intuitions we typically associate with the content of mathematical utterances that are more plausible than those offered by platonistic alternatives, since it avoids attributing to ordinary speakers philosophical beliefs.

Unfortunately, both of Yablo's attempts to meet the communication challenge fail. We saw that figuralism is not independently supported, is *prima facie* implausible, has little hope of explaining our intuition that mathematical discourse does not involve any non-standard speech acts, and fails to explain how non-mathematical content is communicated by mathematical utterances. We saw that presuppositionalism also lacks independent motivation, since it fails to adequately explain the datum of felt truth-value, and its prospects for accounting for how mathematical utterances communicate only non-mathematical content look similarly dim.

8. The Pragmatics of Mathematics (II): Evaluative Salience

8.1 Introduction

In this final chapter, I will develop an account of mathematical communication that promises to meet the communication challenge. To avoid the drawbacks of Yablo's two attempts to do so, I will hold my theory of mathematical communication accountable to the following desiderata:

- (i) Be motivated by general considerations about language use, and independently of philosophical concerns.
- (ii) Explain the available data concerning mathematical language use.
- (iii) Not attribute philosophical beliefs to speakers: be compatible with indifference to or ignorance of the ontological implications of mathematical sentences.
- (iv) Explain how the intuitive content of mathematical utterances is uncovered from the truth-conditional content of mathematical sentences.

My account is based on some independently motivated assumptions that form the basis of a well-established theory of communication: *relevance theory*. If a successful account of mathematical communication can be built on these assumptions, then it stands a good chance of meeting the communication challenge. In the following section, I present relevance theory, and draw out the assumptions out of which my account of mathematical communication will fall.

8.2 *Relevance theory*

In Chapter 5 (§5.2) I gave a brief indication of how pragmatic theory in the Gricean tradition proceeds. The background assumptions of such a theory are as follows. First, the meaning of a sentence is vehicle for conveying what the speaker intends to communicate and so constrains what can be communicated by an utterance the sentence. Second, speaker meaning is not directly perceived, but is grasped by an inferential process that takes the sentence meaning along with features of context as input. Third, the process of uncovering speaker meaning is guided by the assumption that a communicative act will mean certain standards. Recall that for Grice these standards take the form of certain maxims, such as the maxim of Quantity: make your contribution as informative as is required, and no more.

Relevance theory (RT) is a theory of communication in the Gricean tradition which is the result of years of collaboration between linguists, philosophers and cognitive scientists. (For recent presentations and discussions of the theory, see Blakemore 2002, Carston 2002, Wharton 2009, Wilson and Sperber 2012, Clark 2013 and Ifantidou 2014; the theory was originally developed in Sperber and Wilson 1986a.) There are two important respects in which RT departs from Grice. The first is that RT attempts to ground the theory of communication in some plausible assumptions about cognition in general. The second is that, in place of Grice's four maxims, RT says that there is only one standard which communicative acts are required to meet: they must be *relevant*. The notion of relevance appealed to here, however, is a technical one, and requires spelling out in terms of another technical notion: that of a *positive cognitive effect*.

A positive cognitive effect is something that makes a 'worthwhile difference to the individual's representation of the world' (Wilson and Sperber 2002: 251). An

obvious example of such a positive cognitive effect is a true belief concerning some topic of interest. If an individual plans to go swimming in the Amazon River, believing truly that there are piranhas in the Amazon River will make a worthwhile difference to the individual's representation of the world, perhaps leading them to revise their plans.

According to RT, relevance is a potential property of any input to human cognition. It is defined by stating the conditions under which an input is more or less relevant: other things being equal, the greater the positive cognitive effects achieved by processing an input, the greater the relevance of the input to the individual at that time; other things being equal, the greater the processing effort expended, the lower the relevance of the input to the individual at that time (Wilson and Sperber 2002: 252). The most relevant input to an individual at a time, therefore, is that which gives the greatest cognitive effect for the least effort.

This notion of relevance bears the majority of the explanatory weight of RT's account of communication, but three further assumptions are also relied upon. The first is that human cognition is typically geared towards maximising relevance (Wilson and Sperber 2002: 254). That is, human cognition seeks to maximise positive cognitive effects with the least effort.

The second assumption is that, underlying all communication, there are two intentions at work: that of informing the audience of something; and that of informing the audience of the intention to inform them (Wilson and Sperber 2002: 255). This is an important distinction. Suppose I want to inform someone of some international catastrophe. I might do so by switching on the news, but this is not yet communicating. Though I may succeed in informing my audience, I may not succeed in informing them that I intended to inform them about the international crisis. To

communicate with my audience, I might point to the television screen or say something like ‘Have you heard what’s going on?’ As soon as my audience recognises that I intended to inform them of the international crises, understanding occurs, and communication is successful. (Grice 1989 made this claim about communication.)

When an input is relevant enough to be worth the effort of processing, and it is the most relevant input compatible with the abilities and preferences of the communicator, it is said to be *optimally relevant* (Wilson and Sperber 2002: 256). The final assumption RT rests on is that every act of communication carries with it a presumption of its own optimal relevance. In making an audience aware of the intention to communicate, there is an implicit presumption that interpreting the communicative act will yield some positive effect that will be worth the effort spent in processing it. It is assumed that the information communicated is important, and is presented in a way that will minimise the cognitive effort spent in uncovering it. However, this is constrained by the limits of the interests and ability of the communicator. There may be information that the speaker is unwilling to communicate, and ways of presenting this information that the speaker is incapable of.

An act of communication produces in the audience an *expectation* that some beneficial cognitive effect is being made available. For this expectation to be met, and for communication to be successful, the speaker must present the information in such a way as to ensure that the correct interpretation is the first interpretation uncovered that fulfils this expectation. There may be other interpretations available, but if the speaker does not want to be misunderstood, she must make sure that her intended interpretation is the easiest, and therefore the quickest one for the speaker to

get to. As soon as the audience lands on an interpretation that meets the expectation, the interpretation stops, because it is assumed that the speaker has made the correct interpretation the easiest one to find. This is how the expectation of relevance is supposed to explain communication: it licenses the following comprehension procedure (adapted from Carston 2012: 168):

Relevance-theoretic comprehension procedure:

- a. Follow a path of least effort in computing cognitive effects: Test interpretive hypotheses in order of accessibility.
- b. Stop when your expectations of relevance are satisfied.

If all goes well, this ensures that only one interpretation is possible. For example, consider the following uttered by someone arriving late to work:

(1) Traffic was a nightmare.

Two pragmatic processes are at work here. The first is a filling in of contextual information. The interpretive process will fill in ‘The traffic *on the way here*’, because (1) is offered as an explanation for why the speaker is late promises to be the most relevant interpretation, and because, given the competence of the listeners, the speaker could not rationally intend to communicate something about some other instance of traffic, unrelated to the present context. This process is *completion*.

The second process is a case of meaning adjustment called *loosening*. The expression ‘nightmare’ has a rather specific meaning: a dream of an unpleasant experience. However, the present utterance of (1) is intended to describe a real

experience. Interpretations in which ‘nightmare’ keeps its encoded meaning will therefore be rejected as irrelevant—‘The traffic’ is unlikely to be referring to a traffic jam the speaker dreamed of, since this would not help explain his or her lateness. Alternative interpretations tested by the interpretive process include those in which ‘nightmare’ is given a looser meaning. One such meaning is as follows: any experience (real or not) that is unpleasant.

This loosening of ‘nightmare’ along with the completion of ‘The traffic’ results in the following interpretation: the traffic on the way to work was unpleasant. According to RT, implicatures are generated and assessed in parallel with processes such as loosening and completion. The present interpretation has the implicature that the traffic was unpleasant because it took a long time, and that this explains the speaker’s lateness, and once this interpretation is landed on, it meets the expectation of relevance created by the act of communication. Thus we see how mutually adjusting the meanings of expressions until the expectation of optimal relevance is met should result in one interpretation that may or may not be the semantic content encoded by the sentence. (For more detailed accounts of the role of meaning adjustment in relevance-theoretic interpretation, see Carston 2002; Wilson and Sperber 2002; and Wilson and Carston 2007.)

There are good reasons to accept a relevance-theoretic account of communication, some empirically based, others more theoretical. I will mention a few empirical reasons here. First, the claim that the human mind is geared towards maximising cognitive benefit for the least effort is highly plausible. It is something natural selection would plausibly select for, so RT is compatible with an evolutionary explanation of our communicative practices. RT also complements two widely held ideas from cognitive science: that the mind is modular, consisting of

many distinct processes that have evolved to perform specific tasks and solve specific problems; and that modules exploit ‘fast and frugal heuristics’ in performing their functions, meaning they use special-purpose inferential procedures that take advantage of regularities in the modules domain (Cartson 2012: 166). Both of these ideas have been supported by arguments in evolutionary theory (see Cosmides and Tooby 1994 and Gigerenzer et al. 1999, for example). In line with these assumptions, RT assumes that the mental faculty responsible for communication and utterance interpretation is a dedicated module.

At this point, I should say something about the relationship between RT and the minimal view of semantics I defended in chapter 5. There are two issues that require attention. First, in §5.4, argued that we should adopt the minimal view of semantics because semantic comprehension appears to be highly modular, and allowing abductive reasoning to play a part in semantic comprehension does not sit comfortably with this appearance. Isn’t it hypocritical of me to then to assume that utterance comprehension (often an abductive) is modular and recommend adopting a particular account of it because it is compatible with this assumption? The second issue is that many relevance theorists reject minimal semantics, claiming that the RT account of communication obviates the need to posit a minimal semantic level of interpretation. I will deal with both issues in turn.

The first issue can be dealt with by recognising that cognitive faculties can be more or less modular. On the one hand, the faculty of semantic interpretation is *highly* modular: we saw in §5.4 that its qualities (fast, informationally encapsulated, ect.) are comparable with our faculty of visual perception. On the other hand, the communication faculty is not highly modular, since the information it operates over is not restricted: any input could potentially be relevant (non-technical sense!) to the

interpretation of an act of communication, including information spat out by the faculties of semantic interpretation and visual perception. In what sense is the communication faculty modular then? Just in the sense that, where possible, it exploits fast and frugal heuristics to uncover communicated content as quickly as possible. These heuristics include well-established rules that govern particular practices. If this is right, then we might expect the communication faculty to deliver varying results in terms of speed and efficiency depending on whether there are such heuristics available to exploit; and this is exactly what we find. For example, conventions governing conversation at the dinner table make the interpretation of ‘Can you pass the salt?’ as communicating a request for the salt immediate and robust. One has to concentrate to hear it as a question about the audience’s abilities. In contrast, a metaphorical utterance of ‘John fought the Sun’ is likely to take much longer to interpret and be less likely to communicate exactly the same content to different interlocutors.

The second issue can be brought out more clearly by considering again the interpretation of and utterance of (1). Recall that there were two processes at work: completion and loosening. Regarding the latter, the issue arises because relevance theorists tend to consider pragmatic processes such as loosening as occurring pre-semantically. Hence, Carston and Powell write that an ‘important and distinctive’ characteristic of the RT account of these phenomena is ‘the claim that the pragmatic process involved is not a matter of implicature derivation but rather of conceptual adjustment which contributes to the proposition explicitly communicated (the truth-conditional content of the utterance)’ (2006: 345). Again, the cognitive backdrop here is the broadly Fodorian one according to which expressions express concepts which bear the semantic relations to the external world. If the process of loosening

operates on individual concepts, rather than at the level of propositions, then it looks as though it can occur before the purely semantic content of the sentence uttered is processed, if the relevant expression occurs towards the beginning of the sentence uttered, for example. In such cases, it looks as though the adjusted concept is going to be what features in the thought that is entertained in grasping the content of the sentence, so the purely semantic level of representation is bypassed by pragmatic processes. To the extent that such cases are common, it looks as though the semantic minimalist cannot in general help herself to the pragmatic apparatus exploited by RT.

My answer to this worry is to point out that there is nothing incoherent in coupling minimal semantics with pragmatic processes that occur on-line at the level of individual expressions and the concepts they express. According to the minimalist, the semantic module is informationally encapsulated. This just means is that it is not sensitive to any kind of information other than linguistic information. What it does not mean is that the semantic module is like a dark tunnel where linguistic information goes in one end and a full-blown propositional interpretation comes out the other. There is no reason to think that other its inner workings are not transparent to other cognitive faculties, such as the communication faculty. Moreover, there is no reason to think that once a concept has been adjusted by the communication faculty the semantic module the semantic module alters what it is doing in any way. In fact, the kind of semantic minimalism I have defended rules this out by implying that the semantic faculty is highly modular. The semantic faculty continues to generate the full-blown truth-conditional representation; it's just that, in cases where conceptual adjustment occurs, this representation will not be the

one that the communication faculty uncovers as the communicated content of the utterance.

The obvious response to this is to ask what the use of positing this semantic level of representation is. On the assumption that cases of conceptual adjustment are widespread, the output of the semantic faculty will very rarely be what the speaker intended to communicate, so what work is it doing? I hope I did enough in chapter 5 to show that there are good reasons for positing a purely semantic level of content, even if it is rarely what speakers intend to communicate in uttering a sentence.

8.3 Evaluative salience and felt truth-value

In §7.5, I showed that Yablo's proposed explanation of the phenomenon of felt truth-value is unsuccessful. In this section, I will draw on RT to provide an explanation that succeeds where Yablo's fails. The mechanism that underpins this explanation will then be shown to adequately explain how mathematical sentences can be used to communicate non-mathematical content.

Recall the examples from §7.5:

- (2) *Pointing to an empty chair* The King of France is in that chair.
- (3) *Pointing to an empty chair* The president of the U.S. is in that chair.
- (4) The King of France is in Spain.
- (5) The President of the U.S. is in Croatia.

Why does it appear that we are, at least initially, sensitive to the truth-value of something other than the truth-conditional content when these sentences are uttered?

To answer this question, let us go through the interpretation process step by step.

Let us consider (3) first. According to the minimal view of semantics I endorse, the first thing that happens when interpreting an utterance of (3) is that the agent grasps the minimal semantic content by forming the appropriate thought. The thought can be represented as follows, where β is the general concept triggered by ‘The president of the U.S.’ and α is the singular concept triggered by ‘that chair’:

(3') [β is in α]

Importantly, the agent has not yet mapped this representation onto information found in other cognitive areas. This is the job of the communication faculty. This means that β has not yet been enriched by the agent’s knowledge of who exactly the president of the U.S. is, and α has not yet been linked up with contextual information that allows the agent non-linguistically identify its content. At this stage of the process, the speaker merely grasps the content of this sentence via the reflexive descriptive content of each expression. So, β ’s content is fixed via its character, which contains information that might be expressed as follows: *the individual who actually satisfies the description ‘The President of the U.S.’*. Similarly, α ’s content is fixed by its character which contains information that might be expressed as follows: *the chair to which the speaker intends to refer with the utterance of ‘that chair’*.

For the next step of the interpretation process, the communication faculty takes the information delivered by the semantic faculty and maps it onto contextual information in an initial attempt to land on the intended content of the utterance. The most salient information available to the communicative faculty in this context will be the perceptual information which informs the agent that the intended referent of ‘that chair’ is the chair that she is pointing to. So, the next step is to enrich the

thought (3') with this information. The resulting thought can be represented as follows, where THAT CHAIR is the concept α enriched with the perceptual information about the chair which exhausts its content:

(3'') [β is in THAT CHAIR]

At this point in the interpretation process, the communication faculty will plausibly output an evaluation of the utterance as false. This is because the combination of two heuristics that the communication faculty plausibly makes use of in the interpretation of such utterances warrants the conclusion that (3) is false, even before β has been pragmatically enriched. The first heuristic is the knowledge that the sentence 'The president of the U.S. is in that chair' is true only if (3'') is true. The second heuristic is that presuppositions are typically assumed true in conversation. Since <There is a unique president of the U.S.> is the presupposition here, that the object exhausting the content of β exists is never brought into question. These two heuristics ensure that once (3'') is entertained by the agent, she assumes that (3) is false. This feedback is an important part of the interpretation process, since it will trigger the formulation of less straightforward hypotheses about what the intended content of the utterance of (3) is. Given the presumption of optimal relevance, after all, it cannot be that the speaker merely intended to communicate the semantic content of (3), since it is obvious in this context that it is false.

Finally, our knowledge about the identity of the president enriches our representation further and our assumption of the falsity of (3) is verified:

(3''') [OBAMA is in THAT CHAIR]

According to the above story, the communication faculty enriches the minimal thought with easily accessible contextual information until it produces a thought which meets the following criteria: (i) it is easily evaluated in the context; (ii) given its evaluation, an evaluation of the sentence uttered is warranted. Let us call the thought that meets these criteria the *evaluatively salient thought* (EST). With respect to (3), the EST is represented in (3''). The EST is a pragmatically enriched thought, in that it includes information about the objects it concerns that go beyond purely linguistic information provided by the sentence. However, this does not necessarily mean that the content of the EST differs truth-conditionally from the content of the sentence uttered. The content of (3'') is truth-conditionally identical to the content of (3). In §7.5, I suggested that, with respect to (2) and (3), it seems that the content to which our initial evaluation is sensitive to is <Someone is sitting in that chair>. The above story reveals that this is not quite right. It is rather that our initial evaluation is sensitive to a semi-contextually enriched thought the evaluation of which presents itself to the communication faculty as the most effective means of evaluating the sentence uttered.

Aside from the plausibility of the story presented above, there are good theoretical reasons for thinking that the communication faculty initially outputs an evaluation of the EST with respect to a particular utterance. They correspond to the two goals which, according to RT, an agent has when presented with an utterance:

A speaker producing an utterance has two distinct goals: to get the addressee to understand her meaning, and to persuade him to believe it. The addressee

has two corresponding tasks: to understand the speaker's meaning, and to decide whether to believe it. (Wilson oxford handbook)

Moreover, the addressee wants to achieve these tasks as quickly as possible. Take the first task: to understand the speaker's meaning. Sometimes the intention behind an utterance diverges greatly from that of asserting the truth-conditional content of the sentence uttered. A good sign that the speaker is not intending to assert the truth-conditional content of the sentence uttered is that the truth-conditional content is obviously false in the context, as with, say, ironic utterances. So, if the addressee can work out as quickly as possible whether or not the truth-conditional content of the sentence is obviously false in the context, then she can more quickly get to work on testing more complex hypotheses about the meaning of the utterance. Given that evaluating the EST has the appearance of being the quickest means of evaluating the truth-conditional content of the sentence uttered, the quickest means of achieving the goal of getting to the speaker's meaning is to evaluate the EST.

Now take the second task: to decide whether or not to believe the speaker meaning. Learning a truth that bears on one's interests is a positive cognitive effect. By the same token, learning that something of interest is not true can be equally beneficial: it precludes an agent from adopting a faulty representation of a topic of interest, and so makes a worthwhile difference to their representation of the world. So, given that the truth-conditional content of the sentence uttered is the starting point for the interpretive process, being able to quickly as possible rule out the truth-conditional content as false has the potential to yield positive cognitive effects: first, if the truth-conditional content bears on a topic of interest, ruling it out as false or verifying it as true will amount to a positive cognitive effect; second, if the speaker

intends to communicate the truth-conditional content, then learning whether or not the speaker is mistaken about a topic of interest, or perhaps trying to mislead the addressee about a topic of interest, will amount to a positive cognitive effect. (Sperber, Clément, Heintz, Mascaro, Mercier, Origgi, and Wilson 2010 call this exercising one's 'epistemic vigilance'.) Given that evaluating the EST has the appearance of being the fastest means of evaluating the truth-conditional content of the sentence uttered, it is plausible to think that an epistemically vigilant addressee will first evaluate the EST.

Let us assume, therefore, that the first port of call for the interpretive process is to evaluate the EST, and see what this tells us about utterances with false presuppositions, such as (2). The story will be exactly the same as with (3):

(2) *Pointing to an empty chair* The King of France is in that chair.

Again, the salience of the chair ensures that the following thought is quickly formed, where γ is the minimal concept triggered by 'The King of France':

(2'') [γ is in THAT CHAIR]

The interpretive mechanism draws on the same two heuristics as for (3): the presumptions that presuppositions are true; and the knowledge that 'The King of France is in that chair' is true only if (2'') is true. This warrants the initial evaluation of (2) as false, even if we are later squeamish about assigning any truth-value to it. The difference between this story and the story I told for (3) is just that the assumption that the presupposition is true is, in this case, false. Nevertheless, the

evaluation of (2'') still has the appearance of being the most efficient means of evaluating the truth-conditional content of (2), so it is still the EST in this context, so the above considerations ensure that its evaluation is the first port of call when interpreting (2).

Now we move on to cases where what is salient to our evaluation of the utterance is not independent of the presupposition. Consider again (4):

(4) The King of France is in Spain.

This utterance strikes us as false because there is no king of France, and my claim that we typically evaluate the EST first predicts this. There is no salient perceptual information or background knowledge regarding Spain in this context that might help evaluate (4). There is some helpful background knowledge regarding France, however: namely that France does not have a king. This ensures that the EST in this context is the following, where δ is the minimal singular concept whose content is exhausted by Spain, and THE KING OF FRANCE is the concept of the King of France, enriched by the background knowledge that France has no king:

(4'') [THE KING OF FRANCE is in δ]

Our initial evaluation of (4'') as false, along with the heuristic that 'The King of France is in Spain' is true only if (4'') is true, warrants the output that (4) is false. This explains why (4) strikes us as false because there is no king of France.

Finally, let us consider shifts in context which correspond to shifts in our initial evaluation. Consider again (5):

(5) The President of the U.S. is in Croatia.

First, imagine that the utterance occurs in the U.K. with the President of the U.S. clearly in sight. The salience of the President ensures that the first stage in the interpretive process is to non-linguistically identify the referent of 'The President of the U.S.'. The thought that results can be represented as follows, where THAT MAN is the concept triggered by 'The President of the U.S.' enriched by the perceptual experience of the president standing in the room, and ϵ is the minimal concept whose content is exhausted by Croatia:

(5'') [THAT MAN is in ϵ]

In the present context, the interpretive process is not yet in a position to output an evaluation of (5''), so it must search for further information to enrich the thought. The information most relevant to evaluating (5'') in this context is the fact that Croatia, the content of ϵ , is not here, where the President is. The resulting thought can be expressed as follows, where NOT HERE is the concept ϵ enriched with the information that Croatia is elsewhere:

(5''') [THAT MAN is NOT HERE]

This thought is formed quickly, since it requires relatively little contextual information, all of which is easily accessible. Moreover, once it is formed, it can immediately be evaluated as false, and once it is evaluated, it warrants the

conclusion that (5) is false. Hence, (5''') is the EST with respect to (5) in the present context. In the present context, that we first evaluate the EST explains why (5) strikes us as false because the president is here.

Now let us imagine that the utterance takes place in the U.S. amongst people who have no idea about the whereabouts of the President. In this context, I take it we would not make an initial evaluation either way. This is to be expected on the present account, since there is no salient information that promises to bear on the truth or falsity of (5). In this case, we might say that the EST is the thought that results from fully supplementing the concepts triggered by the sentence with background knowledge or perceptual information. This will put the agent in the best position possible with respect to evaluate (5) should some any new information come to light in the course of the conversation.

The pragmatic theory presented here succeeds in one area where Yablo's presuppositionalism failed: it successfully explains the phenomenon of felt truth-value. Moreover, the account falls out of the combination of two independently motivated theses: the minimal view of semantics and the relevance theoretic account of communication. Call the theory *semantic minimalism and relevance theory* (SMART). It remains to be shown whether SMART can be extended to account for communication with mathematical language.

8.4 Mathematical utterances in applied contexts

A key part of the explanation of felt truth-value I gave above was the appeal to the ability of the pragmatic faculty to distinguish between contextual features that are more or less relevant to the evaluation of the sentence uttered. Keeping that in mind, consider the following passage from Yablo:

Numbers... are not (would not be) an original source of information on any topic of interest; their contribution is exhausted by what they are *supposed* to be like. This makes the presupposition that numbers exist ‘failsafe’ in the sense that its failure makes (or would make) no difference whatever to which applied arithmetical sentences count as true. I am tempted to conclude that nothing in the felt truth-values of those sentences has any bearing on the issue of whether numbers exist. (2006: 292)

Something like this idea will be central to my application of SMART to mathematical utterances. Appealing to information about the nature and existence of mathematical objects never, or at least very rarely, has the appearance of being the most efficient means of evaluating mathematical utterances. Or so I will argue in this section.

In §6.7, I discussed evidence appealed to by Hofweber about the way in which we are taught to use arithmetical language. I return to this in more detail now, since my application of SMART to mathematical language relies on it.

First, we start with applied arithmetical sums with numerals occurring as adjectives:

(6) Four lions and four lions are eight lions.

There are informal procedures we learn to perform to evaluate sentences such as (6). For example, faced with a picture of eight lions, we learn to count each lion starting from ‘one lion’ and going back to ‘one lion’ every time we reach ‘four lions’, as

follows: ‘one lion, two lions, three lions, four lions, one lion, two lions, three lions, four lions’. Then, we count the same lions, but this time counting all the way up to ‘eight lions’. This procedure can also be performed imaginatively. Procedures such as this are enough to convince us that the truth-conditions of (7) obtain; that is, that all collections of eight lions are identical to two collections of four lions.

Moving on, we learn to use plural pure arithmetical sentences, with numerals occurring with no argument noun, so perhaps being type-shifted to quantifier expressions:

(8) Four and four are eight.

The truth-conditional content of this sentence is plausibly a general proposition concerning collections of objects, but concerning no objects in particular. For our purposes, it might be adequately captured by the following sentence of second-order logic:

$$(8') \forall X \forall Y (\exists_4 x Xx \ \& \ \exists_4 y Yy \ \& \ \forall x \neg (Xx \ \& \ Yx)) \rightarrow \exists_8 z (Xz \vee Yz)$$

We learn to evaluate sentences such as (8) via the same real or imaginary counting procedures mentioned above, but without being restricted to particular kinds of objects. So, we learn that the truth-condition for (8) is that, for any collection of eight objects whatsoever, it is identical to two collections of four objects.

Finally, we make the step to non-plural arithmetical sentences:

(9) Four and four is eight

and their symbolic counterparts:

$$(10) 4+4=8$$

As argued in §6.7, there appears to be no legitimate motivation for denying that the numerals here are directly referring expressions which purport to refer to numbers. Moreover, in learning how to manipulate arithmetical formulae, we learn to reason with them in accordance with their syntactic and semantic profile.

However, we learn to evaluate such sentences by the same informal means as with plural arithmetical sentences: by performing real or imaginary counting procedures. This is how we come to associate with certain non-mathematical propositions with arithmetical formulae. In learning to use (9) and (10), for example, we internalise something like the following rule: $4+4=8$ if and only if four and four are eight.

Now, let us consider an utterance of (10) in a particular context, and see what SMART predicts with respect to the interpretation process. Suppose (10) is uttered (or, indeed, scribbled down) by Jill to Jane in the process of a conversation about how many eggs to put in a quiche they are making. They are cooking it only for themselves, and Jill is proposes that four eggs per person are enough, then goes on to utter (10). What does SMART tell us she communicates?

First, the semantic faculty will output the appropriate minimal thought. Where α and β are singular concepts whose content is completely exhausted by the number eight, the thought can be expressed as follows:

(10') [$\alpha = \beta$]

At this point the agent only grasps the content of α and β via their character, which can be characterised by the descriptions *the number picked out by '4+4'* and *the number picked out by '8'*. (I am assuming here that the character of the concepts triggered by '4+4' and '8' differ, though they have the same content. I find this plausible at the level of minimal semantic content, since the grasp of content is merely through the reflexive descriptive information offered by the expression. Since the expressions are different, so the reflexive information they suggest is different. However, nothing in what follows rests on this claim.) Though this is an extremely minimal grasp of the truth-conditions of (10), recall that the character of a concept determines both its content and the permissible inferences we can make with thoughts that involve that concept. Minimal as it is, grasping (10') can facilitate arithmetical reasoning the proper use of (10).

However, as I have noted before, knowing the conditions under which a sentence is true does not amount to being able to say whether or not it is true, and SMART demands that Jane's pragmatic faculty search for what appears to be the most efficient way of doing this. In the examples considered in the previous section, the process of finding the EST generally proceeded by supplementing the concepts of the minimal thought with contextual information so as to gain non-linguistic or at least richer grasp of their contents, and so being able to recognise or infer whether or not the thought is true. In the case of pure mathematical language, however, what might this non-linguistic or richer information be? In the spirit of the passage from Yablo above, mathematical objects themselves do not appear to be poor sources of information when it comes to evaluating sentence that purport to describe them.

Above, I claimed that the process by which we learn to use pure arithmetical formulae results in our internalising a rule: the mental representation of which might be expressed as follows, where FOUR and EIGHT are general concepts triggered by the corresponding numerals in (8):

(10') [$\alpha = \beta \equiv \text{FOUR}$ and FOUR are EIGHT]

Jane learnt arithmetic like the rest of us, so this thought is available to her. Evaluating the right hand side of (10') will allow Jane to immediately evaluate the left. Moreover, the right hand side is presumably known by her to be true; but even if it is not, it is easy to verify via some appropriate counting procedure. The right hand side of this internalised rule is particularly easy to evaluate, and its evaluation warrants an evaluation of the sentence uttered, so we can conclude that it is the EST with respect to (10). According to SMART, then, the first stage in Jane's interpretive process is forming and evaluating the following thought:

(10'') [FOUR and FOUR are EIGHT]

This thought clearly has useful implications in the present context. From it, Jane can infer that they will need eight eggs. (8') clearly makes a worthwhile difference to Jane's representation of a topic of interest, namely how many eggs they will need to make their quiche. Moreover, going on to infer that (10) is true will make no further worthwhile difference to Jane's representation of a topic of interest; it will only cost more processing effort. It seems that in forming the thought (8'), Jane's expectation of relevance, caused by Jill's utterance, will have been met, so the interpretive

process will stop. In entertaining the thought (10''), Jane will have grasped the content that Jill wished to communicate in uttering (10).

SMART predicts that, in everyday contexts, utterances of pure arithmetical formulae will typically communicate the non-mathematical verification conditions we associate with pure arithmetical formulae in the process of learning how to use them. This easily generalises to scientific contexts in which quantities and magnitudes of physical objects are calculated via pure arithmetical formulae. Truths about the behaviour of collections in the broadest sense will always be optimally relevant when the topic of interest is how many objects of a certain kind of object there are, or how much of a certain substance there is, or how fast something is going, etc.

8.5 Mathematical utterances in pure contexts

What about utterances of pure mathematical sentences in pure, rather than applied, contexts? It is tempting to think that such utterances are aimed solely at describing mathematical objects. However, it would be a mistake I think to view typical utterances of pure mathematical sentences made within the context of pure mathematics as being intended as independent and straightforward statements of fact. Such utterances are to be understood against the backdrop of the theory on which the mathematician is working. In the context of working on a particular pure mathematical theory A, there are broadly speaking two contexts in which a mathematician might utter a particular pure mathematical sentence S. The first context involves proving theorems of A. The mathematician might utter S as part the final stage of a proof of S, or as a lemma on her way to proving another theorem, or as a conjecture, going on to provide reasons for thinking that S might follow from

A. The second context is that of discussing the axioms of A. I will consider each context in turn.

Take Peano Arithmetic (PA), expressed by the Dedekind-Peano axioms, for example. In proving that a theorem T follow from PA, a mathematician will at some point utter T; but this utterance will be uttered as the conclusion of an argument, which might be expressed as $PA \rightarrow T$. This makes it at least plausible that an utterance of T in this context asserts only that T follows from PA. (Leng 2010: 84-85 makes a similar point, claiming that ‘it is reasonable... to understand mathematical proofs as justifying claims about what does and does not follow from our mathematical assumptions, without regard to the question of whether those assumptions are themselves true’.) This plausibility clearly carries over to utterances of lemmas and conjectures.

Moreover, SMART predicts that this conditional content is exactly what mathematicians communicate when uttering pure mathematical sentences in such contexts. Just as with learning to use pure arithmetical formulae in applied contexts, learning to use PA involves learning to perform certain procedures (formal proofs) which demonstrate that certain non-mathematical conditions obtain, justifying the use of certain sentences (theorems). These conditions are just logical truths of the form $PA \rightarrow T$. Let us suppose that, in the course of working on PA, a mathematician utters Euclid’s theorem:

(11) There are infinitely many prime numbers.

An addressee’s semantic faculty first outputs a minimal thought which captures the truth-conditional content of (11). Then, the pragmatic faculty gets to work testing the

hypothesis that the truth-conditional content is the intended meaning by locating and evaluating the EST. Which thought is such that evaluating it appears to be the most efficient way of evaluating Euclid's theorem? The thought representing the non-mathematical condition the agent learnt to associate with Euclid's theorem, justifying its use in proving theorems in PA, is the most likely candidate:

$$(11') [[PA] \rightarrow [ET]]$$

Given that the aim of mathematicians in the present context is to study PA and explore what follows from it, forming (11') would satisfy the expectation of relevance that the utterance of (11) produced in the addressee. The interpretive process stops at forming (11'), so this thought captures the communicated content of an utterance of (11) in the present context. This same story applies in the case of proving a sentence T, uttering it as a lemma in a further proof, or offering it as a conjecture.

The second context of utterance was that of discussing the axioms of a mathematical theory. Why might mathematicians want to discuss the axioms of a mathematical theory aside from wanting to show that certain theorems follow from it? One reason is to justify the introduction of a new axiom or the revision of one or more of the accepted axioms. This would involve uttering a mathematical sentence as a proposal for a new axiom. Another reason is to affirm the accepted axioms of a given theory. Both these kinds of utterances involve putting forward a sentence with the intention of communicating that it should be an axiom of the relevant theory. It could be argued that the simplest way of understanding this behaviour is to see it as

asserting the truth of the sentence, in which case the truth-conditional content will be what is communicated.

This is too fast, however. We should once again pay close attention to how mathematical theory development/revision is practiced, and see what this tells us about what might be communicated in such contexts. The key question here is this: what are the considerations taken into account when mathematicians decide between axioms? Broadly speaking, there are two. The first constitute what Maddy calls ‘intra-mathematical pragmatic considerations’ (1997: 106). The second are considerations about which axioms strike mathematicians as more obvious. I will take each in turn.

The kind of intra-mathematical pragmatic considerations Maddy has in mind are considerations about the relative ease with which interesting consequences can be drawn from the resulting theory and the relative number of interesting things it entails. Recall that Yablo (2005: 236) appealed to such considerations in developing his account of the content of mathematical utterances; following him, then, let us say that a proposed axiom that scores highly with respect to these considerations is *fruitful*.

If an axiom or system of axioms is proposed because they are fruitful, then the most straightforward interpretation of such proposals is as expressing that the proposed axioms are fruitful. Indeed, the interpretation of such proposals as assertions of the truth-conditional content of the axioms would be puzzling in such a context. As Leng puts it, the axiom of choice has lots of fruitful consequences, but ‘why should *that* give us reason to believe it correctly describes the sets’ (2010: 89).

More problematic are considerations of the second kind. Some axioms are accepted because they strike mathematicians as highly intuitive or obviously true. If

an axiom system is proposed for these reasons, then it seems appropriate to interpret the proposal as an assertion of their truth-conditional content. However, once again, we should recognise that the intuition that a particular mathematical sentence is obviously true is not something that occurs in a vacuum, it occurs in the course of trying to adequately capture and refine mathematical concepts (cf. Balaguer 2001: 90-93). So, the axioms of PA seem to mathematicians to be the obvious facts about our concept of number. Or, to put it another way, if there are numbers, PA gives the obvious facts about them. To claim that mathematicians intuitions are stronger than this, that think it obvious that there exists some sequence of objects answering to PA, is far stronger, and far less plausible in my opinion. Mathematical practice is perfectly explainable on the weaker assumption that certain axiom systems strike mathematicians as obvious characterisations of particular mathematical concepts.

The emerging picture of pure mathematical practice is not that of a science which aims to detect and correctly describe the properties of some independently existing mathematical realm, but rather that of a highly creative discipline whose principle aim is to create fruitful and highly intuitive characterisations of our mathematical concepts and to explore their consequences. (For accounts of mathematical practice in this vein, see Balaguer 2001; Field 1984; and Leng 2010: 76-99.) There is one more thing I want to say in defence of this picture. When proposing new features of theories, disciplines that aim at truly describing some independently existing domain, such as the natural sciences, employ criteria which mathematicians eschew when developing their theories. For example, if it is possible to reduce one class of properties or objects to another, then natural science opts for doing so. That is, natural science is concerned with ontological parsimony. Mathematicians, on the other hand, seem to do exactly the opposite. They inflate

their ontologies wherever doing so increases the fruitfulness of their theories, or where reducing one kind of mathematical object to another seems unduly restrictive. (Leng 2010: 78-80 makes this point; see Tappenden 2008 for similar comparisons between mathematical practice and metaphysical practice.) If ontological parsimony is a criterion that helps ensure truth simpliciter in a theory, then this is a reason for thinking that mathematicians are not aiming for truth simpliciter.

Given these aims of the discipline, let us see what SMART has to say about the proposing of axioms. Say a mathematician proposes we adopt an axiom system A for characterising the natural numbers because it is both more fruitful and more obvious with respect to our concept of number than PA. Call an axiom system that is expressive and obvious with respect to a concept C ‘expressive’ of C. The addressee’s semantic faculty first outputs a minimal thought of the truth-conditional content of A, and the pragmatic faculty sets about testing the hypothesis that this content is intended by the speaker by searching for and evaluating the EST. Which thought is such that its evaluation appears to be the most efficient means of evaluating A? When learning mathematical theories, mathematicians will have learnt that there are certain conditions under which it is considered appropriate to adopt or accept a certain axiom system. The conditions are just that the proposed axiom system is more expressive of the relevant mathematical concept than the alternative. Given that it is via these conditions that mathematicians decide whether or not to adopt an axiom system, the thought that captures the condition with respect to A is the EST. Where EXPRESSIVE is the concept of being obvious and fruitful with respect to a mathematical concept, NUMBER is our concept of number, and $>$ is the concept *to a greater extent than*, we might express the EST as follows:

(12) $[[A \text{ is EXPRESSIVE of NUMBER}] > [PA \text{ is EXPRESSIVE of NUMBER}]]$

Finally, given that the aim in considering alternative axiom systems is to find maximally fruitful and obvious characterisations of our mathematical concepts, forming this thought will meet the addressee's expectation of relevance and the interpretive process will stop there. The content of this thought is therefore the communicated content.

8.6 Conclusion

In this chapter, I have presented an account of communication that falls out of the combination of two independently motivated theories: the minimal view of semantics, and the relevance-theoretic account of communication (SMART). Moreover, in §8.3 I showed that it predicts and explains the occurrence of felt truth-value. As an account of *mathematical communication*, therefore, it certainly meets desideratum the first desideratum I presented in §8.1. Recall that an account of mathematical explanation must:

- (i) Be motivated by general considerations about language use, and independently of philosophical concerns.

In §8.4-§8.5 I applied SMART to account for utterances of pure mathematical language in both applied and pure contexts, with plausible results. However, it remains to be shown how well it explains the data concerning mathematical language use we have encountered along the way.

Fortunately, it does rather well on this front. Recall that I rejected Yablo's presuppositionalism (§7.5) partly because it fails to explain evidence Yablo appeals to in support of figuralism (§7.3). The evidence suggests that speakers are often indifferent to or ignorant of the controversial ontological implications of the mathematical sentences they utter. SMART explains this by suggesting that speakers typically use mathematical sentences to communicate content that has nothing to do with mathematical objects. The first stage in the interpretation process does involve the addressee forming a thought that represents the truth-conditions of the sentence used; but the thought is a minimal one involving singular concepts that have the content they do only via their character. So, for example, the concept triggered by the expression '2' has a character that might be captured by a reflexive description, such as *the bearer of the name '2'*, and so on. Someone might even consciously believe a thought involving this concept while remaining ignorant of the fact that their belief concerns abstract mathematical objects; after all, being able to identify the number two as an abstract mathematical object is to have knowledge that goes beyond the minimal semantic content of sentences containing the numeral '2'.

Moreover, the minimal thought is the output of a highly modular faculty (the semantic faculty) into another modular faculty (the pragmatic faculty) which has fast and frugal heuristics at its disposal, guaranteed by the addressee's mathematical education, with which it can very quickly land on the intended content of the utterance. Given this, it is very plausible that the path to the intended content of mathematical utterances occurs subliminally. SMART therefore rightly groups mathematical utterances with straightforward assertions, such as 'The fridge is empty', where the communicated content is immediately uncovered and the addressee is rarely attends to the literal content, and rightly distances them from

ironic or metaphorical utterances, such as ‘Jill is a vulture’, where both the communicated content takes longer to uncover, and the addressee is typically aware of both the literal and the communicated content. This also verifies the intuition we have that we are doing nothing out of the ordinary when we utter mathematical sentences in assertorically.

Recall the evidence that Hofweber appeals to in support of his alternative semantic interpretation of pure arithmetical language (§6.7). This data suggests that speakers use plural cardinality sentences interchangeably with arithmetical formulae, despite their different meanings. In §6.7, I argued that Hofweber’s alternative semantics was unjustified in light of this evidence, and proposed that a pragmatic explanation would fare better. SMART provides such a pragmatic explanation by explaining that, in applied contexts, speakers typically use arithmetical formulae to communicate content literally expressed by the corresponding plural cardinality sentences. Moreover, it does so while retaining the ‘why’ aspect of Hofweber’s explanation. We learn to use arithmetical formulae in this way because it allows us to more easily draw out consequences about collections and magnitudes. Sentences containing referring expressions are easy to manipulate and have a low semantic type. The corresponding thoughts therefore have a simple character making it easier recognise their consequences. Of course, reasoning in this way will require that the thinker consciously attends to the minimal semantic content of the arithmetical formulae; but drawing any useful conclusions will require the thinker again to attend to the associated plural content.

One final intuition that requires explaining is the one appealed to by the easy argument for platonism (EA), discussed in chapter 6 (see §6.1 and §6.8). It is our intuition that certain elementary sentences of mathematics are obviously true. This is

different from the datum of obviousness I discussed above, with respect to the attitude pure mathematicians have towards the axioms they prefer; rather, this concerns the intuition that ordinary speakers have regarding elementary sentences of pure arithmetic. No one would deny that the sentence ' $2+2=4$ ' is true, for example. According to SMART, this intuition is not surprising, since, while the truth-conditional content of such sentences may be false, what speakers typically communicate by uttering such sentences is obviously true. It seems that the second desideratum for an account of mathematical communication has been met.

(ii) Explain the available data concerning mathematical language use.

In (SECT), I argued that maintaining a strictly literal interpretation of mathematical discourse involves attributing a collection of implausible philosophical beliefs to ordinary speakers. In contrast, SMART explains how someone who has no philosophical beliefs concerning mathematical objects whatsoever. Granted, all competent speakers will have mathematical beliefs that concern mathematical objects; but they need not be aware that these beliefs concern objects that are supposed to exist necessarily, outside of space and time, and so on. Hence, SMART meets the third desideratum:

(iii) Not attribute philosophical beliefs to speakers: be compatible with indifference to or ignorance of the ontological implications of mathematical sentences.

Finally, in §8.5 I demonstrated that SMART explains how speakers use mathematical sentences to communicate the intuitive content of mathematical utterances. SMART meets the final desideratum:

- (iv) Explain how the intuitive content of mathematical utterances is uncovered from the truth-conditional content of mathematical sentences.

The fact that SMART meets (i-iv) above demonstrates that it is a successful account of mathematical communication. Moreover, given that its rivals fail to meet these desiderata, it is the best account available.

Conclusion

It is time to pull the threads together. In Part I, I showed that we should view mathematics as an expressive device, useful only in allowing us to represent more things about the physical world than we would otherwise be able to. This view implies that we *ought* to use mathematically formulated scientific theories to express belief only in what those theories say about the non-mathematical part of the world.

In Part II, I showed that there are good reasons for thinking that people generally do use mathematics in this way; that is, they utter mathematical sentences in order to assert only content that does not imply the existence of mathematical objects. Utterances of pure mathematical sentences, for example, communicate logical truths about objects. Utterances of sentences that appear to describing or characterising mathematical objects, on the other hand, communicate content concerning the consequences of our mathematical theories, or the fruitfulness of a certain conception of a mathematical domain. The content of such utterances can be true even if mathematical objects do not exist.

The two parts of this thesis remove the two largest obstacles facing any nominalist philosophy of mathematics: to explain the indispensability of mathematics to science; and to explain our intuitions that many of the sentences of mathematics are obviously true, and could not have been false. These two obstacles are embodied in the indispensability argument (IA), and the easy argument (EA), respectively. If the role that mathematics plays in science is merely an expressive one, then IA fails. Therefore IA fails.

As for EA, the fact that many of our mathematical utterances communicate logical truths explains our intuitions. We mistakenly take many elementary

mathematical sentences to be necessarily true because what we typically communicate with them is necessarily true. The reason speakers mistake the sentences as true is that, when we interpret particular uses of these sentences, we only grasp the semantic meaning of these sentences initially, perhaps subliminally, in the form of a minimal thought. The interpretation process quickly uncovers a representation of a necessary truth on the way to evaluating the semantic content; but since the necessary truth satisfies the expectation of relevance, the process stops before a richer thought representing the truth-conditional content is formed. In light of this, one cannot take the truth of such sentences for granted in an argument (such as EA) for platonism without begging the question against the nominalist. EA fails as well.

It seems that nominalism can flourish in spite of the two obstacles mentioned above. Let us call the combination of nominalism and the SMART account of mathematical discourse *SMART fictionalism*. SMART fictionalism is a particular form of hermeneutic fictionalism, and its central tenets can be summarised as follows. First, mathematical and non-mathematical discourse is best interpreted continuously. Second, non-trivial mathematical sentences are false because mathematical objects do not exist. Third, mathematical sentences are typically used to assert content concerning non-mathematical things. Fourth, in using mathematical sentences in this way, speakers are not doing anything out of the ordinary; they are making straightforward assertions.

To conclude, I would like to compare SMART fictionalism with its rival platonism. I will argue that SMART fictionalism is the preferable view. I will finish by suggesting how SMART fictionalism might be extended to other areas of philosophy, bringing with it similar benefits.

The most obvious respect in which SMART fictionalism and platonism might be compared is their respective ontologies. Platonism posits mathematical objects and physical objects, while SMART fictionalism posits only non-mathematical objects. By Ockham's razor, then, we should prefer SMART fictionalism. (Leng 2010: 259-260 makes the same argument for her fictionalism.)

The platonist might object to this as follows. In the course of rejecting the indispensability argument and presenting my SMART account of mathematical discourse, I invoked a great many entities in which a fictionalist cannot consistently believe. To name but a few: properties; propositions; sentence types; sets; functions; concepts; and thoughts. There are different things to say about the different objects I have invoked. Perhaps the most ontologically promiscuous endeavour I have engaged with in this thesis is that of formal semantics. In Chapter 6, in the course of providing a semantic analysis of arithmetical language, I paired up mathematically characterised truth-conditions with sentence types. The first thing to note here is that the mathematical vocabulary used in characterising truth-conditions is being used representationally. Having shown in Part I that mathematics can and should be used in this way, I was justified in using mathematical language in Part II.

The second thing to note, regarding sentence types, is that, according to my view of natural language semantics, in pairing up truth-conditions with sentence types, we are providing a characterisation of how semantic interpretation proceeds. Semantic interpretation is a mental phenomenon, which, according to the minimal view, pairs up representations of sentence types with minimal thoughts that have the appropriate content. If there are no sentence types, then our semantic faculty operates over empty representations. However, this should not dissuade us from trying to characterise the operations of the semantic faculty. After all, in doing semantics, we

are trying to account for a mental phenomenon, not trying to give an accurate metaphysics of sentences. As for the mental entities I have invoked, such as concepts and thoughts, I do not consider accounting for their nature to be a problem that only nominalism faces.

Properties and propositions perhaps demand a different response. Given the success of SMART fictionalism with respect to mathematical entities, one can be hopeful that some of the other problematic entities we find ourselves mentioning in the course of articulating our theories might be removed from our ontology in a similar way. I return to this point at the end of this conclusion.

Another means in which we might compare the two philosophies is in terms of their respective epistemological duties. Recall that platonism faces a serious epistemological challenge. The best formulation of the challenge is that provided by Field (1989: 25-30). (See Cheyne 1998; 2001 for another challenge to platonism based on Benacerraf's; see Liggins 2010 for a useful discussion of epistemological objections to platonism.) Field challenges the platonist to show how our mathematical beliefs are reliable, if they concern non-causal non-spatial objects. In contrast, SMART fictionalism does not need to provide an epistemology of mathematical objects at all.

Colyvan (2001: 153) suggests that any platonist view motivated by the role mathematics plays in science can respond to Field's challenge by appealing to mathematics' applications, endorsing a suggestion made by Field himself:

One could argue... that if mathematics is indispensable to laws of empirical science, then if the mathematical facts were different, different empirical consequences could be derived from the same laws... and maybe this would

enable the application-based platonist to argue that our observations of the empirical consequences of physical laws are enough to explain the reliability of our mathematical beliefs (Field 1980: 28-9).

An obvious worry with this might be that, as I showed in Chapter 4, the indispensability of mathematics does not support platonism. However, in the present context, this worry is not charitable. Field's challenge starts with the assumption that platonism is true, and asks how the reliability of our mathematical beliefs might thereby be explained. Regardless of whether the position receives any support from empirical science, let us assume the truth of the more moderate form of platonism discussed in (§4.3). Though unmotivated, we saw that this view can account for the explanatory role of mathematics in optimality explanations. On this view, worlds in which certain mathematical facts are different are worlds in which the physical facts are different, since different physical facts will give rise to different mathematical facts.

A more serious worry is that the scope of this epistemology is limited to beliefs concerning applicable mathematics, which is a small fraction of our mathematical beliefs. Colyvan responds to this by claiming that the reliability of beliefs concerning pure mathematics is explained by the applicability of the relevant mathematics to other areas of mathematics, 'so long as there is a chain of applications that leads eventually to empirical evidence' (2001: 153; see Colyvan 2001: 105-11 for a development of this point). Colyvan accepts that mathematics not featuring in such a chain of application is not ontologically committing, and should be considered mathematical recreation (2001: 107-8).

There is a two pronged response to this. First, it is doubtful that mathematicians make the distinction that Colyvan does between ‘recreational’ mathematics and ontologically committing mathematics. As Eugene Wigner says, *most* advanced mathematical concepts ‘were so devised that they are apt subjects on which the mathematician can demonstrate his ingenuity and sense of formal beauty’ (1960: 187), including those that turned out to have applications. Applications are typically unintended consequences; both applied and non-applied theories of pure mathematics were developed in the same recreational spirit (see Hardy 1940; Maddy 1997: 183-205; Leng 2010: 76-99 for discussion of the norms governing mathematical theory development; see also §8.5).

To this first point, the platonist might respond by claiming that mathematical criteria of justification are irrelevant. The kind of platonism we are assuming here is one according to which only scientific criteria of justification are relevant to ontology. On this view, if mathematicians consider a mathematical belief no more justified now than before the relevant mathematics found an application, they are simply wrong about that.

This brings us to the second prong. If it is only scientific criteria of justification that matter, it is not clear that applicability within mathematics will be enough to explain reliability. If a mathematical theory M ’s applicability in natural science is enough to explain the reliability of beliefs concerning it, this must be due to the methodology of science. It is that the scientific theory involving M is confirmed by scientific standards that explains the reliability of our beliefs about M . Now, suppose a pure mathematical theory $M+$ is developed and adopted by mathematicians because $M+$ allows us to say all sorts of clever and interesting things about the mathematical objects mentioned in M . If it is only scientific standards that

matter, how exactly is the reliability of our beliefs about M supposed to be transferred to our beliefs about M^+ . The standards in virtue of which M^+ was adopted by mathematicians are distinctively mathematical, making it unclear how the scientific standards that ensure the reliability of our beliefs about M have anything to do with our beliefs about M^+ .

The above discussion suggests an alternative to Field's challenge to platonism that precludes the response offered by Colyvan: explain the reliability of the process by which mathematicians *form their mathematical beliefs*. We have seen that, even for theories that turn out to be empirically applicable, the process by which mathematicians formulate their beliefs is typically independent of any considerations of applicability. Moreover, it is doubtful that applicability to other areas of mathematics is sufficient to explain the reliability of the relevant beliefs. The prospects for platonism providing an adequate mathematical epistemology are dim. On this front, SMART fictionalism is preferable.

The third contrast I would like to make between SMART fictionalism and platonism is in their respective accounts of mathematical discourse. Platonists should endorse a literal interpretation of mathematical language. However, in Chapter 7 (§7.3), I showed that defending a literal interpretation of mathematical discourse while explaining away its more puzzling phenomenological aspects involves attributing implausible philosophical beliefs to speakers. In contrast, SMART adequately explains all the relevant phenomenological data concerning mathematical language use while remaining compatible with any or no philosophical beliefs concerning the existence of abstract mathematical objects. SMART fictionalism provides a preferable account of mathematical language use.

The fourth dimension of comparison is the compatibility of each view with a continuous analysis of mathematical and non-mathematical language. In the introduction to this thesis, I suggested that adopting platonism promises to provide a unified account of language according to which both mathematical and non-mathematical assertions aim to describe independently existing objects, and are sometimes true of them. SMART fictionalism can boast to provide an even more unified account: both mathematical and non-mathematical assertions are typically aimed at describing non-mathematical things, and are sometimes true of them.

Finally, I want to return to the challenge of accounting for our intuitions about the truth of elementary mathematical sentences. I showed above that the SMART fictionalist can meet this challenge. In the introduction, I suggested that platonism has an easy time with this, too. According to platonism, mathematical sentences truly describe a realm of necessarily existing abstract objects, so our intuition that such sentences are necessarily true is veridical. The appearance that this explains the present datum, however, is illusory. It explains why mathematical sentences are necessarily true, if they are; it does not explain why speakers typically take them to be necessarily true. To address the latter question, the platonist must claim that speakers believe that mathematical objects exist necessarily and have their properties necessarily. This is wildly implausible. Platonism's means of meeting this challenge is deficient.

The above arguments are I think sufficient to conclude that SMART fictionalism is preferable to platonism. Having established the conclusion I set out to, I will finish by discussing the prospects for extending SMART fictionalism to other areas of discourse.

The success of SMART fictionalism with respect to the philosophy of mathematics indicates that it might be successful in its application to other areas of discourse. For example, we might use similar arguments with respect to our talk of properties and relations so as to undermine semantic and metaphysical evidence often cited in support of realism about properties and relations. If Mary and John are tall, we might say:

(1) Mary and John both share the property of tallness.

(2) There is something that Mary and John are.

(1) contains a referring expression that purports to stand for a property, while (2) appears to quantify over properties. That such sentences appear true is often considered good semantic evidence of the existence of properties. However, one could argue that ordinary utterances of (1) and (2) only communicate things about the individuals Mary and John, and that we only take (1) and (2) to be true because what we typically use them to communicate is true. Successfully arguing in this way would show that the apparent truth of (1) and (2) is no evidence in favour of the existence of properties.

One potential stumbling block is the fact that some philosophical uses of (1) and (2) do purport to communicate things about properties. Many would consider such uses of (1) and (2) to provide explanations of Mary and John's similarity, and so provide metaphysical reasons for positing properties. There are two ways in which we might respond to this line of reasoning. First, we might just flat out deny that apparently mentioning properties brings with it any explanatory benefits. Perhaps similarity facts just don't demand explanations. One might appeal to Melia's

claims about mathematics here. Apparently mentioning properties *seems* explanatory because it simplifies our expression of facts about similarity; but we shouldn't make the mistake of thinking that talking about properties helps to simplify the world we are committed to. Say object x is red and object y is red. In this respect, x and y are similar. Perhaps talking about x and y sharing the property *being red* helps us express some things about x and y that would otherwise be difficult to say. But does it really make the world a simpler place to say that what x 's being red and y 's being red really amounts to is their both containing, in some sense, or otherwise being related to a third object, namely Redness. To my mind, a simpler world view would be one according to which x and y are similar in virtue of the fact that x is red and y is red, understood as a fact that concerns only x and y .

Perhaps it can be shown that talk of properties is indispensably explanatory, as is the case with mathematics; but we have seen that certain ways of speaking can be indispensably explanatory even if the sentences used in that way are false. This leaves open the possibility that an account of the explanatory role of property talk might be developed that is nominalistically acceptable. Clearly, the success of the fictionalism about mathematics I have defended in this thesis does not guarantee the success of similar projects in other areas of discourse, but it at least demonstrates that they are promising avenues of future research.

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