Linguistic realism in mathematical epistemology

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Declaration

I, Ian Hulse, declare that the work presented in this thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis.

Signed

Abstract

One project in the epistemology of mathematics is to find a defensible account of what passes for mathematical knowledge. This study contributes to this project by examining philosophical theories of mathematics governed by certain basic assumptions. Foremost amongst these is the "linguistic realism" of the title. Roughly put, this is the view that the semantics of mathematical sentences should be taken at face value.

Two approaches to mathematics are considered, realist and fictionalist. Mathematical realism affirms the existence of mathematical objects, taking much of what passes for mathematical knowledge as knowledge of such things. It faces the challenge of explaining how such knowledge is possible. The main strategies here are to appeal to the faculty of reason, to a faculty of intuition or to the faculty of sense perception. Recent examples of each strategy are considered and it is argued that the prospects for a satisfactory mathematical realism are limited.

Mathematical fictionalism does not affirm the existence of mathematical objects, claiming that mathematics is, or should be considered to be, a form of pretence. It faces the challenge of explaining how a form of pretence can discharge the roles mathematics has in empirical applications. Strategies here are to argue that mathematics is an eliminable convenience or, acknowledging that this may not be the case, that the roles played by mathematics in empirical applications are played in similar contexts by acknowledged forms of pretence. It is argued that the first strategy is not promising but that there is a version of the second that can be defended against objections.

In closing, consequences of the conclusions reached are explored and directions for future research indicated.

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Introduction

Mathematics occupies a central place in our intellectual endeavours. In our scientific and technological society, the influence of mathematics is everywhere. Mathematics deserves philosophical examination.

This study contributes to the epistemology of mathematics. It addresses the question of how we should conceive of what passes for mathematical knowledge. To cut down on the available answers, certain basic assumptions will be made, one semantic, the other ontological. Within these constraints, two contrasting approaches to what passes for mathematical knowledge will be distinguished, mathematical realism and mathematical fictionalism. Each kind of view faces its epistemological challenge. This study addresses whether these challenges can be met.

1.1 Linguistic realism

What is mathematics about? One might with some plausibility say that mathematics is about proving mathematical results, that it is about solving mathematical problems, or that it is about mathematical ideas. However, in philosophical discussions, there is a strictly semantic use of the word "about" which these replies do not address. On this use, to ask what mathematics is about is to ask what is the right semantic theory for mathematical language.

The most prominent model for semantic theories features in Tarski's writing on truth. It describes how to define truth predicates for formal languages by appeal to axioms governing the reference and satisfaction of non-logical vocabulary, together with the by now familiar axioms for classical logical operators (1933, 1944). Because mathematics can be given a strictly formal development, a truth definition of this kind lends itself particularly well to the semantic analysis of mathematical language. But clearly such a definition ought to be pursued only if the semantic notion of truth should be brought to bear on mathematics and if the logical vocabulary used in mathematics should be interpreted classically. Due to the influence of Wittgenstein (see for example his (1967)) and Brouwer (see for example his (1949)), these points are the matter of some debate. Nevertheless, we will assume that mathematical language should be treated classically and referentially after the fashion of Tarski.

Given this assumption, identifying the right semantic theory for mathematics is a matter of identifying the right interpretation for mathematical language, given the Tarskian framework. To keep track of different approaches to this, it helps to introduce some terminology. Let us say that the actual logical form of a mathematical statement is the form attributed to it by the semantic theory chosen for the language, whatever that theory may be. Let us say that the apparent logical form of a mathematical statement is that suggested by interpreting the vocabulary of the sentence used to express it by comparison to the vocabulary used in everyday talk about commonplace objects. On the comparison we have in mind, logical vocabulary is interpreted in just the same way in the two areas of discourse, and mathematical terms are treated as singular terms and predicates on a par with those used in talking about everyday objects. Thus the apparent logical form of the statement:

Something is both prime and greater than 2

is the logical form of the statement:

Something is both white and larger than John's ball

that is, an existential generalization of the conjunction of monadic and dyadic predications concerning the same object, formally, $\exists x\ [Fx \wedge R(x, a)]$.

Given these explanations, it is obvious that the apparent and actual logical form of a given mathematical sentence may differ, since the semantic theory we chose for mathematics may provide a different interpretation of mathematical language from that produced by the method we have described. When a semantic theory posits a divergence of this kind, there is a sense in which it treats mathematical language as being semantically defective, cloaking mathematical statements in linguistic expressions that are misleading as to actual logical form. This is because the method we have invoked to identify the apparent logical form of mathematical statements is also the most natural method of identifying their actual logical form. Even so, many philosophical accounts of mathematics adopt semantic theories according to which there is a divergence of this kind (a specific example is in Hellman (1989), others can be found in Burgess and Rosen (1997)). However, we will assume that mathematical language is not in this sense defective, taking it for granted that the apparent logical form of mathematical statements coincides with their actual logical form.

This assumption allows us to interpret mathematical language by analogy to the language of everyday talk about commonplace objects, as described above. Call this "taking mathematical language at face-value". One must be circumspect when applying this method. The loose talk used in everyday mathematical practice, abbreviatory idioms and the like, are potentially misleading if taken at face-value, so this method of interpretation should only be used with the claims of rigorously developed theories. For example, one should not apply this method to the claim:

There are infinitely many primes

but rather to something like:

For every prime number, there is a greater prime number.

Moreover, as Wilson (1994) indicates, the best expression of a mathematical result may not at first be clear, but may only emerge when the result can be placed in the context of a home theory. Mathematical language should thus be taken at face-value only when it is being used to express the claims of mature mathematical theories, in which the best expression of results has presumably been achieved. With these provisos, taking mathematical language at face value seems to be a satisfactory method of interpretation.

We have now put forward two substantial assumptions:

(i) Mathematical language should be treated classically and referentially, following Tarski's model for semantic theories.
(ii) Mathematical language should be taken at face-value when used rigorously to express claims of mature mathematical theories.

We will call the conjunction of these claims *linguistic realism*.[1] As we have indicated, linguistic realism is not a trivial position to take in the philosophy of mathematics. However, it coheres with empirical studies in linguistics and it enjoys widespread support in the recent literature. The position is also intuitively appealing, providing a simple, effective way of deciding what, semantically speaking, mathematics is about and it delivers opinions on this that largely agree with the opinions of mathematicians. Linguistic realism is thus an interesting philosophical position with considerable virtues, a good starting point for our investigation into how best to conceive of what passes for mathematical knowledge.

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[1] Borrowing a term introduced by Azzouni (1994, 4) for what is effectively the same point of view.

1.2 Ontological commitment

Our second basic assumption concerns the notion of ontological commitment. Quine introduced this notion by means of the following criterion:

> a theory is committed to those and only those entities to which the bound variables of the theory must be capable of referring in order that the affirmations made in the theory be true. (1948, 13-14)

After this remark, Quine indicates that this principle identifies the ontological commitments of a theory with what it says there is (*op. cit.*, 15). We will be adopting Quine's approach to ontology throughout this study, and, with one modification, will be assuming this criterion.

The modification arises because of a difficulty with taking ontological commitment to be a relation borne towards entities. Consider the "theory" that comprises just this claim:

Something is a prime number

This theory says that there are prime numbers, so it has ontological commitments. However, if these must be entities, as Quine suggests, they must presumably be certain individual prime numbers. In order to state what the ontological commitments of the theory are, therefore, we would have to say to which prime numbers it is committed. But the theory does not allow us to do this; the only information it conveys is that there are prime numbers. We are thus left in the uncomfortable position of not being able to state the ontological commitments of a theory whose commitments should be perfectly clear.

To avoid this, Quine's principle needs to be modified so that ontological commitment becomes a relation borne towards kinds of things:

> A theory is committed to the instantiation of all and only those kinds of thing, to instances of which its bound variables must be capable of referring if its claims are to be true.

From this principle, it follows that the theory "Something is a prime number" is committed to the instantiation of the kind *prime number*. We may express this loosely by saying that it is committed to the existence of prime numbers, or, more simply by saying that it is committed to prime numbers. It is thus quite clear what the ontological commitments of the theory are and, as Quine intended, they are just what the theory says there are. This is not to say that it will always be clear what ontological commitments a theory bears. But if they are hard to identify, this will be because it is not clear what information the theory conveys, not because we have picked a problematic notion of ontological commitment.

We will call this modified criterion the Quinean criterion of ontological commitment, or the Quinean criterion, for short. It is important to realise that the criterion is intended to identify a unique notion of ontological commitment. It thus presupposes that a unique notion of truth has been settled upon in respect of which the ontological commitments of any theory are to be assessed. Note that the Quinean criterion admits no degrees of ontological commitment, because a theory either entails claims about objects of a given kind or it does not. It also takes existentially quantified statements as the indicators of ontological commitment, a result of the Quinean doctrine that the existential quantifier $\exists$ is be read "there exists an x such that ..." or "there is an x such that ...".

The main alternatives to the Quinean criterion are inspired by the Meinongian view that there are non-existent objects (the golden mountain, the round square, etc.). This involves rejecting the Quinean view that the ontological commitments of a theory are indicated by its existentially quantified statements in favour of the view that existence should be expressed by a predicate of the language of the theory. This idea has been defended by T. Parsons (1980), Zalta (1988) and Priest (2000). It has also been brought to bear specifically on the ontology of mathematics by Azzouni (2004).[2] In adopting the Quinean criterion, we reject philosophical accounts of mathematics based on this approach.

As with linguistic realism, we have not proposed arguments in favour of the Quinean criterion. But we can make various observations to justify taking it as an assumption: it is assumed in much contemporary philosophy of mathematics; it is appealing in its own right; it seems intuitively preferable to Meinongian alternatives. The criterion also allows us to give a concise but informative statement of the ontological commitments of mathematics: mathematics is committed to the instantiation of those kinds that must be instantiated if mathematical claims are to be true. Since linguistic realism, demands that we take mathematical language at face-value, so that we must acknowledge mathematical kinds, this means that mathematics is committed to the instantiation of those mathematical kinds that must be instantiated if mathematical claims are to be true. As with our example above, we can express such commitment more loosely, saying either that mathematics is committed to the existence of mathematical objects or that it is committed to mathematical objects. Adoption of the Quinean criterion thus leads us to the attractive view that mathematics is committed to those mathematical objects that it says there are, numbers, sets, functions, etc.

[2] Though the precise import of this move in the case of Azzouni is not entirely clear as he denies that that there is a "fact of the matter" as to what the right criterion of ontological commitment is (1998, 2004).

1.3 Mathematical realism

Having set out our basic assumptions, we can return to the question of how we should conceive of what passes for mathematical knowledge. Clearly two approaches are available, one saying that this should be conceived of as knowledge, the other saying that it should not. We will now consider what possibilities there are for the first approach. For ease of exposition, we will use "apparent mathematical knowledge" as an abbreviation for "what passes for mathematical knowledge".

If apparent mathematical knowledge is to be taken as knowledge, it must be true, for only that which is true can be known. Assuming linguistic realism, this would involve granting the truth of claims implying the existence of mathematical objects. Assuming the Quinean criterion, this would involve acknowledging commitment to the existence of mathematical objects. A philosophical theory of mathematics that endorses our basic assumptions should thus take apparent mathematical knowledge as mathematical knowledge only if it is prepared to affirm the existence of mathematical objects.[3]

Such views can be distinguished according to whether they take mathematical existence to be independent of the mental or linguistic activities of those who engage with mathematics. The sense of dependency here needs to be spelled out with some care, for obviously any view on which there is thought or talk about mathematical objects is a view on which mathematical existence is not entirely independent of mental and linguistic activity. Mathematical objects are independent in the intended sense if and only if they do not depend for their existence on mental or linguistic activity, if and only if they have some properties regardless of what properties they are said or thought to have.

[3] Here we are ignoring the possibility that what passes for mathematical knowledge may contain cases of purely mathematical error, as it does not affect the arguments to be made.

Having clarified this, let mathematical realism be the view that there are independently existing mathematical objects. Let ontological constructivism be the view that there are mathematical objects but that these are not independent of the mental and linguistic activity of those who engage with mathematics. An account of mathematics that assumes linguistic realism and the Quinean criterion, and which portrays apparent mathematical knowledge as knowledge, must embrace either mathematical realism or ontological constructivism.

The most obvious strategy for ontological constructivism is to regard mathematical objects as items of construction. Frege argued against one version of this approach when he criticised the suggestion that numbers are private mental objects (i.e. mental objects in the minds of individual thinkers). His main objections were that this view cannot account for the objectivity of arithmetical truth (*Grundlagen* §§26-27) and that it cannot guarantee that infinitely many numbers exist (§27).[4] These objections seem decisive against this particular view of numbers, as well as the view that mathematical objects in general are private mental objects. Moreover, Frege's concern about the infinitude of numbers suggests that we can expect little better from versions of ontological constructivism that treat mathematical objects as linguistic objects; in order to ensure that there are infinitely many mathematical objects, a view of this kind would presumably have to treat the relevant linguistic objects as independently existing types, rather than tokens, in which case it has reverted to a form of realism.

A more sophisticated strategy for ontological constructivism is to regard mathematical truth as being constructed, taking the ultimate epistemic test for mathematical claims to be derivability from agreed upon mathematical conventions, or something very similar. Mathematical objects can still be

[4] One point being made here is that there would not be sufficiently many numbers to make standard arithmetical theorems true. But Frege also entertained the semantic worry that if there are not sufficiently many numbers, meaningful arithmetical language would be rendered meaningless: "10^{10}, perhaps, might be only an empty symbol, and there might exist no idea at all, in any being whatever, to answer to the name." Frege (1953, 38)

said to be items of construction on this view, in the derived sense of being objects that the mathematical conventions are about. As this kind of view does not require that mathematical objects be constructed one by one, worries about whether infinitely many of them can be produced seem misplaced. As it seems possible to argue that the objectivity of mathematical results resides in the objectivity of our agreement of the relevant conventions, it may perhaps also be possible to allay doubts about the recovery of mathematical objectivity.[5] Thus this kind of approach may perhaps withstand the Fregean critique.

However, if a view of this kind is to qualify as a version of ontological constructivism, derivability from the mathematical conventions must be put forward as a constitutive account of mathematical truth. But, as we said in the previous section, the Quinean criterion of ontological commitment presupposes that there is just one notion of truth in respect of which ontological commitments are to be assessed. Thus, unless the notion of derivability from the mathematical conventions is proposed as the notion of truth according to which the ontological commitments of all theories are to be assessed, which would be implausible, it cannot be proposed in the case of mathematics without abandoning the Quinean criterion. This would be to abandon the project of conjoining our basic assumptions with the view that apparent mathematical knowledge should be taken for knowledge.

In the present context, then, ontological constructivism does not seem very plausible. To unify our basic assumptions with the view that apparent mathematical knowledge should be taken for knowledge, some form of mathematical realism will thus have to be embraced. The initial attraction of the realist approach cannot be doubted. It is consistent with linguistic realism and the Quinean criterion of ontological commitment and so has all the virtues associated with those positions. Moreover, it allows us to

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[5]On the other hand, it may be felt that there is a fundamental tension between ontological constructivism, with its claim that mathematical objects have no properties independently of what we say or think about them, and the idea that mathematics is objectively true.

recover the outward practice of mathematics in a straightforward way. However, mathematical realism faces an epistemological challenge. It is legitimate to regard apparent mathematical knowledge as knowledge only if we are, or could be, in possession of adequately strong grounds in its favour. Mathematical realism must therefore provide good reason to think that we are, or could be, in possession of adequate grounds for knowledge of what passes for mathematical knowledge.

Note that this challenge is not the same as that associated with Benacerraf (1973), that of explaining from a realist perspective how knowledge is possible of abstract mathematical objects.[6] Accounts of mathematics that are realist in our sense do tend to regard mathematical objects as abstract. This is usually taken to mean that mathematical objects exist "outside space and time", i.e. lack spatial and temporal properties, although some realists, notably Hale (1987, Chapter 3), propose more refined criteria. However, we have not assumed that whatever mathematical objects there are are abstract, and neither have we made this an assumption of mathematical realism. Our challenge to mathematical realism thus differs from that of Benacerraf.

[6] Benacerraf argued that on a causal approach to knowledge, such as that of Goldman (1967), knowledge of abstract mathematical objects would not be possible since abstract objects cannot enter causal relations.

1.4 Mathematical fictionalism

Let us now consider what possibilities our basic assumptions allow for the view that we should not conceive of apparent mathematical knowledge as knowledge. One difficulty with this is that mathematics includes sentences like "$\forall x$ (x is a prime number $\rightarrow$ x is a prime number)" that express logical truths according to linguistic realism. To claim that no apparent mathematical knowledge should be conceived as knowledge would thus require denying that these logical truths should be thought of as items of knowledge. But this would be unacceptable; we are very often capable of recognising logical truth when we see it, and this ability allows us to attain knowledge of (some) mathematically expressed logical truths.

The view that apparent mathematical knowledge should not be counted knowledge, if it is to be proposed at all, should therefore be limited to the object-committed fragment of apparent mathematical knowledge, the body of claims that passes for mathematical knowledge and that is committed to the existence of mathematical objects according to the Quinean criterion. Call this "object-committed apparent mathematic knowledge". Such a view might be put forward on the grounds that that there are no mathematical objects or that there are not adequate grounds for belief in standard claims about them. However, without powerful metaphysical assumptions about what kinds of things could exist, it is hard to see why one might claim there are no mathematical objects without assuming or arguing that there are not adequate grounds for belief in standard claims about them. We have not made any such assumptions, and do not consider it wise to do so. So if we are to deny that object-committed apparent mathematical knowledge should be conceived of as knowledge, it will have to be on the grounds that there are not adequate grounds for belief in this body of claims. This approach thus requires claiming that object-committed apparent mathematical knowledge should not be believed.

A second condition on this approach to apparent mathematical knowledge is that it must avoid commitment to the existence of mathematical objects. The question thus arises of whether we should continue to practice mathematics, as this appears to involve putting forward claims that are so committed. In response, it could be claimed either that mathematical practice should be rejected along with belief in object-committed mathematics, or that mathematical practice should be sustained without belief in object-committed mathematics.

The wholesale repudiation of mathematical practice would be highly controversial. Nevertheless, it is worth asking what reasons there are for someone who rejects knowledge of object-committed mathematics to avoid it. One very good reason is that mathematics is of great utility in empirical science. If we were to abandon the practice of mathematics, or at least, if we were to stop practising object-committed mathematics, we would have to abandon those theories of empirical science in which object-committed mathematics appears. But this would be to abandon most contemporary empirical science, which would be a crippling expense.

Philosophical accounts of mathematics claiming that object-committed apparent mathematical knowledge should not be believed must therefore propose a conception of mathematical practice that does not require such belief. If they are to respect linguistic realism, such accounts will have to accept that when we practice mathematics we advance claims that are true only if there are mathematical objects. So they must be committed to the view that we act as if object-committed apparent mathematical knowledge is true without believing it. Since one who acts as if something is true without believing it thereby pretends that it is true, this approach must endorse some version of mathematical fictionalism, the view that mathematics is, or should be, a form of pretence.

Mathematical fictionalism is usually explained or argued for by appeal to an analogy between mathematics and literary fiction. The analogy presupposes that when we engage with literary fiction, we imagine or pretend that we believe certain claims when in fact we do not. It maintains that engaging with object-committed mathematics involves entering into forms of pretence comparable to those into which we enter when we engage with works of literary fiction. However, literary fiction does not play the roles mathematics plays in empirical applications, so one might suspect that holding mathematics analogous to literary fiction makes it unfit to play these roles. Like mathematical realism, therefore, mathematical fictionalism faces an epistemological challenge. The challenge is to explain how mathematics can discharge its roles in empirical applications even if it is a form of pretence.

We should point out here that the literature contains another kind of view that has some claim to be regarded as a version of mathematical fictionalism. On this kind of view, the comparison with literary fiction is supposed to make us think that there are mathematical objects but that, like fictional objects, their nature is fixed by what we say about them (see, e.g., Azzouni (2000)). This approach can perhaps be connected to conceptions of literary fiction, such as that of Searle (1974), according to which fictional objects are creations of the authors of the literary works in which they appear. Perhaps to this extent it can be considered a form of mathematical fictionalism. However, this kind of view must either accept the Quinean criterion of ontological commitment, in which case its claim that there are mathematical objects whose nature is fixed by what we say about them means that it is a form of ontological constructivism, or it must reject the Quinean criterion, in which case it is of no interest to us. It is therefore quite legitimate for us to take mathematical fictionalism to be the view explained above, that mathematics is, or should be, a form of pretence.

1.5 Programme

We have posed the question of how we should conceive of what passes for mathematical knowledge. Having made two basic assumptions, linguistic realism and the Quinean criterion of ontological commitment, we have argued that there are just two promising approaches to our question. Mathematical realism affirms the existence of independently existing mathematical objects and says that apparent mathematical knowledge should be conceived of as knowledge of such objects. Mathematical fictionalism rejects belief in mathematical objects but aims to preserve mathematical practice by saying that object-committed apparent mathematical knowledge is, or should be, a form of pretence. Each approach faces its epistemological challenge; mathematical realism must make plausible that we are, or could be, in possession of adequately strong grounds for knowledge of apparent mathematical knowledge, mathematical fictionalism must explain how object-committed apparent mathematical knowledge can discharge its roles in empirical applications even if it is a form of pretence. Constrained by our basic assumptions, therefore, we can address our original question of how we ought to conceive of apparent mathematical knowledge by investigating whether these challenges can be met. This will be our project.

We will first address whether there is a satisfactory realist account of the grounds of apparent mathematical knowledge. Different approaches here are to be distinguished according to where they locate the ultimate epistemic grounds upon which knowledge of this body of claims is taken to rest. *Rationalist* approaches look exclusively to the human mind, or intellect, for this, arguing that it can produce an epistemic basis for knowledge of mathematics without recourse to evidence from sense-perception. *Empiricist* approaches oppose this point of view, arguing that evidence from sense perception provides ultimate grounds for knowledge of mathematics. We will assess these approaches in Chapters 2 through 5.

Our attention will then turn to whether mathematical fictionalism can explain how object-committed mathematics can perform its roles in empirical applications even if it is a form of pretence. The first strategy for addressing this argues that such mathematics is an eliminable convenience to science, that science could be practised, with no significant loss, without assuming that this mathematics is true. We will assess this in Chapter 6. The second strategy is to argue that the roles object-committed mathematics has in science are played in other contexts by acknowledged forms of pretence. This will be considered in Chapter 7.

Before we embark on this project a terminological note is in order. During our discussion of realist approaches in Chapters 2 through 5 it will be convenient to adopt the realist way of speaking, talking of "mathematical knowledge" rather than "apparent mathematical knowledge" in order to investigate putative explanations of how we could come to posses it. Bear in mind, however, that this way of speaking is only adopted for convenience.

2

Realism and reason

It is a platitude that we arrive at prospective grounds for items of mathematical knowledge with the help of the mind, or intellect. But how far might we get with mathematical knowledge if we do nothing but exercise the mind? In the following passage, Leibniz suggests an optimistic response:

> 33. There are also two kinds of *truth*: those of reasoning, and those of *fact*. Truths of reasoning are necessary, and their opposite is impossible; those of fact are contingent, and their opposite is possible. When a truth is necessary, the reason for it can be found by analysis, by resolving it into simpler ideas and truths until we arrive at the basic ones.
> 34. Thus mathematicians use analysis to reduce speculative *theorems* and practical *canons* to *definitions*, *axioms*, and *postulates*.
> 35. And finally there are the simple ideas, which cannot be given a definition; and there are axioms and postulates – in a word, *basic principles*, which can never be proved, but which also have no need of proof: these are identical propositions, the opposite of which contains an explicit contradiction. *Monadology*[7]

Reading this passage, we can presume that when Leibniz mentions reasons for truths he is talking about considerations knowledge of which would suffice for knowledge of the relevant truths. His claim that "the reason" for a necessary truth "can be found by analysis" thus implies that

[7] This translation is from Woolhouse and Francks (1998, 272)

we can discover grounds for necessary truths, which for Leibniz includes mathematical truths, by exercising our capability for analysis. These grounds depend ultimately on both "simple ideas" and "basic principles". But as it is claimed that the latter "have no need of proof", the position developed in the passage appears to be that as epistemic (rather than conceptual) supports for mathematical truths, these grounds do not require supplementation in order to be adequate.

Leibniz thus appears to have maintained that the human mind can provide compelling evidence for belief in mathematics independently of evidence from sense perception. This striking thought is the fundamental claim of rationalist approaches to mathematical knowledge. If it is correct, it may be possible to provide a satisfactory realist account of how what passes for mathematical knowledge is known. In this and the next chapter, we will address the prospects of this approach.

2.1 Two strategies for rationalist epistemology

Different strategies for rationalist accounts of mathematical knowledge can be distinguished by reference to the cognitive capacities to which they appeal as sources of grounds for mathematical beliefs. What might these be?

Mathematical practice clearly shows that deductive inference is essential to the pursuit of mathematical knowledge. It also suggests that non-deductive inference has an epistemic role to play. However, a mathematical rationalism will not be able to view all mathematical knowledge as inferential, for if there is to be inferential mathematical knowledge, there must be non-inferential knowledge from which it is inferred (regardless of the nature of the inferences involved). As this non-inferential knowledge will have to rest on rationalistically acceptable grounds, it is by reference to the cognitive capacities invoked to supply them that we should distinguish amongst possible forms of mathematical rationalism.

As we have already seen, one cognitive capacity to which rationalists often appeal for grounds for non-inferential knowledge, and which we have already encountered in the passage from Leibniz quoted above, is the ability to analyse concepts. This allows a subject to reflect on concepts in such a way as to arrive at relations they bear to other concepts. Whilst it seems plain that we possess this ability, the rationalist argues for the more controversial view that conceptual analysis, the process of analysing concepts, provides grounds for non-inferential knowledge. It may be claimed, for example, that conceptual analysis can provide a subject who possesses concepts for colours with grounds for the conditional claim that, if something is wholly red, it cannot be wholly green. The idea here would be that the subject, simply by reflecting on what their concepts demand of a thing that is wholly red, could realise that being wholly red excludes being wholly green, so that they could know the conditional claim on this basis (provided they also understand the logical concepts it involves). If conceptual analysis does furnish us with grounds for knowledge in something like this manner, these grounds could perhaps secure non-inferential mathematical knowledge.

Even granting the epistemic effectiveness of conceptual analysis, however, one might reasonably doubt that it could provide enough non-inferential mathematical knowledge to provide premises from which the rest of mathematics could be inferred. If the conditional claim "if something is wholly red, it cannot be wholly green", can be known through conceptual analysis, this may perhaps be because it is about relations between concepts. What matters for the truth of the conditional is what things must be like if they are to be wholly red or wholly green; whether there are things that are wholly red or wholly green is beside the point. But many mathematical claims are not about relations amongst concepts. The truth of $\exists x$ (x is an even prime number), for example, requires the existence of something answering to the concept of an even prime number, not merely the obtaining of certain relations between mathematical concepts. The set theoretic axiom of infinity demands the existence of infinitely many objects, the elements of an infinite set, not just that certain set theoretic relations stand to each other in certain ways. Because they bear ontological commitments, it seems unlikely that these claims could be known on grounds (if such exist) of conceptual analysis. But clearly some such claims will have to be known this way if mathematical knowledge quite generally is to be secured on the basis of conceptual analysis.

In response to pressures such as this, rationalists sometimes appeal to a second cognitive capacity as a source of grounds for non-inferential knowledge; the ability to have intuitions. Intuition is taken to be a cognitive relation obtaining either between subjects and objects, in which case it licenses talk of intuitions *of* mathematical objects, or between subjects and propositions, in which case it licenses talk of intuitions *that* things are thus and so in the mathematical realm. In either case, intuitions are understood to be the deliverances of an epistemically effective faculty of intuition delivering non-inferential knowledge. Note that the epistemic effectiveness of intuition is intended to outstrip that of conceptual analysis, whichever conception of intuition is in play. Rationalists posit a faculty of intuition in an attempt to make grounds available for non-inferential knowledge that could not be established on other rationalistically acceptable grounds, such as grounds of conceptual analysis. Because intuition is typically posited to address the perceived limitations of other rationalistically acceptable sources of knowledge, intuition is an artefact of rationalist philosophy.

Given these two potential sources of rationalistically acceptable grounds for non-inferential mathematical knowledge, it is clear that there are three strategies for mathematical rationalism: the first strategy appeals only to conceptual analysis as source of grounds for non-inferentially known mathematics; the second appeals for this only to intuition; the third appeals to both conceptual analysis and intuition. In the remainder of this chapter we will assess the chances of success for the first strategy. We aim to find out whether the prospects are good for an account of mathematical knowledge that secures non-inferential mathematical knowledge on the grounds of conceptual analysis.

2.2 Conceptual analysis as a source of mathematical knowledge

Cognitive capacities for analysis can be understood in relation to concepts, the constituents of thoughts, or in relation to meanings, the connotations of linguistic expressions. In each case, analysis itself is the process of reflecting upon the things concerned in such a way as to bring to our attention what we understand of them. We will not be making any claims that turn on the distinction between concepts and meanings, so for simplicity we will assume that they are the same (treating thoughts as sentence meanings, and concepts as word meanings).

Our present concern is with the strategy of appealing only to conceptual analysis as a source of grounds for non-inferential mathematical knowledge. This strategy must address the worry we touched on in the previous section, that it seems initially implausible to maintain that conceptual analysis could provide grounds for knowledge of claims with ontological commitments. More specifically, it must address the challenge of explaining how conceptual analysis could deliver knowledge of mathematical objects. There is just one promising approach to this. It depends on two key ideas.

2.2.1 Analytic truth

Whether any thoughts are knowable by reflection on concepts has been the subject of much controversy. Kant introduced the term "analytic" for judgments in which what is predicated of the subject is already contained in the concept of the subject, and held that judgments of this kind could be known through analysis (*Critique of Pure Reason*, Introduction, §4). Frege modified this account, defining analytic truths as those that can be proved on the basis of logical laws and definitions (*Grundlagen*, §3). Afterwards Carnap explained analytic truths as the logical consequences of meaning postulates (1952; 1956, Chapter 1, §2). Each successive definition widens the class of analytic truths, Frege's extending Kant's because it applies also to statements that are not in subject-predicate form, Carnap's extending Frege's because the notion of a "meaning postulate" includes semantic rules over and above those that Frege would have recognised as definitions.

If we are interested in whether there are analytic thoughts, however, we must do more than just pick out a class of truths to call analytic. As Quine's famous attack on analyticity makes clear (1951, 1960), an account of the analytic must also help to show why the truths specified as analytic should be counted as items of knowledge. Recent contributions to the discussion thus focus less on delimiting the analytic truths and more on how conceptual analysis is supposed to provide grounds for knowledge of them. In this vein, Peacocke (1993) argues that in some cases the mere possession of concepts can be sufficient to there being a reflective route to knowledge, whilst Boghossian (1996) argues that knowledge of some sentences or inferences is guaranteed by an understanding of the words they contain.

The common thread to proposals like these is that the epistemic effectiveness of conceptual analysis can be seen from adequate theories of the understanding. If this is correct, then recognition that mathematical knowledge can be grounded on conceptual analysis may emerge from a clear conception of what it is to possess or understand mathematical concepts or meanings. Let us call analytic those thoughts that can be known on grounds of conceptual analysis. The idea is then that appeal to adequate explanations of what it is to possess mathematical concepts helps to show that mathematics is analytic.

2.2.2 Logicism

No consensus has emerged on whether there are analytic truths. But the debate does show that the truths deemed most likely to be analytic are the logical truths, statements like 2 is prime or 2 is not prime, all prime numbers are prime numbers, etc. This, together with the close connection between logic and mathematics, suggests that a reflective route to knowledge of mathematics might start from the analyticity of logic. This is the second key idea for rationalist attempts to ground mathematical knowledge on conceptual analysis.

Early logicists sought to capitalise on this idea by claiming that mathematics is, or is reducible to, logic. The relevant sense of reduction required that mathematical concepts be defined in terms of logical ones, so as to make all theorems of mathematics consequences of logical laws. The analyticity of mathematics would then follow from the analyticity of logic.[8]

However, attempts to follow through this strategy met with a stubborn problem: mathematics could not be reduced to uncontroversially logical systems. *Principia Mathematica*, for example, made use of the axioms of reducibility, choice and infinity, non-logical principles all (these axioms are introduced in *12 of Part I, Section B, the Summary of Part II, Section D and the Summary of Part III, Section C, respectively). Russell and Whitehead responded to this in the second edition by arguing that the axiom of reducibility is pragmatically justified (see the introduction) and by presenting theorems that depend on the axioms of choice (the "multiplicative axiom") or infinity as conditionals with those axioms as their antecedents (see the Summaries to Part II, Section D, and Part III, Section C).[9] But even if the axiom of reducibility is pragmatically justified, it remains a non-logical principle. And even though it is possible to conditionalize theorems in the way described, this seems not to be a method of reduction but rather a method of replacement: theories of concrete mathematics, set theory and real analysis, for example, assert

[8] Other kinds of reduction are possible. Concentrating on this kind of reduction, we are focussing on the logicist tradition of Frege and Russell, rather than that of Dedekind. Dedekind's approach depends on a view of the semantics of mathematical language that conflicts with linguistic realism.
[9] It should be noted concerns with the axiom of reducibility had already been noted and discussed in the introduction to the first edition, Chapter II, §§VI – VII.

the existence of their objects; such assertions cannot be adequately represented by the relevant conditional claims.

Recent logicist approaches to mathematics accept that this kind of reduction is not possible but try to salvage the epistemological project by taking a more sophisticated view of the relationship between logic and mathematics. Rather than saying that mathematics is reducible to logic, these more recent views hold that mathematics is recoverable from logic together with a moderate stock of non-logical machinery. Thus Bostock (1974, 1979) explores the possibility of understanding natural numbers, rationals and irrationals by reference to special quantifiers, as does Hodes (1984, 1990, 1991a, 1991b) for natural numbers and sets. Having given up on the kind of reducibility demanded by early logicism, such approaches can no longer argue that the analyticity of mathematics follows from the analyticity of logic. However, if it can be shown that the extra machinery involved does not itself stand in need of any justification over and above the deliverances of conceptual analysis, then the analyticity of logic might still play an important role in showing that mathematics is analytic.

2.2.3 Mathematics as an analytic extension of logic

The two key ideas for use in rationalist attempts to ground mathematical knowledge on conceptual analysis are thus (a) that the analyticity of a thought might be seen from a proper understanding of what it is to possess the concepts it involves (b) that the analyticity of logic might help to establish the analyticity of mathematics. Together these ideas suggest a strategy with which to address the challenge of explaining how conceptual analysis grounds knowledge of mathematical objects. The strategy argues that standard mathematical concepts are defined, explained or in some other way introduced by principles from which knowledge of mathematical objects can be deduced. Assuming that logic is analytic, it is then argued that mathematics is an analytic extension of logic. Call this the logicist strategy for showing that mathematics is analytic. In the rest of this chapter we will assess its chances of success.

2.3 Neo-Fregeanism

Neo-Fregeanism is a philosophy of arithmetic devised by Crispin Wright (1983) and staunchly defended by Bob Hale (1987). It claims that the natural numbers are mind independent, abstract objects, knowledge of which can be obtained from logic and Hume's Principle, the statement that:

> The number of Fs is the number of Gs if and only if there is a one-one correspondence between the Fs and the Gs.

The background logic is understood to be at least second order. "F" and "G" are schematic, occupying places to be filled by terms for entities of the kind ranged over by the second order variables (these could be properties, predicates, Fregean concepts, etc., more on this later).

The Neo-Fregeans claim that Hume's Principe (HP) can be stipulated to be true, and thus that it stands in no need of justification. They also claim that when it is stipulated to be true, HP introduces a concept for cardinal number and simultaneously fixes truth-conditions for some claims of cardinal identity (claims of the form "the cardinal number of Fs is identical to the cardinal number of Gs"). From this the Neo-Fregeans argue that HP, together with the background logic, provides all the resources necessary to the understanding and justification of arithmetic. Since they feel that HP does not stand in need of justification, they infer that arithmetic is justified in the same way as the background logic, concluding, from the assumption that logic is analytic, that so, too, is arithmetic. The Neo-Fregean account of arithmetic is thus a prime example of the logicist strategy described in the previous section.

2.3.1 The method of Fregean abstraction

Behind this view of arithmetic lies a distinctive method for introducing new ranges of abstract objects. Imagine for a moment that our universe consists of geometric figures in a Euclidean plane and that our knowledge is limited to the behaviour of these figures under mappings of the plane to itself. Much of our thinking will be concerned with the effects such mappings have on particular geometric figures, such as that mapping M shifts figure A three feet to the right, that mapping M' sends figure A to figure B, etc. Assume, though, that we lack concepts for, and entertain no thoughts about, shape.

In an idle moment, some bright spark reflects on the possibility of a new kind of object. These objects are to be correlated with geometric figures in such a way as to keep track of congruence relations amongst them. The idea is to introduce a functional expression "The shape of *x*", whose arguments are terms for geometric figures and whose values are singular terms, and to hold true identities of the form "The shape of *a* = the shape of *b*" when there is a rigid motion of the figure *a* onto the figure *b*.[10] In short, the principle:

The shape of *a* = the shape of *b* $\leftrightarrow$ there is a rigid motion of *a* to *b*

is stipulated to introduce terms for a new range of objects, for shapes, by fixing the truth-conditions of identity statements involving them. The method used here is that of Fregean abstraction.

This method cannot be applied to any relation amongst figures. To avoid some rather obvious contradictions the relation must be symmetric,

[10] That is to say, when there is a linear mapping of the Euclidean plane under which the image of a is b. If the plane is equipped with co-ordinate axes, a linear mapping sends (x, y) to (x', y') where:

$$x' = ax + by + r,$$
$$y' = cx + dy + s,$$

a, b, c, d, r and s real numbers.

reflexive and transitive; it must be an equivalence relation. Since equivalence relations partition the domains over which they are defined into disjoint equivalence classes, in which each element is related to all the others, this gives us a useful way of understanding how Fregean abstraction is supposed to work. The same new object is assigned to each element of a given equivalence class. The stipulation for shapes, for example, assigns the same shape to all figures congruent to a given figure *a* (the equivalence class containing *a*), the same shape to all figures congruent to figure *b* (the equivalence class containing *b*), and so on. In general, then, the method of Fregean abstraction takes an equivalence relation defined over previously accepted objects and introduces a new range of objects as the abstract indices of its equivalence classes. Terminology for the new objects is introduced by stipulations of the form:

$$f(a) = f(b) \leftrightarrow \mathrm{R}(a, b)$$

which fix truth conditions for some identity statements involving singular terms formed with the new functional operator *f*. We will call principles like these abstraction principles.

2.3.2 Application of Fregean abstraction to Hume's Principle

The Neo-Fregean account of arithmetic appeals to a Fregean abstraction from HP. Consider a second-order language L containing no non-logical vocabulary (assume that identity is logical) and imagine that this language is actually used for communication. The Neo-Fregeans suggest extending L to a new language L* by stipulating HP:

$$\forall F \forall G((Nx{:}Fx = Nx{:}Gx) \leftrightarrow \exists R(F\ 1 - 1_R\ G))$$

Here "Nx: Fx" is a new non-logical expression, the so-called cardinality operator, to be read "the number of Fs", and "$\exists R(F\ 1 - 1_R\ G)$" is a formal expression of L for "there is a one-one correspondence between F and G".[11] Note that one-one correspondence is an equivalence relation and that when HP is laid down it is part of what is stipulated that the terms formed by the cardinality operator are singular terms. Thus HP has the form required of an abstraction principle and so the extension of L to L* can be taken as an instance of Fregean abstraction.

The key cognitive claim of Neo-Fregeanism is that when HP is stipulated in this way, the conceptual resources necessary to understanding the arithmetical claims formulable in L* are made available. In support of this, they point out that the cardinality operator alone is a sufficient definitional basis for the arithmetical terms involved in the Dedekind-Peano axioms for arithmetic, the terms "0", "successor" and "number" (see, e.g., Wright (1998, 389)). This striking result makes clear that the only new concept that speakers of L must come to possess if they are to understand arithmetical claims is a concept for cardinal number. However, it does not provide any evidence that speakers of L could acquire possession of such a concept from the stipulation of HP. So why do the Neo-Fregeans think that this is possible?

The argument here is that HP can be taken as a meaning equivalence:

$$\ulcorner \mathrm{Nx{:}Fx} = \mathrm{Nx{:}Gx} \urcorner \text{ means that } \ulcorner \exists \mathrm{R}(\mathrm{F}\ 1 - 1_{\mathrm{R}}\ \mathrm{G}) \urcorner$$

Because speakers of L already understand sentences exhibiting the form displayed on the right hand side, the meaning equivalence puts them in a position to express what they understand by these sentences using sentences of the form displayed on its left hand side. For example they can state the thought expressed by:

There is a one-one correspondence between the concepts *cow* and *goat*

[11] What's needed here is a formal sentence expressing the existence of a relation pairing off each thing satisfying F with a unique thing satisfying G and each thing satisfying G with a unique thing satisfying F, e.g., $\exists \mathrm{R}[\forall x\ (\mathrm{F}x \rightarrow \exists y\ (\mathrm{G}y\ \&\ \mathrm{R}xy\ \&\ \forall z\ ((\mathrm{G}z\ \&\ \mathrm{R}xz) \rightarrow z = y)))\ \&\ \forall y\ (\mathrm{G}y \rightarrow \exists x\ (\mathrm{F}x\ \&\ \mathrm{R}xy\ \&\ \forall z\ ((\mathrm{F}z\ \&\ \mathrm{R}zy) \rightarrow z = x)))]$.

using the sentence:

The number of cows = the number of goats.

For the Neo-Fregeans, however, this is not a mere change in language. They argue that, by taking the terms formed by the cardinality operator in these sentences as singular terms, speakers of L can understand the claims these sentences express as numerical identities. In support of this, they argue that speakers of L who approach the meaning equivalence in the right frame of mind "reconceptualize" facts about equinumerosity as facts about numbers (see, e.g., Wright (1997, 207-209)). To be in the right frame of mind, the speaker must take the left hand side of instances of the equivalence as semantically complex symbols (not merely unstructured labels for the corresponding right hand sides) and they must not allow any reinterpretation of the vocabulary of L ~~meaning~~ during the shift to L*. According to the Neo-Fregeans, a speaker who approaches an instance of the meaning equivalence this way is forced to rethink the content of its right hand, a statement about equinumerosity, as a statement of identity about a new range of objects, the numbers. This process is said to culminate in their having a fully articulate understanding of numerical identities formed with the cardinality operator. Because it is fully articulate, it is alleged to involve possession of a concept for cardinal number. So the stipulation of HP is supposed to put speakers of L in a position from which they can acquire, by a little conceptual surgery, the only new concept they need in order to understand any arithmetical claim. Thus the stipulation of HP is said to provide a conceptual basis for arithmetic.

Let us now consider the second key claim of the Neo-Fregean view, that HP and second order logic provide a sufficient epistemic basis for arithmetic. To help establish this, the Neo-Fregeans argue that, once it has been stipulated, HP lays down truth conditions for (some) arithmetical identity claims of the form $\ulcorner Nx{:}Fx = Nx{:}Gx \urcorner$ (see, e.g., Wright's discussion of abstraction principles at (*op. cit.*, 208-209)). From this it is argued that

HP is able to provide evidence for certain arithmetical thoughts by allowing evidence for the right hand side of an instance of HP to be taken as evidence for the corresponding left hand side. In this way, for example, our evidence for the claim that the concept *proton in a hydrogen atom* is equinumerous to the concept *electron in a hydrogen atom* becomes evidence for the claim that the number of protons in a hydrogen atom equals the number of electrons in a hydrogen atom, as this follows from the statement about equinumerosity together with HP.

To make this point relevant to the justification of arithmetic, Wright demonstrates (1983, 154-169) that it is possible to derive versions of the Dedekind-Peano axioms for arithmetic from second-order logic and HP. Call the resulting theory Frege Arithmetic (FA). Assuming that HP does indeed transfer evidence from one claim to another in the way described, this derivation converts whatever evidence there is for its premises into evidence for its conclusions, i.e. for FA. So if speakers of L possess (or could possess) adequate grounds for knowledge of the premises, they could also come to know the arithmetical theory. Crucially, the premises required by the derivation are theorems of second order logic with identity. So since it follows from the Neo-Fregean assumption that logic is analytic that there are adequate grounds for all theorems of second order logic, this suggests that second-order logic and HP provide a sufficient epistemic basis for arithmetic, as the Neo-Fregeans claim.

We mentioned above that the domain over which the second order variables of HP range is a matter of some ambiguity. In Wright's original exposition it is clear that concepts of some kind are intended, but it is not clear whether these should be understood as Fregean concepts (the things referred to by predicates), as constituents of thoughts or perhaps as something else.[12] The ambiguity seems to be resolved in a later discussion during which Wright refers to the entities in question as

[12] Wright (1983, §iv) discusses extending reference to predicates. But he does not in fact endorse the idea, the point of the discussion is to argue that this move would not threaten the Neo-Fregean conception of objects.

Fregean concepts, but all he really means by this is that, like Fregean concepts, the things in question are extensionally individuated (1997, 208, *n*.13). Thus the ambiguity remains.

This is important because the very content of HP, and therefore its suitability for the role intended in the Neo-Fregean account of arithmetic, depends upon the range of its second order variables. For suppose we took these to range over predicates. Then HP would imply that, if two predicates were satisfied by the same number of things, there would have to exist a relational expression satisfied by unique pairings of the things satisfying the predicates. Since we do not consider statements of numerical identity to depend on language in this way, it would then not be possible to maintain that HP introduces a concept for number. For the sake of definiteness, then, we shall assume that the second order variables range over Fregean concepts. This would seem to avoid the problem associated with predicates, though, of course, it may bring with it problems of its own.

2.4 Problematic cardinal numbers

Given F, an arbitrary sortal concept, there is a one-one correspondence between the Fs and the Fs. If the Neo-Fregeans are right that HP introduces a concept for cardinal number, it follows from this and HP that, for any such concept F, the cardinal number of Fs is identical to the cardinal number of Fs. Then, by existential generalisation, we have that there exists x such that x is identical to the cardinal number of Fs. Thus Neo-Fregeanism must endorse the principle that there is always a cardinal number of things satisfying an arbitrary concept, formally:

$$\forall \mathrm{F}\ \exists x\ (x = \mathrm{N}y{:}\ \mathrm{F}y)$$

This is very similar to the principle of set existence:

$$\forall \mathrm{F}\ \exists x\ (x = \{y{:}\ \mathrm{F}y\})$$

which says that there is always a set of things satisfying an arbitrary sortal concept. As is well known, this principle leads to the existence of inconsistent sets such as the Russell set. By virtue of the similarity, it is natural to wonder whether the former principle suffers comparable difficulties. We are thus lead to ask whether FA implies the existence of cardinal numbers which are inconsistent, or in some other way problematic.

Several commentators have pointed out that FA is consistent relative to classical analysis (e.g. Burgess (1984); Hodes (1984); Wright (1997)). Working under the assumption of mathematical realism, this suffices for the consistency of FA as both classical analysis and the set theory required for the relative consistency proof can be taken as true. Since the Neo-Fregeans originally intended to establish the analyticity of arithmetic without assuming mathematical realism, they would prefer not to rely on this argument. However, it is hard to see what else guarantees that FA is consistent. Wright points out that neither Russell's paradox (1983, §19, 155-158), nor Cantor's paradox (*op. cit.*, *n*.5, 185-187), can be reproduced in FA. But this shows merely that well-known routes to contradiction in naïve set theory cannot be imitated in FA; it does not establish that FA is consistent. Since there does not seem to be anything else to put forward in favour of the consistency of FA, it is thus not obvious how the absence of inconsistent cardinals might be ascertained without weakening the original Neo-Fregean argument.

Assuming there are no inconsistent cardinals in FA, might there be cardinals whose existence is in some other way problematic? Boolos (1997) suggests that there are. Substituting the concept *self-identical* into the principle:

$$\forall \mathrm{F}\ \exists x(x = \mathrm{N}y\text{: } \mathrm{F}y)$$

yields $\exists x\ (x = \mathrm{N}x\text{: } x{=}x)$, so in FA the cardinal number of self-identical things exists.[13] But, as Boolos points out (*op. cit.* 260), the existence of this number is not compatible with set theory. In his view, this suggests that HP is not true, and so casts doubt on the Neo-Fregean claim that it is analytic. What makes matters worse is that this is not an isolated case. Substituting the concept *ordinal number* into the Neo-Fregean principle of cardinal existence, we find that FA is committed to the existence of the cardinal number of ordinal numbers. Substituting the concept *cardinal number* we find a commitment to the cardinal number of cardinal numbers. But the existence of these cardinal numbers is not permitted by set theory. It thus appears there is a deep disagreement between set theory and FA. If Boolos has things aright, this gives reason to doubt that HP is analytic.

The Neo-Fregeans might reply to this by arguing that the incompatibility of set theory and FA is not problematic. However, this does not seem a very plausible response. The conflicts between FA and set theory show that

[13] Here we are assuming that the concept *self-identical* is the concept $x = x$.

they cannot both be true. Since we are currently working under the assumption of mathematical realism, and since this implies that standard mathematical theories such as set theory are true, it follows that FA is false. This result is nothing short of catastrophic for the Neo-Fregean account of arithmetic because the only plausible explanation of the falsity of FA is the falsity of HP (the theory's sole non-logical axiom). If HP is false, then its truth cannot be stipulated as a means of introducing a concept for cardinal number. But it is essential to Neo-Fregeanism that HP can be stipulated, as it is this, together with the fact that Peano arithmetic can be derived in second order logic from HP, that is meant to show that arithmetic is analytic. So, given mathematical realism, the incompatibility of FA and set theory shows that Neo-Fregeanism fails to establish the analyticity of arithmetic.

An objection can also be made here without assuming mathematical realism. Boolos draws our attention to the fact that FA conflicts with an accepted mathematical theory, namely the theory of transfinite cardinal numbers. To argue for the analyticity of arithmetic from the (alleged) analyticity of FA it would thus be necessary to reject a mathematical theory that has hitherto been accepted. Perhaps the Neo-Fregeans will be willing to take this step. Perhaps they will claim to have discovered that the standard theory of transfinite cardinal numbers should be rejected. But although this response is possible, it does not as it stands seem very reasonable; to reject the standard theory of transfinite cardinal numbers without independent argument would be dogmatic.

If the Neo-Fregeans are to meet either of these objections, they must find some way of reconciling FA with set theory, some way of stopping the theories from delivering contradictory claims about cardinal existence. To do this they might argue that there is a well-motivated revision of the standard set theoretic account of cardinal number under which the non-existence of the problematic cardinals cannot be proved in set theory. Or they might argue that there are reasonable revisions of FA under which the existence of the problematic cardinals can no longer be proved in FA. We will consider each possibility in turn.

Rumfitt (2001) argues that the conflict between FA and set theory over the existence of the cardinal number of self-identical things disappears if the set theoretic definition of the cardinal numbers as initial ordinals is rejected. [14] He also suggests that this could be justified on the basis of Frege's argument that cardinals should not be identified with ordinals (the former being introduced to answer questions concerning how many things there are, the latter being introduced to provide answers to questions concerning the position of things in orderings). If Rumfitt is right about this, then perhaps there is a well-motivated revision of set theory that blocks set theoretic proofs of the non-existence of the problematic cardinals.[15]

Revising the set theoretic treatment of cardinals this way certainly blocks the proof Rumfitt describes for the conclusion that there is no cardinal number of self-identical things (*op. cit.*, 516-517). Starting from the definition of cardinal numbers as initial ordinals, this infers, via the axiom of replacement, that if there is a cardinal number of Fs, there is a set of Fs.[16] From the assumption that there is a cardinal number of self-identical things, it would follow from this conditional that there is a set of self-identical things, whence, by the axiom of separation, that there is a set of

[14] The discussion of Rumfitt (2001) focuses almost exclusively on the conflict between FA and set theory concerning the number of self-identical things. He does point out that the existence of a cardinal number of all ordinal numbers conflicts with the Burali-Forti paradox. But the conflict he envisages arises because set theory proves the existence of a set of all ordinal numbers if it assumes that the number of self-identical things exists, so here again it is really the existence of the cardinal number of self-identical things that is at stake. From our point of view, this emphasis is not justified because various disagreements between FA and set theory over problematic cardinals can be generated on their own, independently of claims about other potentially problematic cardinals. As Neo-Fregeanism will be subject to the objections described in the text if any one of these disagreements resists principled elimination, it follows that all the potentially problematic cardinals must be considered.

[15] In fairness to Rumfitt, we should say that he only suggests this defence of Neo-Fregeanism, he does not go into it in great detail.

[16] The axiom of replacement states that, given a function f whose range is a set A, there exists a set containing exactly the images under f of the elements of A. Given a concept F, if the Fs are mapped one-one to an ordinal, α, by a function, g, then, since α is a set, the Fs form a set by virtue of being the image under the inverse of g of α.

sets not members of themselves.[17] But as we know from Russell's paradox, there is no such set, so there can be no cardinal number of self-identical things. Clearly this proof cannot be carried out if cardinals are not treated as initial ordinals.

However, the proposed revision of set theory does not block all set theoretic proofs of the non-existence of the problematic cardinals. There are two reasons for this. The first is that Frege's observation that cardinals cannot be identified with ordinals does not motivate rejecting the definition of cardinals as initial ordinals. Frege's observation, if accepted, stops us taking it as a genuine identification of cardinals with ordinals, but it does not stop us taking it as a description of a model of the cardinals in the ordinals. Thus we can agree with Frege that cardinals are not the same kind of thing as ordinals and yet derive the non-existence of the cardinal number of objects in precisely the way Rumfitt describes.

The second reason Rumfitt's move does not suffice is that there are set theoretic proofs of the non-existence of the problematic cardinals that do not rely on the definition of cardinals as initial ordinals. The cardinal number of Fs can also be defined as the collection of sets of least rank that are equinumerous to the least ordinal with which the Fs are in one-one correspondence (this definition is attributed to Scott). Provided there are sets in one-one correspondence with the Fs, it follows from the axiom of foundation that this constitutes a set. So by the axiom of replacement, if there is a cardinal number of Fs, then there is a set of Fs.[18] Assuming that the cardinal number of self-identical things exists we can now derive a contradiction as before, to conclude that no such number exists. Defining cardinals as initial ordinals is thus not essential to set theoretic proofs of the non-existence of the problematic cardinals. In fact, such proofs do not

[17] The axiom of separation states that, given a unary predicate $\phi(x)$ and a set A, there exists a set containing the elements x of A such that $\phi(x)$.

[18] Note that the definition of cardinals as initial ordinals requires the axiom of choice, or some equivalent to show that it is adequate. This is because the definition requires that there be a cardinal for every set, so that every set must be in one-one correspondence with an initial ordinal, so that every set can be well-ordered. This is the well ordering principle, which is equivalent to the axiom of choice.

demand any particular definition of cardinals in terms of sets. Set theoretic proofs of the non-existence of the problematic cardinals will be possible whenever the chosen definition implies the conditional claim that, if there is a cardinal number of Fs, then there is a set of Fs. This, surely, is a condition on the adequacy of such definitions, as it follows from the basic Cantorian principle that a plurality of things is a set if and only if it has a cardinal number.

A well-motivated revision of set theory that will block proofs of the non-existence of the problematic cardinals therefore seems unavailable. So let us consider the possibility of reconciling FA with set theory by arguing that FA does not imply the existence of the problematic cardinals. The only plausible way to do this is to argue that the concepts of which the problematic cardinals are supposed to be the cardinals cannot be substituted into the principle governing existence for cardinal numbers:

$$\forall F\ \exists x\ (x = Nx{:}\ Fx)$$

On the Neo-Fregean approach, the concepts that can be substituted into this principle are those that occur in true instances of HP. Such concepts are those that enter into one-one correspondences with other concepts. Note that the identity relation sets up such a correspondence between a concept and itself whenever its instances are in the field of the identity relation. This means it can be denied that a concept can be substituted into the principle only if it can be denied that its instances are in the field of the identity relation. Thus, if the Neo-Fregeans are to deny that the existence of problematic cardinals follows from FA, they must deny that the instances of concepts of which they are supposedly the numbers are not in the field of the identity relation. Is this a position the Neo-Fregeans could plausibly adopt? We will argue that it is not.

Consider first concepts for ordinal numbers. Instances of such concepts are ordinal numbers. Our best accounts of these either represent them as sets or identify them with sets. In either case, the axiom of extensionality guarantees that the sets chosen for the ordinals are in the field of the identity relation, identical if and only if they contain exactly the same elements. To deny that ordinals are in the field of the identity relation, the Neo-Fregeans would have to claim that set theoretic treatments of the ordinals misrepresent them in this respect. This would be extremely implausible.

Consider now concepts for cardinal numbers. A key claim of the Neo-Fregean argument is that HP introduces a concept for cardinal number, expressed by the cardinality operator in FA, which is such that sentences in FA formed by placing terms involving the cardinality operator on either side of the sign for identity are genuine numerical identities. Clearly it is essential to this position that instances of the Neo-Fregean concept for cardinal number are in the field of the identity relation. This point also arises in connection with the proof of Frege's Theorem. This requires existential generalisation into positions in identity statements occupied by terms formed by the cardinality operator, such as in the proof of the existence of 0 in which $\exists y(y = Nx: \neg(x = x))$ is derived from $Nx: \neg(x = x) = Nx: \neg(x = x)$. Using existential generalisation in this way is legitimate only if these identity statements express genuine claims of numerical identity, which requires that terms formed with the cardinality operator refer to things that are in the field of the identity relation.

Finally, consider the concept *self-identical*, that is to say the concept $x = x$. The instances of this concept are precisely those things that are in the field of the identity relation so for the Neo-Fregeans to say otherwise would be absurd. Seemingly, then, it is not possible to reconcile FA with set theory by arguing that FA does not imply the existence of the problematic cardinals. This would involve denying that the instances of the concepts from which their existence can be derived are not in the field of the identity relation, which is not possible, for the reasons explained above.

The issue we raised at this beginning of this section was whether FA implied the existence of problematic cardinals. Building from the case put forward by Boolos, we have argued that it does. The Neo-Fregeans are committed to the existence of a cardinal number of things satisfying every concept the instances of which are in the field of the identity relation. We argued that this commits them to the existence of a cardinal number of cardinal numbers, to a cardinal number of ordinal numbers and to a cardinal number of self-identical things. These cardinals are problematic because they lead to contradictions when FA is conjoined with set theory. As we found no grounds for thinking that there is a well-motivated revision of set theory that eliminates the conflict, this means that FA is false and so cannot stipulated to be true. Neo-Fregeanism therefore provides no compelling grounds for thinking that arithmetic is analytic.

2.5 Tennant's logicist account of arithmetic

Tennant's account of arithmetic (1987, Chapters 20, 25; 1997a; 1997b, Chapter 9) shares much in common with the Neo-Fregean account. It is inspired by Frege's analysis of cardinal number, it aims to establish the analyticity of arithmetic and it attempts to do so by deriving a version of arithmetic from principles alleged to be constitutive of arithmetic concepts. Despite these similarities, the accounts are quite different. Working in a second order free logic, not a classical logic, Tennant grounds his version of arithmetic on a system of introduction and elimination rules for number theoretic primitives, not a Fregean abstraction from HP. Most importantly for us, Tennant's version of arithmetic does not imply the existence of the problematic cardinals discussed in the previous section. This means that the claim that this theory is analytic cannot be objected to on grounds of incompatibility with set theory. So for all we have said so far, Tennant's arguments may show that arithmetic is analytic.

When working in a free logic, the classical assumption that every well-formed singular term refers is not made. This means inferences from $F(t)$ to $\exists x F(x)$ (t a well-formed singular term) are not in general valid since there may not be anything answering to t. According to Tennant there is a clear advantage to this approach when investigating matters of mathematical ontology:

> Philosophical discussion about the existence of numbers should be conducted against the background of a logic that is absolutely neutral on the question whether any particular term happens to denote. The whole point is to examine the foundations of our commitment to numbers; and to identify, with the help of our logical techniques, the precise juncture at which explicit existential commitment to numbers is indeed incurred. (1997a, 311)

In other words, classical assumptions about singular terms have the potential to mislead us with respect to our true numerical commitments, but we can guard against this eventuality by working in a free logic. On these points, Tennant is surely correct.

Tennant's free logic is a system of natural deduction, so the principles governing the number theoretic primitives are presented as introduction and elimination rules.[19] The number theoretic primitives are 0, the cardinality operator, Nx: Fx, and the successor operator, $s(x)$.[20] The rule for introduction of 0 makes use of the idea that 0 is the number of any unsatisfied concept. A concept is unsatisfied if assumptions to the effect that there is something satisfying it are contradictory. In a free logic, Fa and $\exists!a$ (defined in free logic as $\exists x$ (x = a)) jointly assert that there is something satisfying F. Accordingly, Tennant's rule for the introduction of 0 allows us to derive Nx: Fx = 0 if the assumptions Fa and $\exists!a$ lead to a contradiction, graphically:

0-introduction

$$\dfrac{\begin{array}{cc} \overline{\mathrm{F}a}^{(i)} & \overline{\exists! a}^{(i)} \\ \vdots & \\ \perp & \end{array}}{\mathrm{N}x : \mathrm{F}x = 0}\ (i)$$

Note that this is a rule of proof, the indexed bars over the assumptions Fa and $\exists!a$ indicating that they are discharged as Nx: Fx = 0 is derived. Note too that a must be arbitrary, i.e. that the derivation of $\perp$ prior to the step in which 0 is introduced must not depend on assumptions containing a other than Fa and $\exists!a$.

[19] Tennant (1978, Chapter 4) presents a natural deduction system of classical logic, which he later modifies to arrive at his free logic (*op.cit.*, Chapter 7).

[20] In what follows, we adopt Tennant's presentation of his rules for the number theoretic primitives (1987, 290-292) with minor changes to the symbolism.

Tennant's elimination rule for 0 complements this introduction rule. If we can prove that Nx: Fx = 0, then given the introduction rule we must have been able to show that the assumptions Fa and $\exists!a$ are contradictory. It follows that if we can prove Fa and $\exists!a$, we must derive the contradiction to which those assumptions lead. What this shows is that it is not possible to assume both the premises and the conclusion of the introduction rule, since by doing so we make contradictory assumptions. Accordingly, Tennant proposes that the elimination of 0 be a rule making clear the incompatibility of Fu, $\exists!u$ and Nx: Fx = 0, u a variable term, graphically:

0-elimination

$$\frac{\mathrm{N}x\colon \mathrm{F}x = 0 \qquad \mathrm{F}u \qquad \exists!u}{\perp}$$

Note that this is a rule of inference, the assumptions not being discharged in the transition to $\perp$.

Tennant's introduction and elimination rules for the cardinality operator are suggested by HP. A rule equivalent of HP would allow us to infer Nx: Fx = Nx: Gx from the equinumerosity of F and G, and vice versa. Such a rule would treat facts about the identity of cardinal numbers as always indicative of facts about one-one correspondences amongst concepts, and facts about one-one correspondences amongst concepts as always indicative of facts about the identity of cardinal numbers. Tennant's rules for the cardinality operator also take the identity of cardinal numbers to be intimately related to one-one correspondences on concepts. But to respect the free logic background, Tennant's rules treat facts about equinumerosity amongst concepts as indicative of facts about cardinal numbers only when assumptions implying the existence of the relevant cardinals have already been established. Accordingly, the introduction rule for the cardinality operator allows us to infer from the assumptions Nx:

Fx = t and Rxy[Fx 1 – 1 Gy] (the Fs and the Gs correspond one-one by R) to the conclusion that Ny: Gy = t, graphically:

Nx: Fx-introduction

$$\frac{\mathrm{N}x\text{: F}x = t \qquad \mathrm{R}xy[\mathrm{F}x\ 1-1\ \mathrm{G}y]}{\mathrm{N}x\text{: G}x = t}$$

The corresponding elimination rule allows us to infer a conclusion ϕ from the assumption Nx: Gx = t, when we have shown that ϕ follows from Nx: Fx = t and Rxy[Fx 1 – 1 Gy]:

Nx: Fx-elimination

$$\dfrac{\mathrm{N}x : \mathrm{G}x = t \qquad \begin{matrix} \overline{\mathrm{N}x : \mathrm{F}x = t}^{(i)} \quad \overline{\mathrm{R}xy[\mathrm{F}x\ 1\text{-}1\ \mathrm{G}y]}^{(i)} \\ \vdots \\ \phi \end{matrix}}{\phi}\,(i)$$

Note that this is a rule of proof allowing us to discharge the assumptions Nx: Fx = t and Rxy[Fx 1 – 1 Gy] when we draw the conclusion Nx: Gx.

Tennant's rules for the successor operator $s(x)$ are similar to those for the cardinality operator. However, for the number of Gs to be the successor of the number of Fs, one-one correspondence between the two concepts is not at stake, but rather one-one correspondence between the Fs and all the Gs save one. Tennant uses the expression "Rxy[Fx 1 – 1 Gy, t]" for this (giving introduction and elimination rules for it in the course of his discussion of correspondences between concepts (1987, 276-281)). His rules for the introduction and elimination of successor as follows:

$s(x)$-introduction

$$\frac{\mathrm{N}x{:}\ \mathrm{F}x = t \qquad \mathrm{R}xy[\mathrm{F}x\ \ 1-1\ \mathrm{G}y,\ r]}{\mathrm{N}x{:}\ \mathrm{G}x = s(t)}$$

$s(x)$-elimination

$$\frac{\mathrm{N}x : \mathrm{G}x = s(t) \qquad \begin{array}{c} \overline{\mathrm{N}x : \mathrm{F}x = t}^{\,(i)} \qquad \overline{\mathrm{R}xy[\mathrm{F}x\ \ 1\text{-}1\ \ \mathrm{G}y, a]}^{\,(i)} \\ \vdots \\ \phi \end{array}}{\phi}\,(i)$$

As with the rules for the cardinality operator, $s(x)$-introduction is a rule of inference and $s(x)$-elimination is a rule of proof. These rules for the successor operator complete Tennant's introduction and elimination rules for number theoretic primitives.

We have already stated that Tennant uses his rules to help derive a version of arithmetic in which the existence of the problematic cardinals discussed in the previous section cannot be proved. But given the background of free logic, how can the rules presented above prove the existence of infinitely many natural numbers? The answer is that they cannot. Besides these rules for introduction and elimination of the arithmetical primitives, two further rules are required.

One of these is supplied by Tennant's free logic. Being a free logic, F(t) does not in general imply t exists, but according to the denotation rule, this implication does hold when the predication is atomic (see Tennant (1978, 168):

Denotation rule

$$\frac{\phi t}{\exists ! t}$$ [21]

This rule allows Tennant to prove the existence of 0 as follows:

$$\dfrac{\dfrac{}{\neg(a = a)}^{(1)} \qquad \dfrac{\dfrac{}{\exists ! a}^{(1)}}{a = a}}{\dfrac{\bot}{Nx : \neg(x = x) = 0}}\,(1)$$

Here 0-introduction is applied to the concept ¬(x = x) to prove that Nx: ¬(x = x) = 0, from which it follows by the denotation rule that 0 exists.

The other rule require to prove the existence of the natural numbers is not supplied by the logic, so like the rules governing the arithmetical primitives Tennant presents it as a stipulation. The rule states that if the number of Fs exists and if the Fs are equinumerous with the all the Gs save one then the number of Gs exists, graphically:

Ratchet principle

$$\frac{\exists !\mathrm{N}x\text{: F}x \qquad Rxy[\mathrm{F}x \ 1-1 \ \mathrm{G}y, t]}{\exists !\mathrm{N}x\text{: G}x}$$

This principle gives Tennant's system the capacity to prove the existence of the successors of cardinal numbers whose existence has already been established. For example, it can be used to prove that the successor of 0 exists, i.e. that ∃x (x = s(0)) (see Tennant (1987, 293-294)). Ultimately, it is this capacity that allows Tennant to prove that the natural numbers

[21] It may seem that this rule could not possibly be valid, as, if we put "$x = x$" for "ϕx" and "the largest number" for "t", it allows us to infer that the largest number exists. However, it is not the rule that is at fault in such reasoning, but rather the premise that the largest number = the largest number. This is false precisely because the largest number does not exist.

exist. For each term reached by iterated application of the successor operator to 0, it allows proof that there exists a cardinal number answering to that term (given that the existence of 0 has already been proved) (*op. cit.*, 293-294). These can then be taken as the natural numbers. For having already given a treatment of ancestrals of one-place functions, in particular introduction and elimination rules for an expression intended to mean "*x* is only finitely many steps of *f* away from *y*" (*op. cit.*, 281-282), Tennant can define the natural numbers as those things that can be reached from 0 by finitely many applications of the successor operator.

Tennant's proofs of the existence of the natural numbers are given during the course of a detailed proof of rule equivalents of the Dedekind Peano axioms for arithmetic (*op. cit.*, Chapter 25, 275-300). Let us call this version of arithmetic FA_T. As we have already stated, FA_T differs from Wright and Hale's arithmetical theory FA in that it does not allow derivation of the existence of problematic cardinals such as the cardinal number of self-identical things. We are now in a position to see why this is so. The crucial point is that cardinal numbers can be proved to exist in FA_T only when they can be represented as n^{th} successors of 0. This is because proofs of numerical existence in FA_T depend ultimately on the existence of 0 and the ratchet principle. Once the existence of 0 as a natural number is established, the ratchet principle transmits existence and "naturalness" up through the progression of cardinals as far as can be reached from zero in any finite number of applications of $s(x)$, but proofs of the existence (or naturalness) of other cardinal numbers are not possible. A proof of the existence of the cardinal number of self-identical things is thus not available because although FA_T allows the formation of terms that refer to this number, if they refer at all, it does not provide a method by which the cardinal number of self-identical things can be represented as an n^{th} successor of 0.

2.6 Stipulation, conceptual analysis and the rules of FA_T

We argued against the analyticity of FA because it implies the existence of cardinals that do not exist, according to set theory. Tennant's claim that FA_T is analytic cannot be rejected on that count since FA_T does not imply the existence of the problematic cardinals. But this does not yet mean that FA_T is analytic. We will now consider whether it is.

A theory is analytic, in our sense, if it is knowable on grounds provided by conceptual analysis. Tennant's development of FA_T thus makes plausible that arithmetic is analytic if it provides reason to believe that conceptual analysis provides adequate grounds for arithmetical claims. The items Tennant puts forward as candidates for such grounds are his derivations of rule equivalents of the Dedekind-Peano axioms. As these derivations have no premises, they constitute adequate grounds for belief in their conclusions if the rules for FA_T are valid. Thus, if the rules can also be formulated from conceptual analysis of our usual arithmetical notions, then the derivations will constitute analytic grounds for arithmetical claims. But does Tennant provide sufficient reason to think that these two conditions obtain? In what follows, we argue that he does not.

2.6.1 Stipulation and validity

Tennant's approach to the validity of the rules of FA_T is comparable to that of Wright and Hale to the truth of HP. The Neo-Fregeans viewed HP as an explanation of a concept for cardinal number, and argued on this basis that it is a stipulation whose truth is guaranteed; likewise, Tennant views the rules of FA_T as introductive of primitive arithmetical notions, taking this to show that they are stipulations the validity of which is guaranteed. In each case, the underlying thought is that since we can explain or introduce whatever concepts we like, the stipulation of these principles, whose role is to explain or introduce concepts, is sufficient to guarantee their truth or validity without any further epistemic support. If this is correct in the case of the rules for FA_T, then clearly they must be valid.

We will argue that this line of argument does not secure the validity of the rules of FA_T. More specifically, we will argue that the validity of 0-introduction and the ratchet principle cannot be established in this way. We will not at this stage challenge the idea that these rules might be (partly) constitutive of our usual arithmetical concepts, nor the related idea that they could be used to help introduce our arithmetical concepts. The argument of this section will rather be that 0-introduction and the ratchet principle perform a further function that puts their validity beyond whatever epistemic support may come from their stipulation.

For any principle P, let us say that P is stipulatively valid under the following conditions:

> P is stipulatively valid if and only if either it can be made valid by stipulation, or good reason to believe it valid can be had from stipulation.

Similarly, let us say that a principle is stipulatively true if and only if either it can be made true by stipulation, or good reason to believe it true can be had from stipulation. Given principles describing matters of convention, points of etiquette, rules from the Highway Code, etc., it can sometimes be plausible to maintain that they are stipulatively true or valid. However, it is not plausible to regard principles this way when they describe independently existing matters of fact. If John stipulates that his son is to be his heir, we might say that it is stipulatively true that John's son is his heir, for John's stipulation helps to make this the case. But we would not say that it is stipulatively true that John's son will be the next winner of *Stars in Their Eyes*, for no comparable stipulation on John's part can help to bring about this state of affairs. Similarly, we would never say that it is stipulatively true that *a* is the first counter example to Goldbach's Conjecture. We might first establish that there is a counter example to

Goldbach's Conjecture, and then resolve to think of this as *a*. But the stipulation involved here would be a conditional governing our use of the singular concept *a*, something like "If there is a counter example to Goldbach's Conjecture, let *a* be the first such counter example". Since a stipulation of this kind could not make it true that *a* is the first counter example to Goldbach's Conjecture, nor provide good reason to think that this is the case, the claim that *a* is the first counter example to Goldbach's Conjecture is not stipulatively true.

Having introduced the idea of stipulative validity, we will argue that 0-introduction and the ratchet principle are not stipulatively valid. Recall that the rule of 0-introduction is as follows:

$$\dfrac{\begin{array}{ccc} \overline{Fa}^{(i)} & & \overline{\exists!\, a}^{(i)} \\ & \vdots & \\ & \bot & \end{array}}{Nx : Fx = 0}\ {\scriptstyle (i)}$$

Because this rule may be used to prove that 0 exists, it cannot be claimed simply to serve as a means of introducing a concept for 0, for whilst it might plausibly be said to introduce a concept for 0, it must also be said to carry with it a substantive theory about the existence of mathematical objects. From the point of view of mathematical realism, it is an independent matter of fact whether such theories are true because mathematical objects do not depend for their existence on thought or talk about them (see section 1.3). 0-introduction thus cannot be stipulatively valid.

To amplify this point, compare the rule of 0-introduction with the following, closely related rule:

0-introduction*

$$\dfrac{\exists!\mathrm{N}x:\mathrm{F}x \qquad \begin{matrix}\overline{\mathrm{F}a}^{(i)} \quad \overline{\exists!a}^{(i)} \\ \vdots \\ \bot\end{matrix}}{\mathrm{N}x:\mathrm{F}x=0}\,(i)$$

It is not possible to derive the existence of an object answering to the concept of 0 if we introduce the concept with this rule. We may derive a contradiction from the assumptions $\exists!a$ and $\neg(a = a)$ and so introduce the claim Nx: $\neg(x = x) = 0$, as Tennant does in deriving the existence of 0, but this remains conditional on the premise $\exists!$Nx: $\neg(x = x)$:

$$\dfrac{\exists!\mathrm{N}x:\neg(x=x) \qquad \dfrac{\overline{\neg(a=a)}^{(1)} \qquad \dfrac{\overline{\exists!a}^{(1)}}{a=a}}{\bot}}{\mathrm{N}x:\neg(x=x)=0}\,(1)$$

The rule of 0-introduction* thus does not allow us to prove that Nx: $\neg(x = x) = 0$, rather it allows us to prove a derived rule, which when expressed as a conditional claim says that if there is such a thing as the number of non-self-identical things, then that thing is 0. As this expresses a resolution to use a given term in a certain way, without making any ontological demands, 0-introduction*, unlike 0-introduction, can plausibly be regarded as stipulatively valid.

It is interesting to note that Tennant himself seems to be committed to viewing matters with the rule of 0-introduction in this way. One of the reasons he puts forward against the Neo-Fregean use of Hume's Principle is that it is not acceptably formulated from the perspective of free logic:

> But is the Humean identity (*N*) [HP] really analytic? Not quite; in due course its proper analytic core will be extracted in the form of two conditionals, (*N*1) and (*N*2)* below. The reason why (*N*) is not analytic as it stands at present is that it has not been suitably qualified within the context of a free logic. (1997a, 311)

Later on, when (N1) and (N2)* are set out (*op. cit.*, 312-313), it becomes clear that the qualification deemed necessary for HP is that it be made conditional on the existence of cardinal numbers (this is why Tennant's introduction rule for the cardinality operator includes a premise which implies the existence of the number of Fs). What is baffling about this, however, is that if this kind of existential conditioning is required for HP to be acceptable, then the same kind of conditioning should be expected of whatever rule is proposed for the introduction of 0. Our rule 0-introduction* is conditioned in just this way: Tennant's rule 0-introduction is not. Seemingly, then, Tennant employs a double standard here.

How might Tennant respond to our charge that 0-introduction cannot be viewed as stipulatively valid? We might hope to find answers to this in the passages in which Tennant discusses the possibility of existentially conditioning 0-introduction. However, Tennant only considers the following rule as a suitably conditioned alternative (*op. cit.*, 323-324):

0-introduction$

$$\dfrac{\exists!0 \qquad \begin{array}{c} \overline{Fa}^{(i)} \qquad \overline{\exists!a}^{(i)} \\ \vdots \\ \bot \end{array}}{Nx : Fx = 0}\ (i)$$

Against the acceptability of this rule, Tennant argues (*op.cit.*, 324) that someone who accepted it and yet denied that 0 exists would (a) be unable to distinguish correct arithmetical claims from incorrect arithmetical claims (since all arithmetical claims would be conditional on the existence of 0, which would be regarded as false) (b) would be left needing an explanation of why he develops his mathematics on the basis of an acknowledgedly false assumption and (c) would be embarrassed by having his applications of arithmetic depend on this assumption. Thus Tennant concludes that it would be incoherent to take 0-introduction$ as the introduction rule for 0.

However, the alleged incoherence of this approach does not increase our confidence in the coherence of Tennant's own approach; perhaps neither is viable. More persuasively, Tennant points out (*op.cit.*, 325) that 0-introduction$ would not be a very good rule for the introduction of 0 as it presupposes understanding of the very concept it would then be supposed to introduce (since possession of a concept for 0 is necessary to understand premises to which it may be applied). This is a good point and a solid reason to prefer Tennant's 0-introduction to 0-introduction$ as a means of introducing a concept for 0. But clearly this has no bearing on our claim that 0-introduction is not stipulatively valid.

Another response to our charge might be for Tennant to argue that commitment to the existence of 0 does not belong with 0-introduction alone but rather with 0-introduction taken together with the existence of necessarily uninstantiated concepts, like the concept $\neg(x = x)$ (similarly, Wright suggests that the ontological commitments associated with Fregean abstraction principles belong with the principles together with true claims concerning the equivalence relations from which they abstract (1997, 208)). However, this response is not convincing. Prior to the extension of the background logic with the rules for FA_T, it is provable that the concept $\neg(x = x)$ is not instantiated. Any logic that includes the logic of identity must prove this, so it is, in a sense, forced upon us. However, it is only after the extension of the system with some rule for the introduction of

0 that we are able to represent this claim as a claim about the number 0. Here, we do have a choice as to which rule to adopt, for we can adopt the rule of 0-introduction* given above. Tennant's rule of 0-introduction is therefore an option which brings with it commitment to the existence of 0, so the commitment does belong with the rule.

So far we have argued that 0-introduction cannot be held to be stipulatively valid because it is committed to the existence of 0. We will now argue that a comparable difficulty affects the ratchet principle. As this principle is essential to Tennant's proof that successors of natural numbers exist, it is committed to the existence of successors of 0. But this is an independent matter of fact from the perspective of mathematical realism, and so the ratchet principle cannot be stipulatively valid.

Tennant addresses worries about the ontological commitments of the ratchet principle with the following argument:

> To the objection that they [the ratchet principle and a related principle of succession] express 'conditional existence' claims, the reply would be: their antecedents involve existential commitments; and all the principles are doing is drawing out the analytic consequences of entering into such commitment. (1997a, 319-320)

However, this does not provide a convincing response to our objection. If Tennant's point is supposed to be that instances of the ratchet principle bear no genuine ontological commitments, then this is false because the ontological commitments of the consequents differ from those borne by the antecedents. In addition to this, the claim that the consequents are analytic consequences of the antecedents does not obviously address the point we are making, that ontologically committed principles cannot be stipulatively valid. It also seems wrong to say that the consequents of instances of the ratchet principle are analytic consequences of the relevant instances of its antecedent; strict finitists can coherently reject the existence of non-finite collections of objects. Tennant's comments thus provide no reason to resist our argument that the ratchet principle cannot be stipulatively valid.

Our argument in this section has been that 0-introduction and the ratchet principle cannot be stipulatively valid, because their validity depends on independently existing matters of fact. One final, general response to this is suggested by Tennant's view that the natural numbers are somehow already present in any conceptual scheme that contains the standard first order apparatus of quantification and identity. Tennant says that the natural numbers are "manifest ontological commitments arising from our ways of speaking and thinking" (*op. cit.*, 325) and that once predicative and quantificational thought is possible "so is its extension (if it is not yet extensive enough) to thought about numbers" (*op. cit.*, 326). If this idea could be satisfactorily developed, then perhaps it would be possible to maintain that the rules we have been considering do not bring with them commitment to the existence of natural numbers, but rather unearth a prior commitment that was already present before their stipulation.

But how might this idea coherently be cashed out? One suggestion might be that concepts for natural numbers are already present in the logical scheme prior to its extension to FA_T. On the current approach to concept formation, however, according to which concepts are constituted by rules dictating the role they play in thought, that would surely not be plausible; the rules said to be constitutive of arithmetical concepts are neither contained in nor derivable from the logical scheme prior to being stipulated. An alternative suggestion might be to say that someone who's thought is structured by the standard predicative and quantificational apparatus is already committed to the natural numbers. But that would just seem to be false. Someone is committed to a kind of thing if they have beliefs from which the existence of such things can be derived using a theory they accept. But prior to the allegedly concept forming stipulations of FA_T, someone who thinks in the usual predicative and quantificational ways does not thereby accept a theory that allows the derivation of the existence of numbers from their beliefs. So prior to the extension of the conceptual scheme to FA_T, they need not be committed to the existence of natural numbers.

Our argument that 0-introduction and the ratchet principle cannot be stipulatively valid thus seems cogent. By viewing 0-introduction and the ratchet principle as stipulatively valid, Tennant's position attempts to settle the ontological issue of whether there are numbers by fiat. But from the perspective of mathematical realism, the existence of numbers is an independent matter of fact that cannot be settled this way. It may be that there are natural numbers out there awaiting description by the concepts constituted by the various rules of FA_T. If so, then, by sheer good fortune, Tennant's stipulations may provide arithmetical concepts that pick out those objects. However, there is no guarantee that this will be the case and the mere fact that Tennant has stipulated the rules for FA_T provides no reason whatsoever for thinking that it obtains. On the assumption that the natural numbers are independently existing objects, the question of what inferences are valid between thoughts about them is an independent matter of fact. Thus 0-introduction and the ratchet principle cannot be made valid by stipulation, nor can adequate reasons for belief that they are valid be had from stipulation.

This conclusion completely undermines the view that the rules for FA_T can be regarded as stipulatively valid because they can be used to introduce our usual arithmetical concepts. Against this, we have argued that 0-introduction and the ratchet principle have another function, that of introducing commitment to the existence of numbers, and that this means they cannot be regarded as stipulatively valid. If this is correct, Tennant provides no grounds for belief in the validity of the rules of FA_T.

2.6.2 Conceptual analysis and formulability

We will now address the question of whether the rules of FA_T can be formulated from conceptual analysis of our usual arithmetical notions. Tennant's main reason for thinking they can appears to be that FA_T satisfies his proposed adequacy condition on prospective theories of natural number.[22] The condition, which Tennant gets from reflection on arithmetic and its applications, is that the candidate theory entails every instance of the "disnumerical schema":

$$Nx{:}\ Fx = \underline{n} \leftrightarrow \exists_n x\, Fx$$

Here "$\underline{n}$" is schematic for a numerical in canonical notation, "$\exists_n x\, Fx$" is to be read "there are n Fs", where this is defined without reference to numbers using indexed variables in the usual way, and F can be instantiated by any predicate the instances of which can be distinguished and identified amongst themselves (1997, 316-317). Call the requirement that a prospective theory of natural number entail every instance of this schema the disnumerical condition.

Tennant argues that imposition of the disnumerical condition is based on two insights; the philosophical insight that natural numbers are governed by clear identity criteria and the mathematical insight that the natural numbers are the elements of the progression formed from 0 by repeated application of the successor relation (1987, Chapter 20). As these insights are at least in the market for being produced by conceptual analysis of our usual arithmetical concepts, this gives a connection between analysis of those notions and the rules from which theories satisfying the disnumerical condition may be derived. But does a theory's satisfaction of the disnumerical condition show that its primitive mathematical rules are formulable from conceptual analysis of our usual arithmetical notions? We claim that it does not.

[22] For discussion of this condition see (1987, Chapter 20; 1997a, §4, §6).

A first point to note here is that some theories satisfying the disnumerical condition obviously have a much richer mathematical content than the arithmetic of the natural numbers. For example, ZFC together with some standard construction of the natural numbers as sets will entail every instance of the disnumerical schema. But it will clearly entail an awful lot more, a great deal of which is to do with sets rather than natural numbers. A theory can thus satisfy the disnumerical condition and yet involve rules concerning mathematical concepts other than those of arithmetic (as would a rule equivalent formulation of ZFC). Such rules would presumably not be formulable from conceptual analysis of our usual arithmetical notions.

A second point to make is that Tennant's remarks about the mathematical insight underlying the disnumerical condition suggest that it is not produced by conceptual analysis alone:

> *Mathematically*, Dedekind (1888) and Peano (1889) made a breakthrough by laying down axioms designed to capture the intuitive picture of the natural numbers 0, 1, 2, 3, ... forming a recursive progression. (1987, 226)

Analysis seems to have dropped out of consideration here, to be replaced by intuition. The shift away from considerations bearing on concepts is re-emphasised shortly after when Tennant says that "the two strands, the philosophical and the mathematical could also be called the *conceptual* and the *structural*." (*op. cit.*, 227). Because of this, it is not clear that we should think of the mathematical insight which underlies the disnumerical condition as purely a product of conceptual analysis. Thus it is not clear why we should think of satisfaction of the disnumerical condition as a test by which to judge whether the mathematical rules of a given theory are formulable from conceptual analysis of our usual arithmetical notions.

If this is correct, then the fact that FA_T satisfies the disnumerical condition does not establish that its rules can be formulated from conceptual analysis of our usual arithmetical notions. However, Tennant does not provide any other general reason for thinking that the rules of FA_T can be produced this way, and neither does he provide reasons specific to each rule of FA_T for thinking that they can be so produced. He does spend some time arguing that 0-introduction and the ratchet principle ought to be considered analytic, but the passages in which he deals with these issues do not provide grounds for thinking that these rules can be formulated from conceptual analysis of our usual arithmetical notions (see (1997a, §6.3-6.5)). For example, Tennant has this to say about why the ratchet principle ought to be considered analytic:

> The ratchet principle is really toothless ontologically. All it expresses is the thought that if one has gone so far as to acknowledge the existence of any one natural number, then there is no reason to refuse to recognize the 'next' number. That seems reasonable: not even the nominalist opponent wishes to visit on the Platonist a prematurely truncated initial segment of the natural number series, denying the Platonist all (and only) the numbers after some allegedly 'final' one! The Ratchet Principle can be expressed by the rhetorical question 'Wherever you are, why stop there?' There is no reason not to regard the Ratchet Principle and the Principle of Succession as analytic. (1997a, 319)

Here Tennant means by analytic any "conceptual truth" which yields to "purely conceptual analysis" (*loc. cit.*), so his intention seems to be to establish that the ratchet principle is analytic in our sense of being knowable on grounds produced by conceptual analysis. But it is quite clear that the passage does not establish this. The strict finitist does wish to deny the Platonist more than an initial segment of the natural numbers, so it has not seemed plausible to everyone that "if one has gone so far as to acknowledge the existence of any one natural number, then there is no reason to refuse to recognize the 'next' number". Explanations of the appeal of the ratchet principle other than that it is analytic are not ruled out; perhaps, in light of Tennant's remark quoted above concerning the intuitive insight that the numbers form a progression, we might consider explanations in terms of some kind of intuition.[23] In addition to this, the ratchet principle does not express the thought that "if one has gone so far as to acknowledge the existence of any one natural number, then there is no reason to refuse to recognize the 'next' number": the ratchet principle states that if the number of Fs exists and if there is one more G than there are Fs, then the number of Gs exists. The quoted passage thus provides no convincing reason for thinking that the ratchet principle is analytic. And more significantly for us, it does not even touch upon the question of whether the ratchet principle can be formulated from conceptual analysis of our usual arithmetical notions.

So far we have argued that Tennant provides neither general nor specific reasons for thinking that the rules of FA_T can be formulated from conceptual analysis of our usual arithmetical notions. Since this does not yet mean that those rules cannot be so formulated, we will now consider whether considerations can be found against thinking that they can. We will argue that such considerations can be found for the ratchet principle and 0-introduction.

Unlike the other rules, the ratchet principle is neither an introduction nor an elimination rule for a concept. Moreover, its use in Tennant's derivations suggests that its sole purpose is to allow proofs of the existence of elements in the logical posterity of 0 under the successor relation.[24] One might therefore suspect that the ratchet principle does not contribute anything to our arithmetical concepts beyond what may be understood from the other rules of FA_T. It thus may seem plausible to conclude that the ratchet principle does not play a constitutive role in the arithmetical concepts of FA_T, which would make it seem unlikely that the principle could be produced by conceptual analysis of our usual arithmetical notions

[23] Rumfitt (2001, 523) also worries that Tennant has not ruled out the possible involvement of intuition.

[24] As Tennant points out in his discussion of ancestral relations (1987, 281-290), given concepts for 0 and $s(x)$ it is possible to define the concept natural number as the logical posterity of 0, and even to show that induction holds for all existent successors, without first having to prove that 0 and the objects in its posterity under $s(x)$ exist.

(even assuming that these and the arithmetical notions of FA_T are the same). For presumably conceptual analysis is a facility that, when correctly applied, enables us to delve deeper and deeper into the nature of the concepts we deploy, revealing the fundamental regularities of thought by which they are constituted. If this is so, then it could be expected to deliver up only rules that are constitutive of the concepts under analysis. The role played by the ratchet principle in Tennant's system of arithmetic thus gives reason to suspect that it is not formulable from conceptual analysis of our usual arithmetical notions, because it suggests that the ratchet principle may not be a constituent of them.

In addition to this somewhat speculative conclusion, the considerations of the previous paragraph point the way to a more general objection affecting both the ratchet principle and the rule of 0-introduction. If, as we have claimed, the function of conceptual analysis is to deliver up constituent concepts of whatever notions are under analysis, then conceptual analysis of our usual arithmetical notions can only produce the rules of FA_T if the latter are constituents of the former. A reason to think that the rules of FA_T cannot be regarded as constitutive of our usual arithmetical concepts would thus be a reason to think that they cannot be formulated from conceptual analysis of them. This is significant because, as we argued in the previous section, 0-introduction and the ratchet principle bear ontological commitments. But on the Quinean approach to ontology that we have assumed, theories, not concepts, bear ontological commitments (see section 1.2). It therefore follows that 0-introudction and the ratchet principle cannot be regarded simply as constituents of our usual arithmetical concepts, because the fact that they bear ontological commitments indicates that they perform a function that cannot be performed by concepts alone. 0-introduction and the ratchet principle thus cannot be formulated from conceptual analysis of our usual arithmetical notions.

Defenders of Tennant's position would perhaps respond to this argument by claiming that his underlying approach is intended to challenge the view that concepts cannot bear ontological commitments, at least for the case of arithmetical concepts. It does not seem credible that the new approach to ontology that this would require could be made to cohere with the Quinean approach that we have assumed. Even if it could, however, convincing reasons would still be required for regarding commitment to the existence of natural numbers as partly constitutive of our arithmetical concepts. As yet, we have seen no reason to think that this should be our approach.

2.6.3 Summary

Tennant's discussion of FA_T establishes that arithmetic is analytic only if (a) it provides reason to believe that the rules for FA_T are valid and (b) it provides reason to believe they can be formulated from conceptual analysis of our usual arithmetical notions. We have argued that Tennant provides no adequate reason to think that these conditions are met. We have also argued that the ontological commitments borne by 0-introduction and the ratchet principle exclude them from being stipulatively valid and show that they cannot be formulated from conceptual analysis of our usual arithmetical notions. We thus conclude that Tenant's development of FA_T provides no compelling reason to think that arithmetic is analytic.

2.7 Rumfitt's principle C

Rumfitt (2001) argues, as we have, that FA_T does not provide a secure basis for the analyticity of arithmetic. However, he maintains that the prospects for showing that arithmetic is analytic are improved when FA_T is combined with a general principle expressing necessary and sufficient conditions for the existence of cardinal numbers. From this principle, Rumfitt derives the conditional claim that if there are 0 Fs then $Nx{:}Fx = 0$ (*op.. cit.*, 534), and also the ratchet principle (*op. cit.*, 534-535). In addition, he considers the general principle to be formulable from conceptual analysis of our notion of cardinal number. If this is correct, then perhaps the availability of this general principle will help to show that arithmetic is analytic after all.

To state Rumfitt's existence principle for cardinal numbers, we need the notion of a generalised tally. This is defined as a concept the instances of which are strictly well-ordered (*op. cit.*, 529). Rumfitt's existence principle for cardinals is then the following:

> (C) There is such a thing as the number of Fs iff either F is empty or F is equinumerous with a bounded initial segment of some generalised tally.

Here a concept F is taken to be equinumerous with a bounded initial segment of some generalised tally whenever there exists G, R and *x* such that R is a strict well-ordering on G, G*x* and F is equinumerous to all the Gs under R up to and including *x* (*loc. cit.*).

Rumfitt arrives at this principle by considering what it is successfully to count, or tally, a collection of objects (*op. cit.*, 524-529). He argues that this requires that the objects be put in one-one correspondence with another collection of objects that could be used as names for cardinal numbers. This is possible when the objects are ordered by a specific kind of relation. The relation must have an initial element, from which a count could begin. It must be transitive and connected, to ensure that that there is always just one correct way of proceeding with the count. It must be irreflexive, since otherwise the presence of the same object in two or more places in the ordering would render the result of the count ambiguous. Finally, the relation must be such that each non-empty collection of the objects has a least element under the relation, because counts proceed by association of an object not yet counted with the least element of the collection of number words that have not yet been used in the count. Together these conditions demand that the relation on the objects used in the count is a well-order.[25] Arguing that to count or tally a collection of objects is to show that they can be put in one-one correspondence with the elements of a generalised tally up to and including some element of it, Rumfitt thus arrives at his principle C.

Our question is whether combining FA_T with principle C helps to show that arithmetic is analytic. Clearly it does only if principle C is analytic. But this seems unlikely, for on the present model of analyticity, an arithmetical principle is analytic only if (a) it can be formulated from conceptual analysis of arithmetical notions and (b) it is stipulatively true or valid. Arguably, however, principle C does not satisfy these conditions.

A first point to make here is that Rumfitt's route to principle C leaves room for doubt that it is formulable from conceptual analysis of arithmetical notions. Rumfitt makes quite clear that the principle is supposed to be formulated from conceptual analysis of our notion of cardinal number, and counting:

> Principle (C) ... gives a quite general necessary and sufficient condition for there to be such a thing as the number of Fs – a condition grounded in a conceptual analysis of the notion of a

[25] Formally, a relation R on instances of a concept G is a well-ordering if and only if: $\forall x\ \forall y\ \forall z\ [Gx \wedge Gy \wedge Gz \rightarrow ((Rxy \wedge Ryz) \rightarrow Rxz) \wedge (Rxy \vee x=y \vee Ryx) \wedge \neg Rxx \wedge \forall H\ [\forall w\ (Hw \rightarrow Gw) \wedge \exists w\ Hw) \rightarrow (\exists u\ Hu \wedge \forall v\ (Hv \rightarrow Ruv \vee u=v))])]$. Rumfitt (2001, 529) has almost this formalisation, though his final clause neglects to demand that the lower bound u is contained in H, a non-empty subconcept of G. This is crucial since otherwise H need not contain a least element.

> cardinal number (and, more particularly, of the connections between that notion and our practices of counting). (2001, 532)

The presupposition made here, that constraints governing what it is for a practice to be a correct counting practice are sufficiently bound up with our notion of cardinal number for reflections on the former to contribute to analyses of the latter, is quite plausible. But since we can only count finite collections of objects, it is obvious that not all the constraints governing our actual counting practices should be thought to play a role in general conditions of existence for cardinal numbers (for example the constraints of time and energy available to the finite counter should not be considered relevant to the existence of infinite cardinals). Supposing, then, that we have identified all the constraints that govern our actual (finite) counting practices, the question would arise of how we are to distinguish between those that govern counting practices in general (including "infinite counts") and those that do not. Rumfitt does not explain how we are to do this, and so we cannot be confident that no processes other than conceptual analysis are involved. There is a suggestion that reflection on idealized versions of our actual counting practices may be involved here:

> The requirement that a concept *F* to which a number belongs should be equinumerous with a *bounded* segment of a tally reflects the fact that in giving an exact non-zero answer to a "How many?" question a respondent indicates that all the *F*s may be put in correspondence with the members of the tally up [to] and including *x*. Even if the count could not be completed in a finite time, the count must in this sense be exhaustive (2001, 529)

But this just intensifies the worry, for since Rumfitt does not describe the kind of idealization involved, the possibility that the relevant reflective processes contain non-analytic elements is not ruled out.

Further reason to doubt the analyticity of principle C comes from our case against the analyticity of FA_T. We argued that, because the ratchet principle and the rule for 0-introduction are committed to the existence of natural numbers, they are not formulable from conceptual analysis of our usual arithmetical notions and are not stipulatively valid (i.e. could not be made valid by stipulation nor reason to believe them valid be got from stipulating them). On the basis that it is committed to the existence of cardinal numbers, similar points can be made with respect to principle C. Whether there are cardinal numbers is, from the realist perspective, a matter of independently existing fact, so principle C cannot be made true by stipulation, nor can we get good reason to believe it true by stipulating it. Moreover, on the assumption that theories, not concepts, bear ontological commitments, the fact that principle C is committed to cardinal numbers suggests that it could not be formulated from conceptual analysis.

One final reason for doubting the analyticity of principle C emerges from the following line of thought. The principle demands that when the number of Fs exists, the Fs must be equinumerous with a bounded initial segment of a generalised tally. Rumfitt treats this as equinumerosity with the elements of a generalised tally up to and including some specific element (*op. cit.*, 529). However, with this interpretation principle C implies that the number of Fs exists only if the Fs form a well order with a maximal element, which is not necessary. The collection of natural numbers, for example, has no maximal element under their natural ordering according to the successor relation, yet it has a cardinal number. Principle C thus makes an inessential demand on cardinal existence.

In itself, this is not a threatening point, for the general thrust of Rumfitt's approach remains if we understand principle C to demand that the cardinal number of Fs exists (when there are Fs) if and only if the Fs are equinumerous with an initial segment of a generalized tally. But having had to deal with one inessential demand on cardinal existence, it is natural to wonder whether principle C makes other inessential demands which cannot so easily be eliminated. We will argue that it does.

We note first that principle C does not allow us to think of cardinal numbers as properties of non-well-orderable sets. Suppose that A is a set that is not well orderable, and consider the concept *element of A*. Suppose that A has a cardinal number. Presumably this has to be the number of the concept *element of A*. Assuming principle C, this means that the instances of the concept *element of A* can be put in one-one correspondence with an initial segment of a generalised tally up to some element *x*. Since a generalized tally is a well order, this correspondence can be used to establish a well order on the concept *element of A*. But since the instances of this concept are just the elements of A, this means that A is well orderable, which is a contradiction. The original assumption must therefore have been mistaken, so A must be well orderable after all.

This shows that if principle C governs cardinal existence, only well orderable sets can have cardinal numbers. However, as we have already explained (see section 2.4), it is possible to represent cardinal numbers in ZF without the axiom of choice (or any equivalent) by taking the cardinal number of a set as the set of sets of least rank equinumerous to it. Such representations allow non-well-orderable sets to have cardinal numbers too. Principle C thus makes a demand on cardinal existence that is not made by some adequate set theoretic representations of cardinal number. This suggests that the principle ought not to be regarded as giving necessary and sufficient conditions on cardinal existence, and this in turn suggests that the principle could not be formulated from conceptual analysis of our usual arithmetical notions.

In response to this, it may seem tempting simply to deny that non-well-orderable sets have cardinal numbers. But it is hard to see what motivation there would be for this restriction of the range of application of cardinal numbers. Frege found out in the most unpleasant way that cardinal numbers cannot be taken to belong to proper classes, but no corresponding threat of contradiction is known to motivate the suggestion that only well orderable sets have cardinal numbers. Denying that non-well-orderable sets have cardinal numbers would also conflict with Cantor's explanation of the cardinal numbers as what we are left with when we abstract away from the identity and order of the elements of a set:

> Let *M* be a given set ... . If we abstract not only from the nature of the elements, but also from the order in which they are given, then there arises in us a definite general concept ... which I call the *power* of *M* or the *cardinal number* belonging to *M*.[26]

Although Cantorian abstraction has received deserved criticism, the thought that a set has a cardinal number regardless of whether its elements are well-orderable cannot be assumed to be a casualty of the standard objections. Finally, denying that non-well-orderable sets have cardinal numbers would conflict with the received mathematical opinion that the Scott representation of cardinal number is adequate.

It thus appears that Rumfitt's method of determining necessary and sufficient conditions for cardinal existence has led him astray. He is right that our canonical method of determining the cardinal number of finitely instantiated concepts, counting their instances, applies only to concepts whose instances can be well-ordered. But to claim that this is a necessary condition on the existence of cardinal numbers for concepts in general seems implausible. This makes it seem unlikely that principle C is formulable from conceptual analysis of our usual arithmetical notions.

To bring this discussion to its close, it helps to set it in context. Rumfitt thinks that Wright and Hale's Neo-Fregean programme is shown to be unworkable by the fact that HP implies the existence of problematic

[26] This quotation is an abbreviated version of a quotation from Cantor translated by Hallett (1984, 122). The corresponding passage in German is in Cantor (1932, 411).

cardinals such as the cardinal number of self-identical things. He believes that Tennant's approach successfully avoids commitment to these cardinals but at the cost of appeal to principles the analyticity of which is in doubt. So far, we are in agreement with him. However, Rumfitt attempts to steer a path between the two approaches by putting forward general necessary and sufficient conditions for cardinal existence that will avoid the excesses of the Neo-Fregean programme and the shortcomings of Tennant's. To anyone who is prepared to appeal to set theory, it is clear how necessary and sufficient conditions for the existence of the cardinal numbers, considered in Frege's way as the number belonging to a concept, should be given. The principle, which derives from Cantor, should be:

The number of Fs exists $\leftrightarrow$ the extension of F is a set.

However, this principle cannot be used to help show that arithmetic is an analytic extension of logic, as it involves the notion of set. If the project is to succeed, therefore, a principle of cardinal existence must be found that is coextensive with this one, that is formulable from conceptual analysis of arithmetical concepts and that is stipulatively true. Principle C is Rumfitt's response to this need. But if what we have argued above is correct, principle C is neither formulable from conceptual analysis of our arithmetical notions, nor stipulatively true.

2.8 Conclusion

This chapter addressed the question of whether mathematics can be shown to be analytic, in the sense of knowable on grounds provided by conceptual analysis. The strategy we identified for this was the logicist strategy (section 2.2). On the basis that logic is analytic and that mathematics follows logically from principles constitutive of central mathematical concepts, this argues that mathematics is an analytic extension of logic.

We considered first the Neo-Fregeanism of Wright and Hale (section 2.3). Following Boolos, we argued against the analyticity of Frege Arithmetic, the second order classical theory in which Hume's Principle is the only non-logical axiom, on the grounds that it proves the existence of cardinal numbers whose existence conflicts with ZF (section 2.4). We claimed that this shows that Hume's Principle is not true, hence not analytic, and that the conflict with set theory is incompatible with the Neo-Fregean programme for mathematics in general. We then considered Tennant's free logic alternative to Frege Arithmetic (section 2.5). We argued that the rule of 0-introduction and the ratchet principle upon which it is based are neither stipulatively valid nor formulable from conceptual analysis, and thus not analytic, because they bear ontological commitments (section 2.6). Finally, we considered Rumfitt's attempted resuscitation of Tennant's approach by appeal to the general necessary and sufficient conditions for cardinal existence allegedly given by his principle C (section 2.7). As with the ratchet principle and 0-introduction, we argued that its ontological commitments ruled against the analyticity of principle C. Moreover, we argued that the fact that principle C does not allow non-well-orderable sets to have cardinal numbers suggests that it is neither formulable from conceptual analysis of the notion of cardinal number nor stipulatively true.

If the arguments made throughout our discussions are correct, they suggest a bleak outlook for the view that mathematics is analytic. Having dismissed the most detailed, recent attempts to prosecute the logicist strategy for arithmetic, we are left with no compelling reason to regard arithmetic as analytic. Since there is no serious prospect of achieving for any more extensive mathematical theory what these views tried unsuccessfully to achieve for arithmetic, the logicist strategy is useless as a means of showing that mathematics is analytic. But there is no other suggestion as to how grounds provided by conceptual analysis might be thought to support knowledge of mathematical claims. We thus conclude that appeal to conceptual analysis does not provide an adequate account of mathematical knowledge.

3

Realism and intuition

We saw in the last chapter that rationalist attempts to ground mathematical knowledge in reason might appeal to conceptual analysis, to a faculty of intuition or to both as sources of grounds for non-inferential mathematical knowledge. Having argued against the view that conceptual analysis is epistemically effective, the purely analytical approach no longer seems attractive. Thus, if rationalism is to provide an adequate account of mathematical knowledge, it will have to appeal to a faculty of intuition for grounds for non-inferential mathematical knowledge. But is this approach likely to succeed? This is the subject of the present chapter.

3.1 Two kinds of intuition

On the approach we are interested in, the faculty of intuition, supposing we have one, is a special cognitive capacity providing us with rationalistically acceptable, sense-experience independent, grounds for knowledge. The suggestion we are concerned with is that a faculty of intuition of this kind provides grounds for non-inferential mathematical knowledge.

If this is to be sustained, it will be necessary to explain how the faculty of intuition issues in non-inferential mathematical knowledge. This will require a clear conception of the faculty of intuition. In broad terms, two conceptions are available. On the first, a subject's having an intuition is construed as his standing in a certain relation to a mathematical object. Assuming this object-relational conception, we can speak of intuitions *of* mathematical objects, we might say, for example, that Hippasus had an intuition of, or intuited, $\sqrt{2}$. Call this kind of intuition *mathematical intuition*. On the second conception of intuition, a subject's having an intuition is construed as his standing in a certain relation to a mathematical proposition or thought. Assuming this propositional conception, we can speak of intuitions *that* things are thus and so in the mathematical realm; we might say, for example, that Pythagoras had an intuition, or intuited, that the square on the hypotenuse is equal to the sum of the squares on the other two sides. Call this kind of intuition *rational intuition*. Clearly a rationalist account of mathematical knowledge might appeal to mathematical intuition, to rational intuition or to both as source of grounds for non-inferential mathematical knowledge.

The direction taken here will distinctively season the resulting explanation of mathematical knowledge. Because it relates subjects to mathematical objects, arguing that we possess a faculty of mathematical intuition suggests that we have a kind of direct access to mathematical objects, by which we get information about them. Epistemological theories appealing to mathematical intuition thus bear comparison to causal accounts of our knowledge of concrete objects, according to which such knowledge depends ultimately on the information-providing access to concrete objects made possible by causal relations.[27] In contrast, appeal to a faculty of rational intuition does not suggest, at least, not initially or immediately, that we have a kind of direct access to mathematical objects, because this kind of intuition connects subjects with mathematical propositions or thoughts. Rationalist attempts to explain mathematical knowledge by appeal to mathematical intuition thus differ fundamentally from those that appeal to rational intuition; the former portray mathematical knowledge as resulting from a kind of access to mathematical objects, the latter do not.

Assuming that some non-inferential mathematical knowledge can be secured on the deliverances of a faculty of intuition, a satisfactory account of mathematical knowledge would then have to explain how this supports inferential mathematical knowledge. Clearly this will require recourse to logic as a means of transferring warrant or justification from one thought to another. However, this leaves room for different accounts depending upon what kinds of logic are permitted. One possibility might be to hold that mathematical inference is exclusively deductive. But it might also be possible to hold that non-deductive inference plays a role.

The preceding remarks make clear where rationalist proponents of intuition need to concentrate their efforts. To begin with, they will have to convince us that we possess a faculty of intuition. It will be necessary to describe the faculty in some detail, explaining how it works, arguing for our possession of it and, crucially, justifying the claim that it provides us with a non-trivial body of non-inferentially known mathematics. Proponents of intuition will also have to convince us that mathematical knowledge in general can be secured on the basis of the intuitive evidence they describe. They will have to show that the intuitively and non-inferentially

[27] Goldman (1967) contains the classic statement of a causal theory.

known body of mathematics supports mathematical knowledge more generally. With this in mind, we proceed to a consideration of the prospects for accounts of mathematical knowledge appealing to mathematical intuition.

3.2 Gödel's account of set theoretic knowledge

Gödel declares his commitment to a faculty of intuition in this well-known passage:

> But, despite their remoteness from sense experience, we do have something like a perception also of the objects of set theory, as is seen from the fact that the axioms force themselves upon us as being true. I don't see any reason why we should have less confidence in this kind of perception, i.e., in mathematical intuition, than in sense perception, which induces us to build up physical theories and to expect that future sense perceptions will agree with them and, moreover, to believe that a question not decidable now has meaning and may be decidable in the future. (1963, 483-484)[28]

The intuition postulated here is understood as a relation between knowing subjects and mathematical objects, a species of intuition *of* mathematical objects. It gives cognitive access to sets and is distinct from sense perception, although analogous to it. In the terminology of the previous section, Gödel here posits a mathematical intuition of sets.

It is a characteristic tactic of proponents of mathematical intuition to expand upon their views with the help of an analogy between mathematical intuition and sense perception. Gödel is no exception, and he returns to his analogy in later remarks:

> It should be noted that mathematical intuition need not be conceived of as a faculty giving an *immediate* knowledge of the objects concerned. Rather it seems that, as in the case of physical experience, we *form* our ideas also of those objects on the basis of something else which *is* immediately given.

[28] The passage occurs in a supplement added to a much earlier version of the paper, Gödel (1947). C. Parsons (1995) investigates just when Gödel became convinced of the existence of this form of mathematical intuition.

> Only this something else here is not, or not primarily, the sensations. (1963, 484)

Here Gödel conceives of sense perception as a faculty giving us indirect access to physical objects on the basis of direct access to sensations, where sensations are understood to result from the causal commerce we have with physical phenomena through our sense organs. He invites us to think of mathematical intuition in a similar way, as a faculty giving us an indirect access to sets on the basis of direct access to certain special data, a distinctively mathematical "given" corresponding to sensations. Expanding on the relationship between these data and sensations, Gödel remarks that the ideas of physical objects arising from our sensory experience contain elements, such as the idea of an object, which are not sensations and that the data of mathematical intuition must be related to these non-empirical constituents of our empirical ideas (1963, 484). The data of mathematical intuition are thus not so far removed from sensations as one might have thought. They are certainly to be distinguished, however; Gödel maintains that the data of mathematical intuition, in contrast to sensations, are not produced in us as a result of causal interactions with particular things (*loc. cit.*).

We saw in the previous section that a rationalist appeal to intuition must provide an explanation of how the faculty of intuition supports non-inferential mathematical knowledge together with an account of how this body of knowledge supports the rest of mathematics. We may assume that for Gödel it is set theoretic knowledge that is to be grounded in intuition, as the conception of intuition he introduces is supposed to issue in intuitions of sets. But which set theoretic claims does Gödel think are non-inferentially supported by intuition? We may approach this question from his remark that the axioms of set theory "force themselves upon us as being true". This is supposed to provide evidence that we possess set theoretic intuition, but it also clearly suggests that the mathematical claims Gödel takes to be known through intuition are those very axioms, presumably the axioms of NBG.[29] Having concluded that Gödel takes set theoretic axioms to be non-inferentially grounded on intuition, it then seems reasonable to read him as proposing that inferential set theoretic knowledge is produced by deduction from the non-inferentially known set theoretic axioms. In this way, we arrive at a view of set theory according to which set theoretic knowledge proceeds deductively from intuitively known axioms.

Some of Gödel's remarks, however, suggest that this picture is not intended. During his discussion of the system developed in *Principia Mathematica*, Gödel writes:

> He [Russell] compares the axioms of logic and mathematics with the laws of nature and logical evidence with sense perception, so that the axioms need not necessarily be evident in themselves, but rather their justification lies (exactly as in physics) in the fact that they make it possible for these "sense perceptions" to be deduced; which of course would not exclude that they also have a kind of intrinsic plausibility similar to that in physics. I think that (provided "evidence" is understood in a sufficiently strict sense) this view has been largely justified by subsequent developments, and it is to be expected that it will be still more so in the future. Gödel (1944, 449)

Here Gödel endorses the view that mathematical and logical axioms can be supported not only by intuitive evidence (intrinsic plausibility) but also by non-deductive means, akin to inference to the best explanation in physics. On this view, an axiom might be justified not because it is

[29] There is textual evidence to support mentioning the axioms of NGB here. Gödel (1963, 475, *n.*13) refers to several works to pinpoint axioms he takes to underlie a conception of set not known to be paradoxical. As these works include Von Neumann (1925), Bernays (1958), and Gödel (1940), the axioms in question are those of NBG. Since there is no indication in Gödel's text of a change of subject when he goes on to discuss our intuitive knowledge of set theoretic axioms, it is reasonable to assume that these are the axioms Gödel believes "force themselves upon us as being true". Thus Gödel's view was that the axioms of NBG are intuitively known.

intuitive but because it allows us to prove intuitive claims. Gödel returns to this idea during his reflections on the continuum problem:

> even disregarding the intrinsic necessity of some new axiom, and even in case it has no intrinsic necessity at all, a probable decision about its truth is possible also in another way, namely, inductively by studying its "success." ... There might exist axioms so abundant in their verifiable consequences [consequences provable without the new axioms], shedding so much light upon a whole field, and yielding such powerful methods for solving problems (and even solving them constructively, as far as that is possibe) that, no matter whether or not they are intrinsically necessary, they would have to be accepted in at least the same sense as any well-established physical theory. (1963, 477)

In this passage, Gödel lays a greater emphasis on his view that even an axiom for which we have no intuitive evidence may nevertheless by justified on the basis of a kind on non-deductive, non-intuitive evidence.

If these passages are to be given as much weight as Gödel's clear belief in the existence of deductive evidence for set theoretic claims, then the picture of set theory as a deductive outgrowth of intuitively known axioms is too simple. Gödel certainly thinks there is intuitive evidence for the axioms but he seems also to think that there is another kind of non-deductive evidence, supporting set theoretic axioms independently of intuition, on the basis of their theoretical role. So it would seem that the picture of set theory he really advances portrays set theoretic knowledge as growing out of intuitively known set theory in not one but two directions, deductively and non-deductively.

However, we should be cautious about drawing this conclusion. Gödel is careful to say only that there "might" be axioms justified on the basis of their non-deductive relations to intuitively known mathematics. He also takes care to describe decisions regarding new axioms made on the basis of such evidence as "probable". This contrasts with the case of intuition, which provides evidence not of probability but of truth. In addition to this, Gödel points out in the revised edition of his paper on the continuum problem that non-deductive evidence is of only limited application to axioms of set theory:

> It was pointed out earlier that, besides mathematical intuition, there exists another (though only probable) criterion of the truth of mathematical axioms, namely their fruitfulness in mathematics and, one may add, possibly also in physics. This criterion, however, though it may become decisive in the future, cannot yet be applied to the specifically set-theoretical axioms (such as those referring to great cardinal numbers), because very little is known about their consequences in other fields. The simplest case of an application of the criterion under discussion arises when some set-theoretical axiom has number-theoretical consequences verifiable by computation up to any given integer. On the basis of what is known today, however, it is not possible to make the truth of any set-theoretical axiom reasonably probable in this manner. Gödel (1963, 485)

In light of these remarks, it is not plausible to accord non-deductive methods the same status in Gödel's set theoretic epistemology as deduction from intuitively known axioms; deduction is the primary mechanism for the growth of intuitively known set theory, non-deductive methods are secondary to this.

Gödel's account of set theoretic knowledge can be summarised as follows. Positing a mathematical intuition of sets, it takes set theoretic intuition to provide evidence for the axioms of NBG and regards set theoretic knowledge primarily as a deductive outgrowth of this intuitively known base. Another source of support for set-theoretic axioms is acknowledged, namely their fruitfulness in independently verifiable consequences, but this is of secondary importance.

3.3 Objections to Gödel's account

Gödel's picture of set theory as primarily a deductive outgrowth of intuitively known axioms provides a clear example of a rationalist appeal to mathematical intuition. Because the axioms of NBG provide a set theory powerful enough to model other central mathematical theories such as arithmetic and real analysis (see, e.g., Mendelson (1997)), and because it would be highly controversial to deny the epistemic effectiveness of deductive inference, acceptance of Gödel's views on mathematical intuition would engender considerable sympathy with the view that mathematical intuition can underpin a successful general account of mathematical knowledge. And provided there are no objections to the rationalistic acceptability of deductive inference, this would be a very satisfying result for mathematical rationalism.

However, we will argue that Gödel's account of mathematical intuition is not satisfactory. Our first argument is that Gödel's description of the faculty of mathematical intuition is not detailed enough to provide an adequate understanding of how we are meant to acquire intuitive knowledge. Gödel provides only the briefest description of his conception of mathematical intuition, through the analogy he draws between it and sense-perception and a few elucidatory remarks. These make clear that mathematical intuition is supposed to provide a sense-perception like access to sets on the basis of a non-causal relationship with immediately given data that play something like the role of sensations in sense-experience. But we are not told what the data are, how they are produced in us or how they lead to intuitions of sets.

Gödel does state that the immediately given data of mathematical intuition are "closely related to the abstract elements contained in our empirical ideas" (1963, 484). From the context, it is clear that these abstract elements are things like our concepts of objects. But this does not really provide us with an adequate grasp of what the data of mathematical intuition are, and Gödel provides no further description, nor any specific examples, of them. As regards the way these data are produced in us, Gödel remarks that they cannot "be associated with the actions of certain things upon our sense organs" (*loc. cit.*), which presumably means that they are not causally produced, but he does not give any positive account of how they are or might be produced. On the subject of how the data lead to intuitions, Gödel says nothing, beyond the claim implicit in the analogy with sense-perception that this must be something like the way in which sensations lead to sense-perceptions.[30]

Our argument here is not that a fuller account of the data given in mathematical intuition is impossible. Gödel could perhaps appeal in his explanation of what the data are to the constituents of ideas of physical objects that represent duration and spatial extent. To explain when these data arise in our experience, he might describe some particularly good examples of the kind of experience in which such data are salient. Perhaps it would then be possible to come up with a perspicuous account of how these data produce intuitions of sets. The point we are making is just that Gödel provides nothing like this to elaborate on his allusive remarks about mathematical intuition. The conception of mathematical intuition put forward is extremely meagre, and because of this it is not clear how we are supposed to be have intuitions of sets. As a result, it is not clear how we are supposed to come to possess intuitive knowledge of the axioms of set theory.[31]

We will now argue that Gödel does not provide us with good reason to believe that we possess a faculty of mathematical intuition. His remarks suggest two putative reasons for thinking that we do, the fact that the

[30] It may seem that Gödel's reliance on the notion of a "given" in sense-perception is problematic (for possible reasons why see Sellars (1956)). However, it is not clear that this does present a difficulty. Gödel's analogy between intuition and sense perception is no use to him as an argument for the existence of intuition if the model of perception it assumes is unsatisfactory. But Gödel uses the analogy to explain his notion of mathematical intuition, not to argue for its existence. For this, the acceptability of the model of perception as an account of perception is not an issue, provided it is not found wanting for reasons of clarity.

[31] Charles Chihara makes the point that Gödel does not provide a characterisation of the class of experiences that he feels ought to be explained as intuitions of sets (Chihara (1982)). This is of a piece with the general complaint that Gödel's conception of mathematical intuition is not adequately explained.

axioms of set theory "force themselves upon us as being true" (*op. cit.*, 484) and the fact that "our ideas referring to physical objects contain constituents qualitatively different from sensations or mere combinations of sensations" (*loc. cit.*). However, it is by no means clear that either of these points support the view that we possess a perception-like access to sets.

Consider first the issue of the axioms "forcing themselves upon us as being true". Presumably a statement forces itself upon us as being true if it demands our assent once we have understood it. However, if this is what is intended, then it can hardly be claimed that every axiom of NBG has this characteristic. It is plausible to claim, for example, that the axiom of union, which states that for every set x there is a set y containing precisely the elements of the elements of x, feels this way. But it is not plausible to claim, for example, that this feel attaches to the axiom of infinity, which states that there is a set containing the empty set and which is such that if x is an element of the set then so is the union of x and $\{x\}$, or to the axiom of limitation of size, which states that for any class C there is a set identical to C if and only if there is no bijection from C to V, the class of all sets. Thus if we have correctly understood what phenomena Gödel has in mind here, then his claim about the axioms of NBG stands in need of qualification. If, on the other hand, we have misunderstood what he means, then it is no longer clear what phenomenon is in question.

A second complaint to be made here is that it is unclear what argument Gödel has in mind for the existence of mathematical intuition. He says that the fact that we have a perception-like access to sets can be "seen from the fact that the axioms force themselves upon us as being true" (*loc. cit.*). Presumably this is not intended as an endorsement of the argument:

(1) The axioms of set theory force themselves upon us as being true

(2) Therefore we possess a faculty of mathematical intuition giving us knowledge of the axioms of set theory on the basis of a perception-like access to sets.

This argument is not sufficiently detailed to be convincing. Then again, Gödel gives no indication of what other premises might be taken to support this inference. Because of this it is just not clear why Gödel thinks that recognition of the phenomenon mentioned in claim (1) should lead us to recognise that we have a perception-like access to sets, as mentioned in (2).

Let us consider now the second reason suggested for thinking that we possess a faculty of mathematical intuition, the fact that "our ideas referring to physical objects contain constituents qualitatively different from sensations or mere combinations of sensations" (*op. cit.*, 484). Gödel appeals to this in support of his view that there are immediately given data functioning in the production of mathematical intuitions in something like the way sensations produce sense-perceptions. His argument appears to be that since the mind cannot create ideas or their constituents save by the recombination of constituents previously grasped, these non-sensory constituents of our ideas must enter our understanding in some other way rather than having been produced by the mind itself (*loc. cit.*).

If this is the argument intended, it is not convincing. The assumption that the intellect is only able to contribute conceptual elements to the organisation of experience by combining and recombining independently given constituents belongs to a dated conception of the mind that contemporary cognitive science rejects. In order to combat the view that the non-sensory constituents of our ideas are subjective, Gödel points out that this cannot be inferred from the fact that they are not produced by sensory experience of physical objects (an inference he attributes to Kant). But one can accept this whilst still maintaining that the mind is capable of contributing conceptual elements to the organisation of experience, for contemporary cognitive science gives much better reasons for thinking that the mind is in this way active. The argument we have described thus does not provide any support to the view that our thought relies on data given to us immediately through some non-sensory interaction with an external reality. Since no other argument is suggested by these remarks, Gödel gives us no reason to believe in these immediately given data of mathematical intuition.

If the points raised in the foregoing discussion are correct, Gödels remarks on mathematical intuition leave us with no grounds for confidence that knowledge of set theory can be satisfactorily based on the deliverances of mathematical intuition. We argued that Gödel's description of the faculty of mathematical intuition is not sufficiently detailed to provide a satisfactory understanding of how it is supposed to issue in intuitive knowledge of the axioms of set theory. In addition, we argued that his remarks do not suggest any convincing reasons to think that we possess such a faculty. We thus remain unconvinced by Gödel's remarks that appealing to mathematical intuition will help to provide a convincing account of our knowledge of set theory.

3.4 Consequences for rationalist appeals to mathematical intuition

Where does our assessment of Gödel's view of set theoretic knowledge leave the rationalist project of accounting for mathematical knowledge by appeal to mathematical intuition? Since our discussion does not provide any reason to think that Gödel's approach to set theory is impossible, and since it does not rule out appeals to mathematical intuition for explaining knowledge in other areas of mathematics, it may seem not to have much bearing on the more general issue, at least, no bearing beyond the obvious conclusion that Gödel's account of set theoretic knowledge cannot be accepted.

However, no-one after Gödel has argued that a comparatively extensive body of mathematical knowledge can be secured on the deliverances of mathematical intuition. We should acknowledge here that C. Parsons (1971, 1980, 1986a, 1993, 1994) has argued that some truths of elementary arithmetic might be known on the grounds of a perception-like access to a specific range of abstract types whose tokens are concrete. However, Parsons is an isolated case. Moreover, he concedes that according to his theory the natural numbers are not objects of intuition (1993, 235, *n*. 5; 1994, 143), that the kind of intuition he has in mind would not secure knowledge of the principle of mathematical induction (1994, 227; 1993, 240), nor even perhaps of any elementary inductive proofs (1994, 227), and that it may not secure knowledge that there are infinitely many objects of the allegedly intuitable kind (1993, 244). Because of these limitations, Parsons' theory provides no assurance that mathematical intuition underpins an adequate general account of mathematical knowledge.

The literature thus contains no alternative to Gödel's account of mathematical intuition that promises to deliver a satisfactory general account of mathematical knowledge. Because of this, our negative assessment of Gödel's views is significant for the wider project; it shows that we have no reason to think that mathematical knowledge can be based on the deliverances of mathematical intuition. If a satisfactory account of mathematical knowledge is to be had by appeal to intuition, therefore, it must invoke the other kind of intuition we described above. Accordingly, we now turn our attention to the prospects of basing mathematical knowledge on a faculty of rational intuition.

3.5 Katz's theory of rational intuition

Jerrold Katz (1995; 1998) maintains that mathematical objects are aspatial, atemporal objects and that mathematical truth is a species of necessary truth. He holds that reason furnishes intuitions into the essential nature of mathematical objects and that these intuitions are items of mathematical knowledge. Katz also believes that the body of intuitively known mathematics is extended by proof and systematisation, that neither of these depends essentially on perception or introspection and that all mathematical knowledge can ultimately be secured by these methods. Katz's account of mathematical knowledge thus provides a clear example of the kind of rationalism we are interested in at present.

Katz introduces intuition in this remark:

> The notion of intuition that is relevant to our rationalist epistemology is that of an immediate, i.e., non-inferential, purely rational apprehension of the structure of an abstract object, that is, an apprehension that involves absolutely no connection to anything concrete. Katz (1998, 44)

Later on, he expands on the theme:

> Intuitions are of structure, and the structure we apprehend shows that objects with that structure cannot be certain ways. ... What is present to our minds in a clear and distinct intuition of abstract objects is the fact that their structure puts the supposition of their being otherwise than as we grasp them to be beyond the limits of possibility. Katz (1998, 45)

Clearly intuition is here intended to be a cognitive capacity of some kind, providing states of apprehension with the characteristics of immediacy, independence and informativeness. Intuitions are immediate in the sense that they are not inferred from other states, they are independent in that they do not depend upon causal relations to concrete objects and they are informative because they give us information about abstract objects. The independence of intuition from causal connections should not be thought to rule out that having an intuition might be partly explicable in terms of being in certain sorts of brain states. Ruling this out would require commitment to a highly improbable form of mind-body dualism. Instead it should be taken to exclude that such states realise an intuition as a result of causal relations to items involved in the informational content of the intuition.

It is important to realise that for Katz intuitions do not depend on any kind of interaction with items involved in their informational content. Not only does he refuse to identify intuitions with perceptions by extending the range of perception to include abstract objects, he even refuses to understand intuition by analogy to perception (see his discussion of "classical platonism" (1998, 14-17)). In thus playing down comparisons between intuitions of mathematical thoughts and perceptions of physical objects, Katz is trying to distance himself from the idea, which he derives from the notion of acquaintance in our understanding of perception, that an apprehensive faculty can produce states of apprehension about given objects only on the basis of interaction with objects of the relevant kind. Katz does not believe this gives the right model for intuition, and he is careful to distinguish his account of intuition from accounts based on it. This strongly suggests that the conception of intuition Katz intends is propositional in form, not object-relational. This is confirmed by his remark that when we have an intuition, "what is present to our minds ... is *the fact* that their structure puts the supposition of their being otherwise than as we grasp them to be beyond the limits of possibility" (see quotation above; my italics). What this shows, in the terminology introduced in section 3.1, is that Katz intends a faculty of rational, not mathematical, intuition.[32]

[32] Katz did once defend an object-relational conception of intuition (1981, Chapter 6). Inspired by Kant's account of synthetic apriori knowledge, this conceived intuition as a faculty similar to, and running in parallel with, perception and introspection. The view was that intuitions are acts of apprehension of abstract objects, which a rational subject could have on the basis of constructing a concept of the object. Since Katz clearly abandons this conception in the later work, it is worth

To help explain his notion of rational intuition, Katz states that the intuitable propositions are a subclass of necessary truths, containing elementary mathematical, linguistic and logical propositions. The cognitive mechanism by which such intuitions are produced is the apprehension of the structure of abstract objects alluded to in the quotation above. However, in defending reason's ability to recognise some necessary truths Katz also mentions conceivability (*op. cit.*, 56) and the ability to recognise inconsistency (*op. cit.*, 57). It seems that intuitions as apprehensions of the structure of abstract objects are bolstered in some way by these further considerations.

Having described his notion of rational intuition, Katz goes on to explain its epistemological characteristics. Intuition provides grounds for belief in claims about abstract objects by revealing the limits of possibility concerning them. Given a supposition about how an abstract object is, grasp of the structure of the abstract object immediately reveals the necessary truth (or falsehood) of the supposition. Thus the intuition of four as a composite of two taken twice shows that it is impossible that four is prime; the intuition of the grammatical structure of the sentence "I saw the uncle of John and Mary" shows that it is impossible that the sentence "I saw the uncle of John and Mary" has a unique sense. Katz holds that intuitions are reliable when they satisfy the Cartesian criteria of clarity and distinctness (*op. cit.*, 45)). However, he does not demand that they are infallible (*op. cit.*, 44). Just as we can be deceived by sense perception, so that we seem to perceive that which is not, so too can we be misled by intuition, seeming to intuit propositions that we later discover are false. Finally, intuition is said to be of limited scope. Some truths about abstract

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asking why. A poor answer (for which see Oliver (2000)) is that Katz rejects Kantian conceptions of intuition. Katz does reject Kantian conceptions of intuition like Parsons' (1980) account, which invoke the idea that we can get information about abstract objects from introspection of mental objects (see Katz (1998, 44)). But Katz's earlier conception of intuition was not of this kind because it did not conceive of the construction of concepts of abstract objects to be or to depend upon an introspective process. The real reason Katz abandons his earlier model of intuition is given in the text, that he rejects all conceptions of intuition for which intuitions must ultimately be produced by interaction with objects of the relevant kind. This is another way of saying that he rejects the idea that intuition requires acquaintance: Katz's earlier conception of intuition, although it did not spell out acquaintance in causal terms, appealed to it nevertheless.

objects are considered too general to be intuited, either in the sense of being about too many abstract objects or in the sense of being about relations amongst such objects that cannot be ascertained from their structure (*op. cit.*, 46). These limitations are comparable to the limitations of sense perception, for instance with the restricted ranges of vision or of hearing.

3.6 Objections to Katz's theory of rational intuition

As we have made clear, Katz's theory of rational intuition can be divided into cognitive and epistemological claims. On the cognitive side, rational intuition is said to be a propositional form of apprehension giving us informative, immediate and independent grasp of truths about mathematical objects (grammatical structure and logical form, also). On the epistemological side, rational intuition is said to be warranting but fallible and limited in the sense that propositions about too many objects or about relations on abstract objects not determined by their structure are beyond its scope. Presumably the epistemological characteristics of rational intuition are intended to be consequences of its cognitive aspects.

We will argue that this theory is too bare to be convincing, that no account of the nature and constitution of rational intuition is given to make clear why its cognitive and epistemological characteristics are as they are claimed to be. As we will show, this completely undermines the only serious argument Katz suggests in favour of the existence of rational intuition, thus leaving us with no reason to believe that we possess the faculty of intuition upon which his mathematical epistemology relies.

3.6.1 Katz's reasons for belief in rational intuition

Katz puts forward several grounds for belief in the existence of rational intuition. When evaluating these, it is important to separate two kinds of intuition that he deals with together. Katz remarks that:

> The intuition of the number four as a composite of two and two shows the impossibility of four's being a prime number. The intuition of the logical structure of an instance of modus ponens shows the impossibility of the truth of the premises without the truth of the conclusion. (1998, 45)

Here we have, on the one hand, intuitions of truths like "Four is a not a prime number", and, on the other, intuitions of the validity of arguments like "If the air in the balloon gets hotter, it expands; the air in the balloon gets hotter; therefore it expands". In each case, the way in which we grasp the content of the intuition seems to be similar, exhibiting the characteristics of immediacy, informativeness and independence laid down in Katz's description of intuition. Nevertheless, what we are grasping is not the same: a direct apprehension of truth is one thing, a direct apprehension of the validity of an inference is another.

That Katz deals with these together is not objectionable. There is no reason why he should not posit one form of intuition issuing in two different kinds of output, or perhaps two forms of intuition, each issuing in just one kind of output. The importance of this distinction is rather that it allows us to be clear about the charge we are making. When we claim that Katz's grounds for intuition do not give us reason to believe in its existence, we mean that he does not provide grounds for belief in a faculty issuing in intuitions of the first kind. It is the existence of rational intuition conceived of as a direct insight into truth that we are calling into doubt.

With this in mind we turn now to the grounds put forward in favour of rational intuition. Katz proposes an argument for rational intuition from the immediacy of certain items of mathematical knowledge (1998, 45). He also puts forward considerations suggesting that conceivability and our ability to recognise inconsistency might bolster reason's ability to determine necessary truth (*op. cit.*, 56-58). As these are clearly intended to support the view that reason contains a faculty of rational intuition, we must address these, as well as the argument concerning immediacy.

One difficulty with Katz's discussion of conceivability is that he does not state what he takes it to be. Is it our ability to form concepts, our ability to recognise conceptual possibilities and impossibilities, something else? We are not told. This makes it unclear why we should accept conceivability as part of the faculty of reason. But rational intuition is supposed to be part of the faculty of reason. So even if conceivability considerations do help us to recognise the necessary truth of certain propositions, this gives no reason to think that we have rational intuitions of them.

A second difficulty with Katz's discussion of conceivability is that it does not give any positive ground for thinking that conceivability considerations allow us to recognise the necessity of certain truths. The discussion is primarily aimed at defusing a possible objection to the suggestion that conceivability is a reliable guide to possibility. The objection is that it is not possible to infer metaphysical conclusions, concerning what is possibly the case, from psychological premises, concerning what is or is not conceivable (Katz (*op. cit.*, 56) cites Mill's *A System of Logic*, Chapter 5, §6, in connection with this objection). Appealing to Yablo (1993), Katz argues that this objection can be taken either as a sceptical doubt that conceivability might not be a fit standard for modal knowledge, or as a non-sceptical demand for explanation of the kind of conceivability that provides this standard. He then urges that it is not necessary to defuse the sceptical doubt but only to satisfy the demand for the right notion of conceivability to be made out:

> From the standpoint of the present defence of reason's ability to determine necessary truth, all that needs to be shown is that intuitions based on the proper sort of conceivability measure up to the prevailing standards for modal knowledge. (1998, 57)

However, Katz does not carry out this last task. He gives no suggestion for what the right kind of conceivability might be, and gives no rationale for thinking that conceivability sets up a standard for modal knowledge that corresponds to that of our modal discourse. We are thus left with no idea why conceivability ought to be taken as a guide to (some) necessary truth, and thus no idea why it should be viewed as issuing, somehow, in rational intuitions.

Katz's remarks on conceivability thus do not provide a case for the view that our possession of a faculty of rational intuition is somehow supported by considerations of conceivability. It may be true to say that conceivability is a guide to possibility, so that for any proposition *p* we have the conditional:

$$p \text{ is conceivable} \rightarrow p \text{ is possibly true.}$$

However, if conceivability considerations are to allow us to recognise necessary truths, this is not enough. The general truth of:

$$p \text{ is conceivable} \rightarrow p \text{ is necessarily true}$$

would suffice, but this conditional is obviously not true in general; we can conceive of lots of things that are not necessarily true. Presumably, then, the hope is for a link between the inconceivability of propositions and their impossibility. If the conditional:

$$p \text{ is inconceivable} \rightarrow p \text{ is not possibly true}$$

is true in general, then certainly one might feel drawn towards an argument that conceivability considerations help us to recognise some necessary truths (the negations of propositions the truth of which cannot, in the relevant way, be conceived). But it is not obvious that there is a notion of conceivability for which this conditional is true. Moreover, to use considerations of inconceivability in conjunction with this conditional as a standard for necessary truth, we would presumably have to use psychological capacities of some kind to determine whether or not given propositions are inconceivable. But then our inability to conceive of the truth of a given proposition *p* could simply reflect limitations of the psychological capacities involved, rather than the inconceivability, in the relevant sense, of *p*'s being true. Mill's point, at least with regard to the hoped for connection between inconceivability and impossibility, has not gone away.

It is worth pointing out that Katz himself suggests that inconceivability considerations should not be viewed as a guide to impossibility:

> when we are right that something is inconceivable, there may still be a question of whether or not it is impossible. Yablo (1993, 36-40) himself offers no final assurance that there is a notion of inconceivability strong enough to provide satisfactory metaphysical grounds for thinking that inconceivability implies impossibility. (1998, 58)

Note, too, that one can take this approach to the epistemic effectiveness of inconceivability considerations whilst accepting that a proposition's being conceivable indicates that it is possibly true. Thus one can deny that inconceivability is a guide to impossibility without embracing a general scepticism concerning the epistemic effectiveness of conceivability considerations.

It appears, then, that considerations of conceivability are not suited to helping us recognise the necessary truth of certain propositions. For this reason, and because Katz's remarks provide no positive support for thinking otherwise, we conclude that it is not clear that considerations of conceivability support the view that we possess a faculty of rational intuition.

Let us now consider Katz's remarks on our ability to spot inconsistency; perhaps these will provide evidence that we possess a faculty of rational intuition. Katz's first introduces this ability as "reason's power to recognise inconsistency" but then goes on to discuss "reason's power to recognize a proposition as contradictory" (1998, 57). These are not the same. Our ability to tell, given any proposition p, that it is inconsistent with $\neg p$, is not alone sufficient to explain how we recognise the contradictoriness of, for example, "That brick is red and green all over". Holding these two abilities apart, therefore, we should ask whether either of them could perform the role assigned to rational intuition in Katz's mathematical epistemology. It seems that they could not. Amongst the mathematical propositions that Katz claims are intuitable are some that imply the existence of particular mathematical objects, for example, the proposition that 4 is composite. However, neither our ability to spot inconsistency nor our ability to recognise a proposition as contradictory seems suited to produce intuitive knowledge of such propositions. The ability to recognise, given p, that p and $\neg$p are contradictory can at most provide grounds for belief in instances of the law of non-contradiction. The ability to recognise a proposition as contradictory may have wider scope, it might perhaps give us grounds for knowledge of claims such as that nothing is red and green all over, which express containment relations amongst the extensions of predicates. But these claims do not include claims that are committed to the existence of mathematical objects. So it is not clear either that reason's power to recognise inconsistency, or its power to recognize propositions as contradictory, can provide grounds for claims that, according to Katz's conception, are knowable on the basis of rational intuition. The fact that reason includes these abilities thus provides no evidence for the existence of rational intuition.

The foregoing discussion shows that neither conceivability considerations nor considerations concerning reason's ability to spot inconsistency deliver what is required of rational intuition. They thus do not support the existence of rational intuition. Katz's claim that we possess such a faculty thus depends entirely on his argument from the immediacy of some items of mathematical knowledge. We will now assess this argument.

Katz states that if we think about the mathematics we know (and more generally about our formal knowledge), and ask ourselves how we know it, we will soon, by a process of elimination, arrive at rational intuition:

> Consider the pigeon-hole principle. Even mathematically naïve people see that, if *m* things are put into *n* pigeon-holes, then, when *m* is greater than *n*, some hole must contain more than one thing. We can eliminate prior acquaintance with the proof of the pigeon-hole principle, instantaneous discovery of the proof, lucky guesses, and so on as "impossibilities." The only remaining explanation for the immediate knowledge of the principle is intuition. (1998, 45)

We may take it that the argument here proceeds as follows:

(1) Some mathematically naïve people have immediate knowledge of the pigeon-hole principle
(2) The theory of rational intuition provides a good explanation of this
(3) No other account provides a good explanation of this
(4) Therefore rational intuition exists and is as the theory describes it.

Clearly this argument is intended as support for the more general contention that only the theory of rational intuition explains the immediate knowledge some mathematically naïve people have of some mathematics. Call this the eliminative argument for rational intuition.

Provided we assume that "mathematically naïve" means something like "without formal mathematical education" rather than "totally innocent of mathematical knowledge and concepts", premise (1) seems, from a realist perspective, reasonable enough. Even if otherwise sound, however, this argument does not provide a compelling reason to believe in the existence of rational intuition in the present context. Our aim in chapters two through to five of this study is to find out whether there is a satisfactory mathematical epistemology given the assumption of mathematical realism. The present chapter deals with the suggestion that such an epistemology can be constructed around an appeal to rational intuition. It is clearly unacceptable to us, therefore, that the argument ostensibly demonstrating that rational intuition exists should assume, as (1) does, that we have mathematical knowledge: for us, appeal to rational intuition is supposed to explain possession of mathematical knowledge, not the other way round.

However, we can drop the knowledge condition from (1) to leave immediacy alone, understanding this to concern the nature of our apprehension of the pigeon-hole principle. What Katz has to say about rational intuition could then be taken as a putative explanation of this immediate apprehension, so that, if this were the only successful explanation, it would be possible to argue in just the same way that such states of apprehension are rational intuitions. Thus, an argument for rational intuition similar to Katz's is available in the present context:

> (5) Mathematically naïve people immediately apprehend the pigeon-hole principle
> (6) The theory of rational intuition provides a good explanation of this
> (7) No other account provides a good explanation of this
> (8) Therefore rational intuition exists and is as the theory describes it.

The corresponding general contention, the new eliminative argument for rational intuition, would be that only rational intuition provides a good explanation of the immediate apprehension the mathematically naïve have of certain mathematical propositions. What we must address is whether this provides good grounds for belief that rational intuition exists.

We will argue that it does not. One way to approach this conclusion might be to attack premise (7), arguing that Katz does not set out sufficient reason for thinking that alternative explanations of the immediacy of the pigeon-hole principle that do not appeal to rational intuition. But even if this charge could be substantiated, it could be argued in response that the appeal to rational intuition gives a better explanation of the phenomena than the available alternatives. This will not be possible, however, if it can be shown that the appeal to rational intuition provides no good explanation of the immediacy of the pigeon-hole principle, that is to say, if (6) is false. More generally, the approach of the eliminative argument is completely undercut if it can be shown that rational intuition does not provide a good explanation of our immediate apprehension of mathematical propositions; there can be no inference to a good explanation, unique or otherwise, if the explanation is not good enough. Our approach should thus be to attack the explanation of the immediacy phenomena putatively given by rational intuition.

3.6.2 The constitution of rational intuition

When describing rational intuition, Katz uses visual imagery when in fact rational intuition is meant to be nothing like sense perception (for example in the claim that people *see* that the pigeon-hole principle is true (1998, 45)). He also uses the object-relational form of the verb to intuit when in fact rational intuition is supposed to be propositional (for example when he says that, "The intuition of the number four as a composite of two and two shows the impossibility of four's being a prime number." (*op. cit.*, 45)). Whilst we may perhaps view such claims as metaphors recruited to help get across a feel for what rational intuition is like, Katz's use of them sets alarm bells ringing, suggesting that he found rational intuition a difficult notion to pin down. This leads to the worry that his conception of rational intuition is not detailed enough to support the burden his mathematical epistemology places upon it.

In the previous section, we saw just how heavy that burden is. Katz needs to provide an account of rational intuition detailed enough to support the argument that rational intuition provides the only good explanation of the immediacy of elementary mathematics. It won't do simply to say that rational intuition is whatever it is that underpins our immediate grasp of elementary mathematical claims. This would not provide a good explanation of the facts about immediacy and so could not support the argument that rational intuition provides the only good explanation. Instead we must be given an account of the nature and constitution of rational intuition that (a) gets the facts about immediacy right, in the sense of making a distinction between intuitable and non-intuitable propositions which matches up to the distinction between immediate and non-immediate ones, and (b) provides a good explanation of immediate apprehensions of mathematical propositions as items of knowledge. However, Katz's account of rational intuition does not satisfy the second of these requirements.

Recall that the epistemological characteristics of rational intuition are that it is limited and fallible. What we want to know is why rational intuition has these characteristics, what is it about the nature and constitution of the faculty that explains why it is as it is in these respects. Katz states that we are unable to intui propositions that are about too many objects or that are about properties of objects not discernible from their structure. But why does rational intuition not allow us to intuit propositions that are about too many objects? How many objects does a proposition have to be about for us not to be able to intuit it? Why is the critical number that number rather than some other? Katz does not say. Why, too, does rational intuition not allow us to have intuitive knowledge of properties of mathematical objects not discernible from their structure? Why could we not have knowledge of such properties through intuition of structures in which the objects appear (such as the natural number structure)? Again, Katz does not say. He thus provides no explanation of the limitations of rational intuition.

Consider, then, the fallibility of intuition. As Russell's paradox shows, we are quite capable of making mistakes about the immediate. For someone like Katz who believes that immediacy is a mark of the intuitable, this means that we sometimes seem to intuit things that are not the case. To this extent, it can seem to us that we have intuitions when in fact we do not. But why does this happen? Why is rational intuition not infallible? Given that it is fallible, how we can we tell cases of genuine intuition from cases of seeming intuition? Once again, Katz does not say. Thus the key epistemological characteristics of rational intuition are left unexplained. We are given an idea of what they are, but no idea of why they are as they are. Thus we are not given a good explanation of why immediate apprehensions of mathematical propositions count as items of knowledge.

It is useful to contrast Katz's account of rational intuition with the understanding of sense perception and the language faculty emerging from the cognitive sciences. Artificial intelligence, neuroscience, psychology, linguistics and anthropology, together with philosophical work building upon studies in these areas, are giving us an ever more detailed account of the nature and constitution of these two faculties, of the powers of the human mind/brain to perceive external phenomena and to acquire and make communicative use of language. For example, studies of non-human animals, brain-imaging studies of humans and observation of humans with brains damaged in known ways has given us information about which areas of the brain are recruited during perceptual and linguistic acts. Studies of babies, infants and children have given us information regarding when and under what circumstances humans acquire these faculties.

This wealth of scientific knowledge can be used to explain features of sense perception and the language faculty. Consider, for example, the limitations of vision. We are able to see medium sized physical objects not subatomic particles. But we can explain this using our detailed knowledge of the nature and constitution of visual perception. We know about how our eyes work, how sensitive they are, what sorts of conditions they need to provide us with accurate visual information, and so forth. This knowledge shows quite convincingly why it is buses and figs we can see, not quarks and electrons. Similarly, this knowledge provides detailed explanations of what goes on in cases of non-veridical sightings, helping us to say just why the hexagonal tower looked circular from a distance. But Katz's account of rational intuition provides no comparable explanations of why the characteristics of rational intuition are as he says they are. It is powerless to explain why intuition only operates within the limits he sets or what happens in cases of seeming intuition. Why can we intuit that 4 is composite but not that *e* is transcendental? Katz's account does not say. Why does it seem intuitive that there is a set of sets not members of themselves? Katz's account does not say. It is too bare to provide answers to these questions.

The more one reflects upon this disanalogy between our accounts of sense perception and the language faculty and Katz's account of rational intuition, the more damaging it becomes. We can, if we choose, describe sense perception as a limited and fallible faculty of apprehension providing immediate and informative states about concrete objects. But we would never dream of explaining facts about perception on the basis of this description. We would not use it to explain, for example, how it is that when a normal mature human experiences a certain continuously varying input of differently coloured light through their eyes, they perceive a red ball travelling in front of them across a predominantly green background. Imagine asking someone how this happens, to be told no more than it happens through sense perception, a limited and fallible faculty of apprehension providing immediate and informative states about concrete objects. If this is all you are told, and if you have no prior knowledge of the faculty of sense perception, then clearly you have not been given a good explanation of this perceptual performance. Indeed, it seems fair to say that you have not been given any explanation of what is going on.

In the case of sense perception, of course, the wealth of knowledge provided by the cognitive sciences helps to flesh out the elements of this description, which becomes the skeleton of a truly explanatory account of the senses. But the situation with Katz's account of rational intuition is not like this. First, cognitive science does not give us the slightest hint about what the alleged faculty of rational intuition might be like. Nowhere in its literature do we find talk of a cognitive faculty giving us direct access to necessary truth. Second, Katz's account does not put forward any alternative way of understanding the nature and constitution of rational intuition that could perform the explanatory role with respect to rational intuition that theories of cognitive science perform with respect to sense perception. Thus, all we know about rational intuition is that, if it exists, it is a limited and fallible faculty providing immediate states of apprehension that contain information about abstract objects. Just as the list of characteristics of sense perception does not explain perceptual acts, so this list of characteristics of rational intuition gives no explanation of alleged acts of intuition. We have thus been given no understanding of why immediately apprehended mathematical propositions count as items of knowledge.

One response to this objection is that demands for explanations of the inner workings of rational intuition are misplaced. If this is taken to mean that it is not appropriate to demand accounts of the workings of rational intuition that, like our causal accounts of sense perception, make essential use of causal relations between the relevant states of apprehension and objects those states are about, then it is quite correct; rational intuition is supposed to provide intuitions without any kind of relation to the kind of objects they are about, let alone causal relations. But this misses the point. We have been demanding a theory of the nature and constitution of rational intuition comparable to our theories of sense perception and the language faculty, but we have not been demanding a causal account of this kind.

On the other hand, if the contention is taken to mean that no demands for an explanation of the workings of rational intuition are appropriate, then it is just wrong. If there can be no explanation of the workings of rational intuition, then there can be no explanation of the immediacy of mathematical knowledge in terms of rational intuition. So given that the eliminative argument for rational intuition requires an explanation of the latter sort, it cannot be the case that demands for the first kind are misplaced. As this response to our objection cannot be upheld, we must therefore conclude that Katz's account of rational intuition does not provide a good explanation of the immediacy of some mathematics. The eliminative argument for rational intuition is thus unconvincing, and so we are left with no reason to believe in the faculty of rational intuition.

3.6.3 Summary

We have been concerned in this section to see whether Katz's mathematical epistemology provides compelling reasons to think that non-inferential mathematical knowledge is intuitive. We have seen that Katz suggests three grounds for belief in the existence of rational intuition (a) considerations concerning conceivability (b) considerations concerning reason's ability to recognise inconsistencies and (c) the eliminative argument. We argued that considerations of conceivability do not seem well suited to delivering knowledge of necessary truth and that reason's ability to recognise inconsistencies does not promise to deliver knowledge of ontologically committed mathematical propositions. As rational intuition is supposed to deliver knowledge of the necessity of some such propositions, we concluded that these considerations do not provide grounds for thinking that it exists.

This left us with the eliminative argument. Pointing out that this depends on the claim that rational intuition provides a good explanation of our immediate apprehension of mathematical propositions, we argued that this claim is not warranted. We found that Katz gives no account of the nature and constitution of rational intuition that makes clear why its epistemological characteristics are as they are claimed to be. In the absence of such an account, and in particular since no such account is suggested by the cognitive sciences, there is no reason to suppose that these characteristics are the characteristics that rational intuition must have. They are, in effect, no more than a rationalist wish list. Consequently, Katz's account of rational intuition does not provide any explanation of immediate apprehensions of mathematical propositions, the eliminative argument for rational intuition fails, and so we are left with no reason to believe in the existence of rational intuition. As this leaves us with no reason to believe that non-inferential mathematical knowledge is known through rational intuition, we must conclude that Katz's account of mathematical knowledge is not adequate.

3.7 Conclusion

This chapter has been concerned with the rationalist strategy of locating the grounds for non-inferential mathematical knowledge in the deliverances of a faculty of intuition. We considered first the possibility of positing a faculty of mathematical intuition, issuing in intuitions *of* mathematical objects. Taking Gödel's account of set theoretic knowledge as an example of this approach (section 3.2), we argued that Gödel's remarks on mathematical intuition are too sketchy to provide us with a clear understanding of how we are supposed to intuit sets (section 3.3). Next we considered the possibility of positing a faculty of rational intuition, issuing in intuitions *that* things are thus and so with mathematical objects. We considered Katz's account of rational intuition as an example of this approach (section 3.5), but argued that, because Katz fails to provide any explanation of the nature and constitution of rational intuition, he provides no convincing reason to think that we possess such a faculty (section 3.6).

Of what significance are these arguments for the general strategy of appealing to intuition in explanations of mathematical knowledge? To answer this, recall that the central point we made against Katz's position was that the cognitive sciences do not provide us with anything like a constitutive account of the alleged faculty of rational intuition. Clearly it can equally be said that the cognitive sciences give us no inkling of the nature and constitution of any alleged faculty of mathematical intuition, either. Our objection to Katz's theory of rational intuition thus depends on considerations of quite general significance. For initial plausibility, any rationalist appeal to a faculty of intuition will have to claim that the faculty exhibits epistemological characteristics like those alleged by Katz for rational intuition. But then for defensibility, any such appeal will have to provide us with an account of the nature and constitution of the faculty described that shows why it has such characteristics. Since the cognitive sciences do not provide this for either kind of intuition, and since the cognitive sciences provide our best understanding of our cognitive faculties, it follows that the issue of the existence of the faculty of intuition will be acute for any theory appealing to it, regardless of whether the appeal is to rational intuition, to mathematical intuition or to both. On the basis of the arguments we have presented in this chapter, therefore, it must be concluded that there is no serious prospect for a satisfactory general account of mathematical knowledge according to which non-inferential mathematical knowledge is intuitive knowledge.

4

Realism and perception

In the previous two chapters, we argued that reason alone does not furnish an adequate account of mathematical knowledge. We argued that conceptual analysis seems unable to provide epistemically effective grounds for mathematical truth (Chapter 2) and that it seems doubtful that we possess a faculty of intuition giving cognitive access to mathematical objects or truths (Chapter 3). These arguments suggest that, despite the cerebral nature of mathematics, it is not likely that mathematical knowledge rests ultimately on rationalistically acceptable grounds. If the realist approach to mathematical knowledge is to be convincingly developed, therefore, it seemingly must appeal to some other source than the intellect for ultimate grounds for mathematical belief. What might this source be?

Mill once addressed a similar question:

> It remains to inquire, what is the ground of our belief in axioms – what is the evidence on which they rest? I answer, they are experimental truths; generalizations from observation. The proposition, Two straight lines cannot inclose a space – or in other words, Two straight lines which have once met, do not meet again, but continue to diverge – is an induction from the evidence of our senses. (*A System of Logic*, Book II, Chapter 5, §4)

Although expressed here with respect to geometry, Mill proposed this view as a quite general account of the mathematical axioms of his day. The specific claim, that mathematical axioms are known by simple enumerative induction, is not now considered plausible. However, the more general contention, that mathematical axioms are supported by evidence from the senses, still appeals.

It is not difficult to see why. To deny that our senses provide us with perceptual knowledge of the physical world, that we can see, touch and smell lots of things from figs to planets, would be extremely controversial. It would also be controversial to deny that we extend this perceptual knowledge of observable objects to knowledge that we cannot immediately get from observation, knowledge of general claims about physical objects and of unobservable physical objects, for example. It thus appears both that perception secures a body of knowledge about a wide variety of things, and that this body of knowledge is significantly richer than any collection of claims known through direct observation of the objects they are about. So why might not this knowledge contain mathematical knowledge? Why might not the epistemic mechanisms that generate this knowledge from sensory evidence also generate knowledge of mathematics? Without some kind of argument, there is no clear reason why not. Perhaps, then, mathematical justification can be grounded in perception, as Mill hoped.

A new realist approach to mathematical knowledge thus beckons. Reason alone could not supply ultimate grounds for mathematical belief, but it seems at least possible that these might be had from perception. Should this be borne out, we would arrive not at a mathematical rationalism but at a mathematical empiricism. In this and the next chapter, we will investigate the prospects for such a view.

4.1 Two strategies for mathematical empiricism

One of the traditional objections to the empiricist approach to mathematics is that it cannot explain our knowledge of the necessity of mathematical truth.[33] However, we are not committed to the view that mathematical truth is necessary. Neither of our basic assumptions, linguistic realism and the Quinean criterion of ontological commitment, entail the necessity of mathematical truth. Neither does mathematical realism, as on our characterisation this demands only that mathematical objects exist but do not depend for their existence on thought or talk about them. The possibility of grounding mathematical knowledge in sense perception is thus consistent with the philosophical background we have assumed.

The broad aim of mathematical empiricism is to account for mathematical justification by appeal to the warranting or justifying nature of perception. Clearly one way to do this is to offer an account of the nature of mathematical objects according to which some mathematical objects can be perceived, for then mathematical knowledge might ultimately rest on perceptions of mathematical objects. However, this approach would involve denying the orthodox view, which we mentioned in section 1.3, that mathematical objects are abstract, lacking spatial and temporal properties. An alternative empiricist approach may therefore be preferred, namely, to argue that perceptions of ordinary empirical objects constitute evidence for mathematical beliefs. The idea here would be that just as perceptions of ordinary empirical objects can constitute evidence for beliefs about objects not involved in those perceptions, so, too, could they constitute evidence for mathematical beliefs.[34]

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[33] Mill addresses this objection in *A System of Logic*, Book II, Chapter 5, §6.

[34] It may be thought that if mathematical objects are abstract, then mathematical truths must be necessarily true, in which case the second strategy described here would invite the traditional objection that this cannot be explained by mathematical empiricism. However, this need not show that the strategy is unworkable. It could be defended by denying the inference from the abstractness of mathematical objects to the necessity of mathematical truth (a move considered by Yablo (2002)), or more simply by being offered as part of an explanation showing how knowledge of the necessity of mathematical truth is in fact attainable within the empiricist perspective.

The literature contains a third version of mathematical empiricism according to which there are no mathematical objects, abstract or concrete. For example, Chihara (1990) takes mathematics to be about possibilities of construction, whilst Kitcher (1980, 1984), inspired by Mill, takes it to be about the idealised operations of an ideal agent. Programmes like these demand that mathematical sentences be reinterpreted so as to eliminate apparent reference to mathematical objects. Given our assumption of linguistic realism, however, we are committed to taking mathematical sentences at face value. Thus we need not consider reconstructive projects of this sort.

Consistent with our fundamental assumptions, then, there are two strategies for the pursuit of mathematical empiricism; to reject the abstractness of some mathematical objects, thus bringing them into the range of perception, or to affirm the abstractness of mathematical objects, but to argue that our knowledge of them is supported by our perceptions of ordinary empirical objects. To bring out the difference between these strategies, it is useful to consider what correspondences they posit between two different distinctions. On the one hand, we have the distinction between knowledge of particular mathematical objects and knowledge of general mathematical claims. On the other hand, we have the distinction between observational empirical knowledge, knowledge of empirical objects that could be had by direct observation, and theoretical empirical knowledge, knowledge of empirical objects for which this would not be possible. The first strategy for grounding mathematical justification in sense perception, that of denying the abstractness of some mathematical objects, assimilates some knowledge of particular mathematical objects to our observational empirical knowledge, and the rest of mathematical knowledge to theoretical empirical knowledge. In contrast, the second strategy, that of using sense perceptions of ordinary empirical objects as evidence for mathematical beliefs, assimilates no mathematical knowledge to observational empirical knowledge (as mathematical objects *qua* abstract objects cannot be observed); rather it assimilates all mathematical knowledge, knowledge of particular mathematical objects and of general mathematical claims, to theoretical empirical knowledge.

These two strategies for mathematical empiricism are therefore quite different, and must be dealt with separately. Accordingly, we shall in the remainder of this chapter consider a mathematical empiricism that brings mathematical objects in range of perception, before turning in the next chapter to a theory that grounds mathematical knowledge on perceptions of ordinary empirical objects. Throughout, however, our question will be the same: Is there a satisfactory realist account of mathematical knowledge that locates its ultimate grounds in sense perception?

4.2 Maddy's set theoretic empiricism

Penelope Maddy's early account of set theoretic knowledge is inspired by the Gödelian epistemology considered in Chapter 3 (Maddy (1980; 1990)).[35] Unlike Gödel, however, Maddy grounds set theoretic knowledge in perception:

> What I want to suggest now is simply that we do acquire perceptual beliefs about sets of physical objects, and that our ability to do this develops in much the same way as that in which our ability to perceive physical objects develops ... .
> Maddy (1980, 126)

Thus Maddy does not claim that we have a special mode of apprehension, a "sixth sense", for detecting sets, but rather that we detect and come to know sets using the same senses we use to detect and come to know ordinary physical objects, in short, through perception. Maddy defends this claim by rejecting the orthodox conception of sets as aspatial, atemporal objects. She uses it in an attempt to give an account of how we come to have knowledge of sets, conceived of as independently existing objects. Maddy's account of set theoretic knowledge thus stands as a prime example of the first kind of mathematical empiricism described above, a realist attempt to ground knowledge of mathematics on perception of mathematical objects.

If the claim that sets of physical objects are perceivable is to form the basis of a satisfactory set theoretic epistemology, it must be justified. To this end, Maddy assumes a theory of perception intended to show, in combination with a philosophical theory of sets, that we can and do perceive sets of physical objects. If convincing, this will show how we know some claims about particular sets of physical objects. But it will not show how we acquire knowledge of general claims about sets, and in

[35] Maddy (1997) rejects her earlier position because of doubts concerning its justification of set theoretic realism. Here we argue on independent grounds that her early epistemology is not an adequate account of mathematical knowledge.

particular, knowledge of standard axioms of set theory. To explain how we come by this, Maddy expounds a theory of intuitive knowledge according to which standard axioms of set theory are supported by intuitively known principles generated from set theoretic concepts we acquire from perception. She also appeals to an analogy between natural science and mathematics according to which there is non-intuitive evidence for some set theoretic claims. Note that the kind of intuition involved here is very different from the kind of intuition to which rationalist accounts of knowledge appeal, as, on Maddy's view, we cannot intuitive knowledge of sets unless sets are perceivable.

Maddy's set theoretic empiricism thus involves four components, her theory of perception, her philosophical theory of sets, her theory of intuitive knowledge and her analogy between science and mathematics. As we are interested in the general strategy of appealing to sense perception of mathematical objects in realist accounts of mathematical knowledge, we shall describe only the theory of perception and the philosophical theory of sets, that is, the aspects of Maddy's account that are relevant to the claim that sets are perceivable. What we want to know is whether Maddy makes a plausible case for thinking that we can have perceptual knowledge of mathematical objects. An account of her theory of intuitive knowledge and her analogy between mathematics and science is included in the appendix to this chapter, however, in which we also defend the theory of intuitive knowledge against an objection from the literature.

4.3 Perceptual knowledge of sets of physical objects

Maddy's claim that we perceive sets of physical objects depends on a theory of perception and a philosophical theory of sets. In this section we will describe these and describe how they are intended to underpin the idea that we perceive sets of physical objects.

4.3.1 The theory of perception

Maddy bases her theory of perception on that of Pitcher (1971). She does not explicitly state it, but she does discuss examples in which subjects perceive objects in the relevant sense (1990, 50-51). She closes her discussion of one of these as follows:

> In sum, then, for Steve to perceive a tree before him is for there to be a tree before him, for him to gain perceptual beliefs, in particular that there is a tree before him, and for the tree before him to play an appropriate causal role in the generation of these perceptual beliefs. (1990, 51)

As Maddy says that this is what it is for Steve to perceive an object in the relevant sense, we may take it that she is offering necessary and sufficient conditions for this kind of perception. The condition that Steve gain the perceptual belief that there is a tree before him is designed to ensure that he perceives a tree only if it seems to him as if there is a tree before him. It thus appears that Maddy is concerned with a strong sense of perception according to which a subject perceives an object of a given kind only if they perceive it as an instance of the given kind. The condition that there must be a tree before Steve if he is to perceive one is intended to exclude the possibility of illusion, so that Steve cannot perceive a tree in this sense if there is no tree present, even if it seems to him that there is a tree present. The causal condition is designed to rule out cases in which it may seem to Steve as if there is a tree before him and in which there is in fact a tree before him, but in which the appearance to Steve of a tree before him is not produced in the right way (as, for example, when light from a different tree is reflected from a mirror between Steve and the tree that he seems to see). Generalising from this example, then, Maddy's theory of perception states that a subject A perceives an object of kind K as a K at a location L if and only if:

(i) there is a K-object at L,

(ii) A acquires perceptual beliefs about K-objects, particularly: that there is a K- object at L,

(iii) the K-object at L is appropriately causally related to the generation of A's belief that there is a K-object at L.[36]

Beliefs are here understood to be psychological states that can be attributed to subjects on the basis of their behaviour (*op. cit.*, 52)). Perceptual states are thought of as bodies of beliefs acquired through perception on particular occasions. These are rich in content, are not inferred from other beliefs, need not be conscious or linguistic, and are contentually interdependent (*op.cit.*, 51). The appropriate causal relation is not described in any detail but a situation in which it obtains is borrowed from Grice (1961); an object plays the right sort of causal role for perception when it plays the role of a normally sighted subject's hand in the production of their belief that their hand is in front of them when they hold it up to their face in good light (*op. cit.*, 51).

By making perception of objects of a given kind depend on acquisition of the perceptual belief that there is an object of the kind at the relevant location, this account of perception ensures that to perceive objects as of a given kind one must possess and deploy a concept for the kind. This is a good condition to insist upon. Only a subject possessing a concept for a given kind can structure sensory input causally produced by instances of

[36] Here, to be a K-object is simply to be an object of kind K.

the kind as information about such instances. Ultimately, this is what it is to have perceptions of them.

Nevertheless, Maddy's theory of perception is unduly restrictive. It does not seem necessary for perception of a K-object as a K that A acquires the perceptual belief that there is a K-object at L. This does suffice, together with the other conditions, but so too does the acquisition of any other perceptual belief that contains a constituent presenting the object as an instance of kind K. Another reason for thinking that the theory is too restrictive is that the forming of perceptual beliefs does not seem necessary to this kind of perception either. For it seems possible that, without forming beliefs in which a K-object is presented as a K, A could perceive a K-object as a K provided they acquire, in the appropriate causal way, perceptual information presenting a K-object to them as a K.[37]

However, these difficulties only show that Maddy's theory of perception cannot be accepted as a set of necessary conditions on what it is to perceive an object as of a given kind. They do not undermine the sufficiency of these conditions for this kind of perception; in fact, these conditions seem to characterise quite well situations in which we would have perceptions of the relevant sort. This is important because Maddy has no need to provide necessary and sufficient conditions for what it is to perceive an object as an instance of a given kind. To argue that we have perceptions of sets in this sense, which is what her account of set theoretic knowledge requires, a collection of sufficient conditions will do. The account can thus be protected from these objections simply by taking Maddy's theory of perception as a description of sufficient conditions for A to perceive an object as an instance of kind K. When taken this way, Maddy's it seems to describe rather well circumstances in which we would

[37] Suppose the redoubtable Steve happens to be in Africa and that he believes there are no tigers in Africa. A tiger escaped from a zoo is in front of Steve and it seems to him as if there is a tiger there. Steve doesn't form the belief that there is a tiger in front of him because he doesn't believe there are tigers in Africa; nevertheless perceptual information about a tiger is being causally produced in him in the right kind of way for perception. It seems reasonable in this situation to say that Steve perceives the tiger.

indeed perceive objects as instances of given kinds. At any rate, we shall assume henceforth that it does.

4.3.2 The philosophical account of sets

Maddy proposes that sets of physical objects have location in space and time. Sets are ordinarily thought to lack spatial and temporal properties, so this doctrine is controversial. But Maddy is undeterred:

> Here I must agree that many sets, the empty set or the set of real numbers, for example, cannot be said to have location, but I disagree in the case of sets of physical objects. It seems perfectly reasonable to suppose that such sets have location in time – for example, that the singleton containing a given object comes into and goes out of existence with that object. In the same way, a set of physical objects has spatial location in so far as its elements do. (1980, 127)

Thus on Maddy's account of sets, my crate of beer contains not just some bottles of beer, but also the set containing those bottles. This set occupies the same space as the bottles of beer and persists while they do. For Maddy, the spatio-temporality of sets of physical objects means that they can be taken as members of more complex sets which, themselves, have location in space and time (1990, 59). So in addition to the set of bottles of beer, my crate also contains the set of bottles in the first row of the crate, the set containing this set and all the other bottles, the set containing all the subsets of the set of bottles, etc. Each of these sets is located in the same place and time as the bottles of beer in their transitive closure.

It is clear that under this conception of sets of physical objects, the world around us is a great deal richer, ontologically speaking, than we might have thought. But it is less clear what kind of objects Maddy considers sets in general to be. In the early version of her set theoretic empiricism, sets of physical objects seem to be classified as abstract despite having spatial and temporal properties:

> The arguments of Benacerraf and Lear [that abstract sets cannot be known, or referred to, respectively] ... involve an inference from the fact that sets are abstract objects to the claim that we cannot causally interact with sets. Abstract objects are supposed not to exist in space and time, which presumably provides at least part of the support for this inference. I have now denied that abstract objects cannot exist in space and time, and suggested that sets of physical objects do so exist. (1980, 127, *n*.39)

However, in her later work Maddy sets out the view that sets of physical objects are located in space and time and then continues:

> On some terminological conventions, this means that sets no longer count as 'abstract'. So be it; I attach no importance to the term. (1990, 59)

No doubt Maddy is entitled to define her terms as she wishes. But as it is customary to regard mathematical objects as being abstract in the sense of "without spatial and temporal properties", the attribution of spatial and temporal properties to sets of physical objects implies that they are not abstract in this customary sense applying to mathematical objects. The question then arises of whether, on Maddy's view, there are any abstract sets. Although Maddy addresses this issue during a discussion of the connection between her set theoretic empiricism and physicalism (which she takes to imply that only spatio-temporal objects, and perhaps just those that are also causally efficacious, exist (1990, 156)), she is careful not to commit herself either way. She regards both the denial and the affirmation as live options. In the first case, she thinks it is possible to argue that abstract sets exist and are known by inference from facts concerning non-abstract sets (*op. cit.*, 156), in the ways described by her theory of intuitive knowledge and her analogy between mathematics and empirical science (we explain how in the appendix); in the second case, she thinks it is possible to take the subject matter of set theory to be the hierarchy of sets of physical objects generated in the usual ways from the collection of physical individuals but without formation of the empty set at any stage (*op. cit.*, 156-157). Maddy's philosophical theory of sets thus proposes that sets of physical objects have spatial and temporal location, and leaves it open whether these are all the sets there are, or whether there are also other sets, abstract in the sense of lacking spatio-temporal properties.

4.3.3 Perceiving sets of physical objects

The theory of perception and the philosophical theory of sets described above are put forward to show that we perceive sets of physical objects. But on what grounds is this supposed to follow? Recall that on Maddy's theory of perception it suffices for A to perceive a K-object as a K at L that:

> (i) there is a K-object at L,
> (i) A acquires perceptual beliefs about K-objects, particularly: that there is a K- object at L,
> (iii) the K-object at L is appropriately causally related to the generation of A's belief that there is a K-object at L.

If we are to have perceptions of sets of physical objects, there must be sets of physical objects for which each of these conditions are fulfilled. On the assumption of Maddy's philosophical theory of sets, condition (i) presents no difficulties; sets of physical objects are located just where their elements are, thus it is quite possible for an agent A to be confronted by a set of physical objects at a location L.

With respect to condition (ii), Maddy (1990, 59-60) mentions the empirical evidence of Kaufman *et al.* (1949), which shows that we form certain numerical beliefs non-inferentially. The beliefs in question arise through subitization, the process underlying our ability immediately to identify the cardinality of new collections of objects without having to count them. Together with the fact that such beliefs bear non-inferential links to uncontroversially perceptual beliefs (concerning the size and colour of the objects perceived, for example), this suggests that these numerical beliefs are themselves perceptual. Armed with her realist theory of sets, Maddy argues that such beliefs should be taken to be about small sets of physical objects, on the grounds that these are more attractive than are any of the alternative candidates for this interpretative role, such as classes, properties and physical aggregates (*op.cit.*, 60-63). She thus concludes that we sometimes acquire perceptual beliefs about sets of physical objects, as demanded by condition (ii).

This leaves us with condition (iii). Assuming that sets of physical objects have spatio-temporal location, Maddy argues that there is no block to the suggestion that we are causally influenced by them (*op. cit.*, 49). But this, alone, is not sufficient to perceive a set of physical objects, for condition (iii) requires the right kind of causal influence. For Maddy, this means we must organise our sensory stimulation using a concept of set produced in us through a combination of evolutionary and developmental factors. Her argument that we possess such a concept is quite detailed.

Maddy assumes that there is a correspondence between psychological states and brain states and that to possess a concept is to have the ability to form certain kinds of belief (*op. cit.*, 52). This suggests that possession of a concept is in part a matter of having a particular kind of brain, namely, one whose development is such as to allow formation of brain states corresponding to psychological states involving the concept. Drawing on the neurological theory of Hebb (1949, 1980), Maddy explains this brain development in the case of concepts of perceivable kinds by reference to networks of synapses. When these networks all fire together, which they do under appropriate stimulation from sensory interaction with the environment, the brain state realised corresponds to a perceptual belief about objects of the kind concerned. Thus in the case of triangles:

> Once an integrated cell assembly of this sort has been formed, looking at a triangle will make it reverberate for half a second or more. This represents a considerable gain both in organization and in duration over the random hum of activity brought by the same visual stimulation before the formation of the assembly. This longer, repeatable, trace should persist long enough to allow the structural changes required for long-term memory. In other words, the cell assembly is what permits the subject to see a triangle with identity, to acquire perceptual beliefs about it ... it provides the subject with her concept of triangle. (1990, 57)

According to Maddy, then, there are cell-assemblies in the brain that act as neural "detectors" of instances of perceivable kinds. These assemblies, which are activated by sensory interaction with instances of the relevant kind, are what make possible perceptions of them. So in a manner of speaking, these cell assemblies are physical proxies for concepts of perceivable kinds. Hebb's theory implies that assemblies of this sort form in the brain even for very general categories such as that of physical objects (*op. cit.*, 58).[38]

Assuming this account of the mind, Maddy appeals to behavioural research conducted by Piaget and others on the development of the ability to acquire beliefs about sets (she refers to Piaget (1937), Piaget and Szeminska (1941), Phillips (1975), and Gelman (1977)). This research reveals an analogy between the developmental process resulting in possession of a concept for physical objects and that resulting in possession of a concept for sets. On the basis of this analogy, Maddy

[38] Maddy uses the term "detector" in connection with her triangle example, describing the cell-assembly that responds to sensory stimulation by triangles as a "triangle detector" (1990, 57). Later she talks about "object detectors" responsive to physical objects (*op.cit.*, 58).

argues that our brains develop neural set detectors in much the same way that they develop neural triangle detectors and neural object detectors:

> Given the evidence that the set concept requires a similar developmental period involving repeated experience with sets in the environment parallel to the required experiences with triangles and physical objects, it seems reasonable to assume that these interactions with sets of physical objects bring about structural changes in the brain by some complex process resembling that suggested by Hebb, and that the resulting neural 'set-detector' is what enables adults to acquire perceptual beliefs about sets. (1990, 65)

On the basis of this analogy, Maddy claims that we can enter into the kind of causal relationship with sets of physical objects that is, on her theory, for perception of them, i.e. that condition (iii) can be fulfilled.

4.3.4 Summary

Maddy argues that sets of physical objects are spatial and temporal objects that we can encounter in the world around us; she argues on the basis of Kaufman *et al.* (1949), that we sometimes acquire perceptual numerical beliefs and urges that these should be understood as being about sets of physical objects; finally, by appeal to the work of Hebb, Piaget and others, she argues that our possession of a neural proxy for a concept of set is the result of the same sort of evolutionary and developmental factors that produce the neural apparatus required for perception of other kinds of objects. In this way, it is argued that sets of physical objects can fulfil three conditions sufficient for perception. Thus Maddy concludes that we can perceive sets of physical objects.

Before considering objections to Maddy's view, we should note a difficulty of interpretation. Maddy's theory can be taken either as a description of how in fact we know set theory, or as a description of how empirical knowledge of set theory is possible if Hebb's neurophysiological theory is assumed to be true. Maddy herself explicitly states that her appeal to Hebb's theory is intended heuristically (1990, 55), as part of an explanation of how the claim that we perceive mathematical objects might be made plausible by a respectable neuroscientific theory (*op. cit.*, 67). This strongly suggests that the second reading is the correct one. However, the claim Maddy aims at establishing is that we "can and do perceive sets" (*op.cit.*, 58), which is strong enough to make a merely heuristic appeal to Hebb's theory seem insufficient. This suggests that the first reading must be right. It is thus not absolutely clear which reading of Maddy's theory should be preferred. To avoid this difficulty, our discussion below will concentrate on issues that are neutral between the two readings.

4.4 Do we acquire perceptual beliefs about sets?

As we have explained, the claim that we can perceive sets is of the utmost importance to Maddy's account of set theoretic knowledge. If we do not perceive sets, we would acquire neither perceptions of sets nor intuitive knowledge about sets, and thus would lack an epistemic basis for knowledge of set theory. In the next few sections, we will assess whether Maddy's claim that we can perceive sets is plausible, given her assumption that sets of physical objects are located in space and time. Given this assumption, it is trivial that perceiving subjects are sometimes confronted by sets of physical objects. But can we be so confident that the other conditions of Maddy's theory of perception are sometimes fulfilled so as to lead to genuine cases of set perception? We will consider this below, addressing first the claim that we sometimes acquire perceptual beliefs about sets.

Maddy maintains that numerical beliefs acquired through subitization should be considered examples of perceptual beliefs about sets. Her argument for this claims that these beliefs are perceptual, which seems reasonable, and that they should be interpreted as being about sets. But what reasons are given for this latter view? Maddy maintains that this interpretation of subitized is "the simplest and most reasonable" given the assumption of sets of physical objects (1980, 128). Later she supplements this point with a holistic argument to the effect that sets are "best suited to playing the role of the most fundamental mathematical entity" (1990, 61). This argument is summed up as follows:

> The elementariness of the notion of set, the ease of manipulation, and the immense success of set theory, both as a foundation for other branches of mathematics and as a mathematical theory in its own right, all help to make the set ... the most attractive candidate for the role of number-bearer. (1990, 62)

On the basis of holistic considerations, therefore, Maddy concludes that it is better to take sets as the bearers of number properties than the other available candidates, classes, properties, aggregates and the like.

A first point to make in response to this is that Maddy's holistic argument is not sufficient for her conclusion that subitized numerical beliefs are about sets of physical objects. The argument assumes that all numerical beliefs have to be interpreted in the same way. But it is implausible to put this forward without support when there are various different kinds of situations in which we make determinations of number, subitization, counting and calculating for example. Maddy's argument also assumes that the only kind of interpretation available for numerical beliefs is an ontological interpretation in which numbers are taken to be properties of some general kind of thing like sets or classes. But this assumption can also be questioned. An alternative kind of method takes its cue from Boolos (1984; 1985), using plural logic to understand numerical beliefs as collective attributions of numerical properties to collections of objects. In this way:

There are three eggs in the box

is not interpreted as saying:

The number of members of the set $\{x: x \text{ is an egg in the box}\}$ is 3

But rather is understood to mean:

The eggs in the box are three.

where generalisation on the expression "the eggs in the box" would require use of plural quantification and "three" is understood to express a numerical property.[39] We do not claim that this method of interpretation is

[39] See Yi (1998) for a recent attempt to cash out this method of interpreting arithmetical claims.

the best for subitized numerical beliefs, though it does seem well suited to the task. The point we are making is just that Maddy does not even consider the possibility of this kind of interpretation.

Maddy's argument that sets must always be taken as the bearers of number properties thus rests on implausible assumptions. A second point against it is that it does not establish that concepts for set enter into the content of subitized numerical beliefs, not even if it establishes that subitized beliefs are about sets. A perceptual belief may be about sets in the weak sense of having a content a constituent of which refers to a set. Then again, it may be about sets in the stronger sense of having a constituent that presents the set to which it refers as a set; in this latter case the belief-content involves a concept for set. Clearly the global considerations described above for taking sets as the bearers of number properties give no reason to prefer one interpretation over the other as they do not tell us anything about the constituent concepts our subitized numerical beliefs involve. So even if we accepted Maddy's argument that subitized numerical beliefs must be taken to be about sets it would still be possible for us to take them as being about sets in the weak sense, and thus as not involving a concept for set.

This presents a difficulty because it is crucial to Maddy's account of set theoretic knowledge that we acquire perceptual beliefs that are about sets in the strong sense of involving a concept for set. As we have explained in previous sections, the idea of Maddy's approach is to argue that we have perceptions of sets as sets, to infer from this that we possess a concept for set the structure of which is responsible for intuitive knowledge about sets (this is the neural set-detector), and then to argue from this that perceptions of sets together with intuitive knowledge about them provides a sufficient basis for knowledge of the axioms of ZFC. Clearly if we do not have perceptions of sets as sets, this approach falls apart. So the fact that Maddy fails to make a case for taking subitized perceptual beliefs as being about sets in the strong sense leaves her with no reason to think that we perceive sets in the way required by her explanation of set theoretic knowledge.

For two reasons, then, Maddy's argument that sets should be taken as the bearers of number properties does not establish the required conclusion; it does not establish that sets should always be the taken as the bearers of number properties, and it does not establish that we acquire through subitization the kind of perceptual beliefs required by her account of set theoretic knowledge. Maddy's claim that we do perceive sets of physical objects in this way is thus beginning to look decidedly shaky. But should we reject it? Not yet. The real difficulty here is that Maddy provides no good examples of perceptual experiences in which we might plausibly be said to form perceptual beliefs of the right sort. But this does not mean that there are no such examples. And if we shift our attention away from subitization and direct it instead at situations in which we perceive things to be grouped together in collections like flocks of birds and herds of cattle, then examples of the right kind can perhaps be found.[40]

When we watch some birds flying over a field, we are sometimes visually aware only of the individual birds. However, if the birds are grouped together closely enough in space, and especially if they move together as one, flying in the same direction and changing direction at the same time, then we also become aware of another object over and above the individual birds, namely, the flock of birds. What we are aware of in such a case has a distinctive phenomenological character, for what we see when we see a flock of birds looks different to what we see when we just see some birds individually. Such experiences can be quite marked, watching a group of redshanks flocking over an estuary, making tight turns low over the sand, one gets a real feeling of the flock being an individual in its own right, as if it had a life of its own. This effect arises because of the character of our visual awareness. It thus seems perfectly in order to describe this as a situation in which we perceive the birds as a flock of

[40] I owe the suggestion that we might perceptually gain information about sets in situations like this to Marcus Giaquinto.

birds, where a flock of birds is a collection of a certain kind. Moreover, it seems appropriate to describe this as a situation in which we have such a perception because we acquire perceptual beliefs about the flock in which it is presented as a flock; on the approach to perception we are assuming, this is what explains why it seems to us as if a flock is present rather than its just seeming to us that there are birds present. Given the assumption that sets of objects are located in space and time, and given the platitude that flocks of birds consist, in some sense, of the birds they contain, it may perhaps be reasonable to say that this kind of situation involves perceptual belief about a set, the set of birds in the flock, as a set.

The suggestion that there are situations in which we acquire perceptual beliefs that present sets as sets may also receives support from recent empirical research. The limit for reliable subitization of the cardinality of collections is very low, normally considered to be three. However, by perceptually arranging objects into distinct collections, "chunking" them, as the scientific literature has it, we are able to overcome these limitations and thus keep track of the whereabouts of collections of greater cardinalities. Infants are able to do this as well as adults, which would suggest that there is an evolutionarily selected basis for this ability. Scientists are even beginning to talk in terms of infants looking for sets:

> Research suggests that, using representations from object-based attention, infants can represent only 3 individuals at a time. ... In the present experiments we used a manual search procedure to ask whether infants can overcome this limit of 3 by chunking individuals into sets. ... Experiment 3 demonstrated that infants tracked the 4 objects as two sets of 2, searching for each set in its correct hiding location. That infants represented the number of individuals in each set is demonstrated by their reaching for the correct number of objects in each location. These results suggest that by binding individuals into sets, infants can increase their representational capacity. Feigenson and Halberda (2004)

Because the abilities described here are based in perception, it does not seem too far-fetched to suggest, on the basis of this research, that there may be situations in which we acquire perceptual beliefs about sets represented as sets. This would certainly make good sense of the quoted remarks concerning infants chunking four objects into two sets of two, keeping track of the sets they formed in this way, looking for those sets in hiding locations and representing the number of the sets to themselves.

To summarise this discussion: Maddy's grounds for thinking that we form perceptual beliefs about sets in the strong sense of containing constituents representing sets as sets are not convincing. Her argument that bearers of numbers properties must always be taken to be sets rests on questionable assumptions and is too general to deliver the required conclusion. Nevertheless, initial grounds for thinking that we acquire perceptual beliefs about sets as sets do seem to be available. Everyday experience of collections of objects like flocks of birds, etc., suggests that we acquire such beliefs as does empirical research on chunking abilities in infants. Assuming that sets of physical objects are located in space and time, Maddy's claim that we acquire the kind of perceptual belief required for perceptions of sets as sets may thus not be an unreasonable one to make.

4.5 Are we causally influenced by sets?

The condition (iii) of Maddy's theory of perception connects a subject's perception of an object to their being causally influenced by it in the right way, the way one is influenced by one's hand when one sees it in front of one's face under normal conditions. Since Maddy's sets of physical objects are unusual new additions to our spatio-temporal ontology, it requires an argument to establish that they satisfy this requirement. Black holes and the centre of the universe are located in space and time, yet we do not have any kind of casual commerce with them. Electrons are located in space and time, yet we do not have the right kind of causal commerce with them. So why should we think that the causal precondition for perception is ever satisfied in the case of sets of physical objects?

In answer to this, Maddy argues that the causal efficacy of sets of physical objects is inherited from the causal efficacy of their elements. She draws an analogy between the relation between a physical object and its aspects and the relation between a set of physical objects and its elements (1990, 49). Strictly speaking, we only causally interact with aspects of physical objects, yet still we take it that physical objects genuinely enter into causal relations. Similarly, Maddy argues, sets of physical objects genuinely enter into causal relations, even though, strictly speaking, we only interact with their elements. She suggests thinking of causal interaction with sets of physical objects either (a) as causal interaction with sets of aspects of physical objects or (b) as causal interaction with aspects of objects that are elements of sets (*op.cit.*, *n*.34).

The first of these suggestions does not provide the reassurance we seek. If sets of aspects of physical objects are themselves simply sets of physical objects, the suggestion presupposes what it is meant to show, i.e. that sets of physical objects are causally efficacious. But if sets of aspects of physical objects are not themselves sets of physical objects then it remains unclear how we causally interact with them. Are aspects of physical objects abstract in the customary sense of being aspatial and atemporal? If so, we surely cannot causally interact with them. The suggestion that causal interaction with sets of physical objects comes about through causal interaction with sets of their aspects is thus does not seem plausible.

However, the suggestion that causal interaction with sets of physical objects arise through causal interaction with the physical objects they contain is better. It is part of Maddy's philosophical theory of sets that a set of physical objects is located in space and time in the region occupied by their elements (1990, 59)). Because of this it seems reasonable to consider causal interaction with the elements of such a set as constituting causal interaction with the set itself. Provided we accept Maddy's philosophical account of sets of physical objects, then, it seems reasonable to claim that some of them are causally efficacious. Clearly this approach would leave some sets of physical objects, the set of black holes for instance, lacking causal efficacy, because they contain elements that are not themselves causally efficacious. However, this is not a problem, as Maddy's theory does not require that all sets of physical objects are causally efficacious, only that some of them are.

Assuming that sets of physical objects are causally efficacious, can they enter into the right kind of causal relations to produce perceptions? On Maddy's approach, one causally interacts with a set in this way when the interaction triggers a neural set-detector, a cell assembly the development of which was evolutionarily selected because of its propensity to respond to that kind of causal interaction with sets (though the point is made with reference to the perception of physical objects (*op. cit.*, 58)). If we are to accept this, we need to have a description of the circumstances required for triggering of the alleged set-detector, and we need reasons for thinking that its development is evolutionarily selected.

On Maddy's development of the view, the relevant circumstances would have to be those in which subitization produces numerical beliefs, for, as we explained in the previous section, these are the situations in which she argues we acquire perceptual beliefs representing sets as sets. Since we have argued that this interpretation of subitized beliefs is unfounded, however, these may not be the right conditions to pick. But this does not mean that we lack a way of characterising the relevant circumstances. Having argued that we can regard perception of collections like flocks, herds etc., as examples in which we acquire the right kind of perceptual belief, we can say simply that the relevant circumstances are like those in which we perceive flocks of birds, herds of cattle, etc.

We should consider, then, whether there are reasons for thinking that the development of neural set detectors is evolutionarily selected. If sets of physical objects really are located in space and time, and if we grant Hebb's neurophysiological theory, then it seems such reasons can be found, for the ability to represent sets of physical objects would have been advantageous to our ancestors. Developing this ability would have allowed them to respond more effectively to their environment, helping them to determine which of two sources of food is the more rich, which of two threats is the more dangerous, etc. It may be objected that developing this ability need not involve the development of a neural set detector, and that an assembly for some other kind of thing might do. Well perhaps it might. But it is not necessary to establish that development of a set detector is the only hypothesis available here, merely that it is a plausible hypothesis, given the Hebbian background.

The foregoing remarks suggests that once sets of physical objects are accepted as being located in space and time, the suggestion cannot be dismissed that they enter into the right kind of causal interactions for us to have perceptions in which they are represented as sets. Seemingly, then, sets of physical objects might well satisfy condition (iii) of Maddy's theory of perception. Having already argued that condition (ii) might be satisfied, and having accepted that satisfaction of condition (i) is trivial, given the assumption of spatio-temporally located sets, the conclusion that some sets might be perceivable is therefore beginning to look quite attractive.

However, the literature contains objections to this claim that must be overcome if it is to be borne out.

4.6. Balaguer's objection that we cannot see sets

Balaguer (1998, 28-35) argues that it is not possible to explain our knowledge of set theory by bringing sets in range of sense perception. In his view, any account of sets must take them to be abstract in some sense (even if sets of physical objects are taken to be located in space and time), and it would seem that the mathematically relevant features of sets belong with this abstract component. Thus we could not get any relevant information about sets through perception (*op.cit.*, 30-31). If Balaguer is right about this, then we should accept his conclusion that, "there is still an unexplained epistemic gap between the information we receive in sense perception and the relevant facts about sets" (*op. cit.*, 33)

Why must someone who takes sets of physical objects to be located in space and time nevertheless regard them as being abstract "in some sense"? One point Balaguer puts forward for this view is that if they are not so regarded, the challenge of explaining how knowledge of abstract objects is possible cannot be met by showing how knowledge of sets of physical objects is possible (*op. cit.*, 30).[41] Whatever the truth of this, however, it is not relevant to us. Our project is not to show how knowledge of abstract objects is possible but to discover whether there is a good, realist account of mathematical knowledge whether mathematical objects are taken to be abstract or not.

But Balaguer has a second reason for his view that sets must be abstract in some sense (*op.cit.*, 30-31). If sets are located in space and time just as the agglomerations of matter comprising their elements, and if they consist of the same matter, then it would seem that nothing physical distinguishes the sets from the agglomerations of matter. Yet these are different objects, as can be seen from the fact that the set has a

[41] Actually Balaguer couches his argument in terms of "naturalized-platonist sets". But this makes no difference to the point being made.

determinate number property whereas the agglomeration does not.[42] Similarly, nothing physical seems to distinguish a relatively simple set from a more complex one involving the same urelements ({*a*, *b*, *c*} from {{*a*, *b*}, *c*}, for example). Yet clearly these, too, are distinct objects. Thus what distinguishes sets from agglomerations and complex sets from more simple ones must be non-physical, or "abstract" in some sense (*op. cit.*, 30).[43]

Taking this to establish that sets (all sets) must be abstract in some sense, Balaguer argues from this premise that sense perception cannot provide a basis for knowledge of sets:

> Can I perceive this abstract component of the set? It seems that I cannot. For since the set and the aggregate are made of the same matter, both lead to the same retinal stimulation. Maddy herself admits this. But if I receive only one retinal stimulation, then the perceptual data that I receive about the set are identical to the perceptual data that I receive about the aggregate. More generally, when I perceive an aggregate, I do not receive *any* data about *any* of the infinitely many corresponding naturalized-platonist sets that go beyond the data I receive about the aggregate. This means that naturalized platonists are no better off than traditional Platonists, because we receive no more perceptual information about naturalized-platonist sets than we do about traditional non-spatiotemporal sets. Thus, the Benacerrafian worry still remains: there is still an unexplained epistemic gap

[42] Balaguer uses the word "aggregate" rather than agglomeration. However, Maddy uses the word "aggregate" to mean an agglomeration of matter divided up in some way or other, for example an agglomeration of egg-matter divided up by the property of being an egg (1990, 60). Thus Balaguer says that aggregates (meaning agglomerations) do not have determinate number properties, but Maddy says that aggregates (meaning agglomerations divided up in some way or other) do have determinate number properties. To avoid confusion, we have used "aggregate" in accordance with Maddy's definition and have stated Balaguer's argument in terms of agglomerations.

[43] Balaguer claims that Maddy accepts this when she identifies singleton sets with their elements regarded as individuated things. He argues that this commits her to the view that sets are distinct from agglomerations in virtue of being structured differently, and urges that being structured differently must be a non-physical or abstract feature of the sets (Balaguer (1998, 31)).

> between the information we receive in sense perception and
> the relevant facts about sets. (1998, 32-33)

Balaguer's argument thus seems to be as follows. Any set constructed from physical urelements shares spatio-temporal location with and is constructed from the same matter as the agglomeration of the urelements. Thus all such sets produce the same retinal stimulation as the agglomeration of matter. This means that we get the same "perceptual data" about all these sets as we do about the agglomeration. Thus, since we get no "perceptual data" about anything abstract from the agglomeration (because there is nothing abstract about the agglomeration), we cannot get any "perceptual data" about the abstract features of the sets. But these abstract features of the sets are the mathematically important features of sets (hence Balaguer's description of them as the "relevant facts" about sets). So it follows that we get no "perceptual data" about what is mathematically important about sets. Therefore perception cannot play a useful role in our account of set theoretic knowledge.

What should we make of Balaguer's objection? It is worth noting that the whole argument is in danger of being absurdly strong. My pen shares location with and is constituted by an agglomeration of ink, plastic and metal. Does this mean it is abstract in some sense? And if it is, does this mean that important information about my pen cannot be acquired through sense perception? Balaguer's argument that sets are abstract in some sense relies on the fact that sets have different properties to agglomerations of matter. But surely the same is true of many everyday objects like my pen. If, then, Balaguer's argument about sets is correct, it begins to look as if we might have been quite wrong about how useful sense perception is as a means of acquiring knowledge about ordinary physical objects.

This is, admittedly, no more than a vague worry. But more specific complaints about Balaguer's argument can also be made. The argument assumes that whenever one has the same retinal stimulation one receives the same "perceptual data". One problem with this is that it is unclear just what these perceptual data are meant to be. Clearly they cannot be retinal stimulations, for they lack the necessary intentionality (it makes no sense to say that my retinal stimulation at time *t* is about the tree in front of me, or indeed that it is about anything else). But neither can the perceptual data be perceptions because then it would just be false that the same retinal stimulations always lead to the same perceptual data (the duck-rabbit picture always gives the same retinal stimulation, but sometimes a picture of a duck is seen, at other times a picture of a rabbit) so Balaguer's argument would fail. Perceptual data thus appear as things of another kind, somewhere between retinal stimulations and perceptions proper. But what are they exactly? Without a satisfactory answer to this question, it is not possible to accept Balaguer's argument.

Following on from this, there is a strong feeling that with the theoretical background put forward by Maddy, there can be no perceptual data lurking between retinal stimulations and perceptions (or as Maddy, following Pitcher, would say, perceptual beliefs). The theory of perception described above, taken together with Hebb's neurophysiological theory, paints a picture of sense perception in which our senses receive a continuous flow of sensory input (retinal stimulation in the case of our visual sense) which the very structure of our sense organs and brains presents as perceptual beliefs about various objects. There is no room here for intermediaries to call "perceptual data". So presuming that perceptual data are meant to be items of information acquired through sense perception, the only things these could be on Maddy's account are perceptions themselves. Of course Maddy's account of perception could be wrong, but Balaguer provides no reason to think that it is. So because his argument fails when perceptual data are taken to be perceptions, it gives no clear reason to reject the perceivability of sets.[44]

[44] Balaguer at one point records his opinion that the development of cell-assemblies responsive to sets does not undermine his claim that the perceptual data we get from agglomerations includes no perceptual data about sets: "But this would still be true even if we *grant* that in response to these

These problems over "perceptual data" also affect the other assumption of Balaguer's argument, that perceptual data about abstract matters could not be had from a concrete object. Why not? Why do the perceptual data that arise from retinal stimulations caused by light bouncing off concrete objects never carry information about abstract objects? Without a clear understanding of what these perceptual data are it is not possible to say. And once again, such data are illegitimate from the point of view of Maddy's account of perception. So this second assumption of Balaguer's argument seems to be as objectionable as the first.

Before closing this discussion, it is worth mentioning that Maddy's view is consistent with some of Balaguer's conclusions. The view does not hold that everything about sets can be known through perception, and it would agree with Balaguer's assessment that the existence and structure of many sets is something that we do not, directly anyway, know through sense perception. The disagreement arises over whether a very specific body of beliefs, the numerical beliefs acquired in instances of subitization, should be counted as evidence that we perceive sets of physical objects. Balaguer gives no reason to think that the empirical evidence for taking these beliefs to be perceptual is flawed. Neither does he refute Maddy's claim that the numerical beliefs are best understood as beliefs about sets. Instead, he tries to persuade us to reject Maddy's view with the argument above that no set "abstract in some sense" could be perceived. For the reasons described above, the argument is not convincing. But even had we not arrived at this conclusion, Maddy's detailed account of how we see sets of physical objects may have been felt to be more compelling than Balaguer's somewhat diffuse claims about what is and what is not perceivable.

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data [the data we get from agglomerations], my brain has gone ahead and developed two different cell-assemblies, one for sets and one for aggregates, and that sometimes when I point my eyes at an aggregate, my cell-assembly is activated." (1998, 33). However, this suggests a misconception of what perception is like on the view Maddy puts forward. On that view, there are no perceptual data to which the brain responds; rather, the structure of the brain transforms sensory stimulation into perceptual data.

4.7 Chihara's objection that we cannot see sets

Chihara (1982, 1990) objects to set theoretic realism on phenomenological grounds:

> Suppose that I have completely cleared the surface of my desk, leaving nothing but a single apple. According to Maddy, we may think that there is nothing else on the desk, but in fact, there is something else there, namely the set whose only element is the apple. ... Now what does this set look like? Evidently, it looks exactly like the apple. After all, I cannot see anything else there on my desk that looks different from the apple. Perhaps, then, this set feels different. But when I put my hand where the set is supposed to be, what I feel is no different from what I feel when I put my hand on the apple. Well, does the set taste different from the apple? Take a bite and see. No, the set tastes just like an apple. We thus have an answer to the riddle: What looks like a duck, waddles like a duck, quacks like a duck, smells like a duck, tastes like a duck, ... but isn't a duck? It's the set whose only member is the duck. (1990, 201)

Chihara's main target here is Maddy's view that sets of physical objects are located in space and time (which we will discuss in the next section). But his remarks suggest a difficulty with the idea that we perceive sets of physical objects, even if they are located in space and time. The point is that we do not seem to notice sets as we notice ordinary physical objects. If one were to walk into Chihara's office set up as he describes one would immediately notice, provided one is not blindfolded, one's sight is functioning normally, there is plenty of light, etc., an apple on Chihara's desk. But one would not notice a set containing the apple (and clearly Chihara thinks it is not possible to notice this set in the relevant sense even when one is trying hard to do so). But why not? Why, if we perceive sets of physical objects in the same way we perceive ordinary physical objects, do we not notice them all the time, just as we notice ordinary physical objects like apples and philosophers' desks? Without some plausible answer, the fact that we do not seems to be evidence against the claim that we perceive sets of physical objects, in particular, against the view that we acquire perceptual beliefs about them.

The first issue at stake here is whether the phenomenology of (our alleged) set perception differs from the phenomenology of perception of ordinary objects. Do sets have their own special appearances, so that sets look like sets, just as desks look like desks and apples look like apples? Or do they not, so that sets actually look like desks and apples, or perhaps, don't look like anything at all? The second issue is whether, if there is a difference in the phenomenology, it undermines the suggestion that we acquire perceptual beliefs about sets.

One option here might simply be to deny that any set has its own special appearances. However, given the general approach behind Maddy's account of set theoretic knowledge, it does not seem possible coherently to adopt this position. Only objects that have their own special appearances can seem to us to be present, in the phenomenological sense of "seem" relevant to perception. As we have remarked before, it is crucial to Maddy's account of set theoretic knowledge that our perceptions of sets of physical objects involve it seeming to us in this sense that sets are present; this is what motivates the claim that the content of our perceptual beliefs about sets involve a concept for set, which is required for the account of intuitive knowledge of sets. Thus there is no way to deny that any set has its own special appearances without totally undermining the proposed account of set theoretic knowledge.

A second option would be to argue that all sets of physical objects have their own special appearances, including sets like the set containing Chihara's apple. Chihara (1990, 203) reports that Maddy once claimed her perceptual experiences of an apple were different from her perceptual experiences of a set containing an apple, and takes this to mean that, to her, apples do not look like their unit sets. This suggests that Maddy once thought it possible to respond to Chihara's objection by arguing that, generally speaking, sets of physical objects do have their own special appearances just like ordinary physical objects, and that in the kind of case Chihara remarks upon these different appearances are salient. In response to this, Chihara argues that a difference in perceptual experiences need not indicate a difference in how objects look. Taking the duck-rabbit figure of Gestalt psychology as an example, he claims that objects that look the same sometimes produce different perceptual experiences, and so takes it that Maddy should not infer from the fact that she has different perceptual experiences of apples and their unit sets that these things do not look the same (*op. cit.* 203-204).

It is not clear that Chihara is right about this, for if we are not to detect differences in the way things look by examining differences in our perceptual experience, it becomes rather unclear how we are to detect such differences. Nevertheless, the idea of arguing in response to his objection that sets of the kind he mentions look different from the objects they contain is implausible. Chihara is quite right to think that we should notice sets if they look different from the objects they contain; but in the case he mentions, we don't.

A third way of responding to Chihara's objection would be to claim that some sets of physical objects have their own special appearances, but that some do not, and that sets of the kind he mentions, single element sets that contain objects which have their own special appearances, are of the latter kind. This would be plausible for the straightforward reason that some ordinary physical objects have their own special appearances (apples, cars, etc.) whilst others (electrons, black holes, etc) do not. However, if this is to ground a successful response to Chihara's objection a good reason will have to be given for holding that singleton sets of perceivable objects lack special appearances.

It is clear how to provide such a reason on Maddy's view, for we would say that an object of a given perceivable kind lacks its own special appearances if it is not apt to trigger a neural detector the resonating of which in part constitutes a perception of the object as an instance of the kind. However, as Maddy develops her view, it is clear that this cannot be claimed for sets such as that containing Chihara's apple. Maddy claims that numerical beliefs gained through subitization are perceptual in the sense relevant to her account of set theoretic knowledge. But collections of one object are small enough for us to subitize their number, and this is what we do every time we see a single object. So the way Maddy develops her view, it has to be claimed that singleton sets like that containing Chihara's apple are apt to trigger our neural set detectors. Apparently, then, this option of denying that the set containing Chihara's apple triggers our neural set detector is not available either.

However, in section 4.4 we suggested modifying Maddy's theory in precisely this respect. Instead of arguing that we acquire perceptual beliefs about sets by appeal to examples of subitization, we suggested arguing for this by appeal to examples of perception of collections of objects like flocks of birds and herds of cattle. Once the argument for the acquisition of perceptual beliefs about sets is made with respect to situations like this, we are not committed to thinking of examples of subitization as situations in which perception of sets as sets occurs, and thus can coherently deny that the set containing Chihara's apple is of a kind appropriate to trigger our neural set detector. On this basis we can deny that it has its own special appearance, and thus explain why we do not notice it, without having to deny that all sets lack such appearances. Thus the approach of Maddy's theory, if not the theory as she herself develops it, can be defended against Chihara's objection.

4.8 Problems with the metaphysics of sets

In the preceding sections, we considered whether there are objections to the claim that spatio-temporally located sets of physical objects can be perceived as instances of the kind *set*. With a judicious alteration of Maddy's theory (by arguing for our acquisition of perceptual beliefs in which sets are represented as sets by appeal to perceptual experience of collections of objects like flocks and herds, not examples of subitization) we argued that the claim stands up well to critical evaluation. It may be a surprising view, and it may be wholly at odds with what many philosophers of mathematics have considered to be the right view so far. But granting the underlying metaphysics, and the theory of perception, the claim that we sometimes perceive sets of physical objects seems a reasonable one to make. But does Maddy's philosophical theory of sets paint a coherent picture of sets? In this section we will find out.

One puzzle raised by trying to think of sets of physical objects as located in space and time concerns just where and when to locate them. The principle governing Maddy's thinking about this appears to be that any set whose construction proceeds from physical objects alone has spatio-temporal location:

> But notice: there is no real obstacle to the position that the set of eggs comes into and goes out of existence when they do, and that, spatially as well as temporally, it is located exactly where they are. ... In this way, even an extremely complicated set would have spatio-temporal location, as long as it has physical things in its transitive closure. (1990, 59)

Presumably what Maddy means here is that a set has spatial and temporal location if it has only physical things in its transitive closure. The transitive closure of a set is the set containing its elements, the elements of its elements, the elements of those elements, etc., until the ultimate elements are found (as they must be given the axiom of foundation, AF).

Thus we can imagine the application of this criterion of spatio-temporality in terms of membership trees. Given a set, work your way up through the membership tree; if you find a non-physical object at the end of any branch then the set from which you started is not spatio-temporally located.

Consider, then, the set containing Kant and Plato. Both were physical objects, so the set containing them, the set {Kant, Plato}, has spatio-temporal location, according to Maddy's criterion. But where and when is this set located? Kant did not exist at any time during which Plato lived. Does the set pop into existence with Plato, pop out of existence when Plato dies, only to make a triumphant return with the birth of Kant? This sounds odd.

Admittedly, some spatio-temporal objects can exist for a while, cease to exist and then exist again later. An artefact that consists of several parts can be assembled, dissembled and then reassembled at a later date. However, in this kind of example we can explain what is going on. The artefact can be regarded as a mereological sum of its parts that exists only when the parts are joined together in the right way. It is apt to come in and out of existence because of the continued existence of its parts when they are not joined together the right way. However, this kind of explanation cannot be applied to the set containing Kan and Plato. For a start, there is a long period during which neither Kant nor Plato exist. So even if it is appropriate to think of Kant and Plato as parts of the set, there is a long period between the early and late periods of its existence during which even its parts do not exist. More importantly, it is just not appropriate to think of sets of physical objects as mereological sums of their members. The mereological sum of parts *a* and *b* has number property one; the set containing those parts has number property two.

Perhaps, then, Maddy should set a more stringent condition on which sets exist in space and time. A more demanding criterion would state that a set exists in space and time if and only if it has contemporaneous physical objects in its transitive closure. But this criterion seems to invite baffling questions too. We saw in section 4.7.4 that Chihara (1982, 1990) objects to set theoretic empiricism on the basis of a difference between the phenomenology of our (alleged) set perception and the phenomenology of our perception of ordinary objects. However, Chihara also has serious misgivings about the underlying metaphysics of set theoretic empiricism. Immediately following the passage concerning the apple, Chihara adds these remarks:

> Actually, the situation is worse than it may appear at this point, for it is not just the unit set whose only element is the apple that is on my desk. Since this unit set has location in space, can be perceived, etc., we can infer that the unit set whose only element is the unit set also has location in space, can be perceived, etc. For Maddy accepts the following principle: If objects A, B, ... have location in space, then the set of these objects has the same location in space. It follows that there is on my desk, not only the apple and the unit set whose only element is the apple, but also the unit set whose only element is this unit set whose only element is the apple. Clearly, by this line of reasoning, we can infer that there are infinitely many such objects on my desk. And all these different objects take up exactly the same amount of space on the desk. Furthermore, this infinity of abstract objects came into existence when the apple came into existence and they will all go out of existence when the apple goes out of existence. (1990, 201-202)

The tone of Chihara's remarks, and the context in which they appear, make clear that he finds all these consequences of Maddy's metaphysics quite unbelievable. As he points out (1990, 202), the difficulty is not specific to sets containing just one element, if we start with a cup and a saucer, we also have a set containing the cup and the saucer, a set containing that set and the cup, a set containing that set and the saucer, etc. Clearly, then, Chihara doubts that whenever there are some contemporaneous physical objects, there is a set in space and time whose elements are those objects.

One difficulty that seems to underlie Chihara's complaint concerns the massive amount of superposition that goes on under Maddy's view of sets, given the principle of set existence suggested above. We do not find it easy to accept that distinct objects can share location in space and time. So is there not something wrong with a view that leads to all these supposedly distinct sets occupying the same place? In response to this worry, Maddy (1990, 59) claims that it is no more objectionable that sets of physical objects are distinct from, but share location with, the aggregates formed from their elements than it is that a pack of cards is distinct from, but shares location with, its fifty two cards. But this example does nothing to dispel the worry. The fifty-two cards taken together constitute the pack of cards. Thus we do not have here an example of distinct objects sharing the same location.

Perhaps, though, the pack of cards was just a poor example to pick. There is a school of thought in philosophy which takes physical objects to be distinct from the matter of which they are constituted, for example that takes a statue like Rodin's *Thinker* to be distinct from the lump of bronze from which it was formed. If this is a plausible approach to take to some physical objects, then it may be that Maddy could maintain a similar view in the case of her sets of physical objects.

However, in the case of statues and the matter from which they are formed, we have a plausible reason for believing in distinctness. The *Thinker* has different modal properties with respect to its location in space and time than does the lump of bronze from which it is made: it could not exist before Rodin was born, for example, whereas the lump of bronze could. Thus, by the indiscernibility of identicals, the *Thinker* and the lump of bronze are distinct. On Maddy's view of sets, in contrast, it appears that the set containing Chihara's apple necessarily exists exactly where and when the apple exists. Thus, the comparison with statues and the lumps of matter from which they are constituted does not help us to understand how sets of physical objects could be located as are the aggregates of their elements.

Note, too, that even proponents of the view that distinct objects can occupy the same spatio-temporal location do not claim that this is possible for distinct objects of the same kind. Chihara's apple and the set containing it are different kinds of thing, so maybe that is sufficient reason to think they can be co-located yet distinct. But what of the sets {apple} and {{apple}}? These are the same kind, and necessarily share the same spatio-temporal location, on Maddy's view. In fact, this view implies that every one of the class-many singletons (sets containing one element) that can be constructed from the apple are of the same kind and share exactly the same spatio-temporal location. Comparison to the case of statues and the lumps of matter from which they are constituted does help to dispel our unease at the idea of all these objects existing in the same place. Thus far, then, it does seem reasonable to avoid the difficulty by denying that there are sets of physical objects in space and time.

A related puzzle that Maddy herself draws from Chihara's apple example is that of saying what difference there is between the apple and the set containing it, given that they look the same.[45] Maddy claims that one can respond to this by saying that there is an unperceivable difference between the apple and the set (1990, 152). However, this does not resolve the problem. We want to know what the feature is whose presence in one and absence in the other is unperceivable. Maddy also claims that it is possible to respond to the difficulty by identifying, as a matter of convention, the sets containing a single physical individual with the individual they contain (*loc.cit.*), indeed she states a preference for this response (*op.cit.,* 153). Certainly this resolves the difficulty, as it implies that there is no difference between the apple and the set containing it,

[45] It is unclear, for the reasons discussed in section 4.6.4, that Maddy is entitled to the view that sets containing one perceivable object look different from the perceivable object they contain. However, when her claim that sets are perceivable as sets is defended in the way suggested in section 4.6.1, this view can be maintained. Thus the puzzle to which Maddy responds does arise.

which is at least an answer to our question. However, the very fact that she is considering this *ad hoc* move suggests the lack of a stable metaphysical view. Moreover, the suggestion is not workable, not least because it involves attributing to physical objects lots of properties that we hitherto did not suspect that they had, but also for mathematical reasons.

Identifying singletons with their elements would contradict the axiom of foundation, AF, which states that every non-empty set x contains an element y with no elements in common with x. For let a be a physical object. Then, if we identify singletons with their elements, so that $a = \{a\}$, the only element of $\{a\}$, a, has an element in common with $\{a\}$, namely a. So it is not true that every set contains an element minimal in the sense required by AF.

A response to this might be to argue that AF can be given up. It is well known that the axiom system ZF + ¬AF is inconsistent only if ZF is inconsistent. Moreover, it can be argued that AF is not mathematically active in the way the other axioms are, since it is not assumed in the proofs of important mathematical results (in contrast, say, to the axiom of choice). However, being assumed in proofs is not the only way for an axiom to be mathematically active. It is only by virtue of satisfying AF that sets form the cumulative hierarchy anticipated by the iterative conception of sets (for details of which see Boolos (1971)). Denying AF thus requires abandoning the iterative conception, together with whatever support it lends to axiomatic set theory (for an explanation of how the iterative conception supports the axioms see Schoenfield (1977)). Clearly it would be unwise to take this course.

In addition to contradicting AF, identifying singletons with their elements would disrupt even the axiom of extensionality, AE, which says that sets are identical if and only if they have the same members. Maddy tries to block this consequence by limiting the identificatory move to singletons containing physical individuals, that is, singleton sets formed in the first stage of set construction, from physical individuals (*op. cit.*, 152-153).

Thus, for example, {{*a*, *b*, *c*}} is not to be identified with {*a*, *b*, *c*}; these sets are to be held distinct as required by AE. However, it is by no means clear that what Maddy says is enough to stop the process of identification from ascending to higher stages of set construction. Since she claims that we can perceive sets of physical objects, and since we cannot perceive non-physical things, sets of physical objects are themselves physical, on Maddy's view. So unless there is something stopping sets of physical objects from being "individuals" in some peculiarly relevant sense, saying that the identification of singleton sets is only to apply to singletons containing physical individuals is not sufficient to limit the identification to singletons formed at the first stage of set construction. Moreover, it is not clear that there are grounds on which to insist that the process of identification stop there. If it is legitimate to identify a physical non-set with the singleton containing it (even though we ordinarily think that non-sets have no members, are not produced by set construction, and so on), why is it not legitimate to identify a physical set with the singleton containing it (even though we ordinarily think that the sets contains different members from the singleton sets containing them, that they are formed at different stages of set construction, and so on)? There appear to be no grounds upon which to make this distinction. Thus it seems that the identification of singleton with element should be made at higher stages of set construction if it is made at the first stage.

Now an example of what this means is that the set {{*a*, *b*, *c*}} is to be identified with the set {*a*, *b*, *c*}. But if we accept this identification, taking {{*a*, *b*, *c*}} and {*a*, *b*, *c*} to be the same set, then we must abandon AE. For otherwise it becomes utterly unclear what the members of this set are. As we cannot possibly abandon AE, we cannot identify singletons with their elements, at any stage of set construction.

What this shows is that identifying sets of physical objects with the aggregates of their elements does not provide a workable answer to the question of what, if anything, distinguishes them. The criterion of spatio-temporality we suggested for Maddy in response to our first puzzle, that sets are located in space and time that have contemporaneous physical objects in their transitive closure, thus seems not to address Chihara's puzzle. This does not mean that there is no adequate solution but we have not yet seen what a solution might be like.

Our discussion in this section started from Maddy's criterion of set spatio-temporality, suggesting that it could not deal with a puzzle concerning sets of non-contemporaneous physical objects. The alternative criterion we suggested ran into Chihara's puzzle of explaining the difference between sets of physical objects and the aggregates of their elements. The solution to this put forward by Maddy turned out to conflict with the very axioms of set theory, principles that any version of set theoretic empiricism really must support. In trying to answer the very straightforward question of where and when sets of physical objects are located in space and time, we have thus had to deal with first one, then another baffling puzzle about the nature of sets, finally arriving at reason to doubt that the view that sets are located in space and time can coherently be maintained for objects satisfying the axioms of ZFC. What we have witnessed, therefore, is not just a series of objections and replies but rather the erratic unravelling of a bad idea. Our discussion shows not only that it is unclear whether a coherent conception of the nature of sets underlies this account of set theoretic knowledge, it also shows that no such conception is available that agrees with the axioms of set theory. In light of this, we should conclude that the very idea behind this approach, that of making set theoretic knowledge depend on perception by bringing sets into space and time, lacks a satisfactory metaphysical basis.

4.9 Conclusion

In this chapter, we have considered the empiricist strategy of bringing mathematical objects into space and time so as to make them perceivable and hence knowable. We considered Maddy's account of set theoretic empiricism as an example of this approach but argued that it is not convincing.

Maddy's tactic was to regard (some) sets of physical objects as on a metaphysical and epistemological par with ordinary physical objects like chairs and tables. She proposed a philosophical account of sets according to which sets of physical objects are located in space and time (section 4.3.2). By thus bringing sets into the causal nexus, and by appealing to certain theories of perception and neurophysiology, she hoped it would be possible to view set theoretic knowledge as being based on perceptions of sets. We defended Maddy's theory of perception as a collection of sufficient conditions for perception of objects as instances of given kinds (section 4.3.1). We also defended her view that, given her theory of perception and her philosophical theory of sets, sets are perceivable. In particular, we defended this claim against objections from Balaguer (section 4.6) and Chihara (section 4.7). However, by appeal to some observations of Chihara concerning the metaphysics of sets, we subsequently argued that the claim that sets of physical objects are located in space and time conflicts conceptually with the axioms of set theory, that there is no coherent conception of sets under which both the axioms of set theory and the claim that sets of physical objects are located in space and time can be maintained (section 4.8). Maddy's account of set theoretic knowledge thus seems deeply implausible, as it depends on a philosophical conception of sets at odds with our mathematical conception of sets.

Of what significance is this for the empiricist strategy under consideration? As any satisfactory account of mathematical knowledge will have to account for our knowledge of set theory, our conclusion suggests that regarding mathematical objects as on a metaphysical and epistemological par with ordinary physical objects like chairs and tables is not a good ploy for empiricism. But another approach is to regard the nature and knowledge of mathematical objects as comparable to that of properties of physical objects like redness. This suggestion cannot simply be dismissed as both its ontological and epistemological aspects have been defended in the literature (see, e.g. Bigelow (1988) and Giaquinto (2001), respectively). Moreover, our objection to Maddy's theory does not obviously transfer over to this kind of view, even though it may perhaps be the case that a similar kind of objection can be made. The effect of our arguments, then, is to rule out one tactic for the empiricist strategy of bringing mathematical objects into space and time so as to make them perceivable and hence knowable. But for all we have said, it may still be possible successfully to develop this approach the other way, by making mathematical objects participate in our sense perceptions in something like the way that physical properties do.

Appendix: Knowledge of the axioms of set theory

We said in section 4.2 that Maddy's theory of set theoretic knowledge depends on a theory of intuitive knowledge and on an analogy between science and mathematics. In this appendix, we will explain how Maddy uses these claims to argue for her view that set theoretic knowledge is empirical knowledge. We will also defend her theory of intuitive knowledge against an objection from the literature.

A. Intuitive knowledge of sets

Maddy claims we have intuitive knowledge about sets. The idea here is that some set theoretic beliefs arise spontaneously in subjects who have had perceptual commerce with sets of physical objects, and that such beliefs are justified by the complex causal relationship between sets of physical objects and agents that ultimately explains how these beliefs are produced.

This idea depends on the suggestion (considered in section 4.3.3) that the brain develops neural detectors of one sort and another, cell assemblies that are active in our perceptions of objects of the relevant kinds. It is important to realise that these assemblies are potentially quite complex, built up of various neural substructures. Maddy thinks these substructures could correspond to features of the kind that the larger cell assembly detects. And on this basis, she claims that some beliefs about the objects our brains detect using these cell assemblies could be produced by virtue of the structure of the cell assemblies doing the detecting:

> A subject with such an assembly would automatically have various general beliefs about the nature of the objects that stimulate it; we might say that these beliefs are 'built into' the cell assembly much as three sidedness is built into the triangle detector in the form of mechanisms stimulating eye

> movements from one corner to another ... . Crudely put, the very structure of one's triangle-detector guarantees that one will believe any triangle to be three-sided. Similarly, anyone with a general physical object assembly would believe that physical objects are 'space-occupying and sense-stimulating', to use Hebb's examples, or observation and trajectory independent, to use examples mentioned earlier. These are primitive, very general beliefs about the nature of whatever stimulates the appropriate higher-order assembly. I call them 'intuitive beliefs'. (1990, 69-70)

According to Maddy, then, intuitive beliefs about objects of perceivable kinds could be produced in us by the structure of the neural substrate underlying our ability to acquire perceptual beliefs. But this is not all. Maddy points out that the presence in our brains of these neural structures is supposed to result partly because of the evolutionary pressures on our distant ancestors and partly because of our own experiences with physical objects of the appropriate kinds during early stages of cognitive development (1990, 58, 72). Thus the presence in us of intuitive beliefs is to be explained by the existence of a complex causal relationship between us and the objects the beliefs are about. This, in Maddy's opinion, would be sufficient to show that intuitive beliefs are justified (*op. cit.,* 72). The idea, familiar from causal theories of knowledge, is that the causal relationship responsible for the production of intuitive beliefs gives a sense in which their content is constrained by the actual nature of the objects they are about.

From the picture of the mind given in the Hebbian neurological theory, then, Maddy draws a theory of intuitive knowledge. This states that the structure of the neural assemblies required for perception of objects of various kinds gives rise to certain intuitive beliefs about such objects and that these beliefs are justified by the complex causal relationship that explains their production. According to Maddy, certain consequences of this theory are as follows:

(i) Intuitive beliefs, or, at least, linguistic formulations of intuitive beliefs (recall from 4.3.1 that for Maddy beliefs need not be linguistic entities), are distinguished from other beliefs by their obviousness (*op. cit.*, 70, 72).

(ii) Intuitive beliefs are fallible, as there is no absolute guarantee that the perceptual concepts we develop correspond to existing kinds of object (*op. cit.*, 71).

(iii) A linguistic formulation of an intuitive belief may be false for this reason, or because it inaccurately expresses the content of an intuitive belief, (*op. cit.*, 71).

In some respects, then, Maddy's theory of intuitive knowledge resembles the theories of intuition considered in Chapter 3. However, Maddy's theory does not posit a faculty of intuition peculiar to mathematics (as Gödel's did) or peculiar to formal knowledge more generally (as Katz's did). Hers is a general theory of intuitive knowledge that, if successful, shows how intuitive knowledge explicable in empiricist terms arises in any serious study of perceivable objects.

This generality in the theory of intuitive knowledge, together with the suggestion that we can perceive sets of physical objects, is what makes possible intuitive knowledge of general claims about sets. Having suggested in her account of the perception of sets that we develop neural cell assemblies sensitive to particular sets of physical objects, Maddy uses this to explain how we would come to have intuitive knowledge about sets:

> What goes for physical objects should also go for sets: the development of higher-order cell-assemblies responsive to particular sets gives rise to an even higher-order assembly corresponding to the general concept set. The structure of this general set assembly is then responsible for various intuitive beliefs about sets, for example that they have number

> properties, that these number properties don't change when the elements are moved (barring mishap), that they have various subsets, that they can be combined, and so on. And these intuitions underlie the most basic axioms of our scientific theory of sets. (1990, 70)

As with other intuitive beliefs, intuitive set theoretic beliefs would count as knowledge by virtue of a complex causal relationship, one which this time explains the generation of these beliefs on the basis of our, and our ancestors', causal interactions with sets of physical objects. And as with other intuitively known claims, it is their obviousness (when linguistically formulated) that marks them out as set theoretic intuitions, and so provides evidence for their truth.

B. The analogy between mathematics and science

Although Maddy believes that some general claims about sets are intuitively known, she does not think that all axioms for set theory can be known this way. Indeed, the only axioms she talks about in connection with purely intuitive knowledge are the axiom of unions and the axiom of pairs, and even these are carefully described as "nearly unadorned" intuitions rather than intuitions proper (1990, 125).[46] To explain knowledge of other standard axioms for set theory from the perspective of set theoretic empiricism, Maddy appeals to her analogy between science and mathematics.

The claim, which Maddy attributes to Gödel, is of a methodological parallel between mathematics and science:

> According to Gödel, higher set theory bears a relation to the rest of mathematical knowledge and to practical mathematical

[46] The axiom of unions states that for any set x there is a set containing all and only the elements of the elements of x. The axiom of pairs states that for any set x and any set y there is a set containing x and y.

> dealings of everyday life which is analogous to the relation borne by theoretical physics to physical science in general and to common-sense knowledge of the world. Sense perception gives us knowledge of simple facts about physical objects, and a faculty of mathematical intuition gives us knowledge of sets, numbers, and of some of the simpler axioms concerning them. In both cases, theories involving 'unobservable' entities or processes (that is, entities or processes beyond the range of sense perception or mathematical intuition) are formed in order to explain, predict, and systematize the elementary facts (of perception or intuition) and are judged by their success. (1980, 114)

The parallel is this: there are corresponding justificatory foundations for set theory and theoretical physics, and these two disciplines grow from their respective justificatory foundations in similar ways. In the case of theoretical physics, sense perception yields knowledge of truths about the observable, and the succession of physical theories that delve beyond, culminating in theoretical physics, are each justified by their function as explanations, systematisations and predictive mechanisms for this base. In the case of set theory, mathematical intuition yields knowledge of sets of physical objects, and the succession of theories about other sets, culminating in full set theory, are justified by their function as explanations, systematisations and predictive mechanisms for this base.

Now Maddy rejects the suggestion that we possess a faculty of mathematical intuition. Thus she must reject this methodological parallel between science and mathematics. However, by bringing sets of physical objects in range of sense perception Maddy is able to preserve one of its underlying ideas. Set theory is still to be considered analogous to theoretical physics in the way it grows from its justificatory foundation. But we are to think of the justificatory foundation in both cases as a body consisting of sense perceptions and intuitive beliefs generated by the neural foundations of our sense perception. Thus a methodological parallel between science and mathematics is preserved from the point of view of set theoretic empiricism, which makes available non-intuitive grounds for set theoretic beliefs.

Maddy calls these non-intuitive grounds for set theoretic beliefs extrinsic justifications (contrasting with the "intrinsic" grounds of intuition). She lists a variety of features of set theoretic principles that might provide such extrinsic evidence: they may be rich in verifiable consequences; they may provide new approaches to as yet unanswered problems; they may contribute to the simplicity and systematicity of our theory of sets; they may imply prior conjectures or results considered to be "natural"; they may be supported by connections to other branches of mathematics (1990, 145). She also conducts a detailed investigation into how considerations such as these were brought to bear on the axioms of ZFC by a particular group of practicing set theorists (1988a, 1988b). Maddy endorses justifying set theoretic axioms by appeal to extrinsic justifications and takes it that they will complete the grounding of set theoretic axioms not wholly established by intuition.

C. Lomas's objection to Maddy's theory of intuitive knowledge

Lomas (2002, 219-220) argues that Maddy's theory of intuition cannot deliver knowledge of the axiom of pairs, on the basis that no account of intuition grounding intuition on perception could do this. One point that should immediately be made in response is that Lomas draws his understanding of set theoretic empiricism from Maddy (1980) rather than Maddy (1990). Although the spirit of the enterprise is preserved from the earlier to the later work, some of the details do change. In particular, Maddy (1990) no longer takes the axiom of pairs to be intuitive; rather it is presented as the closest thing we have to an "unadorned intuition". Taken at its word, therefore, Lomas's criticism is irrelevant because the point it seeks to demonstrate, that the axiom of pairs is not intuitive, is no longer part of Maddy's set theoretic realism. However, the gist of his objection can be preserved if it is taken to be that the axiom of pairs could not even come close to being an "unadorned intuition"; it is thus still instructive to consider it.

The claim that no account of perception-based intuition could deliver intuitive knowledge of the axiom of pairs is quite general. The first reason Lomas puts forward in its favour is that sets exist, according to the axiom, which are not perceivable physical sets:

> Consider sets which consist of two elements: another set and an element of that other set, e.g., $\{\{x, y\}, x\}$. If the above mentioned axiom holds, this should constitute a set. $\{x, y\}$ is an object (namely a set) and x is an object; therefore by the axiom, $\{\{x, y\}, x\}$ should be a set. However, it is not at all clear that there are perceivable physical sets which have the structure of $\{\{x, y\}, x\}$. The difficulty pertains to the element x which is both an element of the internal set and an element of the external set. An attempt to describe an example of a physical set with this structure illustrates the difficulty. Suppose one has two baskets, one large, the other small. Put the small basket inside the large basket. Put a white egg and a brown egg inside the small basket. So far we have constructed the physical set {{*brown egg, white egg*}}, if Maddy's construal of physical sets holds. (The external set is the large basket and the internal set is the small basket.) What we need is a physical set described by {{*brown egg, white egg*}, *brown egg*} where the brown egg appears both inside the small basket and outside it and in the big basket. This is impossible. The brown egg cannot be in both places at once. (2002, 220)

Here we must assume that talk of constructing the set {{*brown egg, white egg*}, *brown egg*} is metaphorical. On Maddy's view this set exists independently of our constructions, for as long as the brown and white eggs exist, regardless of where they are located. Presumably what Lomas really means is that by moving eggs into baskets in the way described we bring sets containing them to our visual attention. Then his point is that there is no way of bringing a set with the relevant mathematical structure to our visual attention, so that we cannot see such sets (or at least, see them as having the relevant mathematical structure).

This seems to be a very poor reason for thinking that the axiom of pairs is not intuitive. To being with, it is possible simultaneously to receive two visual presentations of one object at the same time, for example if we have a system mirrors making a given egg appear at two locations in our visual field, or if, as a result of gravitational lensing, we see the same star appearing in two different places in the night sky. We are thus capable of seeing two objects x and y as grouped together and of simultaneously experiencing a visual presentation of x some way apart, as for instance if we see a brown egg and a white egg close together and experience a mirror image of the brown egg some distance away. As Lomas gives no reason to think that this would not count as having a set with the relevant mathematical structure brought to our visual attention, he cannot claim to have shown that such sets cannot be seen.

In addition to this, the fact that some sets whose existence is implied by the axiom cannot be perceived does not mean that no sets whose existence is implied by the axiom can be perceived. On Maddy's view there are many pairs of perceivable objects that form a perceivable set containing two elements (Lomas's white and brown eggs, for example). So she can say that perceptual experiences we have in situations involving such objects during the developmental phase relevant to acquisition of the neural set detector gives rise to a set detector which has "built into it" the idea that any two objects can be taken together to form a set. This would suffice, on her view of intuitive knowledge, to make the axiom of pairs intuitive.

Lomas puts forward a second reason for thinking that no account of intuition based in sense perception could make the axiom of pairs intuitive:

> Moreover, perception-based intuition would suggest that such a set is impossible because we do not simultaneously perceive the same object in two places at the same time. Accordingly, perception-based intuition can conflict with the axiom (stated above). Perception-based intuition stands in the way of accepting the truth of the axiom because perception-based intuition does not accept the set {{*brown egg, white egg*}, *brown egg*}, even though the object *brown egg* and the object *small basket containing a brown egg and a white egg*, {*brown egg, white egg*}, can be both separately perceived and accepted by perception-based intuition. (2002, 220)

It is hard to assess precisely what claim is being made here because Lomas does not explain what "perception-based intuition" is nor what it is for a set to be "accepted" by perception-based intuition. But the point seems to be that because we are unable to perceive the set {{*brown egg, white egg*}, *brown egg*} and because we are able to perceive the object *brown egg* and the set {*brown egg, white egg*}, perception-based intuition will tell us that there are pairs of sets that do not form sets so that a principle conflicting with the axiom of pairs is intuitive. Thus, perception-based intuition will "stand in the way of accepting the truth of the axiom".

Again, however, this gives us no reason to reject the intuitiveness of the axiom of pairs. Given what we have said concerning the possibility of simultaneously receiving distinct visual representations of the same object at two different locations, it is not clear that it is not possible to perceive a set if this requires that the same object be perceived at two different locations. Maddy's theory of perception does give grounds to reject this if it is taken as expressing necessary conditions for perception. But we are taking it only to express sufficient conditions, so there is no reason for us to think that this is ruled out. Moreover, the phenomenon of gravitational lensing suggests that we do sometimes see the same object at different locations. Contra Lomas, then, it may be that there are cases in which we can perceive sets of objects through perceiving the same object in different locations.

A second reason to reject Lomas's argument is that he provides no reason to think that perception-based intuition will tell us that there are pairs of sets that do not form sets. On the basis of his considerations, one might think it would be intuitive that there are pairs of perceivable sets that do not form perceivable sets. But clearly this does not stand in the way of accepting the truth of the axiom of pairs. This axiom does not demand that a set formed from two perceivable sets be perceivable, it just demands that the set exist. Maddy's theory of sets does not make the demand that a set formed from two perceivable sets be perceivable, either, because it does not demand that every set be perceivable. The suspicion that perception-based intuition must deliver intuitive principles that conflict with the axiom of pairs thus appears groundless, and so the axiom might still be intuitive. For this reason, and for the reasons explained above, we conclude that Lomas's attack on perception-based intuition is not successful.

5

Realism and the applications of mathematics

In the last chapter, we distinguished two empiricist strategies for realist accounts of mathematical knowledge. These were to argue that mathematical knowledge is grounded on sense perceptions of mathematical objects, and to argue that mathematical knowledge is grounded on sense perceptions of ordinary empirical objects. Having considered the first strategy in the last chapter, we proceed now to a consideration of the second.

The argument that mathematical knowledge is grounded on sense perceptions of ordinary empirical objects assimilates mathematical knowledge to theoretical empirical knowledge. It claims that mathematical beliefs enjoy the same kind of support as theoretical empirical knowledge by virtue of the fact that they enter into our theorizing about empirical matters. Clearly mathematics only enters our theorizing about empirical matters in its empirical applications. So to assess this strategy for mathematical empiricism, we need to address the following question: Do the empirical applications of mathematics provide adequate grounds for mathematical knowledge?

5.1 Quinean realism

The idea that evidence for mathematics is provided by its empirical applications is an important theme of Quine's philosophy.[47] So, too, is mathematical realism, the view that there are independently existing mathematical objects.[48] We can thus take from Quine's work a theory according to which mathematical knowledge is secured on its empirical applications. Convenience demands that this theory have a label, so we will refer to it as Quinean realism. Bear in mind however, that this is more in homage than in attribution, for although the view in question is strongly suggested by Quine's work, it is not explicitly stated in it.

5.1.1 Quine's theory of evidence

The first component of Quinean realism is a theory of evidence. With the same qualification as before, we will refer to this as Quine's theory of evidence. The background to this is set by a kind of epiphany:

> the recognition that it is within science itself, and not in some prior philosophy, that reality is to be identified and described. Quine (1981a, 21)

The philosopher who undergoes this experience:

> sees natural science as an inquiry into reality, fallible and corrigible but not answerable to any supra-scientific tribunal, and not in need of any justification beyond observation and the hypothetico-deductive method. Quine (1981b, 72)

[47] It is also clearly voiced by Putnam (1971). In fact, the suggestion that the empirical applications of mathematics are of significance for philosophical debates concerning mathematical knowledge has a long and distinguished pedigree. Frege put the point like this: "it is applicability alone which elevates arithemetic from a game to the rank of a science." (Translated by M. Black from Frege's Grundgesetze der Arithmetik, II, §91, see Geach and Black (1952, 187).

[48] Though this was not always Quine's position, see section 5.1.2.

Together these quotations home in on naturalism, one of the most important and widely discussed aspects of Quine's philosophy, and the source of two components of Quine's theory of evidence.

The first of these is the core empiricist belief that the evidence there is for science is provided by sense perception, and sense perception alone. Quine's remark that science is "not in need of any justification beyond observation and the hypothetico-deductive method" presupposes the positive aspect of this belief, namely, that sense perceptions (in the form of observations) do in fact provide evidence for scientific beliefs. His remark that science is "not answerable to any supra-scientific tribunal" appears to endorse its negative aspect, the idea that the only evidence there is for scientific beliefs is evidence provided by the senses. If any room is left for doubt about this, Quine clarifies matters elsewhere, stating that "whatever evidence there *is* for science *is* sensory evidence" (1969, 75). Provided we understand this to refer to sense perceptions rather than mere sensation, we have here an unambiguous endorsement of the empiricist belief described.

The second naturalistic component of Quine's theory of evidence is suggested by the remarks that "it is within science that reality is to be identified and described" and that "science is not answerable to any supra-scientific tribunal". This vote of confidence in the methods of science rejects doubt that what according to science provides compelling support for a claim might nonetheless not be sufficient to warrant belief that it is true. Quine's theory of evidence thus contains the principle that, in normal circumstances, what counts by ordinary scientific criteria as compelling support for a claim warrants belief in its truth. Call this the "evidence-warrant principle".

One more thesis is to be included in Quine's theory of evidence, but this is not a consequence of Quinean naturalism. It comes from Duhem:

> the physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses; when the experiment is in disagreement with his predictions, what he learns is that at least one of the hypotheses constituting this group is unacceptable and ought to be modified; but the experiment does not designate which one should be changed. Duhem (1954, 187)

The point being made here is quite simple. If two hypotheses jointly imply a claim that is disconfirmed by observation, and if neither of the hypotheses imply that claim on their own, then, logically speaking, we can say only that our observation disconfirms the conjunction of the hypotheses, not that it disconfirms one or the other. On the positive side of the same logical coin, if observation confirms the implied claim, then it is the conjunction of the hypotheses that is confirmed. Duhem's point suggests that evidence attaches to hypotheses not individually but in systems, regardless of the nature or number of the hypotheses involved. It thus leads us to confirmational holism, the claim that scientific evidence attaches to all the hypotheses required to generate (successful) predictions.

Struck by this thought, Quine compares the evolving corpus of human knowledge to a fabric or a force field:

> The totality of our so-called knowledge or beliefs, from the most casual matters of geography and history to the profoundest laws of atomic physics or even of pure mathematics and logic, is a man-made fabric which impinges on experience only along the edges. Or, to change the figure, total science is like a field of force whose boundary conditions are experience. Quine (1951, 47)

The comparisons suggest that our beliefs, knitted together by their logical interrelations, form a unitary theory of the world in which each element is inseparable from the others. This idea of total science, a grand theory of everything we know, is at the heart of Quine's account of knowledge. As he claims that total science is the smallest unit that can be measured against the evidence of sense-experience (1951, 47), the thesis of conformational holism appears in his philosophy in an extreme form.

Confirmational holism completes what we are calling Quine's theory of evidence. This involves three claims: (a) that, ultimately, evidence for beliefs is provided by, and only provided by, sense perception; (b) that in normal circumstances what counts by ordinary scientific criteria to be compelling evidence for a claim warrants belief that it is true (the evidence-warrant principle); (c) that scientific evidence attaches to all the hypotheses required to generate successful predictions (confirmational holism). Taken together these theses tell us what evidence is, what it does and how it distributes across hypotheses. However, they do not tell us how evidence is brought to bear on scientific theory. So let us consider this now.

Quine maintains that scientific theory is tested with the help of *observation categoricals*, conditional sentences such as "When an athlete runs, their heart-rate increases" and "When alcohol is heated to 78 degrees C, it boils" which are directly evaluable by observation.[49] When an observation categorical follows logically from a scientific theory, it can be viewed as part of the content of that theory. Observation categoricals are thus able to act as brokers for the evidential deal between theory and sense experience, bringing the content of a theory into contact with sense experience in the form of observation. Accordingly, Quine holds that a scientific theory is tested by observational evaluation of observation categoricals that it implies.

[49] The following account of observation categoricals and the way they are used in testing theory is based on Quine (1992, Chapter 1, §§ 4 - 5).

We can now spell out how Quine's theory of evidence promises to secure mathematical knowledge on its empirical applications. To gather evidence for scientific theories we need to test them against sense experience, by virtue of the empiricist view that all evidence is sensory evidence. To carry out the test, we derive observation categoricals from our scientific theories and check these observationally, as we have just described. Because scientific theories are mathematically stated, this will often require use of purely mathematical claims as premises in our deductions, so that the systems of hypotheses that get measured up against observational data will involve mathematical hypotheses. By confirmational holism, these mathematical premises share in the confirmation provided by agreement with the observational data. By the evidence-warrant principle, such agreement provides grounds for belief in the truth of the mathematical hypotheses. So if Quine's theory of evidence is accepted, mathematics, through its use in empirical applications, is supported by the observational evidence we have for science; in short, the Quinean view is that mathematical justification is constituted by scientific confirmation.

5.1.2 Indispensability

The theory of evidence described above brings scientific evidence to bear on mathematics only if mathematics is included in total science. However, one might feel that there are reasons to resist including mathematics in total science. Quine himself for a time eschewed mathematics as ordinarily understood, declaring with Goodman that there are no abstract objects (Goodman and Quine (1947)). But he later abandoned this point of view as his conviction grew that science cannot be done without using mathematics. Quine's reason for including mathematics in total science is thus that it is indispensable.

What does this mean exactly? Loosely speaking, mathematics is indispensable to science whenever it is necessary to our best scientific account of the world. More precisely, a mathematical theory is indispensable when it is necessary to make use of it to give the best possible scientific account of given observations. More formally, letting T be a scientific theory accounting for observations O, and assuming T makes use of a mathematical theory M:

> M is indispensable if and only if any theory T' not making use of M is inferior to T as a scientific account of O.

Given the Quinean perspective, the theories of interest are global scientific accounts of all our observational data (prospective "total sciences"). So for Quine, a mathematical theory M is indispensable if and only if there is a global scientific account T of all our observational data such that T makes use of M and any theory T' not making use of M is an inferior scientific theory of all our observational data.

It can seem as if two different indispensability theses emerge from Quine's work. When Quine is writing about contemporary science, he seems happy to regard total science as a kind of agglomeration of all the theories put forward by scientists in their various fields. Thus every mathematical theory used by scientists from day to day ends up being part of total science, with the result that all of standard mathematics, the arithmetic of the natural numbers, real and complex analysis, set theory, etc. is regarded as indispensable. However, when Quine is writing about ontology, and in particular about how to find out what exists, he prefers to regard total science as a translation of the everyday theories of scientists into an ontologically most perspicuous language (first order logic together with the language of set theory). The only mathematical theory regarded as indispensable from this perspective is set theory.

It would be possible to construct an account of mathematical knowledge from either of these positions on the indispensability of mathematics. But in the present context, it is best to pick the approach that seems most likely to provide a satisfactory account of how mathematical knowledge is grounded on its empirical applications. For this reason it seems advisable not to concentrate solely on set theory, but to take a wider view. We thus propose to view Quinean realism as being based on the claim that standard mathematics is indispensable to our best overall scientific account of all our observational data. Call this the scientific indispensability of standard mathematics.

5.1.3 Summary

This completes our description of (what we are calling) Quinean realism. The view involves the following four theses:

> (i) Sense perception provides evidence for beliefs; the only evidence we have for belief is that of sense perception;
> (ii) In normal circumstances, what counts by ordinary scientific criteria to be compelling evidence for a claim warrants belief that it is true;
> (iii) Scientific evidence attaches to all the hypotheses required to generate (successful) predictions;
> (iv) Standard mathematics is indispensable to our best overall scientific account of all our observational data.

This theory is supposed to deliver mathematical knowledge on the basis of scientific confirmation. The theory of evidence constituted by theses (i) to (iii) is meant to show that mathematics is scientifically confirmed if it is to be taken as part of our best overall science. The scientific indispensability of standard mathematics (iv) is meant to show that mathematical science is to be taken as part of our best overall science. Quinean realism thus infers from the premise that standard mathematics is indispensable to the conclusion that it is scientifically confirmed.

5.2 Objections to Quinean realism

Quinean realism is susceptible to three serious looking lines of attack.[50] The first takes issue with the indispensability of mathematics, arguing either that science can be replaced by a non-mathematical alternative (Field (1980, 1989a); Balaguer (1996a, 1996b)), or that the mathematics that is indispensable to science falls short of standard mathematics (Feferman (1993a)). We shall consider the first of these ideas in Chapter 6 but will be addressing the second below.

The second threatening response to Quinean realism denies the evidence-warrant principle (Van Fraassen (1980)). This involves denying the efficacy of abductive inference as a means of supporting knowledge about unobservable scientific objects and so represents a radical departure from our usual ways of thinking about evidence. Because of this, we shall not attempt to assess this approach; instead we will assume that the evidence-warrant principle is correct.

The third promising line of argument appeals to facts of scientific and (or) mathematical practice to argue against confirmational holism (Chihara (1990); Maddy (1992, 1997); C. Parsons (1980, 1986b); Sober (1993)). This is explored in particularly close detail by Sober and Maddy. In Sober's opinion, scientific practice shows that hypotheses are confirmed only when they appear in scientific accounts of observational data that outperform competing accounts from which the hypotheses in question are absent (1990, 1993). Assuming that indispensable mathematics is common to all competing scientific accounts of given observational data, he concludes that indispensable mathematics is not scientifically confirmed (1993, 44). However, the conception of indispensability involved here is quite different from the Quinean conception, according to

[50] The literature also contains two putative objections that we do not consider to constitute genuine threats. The first is that Quinean realism may be capable of delivering knowledge of mathematical objects only if these are taken to be, in some sense, concrete (Cheyne and Pigden (1996)). The second is that it is possible to invoke a non-standard theory of pragmatics in order to portray mathematical utterances in scientific contexts as something other than assertions (Melia (1995, 2000); Yablo (1998)).

which a mathematical theory is indispensable if and only if it is contained in our best overall scientific account of all our observational data, and any overall account not containing it would be an inferior account of the data. On this conception, it does not follow from the fact that a mathematical hypothesis is indispensable that it appears in all competing accounts of the relevant observational data; so the indispensability of a mathematical hypothesis does not rule out the possibility that it is scientifically confirmed. Sober's argument therefore misses its intended target.

Accordingly, our appraisal of Quinean realism will start with a discussion of Maddy's objections from scientific and mathematical practice. For the interested reader, we provide a fuller discussion of Sober's criticism in the appendix to this chapter.

5.3 Maddy's objections from scientific practice

Maddy (1997, II, Chapter 6) raises three objections to Quinean realism from scientific practice: (a) that scientific confirmation is in practice more finely grained than it is in the Quinean view of science; (b) that there is a marked difference in practice between the scientific treatment of mathematical and physical hypotheses and (c) that scientific applications of mathematics are predominantly idealizations. We will consider these in turn.[51]

The first objection depends on a fragment of the history of science.[52] Early in the nineteenth century, Dalton introduced modern atomic theory. He suggested that a sample of any given element is made up of identical atoms, that these persist through chemical reactions and that compounds are constituted by aggregates of atoms of different elements. Dalton also applied this theory to explain the proportions in which substances and gases combine. In the following years, many scientists made important contributions to the theory. Frankland introduced the concept of valence in the early 1850s, Cannizzaro used it to calculate atomic weights in 1858, and Maxwell, Boltzmann and others used it as the basis of the kinetic theory of heat in the early 1860s. Despite these successes, the theory was not generally accepted until early in the twentieth century. Between 1908 and 1911, however, Perrin published the results of experimental data on the basis of which he explained Brownian motion and calculated Avogadro's number. By doing so, he won almost universal acclaim for the atomic hypothesis.

Maddy claims that the atomic theory had become indispensable by the turn of the eighteenth century. But as she points out some sceptics, including luminaries such as Mach and Poincaré, wanted to see the

[51] Maddy actually presents her objections as part of a case against the indispensability argument for mathematical realism, which infers mathematical realism from the claim that mathematics is indispensable to science, and other assumptions. However, as our discussion will make clear, Maddy's objections all concern alleged difficulties with the Quinean account of mathematical knowledge.
[52] This account of highlights from the period is derived from Maddy (1997, 135-142).

existence of atoms put to experimental test. These sceptics were not regarded as unscientific cranks by the rest of the scientific community. On the contrary, the community accepted that the atomic hypothesis had not passed a test of the kind envisaged and accepted that, in the absence of such a test, disbelief in atoms was scientifically respectable. This situation changed with the publication of Perrin's results. Almost everyone (Mach a notable exception) took these to provide the experimental confirmation that had been demanded, and thus scepticism with respect to atoms was no longer considered scientifically acceptable. Thus in Maddy's mind, Perrin's results take on a special philosophical significance. It was these results, and not merely the indispensability of atomic theory, that finally established its acceptance.

Generalizing from this example, Maddy argues that evidence for hypotheses can be brought to bear more closely on them than it is by their mere indispensability. She also claims that belief in a hypothesis is only a requirement when it has been connected to the evidence by this tighter relation:

> scientists do not, in practice, view the overall empirical success of a theory as confirming all its parts. In some cases, a central hypothesis of an empirically successful theory will continue to be viewed as a 'useful fiction' until it has passed a further, more focused, and more demanding test. (1997, 142)

Thus Maddy is lead to the view that scientific confirmation has a finer structure than Quinean realism allows and that when this fine structure is taken into account indispensability does not automatically lead to confirmation. She concludes that the inference from the indispensability of mathematics to its scientific confirmation is invalid.

What should we make of this objection? It is certainly true that the Quinean theory of evidence does not make sense of this episode from the history of science. But this was never its aim: it was meant as a theory about the nature of scientific evidence, not as a description of scientific practice. So the fact that scientists take a more fine grained view than Quinean realism of the relationship between observation and hypotheses does not show that it must be rejected but rather that it needs an addition, a theory explaining why it is rational to practice science the way we ordinarily do even though confirmation is holistic. If we looked into this to find that no such addition were possible, we would have to conclude that the Quinean theory of evidence could not be reconciled with any reasonable explanations of scientific practice, which would be a forceful objection to it and Quinean realism. As things stand, however, we only have reason to question whether such reconciliation is possible, not to conclude that it is impossible. Thus we do not yet have grounds to reject Quinean realism, but only to suspend judgment on it.

We turn now to Maddy's objection that there is a marked difference in practice between the scientific treatment of mathematical and physical hypotheses. Maddy claims that scientists seem content to use whatever mathematics they find most useful in constructing and manipulating their theories, without worrying about the mathematical existence assumptions entailed (1997, 155). She claims also that they often do not care about the physical structural assumptions implied by the mathematics they invoke (*loc. cit.*) and that they do not seem to think of their observational evidence as evidence for their mathematics (*op. cit.*, 156). This laissez-faire attitude towards mathematical hypotheses contrasts starkly with scientific attitudes towards physical hypotheses. Scientists care very much about the existence assumptions that their physical hypotheses entail, they are very much interested in the structural implications of these hypotheses and they certainly think of their observational evidence as evidence for their physical theories. Thus Maddy's view is that there is an epistemic disanalogy between the way physical and mathematical hypotheses are treated in scientific practice (*loc. cit.*). As she thinks also that Quinean realism implies a scientific practice that does not vary in this way, she takes the disanalogy to be evidence against that view.

One way of responding to this is to argue that the epistemic disanalogy is illusory. In this vein, Colyvan (1998, 51-53) argues that scientists do worry about the ontological commitments of the mathematics they use but that this is overlooked because they tend to do so only when new mathematics is applied. In support of this, he contrasts the passive acceptance of real analysis in contemporary science with the controversial introduction to science of the calculus (in particular infinitesimals), Dirac's delta function and complex numbers.[53]

However, these examples are not decisive. Colyvan is correct to say that seventeenth century scientists had specifically ontological worries about infinitesimals, as Berkeley's comments in *The Analyst* about the "ghosts of departed quantities" make clear. However, contemporary standard mathematics does not imply that infinitesimals exist, so these ontological worries may not be relevant.

On the subject of the delta function, Colyvan quotes a passage from Dirac to illustrate the kind of worries people had:

> although an improper function [e.g., the Dirac delta function] does not itself have a well-defined value, when it occurs as a factor in an integrand the integral has a well-defined value. In quantum theory, whenever an improper function appears, it will be something which is to be used ultimately in an integrand. Therefore it should be possible to rewrite the theory in a form in which the improper functions appear all through only in integrands. One could then eliminate the improper functions altogether. The use of improper functions thus does not involve any lack of rigour in the theory, but is merely a convenient notation,

[53] For the attitudes of contemporary science towards real analysis, Colyvan considers the views of Richard Feynman, whose use of real analysis in measurement contexts is one of Maddy's leading examples of scientific nonchalance towards the mathematics used in science. Colyvan (1998, 51) suggests that Feynman may have been content to apply real analysis in measurement contexts because he believed that its ontological commitments were already supported by other applications in physics. However, Colyvan does not produce a quote in which Feynman expresses this belief (or, indeed, any others, Colyvan doesn't quote from Feynman at all), so this is speculative.

> enabling us to express in a concise form certain relations which we could, if necessary, rewrite in a form not involving improper functions, but only in a cumbersome way which would tend to obscure the argument. Dirac (1958, 59)

Perhaps this remark can be read as a concern with the mathematical ontology of quantum theory. But given the lack of any explicit reference to such worries, given that the explicit concern is with a possible "lack of rigour in the theory", it is not obvious that this how it should be taken.

This leaves the case of complex numbers. Colyvan cites passages in Kline's standard history of mathematics (Kline (1972)) to show that Descartes, Newton and Euler had apparently ontological worries about the square root of negative one and other imaginary numbers (Colyvan (1998, 53)). He explains that Newton's worries were based on the fact that the imaginary roots of equations lacked physical significance (*op. cit.*, 53). However to take this as evidence of ontological worries about complex numbers as we now understand them involves a somewhat questionable slide; presumably Newton, Descartes and Euler had no worries about complex numbers as we now understand them.

It is thus debatable whether these examples establish that scientists had specifically ontological worries about the introduction of currently accepted mathematical objects. Even if they did show this, they would not establish that it is normal for scientists to have ontological worries about the scientific uses of new mathematical objects.[54] Since ontological worries of this kind are not the only basis for Maddy's epistemic disanalogy between the scientific treatment of mathematical and physical hypotheses, it therefore appears that these examples are not a sufficient from which to show that there is no such disanalogy.

[54] Colyvan himself concedes this: "Whether controversy surrounding the use of novel mathematical entities in physical theories is widespread or not I am in no position to say, but at least it seems that there are *some* cases where physicists are genuinely suspicious of new mathematical entities." (1998, 53)

However, arguing against the disanalogy is not the only available response. Maddy's objection here depends on the thought that Quinean realism implies that there should not be such a disanalogy. But Maddy does not provide any argument in favour of this, and on reflection it does not seem particularly attractive. For why should the fact, if it is a fact, that mathematics shares in the evidence for science make scientists at all concerned about the ontological implications of the mathematics they use? What reason would this give them to worry about the physical assumptions their mathematics implies? And why should it make them show an interest in taking their experiments to confirm mathematics? There is no reason why it should. Scientists are interested in their own research projects and they have their own agendas. The discovery that mathematics is scientifically confirmed would not provide them with a reason to swap those projects and agendas for others. They could, if they chose, start addressing mathematical questions by asking what answers to those questions are most useful in their theories. But no requirement to do so falls out of adopting Quinean realism, so it does not necessarily bring with it a deviant scientific practice.

This indicates that the epistemic disanalogy does not provide grounds for rejecting Quinean realism. It would do so, if the view were meant as a description of scientific practice, for then the lesson of the disanalogy would be that the description is inaccurate. But just as the Quinean theory of evidence is not meant as a description of scientific practice, neither is Quinean realism meant as a description of the treatment of mathematics in scientific practice. It is true that the existence of the epistemic disanalogy lends urgency to the challenge raised by Maddy's first objection, emphasizing once again that we stand in need of a theory explaining why our current scientific practice is appropriate, given that scientific confirmation is holistic. But this is the most that can be claimed for it. Having agreed that there is a difference in the way mathematical and physical hypotheses are treated in scientific practice, a valid question is raised as to whether an account of scientific practice can be found that coheres well with Quinean realism. But that is all.

We now turn to Maddy's third and final objection from scientific practice. This argues that mathematics cannot generally be scientifically confirmed because the applications in which it appears are predominantly idealizations (1997, 143-154). If this is correct, it would not only refute Quinean realism as it currently stands; it would also show that Quinean realism cannot be developed so as to satisfactorily explain the facts of scientific practice we have already discussed. Maddy's argument from idealizations thus promises a powerful new objection to the Quinean approach. Let us examine it in more detail.

The argument depends on a distinction between two kinds of idealization. Ballistics experts assuming that the Earth is flat in order to calculate the trajectory of a missile are making idealizing assumptions of the first kind; they are adopting idealizing assumptions in full knowledge that they are false. In contrast, physicists assuming that space and time are continuous in order to apply real analysis in measurement contexts are making idealizing assumptions of the second kind; they are adopting idealizing assumptions the truth (or falsehood) of which they regard as being open. The attractions of each kind of idealization are the same, they may facilitate the scientific task by allowing us to apply simpler mathematics, they may allow us to apply mathematics where otherwise none could be applied, etc. However, the way they deliver these benefits are different; the first kind of idealization involves deliberate misrepresentations, the second does not.

Maddy contrasts idealizations of both these kinds with "literal applications". Presumably, these are applications in which no assumptions are made that are not thought to be true.[55] For Maddy, only literal applications could forge the strong connection between mathematical hypotheses and scientific evidence, which is provided by the focused scientific testing of scientific hypotheses, and which is required, on Maddy's view, if they are to be scientifically confirmed (*op. cit.*, 146). In light of this, she argues that

[55] Maddy does not actually define what is meant by a literal application but this reading seems to accord best with the argument she makes.

there are not enough uncontroversial examples of mathematics appearing indispensably in literal applications to support the view that mathematics is scientifically confirmed.

To help establish this, Maddy considers the use in science of "continuum mathematics", the body of mathematical theories built up from assumption of the real number continuum, real analysis, complex analysis, etc. Maddy concedes that there are very many indispensable applications of this mathematics. However, she argues that many of these applications are the kind of idealization in which false assumptions are knowingly made. For example, such idealization is involved when we assume that oceans are infinitely deep to represent water waves, when we use continuous functions on real numbers to represent discontinuous phenomena like charge and angular momentum, when we assume that liquids are continuous substances in fluid dynamics, etc. (*op. cit.*, 143). Maddy also points out that other applications of continuum mathematics typically assume that space and time are continuous, and that whether this is the case remains an open scientific question (*op. cit.*, 146-152). This makes them idealizations of the second kind. Thus, after arguing for the intermediate conclusion that uncontroversial examples of literal applications are typically dispensable, Maddy infers that there is scant evidence of literal applications in which mathematics is indispensable (*op. cit.*, 152-153). And this leads her to the conclusion that there are insufficiently many uncontroversial examples of mathematics indispensably appearing in literal applications to support the view that mathematics, in general, is scientifically confirmed (*op. cit.*, 154).

What should we make of this argument? It is uncontestable that some of the assumptions used in idealizations are not scientifically confirmed. Moreover, it seems plausible to maintain, with Maddy, that idealized applications could not provide the sort of focused tests that are required, on her view, to warrant belief in mathematical hypotheses. Nevertheless, we shall argue that there is not a telling objection here to the Quinean approach.

To see why, we need to take note of two separate points. The first is that passing a focused test of the kind envisaged by Maddy does not seem to be the only way a scientific hypothesis can come to be scientifically confirmed. As Resnik (1997, 20) observes, highly theoretical empirical hypotheses such as the conservation of mass/energy are not put to such tests. Apparently, however, such hypotheses are confirmed, presumably by an accumulation of evidence made available by their use as background assumptions to many successful applications. The second point is that it is highly implausible to claim that idealized applications provide no evidence for any of the assumptions they involve. Scientific evidence gathering in some areas of science (e.g. particle physics) is conducted primarily through the development of idealized applications; if we did not view such applications as capable of delivering evidence for scientific hypotheses, it would be difficult to make sense of this activity.

Appealing to the first of these points, the Quinean can reject Maddy's presupposition that the only way a scientific hypothesis can be confirmed is by passing a focused scientific test. This already constitutes a telling response to her argument for, without this presupposition, the best it can establish is that mathematics is not scientifically confirmed by focused scientific test. In addition to this, the points taken together allow the Quinean to propose that mathematics may be confirmed by accumulated evidence emerging from idealized applications, which would undermine Maddy's position even more.

To make this proposal attractive, it helps to have reasons for thinking that the mathematical hypotheses of idealized applications should be assimilated to empirical hypotheses confirmed by accumulated scientific evidence, rather than to idealizing assumptions, which of course are not confirmed at all. Recall, then, that Maddy's argument posits two kinds of idealization, those in which we make assumptions that are known to be false and those in which we make assumptions the truth of which is regarded as being open. When we use assumptions in this way it shows that we are making them for purely instrumental purposes. Typically, however, these indicators of instrumental use are absent from the mathematical assumptions used in idealized applications. The epistemic role of the mathematical hypotheses used in idealized applications thus appears disanalogous to that of the idealizing assumptions, in typical cases.

In contrast, the epistemic role of the mathematical hypotheses appears broadly similar to that of the highly theoretical empirical hypotheses we mentioned earlier (such as the conservation of mass/energy). For example, both tend to function as part of the theoretical background applications, neither are required to pass focused tests in order to be accepted and both are shielded from revision. It therefore does seem plausible to assimilate the mathematical hypotheses of idealized applications to empirical hypotheses confirmed by accumulated evidence, rather than to idealizing assumptions. So the proposal that mathematics can be viewed as being scientifically confirmed by accumulated evidence emerging from idealizations appears quite credible.

Given these considerations, Maddy's argument from idealizations seems far from convincing. Its premise that scientific hypotheses can only be confirmed by passing a focussed scientific test seems not to be true; in fact, some empirical hypotheses appear to be confirmed by accumulated evidence, without passing such tests. Moreover, it appears plausible for the Quinean to claim that mathematics is scientifically confirmed by accumulated evidence emerging from idealized applications, thus drawing a comparison between the confirmation of mathematics and the confirmation of highly theoretical empirical principles such as the conservation of mass/energy. For these reasons, Maddy's argument from idealized applications does not appear to present a convincing objection to the Quinean approach.[56]

[56] We should acknowledge here that it does not seem possible, on the Quinean view, to take all idealized applications as providing confirmation for the mathematics they use. For example, it seems unwise to view matters this way when mathematically illegitimate numerical methods are used to fix otherwise unsuccessful applications, as for instance when renormalisation is used to apply continuum mathematics in quantum theory. However, the reason Quineans might be cautious with such examples is not that they are idealizations. Rather, it is that the use of illegitimate numerical methods makes it difficult to view them as genuine applications of viable mathematical theories.

5.4 Maddy's objections from mathematical practice

Maddy makes three objections from mathematical practice: (a) that we find elementary mathematics obvious (1997, 106-107); (b) that the justifications mathematicians propose in favour of their claims typically do not mention indispensable applications (*op. cit.*, 106) and (c) that mathematicians typically do not appeal to applications to address questions independent of ZFC (*op. cit.*, 159). In each case, her argument is that if mathematics were scientifically confirmed, we would not expect mathematical practice to be like this in the relevant respect.

The objection from obviousness was first raised by Charles Parsons:

> The empiricist view, even in the subtle and complex form it takes in the work of Professor Quine, seems subject to the objection that it leaves unaccounted for precisely the *obviousness* of elementary mathematics (and perhaps also of logic). C. Parsons (1980, 101)

The problem, as Parsons sees it, is that by treating mathematical claims in the same way as the theoretical claims of natural science, it becomes necessary to regard obvious claims of elementary mathematics as "bold hypotheses, about which a prudent scientist would maintain reserve, keeping in mind that experience might not bear them out" (1980, 102). But of course, we do not regard elementary mathematical claims like this, so it is implausible to suggest that mathematics is scientifically confirmed.

It is true that Quinean realism does not immediately explain the obviousness of elementary mathematics, but that this is a problem remains unclear. Presumably Parson's is claiming that the obviousness of elementary mathematical claims provides grounds for belief in their truth and that this cannot be explained from the Quinean perspective. However, the Quinean has two responses. Affirming that the obviousness of elementary claims provides grounds for belief in their truth, he can argue that this is because each of us has sensory evidence for such claims. Simple sums like 2 + 2 =4, for example, can be regarded as over-learnt, hence obvious, because of the very large number of confirming instances of them that we each experience every day. But then such claims are obvious because they are amply confirmed, and so there is no difficulty in explaining why obviousness is a ground for belief. Alternatively, the Quinean could simply deny that the obviousness of elementary mathematics is a ground for belief. There are good examples both in mathematics and without of obvious-seeming claims that turned out not to be true (the existence of the Russell set, the claim that the earth is flat, etc.). Hence it remains unclear that the obviousness of elementary mathematics is a ground for belief. For these reasons, the objection from obviousness is not effective.

Let us turn then to the second objection from mathematical practice. Maddy is quite correct to say that mathematicians typically do not mention indispensable applications when trying to justify their claims: what mathematics typically demand for justification is proof. However, it is not clear that Quinean realism requires a revision of this practice. If proof is to be the standard vehicle of mathematical justification, there must be justified mathematical claims that are not proved from more basic claims. But then how are these axioms justified? Quineans answer that they are scientifically confirmed in their indispensable applications. Thus the proper focus of Quinean realism is with the justification of the axioms of mathematics, and only indirectly with the justification of results derived from them, via the mechanism of proof. So the view does not recommend supplanting proof from previously established results by appeals to indispensability; rather, it supports proof from previously established results as an epistemically effective method, by providing justification for the unproved assumptions that are necessary for the construction of proofs. Once again, then, no telling objection has emerged.

Maddy's last objection from mathematical practice observes that there are mathematical questions whose answers are open in the sense of being independent of the axioms of ZFC (they can neither be proved nor disproved from them). Some examples are whether there are non-measurable Σ^1_2 sets, whether there are thin Π^1_1 sets and whether every free group is a Whitehead group (Maddy (1997, 66-69)). If we are to solve questions like these we will have to establish new axioms for set theory from which answers can be deduced. But how are we to do this? Quine's approach here is firmly rooted in applications. He thinks we should believe whichever axiom candidates are necessary to generate the mathematics indispensably required in science, together with axiom candidates necessary to streamline this mathematical theory:

> So much of mathematics as is wanted for use in empirical science is for me on a par with the rest of science. Transfinite ramifications are on the same footing insofar as they come of a simplificatory rounding out, but anything further is on a par rather with uninterpreted systems. Quine (1984, 788)

In contrast, Maddy maintains, the approach exhibited in practice seems to have little to do with empirical applications (1997, 159). Set theorists offer mathematical considerations in favour of new axiom candidates, noting their connections to other axioms and axiom candidates, their effect on open and independent questions, their intrinsic plausibility, etc. For Maddy, it is already an objection to Quinean realism that the methodologies here are different, but worse is to come. Guided by his methodology, Quine affirms the axiom of constructibility, V = L (see Quine (1992, 94-95)): following the methodology of practice, the set theoretic community denies V = L (for a philosophical account of possible reasons why see Maddy (1993)). Seemingly, then, Quine's methodological approach disagrees with that of practising set theorists over which new axiom candidates should be accepted.

Maddy is surely right that the selection of new axioms is an aspect of mathematical practice that philosophical accounts of mathematics must address and she is right to argue that this is an area of some significance for Quinean realism. However, as with the objections from scientific practice that we considered before, the effect of the points raised here is not to refute Quinean realism but to raise certain challenges that it must address.

Consider the first point, that in practice set theorists work with a methodology quite different from the one that would be expected if the evidence for mathematical axioms comes from empirical applications. This constitutes an objection to Quinean realism only if it is not possible to explain why set theorists operate with their methodology even though mathematics is ultimately confirmed in applications. But Maddy does not provide any such reason. So we are left not with an objection but with a challenge: to show how Quinean realism can explain this fact about mathematical practice.

The second point, that Quine's methodology and the methodology of practice demand different decisions on prospective axioms, is a little different. If we are to side with the set theorists in rejecting V = L, as clearly we should, then we will have to explain in what way Quine erred when he evaluated V = L as true. Since one possible explanation is that Quinean realism is incorrect, the case of V = L does initially threaten this position. However, one could equally well criticize Quine's approach to potential axioms instead. Quine may say that we should believe indispensable mathematical axioms and those that satisfactorily round them out; but we could say that Quine's application of the "rounding out process" is too crude since it delivers as truths axioms that are not accepted in practice. Instead of abandoning Quinean realism, therefore, we could try to show that there is an available methodology for mathematics which leads us to the view that V = L false but which does not conflict with the Quinean view that the evidence for mathematical claims derives ultimately from empirical applications. Once again, we are left not with a solid objection but with a challenge: to find a methodology that coheres with Quinean realism and that delivers the right verdict on V = L.

It is important to note that Maddy's line of argument with V = L can be imitated whenever we are presented with an axiom whose truth-value is settled according to mathematics but that seems not to be required by empirical applications (an example is the axiom of inaccessibles, AI). The acceptance of such axioms in mathematics seems unreasonable from the point of view of Quinean realism, as indispensability considerations do not appear to provide reason to accept them. Clearly if this is the situation with many mathematical axioms, it will constitute a serious objection to Quinean realism. Maddy's discussion of V = L thus raises a more general challenge than that specified in the last paragraph. Let us say that a mathematical axiom is dispensable if and only if it has no indispensable applications (so that it is either not applied or not indispensably applied). Then the challenge can be put as follows: to show that Quinean realism can be coherently extended by a methodology delivering confirmation for dispensable mathematical axioms in just those cases for which it is required.

5.5 Resnik's development of Quinean realism

Having considered Maddy's objections from scientific and mathematical practice we find ourselves with no reason to reject Quinean realism. The facts of practice do not show that Quinean realism must be abandoned because it is intended as a theory of scientific and mathematical evidence, not as a description of scientific or mathematical practice. As we have seen, however, the facts of practice raise challenges that must be met by proponents of the Quinean approach. Quineans must show that there is a methodology that reconciles the facts of scientific and mathematical practice with the scientific confirmation of mathematics and that provides a plausible account of the confirmation of dispensable mathematics, in particular axiom candidates such as V = L and AI.

Resnik (1995, 1997) agrees that Quinean realism does not explain the facts of scientific and mathematical practice. Nevertheless he thinks the idea of grounding mathematical justification in the applications of mathematics is a good one and he agrees that it should be pursued by appeal to confirmational holism and indispensability. Resnik explains his method as follows:

> I will show that 'good sense', in the form of pragmatic rationality, underwrites the special role mathematics has come to play in science and bids us to treat it *as if* it were known a priori. (1997, 120)

By appeal to pragmatic rationality, then, Resnik aims to extend the framework laid down by Quine, to arrive at a position from which the facts of scientific and mathematical practice will be explicable. In this section, we will consider the ways in which Resnik extends Quinean realism. In the next, we will find out whether these innovations are sufficient to meet the challenges we have identified.

Following Kyburg (1984, 1990), Resnik thinks of scientific practice in terms of models, mini-theories of physical situations that include both scientific and mathematical principles (Resnik (1997, 121-123)). Scientists gather evidence for scientific theories by constructing models and testing them, comparing their logical and statistical implications with observational data. As Resnik points out, scientific modeling requires taking diverse hypotheses for granted; when scientists working within a given discipline are constructing and testing their models, they tend to assume fragments of theory from other specialties together with methods appropriate to them. Models from one specialty will tend to invoke similar assumptions from other specialties and this suggests distinguishing amongst scientific theories according to the kind of assumptions made in the models used to test them. Because the results and methods of some theories are more widely applied than others, Resnik claims that this method of discrimination leads to a kind of order on scientific theories; the widely applied theories are more global in this respect, the others more local (*op. cit.*, 125). In this way, scientific practice shows mathematics to be more global than physics, which is more global than chemistry, which is more global than biology, etc. (*loc. cit.*).

Hand in hand with this distinction between local and global theories goes the idea of local conceptions of evidence. Local conceptions of evidence are not something to do just with local theories (as Resnik's terminology might misleadingly suggest). Rather they are special conceptions of evidence that apply locally to specific theories, subject-specific ideas of what counts as evidence for given propositions within the practice of a scientific specialty and what methods it is appropriate to use in testing them (*op. cit.*, 126).

To see what Resnik is driving at with this idea, consider the prediction that fifty percent of the rabbit population of Wiltshire will suffer from myxomatosis. To find out whether this claim were true, we would appeal to data about the interaction of rabbit populations in Britain and about the geographical spread of myxomatosis, and we would appeal to various theories from fields such as vetinary science and mathematics. To do this, we would have to make judgments about what data and theory were relevant to the evaluative task concerned, judgments we would be qualified to make on the basis of our prior experience of dealing with that kind of evaluative task. It appears that Resnik's local conceptions of evidence are intended to make sense of this kind of activity. Presumably, our expertise to make the kind of choices required by these subject specific evaluations consists in our sensitivity to local conceptions of evidence corresponding to the specific subjects concerned.

In positing local conceptions of evidence, Resnik does not abandon confirmational holism (the idea that evidence attaches to all the hypotheses necessary to generate a subsequently confirmed prediction), nor does he abandon the Quinean idea that questions of evidence are ultimately referred to our overall scientific theory. Quite the contrary in fact, as there are several ways in which global considerations regulate evidence gathering at the local level; local conceptions of evidence are overridable by global considerations (*op. cit.*, 100) and they are justified by pragmatic judgments as to what best promotes science as a whole (*loc. cit.*, see also *op. cit.*, 126)). Pragmatic considerations at the global level also serve to shape the local conceptions of evidence operative in scientific practice. Specialists may sometimes be tempted to make revisions in the more global theories from which they draw hypotheses for use in their models. But because such alterations have the potential to rule out models in other scientific specialties that perform well, considerations of what is good for science as a whole counsel specialists to revise their models by revising the hypotheses specific to their own specialty before altering hypotheses drawn from more global theories. Ultimately this is just good sense, as this way of proceeding minimizes disruption to science as a whole (*op. cit.*, 125-126).

Another way Resnik builds on Quinean realism is by introducing a new use for indispensability. This is made clear in the following argument:

> (1) In stating its laws and conducting its derivations science assumes the existence of many mathematical objects and the truth of much mathematics.
> (2) These assumptions are indispensable to the practice of science; moreover, many of the important conclusions drawn from and within science could not be drawn without taking mathematical claims to be true
> (3) So we are justified in drawing conclusions from and within science only if we are justified in taking the mathematics used in science to be true.
> (4) We are justified in using science to explain and predict.
> (5) The only way we know of using science thus involves drawing conclusions from and within it.
> (6) So, by (3) above, we are justified in taking this mathematics to be true. (1997, 46-48)[57]

The appeal to this argument represents a departure from Quinean realism, in which the indispensability of mathematics justifies taking the evidence for science as evidence for mathematics. For Resnik, the indispensability of mathematics also justifies taking the justification we have for doing science as justification for believing mathematics. In both cases, indispensability transfers support from science to mathematics. But in the first case, evidence is transferred from one body of theory to another, whereas in the second the justification for pursuing one kind of activity is transferred to another. Following Resnik, we will call the first of these the *confirmational* approach to indispensability and second the *pragmatic* approach.[58]

Having reviewed Resnik's additions to Quinean realism, we can describe his account of mathematical knowledge. From the Quinean account it inherits the Quinean theory of evidence (the view that evidence is sensory evidence, confirmational holism and the evidence warrant principle)

[57] Here the argument (1)-(3) is quoted from (1997, 46-47), the continuation (4) – (6) from (1997, 48).
[58] The terminology is introduced in Resnik (1995).

together with the scientific indispensability of mathematics. The confirmational approach to the indispensability of mathematics is used to justify taking the evidence for science as evidence for mathematics, as before, but now the pragmatic approach to indispensability is also used, to justify taking the justification for doing science as justification for believing mathematics. In addition, Resnik proposes an account of the practice of the sciences in which there is a distinction between global and local scientific theories, with one theory being more global than another if its results and methods are more widely applied. Scientific theories have local conceptions of evidence corresponding to them, regulated by pragmatic considerations concerning what is best for science as a whole. Our specialized sciences (physics, chemistry, etc.) are thus portrayed as sociologically distinguished theories practiced according to local conceptions of evidence the nature of which is determined by global considerations and the grounds of good sense.

5.6 Resnik's response to the challenges of practice

Having set out Resnik's account of mathematics, or at least that part of it pertaining to the idea that mathematics is scientifically confirmed, we return to the challenges of practice. In this section, we will assess whether Resnik's theory adequately explains the facts of scientific and mathematical practice and whether it deals successfully with the confirmation of dispensable mathematics.

5.6.1 Explaining the facts of scientific practice

The features of scientific practice not explained during the discussion of section 5.3 are the practice of taking experiments to bear on individual hypotheses and the three points of Maddy's epistemic disanalogy between the scientific treatment of mathematical and physical hypotheses. All of these can be adequately explained with the help of Resnik's account of mathematics.

Resnik himself deals with the practice of taking experiments to bear on individual hypotheses, during his discussion of the scientific use of models. Resnik points out that to investigate the performance of their models scientists freely draw upon whatever theoretical resources they deem best fit for their purposes, that they idealize their models for reasons of simplicity, tractability etc. and that they manipulate the closeness of fit between prediction and observation. According to Resnik, this activity is all pragmatically justified; it is part and parcel of operating with a local theory and a local conception of evidence that are justified on the grounds of good sense. Moreoover, Resnik argues that it is on these same pragmatic grounds that the success of a model can be taken as evidence for one of its hypotheses. It makes good sense to practice science this way as this best promotes the development of science as a whole. And in particular, the fact that scientists take experiments to bear on individual hypotheses does not refute confirmational holism because hypotheses are isolated for experimental testing only by pragmatic grounds, not epistemological ones (Resnik (1997, 128-129)).

Turning now to Maddy's epistemic disanalogy between the scientific treatment of mathematical and physical hypotheses, recall that it had three aspects: that scientists do not worry about the ontological commitments of the mathematics they use; that they do not usually care about physical structural assumptions it implies and that they do not take their observations to provide evidence for their mathematics. These all make sense when we think of our special empirical sciences as local theories practiced according to local conception of evidence in accordance with pragmatic rationality. Scientists working in a particular field, particle physics, say, are interested in the local issues specific to that field, so of course they do not worry about the ontological commitments of the mathematics they use because these are not included in those local issues. They do not worry about the physical structural assumptions implied by their mathematics because, for pragmatic reasons, they are interested in assessing other hypotheses more closely related to the local issues with which they are concerned. Finally, they do not take their observations to support the mathematics they use because, for pragmatic reasons, they are using those observations to test hypotheses germane to their own specialty. As before, it makes good sense to practice science like this.

For Resnik, there is a further aspect to the special role mathematics plays in science, namely that scientists shield mathematics from revision. Resnik argues that we should practice science this way even though confirmation is holistic because mathematics is our most global theory (*op. cit.*, 125-126).[59] This is a good point to make; clearly one should shield mathematics from revision because changes to mathematics have consequences throughout science. By appeal to his pragmatic approach to indispensability, Resnik also suggests that we would always have

[59] Resnik does not take logic to be a more global theory because he does not regard logic as a theory, see (1997, 125, *n*.22)).

pragmatic reasons for believing mathematics and that these would counsel us to protect mathematics from revision (*op. cit.*, 124). However, since the pragmatic approach to indispensability presupposes that there are mathematical objects and that the mathematics used in science is true, this gives a reason for shielding mathematics from revision only to those with a prior commitment to mathematical realism.

Be that as it may, Resnik is right to think that the global local distinction and local conceptions of evidence provide a sufficient basis from which to explain the facts of scientific practice. Conceiving of the physical sciences as more or less global theories, and of scientists as specialists in their fields, operating under the guidance of a local conception of evidence, grounds of good sense counsel that experiments be used to test particular hypotheses and that mathematics should play a special role insulated from the kind of treatment ordinarily meted out to physical hypotheses. In this way, Resnik's account of mathematics provides a methodology that reconciles the Quinean approach with the facts of scientific practice.

5.6.2 Explaining the facts of mathematical practice

We return now to the aspect of mathematical practice outstanding from our discussion of 5.4, that mathematicians (more specifically, set theorists) do not look to applications to help them decide upon new axioms. Presumably, it was features of practice such as this that moved Resnik to remark that we practice mathematics "as if it were an a priori science" (1997, 120). As his view is that mathematics is not an apriori science, this impression is of course misleading. How, then, is the appearance of apriority to be explained away?

To help with this, Resnik first explains why we should expect mathematical standards of evidence to appeal to methods internal to mathematics, rather than appealing to the justificatory canons of other theories. Given the pragmatic appeal to indispensability, this expectation is said to derive from the constraints governing the local conception of mathematical evidence:

> From this perspective [the perspective of pragmatic indispensability] we may encourage mathematicians to develop their own standards of evidence, so long as the result does not harm science as a whole. Because mathematics is our most global science we should expect that many mathematical methods and principles will be justified by means of considerations neutral between the special sciences, and thus often pertaining to mathematics alone. (1997, 129)

The argument here is rather short, so it is not obvious why being "our most global science" should make us expect that the standards of evidence constitutive of the local conception of mathematical evidence should ratify mathematical justifications "neutral between the special sciences". Nevertheless, a little reflection shows that we should.

Suppose, first, we were to evaluate given mathematical claims by considering only their biological applications. As there is no reason to expect that the mathematical needs of other special sciences will be just the same as those of biology, using these applications as the main source of evidence for mathematics could limit the usefulness of mathematics elsewhere in science. This would be damaging to science, so, because local conceptions of evidence are required to minimize damage to science, we would expect a local conception of mathematical evidence that does not accord biological applications a central role in mathematical justification.

Next, observe that mathematics is our most global science. For Resnik this means that mathematics is presupposed by all the special sciences, so clearly the argument we have just made from biological applications could be made from the mathematical applications of any special science.

Thus we can expect a local conception of mathematical evidence that does not accord a central role in mathematical justification to the mathematical applications of any special science, a local conception that is, in this sense, neutral between them. It follows that whatever methods become the norm in the local conception of mathematical evidence, they must either be internal to mathematics, or drawn from an even more global theory. Since there is no more global theory than mathematics, it follows that these methods must be internal to mathematics. Thus the constraints regulating the local conception of mathematical evidence, that mathematics is our most global theory and that we should always aim to minimize disruption to science as a whole, lead us to expect a local conception of evidence the standards of which are primarily concerned with intra-mathematical considerations.

In as much as this should make us expect a local conception of mathematical evidence that does not typically appeal to empirical considerations, it would certainly make us expect mathematics to be practiced "as if it were an apriori science". To reinforce this expectation, Resnik explains how allowing mathematicians to develop the local conception of mathematical evidence as they see fit (subject to the constraint of not harming science as a whole) would also explain specific features of mathematical practice. In the following passage, for example, he explains how this approach might have led to the dominant role played by proof:

> Early mathematicians probably took their experience with counting, book-keeping, carpentry and surveying as evidence for the rules and principles of arithmetic and geometry that they eventually took as unquestionably true. They began to put more emphasis on deduction after they became aware of the difficulties in deciding certain mathematical questions by appealing to concrete models, which, for example, are notoriously unreliable in deciding geometric questions. ... Even today we could (and

> sometimes do) use concrete models to decide certain mathematical questions; for example, we might simulate a Turing machine on a computer to determine whether it gets into a certain state when processing a given input. But the advantages of proof to the practice of mathematics are so obvious that frowning on experimental approaches has served the goals of mathematics better than allowing or promoting such approaches. Moreover, proof wins out from the perspective of science as a whole. For requiring mathematics to prove its results increases its reliability, and decreases its susceptibility to experimental refutation. (1997, 129-130)

The central role of proof in mathematical practice, its function as the primary vehicle of mathematical justification, thus emerges on Resnik's view as a sensible reaction to difficulties met within mathematics and also as a sensible means of ensuring the usefulness of mathematics in applications. And so we see that this key feature of the seeming apriority of mathematics is easily explained by Resnik's methodological theory.

Taking our cue from this example, it is also possible to explain why mathematicians do not look to applications to help decide on new axiom candidates. Mathematicians are looking for answers to these questions from the perspective of the local conception of mathematical evidence, which as we have seen will be neutral between the special empirical sciences. Mathematicians practice their science this way because this approach has proved beneficial for both mathematics and science. That they practice their subject like this is thus not an objection to the view that mathematics is scientifically confirmed; in fact, it is what we should expect if we adopt Resnik's account of mathematical and scientific method.

We thus conclude that Resnik's account of mathematics does provide sufficient resources to reconcile the facts of mathematical practice with the scientific confirmation of mathematics.[60] Because of the global nature of mathematics, the local conception of mathematical evidence develops in such a way that mathematics is practiced as if it were an apriori science. However, this is not a true reflection of its nature because the local conception of mathematical evidence is justified by reference to the system of science as a whole and is overridable by more global conceptions. So the evidence for mathematics ultimately derives from its applications even though its practice suggests otherwise.

5.6.3 The confirmation of dispensable mathematics

We turn finally to the challenge of accounting for the confirmation of dispensable mathematics. To find out whether Resnik's development of Quinean realism meets this challenge we must find out whether it delivers confirmation for dispensable axioms in just those cases for which it is required, i.e. for those that are accepted in practice.[61] Resnik's theory aims to explain the confirmation of such axioms by appeal to the local conception of mathematical evidence, the idea presumably being that they are to be believed if and only if they have been shown to measure up to the standards provided by the local conception. But does measuring up to these standards make it more likely that a dispensable axiom is true?

To approach this question it helps to know what Resnik takes our local conception of mathematical evidence to be. On the deductive side it is said to include proof and calculation (1997, 138). On the non-deductive side it is said to include a variety of methods, including: the modelling of new structures in previously accepted ones (*op. cit.*, 142); the direct

[60] This is not to say that it is all necessary, however, it may be that the principle that we should always act to minimize harm to science is alone sufficient to deliver these results. But that need not concern us here. We are interested in whether there is a satisfactory extension of Quinean realism that can explain the facts of scientific and mathematical practice, not in finding the most economical extension possible that allows us to do this.

[61] The reader will recall that by dispensable axioms we mean axioms, or axiom candidates, that are not indispensably applied, i.e. those that either lack applications or that have only dispensable applications.

verification of elementary geometric and numerical claims by inspection of simple physical structures (*op. cit.*, 143); the indirect verification afforded by deriving previously established claims from new ones (*op. cit.*, 147-148); the testing of new claims by assessment of their (relative) consistency (*op. cit.*, 148); the use of computations in non-deductive inference from empirical premises (*op. cit.*, Chapter 8, §2). To say that such sources constitute a conception of evidence is just to say that if a mathematical claim enjoys a measure of support from one, or more, of them, then it may be worthy of (a corresponding degree of) belief.

Now, it is not immediately obvious that these should all be considered sources of evidence. Why, for example, should the usefulness of a new claim in proving previously established results be taken as a reliable guide to its truth? Why should facts concerning the (relative) consistency of a mathematical claim be taken in this way?[62] We are given no reason to think they should. However, this need not be a problem for Resnik's position as his view is not that these sources of support are all genuinely evidential, in the sense of providing evidence for mathematics in the absence of other considerations. Rather his view is that they are genuinely evidential when considered as parts of a local conception of evidence pragmatically justified by global considerations.

Resnik's argument that the local conception of mathematical evidence is genuinely evidential appears to rely on two strands: (a) his use of the pragmatic approach to indispensability to justify the practice of positing mathematical objects, truths about mathematical objects and truths linking mathematical objects to physical objects (*op. cit.*, 129); (b) his insistence that local conceptions of evidence are overridable by global considerations (*op. cit.*, 100). Clearly the first of these is intended to provide a foundation for the local conception of mathematical evidence, setting up mathematical positing as a reliable route to mathematical knowledge, whilst the second

[62] These questions are particularly pressing in the present context because, if legitimate, these methods will be of assistance in evaluating the dispensable mathematical claims we are really interested in, namely unapplied axiom candidates.

is intended to act as a check on the ways the local conception of mathematical evidence can develop, so as to help ensure its continued reliability as it evolves. What we are concerned to find out is whether these constraints are sufficient to ensure that the standards constituting the local conception of mathematical evidence should be regarded both as genuinely evidential and as able to provide confirmation for dispensable axiom candidates.

Consider, first, the overridability constraint. One way to understand this might be in terms of ratification, as the requirement that local conceptions of evidence must deliver verdicts on hypotheses that agree with the verdicts delivered by global considerations. If we assume that the global considerations in question are genuinely evidential, then this reading of the overridability constraint would suffice to ensure that the local conception of mathematical evidence is genuinely evidential. However, it would do so only at the cost of an adequate account of the confirmation of dispensable axioms.

To see why, observe that Resnik's system provides two kinds of global consideration with which to assess whether given mathematical claims should be endorsed. The first kind concerns their indispensability, delivering a positive verdict if and only if the claims are indispensably used in science. Clearly considerations of this kind will not deliver confirmation for any dispensable axiom. The second kind of global consideration concerns pragmatic arguments about the good of science as a whole. But when an axiom is dispensable it either lacks applications or has only dispensable applications. In the first case, the axiom is not involved in science at all. In the second, the axiom is involved in science but only in applications for which there are alternatives that do not use the axiom and which are not inferior, scientifically speaking, to those that do. In neither case, then, is it clear that the truth of the axiom is required by the good of science as a whole. This suggests that global considerations of the kinds available in Resnik's system do not provide conclusive reasons to endorse dispensable axioms.[63]

For this reason, the current approach to overridability blocks an adequate account of the confirmation of dispensable axioms. If we understand the overridability constraint in terms of ratification, so that verdicts on mathematical claims delivered by the local conception of mathematical evidence must agree with the verdicts of global considerations, then the local conception of mathematical evidence must agree with global considerations on the confirmation of dispensable axioms. If these are not confirmed in the light of available global considerations, then they are not confirmed on the local conception of mathematical evidence either. So, if we are correct that the global considerations available in Resnik's system do not provide confirmation for dispensable axiom candidates, we must conclude that such confirmation cannot be had on the local conception of mathematical evidence.

To find a new approach to overridability, it helps to reflect on Resnik's idea that decisions about what to believe should respect the good of science as a whole. A plausible aspect of this thought is that we should not form belief in given principles if this would be to the detriment of science. This suggests understanding the overridability constraint to be the requirement that disagreements between the local conception of mathematical evidence and global considerations tend to be resolved in favour of the latter, when the disagreements threaten to harm science as a whole. This understanding of the overridability constraint promises a better approach to the confirmation of dispensable mathematics because it no longer

[63] We should remark here that Resnik's discussion of $V = L$ suggests that considerations of what might be to the good of the system of science in the future should play a role in evaluating dispensable axioms (1997, 146-147). But whilst Resnik is correct to argue that adopting limitative axioms like $V = L$ might be detrimental to future science, and whilst he may be correct to say that this provides reason not to endorse $V = L$, it is not clear that considerations of this kind should persuade us to endorse $\neg(V = L)$, or, more importantly, non-limitative axioms such as AI that assert the existence of certain kinds of mathematical objects. Future science may depend on applications of mathematical theories that assume axioms like these. But this does not give a reason to endorse these axioms now; it only gives a reason to investigate the consequences of taking them as axioms.

demands that the local conception of mathematical evidence agrees with global considerations on all mathematical questions. Agreement would be required when global considerations give a clear ruling for or against a mathematical claim, i.e. when the claim is indispensable, because in cases such as these it would be to the detriment of science as a whole if the verdict delivered by the local conception of mathematical evidence were allowed to take precedence. Nevertheless, the local conception of mathematical evidence would be unconstrained with respect to mathematical questions for which global considerations provide no firm verdict either way.

However, with this reading of the overridability constraint, it is by no means clear that the local conception of mathematical evidence can be viewed as genuinely evidential with respect to dispensable axioms. Recall that the pragmatic justification of the local conception of mathematical evidence had two components, the pragmatic approach to indispensability as a means of establishing the reliability of the practice of mathematical positing, and the overridability constraint as a means of ensuring continued reliability as the local conception of mathematical evidence develops. On the current understanding of the overridability constraint, the local conception of mathematical evidence is only negatively regulated by the perspective of the whole of science: we are not to form belief in any mathematics that conflicts with the mathematics indispensably applied in science. Whilst this leaves us free to believe whatever dispensable axioms there are that do not conflict with indispensably applied mathematics, it does not give us any reason to believe them. Ratification by the local conception of mathematical evidence thus does not increase the likelihood that a dispensable axiom is true unless this is a consequence of the fact that the axiom is generated by the method of mathematical positing. We can be sure of this in the general case only if we possess a general defence of the reliability of this method. So with this reading of the overridability constraint, the local conception of mathematical evidence is genuinely evidential only if the pragmatic approach to indispensability provides a general defence of the reliability of mathematical positing.

This is problematic because the pragmatic approach to indispensability provides no such a defence. For recall how pragmatic indispensability made its appearance:

> (1) In stating its laws and conducting its derivations science assumes the existence of many mathematical objects and the truth of much mathematics.
> (2) These assumptions are indispensable to the practice of science; moreover, many of the important conclusions drawn from and within science could not be drawn without taking mathematical claims to be true
> (3) So we are justified in drawing conclusions from and within science only if we are justified in taking the mathematics used in science to be true.
> (4) We are justified in using science to explain and predict.
> (5) The only way we know of using science thus involves drawing conclusions from and within it.
> (6) So, by (3) above, we are justified in taking this mathematics to be true. (1997, 46-48)

In (6), "this mathematics" is the mathematics of premises (1) and (2), the mathematics indispensably used in scientific practice. At best, then, the argument provides reason for thinking that we are justified in believing an axiom we get by mathematical positing only if it is needed in science. Clearly, then, it provides no general defence of the reliability of mathematical positing. Thus on the second reading of the overridability constraint, the local conception of mathematical evidence appears not to be genuinely evidential with respect to verdicts concerning dispensable axioms.

These considerations suggest that Resnik's approach to dispensable mathematics does not deliver the goods hoped for. If the overridability constraint is taken to be the requirement that the local conception of mathematical evidence agrees with global considerations on all verdicts concerning mathematical claims, then the local conception may be genuinely evidential but it will be constrained not to ratify dispensable axioms as true. If, on the other hand, the overridability constraint is taken to be the requirement that the local conception agree with the verdicts of global considerations for all mathematical claims for which disagreement would harm science as a whole, then we have no reason to think that the local conception is genuinely evidential with respect to dispensable axioms. It is possible there is a reading of the overridability constraint somewhere between the readings we have considered, allowing the local conception of mathematical evidence greater freedom from global considerations than our first reading but not as much freedom as our second. If there is, then perhaps it will avoid the difficulties associated with the readings we have considered. But Resnik does not provide any reason to think that there is such a reading. His account of mathematical knowledge thus cannot be said to provide an adequate account of how dispensable axioms can be confirmed.

What does this conclusion mean for the argument that mathematics is scientifically confirmed? It is certainly a worry. But if standard mathematics is indispensably applied it may not constitute a great threat, for then we may at least be able to claim that this is scientifically confirmed. Against this, however, a significant body of research on the foundations of mathematics suggests that applications of standard mathematics in science are not typically indispensable. Work done in particular by Feferman, Friedman, Takeuti and Simpson shows that the mathematical axioms required to prove a great deal of scientifically applicable mathematics are considerably weaker than those required to derive standard mathematics. On the basis of this research, Feferman (1993a) presents a predicative system of mathematics, W, which he argues is a sufficient basis for almost all the mathematics required in scientific applications. The system W is considerably weaker than ZFC; it proves the existence only of those sets that can be produced from the set of natural numbers by predicative set-forming operations. W thus lacks the (unrestricted) power set axiom, does not prove the existence of the real numbers and cannot develop real analysis properly so called (it suffices for a predicative version of analysis, but this is not the same). The suggestion is that W, or some mathematical theory of comparable strength, may provide an adequate mathematical basis for all scientific applications.

It is important to realize that the claim being made here is not that there are no indispensable applications of impredicative mathematics. Feferman himself concedes that some mathematics that cannot be represented in systems like W is used indispensably in "a couple of cases in some approaches to the foundations of quantum field theory" (1999, 109). Moreover, there is the interesting example of Kruskal's Tree Theorem, which has applications in computer science, and which has been shown by Friedman not to be predicative (Friedman's result is explained in Simpson (1985)).

However, it is not clear that isolated examples such as these are a sufficient basis from which to mount a convincing defence of the claim that impredicative mathematics is scientifically confirmed. The initial attraction of Quinean realism was that it seemed obvious that standard mathematics, including impredicative theories like real analysis and full set theory, is indispensably applied all over empirical science. Assuming that it would be possible to show that this mathematics shares in the scientific evidence provided by its indispensable applications, the anticipation was that there would be a wealth of evidence in its favour, so that it could not reasonably be considered to be unconfirmed. Surprisingly, however, it turns out that most scientific applications can be satisfactorily carried out using only predicative mathematics. The wealth of evidence anticipated for impredicative mathematics thus does not appear so that it still seems quite reasonable to think that this mathematics remains unconfirmed. It is thus not clear that Quinean realism provides adequate grounds for impredicative mathematics including central theories like real analysis and impredicative set theory. And if our previous argument is correct, the same holds for Resnik's development of the Quinean view.

5.7 Conclusion

This chapter addressed the empiricist strategy of arguing that sense perceptions of ordinary physical objects constitute evidence for mathematical beliefs. We considered Quinean realism, which infers from the Quinean theory of evidence and the scientific indispensability of standard mathematics that mathematics is scientifically confirmed (section 5.1). We defended this against Maddy's objections from scientific and mathematical practice, arguing that Quinean realism is intended as an account of scientific and mathematical confirmation, not as a methodological account of scientific and mathematical practice (sections 5.3 and 5.4). But we conceded that the objections raised valid concerns about the Quinean view, whether it can be extended by a methodological theory that satisfactorily explains the facts of practice, and that successfully explains the confirmation of dispensable mathematics.

Resnik's development of Quinean realism promised to address these challenges (section 5.5). But although we were impressed by the way Resnik's view dealt with the facts of scientific and mathematical practice (sections 5.6.1 and 5.6.2), we were not impressed by the way it dealt with dispensable mathematics (section 5.6.3). The view posits an overridable local conception of mathematical evidence that is ultimately supported by the pragmatic approach to indispensability. The hope was that dispensable mathematics could be viewed as being confirmed by considerations made available by this conception. But against this, we argued that considerations from the local conception of mathematical evidence would either be constrained not to ratify dispensable mathematics as true, or would not be genuinely evidential with respect to dispensable mathematics (depending on how the overridability constraint is understood). Thus we concluded that Resnik's theory does not adequately account for the confirmation of dispensable mathematics.

We then argued that this creates a serious difficulty for the view that mathematics is scientifically confirmed (section 5.6.3). Appealing to research on the foundations of mathematics, in particular to work of Feferman, we claimed that the scientific applications of impredicative mathematics are usually dispensable. On this basis we argued that it is not clear, on either Quinean realism or Resnik's theory, that the indispensable applications of mathematics provide adequate confirmation for impredicative mathematics, including important mathematical theories like real analysis and (impredicative) set theory.

We are thus left with no reason to maintain that mathematical knowledge quite generally may be grounded in empirical applications, no reason to think that all of what passes for mathematical knowledge can be known on grounds provided by empirical applications. This is a disappointing result for the current empiricist approach to what passes for mathematical knowledge. Some consolation is available, however, as our discussion does not provide any reason to dismiss the idea that scientific applications provide adequate support for mathematical theories whose widespread indispensability is in doubt.

Appendix: Sober's objection to Quinean realism

We mentioned in section 5.2 that Sober (1993) mounts an attack on Quinean realism. We have already explained that Sober's is unsuccessful. But in this appendix we describe and discuss Sober's critique in more detail.

A. Sober's objection

Sober argues that once we have the right view of scientific confirmation, his own view, *contrastive empiricism*, we find that mathematics could not be confirmed in its indispensable applications. Contrastive empiricism is distinguished by its sophisticated approach to inference to the best explanation, which it takes to be a method of reasoning capable of delivering justified belief in statements about unobservable objects save when the statements are parts of empirically equivalent competing explanations. In so doing, contrastive empiricism sustains both the empiricist idea that the only grounds for forming belief are empirical grounds (an idea that scientific realists typically reject) and the scientific realist idea that empirical grounds justify belief in thoughts the content of which outstrips possible experience (an idea that empiricists typically reject). Thus contrastive empiricism is a compromise between scientific realism and empiricism. According to Sober, it shows that mathematics is not scientifically confirmed.

Contrastive empiricism has some things in common with Quine's theory of evidence. Both accept the empiricist idea that scientific evidence is sensory evidence and both accept the evidence-warrant principle; that in normal circumstances what counts by ordinary scientific criteria as conclusive evidence for a hypothesis warrants belief in its truth. Where they differ is on the distribution of evidence across theory; Quine's view maintains that confirmation is holistic, contrastive empiricism maintains that it is not. Why not?

Contrastive empiricism views the testing of scientific theories as an essentially contrastive process. Thus science has decided that our current theory of combustion provides a better account than the phlogiston theory of why the ashy remains of a fire weigh more than the fuel burnt, science has decided that the heliocentric model of our solar system gives a better account than the Ptolemaic model of astronomical observations, etc. Given this view of theory testing, it does not really make sense to think of scientific hypotheses as being confirmed absolutely. Instead we must think of hypotheses as being confirmed relative to the hypotheses with which they compete as accounts of the observations. This means acknowledging that, relative to agreed and accepted background assumptions, observations provide differing degrees of evidential support to the hypotheses between which they discriminate. Since this is inconsistent with confirmational holism, according to which observations do not favour individual hypotheses over others, confimational holism has to be denied.

Drawing on Edwards (1972), Sober suggests that the differential support lent by given observations to different hypotheses should be understood in terms of the probabilities the hypotheses confer on the observations (see Sober (1993, 38)). Following this idea, he puts forward the Likelihood Principle, which states that when the probability conferred on an observation by one hypothesis is greater than that conferred on it by another, the observation favours the first hypothesis over the second, more formally:

> O favours hypothesis H_1 over another H_2 if and only if $P(O/H_1) > P(O/H_2)$.

Contrastive empiricism thus differs from the epistemology of total science by rejecting confirmational holism, allowing that different degrees of evidential support attach to individual hypotheses and then by explaining these differing degrees of evidential support by appeal to the Likelihood Principle.

This approach to scientific confirmation implies that confirmation is symmetric: a hypothesis can be confirmed by observation if and only if it can be disconfirmed by observation. This is because a hypothesis H_1 is confirmed only if an observation O favours it over another hypothesis H_2. By the Likelihood Principle, this happens when $P(O/H_1) > P(O/H_2)$. Now by the mathematics of probability, $P(O/H_1) > P(O/H_2)$ if and only if $P(\neg O/H_1) < P(\neg O/H_2)$. So if we had observed $\neg O$ rather than O, we would have preferred H_2 to H_1 on the basis of the Likelihood Principle, so H_1 would have been disconfirmed. Sober believes that the symmetry of confirmation rules out any possibility that indispensable mathematics is scientifically confirmed:

> If the mathematical statements *M* are part of every competing hypothesis [if they are indispensable], then, no matter which hypothesis comes out best in the light of the observations, *M* will be part of that best hypothesis. *M* is not tested by this exercise, but is simply a background assumption common to the hypotheses under test. Sober (1993, 44)

Sober's objection to Quine's mathematical epistemology is therefore as follows: A hypothesis is an indispensable component of the scientific explanation of given observations if it features in all competing explanations of those observations. Since to be disconfirmed is to be selected against in our choice of explanation, if a hypothesis is indispensable, it cannot be disconfirmed. Thus, by the symmetry of confirmation, if a hypothesis is indispensable, it cannot be confirmed. This holds for indispensable hypotheses whether they be mathematical or not, therefore mathematics is not confirmed in its indispensable applications.

B. Responses to Sober's objection

Sober's objecton can be undermined by attacking contrastive empiricism. Resnik (1995, 1997) argues that this leaves high-level empirical principles without empirical support. Principles such as the conservation of mass-energy or the continuity of space-time feature as background hypotheses to all our explanations of certain physical phenomena[64], so if the account of scientific confirmation put forward by contrastive empiricism is correct, these principles would lack empirical support, and perhaps even empirical content. Resnik cannot bring himself to accept this, so he rejects contrastive empiricism. In addition, Hellman (1999) points out that, when theories make new predictions about phenomena of which we previously had no account, scientists adopt them in the absence of alternatives. For example, the theory of relativity makes the right prediction about how relativistic mass and total energy are related, something not predicted by any prior physics, and this functions as part of our grounds for accepting it. But if contrastive empiricism is correct, so that a hypothesis is only ever confirmed relative to competing hypotheses, theories should not be chosen this way, so again contrastive empiricism must be rejected. Finally, Colyvan (1999) has argued that scientists often appeal to factors other than likelihoods when choosing amongst theories, so that the approach to theory choice suggested by contrastive empiricism, which would rationalize theory choice solely through likelihoods, must be rejected.

The last of these points does not seem particularly strong. Colyvan thinks that given two theories that perform equally well under empirical testing:

> The question of which is the better theory will be decided on the grounds of simplicity, elegance and so on – grounds

[64] The examples are Resnik's (1997, 120).

> explicitly ruled out by contrastive empiricism. Colyvan (1999, 325-326)[65]

However, it is not true that contrastive empiricism rules out choosing between theories on these grounds; it makes them subordinate to likelihoods, but it does not legislate against them altogether:

> I ... don't object to the idea that the concepts cited – simplicity, *ad hoc*ness, and explanatoriness – sometimes provide reasons that are pertinent to judging truth values. What I deny is that they do so in a way that transcends the bearing of likelihood. Sober (1993, 43)

Thus the aspects of theory choice mentioned by Colyvan do not provide evidence against contrastive empiricism.[66]

However, the objections due to Resnik and Hellman are quite compelling. As contrastive empiricism stands it does not explain how very general empirical principles get confirmed, as surely they are. Neither does it permit the confirmation of theories that compare favourably with observation but have no competition. Sober (1993, 52) suggests that in such cases we can think of the theories as being tested against either their negations or against competitors constructed out of the theory by making adjustments to it.[67] However, this is not what scientists do in practice and

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[65] The competing theories Colyvan is considering at this point are the usual mathematical formulation of Newtonian mechanics and Field's allegedly nominalistic reconstrual (Field (1980)). But presumably the point is meant quite generally.

[66] Elsewhere Sober discusses the alternatives to science dreamt up by philosophical sceptics, and puts forward his view that strictly speaking we have no evidence against them. But he goes on by saying, "This is not to deny that human beings look askance at evil demons and their ilk. We do assume that they are implausible. ... But I cannot see a rational justification for thinking about the world in this way. I cannot see that we have any non-question-begging evidence on this issue." Sober (1990, 129). This suggests that he is sympathetic to pragmatic reasons for preferring one theory over another even when these do "transcend likelihoods". His position would just be that these are not evidential reasons for choosing the one over the other.

[67] Alternatives of this kind to Einstein's famous equation $E = mc^2$ would be $\neg (E = mc^2)$, $E = 2mc^2$, $E = mc^3$, etc. That Sober regards these theories as potential competitors to the original theory is at least a partial retrenchment, for in his original explanation of contrastive empiricism, he explicitly distances himself from the thought that contrasting a theory with its negation constitutes a genuine discrimination problem: "I mentioned before that theory testing is a contrastive activity. If you

there seems to be no reason to pretend otherwise unless we have a prior commitment to contrastive empiricism. More importantly, if this method is effective for scientific hypotheses, then it could equally well be used to bring support to bear on the mathematics applied in science. Any non-mathematical theory of observable phenomena counts as an alternative to mathematical theories of the same phenomena. So do theories that use wacky alternatives to standard mathematics. Many of these alternatives will be inferior to our mathematical theories as scientific accounts of the relevant phenomena. So if the method Sober describes for bringing observational evidence to bear on otherwise untested scientific hypotheses is acceptable, then mathematical hypotheses can be viewed as having been tested in a similar fashion.

However, such gerrymandering of practice is clearly undesirable, unless it helps to achieve some otherwise unattainable goal without too great a cost. This is not the case here. The underlying point is that if a scientific theory performs well enough as an account of a sufficiently broad range of observations, it is confirmed even in the absence of a contrasting theory. Rather than distort practice by claiming that such theories are tested against contrived alternatives, we should conclude that confirmation is not essentially a contrastive phenomenon.

Sober's objection to Quinean realism is therefore not convincing. However, a variant of it survives. Recall that Sober's objection went as follows: A hypothesis is an indispensable component of the scientific explanation of given observations if it features in all competing explanations of those observations. Since to be disconfirmed is to be selected against in our choice of explanation, if a hypothesis is indispensable, it cannot be disconfirmed. Thus, by the symmetry of

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want to test a theory T, you must specify a range of alternative theories – you must say what you want to test T *against*. [New paragraph] There is a trivial reading of this thesis that I do not intend. To find out if T is plausible is simply to find out if T is more plausible than *not*-T. I have something more in mind: there are various contrasting alternatives that might be considered. If T is to be tested against T', one set of observations may be needed; but if T is to be tested against T'', a different set of observations may be needed. By varying the contrasting alternatives, we formulate genuinely different testing problems." Sober (1990, 123) confirmation, if a hypothesis is indispensable, it cannot be confirmed. This holds for indispensable hypotheses whether they be mathematical or not, therefore mathematics is not confirmed in its indispensable applications.

For Sober, contrastive empiricism lent support to this argument because it implies the symmetry of confirmation, that a hypothesis can be confirmed by observation if and only if it can be disconfirmed by observation. However, one might believe that confirmation is symmetric without believing contrastive empiricism. If this is a reasonable position, then an argument like Sober's can be mounted against the Quinean account of mathematical knowledge by assuming the symmetry of confirmation instead of contrastive empiricism. Call this new objection the argument from the symmetry of confirmation.

Colyvan (1999) tries to defend the Quinean view against the new threat:

> When a theory is confirmed, the *whole* theory is confirmed. When it is disconfirmed, it is rarely the fault of every part of the theory, and so the guilty part is to be found and dispensed with. It's analogous to a sensitive computer program. If the program delivers the correct results then every part of the program is believed to be correct. However, if it is not working it is often because of only one small error. The job of the computer programmer (in part) is to seek out the faulty part of the program and correct it. Colyvan (1999, 329)

By analogy, the job of the scientist, when faced with a theory that delivers mistaken predictions, is to identify the part of the theory responsible for the mistaken prediction and to find a way of replacing it so the mistaken prediction is no longer entailed. Colyvan thinks that this shows there is a "partial asymmetry" (*loc. cit.*) between confirmation and disconfirmation on the holistic view: theories are confirmed as wholes, but disconfirmed in pieces. Thus he takes it that the argument from the symmetry of confirmation against Quinean realism is unsound.

However, this is not sufficient to protect the Quinean position from the argument from the symmetry of confirmation. It is true that when presented with a scientific theory that makes false predictions, we look for individual hypotheses to revise so that the predictions are no longer made. But to take this as evidence that confirmation is not symmetric is to assume that our practice of theory revision is justified by aspects of the logic of confirmation, which, given the Quinean approach to confirmation, is a mistake. Assuming the Quinean theory of evidence, if a theory T predicts P, our observation that ¬P disconfirms T as a whole and, as a whole, we reject T. Of course this does not mean that we reject every individual hypothesis of T. Rather we look to make small changes to some parts of T hoping to arrive at a new theory T' which possesses all the virtues of T but which does not imply P. But we do this because it is sensible to proceed this way, as it is normally harder to produce an entirely new theory dealing with given phenomena than it is to make improvements in an old one. Thus our practice of theory revision reflects good sense in scientific practice, what we might call scientific know-how, rather than facts about the logic of confirmation. So from the Quinean perspective, this practice does not show that the symmetry of confirmation should be denied. Colyvan thus gives no reason to think that the symmetry of confirmation is incompatible with the Quinean theory of evidence, and therefore no reason to think that the argument from the symmetry of confirmation is unsound, on the Quinean view.

There is another difficulty with Colyvan's response. The variant of Sober's objection that we are considering uses the argument the symmetry of confirmation only for an inference between two conditionals (from the claim that if a hypothesis is indispensable it is not disconfirmable, to the claim that if a hypothesis is indispensable it is not confirmable). But this inference remains valid if we assume, instead of the symmetry of confirmation, the weaker premise that a theory is confirmable only if it is disconfirmable. There is thus a more refined variant of Sober's objection that argues from this weaker claim together with the indispensability of mathematics to the conclusion that mathematics is not scientifically confirmed. Colyvan's claim that confirmation is partially symmetric is consistent with the premises of this new argument, in particular, with the premise that a theory is confirmable only if it is disconfirmable. So for this reason, too, Colyvan's remarks on the symmetry of confirmation are not sufficient to sustain the Quinean account of mathematical knowledge.

However, this does not mean there is no defense against the latest variant of Sober's objection. In fact, there is a very clear reason why the new attack fails. This most recent argument assumes the conception of indispensability involved in Sober's original argument. According to this conception, a portion of mathematics is indispensable to given observations if it appears in all the competing scientific accounts of them:

> Let us suppose that mathematics is an indispensable part of any scientific explanation of the observations we have at hand. That is, each of the competing hypotheses (H_1, H_2, ... , H_n) embeds a set (M) of mathematical propositions. Sober (1993, 44)

But the understanding of indispensability assumed by Quinean realism is different. As we saw in section 5.1.2, a mathematical theory M is indispensable in this sense to the scientific explanation of observations O if and only if:

> There is a theory T involving M such that any theory T' not involving M is an inferior to T as a scientific account of O.

Given this conception of indispensability, it is clearly possible for a mathematical theory to be indispensable to a scientific theory without appearing in all competing explanations of the observations that scientific theory is supposed to explain. But if the variant of Sober's argument we are considering is to succeed, this must be impossible, for its very first step, which is inherited from Sober's original argument, is to infer from the claim that a mathematical theory is indispensable to the claim that it appears in all the competing scientific explanations of the relevant observations. On the Quinean understanding of indispensability, therefore, the most recent variant of Sober's objection, and, indeed, the original argument, is not valid.

It is worth pointing out that Sober vacillates between something like the Quinean conception of indispensability and his own conception of indispensability as appearance in all competing alternatives. Sober introduces indispensability like this:

> How is the idea of "indispensability" connected with the Likelihood Principle? When a scientist considers a set of competing hypotheses, and one of them says that the observations were quite probable, while the other hypotheses say that the observations were immensely improbable, it is natural to conclude that only the first hypothesis makes the observations nonmiraculous. The scientist may be inclined to regard the first hypothesis as indispensable. (1993, 38)

A hypothesis that is indispensable in the sense involved in this passage clearly need not be a component of all competing explanations of the observations; it just needs to be part of the best scientific account we have of those observations. Something like the Quinean conception of indispensability is in play. As we have seen, however, Sober later prefers the conception of indispensability as appearance in all competing alternatives later on (see the previous quotation). In fairness, both conceptions of indispensability are plausible refinements of the vague idea that mathematics is somehow essential to science, at least when the underlying view of science is contrastive empiricism. If science aims at solving discrimination problems and if the alternatives available to solve one problem all incorporate a common nucleus of hypotheses, it is very natural to say that those hypotheses are indispensable to the solution of that problem. But it is also very natural to say that the indispensability of a appears in all the competing scientific explanations of the relevant observations. On the Quinean understanding of indispensability, therefore, the most recent variant of Sober's objection, and, indeed, the original argument, is not valid.

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Be that as it may, this equivocation between notions of indispensability totally undermines both Sober's argument against the Quinean account of mathematical knowledge and the variants on that argument that we have considered. The variants are not sound when read with respect to the relevant, Quinean notion of indispensability. The original argument fails for this reason and also because it assumes the mistaken view that confirmation is an essentially contrastive phenomenon. Sober thus provides no telling objection to the view that mathematics is scientifically confirmed.

6

Fictionalism and nominalization

If correct, the arguments of the previous four chapters considerably weaken the credibility of mathematical realism. Chapters 2 and 3 suggest that rationalist approaches to mathematics are incapable of providing adequate grounds for mathematical knowledge. Chapters 4 and 5 suggest that empiricist strategies, at least those we have considered, are presently of only limited application. We are thus left without a satisfactory realist account of what passes for mathematical knowledge.

Our attention thus turns to alternatives to the realist approach. Many alternatives to the realist point of view have been suggested in the literature. But as we explained in section 1.4, the most promising ones that can be developed in accordance with the fundamental assumptions of this study are versions of mathematical fictionalism, the view that mathematics is, or should be, a form of pretence. The apparent limitations of mathematical realism thus raise for us the question of whether mathematical fictionalism provides a viable alternative. In this and the next chapter we shall address this question.

6.1 Mathematical fictionalism

Mathematical fictionalism attempts coherently to combine the Quinean criterion of ontological commitment, linguistic realism and rejection of mathematical objects (see section 1.4). Its method is to put forward a pragmatics for mathematics according to which attitudes taken towards mathematical claims are not such as to involve commitment to the existence of mathematical objects, even though many mathematical claims are true only if mathematical objects exist.

To foster this approach, an analogy is drawn between mathematics and literary fiction.[68] When we use language to tell a story, and when we engage in literary criticism, we frequently make claims that are true only if there are fictional objects, Sherlock Holmes, the Jabberwock, etc. According to a plausible account of literary fiction, we play along with such claims, imagining or pretending them to be true, but do not believe them. Our attitudes to such claims thus do not commit us to their truth and so we are not committed to the existence of fictional objects.[69]

Mathematical fictionalism takes this situation as a model for mathematics. In doing mathematics, we frequently put forward claims that are true only if there are mathematical objects. But the fictionalist maintains that these object-committed mathematical claims function, or ought to function, in something like the way claims about fictional objects function when they are used to tell a story. One thought underlying this point of view is that it

[68] Most commentators who have defended this kind of fictionalism make use of an analogy between mathematics and literary fiction (see Field (1989a, 3), Wagner (1982, 259-260), and Balaguer (1996b, 291)). Papineau (1993) does not explicitly mention literary fictions although he does take fictionalism to include the view that mathematical claims are accepted "as fictions" (*op. cit.*, 176). More recently, Yablo (2001, 2002, 2005) draws a comparison between mathematics and figural discourse, but it does no harm at this stage to speak as if all fictionalist views invoked an anlogy with literary fiction.

[69] Lamarque and Olsen (1994) develop a detailed account of literary fiction of this kind. Other approaches are possible according to which we are committed to the existence of fictional objects. For example, Searle (1974) regards fictional objects as creations of the author of the fiction in which they appear, whilst Van Inwagen (1977) regards them as posits of literary criticism. The kind of view described in the text seems more intuitively appealing than such alternatives, but this does not mean it is the best available. Nevertheless, the fictionalist analogy between mathematics and literary fiction is to be understood with reference to this approach to literary fiction.

is possible to think of object-committed mathematical claims as being imagined or pretended for the purposes of doing mathematics, rather than as being believed. Taking this up, mathematical fictionalism claims either that object-committed mathematics is, or that it should be, a form of pretence. In either case, belief in mathematical objects is rejected and so we are no more obliged to believe in mathematical objects than we are to believe in Sherlock Holmes or the Jabberwock.

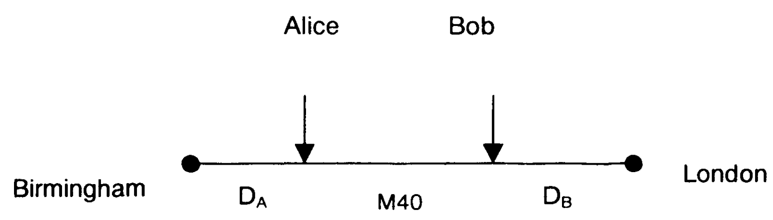
It is easy to see how this approach is supposed to achieve the fictionalist goal. If mathematics is viewed as analogous to literary fiction in the way described, mathematical sentences can still be interpreted at face value, as demanded by linguistic realism, and belief in the claims they express when so interpreted can still be regarded as ontologically committing, as demanded by the Quinean criterion of ontological commitment. Nevertheless, there will be no contradiction in rejecting mathematical objects because belief in object-committed mathematics will be rejected. In this way, Quinean criterion of ontological commitment and linguistic realism will be reconciled with rejection of mathematical objects.

If a satisfactory mathematical fictionalism is to be developed, the analogy between mathematics and literary fiction will have to be superseded by a detailed explanation of the claim that object-committed mathematics is, or should be considered to be, a form of pretence. However, the analogy as it stands allows us to explain the central challenge for mathematical fictionalism, together with the main strategies for dealing with it. We already mentioned these briefly in section 1.4. In the next section, we will set them out more fully.

6.2 The challenge to mathematical fictionalism from applications

It is one hundred miles from Birmingham to London via the M40. If Alice leaves Birmingham at 1100 and drives at 60 mph towards London and if Bob leaves London at 1100 to drive towards Birmingham at 90 mph, at what time of day do they meet? Call this the meeting problem.

Someone who knows basic mathematics and physics is likely to address the meeting problem as follows. t hours after 1100, Alice and Bob have travelled distances D_A and D_B, respectively:



When Alice and Bob meet, the total distance they have travelled, that is the distance travelled by Alice taken together with the distance travelled by Bob, is the distance between London and Birmingham. Letting t be the duration in hours of the period elapsed between the start of their journeys and their meeting, this implies:

$$100 = D_A + D_B$$

Since distance travelled equals speed times time, and since Alice travels at 60 mph, it follows that $D_A = 60t$. Similarly, since Bob travels at 90 mph, it follows that $D_B = 90t$. So by substitution:

$$100 = 60t + 90t$$

Then a simple calculation shows that $t = 2/3$. Thus Alice and Bob meet after two thirds of an hour, that is at 1140.

This problem could have been addressed empirically. It would have been possible to have Alice and Bob make the journeys described so as to observe what time they cross. However, this experiment would not have been easy to perform; it might have been difficult to proceed at the assigned constant speeds, the drivers might have missed each other in the traffic, Bob might have been stopped for speeding. Moreover, it would have been costly and time-consuming. For the metting problem, then, solution by mathematics is preferable to resolution by experiment.

What if we had wanted to find out the age of the universe? How could we address this problem? It seems clear that in this case, no experiment is possible, the only scientific approach being to apply mathematics. Applications of mathematics are not merely desirable for reasons of ease and economy, therefore, but necessary: we need to apply mathematics to solve problems we have no other way of addressing. In consequence, philosophical accounts of mathematics must include a satisfactory explanation of applications.

This requirement raises a worry about mathematical fictionalism. More familiar forms of fiction do not play the roles mathematics plays in solving problems, shoppers do not keep track of their expenditure by reference to *The Merchant of Venice* and biologists tracking changes in Britain's badger population do not appeal to *The Wind in the Willows*. Thus one might suspect that holding mathematics analogous to fiction makes it unfit to play the applied roles that in fact it has. If so, the analogy will have to be rejected, mathematical fictionalism abandoned.

6.2.1 How is mathematics used to solve the meeting problem?

If mathematical fictionalism blocks the successful application of mathematics, then someone who solves the meeting problem in the way described above must do something with mathematics that cannot be explained from the fictionalist perspective. What might this something be? What uses of mathematics must be available if our solution to the meeting problem is to be carried out?

To answer this it helps to consider more closely how a problem solver might arrive at the solution described above. The conditions of the problem together with their knowledge of physics provides them with the necessary background:

(1) The distance from Birmingham to London is 100 miles
(2) Alice travels to Birmingham from London at 60 mph
(3) Bob travels to London from Birmingham at 90 mph
(4) $d = vt$ (distance in miles equals velocity in miles per hour times time in hours)

(1), (2) and (3) contain information about specific places and the movements of two people between them. (4) conveys a theory of motion. All this information is mathematically expressed, with references to individual numbers appearing in (1), (2), and (3), and mathematical concepts of distance in miles, velocity in miles per hour and time in hours appearing in (4).

In addition to the right background, the problem solver needs to have an insight, realizing that when Alice and Bob meet the sum of the distances they have travelled equals the distance between their starting points. This insight is partly mathematical and partly physical as it is an insight into the correct mathematical description of the physical situation.

We can reconstruct the way background and insight act together to yield the solution to the meeting problem in a succession of stages. The first stage is taken up with defining terminology:

(5) Let D = the distance in miles between London and Birmingham
(6) Let t = the time in hours that it takes Alice and Bob to meet
(7) Let D_A and D_B be the distance in miles travelled at t by Alice and Bob, respectively

The second stage uses this new terminology to construct a solvable equation in t. Here the insight, prompted in the solution described above by reflection on the diagram, yields:

(8) $D = D_A + D_B$

Note that it is not essential to the having of the insight that a diagram be used. One could equally well arrive at (8) on the basis of the definitions in (5) and (6) and the information given in (2) and (3). The diagram functions heuristically in the solution to this problem.

Deduction now takes over from insight and definition. The solver deduces from (2), (4), (6) and (7) that:

(9) $D_A = 60t$.

A similar deduction yields:

(10) $D_B = 90t$.

Then from (1), (8), (9) and (10) substitution of equals for equals yields

(11) $100 = 60t + 90t$

In the final stage, calculation takes over to allow the solution of this equation for t, the solver carrying out the following steps in accordance with their mathematical knowledge:

(12) $100 = 150t$

(13) $t = 100/150$

(14) $t = 2/3$

In (14) the value of t is presented in an acceptable form and the solution to the problem is almost complete: it takes Alice and Bob two thirds of an hour to meet, so they meet at 1140.

This reconstruction of our solution to the meeting problem shows very clearly that it uses mathematics in two ways; descriptively, to express information about the physical situation in mathematical terms (for instance in (1), (2) and (3)), and deductively, as a means of validating inferences amongst mathematically expressed claims (for instance to validate the inferences between (12) and (13)). The reconstruction also shows that the descriptive uses of mathematics are governed by definition and insight and that calculation often supplies mathematical premises for deduction. Be that as it may, the uses of mathematics that mathematical fictionalism must preserve if it is not to block the solution to the meeting problem are the descriptive and deductive uses that allow construction of the argument set out above.

6.2.2 The challenge from applications

The fictionalist analogy between mathematics and literary fiction raised a worry concerning whether mathematics is fit to be applied. By attending to specific claims to which mathematical fictionalism is committed and by paying attention to the ways mathematics is used in the solution of the meeting problem, we can make this worry more precise.

Mathematical fictionalism rejects mathematical objects. It must therefore maintain, on pain of incoherence, that science is not committed to mathematical objects. But reference to mathematical objects seems to be important to the descriptive use of mathematics in science. How, after all, can "100 = D_A + D_B" express anything about the relative position of Alice and Bob *t* hours after the beginning of their journeys if there is no number 100? We might expect such a worry to be handled by re-interpreting the mathematics used, so that purported reference to mathematical objects is eliminated. But since mathematical fictionalism rejects such re-interpretation, this is not an option. Thus the worry remains that rejecting mathematical objects blocks a satisfactory account of mathematical description.[70]

Another central claim of mathematical fictionalism is that the attitudes we adopt towards object-committed mathematical claims (claims that are true only if there are mathematical objects) need not include belief. However, belief in such claims seems to be important to preserving the deductive use of mathematics. We want to regard conclusions drawn with the help of mathematics as being warranted thereby. But how can we take this position if we reject belief in object-committed mathematical claims? How, for example, does the sequence of inferences from (12) to (14) help to warrant belief that Alice and Bob will meet at 1140 if the deducer does not believe that 100 = 150*t*? How does the law of motion $d = vt$ help with this if it is not believed that $d = vt$? The worry here is that in eschewing belief in object-committed mathematical claims, mathematical fictionalism eschews the only beliefs that could explain why this solution to the meeting problem, and mathematical reasoning more generally, is reliable.

It has also been argued, for example by Liston (1993), that by rejecting belief in mathematical objects, mathematical fictionalism threatens the

[70] Note that this conclusion holds irrespective of whether the mathematical fictionalism we have in mind affirms that there are no mathematical objects or simply does not maintain that there are mathematical objects.

discoveries made possible in applications.[71] If this is to constitute a worry distinct from the two we have already mentioned (that rejecting mathematical objects might block a satisfactory account of mathematical description and that rejection of belief in mathematical objects might block a satisfactory account of the reliability of mathematical reasoning) it must presumably be taken to concern the cognitive processes required to make these discoveries. The worry would be that without belief in mathematical objects, some cognitive processes necessary for mathematical applications could not be brought to bear on object-committed mathematical claims.

However, if we return to the analogy with literary fiction, we find grounds for thinking that this will be possible even if object-committed mathematics is taken as a form of pretence. Some examples will help to make this clear. Someone who reads *The Glass Bead Game* may infer from it that Joseph Knecht felt unfulfilled by his life in Castelia. To do so, they deploy whatever cognitive processes are required to draw out the consequences of the content of a text. Someone who reads *Macbeth* may explain that Macbeth murdered the king in part because he wanted to satisfy Lady Macbeth's desire for power. To do so, they must deploy whatever cognitive processes are required to construct explanations of claims given in, or implied by, the content of a text. Someone who reads *The Lord of the Rings* will realise that Gollum, being ultimately governed by his desire for the ring, will not allow Frodo to destroy it. To do so, they must deploy

[71] By appeal to an example of the use of Fourier analysis in the theory of harmonics, Liston argues that rejection of belief in mathematics makes it hard to understand (a) how we use mathematics to discover new physical beliefs (b) how we use mathematics to justify physical beliefs (c) how we use mathematics in explanations (specifically of facts concerning the reliability of applications) (d) how we take advantage of mathematics to give us insight into the physical phenomena dealt with in applications (1993, 435-441). Our focus in the text on the claim that rejection of belief in mathematics threatens the discoveries made in applications may seem to ignore the other points. However, it seems reasonable to think of mathematical insights into physical phenomena as a special case of the kind of discovery that applications of mathematics allow us to make, so the discussion in the text can be viewed as dealing with both (a) and (d). What Liston calls the justificatory use of mathematics appears to be subsumed by what we are calling the deductive use, so we have already covered (b). Similarly, the worry concerning the use of mathematics in explanations, point (c), appears to be of a piece with our worry about the reliability of mathematical reasoning (for if fictionalism can explain how mathematical reasoning warrants beliefs about non-mathematical things, it is hard to see why it should not also be able to account for uses of mathematics in explaining beliefs about non-mathematical things).

whatever cognitive processes are required to make discoveries about the content of a text. Note that, in each case, the reader does not believe what the relevant text says about the fictional objects it concerns but that this does not affect their ability to bring these various cognitive processes to bear on the relevant claims.

It is not difficult to think of further examples of this kind in which we would be confronted by a reader who wonders about what might happen in a given literary fiction, or forms conjectures about what will happen, or makes guesses about how events will turn out, etc. Seemingly, then, we can reason in a wide variety of different ways with claims about fictional objects. This already suggests that comparing mathematics to literary fiction need not threaten the cognitive processes required for applications, for it suggests that the kind of cognitive processes applied in reasoning can be seen to be involved in our understanding and appreciation of literary fiction. More importantly, however, such examples raise the general point that reasoning operates on thoughts independently of the attitudes we bear towards them. The fact that mathematical fictionalism eschews belief in object-committed mathematical claims thus does not seem a likely obstacle to our being able to reason with such claims in all sorts of ways, in particular in the ways required for making discoveries. The putative third worry arising from applications, that mathematical fictionalism would block a satisfactory cognitive account of mathematical discovery, thus does not appear to be a threat.

Nevertheless this leaves us with the two worries identified above, that rejecting mathematical objects might block a satisfactory account of mathematical description and that rejection of belief in mathematical objects might block a satisfactory account of the reliability of mathematical reasoning. The uses to which mathematics is put in solving the meeting problem can thus be adequately explained from the fictionalist point of view if and only if it can show that these worries are unfounded. Despite the mathematical simplicity of the meeting problem, it does not seem likely that other problems will throw up uses of mathematics that are different in kind from the ones we have considered. We may thus take the challenge to mathematical fictionalism from applications to be as follows: (a) to recover the descriptive use of mathematics without invoking the existence of mathematical objects; (b) to recover the reliability of mathematical reasoning without invoking belief in mathematical objects.

6.2.3 Two strategies for meeting the challenge

The challenges identified above are by no means trivial difficulties. So how might they be met? One strategy is to attack the idea that mathematical applications are necessary to the successful prosecution of science. If it could be shown that we can do without the uses of mathematics described above, then it might also be possible to argue that tension between these uses of mathematics and claims made by mathematical fictionalism are irrelevant. The idea here would be to argue that the mathematical theories we actually use in science can be thought of as instrumental shortcuts for non-mathematical theories dealing with the same phenomena, in such a way that use of the mathematical theories could be replaced without loss to science by use of the non-mathematical theories. This strategy is developed by Field (1980, 1989a) and Balaguer (1996a, 1996b).

A second strategy for fictionalism concedes that mathematical applications may be necessary to science, thereby forgoing the claim that we can always replace our use of mathematical scientific theories with use of non-mathematical theories. Instead, this strategy describes uncontroversial examples of pretence playing the kind of roles mathematics has in science, using this to argue that mathematics can be applied in these ways even if it is a form of pretence. This strategy is developed by Yablo (1998, 2001, 2002, 2005) and appealed to by Balaguer (1996b).

In the remainder of this chapter, and in the next, we will consider examples of these strategies to see whether it is possible to meet the challenge to mathematical fictionalism arising from applications. Before doing so, it is worth pointing out a further dimension to our investigation. The claim that mathematical applications are necessary to science, over which these strategies for fictionalism differ, is in effect the thesis of the indispensability of mathematics. As we saw in Chapter 5, this is a key ingredient of attempts to defend mathematical realism by grounding mathematical justification on scientific confirmation. Our investigation into whether fictionalism can be defended by arguing that mathematics can be eliminated from science will thus give us more information about the strengths of that position.

6.3 Field's programme for the nominalization of science

Field (1980, 1989a) defends a version of mathematical fictionalism according to which there are no abstract objects. In support of this view, he argues that the use of mathematics in science is a useful, but in principle eliminable, tool. Field's programme for the nominalization of science is intended to help establish this. The idea is to show (a) that each scientific theory that uses mathematics could in principle be replaced by a theory that does not use mathematics but (b) that it is nevertheless legitimate, from a fictionalist perspective, to make use of mathematical derivations in science for the convenience this brings.

Let us first consider how Field aims to establish that the scientific use of mathematics could in principle be eliminated from science. The key claim here is that mathematics is dispensable. What Field means by this, roughly speaking, is that there is a scientifically viable but nominalistically acceptable alternative to every scientific theory that uses mathematics. More precisely, the thought is that for every scientific theory T of phenomena P that uses mathematics, there is a nominalistically acceptable theory N that is not inferior to T as a scientific account of P.

In support of this, Field produces what he takes to be nominalistically acceptable reformulations of non-trivial scientific theories. He deals in particular with Newtonian Gravitional Theory (1980), however the methods he illustrates could also be used to reformulate other field theories that apply classical mathematics to a flat space-time.[72] Field suggests that his nominalistic reformulations are preferable to mathematically formulated originals because (a) they offer intrinsic rather than extrinsic explanations of the same phenomena (*op. cit.*, 43), (b) they minimise arbitrary choices in the presentation of our scientific beliefs (*op. cit.*, 45) and (c) they are not appreciably more unwieldy to use than the mathematical originals (*op. cit.*, 90-91). If these claims are correct, they may perhaps establish not just

[72] They are due to Krantz *et al.* (1971).

that the use of mathematics is eliminable from science, but also that the use of mathematics should be eliminated from science (as we should always prefer Field's reformulations to the original theories). However, Field does not actually need this stronger claim for his argument that mathematics can be viewed as a useful, but in principle, eliminable tool. The argument requires only the weaker claim that mathematics is dispensable. If this is correct, if non-mathematical scientific accounts are always available that are not less good, scientifically speaking, than mathematical competitors, then clearly mathematics is in principle eliminable from science.

Let us now consider how Field hopes to establish that it is legitimate for a fictionalist to use mathematical derivations as support for nominalistic conclusions. The central claim here is that mathematical theories are conservative over nominalistic theories. Informally, this means that a nominalistically acceptable claim follows from a mathematical theory and nominalistic theory together only if it follows from the nominalistic theory alone. Using symbols:

> A mathematical theory M is conservative over a nominalistic theory N if S is a consequence of M + N only if it is a consequence of N alone, S a sentence of the language of N.

It is important to note that this claim is ambiguous until the underlying notion of logical consequence is specified. Recognising this, Field (1984, 1985b, 1991) develops a modal analysis of logical consequence and related notions. We will not consider this analysis just now, for at this stage in our discussion, the ambiguous claim of conservativeness is nonetheless sufficiently clear for us to appreciate the contribution that conservativeness is supposed to make. However, we will return to Field's modal analysis of logical notions in section 6.5.

The intuitive idea behind Field's appeal to conservativeness is clear. If mathematics is conservative, then, given a mathematical theory M and a nominalistic theory N, the joint theory M + N has the same nominalistic consequences as N. So if M + N has any false nominalistic consequences, these are also consequences of N. Someone who uses mathematics to draw consequences from nominalistic theory will thus be no more prone to error than someone who does not, as it will not be possible for them to reach any falsehoods that they could not have reached without using mathematics.

To see how this is supposed to make it fictionalistically legitimate to use mathematical derivations in science, suppose that D is a mathematical derivation in M + N of a sentence which expresses a nominalistically acceptable claim ϕ. Assuming that some of the mathematical premises of D express claims that are committed to mathematical objects, a fictionalist cannot conclude that ϕ on the basis of the deduction expressed by D; this deduction is sound only if there are mathematical objects. However, if it is assumed that mathematical derivations show that the claims expressed by their conclusions are consequences of the theory expressed by their premises, a different deduction is available:

(1) D shows that ϕ is a consequence of M + N
(2) M + N is conservative over N
(3) Therefore, ϕ is a consequence of N
(4) N
(5) Therefore, ϕ.

What we have, then, is that formally correct mathematical derivations of sentences expressing nominalistic claims correspond to sound meta-theoretical deductions of those claims. This is why Field claims that it is fictionalistically legitimate to use mathematical derivations to support their nominalistic conclusions. He must therefore believe that these meta-theoretical deductions are fictionalistically acceptable. If this view is correct, then it is fictionalistically legitimate to use mathematical derivations to support nominalistic conclusions because formally correct mathematical derivations of sentences expressing nominalistic conclusion correspond to fictionalistically acceptable and sound arguments for the same conclusions.

The final aspect of Field's position concerns representation theorems (1980, 24-29). Given a mathematical and a nominalistic theory, a representation theorem proves the existence of a structure-preserving mapping from the domain of the nominalistic theory to a substructure of the domain of the mathematical theory. Such a theorem establishes a correlation between sentences of the mathematical theory and sentences of the nominalistic theory that, according to Field, allows us to take mathematical sentences as "abstract counterparts" of the correlated nominalistic sentences. Presumably he means by this that we can regard the former as standing in some way for whatever it is that the latter express about the world (Field says that "premises about the concrete can be 'translated into' abstract counterparts" (*op. cit.*,, 25)).

In any case, representation theorems are essential if we are to construct mathematical derivations of nominalistic conclusions. When a representation theorem can be proved, and only when a representation theorem can be proved, derivations in the mathematical theory of mathematical conclusions can be extended to derivations of the nominalistic sentences to which those conclusions are correlated by the representation theorem. This provides the main reason why representation theorems are important to Field's position; if there were no representation theorems, there would be no mathematical derivations of nominalistic conclusions, and thus no possible fictionalist use to be made of them. However, representation theorems are also implicated in the support Field gives for the claim that mathematics is dispensable. In his view, representation theorems, or more accurately, the uniqueness theorems that accompany them, help to show why intrinsic explanations of phenomena are desirable. This is meant to support Field's suggestion that his nominalistic theories are attractive because they provide intrinsic explanations of phenomena, which in turn is intended to bolster the claim that mathematics is dispensable. A final reason representation theorems are important is that the method Field uses to nominalistically reformulate mathematically expressed theories involves proving a representation theorem in the relevant mathematical theory.

Assuming that Field is correct to say that mathematics could be eliminated from science in the manner he describes, we are left with rather obvious question: Why should fictionalists not just do without mathematics altogether, using nominalistic formulations of theories even in the everyday practice of science? Field's answer to this is that it can be advantageous to use mathematical derivations to establish nominalistic conclusions because they are sometimes more manageable than purely nominalistic arguments for the same conclusions.

We will now summarise Field's position. Field claims that the use of mathematics in science is a useful, but in principle eliminable, tool. He argues that when a representation theorem can be proved between a mathematical and a nominalistic theory, mathematical derivations of claims from the nominalistic theory are possible which are sometimes more manageable than purely nominalistic arguments for the same conclusions. He argues that it is legitimate for a fictionalist to use mathematics derivations to support their nominalistic conclusions because mathematics is conservative. He argues that mathematics is eliminable because it is dispensable. If these arguments are all correct, they present a powerful case for viewing the use of mathematical derivations in science as an eliminable convenience.

6.4 Field's programme and the challenge from applications

In section 6.2 we saw that the applications of mathematics raise the following challenges for mathematical fictionalism: (a) to recover the descriptive use of mathematics in the absence of mathematical objects; (b) to recover the reliability of mathematical reasoning in the absence of belief in mathematical objects. What we now want to find out is whether Field's programme provides promising responses to these challenges.

We consider first the descriptive use of mathematics. Field's remark that mathematical claims can be thought of as "abstract counterparts" seems relevant to this because it allows us to say that mathematics discharges its descriptive function by providing abstract counterparts of nominalistic claims. However, Field does not say enough about the abstract counterpart relation for a clear understanding of this; he identifies which mathematical claims are to be considered the abstract counterparts of given nominalistic claims, but he does not explain what it is for a mathematical claim to be an abstract counterpart of the other. Despite its suggestive nature, therefore, Field's talk of abstract counterparts does not provide a clear response to the challenge of recovering the descriptive uses of mathematics.

However, Field does provide a clear response to the challenge of recovering the reliability of mathematical reasoning in the absence of belief in mathematical objects. Field argues that formally correct mathematical derivations with nominalistic conclusions correspond to sound, nominalistically acceptable meta-theoretical deductions of the same conclusions. As we have seen, the meta-theoretical deductions in question have the following form, where D is a formally correct mathematical derivation of a sentence expressing a nominalistic conclusion ϕ:

(1) D shows that ϕ is a consequence of M + N

(2) M + N is conservative over N

(3) Therefore, ϕ is a consequence of N

(4) N

(5) Therefore, ϕ.

If there really is a deduction of this form corresponding to every formally correct mathematical derivation of a nominalistic conclusion, and if these deductions really are nominalistically acceptable, then there are nominalistically acceptable reasons for belief in nominalistic conclusions that can be derived using mathematics. So on these assumptions, it is compatible with the rejection of mathematical objects to form belief in nominalistic conclusions that are correctly mathematically derived from true nominalistic premises. This method will be reliable because it will not permit one to form false beliefs. If mathematics is dispensable to science, all derivational uses of mathematics in science will conform to this model. Thus the reliability of all uses of mathematical reasoning will have been explained.

Field's account of the scientific use of mathematics thus provides a clear response to one of the challenges facing mathematical fictionalism, that of explaining the reliability of mathematical reasoning in the absence of belief in mathematical objects. In what remains of this chapter, we will consider whether this response is convincing.

6.5 Objections to Field's modal commitments

We noted in section 6.3 that the claim that mathematics is conservative is ambiguous until the underlying notion of logical consequence is specified. The usual definitions treat logical consequence as either a syntactic or a semantic notion. Let F be a formal system in language L, Γ a set of sentences of L and S a sentence of L. We say that S is a syntactic consequence in F of Γ when S can be derived from Γ using the rules of inference of F. We say that S is a semantic consequence of Γ in L whenever S is true in all models of Γ compatible with the semantics of L. As these definitions assume the existence of models and derivations, respectively, it is questionable whether they are nominalistically acceptable. For models are usually taken to be sets of some kind, hence as abstract objects. And whilst derivations can be treated as concrete inscriptions, it can be argued that the definition of syntactic consequence promises to be adequate only when they are treated as abstract objects (if derivations are treated as concrete inscriptions, then whether a given sentence is a syntactic consequence of another depends upon what concrete inscriptions exist).

Accordingly, Field (1985b, 1989b, 1991) puts forward an alternative, modal account of logical consequence. The account assumes a primitive, logical notion of possibility, and says that q is a modal consequence of claims $p_1, \ldots, p_n$ if and only if it is not logically possible that $\neg(p_1 \wedge \ldots \wedge p_n \rightarrow q)$.[73] Equivalently, we can say that q is a modal consequence of claims $p_1, \ldots, p_n$ if and only if it is not logically possible that $p_1 \wedge \ldots \wedge p_n \wedge \neg q$. With this approach, notions defined in terms of logical consequence such as consistency and conservativeness also come to be modally viewed. Consequently the claim that mathematics is conservative comes to be

[73] When there are infinitely many claims in the set of premises, this definition requires that the underlying logic be compact. Field does not actually call his notion the notion of "modal consequence" but using this term will help us not to confuse it with the usual syntactic and semantic notions.

viewed as a modal claim of sorts too. In this section we will consider possible reasons for thinking that this approach is objectionable.

6.5.1 Hale and Wright's objection from insularity

Hale and Wright argue at some length that Field's approach brings with it unsustainable modal commitments (Hale (1987, Chapter 5, Section II); Wright (1988); Hale (1990); Hale and Wright (1992; 1994)). In its final version, their objection depends on the regulative principle that there are no "absolutely insular contingencies", no conceptually contingent claims that both do not explanatorily depend on other conceptually contingent claims and do not have other conceptually contingent claims explanatorily depending on them (1994, 176). Call this the anti-insularity principle.[74] Hale and Wright argue that Field's position conflicts with this principle by making the claim that there are no mathematical objects both conceptually contingent and absolutely insular. For this reason they claim that Field's account of mathematics is unsatisfactory.

It will help our discussion to introduce two abbreviations. We will write "the conceptual contingency of mathematical existence" for "the conceptual contingency of the claim that there are no mathematical objects" and we will write "the absolute insularity of mathematical existence" for " the absolute insularity of the claim that there are no mathematical objects". Using these abbreviations, Hale and Wright's complaint is that Field's position is committed both to the conceptual contingency and to the absolute insularity of mathematical existence, and that this conflicts with the anti-insularity principle.

Field's initial response to this objection (1989, 43), re-iterated later on (1993, 291-292), is that it equivocates over the notion of contingency. Hale and Wright argue that Field is committed to the conceptual

[74] The term is Colyvan's (2000).

contingency of mathematical existence, where a claim is conceptually contingent if and only if both it and its negation are "not analytic in the broadest sense ... i.e. not 'true just in virtue of its meaning' " (Hale and Wright (1994, 170, *n*.4)). But Field argues that he is only committed to the logical contingency of mathematical existence (i.e. the logical contingency of the claim that there are no mathematical objects), where a claim is logically contingent if and only if neither it nor its negation is logically true (1989, 43).[75]

If Field's view of his position's commitments is correct, there is a satisfactory rejoinder to Hale and Wright's argument. As Field rightly argues, the logical notion of contingency does not appear to be a notion for which the possibility of absolutely insular contingencies should be ruled out (1993, 291). Given a claim, the fact, if it is a fact, that neither it nor its negation is logically true does not give any indication as to whether it stands in explanatory relations to other logically contingent claims. So there seems to be no reason to think that logically contingent claims cannot be absolutely insular. As stated, then, Hale and Wright's argument simply misses the point, making play with the wrong notion of contingency. But when reformulated with the right notion of contingency, the anti-insularity principle, which now reads that there are no absolutely insular logical contingencies, does not seem plausible.

Hale and Wright's argument is thus threatening only if Field's position is committed to the conceptual contingency of mathematical existence. So let us assume that this is a commitment of Field's account of mathematics. Under this assumption, Hale and Wright's charge of a conflict between the anti-insularity principle and Field's account of the nature of mathematical objects must be met. One way to do this is to argue against the anti-insularity principle. In what follows, we will consider this response.[76]

[75] Field (1993) goes on to defend the view that the existence of mathematical objects is conceptually contingent. However, he does not grant that his modal approach to logical notions commits him to defending this view.

[76] This is not the only possible response available. Having assumed that mathematical existence is conceptually contingent on Field's view the other obvious response is to grant the anti-insularity

Field argues against the anti-insularity principle as follows:

> Call something a *surdon* iff
>
> (A) its existence and state are in no way dependent on the existence and state of anything else
> (B) the existence and state of nothing else are in any way dependent on the existence and state of it.
>
> This certainly seems to be a conceptually consistent concept; but (A) and (B) guarantee insularity, so principle (3) [the anti-insularity principle] immediately guarantees the existence of surdons – indeed, the conceptual necessity of their existence. Of course, Hale and Wright accept this conclusion, since they take numbers to be surdons, but even they should baulk at the idea that establishing the existence of mathematical entities is as easy as this! Field (1993, 296-297)

Field's argument is that the anti-insularity principle implies the existence of surdons and that there is something absurd about this. So what we have here is an attempted reductio of the anti-insularity principle.

Clearly this reductio is convincing only if the existence of surdons really does follow from the anti-insularity principle. Field's reasoning for this appears to be the following argument. The definition of surdons makes clear both that it is conceptually consistent that there are surdons and that it is absolutely insular that there are surdons. From the latter claim it follows, via the anti-insularity principle, that it is conceptually necessary or conceptually impossible that there are surdons. From the former it follows that it is not conceptually impossible that there are surdons. Thus, it is conceptually necessary that there are surdons.

---

principle, which says that there are no absolutely insular conceptual contingencies, but to deny that Field account of the nature of mathematical objects is committed to the absolute insularity of mathematical existence. Colyvan (2000, 90-91) discusses this response, showing that it is initially quite plausible.

Responding to Field's attempted reductio, Hale and Wright argue that this argument is not persuasive because Field does not establish that it is conceptually consistent that there are surdons (1994, §IV). To defend this view, they distinguish between the conceptual consistency of a claim and its apparent conceptual consistency, arguing that the latter only provides defeasible evidence for the former (*op. cit.*, 181-182). Granting that it is apparently conceptually consistent that there are surdons, Hale and Wright then deny that this establishes its conceptual consistency on the grounds that it is also apparently conceptually consistent that there are no surdons (*loc. cit.*). Hale and Wright can thus claim that Field's attempted reductio of the anti-insularity principle rests on an unjustified premise.

This looks to be quite a good response to Field's attempted reductio; the only support Field puts forward for thinking that it is conceptually consistent that there are surdons is that the concept of a surdon "seems to be a conceptually consistent concept" (see quotation above), so a slide from "seeming" or "apparent" conceptual consistency to conceptual consistency outright does occur. Nevertheless, MacBride (1999) argues that the response is not good. He claims that Hale and Wright's notion of apparent conceptual consistency is the same as Field's notion of logical consistency (*op. cit.*, 445) and therefore not governed by the anti-insularity principle (*op. cit.*, 444).[77] Seemingly, then, MacBride thinks that Hale and Wright's response to Field's reductio is unstable, permitting the defender of Field's position to revert to the earlier charge of equivocation over the notion of contingency.

However, Hale and Wright's notion of apparent conceptual consistency cannot be identified with Field's notion of logical consistency. Hale and Wright argue that the apparent conceptual consistency neither of the claim that there are surdons nor of the claim that there are no surdons provides evidence for conceptual consistency. With these claims, they say, "there is a stand-off between our two impressions of consistency" (1994, 181-

[77] Of course, it is really the relevant related notion of contingency that is not governed by the anti-insularity principle, but MacBride's simplification is convenient.

182). Hale and Wright also make plain that a claim is apparently conceptually consistent if it is "readily intelligible and, as far as one can see, coherent" (*op. cit.*, 181). These remarks strongly suggest that according to Hale and Wright's notion of apparent conceptual consistency, a claim is apparently conceptually consistency if and only if strikes us as being conceptually consistent. Apparent conceptual consistency is therefore something phenomenal, not a genuine notion of consistency at all. In contrast, Field's logical notion of consistency, according to which a claim is logically consistent if and only if neither it, nor its negation, is logically true, is a genuine notion of consistency. Thus the two notions cannot be identified.

There is an important lesson to be drawn from this. Having recognised the phenomenological character of Hale and Wright's notion of apparent conceptual consistency, one must concede their point that apparent conceptual consistency provides only defeasible evidence for conceptual consistency; conceptually inconsistent things can presumably sometimes seem to be conceptually consistent. Hale and Wright's response to Field's attempted reductio of the anti-insularity principle thus appears to be effective, and so, at this stage, we have no reason to reject the anti-insularity principle. So far then, the strategy of arguing against the anti-insularity principle has not produced a satisfactory reply to Hale and Wright's original objection. Nevertheless, as we shall now proceed to argue, there is enough doubt over whether the principle should be accepted to render Hale and Wright's objection unconvincing.

To make our case we will change focus slightly. Instead of looking for reasons to dismiss Hale and Wright's anti-insularity principle, we will look for reasons to think there is an equally attractive alternative to it. Field himself suggests an alternative anti-insularity principle, which states that we should not assume the existence of absolutely insular things without very good reason (the principle assumes that something is absolutely insular if it fulfils the conditions for being a surdon given in the quotation above) (1993, 297). Field clearly thinks this principle is preferable to Hale and Wright's because it appears not to be committed to the existence of surdons. But from our preceding discussion, we must conclude with Hale and Wright that their principle is not committed to surdons either, so we cannot accept this view. However, we can claim that Field's principle is as initially plausible as Hale and Wright's as a way of making more precise the intuition underlying their rejection of absolute insularities. And in fact this is all we need to establish in order to nullify their objection.[78]

To see that this is so, note that Hale and Wright do not claim to have established the truth of their anti-insularity principle but only to have established that it is attractive. Their sole reason for this appears to be that it offers a more refined version of "the idea that the realm of contingency forms a single integrated system – a tree-like structure in which every node is linked to the others" (1992, 134). This point emerges again later when they suggest that denying the principle "jars with an exceedingly natural and plausible conception of the realm of contingency as forming a single integrated system" (1994, 176). One might very well think there is something right in these vague remarks about contingencies all hanging together in one big system. But because of their vagueness, it seems perfectly justifiable to claim that Field's anti-insularity principle is as attractive a refinement of them as Hale and Wright's. For all Hale and Wright have said, we have no grounds upon which to reject Field's anti-insularity principle in favour of their own.

Despite the fact that we have found no firm reason to reject Hale and Wright's anti-insularity principle, therefore, it appears there is a satisfactory rejoinder to their objection to Field's position. Field's anti-insularity

[78] Colyvan (2000, 90) argues that Field's anti-insularity principle is preferable to Hale and Wright's since it is consistent with contingent nominalism (defined as the view that mathematical existence is conceptually contingent and there are no mathematical objects) and contingent Platonism (defined as the view that mathematical existence is conceptually contingent and there are mathematical objects) whereas Hale and Wright's is not (being, at least apparently, inconsistent with contingent nominalism). If this is correct, it provides a satisfactory response to Hale and Wright's objection. However, the fact that the two principles differ in this way provides a reason to prefer Field's principle to Hale and Wright's only if it is assumed that that the correct anti-insularity principle, whatever it may be, should not influence the decision between contingent nominalism and contingent Platonism. But it is hard to see why this assumption should be accepted. Certainly Colyvan gives no adequate reason for thinking it should.

principle seems to enjoy as much support as Hale and Wright's, so the latter cannot be preferred to the former. It is thus not clear that the conflict between Hale and Wright's anti-insularity principle and Field's (assumed) commitment to the conceptual contingency of mathematical existence is objectionable. Hale and Wright's objection to Field's account of mathematics is therefore unconvincing.

6.5.2 Knowledge of the modal conservativeness of mathematics

Field's appeal to the notion of modal consequence affects the content of his claim that mathematics is conservative. Let us say that a mathematical theory M is modally conservative over a nominalistic theory N whenever it is the case that:

> ϕ is a modal consequence of M + N only if ϕ is a modal consequence of N alone, ϕ a claim expressible in the language of N.

Field's assumption that mathematics is modally conservative requires, at the very least, that whenever N is a nominalistically acceptable reformulation of a scientific theory T using mathematical theory M, there are nominalistically acceptable grounds for thinking that M is modally conservative over N. Let us investigate whether it seems likely that there are such grounds.[79]

Given Field's remark that "neither I nor anyone else that I know of has a great deal to say about the epistemology of modal claims" (1985b, 140), it may seem as if he has nothing useful to say about this. But the original

[79] One reason to think there are no nominalistically acceptable grounds for the relevant claims of modal conservativeness would be if they involved reference to or quantification over mathematical objects. MacBride (1999) suggests that this could be a difficulty because modal claims are ordinarily taken to be claims about possible worlds. However, this is not a telling complaint as the notion of possibility Field introduces is primitive, that is, neither definable nor explicable in terms of more basic notions, and so it cannot be said to be about possible worlds.

exposition of Field (1980) contains three putative sources of grounds to think that mathematics is conservative. Perhaps these might supply nominalistically acceptable grounds for belief that mathematics is modally conservative.[80]

Field's first suggestion is that if mathematics is conservative, that would help to explain why the view that mathematics is true in all possible worlds and the view that mathematics is apriori have been so popular (*op. cit.*, 12-13).[81] The idea here seems to be that since these views both imply that mathematics is conservative, we can take their popularity as evidence that philosophers who incline towards them are dimly sensing that mathematics is conservative. But whilst we could perhaps take this approach, it surely doubtful that we have to take it, that it provides the only, or best, explanation of the popularity of these views. So this does not provide a firm reason to think that mathematics is modally conservative.

The second suggestion is that we have the same kind of quasi-inductive grounds for the conservativeness of mathematics as we do for the consistency of mathematics (*op. cit.*, 14). These are supposed to have something to do with the fact that we would be very surprised to find that mathematics is not conservative, and the fact that we would revise our mathematics if we found counter-examples to its conservativeness (*op. cit.*, 13). But these points do not establish that mathematics is conservative. If we were surprised to find that mathematics is not conservative, that would only show that we believe it to be conservative, not that we have good grounds for this belief. And the mere fact that we

[80] At this early stage it is not the modal conservativeness of mathematics that is at stake, since Field does not introduce this notion until his (1984). Rather, as Field makes clear at (1980, Chapter 4, *n*.30, 115), it is the semantic notion that is in play (i.e. conservativeness defined in terms of semantic consequence). Nevertheless, we can still ask whether what Field has to say in favour of the semantic conservativeness of mathematics provides nominalistically acceptable grounds for belief in its modal conservativeness.

[81] In the early work, it is the semantic notion of conservativeness that is at stake. In later work, he identifies belief in the semantic conservativeness of mathematics with a modal belief (1985b, 139). Still later, the notions are not identified but it is claimed that fictionalists can use mathematics to find out about modal consistency (1991, 13).

would eliminate counter-examples to the modal conservativeness of mathematics if we came upon them clearly provides no guarantee that mathematics is, as a matter of fact, modally conservative.

The third and last suggestion for grounds for the view that mathematics is conservative comes in the form of two alleged proofs of the conservativeness of certain mathematical theories (*op. cit.*, 16-19). One of these is an argument in model theory, the other an argument in proof theory. Obviously, these arguments prove that the theories in question are conservative only if theories in which they are carried out are assumed to be true. This assumption is not nominalistically acceptable, however, as model theory and proof theory are branches of applied set theory. Thus, these arguments cannot be taken as nominalistically acceptable grounds for the modal conservativeness of mathematics.

At this stage, then, it appears that Field has not produced adequate, nominalistically acceptable grounds for belief in the modal conservativeness of mathematics. However, Field (1991) argues that it is nominalistically acceptable to use model and proof theoretic arguments to support modal claims without taking them as genuine proofs. If he is right about this, then his model and proof theoretic "proofs" of the conservativeness of mathematics may yet provide nominalistically acceptable grounds for belief in the modal conservativeness of mathematics, just not in the same way that genuine proofs would.

The approach here is of a piece with Field's general strategy of arguing that it is fictionalistically legitimate to use mathematical reasoning to establish nominalistically acceptable conclusions. This time, however, his more specific claim is that model and proof theoretic reasoning helps to construct nominalistically acceptable arguments for modal conclusions. If the mathematical reasoning in question is model theoretic, the modal argument it will allow us to construct will rely on a premise of the form:

(1) $\Box\,(S \supset \text{there is a model of } A) \supset \Diamond A$

where "S" stands for some "reasonable finitely axiomatised set theory" such as NBG (*op. cit.*, 12-13). In our terminology, this says that, if it is a modal consequence of set theory that there is a model of A, then A is possibly true. If the mathematical reasoning in question is proof theoretic, the corresponding modal argument will rely on a premise of the form:

$$(2) \qquad \Box\,(S \supset \text{there is a refutation in F of A}) \supset \neg \Diamond A.$$

where "F" stands for one of the usual formalizations of first order logic (*op. cit.*, 12). For us, this says that, if it is a modal consequence of set theory that there is a refutation of A in first order logic, then A is not possibly true.

Obviously modal arguments from these premises provide grounds to believe their conclusions only if there are grounds to believe their premises. Field's main claim concerning the reasons there might be for believing (1) and (2) is a comparative one; that such reasons will always require weaker assumptions than the reasons that a mathematical realist might put forward for belief in certain analogues of (1) and (2). However, to justify this Field suggests that a nominalist could argue for (1) and (2) from prior knowledge of the modal consistency of set theory, i.e. prior knowledge that it is possible for the claims of set theory to be jointly true (op. cit., 14-17).[82] This raises the question of whether there are nominalistically acceptable grounds for belief in the modal consistency of set theory.

Without claiming to have a conclusive argument, we will put forward considerations to suggest that there are not. It is often said that knowledge of what is possibly true is grounded on knowledge of what is actually true. So let us consider first whether a nominalist could appeal to the actual truth of a theory in order to argue for the possible truth of set theory. What we would be looking for here would be a theory T such that

[82] In the case of (2), Field actually suggests that prior knowledge of claims of the form $\Diamond (S \wedge I)$, where I is an instance of a schema he has previously discussed, would suffice (1991, 16). However, all such assumptions imply that $\Diamond$ S.

the nominalist could know both T and $AX_T \rightarrow \Diamond AX_S$.[83] Clearly a nominalist cannot appeal to the actual truth of any mathematical theory, so T will have to be empirical. However, for $AX_T \rightarrow \Diamond AX_S$ to be true, T will presumably have to entail the existence of a collection of objects exhibiting the structure of the cumulative hierarchy of sets. As these would have to be concrete objects, it is unclear that the nominalist could have adequate grounds for belief in T.

This suggests it may not be possible for a nominalist to ground knowledge of the possible truth of set theory on knowledge of the actual truth of some other theory. So let us now consider whether knowledge of the possible truth of set theory could be grounded on knowledge of the *possible* truth of some other theory. What we would be looking for here would be a theory T such that a nominalist could know both $\Diamond T$ and the conditional $\Diamond AX_T \supset \Diamond AX_S$. Presumably this conditional, too, could be true only if T entailed the existence of a collection of objects exhibiting the structure of the cumulative hierarchy of sets. Thus, as it would be useless to appeal to the possible truth of a mathematical theory other than set theory, T would have to be an empirical theory whose possible truth entailed the possible existence of a collection of concrete objects exhibiting the structure of the sets.

But on what grounds could the nominalist know the possible truth of such a theory T? Appeal to the actual truth of T would take us back to the failed first strategy of grounding the possible truth of set theory on knowledge of actuality. So other grounds will have to be found. Seemingly the only option here is to appeal to conceivability considerations, arguing that our ability to imagine or describe possible situations allows us to find out about what is possibly true (see, e.g., Yablo (1993)). This position has long attracted criticism on the basis that metaphysical conclusions cannot be deduced from psychological premises.[84] But even prescinding from general worries of this kind, it is questionable that conceivability

[83] Here "AX_T" and "AX_S" are the conjunction of the axioms of T and S, respectively.
[84] As we saw in section 3.6.1, this objection can be found at least as far back as Mill.

considerations could support the specific kind of knowledge required here, of the possible existence of a concrete instantiation of an infinite mathematical structure. As Hale (1996) suggests, such knowledge would seemingly have to be based on an ability to describe a situation which is both obviously possible and of which we should say that it contained a concrete instantiation of the structure in question (*op. cit.*, 139). Given that any description would have to be both acceptable to the nominalist and finite (given our obvious limitations) it does seem unclear, as Hale goes on to argue, that the available descriptions could rule out there being no concrete instantiation of the structure in question (*op. cit.*, 145).

We should stress again that these remarks are not intended conclusively to show that there are no nominalistically acceptable grounds for belief in the modal consistency of set theory. But they do at least suggest that it is not clear where such grounds are to be found. Field thus provides no reason to think there are nominalistically acceptable grounds for thinking that set theory is possibly true. Because of this there seems to be no prospect of a satisfactory modal argument for the modal conservativeness of mathematics based on model or proof theoretic considerations. Field's claim that mathematics is modally conservative thus lacks adequate justification.

6.6 Shapiro's dilemma

Shapiro (1983) thinks that Field should have appealed to the syntactic notion of conservativeness in his explanation of the applications of mathematics. Moreover, he argues that this choice would have lead to a dilemma. If the underlying logic is second-order, then mathematics will not be syntactically conservative. But if the underlying logic is first-order, then if the mathematics is syntactically conservative it will not be possible to prove the representation theorems Field invokes. Since both conservativeness and representation theorems are required in Field's account of applications, this means the position is unstable.

To assess this objection, it helps to note that Field eventually settles on a background of first order logic. In his original exposition, he prefers second-order formulations of scientific theories, arguing that these are nominalistically acceptable on the grounds that the second-order variables can be interpreted over mereological sums of space-time points (1980, Chapter 4). Later, however, Field argues that first-order formulations should be preferred on the grounds that logic should not make existential claims (1985b, 141). This seems to be the right approach. When the formulations are second order, the mereological sums of space-time points whose existence is entailed appear as ontological commitments of the logic rather than the theories formulated in the logic; but logic should not have ontological commitments like this.

To defend Field's position against Shapiro's objection it is thus necessary to address that part of it which deals with first order logic. Shapiro's central point here is that if mathematics is syntactically conservative the representation theorems Field invokes cannot be proved.[85] The argument for this relies on the Gödel's incompleteness theorems. A somewhat

[85] Shapiro in fact argues that Field's model theoretic proof of the semantic conservativeness of mathematics establishes, in the first order context, its syntactic conservativeness as well. He then shows that this leads to the difficulty described in the text. However, the force of his objection clearly depends only on establishing the conditional that we called the "central point". For this reason we treat syntactic conservativeness as an assumption of Shapiro's argument.

simplified version of it runs as follows.[86] Assume that mathematics is syntactically conservative. Consider a nominalistic formulation N of Peano Arithmetic (recall that Field's space-time contains uncountably many concrete points, so that models for the natural numbers are guaranteed). Let M be some standard set theory equipped to apply to N in the manner described by Field. In this situation, sentences expressing the consistency of N in the manner exhibited in the proof of Gödel's theorems, call these Gödel sentences, may be formulated in both the language of N and the language of M. Moreover, if a representation theorem linking M + N to N can be proved, then such sentences are all provably equivalent in the mathematical theory M + N, regardless of in which language they are expressed. Now we know that Gödel sentences for N in the language of M + N can be proved in M + N, because standard set theory proves the consistency of Peano arithmetic. Thus, if M is syntactically conservative over N, it follows that Gödel sentences for N in the language of N can be proved in N. But this is a contradiction, as (an analogue of) Gödel's first incompleteness theorem tells us that N does not prove its own consistency. As the only way to avoid the contradiction is to deny that the relevant representation theorem can be proved, we have that, in the first order case, if mathematics is syntactically conservative, the representation theorems Field invokes cannot be proved.

How might the defender of Field's position respond to this argument? Observing that the argument assumes the syntactic notion of conservativeness, it may seem tempting to argue that Field's appeal to his modal notion of conservativeness renders the argument irrelevant. However, this conclusion cannot be sustained. From the realist point of view, both the following principles are to be accepted as true:

(i) If ϕ is a syntactic consequence of set theory (relative to a standard formalization), then ϕ is a modal consequence of set theory

[86] See Shapiro (1983, 232) for his version.

(ii) If ϕ is a modal consequence of set theory, then ϕ is a semantic consequence of set theory[87]

If we now assume a background of first order logic, the fact that first order logic is both sound and complete shows that the notions of syntactic and semantic consequence are co-extensive (assuming a reasonable formalization of the logic). But (i) and (ii) indicate that the modal notion lies between these two, extensionally speaking. Thus all three notions are co-extensive. The fact that Shapiro's argument about representation theorems assumes the syntactic notion of conservativeness thus does not mean that his objection can be discarded as irrelevant. For since syntactic conservativeness is co-extensive with modal conservativeness, Shapiro's argument can be reformulated with respect to the modal notion without any loss of plausibility.[88]

Shapiro's argument must therefore be met head on. This raises the question of whether Field's account of mathematical applications can do without representation theorems. Clearly they cannot be abandoned altogether, as without representation theorems it is not possible to think of mathematical sentences as abstract counterparts of nominalistic ones, which idea is crucial to explaining the nominalistic usefulness of

[87] To see why principle (i) holds suppose that we have deduced in proof theory that ϕ is a syntactic consequence of set theory. By definition, this implies that there is a derivation of ϕ from the axioms of set theory. But then there is a derivation of $AX_S \rightarrow \phi$ that has no premises, where "AX_S" is the conjunction of the axioms of set theory. The necessitation rule of modal logic thus yields $(AX_S \rightarrow \phi)$, i.e. ϕ is a modal consequence of set theory. So from the perspective of mathematical realism, if ϕ is a syntactic consequence of set theory, ϕ is a modal consequence of set theory..

To see why principle (ii) holds, suppose now that we have somehow established that ϕ is a modal consequence of set theory, i.e. that $(AX_S \rightarrow \phi)$. Let M be a model of set theory. Since $(AX_S \rightarrow \phi)$ is true, $AX_S \rightarrow \phi$ is true in all interpretations and so must be true in M. Since M is a model of set theory, the axioms of set theory are all true in M, so ϕ must also be true in M. ϕ is therefore true in all models of set theory, i.e. ϕ is a semantic consequence of set theory. So from the perspective of mathematical realism, if ϕ is a modal consequence of set theory, ϕ is a semantic consequence of set theory.

[88] For economy we speak here as if the notions of syntactic, semantic and modal consequence relate the same kind of things. This is not really correct, when we say that ϕ is a syntactic consequence of set theory, "ϕ" must be taken for a sentence and "set theory" for a set of sentences, but when we say that ϕ is a modal consequence of set theory, "ϕ" should be taken for what a sentence expresses and "set theory" for what the sentences of set theory collectively express. This manner of speaking is not objectionable however, as the main thrust of the argument could be expressed more long-windedly without it.

mathematical reasoning. However, it may be possible to get by with representation theorems that do not establish such a close relationship between the claims of mathematical and nominalistic theories, in particular that do not supply "concrete counterparts" of every mathematical sentence. CON_{PA} (the usual Gödel sentence for PA) is not applied in mathematical science. So if a representation theorem could be proved that provided a concrete counterpart for every mathematical sentence used in applications but which did not supply a counterpart for CON_{PA}, then we could invoke that theorem to explain the nominalistic usefulness of mathematical reasoning without contradicting Gödel's theorems. More generally, if a representation theorem can be proved that establishes an abstract counterpart relation strong enough to justify the use of every applied mathematical sentence but not so strong as to demand that problematic sentences like CON_{PA} are applied, then Field's appeal to representation theorems would be defensible.

Field (1985b) ponders this idea at some length. He points out that a representation theorem can be proved linking the first-order formulation of N to a mathematical theory (M + N)' which does not attribute to space-time the structure of a four dimensional Gallilean space defined over the real numbers. Rather, it attributes to space-time the structure of a four dimensional Gallilean space defined over a field similar to the real numbers but for which the least upper bound axiom is restricted to sets of reals definable in a specified expansion of the first-order theory of quadruples of real numbers, see Field (*op. cit.*, 134, *n*.10). Field points out that (M + N)' does not imply CON_{PA}. Moreover, he argues that (M + N)' is scientifically preferable to M + N because there is no direct observational support for the extra nominalistic content of M + N, as represented by the non-mathematical claims that it implies but that (M + N)' does not. Thus, Field concludes, the representation theorem linking (M + N)' to N is sufficient for his needs.

However this response is not convincing. We would have little reason to believe (M + N)' unless M + N were true. Furthermore, Field's new representation theorem does not show the usefulness of applying the theory of real numbers to N, but only the usefulness of applying the theory of an isomorphism class of fields rather like the reals but different from it in certain regards. But we use the real numbers in science, not these other fields; most scientists probably would not even know what these other fields were. Thus, even if Field's preferred representation theorems do establish that it is useful to make use of this other mathematics in carrying out nominalistic science, this would leave us without an explanation of the usefulness of the mathematics scientists actually use.

This response to Shapiro's argument thus does not seem convincing. Moreover, there appears to be no alternative response. Shapiro's observation that the original representation theorems are not forthcoming when the background logic is first order thus presents a telling objection to Field's claim to have established why it is useful to use standard mathematics instrumentally in nominalistic science.

6.7 Objections to Field's theory of space-time

If the representation theorems Field relies upon are to be proved, then physical space-time must be very rich. The mappings required by the representation theorems will only exist if there are as many space-time points as there elements of R^4, so there must be uncountably many space-time points if the theorems are to be proved. Moreover, the mappings required will exist only if regions of space-time (mereological sums of space-time points) exhibit a similar structure to certain sets of elements of R^4, so space-time must be endowed with significant mathematical structure. Some critics feel that by assuming such a rich space-time Field must forfeit his claim to have shown how to nominalize theories of mathematical science. For instance, Resnik (1985a) argues that the space-time points Field appeals to are no more nominalistically acceptable than mathematical objects, whilst Resnik (1985b) argues that a space-time with mathematical structure is itself nominalistically unacceptable. In what follows, we will consider objections of this kind, focusing on issues concerning space-time points.[89]

Resnik claims against the nominalistic acceptability of space-time points that they should be regarded as abstract, or at least, as more like paradigmatic mathematical objects, metaphysically speaking, than paradigmatic concrete objects.[90] To support this he points to disanalogies between the ways such objects enter our physical reasoning:

> The explanatory, historical and evidential place of space-time points in physics is much closer to that of standard mathematical objects than it is to that of standard physical objects. In contrast to the case of electrons, forces or planets, no particular body of observable phenomena led to the

[89] Though we should note that Field at least addresses the second point; he argues that on his view space-time displays characteristics such as causal efficacy (of space-time regions) that cannot be understood as arising from mathematical structure (see, e.g., (1980, *op. cit.*, 31-33)).
[90] He also raises an epistemological point, namely, that it is unclear that knowledge of space-time points will be more easily accounted for than knowledge of mathematical objects. However, we do not consider this in the text.

> introduction or discovery of space-time points. We postulate space-time points not to explain how or why something happens, but rather to structure or organize happenings. (1985a, 167)

For Resnik, then, the roles played in physics by space-time points suggest metaphysical kinship with mathematical rather than concrete objects. Field (1985a) responds to this by arguing that space-time points are to be regarded as concrete as by so doing we achieve an adequate account of field theories such as classical electromagnetism. Some of what he has to say about field theories concerns whether it is possible to construe them without reference to space-time points. But note that this, alone, cannot show that space-time points are concrete. As Field's ultimate aim is to demonstrate that mathematical objects are dispensable by providing nominalistic reformulations of mathematical scientific theories, he cannot respond to the objection that space-time points are abstract simply by inferring from their indispensability to their concreteness. For this would be equivalent to assuming that that which is abstract is dispensable, and so would be question-begging.

Field's view must therefore be that field theories imply claims about space-time points that indicate that they can be considered to be concrete objects, or at least, sufficiently like concrete objects to be unobjectionable from a nominalistic perspective. And indeed, this is what he thinks. Field believes that on our best understanding of field theories, we are not only committed to the existence of space-time points, we are also committed to attributing causal properties to them, properties such as electromagnetic field intensities and the values of gravitational tensors (1982, 70-72). To combat the impression that the objects of which we predicate such properties are actually the pieces of matter occupying space-time points, he elsewhere stresses (1985a, 181) that we make such attributions to space-time points whether they are occupied or not. Thus, in Field's view, it is appropriate to think of space-time points as concrete things (or at least, as not on a metaphysical par with mathematical objects).

It seems clear that if Field is correct about how we ought to understand field theories, then he has the resources to meet Resnik's objection. For in this case, it is simply not true that space-time points do not play the role in explanations of happenings that more familiar concrete objects play. So does this mean that space-time points are nominalistically acceptable after all? We cannot say. As we are not in a position to decide what is the best way of interpreting field theories, we are unable to draw a firm conclusion concerning the nature of space-time points.

However, this does not leave us without a firm objection to Field's use of space-time points. The debate between Resnik and Field over the nature of space-time concerns whether, if space-time points exist, they should be considered nominalistically acceptable. But Field requires not just that this conditional be true, but also that its antecedent be fulfilled. That is, he requires that space-time points exist. And as we pointed out above, he requires that they exist in uncountable multitudes. So where is the evidence that they do?

One might expect that it is to come from the very same field theories that are supposed to show that space-time points are concrete. However, the existence of uncountably many space-time points follows from those theories only if they treat space-time as a continuous manifold. And although this is assumed in the theories, it appears that the assumption is made for instrumental purposes. Thus, in response to the way our mathematical description of gravity breaks down at quantum distances, Richard Feynman writes:

> I believe that the theory that space is continuous is wrong, because we get these infinities and other difficulties, and we are left with questions on what determines the size of all the particles. I rather suspect that the simple ideas of geometry, extended down into infinitely small space, are wrong. (1965, 166-167)

More recently, Chris Isham writes:

> In general terms it is clear that the major interest in quantum gravity will always tend to focus on problems like the 'big bang' and the profound implications of the fundamentally different conceptual structures of quantum theory and general relativity. What is unclear is how much, if anything, will be left of these structures at the end of the day. The idea of a spacetime 'continuum' seems particularly vulnerable, and many attempts have been made to suggest ways of dispensing with this concept at a sub-microscopic level. Some of these (e.g. quantum topology) have been mentioned already, but there are many others. Perhaps spacetime is really a fractal (a mathematical space with a fractional number of dimensions), or a lattice, perhaps modelled on a finite number field; or perhaps, as in Penrose's twistor theory, there are no spacetime points as such, but rather systems of lines whose intersections are equivalent to points in the classical theory but which fail to intersect at all when subject to quantum fluctuations. (1989, 93)

The difficulties alluded to in these quotations remain with us today; we still lack a satisfactory resolution of the tensions between the "fundamentally different conceptual structures of quantum theory and general relativity". Whether space-time is continuous, or whether, at the incredibly tiny scales at which quantum effects are significant, the assumption of continuity cannot be maintained, must therefore be viewed as an open question.

It follows that the assumption of uncountably many space-time points in science must itself be instrumental, part of an idealization, a claim made for convenience that need not be true. The scientific evidence, it seems, does not show that there are uncountably many space-time points. Thus, since Field's method of nominalization depends on the assumption that uncountably many space-time points exist, it cannot, given the present state of scientific knowledge, be viewed as correct.

6.8 Conclusion

We will now collect together the conclusions we have reached about Field's programme for the nominalization of science, and assess their significance for the prospects of mathematical fictionalism.

We first considered objections to the modal commitments of Field's theory (section 6.5). We dismissed Hale and Wright's argument that Field's logical notion of possibility should be rejected (section 6.5.1) but argued that it is unclear there are nominalistically acceptable grounds for knowledge of the modal consistency of standard mathematical theories (section 6.5.2). If this is correct, it is not clear what supports the claim that mathematics is conservative. We then considered and endorsed Shapiro's argument that, when Field's programme is pursed against the background of first order logic, if mathematics is conservative, the required representation theorems cannot be proved (section 6.6). If this is correct, then the model of applications Field puts forward cannot be invoked. Finally, we considered Resnik's arguments against the nominalistic acceptability of Field's reformulations of field theories in flat space-time, arguing that there is not adequate evidence for the view that there are uncountably many concrete space-time points (section 6.7). If this is correct, Field's claim that these theories can be nominalistically reformulated cannot be accepted. It thus appears that there are serious difficulties with every aspect of Field's position.

Field's programme for the nominalization of science thus does not reassure us that mathematical fictionalism can meet the challenge from applications. We saw that this had two parts: (a) to recover the descriptive uses of mathematics in the absence of mathematical objects and (b) to recover the reliability of mathematical reasoning in the absence of belief in mathematical objects (section 6.2). As we pointed out, Field's theory did not promise to address the first of these but did promise to address the second, by arguing that mathematical deductions of nominalistically acceptable conclusions correspond to nominalistically acceptable deductions of the same conclusions (section 6.4). Given the objections recently described, this attempted explanation of the reliability of mathematical reasoning cannot be regarded as satisfactory. Our discussion of Field's programme thus leaves us without an adequate response to either aspect of the challenge from applications, and leaves us with the impression that the fictionalist strategy it represents, that of arguing that mathematics can be eliminated from science, is ill conceived.

7

Fictionalism and pretence

The conclusions of the previous chapter suggest that science cannot be done without mathematics. Given our assumption of linguistic realism, we cannot eliminate mathematics from science by rewriting mathematics. Given the failure of Field's programme, we lack a method for extracting mathematics from science by rewriting science. Consequently, it does not seem possible for mathematical fictionalism to meet the challenge from applications by arguing that mathematics is dispensable.

The question of whether mathematical fictionalism can meet the challenge thus becomes the question of whether it can do so without claiming that mathematics is dispensable to science. This brings us to the other fictionalist strategy we identified for meeting the challenge, namely, to argue that the roles mathematics has in science are played in other contexts by acknowledged forms of pretence. This does not appear to involve a commitment as to whether mathematics is dispensable to science. Our question in this chapter is thus whether it is possible, on this approach, to provide a satisfactory account of applications.

7.1 Prescriptive fictionalism

Prescriptive fictionalism takes its cue from the analogy between mathematics and fiction drawn in section 6.1. When we use language to tell a story, and when we engage in literary criticism, we make claims that are true only if there are fictional objects. On a plausible account of literary fiction, our listeners play along with these claims, imagining or pretending them to be true, but not believing them. The analogy maintains that mathematics is comparable to literary fiction in this regard. In doing mathematics, we put forward claims that are true only if there are mathematical objects, we imagine or pretend that these object-committed mathematical claims are true, but we do not thereby commit ourselves to their truth. Consequently, we are no more obliged to believe in mathematical objects than we are to believe in fictional objects.

This analogy could be intended descriptively, as being true to our actual use of mathematics, or prescriptively, as an indication of how we ought to use mathematics. As the question of how people actually engage with mathematics is an empirical one, to be addressed by empirical research, the descriptive approach does not seem appropriate for a philosophical study of this kind. Accordingly, we will take the prescriptive approach, using the analogy to guide construction of a prescriptive conception of mathematics as a kind of pretence.

If this approach is to be defensible, the conception of mathematics proposed must represent mathematics as an area of enquiry, that is, as a discipline in which claims are evaluated relative to prevailing standards. Drawing on the analogy between mathematics and literary fiction, we will set out a picture of mathematics as an area of enquiry in the next section. In the following section we will return to the challenge to fictionalism from applications, building from what has gone before to show how mathematics, conceived as a kind of pretence, can discharge the roles it has in applications.

7.2 Mathematics as an area of enquiry

7.2.1 Acceptance

In an area of enquiry, we evaluate claims relative to certain standards, aiming to accept only those claims that satisfy them. The notion of acceptance involved here is technical, due to Van Fraassen (1980). It can be explained as the state of mind that is to be adopted towards a claim governed by standards when it has been shown to meet those standards. When introduced this way, there is a substantive question as to what state of mind constitutes acceptance relative to the standards of a given area of enquiry. If prescriptive fictionalism is to represent mathematics as an area of enquiry, it must therefore provide an explanation of what state of mind constitutes acceptance of mathematical claims. This state of mind must not involve belief in mathematical objects, since otherwise it will not be possible to think of mathematics as a kind of pretence.

It may perhaps be thought that prescriptive fictionalism could just state that the acceptance of mathematical claims does not require belief in mathematical objects, and leave it at that. However, this would not be sufficient. What must be shown is that there is a state of mind which, when taken as mathematical acceptance, gives the right dispositions to utterance but does not involve belief in mathematical objects. Simply stating that no belief in mathematical objects is involved does not demonstrate that there is such a state of mind, so a positive characterisation of what constitutes mathematical acceptance is required.

As we are working towards a conception of mathematics as pretence, it may seem appropriate to take acceptance in mathematics to be constituted by pretence, i.e. to view the acceptance of a mathematical claim *p* as pretence that *p*. But this would be inadequate. When the notion of acceptance is explained as above, it has a standing character: a subject who once accepts that *p* remains in the state of accepting that *p* unless they suffer a lapse of memory or come to suspect that their reasons for accepting *p* were mistaken or insufficient. In contrast, a subject pretends that *p* only whilst they act as if they believe that *p* (whilst not believing that *p*). Since acting as if we believe that *p* is something we do only intermittently, for example, whilst uttering a sentence that expresses *p*, it follows that pretence does not have the requisite standing character. Thus acceptance in mathematics cannot be constituted by pretence.

Having recognised this, it is natural to wonder whether mathematical acceptance could be constituted by dispositions to pretend; this would preserve a link between acceptance and pretence whilst invoking states of mind with the standing character of beliefs. But it appears that someone could be disposed to pretend that *p*, for a given mathematical claim *p*, and yet not actually accept it, in the relevant sense. A schoolboy intending to mislead his classmates over what mathematical claims he accepts might be disposed to pretend that *p* even though he did not himself accept it. An actor whose lines are supposed to give the impression that his character accepts *p* would be disposed to pretend that *p* even though he did not himself accept it. So prescriptive fictionalism cannot identify acceptance of a mathematical claim *p* with the disposition to pretend it.

Nevertheless, the idea of shifting to dispositions, or at least to dispositional states, is a good one. This becomes clear if we think about what happens when we engage with literary fiction. On first reading Hemingway's novel *The Old Man and The Sea*, one encounters many claims about Santiago and his struggle with the fish. Provided one realises one is reading fiction, one does not believe these claims and so one does not become prepared to make or endorse the statement that they are true. Nevertheless one does become prepared to use these claims on certain occasions as if one believed them to be true; for example, one becomes prepared to use them in order to tell the story of *The Old Man and the Sea*. The analogy between mathematics and literary fiction thus suggests taking mathematical acceptance to be a similar state of preparedness, a state of being ready to make use of mathematical claims in certain contexts as if believing them to be true, whilst not being prepared to go so far as to assert or endorse that they are true.

This proposal is on the right lines, but it does not adequately characterise a potential candidate for mathematical acceptance unless accompanied by a specification of the relevant contexts of use. Since it has been germane to our discussions for some time now that mathematics has empirical applications, the obvious ploy here is to take these as the relevant contexts. Accordingly we can propose that, for an object-committed mathematical claim *p*:

> A accepts *p* if and only if (i) A is prepared to use *p* in empirical applications and (ii) A is not prepared to assert or endorse the statement that *p* is true.

Note that to be in this state of mind with respect to a given claim *p* does not require that one believe that *p*. Note, too, that it has the standing character of belief; once having become prepared to act this way with *p*, one will remain so prepared unless one has a lapse of memory or comes to believe that one's original motivation for becoming so prepared was faulty or insufficient. It thus satisfies both constraints we have identified for candidates for mathematical acceptance.

Let us now consider what it is to reject a mathematical claim. Any constitutive account of mathematical acceptance must address this, for when we find that a mathematical claim conflicts with the standards governing mathematical acceptance, we should reject it. In mathematics, at least, we can assume that when a claim may be accepted, its negation may be rejected (and vice versa). Accordingly, prescriptive fictionalism can simply take rejection of *p* as being constituted by acceptance of the negation of *p*. Someone rejects *p* when they are prepared to use not-*p*, or propositions deduced from it, just as if it were true, but are not prepared to assert or endorse the statement that not-*p* is true.

7.2.2 Standards of acceptability

If mathematics is to be viewed as an area of enquiry, the emergence of standards governing acceptance must be explained. It can seem as if comparisons with literary fiction are of no assistance here. When Hemingway wrote *The Old Man and the Sea* there was no question of his making mistakes about Santiago, about what he is like and what experiences he has during his struggle with the fish. Legitimate evaluative questions here concern literary matters, whether Santiago's character is adequately developed, whether Hemingway's style contributes in a positive way to the effect of the narrative, etc. This makes it seem that mathematics, in which acceptance is governed by standards of acceptability, is quite unlike literary fiction, which in this case is not.[91]

However, a contrast can be drawn between telling a new story, as Hemingway did when he wrote *The Old Man and the Sea*, and drawing out the consequences of an old one, as we do when we extend the story Hemingway wrote. When we draw out consequences of *The Old Man and the Sea*, we cannot say whatever we like about Santiago, rather we must say things about him that follow from the literary work; what we say is in this sense constrained by a standard of acceptability that it provides.

Taking situations in which we draw out the consequences of old stories as our model, we can view the correct acceptance of mathematical claims as being constrained by standards provided by mathematical theories. For example, we can say that the intermediate value theorem may be correctly accepted because it is known to follow from the axioms of real analysis. However, this will not indicate that the intermediate value theorem may be correctly accepted in any interesting sense unless those axioms may themselves be correctly accepted. Clearly, then, this account of the acceptability of mathematical claims must be supplemented by a

[91] This seems to be one of the reasons for which Resnik distinguishes the positing of mathematical objects from the introduction of fictional characters. See Resnik (1997, 188-189).

description of the conditions under which we are justified in accepting the axioms of a mathematical theory.

To address this, it helps to consider allegorical works of literary fiction such as Orwell's *Animal Farm*. These often present, in a metaphorical way, facts and opinions about real objects and events. When a work is intended to have this function, we can think of its acceptability as being constrained by its appropriateness as a means of conveying the relevant facts and opinions; the work may be correctly accepted if it constitutes a good allegory for the relevant facts and opinions, otherwise not.

This provokes the idea that the acceptability of mathematical theories could be made to depend on their utility for certain purposes. As it has been germane to our discussions for some time that mathematical theories have empirical applications, the obvious ploy here is to appeal to these to explain mathematical acceptability. Since we are trying to formulate conditions under which the acceptance of a mathematical theory is justified, the relevant conditions must presumably concern what is known about the mathematical theory in question.

A first suggestion is thus the following:

> A mathematical theory M may be correctly accepted if and only if it is known to have empirical applications.

However, by making the acceptability of M depend on its having empirical applications, this proposal ties mathematical acceptability to the vagaries of empirical research. This seems to be wrong; we should be able correctly to accept mathematical theories even if they are not, as a matter of fact, used in empirical studies. It seems better, therefore, to explain mathematical acceptability not by reference to the having of empirical applications, but rather by reference to the possibility of empirical applications. If we say that a mathematical theory is empirically applicable if and only if it could have empirical applications, we can follow through this idea with the following proposal:

> A mathematical theory M may be correctly accepted if and only if it is known to be empirically applicable.

But mathematical theories frequently find application in other mathematical theories (they allow us to simplify previously accepted mathematics, to formulate previously accepted mathematics in fruitful new ways, to unify disparate branches of mathematics, etc.). Moreover, a mathematical theory could find application in mathematics even if it were not empirically applicable, in which case it still ought to be acceptable if the mathematics to which it is applicable is acceptable. This may incline us towards the following modification:

> A mathematical theory M may be correctly accepted if and only if it is known to be empirically or mathematically applicable.

(understanding M to be mathematically applicable if and only if it could have mathematical applications). However, where the previous proposal was too restrictive, this proposal is too liberal, for any pure mathematical theory could have mathematical applications. Our explanation of mathematical acceptability thus stands in need of a more restrictive notion of applicability that retains some connection with, but is less restrictive than, empirical applicability.

To see how to proceed, it helps to stress that the applicability of M to another mathematical theory should contribute to M's acceptability only if the mathematical theory to which it is applicable is itself acceptable. This suggests making acceptability depend in the first instance on empirical applicability, to produce an initial stage of acceptable mathematical theories, but then acknowledging subsequence stages of theories whose acceptability depends on applicability to theories of the preceding stages. This can be achieved by recursively defining for mathematical theory M:

> (i) M is applicable at level 1 if and only if M is empirically applicable
> (ii) M is applicable at level $n + 1$ if and only if M is applicable to theories applicable at level n.

We can then propose that:

> A mathematical theory M may be correctly accepted if and only if it is known to be applicable.

where a theory is applicable just in case it is applicable at some level.

This is very nearly the principle we are looking for, but one final issue must be addressed. There are examples of empirically applicable mathematical theories that are known to be inconsistent.[92] Such theories clearly should not be considered mathematically acceptable, but they are ratified as such by the present proposal. Similarly, mathematical theories that are known to be inconsistent and applicable to other acceptable mathematical theories (but which are not empirically applicable) should not be, but are, ratified as acceptable. We can rectify this with the following proposal:

> A mathematical theory M may be correctly accepted if and only if it is not known to be inconsistent but is known to be applicable.

which builds on the previous suggestion by making knowledge of the inconsistency of a mathematical theory a defeater of mathematical acceptability.

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[92] Mathematical theories involving claims about the Dirac delta function were used in physics prior to the development of the theory of distributions, when it was still said to be a function, despite having properties that no function could consistently have. See Steiner (1992) for discussion of this example and others.

Prescriptive fictionalism uses this account of the acceptability of mathematical theories to finalise its account of the acceptability of mathematical claims. This is given as follows:

> A mathematical claim *p* may be correctly accepted if and only if it is known to be an axiom of, or to have been validly deduced from, a mathematical theory that is not known to be inconsistent but is known to be applicable.

This makes standards of correct acceptance depend on the acceptability of mathematical theories together with the rules of inference associated with the logical concepts they employ. A mathematical claim *p* may be correctly accepted just in case it is known to be an axiom of, or to follow by the relevant rules from, a theory satisfying the given constraints.

It is worth noting that this account of standards of acceptability brings with it a degree of relativity. A sentence of the form:

$$\forall n\,(n > 1 \rightarrow n^2 + n > 1)$$

may be deduced in natural number arithmetic, if "*n*" is taken as a variable for integers, or set theory, if it is taken as a variable for finite ordinals. The standards according to which it is appropriate to assess the acceptability of the claim expressed thus cannot be determined from the sentence on its own but depend also on which mathematical theory is at stake. Since it is reasonable to assume that the theory is contextually supplied on typical occasions of use, however, this presents no objection to the account of acceptability we have described.

7.2.3 Summary

The foregoing discussion sets out a view of mathematics as an area of enquiry. This view states (a) that someone accepts a mathematical claim if and only if they are prepared to rely on it in empirical applications but are not prepared to assert or endorse the statement that it is true, and (b) that a mathematical claim may be correctly accepted if and only if it is known to be an axiom of, or to have been validly deduced from, a mathematical theory that is not known to be inconsistent but is known to be applicable. Bear in mind that prescriptive fictionalism does not put this forward as a description of how we in fact engage with mathematics, but rather as a depiction of how we should engage with mathematics.

7.3 Pretence theoretic pragmatics and the challenge from applications

In the previous section we saw how prescriptive fictionalism views mathematics as an area of enquiry. In this section, we will see how it extends and refines this view, arriving at a conception of mathematics as pretence that promises to meet the challenge from applications. The key innovation here is to advance a pretence theoretic pragmatics for the uses of mathematical language. This theory does not view utterances of object-committed mathematical sentences as assertions of contents true only if there are mathematical objects. Rather, it views such utterances as pretence, as ways of behaving as if such contents were believed, in the absence of such belief.[93]

It will be recalled, from section 6.2, that the challenge from applications is (a) to recover the descriptive uses of mathematics in the absence of mathematical objects, (b) to recover the reliability of mathematical reasoning in the absence of belief in mathematical objects. We will explain the pretence theoretic response to each in turn.

7.3.1 Description

To explain how a pretence theoretic pragmatics for the uses of mathematical language helps to recover the descriptive role of mathematics in science, a distinction must be drawn between two modes of utterance. To do this it helps to consider an example of a game of make believe.

Suppose Jack and Jill are playing cops and robbers. Jill is speeding down a narrow back-street, in flight from the long arm of the law; Jack's siren is

[93] The idea of developing a pretence theoretic pragmatics for mathematical language in order to provide a fictionalist account of mathematical applications is due to Yablo, see his (2001, 2002, 2005).

wailing madly; his revolver pumps round after round after the escaping villain. Or at least, so things seem to Jack and Jill. But actually Jill is running down the passage from the kitchen to the lounge pursued by an intermittently wailing and banging Jack, whose outstretched hand holds a toy pistol. Jack and Jill are playing a game of make-believe.

Playing games of make believe involves imagining and pretending that things are in certain ways. When something is to be imagined in a game, we say it is true in the game. Following Walton (1990), we can think of games of make believe as governed by rules determining what is true in the game. Categorical rules lay it down that such and such is true in the game, for example, that Jill is a robber. Conditional rules make truth in the game depend upon real conditions, for example that Jack-the-cop has caught Jill-the-robber if Jack catches Jill. Such rules are very often associated with props, real objects the properties of which help to determine what is true in the game. In our example, Jack's toy gun is a prop, if it points at Jill's back, then it is true in the game that Jack-the-cop's gun points at Jill-the-robber's back. [94] The rules governing games of make believe are invoked by their participants, who perform acts designed to make things true in the game that make it a fun game to play. Clearly linguistic acts can play a part here, in particular, utterances of sentences true only if fictional objects exist. This brings us to the distinction between modes of utterance alluded to above.

Suppose Jack and Jill continue with their game of cops and robbers when they reach the lounge. Things get a bit out of hand and Jill ends up injured. Hearing the commotion, Mummy comes in and comforts Jill who explains between sobs, "Jack shot at me when I jumped out of the car and then he hit me with his gun!" Taken assertively, this is false. But Mummy realises that Jill has expressed herself from within the game she had been playing with Jack, explaining what has happened by saying things that are

[94] We have not followed Walton's use of terminology here. He talks of rules as determining what is fictionally true, explaining conditional rules as those that make what is fictionally true depend upon real conditions and props as generators of fictional truth. We prefer to talk of truth in games to avoid any suggestion that the fictional truth about fictional objects might be a species of truth.

true in the game. Looking around at the scattered furniture, the guilty expression on Jack's face and the toy gun dangling from his hand, Mummy can very easily work out the main facts of the case, in particular, that Jack hit his sister with his toy gun.

How does Mummy arrive at this conclusion? Having realised that Jack and Jill were playing a game of make believe in which Jack's toy gun is a prop, she understands that the conditional rules associated with it make it true in the game that Jack-the-cop hit Jill-the-robber with his gun if Jack hit Jill with his toy gun. Assuming that Jill's utterance "he hit me with his gun!" is true in their game, Mummy reads this conditional rule backwards to find the condition that makes it true in their game, thereby concluding that Jack hit Jill with his toy gun.

Utterances of sentences whose use in games of make believe are governed by conditional rules can thus express the obtaining of real conditions. Whilst it is implausible to suggest that Jill, in the scenario described above, knowingly takes advantage of this in order to report Jack's misdemeanour, there is nothing in principle to stop us from doing so. That is, we might deliberately utter such a sentence in order to draw attention to the fact that real things are as they must be for it to be true in the game. We thus arrive at a distinction between game-directed utterances, which are aimed simply at exploring what is true in the game, and world-directed utterances, which are aimed at representing real conditions in the world. In a game-directed utterance, a participant in the game utters a sentence in order to indicate its truth in the game. But in a world-directed utterance, the sentence is uttered in order to draw attention to the obtaining of the real conditions that make it true in the game. Returning to the scenario described above, we can say that Jill's utterance of "Jack hit me with his gun" has associated with it two thoughts (i) Jack-the-robber hit Jill-the-cop with his gun and (ii) Jack hit Jill with his toy gun. Jill's utterance puts forward the first thought as being true in the game, and the second thought as being true.

Prescriptive fictionalism takes advantage of this distinction between modes of utterance to explain the descriptive use of object-committed mathematical sentences. Mathematics can only be applied when it is furnished with bridge principles linking what it says about mathematical objects with what is true about the physical phenomena to which it is applied. These bridge principles are part and parcel of the applied mathematical theory. Thus, prescriptive fictionalism views this theory as setting up a standard of acceptability for mathematical sentences containing its vocabulary, the bridge principles set up connections between how things are in the world and what is true according to the theory. When the acceptability of mathematical sentences depends in this way on conditions in the world, they can be uttered in the world directed mode, i.e. with the intention of drawing attention to the obtaining of those conditions. Thus, just as utterances about fictional objects in a game of make believe can be used to assert the obtaining of the real conditions that make them true in the game, utterances about mathematical objects can be used to assert the obtaining of the real conditions that make them true according to mathematics. This is how mathematics, conceived as pretence, is able to discharge its descriptive role.

7.3.2 Reliability

We turn now to the reliability of mathematical reasoning. Prescriptive fictionalism views mathematical argument as a warranting process, i.e. it views correct mathematical arguments as warranting belief in the world directed contents of their conclusion. But what ensures that correct mathematical arguments are reliable with respect to the world directed contents of their conclusions?

The answer to this must have something to do with the standards of acceptability, as these are what ratify mathematical arguments as correctly or incorrectly acceptable. We saw in section 7.1.2 how prescriptive fictionalism sees mathematical theories as providing their own standards of acceptability. When the theory in question is an applied mathematical theory it will involve bridge principles in which both mathematical and physical vocabulary appear. Thus bridge principles help to establish the standards of mathematics correctness.

In the previous section, we saw how the bridge principles of an applied mathematical theory make for world directed utterances of mathematical sentences containing their vocabulary. It thus emerges that bridge principles play a dual role, not only helping to set up the standards of acceptability but also endowing mathematical sentences with world directed significance. Given that mathematical theories are invented, prescriptive fictionalism can thus clam that their invention is conditioned by the requirement that the standards of acceptability they set up are reliable in the required sense. A teleological account of the reliability of mathematical reasoning thus emerges: correct mathematical arguments warrant belief in the world directed interpretations of their conclusions because the theories from which they are drawn were invented in order to provide means of reasoning which are reliable in this way.

To see how this suggestion recovers the reliability of mathematical reasoning, we will consider a simple example from the applied arithmetic of finite cardinal numbers. Prescriptive fictionalism will view this theory as having been developed in stages, with the developments of each stage conditioned by the reliability of the arguments they validate. It is not claimed that this is how we actually arrived at applied arithmetic, nor even that it presents a plausible hypothesis about how we arrived at applied arithmetic; we claim merely that applied arithmetic could have developed this way, and that this would have been sufficient for the reliability of reasoning with it.

In the first stage, schematic bridge principles such as these introduce terms for numbers to help with expressing the results of counts:

There are no Fs $\rightarrow$ the number of Fs = 0

$\exists x\ \exists y\ (Fx \wedge Fy \wedge x \neq y \wedge \forall z\ (Fz \rightarrow z = z \vee x = y)) \rightarrow$ the number of Fs = 2

Such principles introduce a new predicate "number" together with new terms (like "0" and "2") that have the syntactic function of singular terms. In addition to the new sentences with which we express the results of counts, such principles allow the formulation of further sentences involving the new vocabulary, sentences like "0 is a number" and "∃x (x is a number)"). These must be accepted as logical consequences of the results of counts (expressed in the new way). But since the point of the new vocabulary is to give a convenient way of expressing the results of counts, the attendant ontological commitments need not be taken seriously. Thus numbers first appear as fictions introduced to help discharge expressive tasks.

At this stage of its development, applied arithmetic is very limited; we are unable to say very much about arithmetical relations and so are unable to make extensive use of arithmetical reasoning as a means of thinking about the results of counts. In subsequent stages of development, terms for relations on numbers are introduced to address this deficiency. This creates the opportunity to build reliability into the development of applied arithmetic, as the principles laid down to address these requirements come in two kinds. The first kind involves arithmetical vocabulary and logical vocabulary alone, and provides the foundation of pure theory of arithmetic. The second kind makes the acceptability of sentences containing the new arithmetical vocabulary depend upon conditions in the world, possibly expressed using some of the arithmetical vocabulary that has already been introduced. These provide bridge principles by which the new pure theory will be applied. Once principles of one kind are fixed (at any stage of arithmetic development), principles of the other kind are determined which make the applied arithmetical arguments formulable at that stage potential means of reasoning. And reliable bridge principles of the second kind can be viewed as having been picked from these.

Let us consider how principles of the two kinds governing a new term "+" might have been introduced to make this happen (again, we do not claim that this is how individuals acquire a concept of cardinal addition, nor that it is a plausible hypothesis about how "+" was introduced). Our experience with counting collections of objects, especially collections of objects that have been formed from collections we have already counted, would lead us to consider the possibility of introducing terms for relations on numbers in order to keep track of the changing cardinalities of collections. One situation of interest would be that in which we collect together the elements of two disjoint collections of things. We would introduce a sign "+" for a relation on numbers that we hope to use in applied arithmetical arguments about this kind of situation, laying down the bridge principle:

$\forall x(\mathrm{H}x \rightarrow \mathrm{F}x) \wedge \forall x(\mathrm{G}x \rightarrow \mathrm{F}x) \wedge \neg\exists x(\mathrm{G}x \wedge \mathrm{H}x) \wedge \forall x(\mathrm{F}x \rightarrow \mathrm{G}x \vee \mathrm{H}x) \rightarrow$ The number of Fs = the number of Gs + the number of Hs

Together with this, we would stipulate the pure principles

For all n (n +0 = n)
For all n, m (n + m) = (m + n)
For all n, m, (n + m) + r = n + (m + r).

Now suppose we were to round out this pure theory by laying down the equations:

0 + 1 = 1
1 + 1 = 2
2 + 1 = 3
3 + 1 = 4
4 + 1 = 0

Of course, these are not the equations we actually accept, but together with the other rules for "+", they would provide an applied arithmetic which allows the formulation of potentially useful arguments about how many objects there are in disjoint collections.

However, we would very soon discover that this theory is not reliable. For suppose we were presented with a basket of fruit. Having established that the number of apples is 4, that the number of pears is 1 and that there are no other pieces of fruit, we could argue using the equation "4 + 1 = 0" (and other assumptions) that the number of pieces of fruit is 0. Given our original stipulation regarding the use of the term "0", this would express that there are no pieces of fruit in the basket. Of course this would be inaccurate and so our argument would not lead us to a true conclusion about the world. But many other theories of this kind could have been stipulated. In particular, we could have written a theory identical save in having "4 + 1 = 5" instead of "4 + 1 = 0". With this theory, our argument about the number of pieces of fruit would have accurately described the basket with respect to how many pieces of fruit it contains. Thus a pure theory of addition can be picked that makes arguments formulated with the help of "+" world directedly reliable.

Although simple, this example shows how applied mathematical theories can be viewed as having been developed stage by stage, with developments in each stage being conditioned by the reliability of the arguments they validate as correct. Such an account will view novel mathematical theories as having been introduced for application to certain kinds of situation. But having introduced mathematical theories in one kind of situation, we tend to apply them to a wide variety of other kinds of situation. What would explain the reliability of the applied theories in these new contexts?

To address this issue it is helpful to consider a situation in which a mathematical theory that has successful applications fails to apply. Suppose one tried to apply arithmetic to reach the conclusion that the population of the UK is proportionately more content that the population of France. We would say that this is the case if the ratio of the number of content people living in the UK to the total number of UK residents is greater than the corresponding ratio concerning the French population. But how do we determine how many residents of the UK or France are content? We could start by asking everyone, but not everyone is sure. Lots of people would be unable to answer, which would lead to perplexing questions such as whether infants are content. So arithmetic cannot be satisfactorily applied to this problem.

The difficulty here is that it is not sufficiently determinate which residents of the UK (or France) count as content. One condition of the applicability of arithmetic to a range of objects is thus that they have determinate identity conditions, i.e. that questions of identity and distinctness amongst them have determinate answers (note, though, that it is not necessary that we be able to decide these questions). This condition can be read off the principles by which applied arithmetic is introduced. The principle governing use of the term "2", for example:

$$\exists x\, \exists y\, (Fx \wedge Fy \cancel{\wedge \wedge x \neq y} \wedge \forall z(Fz \rightarrow z = x \vee z = y\,) \wedge x \neq y) \rightarrow$$
$$\text{The number of Fs} = 2$$

cannot be used to introduce a sentence expressing the result of a possible count of the Fs unless the identity and distinctness of Fs is determinate, for in this case it would not be determinate that $\exists x\, \exists y\, (Fx \wedge Fy \cancel{\wedge \wedge x \neq y} \wedge \forall z(Fz \rightarrow z = x \vee z = y\,) \wedge x \neq y)$.

That the sentences of an applied mathematical theory can express facts about the objects and processes it was introduced to deal with thus depends on certain features of those objects or processes. At each stage of its development, the new vocabulary introduced is capable of performing its representational function by virtue of the relative stability and permanence of the objects or processes in respect of these features.

Accordingly, prescriptive fictionalism explains the reliability of applied mathematical reasoning in contexts for which it was not originally developed by appealing to similarities, in respect of these features, between the objects or processes addressed in the different contexts. If applied arithmetic had originally been developed to help reason about the results of counts of apples, for example, its reliability in reasoning about the results of counts of pears would be explained on the basis that pears, like apples, have sufficiently determinate identity conditions to be counted, and that this feature is a stable and permanent enough characteristic of them during the timescales determined by our interest.

The similarities explaining why a mathematical theory developed for application to one kind of situation applies equally well to another will vary with the theory. But this presents no objection to the acceptability of this account of reliability. Of the kinds of similarity that can play this part, structural similarity deserves a special mention, as it explains why mathematical reliability is preserved in a wide variety of the cases in which an applied mathematical theory successfully migrates to a new context of application. But as the example of applied arithmetic shows, it is not always necessary to think of successful migrations in terms of structural similarities.

7.3.3 Summary

The challenge from applications demands an account of the descriptive uses of mathematics that makes no appeal to mathematical objects. Prescriptive fictionalism addresses this by arguing that object-committed mathematical sentences can be world directedly uttered. This is possible when bridge principles associate the sentence in question with conditions in the world, so that the sentence can be uttered to assert the obtaining of those conditions. Thus utterances of object-committed mathematical sentences, viewed as parts of pretence, can be used to make assertions about the real world.

The challenge from applications also demands an account of the reliability of mathematical reasoning in the absence of belief in mathematical objects. Prescriptive fictionalism addresses this by arguing that the invention of mathematical theories can be thought of as having been conditioned by reliability, with reliability being built into them at each stage of their construction. The question then arises of how a theory invented for application to one kind of situation comes to be successfully applied in another. To explain this, appeal is made to similarities between the two kinds of situation that makes sentences of the mathematical theory a useful means of expressing facts about them both.

7.4 Stanley's objections to pretence invoking fictionalism

Prescriptive fictionalism applies a pretence theoretic pragmatics to mathematics, in order to avoid commitment to mathematical objects. Clearly this approach could be tried with any area of enquiry in which the claims we are disposed to make appear to bring with them problematic ontological commitments. It is natural, then, to wonder if there are difficulties with the approach in general. Stanley (2001) argues that there are. Although his criticisms are targeted at descriptive appeals to pretence theoretic pragmatics, all save one carry over to the case of prescriptive fictionalism. We are thus obliged to consider them here.[95] [96]

7.4.1 Systematicity and the theory of understanding

Stanley argues that interpretation via pretence is not in general sufficiently systematic for an adequate theory of understanding (2001, 6-7). When a pretence theoretic pragmatics is applied to an area of discourse, sentences convey real world truth conditions via the rules governing the pretences in which they have uses. A pretence invoking fictionalism thus stands in need of a theory of understanding of real world truth conditions, an explanation of how speakers of the language understand the real world

[95] Stanley does not use our terminology. Borrowing labels introduced by Burgess and Rosen (1997) to mark a contrast between different kind of nominalism, Stanley distinguishes "hermeneutic" from "revolutionary" fictionalism: "*Revolutionary fictionalism* would involve admitting that while the problematic discourse does in fact involve attempted reference to nonexistent entities, we *ought* to use the discourse in such a way that the reference is simply *within the pretense.* The *hermeneutic fictionalist*, in contrast, reads fictionalism into our actual use of the problematic discourse. According to the pretense, and only according to the pretense, there exist the objects to which the discourse would commit its users, were no pretense involved." Stanley (2001, 1) This corresponds to the contrast we have drawn between descriptive and prescriptive fictionalism.

[96] The complaint that can be dismissed for irrelevance is that fictionalist appeals to pretence theoretic pragmatics require an objectionable failure of first person authority (see Stanley (2001, 13-14). Stanley bases this view on the claim that people using the kinds of language for which pretence invoking versions of fictionalism might be thought advantageous will deny that they are pretending when using that language. However, this denial will act as a constraint on what we can say about the attitudes adopted towards claims made with the language only if we are trying to describe peoples' actual attitudes to such claims. Prescriptive versions of pretence invoking fictionalism do not attempt to do this, so even if the descriptive approach can only address such denials by invoking a non-transparent attitude of pretence, this would have no bearing on prescriptive views.

truth conditions of its sentences. In the cases of interest to fictionalists, the language will allow formulation of indefinitely many sentences with real world truth conditions. Thus, because of the limited cognitive capacities of speakers, it will be necessary to give a systematic account of how real world truth conditions are assigned. Stanley gives two important reasons for thinking that assignment via pretence lacks the degree of systematicity required: that the rules governing particular pretences do not in general assign truth conditions in a sufficiently orderly way and that too much non-systematic switching between particular pretences is required (*op.cit.*, 7).[97] He infers that "if there are apparently literal discourses that involve the mechanism of pretense, then no such [systematic] explanation [of the assignment of real world truth conditions] appears forthcoming" (*op.cit.*, 7). Thus, in Stanley's view, it is unlikely that pretence invoking versions of fictionalism can provide adequate theories of understanding.

If a descriptive appeal to pretence theoretic pragmatics leaves us unable to explain how we understand the real world truth conditions of given claims, then a corresponding prescriptive appeal leaves us unable to explain how we would understand them were we to follow the prescription. Thus, this argument threatens not just descriptive but also prescriptive versions of pretence invoking fictionalism. As Stanley uses "real world truth conditions" for what we have called world directed contents (see his account of pretence theoretic interpretation (2001, 3-5)), it follows that there is a threat here for prescriptive fictionalism. The argument suggests that, as a version of pretence invoking fictionalism, prescriptive fictionalism will struggle to provide a plausible account of real world understanding, i.e.

[97] Stanley suggests a third point, that pretence theoretic pragmatics is insufficiently systematic for a satisfactory theory of understanding because we lack a general theory explaining how truth in a pretence depends on what is actually true (i.e. a theory explaining how truth in pretence P depends upon truth for variable P) (see the approving quotation from Walton (1990) for this point, Stanley (2001, 8)). However, it is hard to see why this should be thought problematic. A descriptive version of pretence invoking fictionalism is committed to making sense of its target area of language use by appeal to the mechanism of pretence. It will be do so by considering certain kinds of pretence. As far as systematicity and understanding are concerned, the relevant constraint is that these pretences, and the way they are used, be sufficiently systematic for a satisfactory theory of understanding. But these conditions could be fulfilled even if we lacked a general theory of the dependency of truth in pretence on truth; possession of such a theory might help to show that a given pretence theoretic pragmatics is sufficiently systematic for an adequate theory of understanding, but it is not a necessary condition for this.

of the understanding of the real world truth conditions of object-committed mathematical claims.

Let us first address Stanley's worry that the rules governing particular pretences are insufficiently orderly for an adequate theory of understanding:

> The first [worry about systematicity] is whether or not, within a particular pretense, the principles of generation [rules] are sufficiently systematic as to account for our ability to grasp the real world truth-conditions of all potential sentences that are evaluated within that pretense. Linguists and philosophers have long held that the type of systematicity required to explain this ability requires attribution to language users of a compositional semantic theory. But the mechanism of pretense certainly does not respect compositional interpretation of the truth-conditions expressed by a sentence relative to a context. (2001, 7)

It appears from this that Stanley's complaint with the rules governing particular pretences is that they do not identify the real world truth conditions of sentences with the contents they are assigned by the standard compositional semantics. However, this cannot quite be what Stanley means as he later concedes that there are theories of pragmatics that do not assign truth conditions to sentences in the standard compositional way (on given occasions of use), but which are sufficiently systematic, nevertheless, for an adequate theory of understanding.[98] Stanley explains that these views depart from standard compositional interpretation by invoking pragmatically indicated constituents to supplement the content indicated by the compositional semantics. In contrast, he believes that pretence theoretic versions of fictionalism are committed to "rejecting compositional interpretation *tout court*" (*op. cit.*, 9).

[98] He considers in particular the view of Bach (1994), according to which the compositional interpretation of some sentences, such as "John is tall", is supplemented by pragmatically indicated constituents, such as a context relative comparison class for tallness, in order to arrive at its truth condition on a given use.

It is this that Stanley finds objectionable, taking it as a sign that the rules governing particular pretences are insufficiently systematic to allow satisfactory explanations of real world understanding.

Faced with this, we should ask whether pretence theoretic interpretation departs from compositional interpretation so very disruptively to adequate accounts of understanding. Reflection on simple examples suggests that it does not. We described above (section 7.2) Jack and Jill's game of cops and robbers. One of the rules of this game was that it is true in the game that Jack-the-robber hit Jill-the-cop with his gun if Jack really hit Jill with his toy gun. We explained how an onlooker could quite easily recognise this and take advantage of it to understand Jill's utterance of "Jack hit me with his gun" to mean that Jack hit Jill with his toy gun. Real examples in which a particular pretence discharges this kind of function abound. So particular pretences can be used to depart in a controlled manner from the compositional interpretation of given sentences, such that a satisfactory explanation of how the resulting real world truth conditions are understood is possible.

Detractors of pretence invoking versions of fictionalism therefore need to explain why the pretences invoked cannot be assimilated to these simple games of make believe. Stanley tries to give a reason for this after considering an example rather like our own (*op.cit.,* 8). He claims that the pretence theoretic method of interpretation works in such cases because the rules involved are analogical, i.e. because they make the occurrence in the game of a certain kind of action depend on the actual occurrence of a similar kind action. (The analogy involved in our example is that the action of hitting someone with a toy gun is similar to the action of hitting someone with a real gun). Moreover, he suggests that in the cases of interest to the fictionalist, non-analogical rules will be involved. This would entail that, in those cases, a satisfactory theory of real world understanding is not possible (*op.cit.,* 9).

However, the rules governing a particular pretence need not be analogical if pretence theoretic interpretation is to be comprehensible. What is required is rather that the rules are unambiguous and known by the speakers who take communicative advantage of them. Clearly, these features can attach as well to non-analogical rules as they can to analogical ones. This contrast between analogical and non-analogical rules thus gives no reason for thinking that the kinds of pretence required by pretence invoking versions of fictionalism should not be assimilated to simple games of make believe.

Stanley is undoubtedly right to point out that understanding a language is ordinarily taken to require possession of a compositional semantic theory for it. As prescriptive fictionalism departs from the standard compositional interpretation of object-committed mathematical sentences, it may seem that there is tension between the theory and this standard view of what linguistic understanding requires. But if there appears to be a conflict here, it is because too much is being made of the way pretence theoretic interpretation differs from compositional interpretation. Although prescriptive fictionalism assigns non-standard (i.e. non-compositional) real world truth conditions to object committed mathematical claims, this does not mean that it totally disregards their standard compositional semantics when it comes to explaining how we understand them. Far from it. The suggestion is that our understanding of the real world truth conditions of object-committed mathematical sentences is produced by our understanding of what they mean according to the standard compositional semantics together with our knowledge of the bridge principles linking mathematical claims to claims about real things. Together, these allow us to comprehend what is required of the world if a mathematical sentence whose use is governed bridge principles is to be acceptable. Note, too, that bridge principles can be given systematically, as opposed to seriatim. For example, defining "$\exists_{\geq n}$ F" as "there are at least n Fs" as follows:

(i) $\exists_{\geq 1}$ F iff$_{df}$ $\exists x$ Fx;

(ii) $\exists_{\geq n}$ F iff$_{df}$ $\exists x_1 \ldots x_n [\wedge_{1 \leq i < j \leq n}\ x_i \neq x_j \wedge \wedge_{1 \leq i \leq n}\ Fx_i]$, $n > 1$

it is possible to give bridge principles governing terms for numbers as follows:

(i) the number of Fs = 0 iff$_{df}$ $\neg\exists x\ Fx$;

(ii) the number of Fs = n iff$_{df}$ $\exists_{\geq n}$ F $\wedge$ $\neg\exists_{\geq n+1}$ F.

Thus the original problem of explaining how a finite mind can grasp the truth conditions of infinitely many sentences can be dealt with even after shifting to the pretence theoretic mode of interpretation.

Let us now address Stanley's second reason for thinking that pretence invoking versions of fictionalism are too unsystematic for adequate theories of real world understanding:

> The literature on pretense analyses suggests that we often switch quite rapidly between pretenses in understanding discourses. Switching between pretenses amounts to learning a new set of rules, the rules governing the new pretense. It is therefore akin to acquiring a new lexical item, or coming to grasp a metaphor one has never before encountered. These processes are not systematic. But our understanding of novel sentences containing familiar lexical items that are used literally does not seem to involve the same unsystematic processes that are at work in the acquisition of new lexical items, or new metaphors. (2001, 7)

This paragraph suggests two lines of thought. One (suggested by the final remark) is that because the experience of understanding novel utterances is not in general like the experiences of understanding a new metaphor or learning a new word (in the areas of interest to fictionalists), it is wrong to view the former as comparable to the latter. However, this is quite irrelevant to the question of whether pretence theoretic pragmatics is sufficiently systematic for adequate theories of real world understanding.

The other line of thought is that because interesting applications of pretence invoking fictionalism require that we switch between many different pretences when we understand the relevant discourses, and because switching between pretences is an unsystematic process, pretence theoretic analysis are too unsystematic for a satisfactory theory of understanding. This argument is relevant to our present concern. How can prescriptive fictionalism respond to it?

The crucial point to make is that switching between pretences "amounts to learning a new set of rules" only when it involves movement to a pretence with which we are not already familiar; when we switch between familiar pretences, we simply respond to contextual cues to invoke one previously known set of rules rather than another. It follows that when we switch between familiar pretences, what we do is not comparable to what we do when we understand a (brand) new metaphor or learn a new word, as Stanley claims. He is thus wrong to claim on the basis of this comparison that the process in question is not sufficiently systematic for understanding. Admittedly, learning the rules of unfamiliar pretences is comparable to these unsystematic processes. But pretence invoking versions of fictionalism need only appeal to the acquisition of such rules in their explanations of how we first acquire the fictionalistically used vocabulary. Since this is a situation in which new words are being learnt, the comparison is surely harmless.

For clarity, it may help to explain how prescriptive fictionalism, in particular, allows for well disciplined switching between "mathematical pretences". We saw in section 7.2 that pretence theoretic mathematical fictionalism takes mathematical theories to set up standards of acceptability by which claims made with mathematical sentences are to be assessed. We thus "switch between mathematical pretences", as Stanley might put it, when we switch between mathematical theories. The number of mathematical theories we apply is actually quite small, so we are not required to think of ourselves as switching between very many of them. Furthermore, in mathematical applications we typically switch from one familiar mathematical theory to another familiar mathematical theory (arithmetic to real analysis, say); this is comparable to switching between familiar pretences and so not objectionably unsystematic. Finally, contextual cues such as the mathematical language used and the claims taken for axioms can be taken as indicators of which theory is being applied. There is thus nothing here to suggest that the pragmatics of prescriptive fictionalism, by demanding switching between "mathematical pretences", is insufficiently systematic for an adequate theory of real world understanding.

Before closing his discussion of systematicity, Stanley mentions two more points that he takes to detract from the plausibility of descriptive pretence invoking fictionalism. The first is that because the rules governing pretences are not systematic, there are no constraints on which real world truth conditions might be claimed to be expressed by which uses of sentences in pretence (*op.cit.*, 9-10). The second is that descriptive versions of pretence invoking fictionalism are simply prescriptive versions of fictionalism in disguise (*op.cit.*, 10). However, Stanley's arguments for these points ultimately depend on the alleged reasons considered above for thinking pretence theoretic pragmatics too unsystematic for understanding (for the connection between these alleged reasons and the second point, see Stanley's discussion of error theories (*op.cit.*, 13)). These additional points thus add no weight to Stanley's prior case against pretence invoking fictionalism from considerations of systematicity. Having found that case unconvincing, we conclude that it provides no reason to dismiss prescriptive fictionalism.

7.4.2 Conflicting evidence from psychology

Another of Stanley's worries about fictionalism is psychological:

> The most straightforward way to understand the hermeneutic fictionalist is that the way in which engaging in games of make-believe is like engaging in the ontologically controversial discourse

> is that the very same psychological capacity is involved in both activities. The fourth worry is that, in any non-explicitly fictional discourse of interest to metaphysicians, the thesis that the same psychological capacity is involved in engaging in games of make-believe and grasping the relevant discourse is likely to be subject to empirical refutation. (2001, 14)

The reasoning behind this allegation depends on some psychological facts relating to autism. Children with autism tend not to play games of make believe, and autistic adults tend not to engage with fiction and find it hard to follow figurative language. But autistics do not struggle with the areas of discourse for which fictionalist treatments have been suggested. Thus, Stanley argues, it seems unlikely that the same psychological abilities underlie these disputed areas of discourse, on the one hand, and fiction, or make believe, on the other; fictionalist accounts of these areas of discourse should thus be rejected for making them depend on the wrong psychological base. Call this the argument from psychology.

This argument threatens not just descriptive but also prescriptive versions of fictionalism. If some people are competent in a given area of discourse but lack a psychological ability which is required to enter into it in the manner set out by a descriptive fictionalism, then clearly we should not think of their competence in that manner. Assuming that everyone's competence in the area is to be viewed in the same way, it would thus not be possible to endorse a prescriptive fictionalism with respect to that are of discourse. So if Stanley is right to argue that autistics display just this mix of competence and deficiency in the areas of discourse of interest to fictionalists, then that would provide reason not to consider them from the prescriptive fictionalist perspective.

If the argument from psychology is to provide an objection to pretence theoretic mathematical fictionalism, it must be shown that, on that view, engagement with mathematics depends on a psychological capacity that mathematically competent autistics lack. Do the psychological facts

Stanley cites establish this? With references to Baron-Cohen, Leslie and Frith (1985) and to Leslie (1987), Stanley (2001, 15) argues that autistics lack a "theory of mind mechanism", a mechanism for developing notions like belief and pretence. He mentions two reasons for this, that autistics tend to fail false-belief tests and that autistic children as a rule do not engage in games of make believe. With references to Happe (1994, 1995), Stanley then points out that autistic adults display a similar lack of imaginative activity, finding it hard to see the point of, and to engage with, fiction, and finding it hard to understand figurative language. Stanley acknowledges that our scientific understanding in this area is by no means complete. But on the basis of this research he considers it plausible that there is a psychological ability, lacked by autistics, which children must possess if they are to play games of make believe and which adults must possess if they are to engage with fiction and understand figurative language. During the course of his discussion (though without references to studies from empirical cognitive studies), Stanley also observes that autistic children do not struggle to grasp mathematics, more particularly arithmetic. Thus he suggests that pretence theoretic approaches to mathematics make engagement with mathematics depend on psychological capacities that mathematically competent autistics lack.

Stanley's reading of this research is plausible, but it is by no means the only one available. One response to his argument from psychology is thus to put forward, if possible, a reading of this research which does not support the conclusion that autistic people lack a psychological capacity necessary for engaging with mathematics in the way described prescriptive fictionalism. We shall attempt to do this now.

Our first claim is that the autistic failings Stanley mentions with respect to acknowledged forms of pretence can be ascribed to lack of a theory of mind.[99] In a typical false belief test, one might ask an autistic child into which of several boxes a doll thinks a given object has been placed, in a

[99] Or failure to deploy such a theory. For brevity we omit this qualification.

scenario set up so that the doll, considered as a person, would have a false belief about this. Clearly lack of a theory of mind would explain failure on such tests, as to respond correctly it is necessary to be able to think of the doll as a person with beliefs. Childhood games of make believe typically involve pretending to be a different kind of person or being to the person or being you actually are. Similarly, the literary fictions enjoyed by adults typically explore episodes from the lives of fictional people. We would thus expect people who lack a theory of mind, and so are not capable of thinking about, or imaginatively entering into, the mental worlds of others to find it hard to play such games and to develop an interest in such fictions. Our understanding of figurative language can also be viewed as depending on a theory of mind. When someone says something figuratively, our recognition that their remark is intended figurally and our interpretation of what it figurally means draws heavily on our understanding of the kind of person they are, what beliefs they have, what their likes and dislikes are etc. Thus we would expect that people who lack a theory of mind would find this kind of language difficult to follow.

Our second claim is that, even when viewed as pretence in the manner required by prescriptive fictionalism, engagement with mathematics does not demand possession of a theory of mind. Mathematical claims do not involve concepts such as belief and pretence, and mathematical enquiry is not a sociological investigation into the propositional attitudes borne by its participants towards given mathematical claims. It thus appears that prescriptive fictionalism requires of mathematical enquirers that they possess a theory of mind only if possession of such a theory were required to act in accordance with its prescriptions. But it does not appear that this is required. In section 7.2, we saw that prescriptive fictionalism states that our acceptance of object-committed mathematical claims should be constituted by a preparedness to rely on them in empirical applications, but not to make or endorse the statement that they are true. Clearly one could be in this state of preparedness regardless of whether one possesses concepts from the theory of mind. In section 7.3, we saw that prescriptive fictionalism states that when we utter sentences expressing object-committed mathematical claims we should only be pretending that those claims are true. But one can pretend that one believes a claim, i.e. one can act as if one believes it, without actually believing it, whether or not one possesses a theory of mind. It seems, then, that someone who lacks a theory of mind is nevertheless able to engage with mathematics in the way required by prescriptive fictionalism.

These two claims suggest that the view that prescriptive fictionalism makes engagement with mathematics depend on a psychological capacity that autistics lack does not follow from the literature on autism to which Stanley refers. This research can be viewed as indicating that autistic people lack a theory of mind and that they cannot engage in the kinds of pretence for which possession of a theory of mind is essential (first claim). But we have argued that prescriptive fictionalism does not make engagement with mathematics depend on possession of a theory of mind (second claim); on this view you do not need to possess concepts like belief and pretence in order to enter into "mathematical games of make believe" because the content of the games does not depend on such concepts and so can be played (non-self-consciously, of course) even by those who lack them. Thus Stanley's argument from psychology does not provide an objection to prescriptive fictionalism.

We have argued thus far that the psychological facts about autism Stanley cites can be explained from the pretence theoretic perspective. But perhaps there is other empirical research on autism that suggests otherwise. And if there is not, perhaps such research will be conducted in the future. To provide some insurance against these possibilities, we will now explore a second line of response to Stanley's argument from psychology.

The suggestion is that autistic mathematical competence be dealt with as a special case. The idea here would be that we should view acceptance of object-committed mathematical claims in the way described in section

7.2.1, for people with normal psychological abilities, but as belief, for autistics. On the assumption that autistics are not able to engage in pretence, it could be argued that this would be to view them as responding to their mathematical training in the best available way, but as forming the wrong attitudes, philosophically speaking, to acceptable mathematical contents.

Stanley argues against this kind of view. For him, such views would have to claim that autistic acceptance of claims made in the language of the discourse is constituted by belief, that normal acceptance of the relevant claims is constituted by pretence, but that the two groups are nevertheless behaviourally identical as regards their use of the discourse. He objects to this position as follows:

> Someone who is not aware that a given sentence is used figuratively will have a vastly different reaction to its use than someone who is so aware. If John says (metaphorically) "Hannah is the sun", and Hannah does not recognize the figurative nature of his discourse, she will not behave appropriately in response to his utterance. The figurative nature of a discourse has obvious repercussions for action. In contrast, if this reply is correct [if autistic mathematical acceptance is dealt with in the way suggested], the figurative nature of the disputed discourse would have not have any clear repercussions for action. Someone who does not know that the discourse is figurative may still nevertheless be indistinguishable from someone perfectly competent in its use. This is a significant disanalogy between figurative speech, on the one hand, and any one of the ontologically disputed discourses, on the other. (2001, 17)

We may take it that Stanley intends figurative discourse broadly here, to cover all uncontroversially indirect discourse. His claim is thus that there are no examples of uncontroversially indirect discourse for which belief could not be behaviourally distinguished from pretence, i.e. no examples of an utterance for which there could not be behavioural evidence to mark the difference between a speaker believing in its content and their merely pretending that it is true. From this he infers that it is implausible to advance pretence invoking versions of fictionalism that are committed to there being states of pretence and belief the difference between which cannot be made manifest in behaviour.

However, the behavioural reactions available to the non-autistic person outrun those available to the autistic person. If someone who quasi-asserts an object-committed mathematical claim is heard and understood by two people, one autistic, the other not, we can assume that they would agree on its acceptability. Thus their patterns of response to the utterance would share much in common. However, the normal person would be able to discuss the place and status of the mathematical claim in ways that the autistic person would not (because of his richer psychological abilities); so his pattern of response would be distinct from the autistics. Stanley's claim that the autistic and the non-autistic are behaviourally identical in respect of their use of mathematical discourse thus does not seem credible.

As a result of this, Stanley provides no reason why we should not think of autistic acceptance of object-committed mathematical claims as false belief whilst taking normal acceptance of the same claims as pretence. Thus, even if the psychological facts ultimately do support his view that autistics are incapable of entering into any kind of pretence, the argument from psychology would still not refute prescriptive fictionalism. Autistic mathematical acceptance of object-committed mathematical claims could be treated as a special case, taken not as pretence but as belief.[100]

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[100]Stanley puts forward a second point against the possibility of an error theoretic approach to defend descriptive fictionalism against his argument from autism. This does not sit well with the descriptive outlook, he claims, "For surely the default assumption, when someone believes they are not engaged in pretense, is that they are not engaged in pretense." (2001, 17). It is hard to see how this would support his position when the people in question are autistic, however. Given his prior assumptions, Stanley could claim that an autistic person lacked belief that they are not engaged in a pretence when dealing with mathematics. But he could not claim that they believed that they were not engaged in pretence, for this is the kind of self-attribution that lacking a theory of mind makes problematic.

7.4.3 Lack of coherent motivation

Stanley's final worry concerns motivation. In his view, descriptive fictionalist appeals to pretence theoretic pragmatics necessarily operate with the notion of "the ontological commitments a speaker believes they incur when entering into a discourse" (2001, 18). We will call this the notion of acknowledged ontological commitments. Stanley claims that this notion is uninteresting from an ontological point of view, argues that the pretence theoretic pragmatics is a poor instrument for its investigation, and concludes that descriptive fictionalism is not well motivated (*op.cit.*, 19-21).[101]

As with the previous worries, the worry here transfers over to prescriptive versions of fictionalism. Prescriptivists and descriptivists agree that by viewing object-committed mathematics as pretence, ontological commitment to mathematical objects can be avoided. The descriptivist claims further that object-committed mathematics is actually pretence, but the notion of ontological commitment in play is introduced at the prior stage of agreement. So if descriptivist versions of fictionalism necessarily operate with the notion of acknowledged ontological commitments, so do prescriptive versions. Stanley's suggestion thus poses a threat to prescriptive fictionalism. This aims to establish that we are not ontologically committed to mathematical objects, in the Quinean sense according to which one is committed to the kinds of objects that are required to make one's beliefs true. As people can be mistaken about what kinds of objects must exist in order for their beliefs to be true, this notion differs from the notion of acknowledged ontological commitments. So if appeal to pretence theoretic pragmatics necessarily brings the latter notion into play, prescriptive fictionalism may be unable to achieve its objective.

[101] Stanley's antipathy seems justified; the notion of acknowledged ontological commitments does not seem to be a notion of ontological commitment, properly so called, at all.

Stanley's suggestion that pretence invoking fictionalism necessarily operates with the notion of acknowledged ontological commitments first appears in the following passage:

> The hermeneutic fictionalist holds that the best semantic theory for a discourse may not be a good guide to the ontological commitments of the person who uses that discourse. For example the best semantic theory for arithmetic commits someone who believes what is expressed by "There are several prime numbers between one and ten" to the existence of numbers. But, according to the hermeneutic fictionalist about arithmetic, a nominalist could believe what is expressed by this sentence, without thereby being committed to numbers. The hermeneutic fictionalist believes that semantic theory does not capture this notion of a speaker's ontological commitments. Hermeneutic fictionalism is motivated by the desire to account for *the ontological commitments the speaker believes she incurs* when she endorses the truth of an utterance. (2001, 18)

In the first three sentences, Stanley draws attention to two general claims that descriptive/hermeneutic fictionalism makes: that the best semantic theory for a collection of sentences is not always a reliable guide to the ontological commitments exhibited in standard uses of the sentences; that belief in the content expressed by a sentence on a given use is not always belief in the content expressed by the sentence according to our best semantic theory. Whilst these are fundamental principles of any form of fictionalism, they leave undecided just which notion of ontological commitments is in play. So when Stanley refers in the penultimate sentence to "this notion of a speaker's ontological commitments", he has not established that this must be taken for the notion of acknowledged ontological commitments. This passage thus contains no argument that, by appealing to pretence, one necessarily deals with acknowledged ontological commitments. In fact, the two general claims make perfect fictionalist sense when the Quinean notion of ontological commitment is assumed.

Perhaps Stanley's argument that pretence invoking fictionalism necessarily deals with acknowledged ontological commitments appears in the next paragraph:

> Here is the idea. Suppose the best semantic theory for a discourse entails that endorsing the truth of a certain utterance commits one to some objects the existence of which some speakers who would endorse the truth of the utterance repudiate. In such a case, the speakers are simply pretending that the objects in question exist, in order to express something ontologically innocent. For example, a nominalist who utters "The number of apostles is twelve" is only pretending that there are numbers, in order to express that there are twelve apostles. Similarly, one might think (cf. Melia (1995)) that someone who utters "The average star has 2.3 planets" is only pretending that there is an average-star-thing, which can have properties like having 2.3 planets. Upon reflection, a speaker would reject the *actual* existence of these entities to which the best semantics appears to commit her. (2001, 18-19)

Stanley is correct to say that it is significant for pretence invoking versions of fictionalism that a speaker "would reject the *actual* existence of these entities to which the best semantics appears to commit her". But he is wrong to suggest that the fact that such disavowals are significant shows that the notion of acknowledged ontological commitments is in play. The Quinean notion of ontological commitment applies primarily to theories (more precisely, regimented theories). Pretence invoking fictionalists can thus take disavowals such as those indicated (rejections of the actual existence of the relevant kind of objects) as evidence that the speakers do not believe the theories that are ontologically committed, under the Quinean notion, to the kind of object in question. This would be quite consistent with the speakers activity in putting forward sentences expressing claims that are true only if objects of that kind exist, for on the pretence invoking approach, this activity is a kind of pretence, and therefore does not involve belief in the contents in question.

Outside the passages quoted above, Stanley gives no suggestion as to why pretence invoking versions of fictionalism necessarily address acknowledged ontological commitments. It thus seems that he lacks a satisfactory argument for this view. Because of this, and because our discussion suggests that the Quinean notion of ontological commitment sits perfectly well with the pretence theoretic perspective, we see no reason to believe that a pretence invoking fictionalism cannot operate with the Quinean notion. Stanley's complaints about the notion of acknowledged ontological commitments thus provide no grounds for the dismissal of prescriptive fictionalism.

7.4.4 Summary

In the preceding sections we discussed difficulties Stanley alleges for versions of fictionalism that make use of pretence theoretic pragmatics. We argued that his grounds for thinking that pretence theoretic pragmatics are insufficiently systematic to make for a satisfactory theory of understanding are unconvincing in the case of prescriptive fictionalism. We argued that his claim that psychological facts relating to autism block pretence invoking theories was too vague to constitute a convincing objection and that prescriptive fictionalism can deal with those facts in satisfactory ways. Finally, we argued that his claim that pretence invoking fictionalism necessarily addresses the notion of acknowledged ontological commitments is not well supported, so that there was no reason not to take prescriptive fictionalism as addressing the Quinean notion of ontological commitment, as intended. For these reasons, we claim that Stanley's critique of pretence invoking versions of fictionalism does not provide grounds upon which to reject prescriptive fictionalism.

7.5 Burgess's objections to mathematical fictionalism

Having found responses to Stanley's complaints about pretence invoking fictionalism, our confidence in prescriptive fictionalism grows. However, one might still wonder whether there are considerations specific to mathematics that show that the pretence theoretic approach is not appropriate there. Burgess (2004) thinks there are. Distinguishing between hermeneutic and revolutionary forms of fictionalism, he argues that neither is adequate to mathematics and concludes that mathematical fictionalism should be rejected.

Burgess's argument against hermeneutic fictionalism is that it incurs an unsatisfied obligation to provide evidence that mathematicians intend their mathematical utterances non-literally, i.e. as parts of a mathematical fiction, not as expressions of mathematical beliefs. Whatever the merits of this claim, it clearly does not threaten prescriptive fictionalism, for this does entail the defining characteristic of hermeneutic fictionalism, that mathematicians themselves understand their talk to be a kind fiction (*op. cit.*, 23). However, the complaint Burgess directs at revolutionary fictionalism cannot be so easily dismissed. Although not explicitly defined, revolutionary fictionalism appears to be the view that mathematicians should understand mathematics as a form of fiction, that they should intend their mathematical claims to be taken as fictions. This view is sufficiently similar to prescriptive fictionalism, which claims that mathematics should be taken as a kind of pretence, that any objection to the former might well provide an objection to the latter. We are thus obliged to consider what Burgess has to say against revolutionary fictionalism, to find out if this is the case.

Burgess first claims that revolutionary fictionalism is sometimes argued for by appeal to Gödel's Incompleteness Theorems. To reconstruct such an argument he makes two assumptions: that Gödel's theorems show that some mathematical questions will never be decided (*op.cit.*, 29), and that the propensity to throw up undecidable questions distinguishes mathematics from other seemingly scientific pursuits (*loc. cit.*). From these premises, Burgess thinks one can infer revolutionary fictionalism if one also assumes that the propensity to throw up undecidable questions distinguishes fact from fiction. One is thus left with a choice between accepting this assumption and concluding that mathematics is in error about what mathematical objects there are, or rejecting it and resisting this conclusion. Burgess leaves us in no doubt about which option he finds the more attractive. Given the histories of philosophy and science, he considers it far more likely that philosophy is in error with this criterion for distinguishing fact from fiction than that mathematics is in error regarding what mathematical objects there are. And he thinks this contains a general lesson:

> One argument against revolutionary fictionalism is thus just that, given the historical record, on simple inductive grounds it seems extremely unlikely that philosophy can do better than mathematics in determining what mathematical entities exist, or what mathematical theorems are true, and much more likely that for the $(n + 1)^{st}$ time, philosophy has got the nature of truth and existence wrong. (2004, 30)

Thus Burgess finds revolutionary fictionalism implausible.

Do these considerations provide an objection to prescriptive fictionalism? To answer this, it is important to realise that Burgess is not just targeting fictionalist views that appeal to mathematical incompleteness for support.[102] Rather, he is taking aim at views that recommend wholesale revisions in our attitudes to mathematical theories for philosophical reasons to do with truth and existence. His objection is that the histories

[102] If he were, there would be an easy rejoinder since pretence theoretic mathematical fictionalism does not involve such an appeal. It is worth mentioning that although fictionalist views in the literature often appeal to considerations of incompleteness, only Wagner (1982) makes his case depend solely on considerations of incompleteness, and he limits his fictionalism to the natural numbers. Other defenders of fictionalist accounts of mathematics (such as Yablo (2001, 2002, 2005), Balaguer (1996b)) appeal to other factors as well. The consensus, which is surely correct, seems to be that the incompleteness of mathematics can support a wide-ranging fictionalism only as part of a more expansive case.

of science and philosophy suggest that such views are wrong. This is a pessimistic induction about a certain kind of error theory of mathematics.

However, prescriptive fictionalism is not an error theory of the relevant kind, since it does not propose criteria for truth and existence and then use these to deduce that mathematics should be viewed as fiction. So even if the historical record shows that philosophical theories that do this are mistaken, this would not give a reason to reject prescriptive fictionalism.

It may perhaps be responded that this reply places too much weight on the kind of support Burgess considers for revolutionary fictionalism and not enough on the fact that it is an error theory. But if it is the error theoretic aspect of revolutionary fictionalism that is the more important, we need to pay attention to what Burgess says about it. He makes clear that he considers revolutionary fictionalism to be an error theory intended to provide "corrections" to mathematics (*op.cit.*, 30). This is why he thinks it involves the odious assumption that "philosophy can do better than mathematics in determining what mathematical entities exist, or what mathematical theorems are true" (see quotation above). Now prescriptive fictionalism does entail that we should not believe what mathematics says about mathematical objects, so to the extent that mathematicians believe object-committed mathematics, prescriptive fictionalism does consider them to be in error. However, this feature of the theory is not objectionable.

The reason for this has to do with the contrast between acceptance and belief (see section 7.1). Having separated these we must be careful to separate mathematical issues, such as which mathematical claims are acceptable according to the prevailing standards, and philosophical issues, such as which attitudes it is appropriate to take to mathematical claims ratified by the standards. Even with this division of issues, prescriptive fictionalism implies that mathematicians are in error if they form belief in the object-committed mathematical claims they judge to be correct according to the prevailing standards. However, prescriptive fictionalism does not make any demands as to which mathematical claims should be accepted, so this is a point about the right attitudes to adopt to the results of mathematical practice, not an assault on that practice and its results. It is thus unfair to depict prescriptive fictionalism as an objectionable philosophical "correction" to the results of mathematical practice, so even if Burgess's pessimistic induction shows that such corrections are problematic, this does not provide an objection to prescriptive fictionalism.

Burgess directs one further, general complaint at fictionalist accounts of mathematics. We generally assume that it would make no difference to the way things seem to us whether mathematical objects exist or not. To this extent, the question whether mathematical objects really exist lacks empirical significance. In contrast, we acknowledge that it would make a great deal of difference to how things seem to us if typical literary fictions were true. Thus the question whether creatures of literary fiction really exist is empirically significant. For these reasons, Burgess considers the analogy between mathematics and fiction inappropriate (*op.cit*, 35).

Burgess presents this line of thought as a "supplement" (*op.cit*, 30) to the pessimistic induction we discussed above. This suggests it is targeted at revolutionary fictionalism since that was the target of the pessimistic induction. However, the conclusion he draws is not restricted to this kind of fictionalism:

> I think that in view of this radical difference between mathematics and novels, fables, or other literary genres, the slogan 'mathematics is a fiction' not very appropriate, and the comparison of mathematics to fiction not very apt. (2004, 35)

Thus it appears that we have here a general attack on mathematical fictionalism of all stripes.

One point to make in response to this complaint concerns the role of the fictionalist analogy between mathematics and literary fiction. When this is heuristic rather than justificatory, when the analogy is used to help explain the fictionalist position in question, not as a premise in an argument for it, there is no need to demand that mathematics be like literary fiction in every respect. Instead, a much looser analogy can be posited, according to which mathematics shares certain key characteristics with various different kinds of literary fiction. Such an analogy can be compared to the republican's claim that the Royal family live lives of soap opera. This says that members of the Royal family behave in ways quite often seen in soap operas such as *Coronation Street*, etc., not that their every idiosyncracy is exhibited by some character from a soap opera.

Prescriptive fictionalism invokes an analogy between mathematics and literary fiction for heuristic purposes only, so it can make do with a loose analogy of this kind. Clearly such an analogy allows the possibility of differences between mathematics and literary fiction. Thus Burgess may be correct to claim that lack of empirical significance for the question of whether there really are mathematical objects distinguishes between mathematics and literary fiction. But this does not entail that we must abandon the loose analogy between mathematics and fiction put forward by prescriptive fictionalism.

Another point to make in response to Burgess's complaint is that, if it is a fact that the real existence of mathematical objects lacks empirical significance, this can be explained perfectly well from the fictionalist perspective. But prescriptive fictionalism can assume that mathematical objects, if they exist at all, are abstract in something like the customary sense of lacking spatial and temporal properties. As such, they would be empirically undetectable even if they were to exist, and so the question of their real existence would lack empirical significance. So for this reason, too, it seems that Burgess's complaint provides no grounds for giving up the pretence theoretic analogy between mathematics and literary fiction.

Our discussion of Burgess's criticisms of mathematical fictionalism is now complete. We dismissed his objection to hermeneutic fictionalism on the grounds that it is not relevant to prescriptive fictionalism. We argued that the pessimistic induction against revolutionary fictionalism is not persuasive and that prescriptive fictionalism does not objectionably impute error to mathematicians who believe in object-committed mathematics. Finally, we argued that lack of empirical significance for the question of whether mathematical objects really exist does not undermine the loose analogy required by, and can be explained from the perspective of, prescriptive fictionalism. On these grounds we conclude that Burgess's objections to mathematical fictionalism do not constitute grounds to reject prescriptive fictionalism.

7.6 Consistency

In the preceding sections we discussed objections to fictionalism due to Stanley and Burgess. We argued that these are unsuccessful against prescriptive fictionalism. However, a further worry must be addressed.

Prescriptive fictionalism presupposes that we lack a satisfactory account of knowledge of object-committed mathematics but that it is desirable to use this in empirical applications. For this reason, it faces the epistemological problem of explaining how we can apply such mathematics without illegitimately presupposing that it is known. As we have seen, prescriptive fictionalism addresses this difficulty by proposing that we engage with object-committed mathematics as a useful form of pretence. In so doing, it aims to solve an epistemological problem by appeal to a version of mathematical instrumentalism. Prescriptive fictionalism is thus comparable to the views of Hilbert (1925, 1927) and Field (1980, 1989a).

To see why this leads to a worry about prescriptive fictionalism, we need to note two connections between the instrumentalist theories of Hilbert and Field. The first is that they both require that the consistency of the instrumentally viewed mathematics be established (in fact, as we saw in chapter 6, Field requires that the conservativeness of instrumentally viewed mathematics be established, but conservativeness can be regarded as a strong form of consistency). Call this the consistency condition. The second is that they both depend on epistemological presuppositions that render doubtful that this condition can be met. On the one hand, Hilbert thought that only finitary methods are immune to the threat of paradox, and thus required a finitary proof of the consistency of non-finitary mathematics (which was to be viewed instrumentally).[103] But most critics now believe that Gödel's second theorem (theorem XI of his (1931)) shows that a finitary proof of the consistency of non-finitary

[103] For our purposes, it does not matter precisely what the distinction between finitary and non-finitary mathematics is, just that Hilbert took much important mathematics to be non-fionitary. See Tait (1981) for statement of what is now the prevailing account.

mathematics is not possible. On the other hand, Field thinks that only nominalistically acceptable evidence is legitimate, and thus requires a nominalistically acceptable demonstration that mathematical science is conservative over nominalistic science. But, as we saw in Chapter 6, it is controversial that there are nominalistically acceptable grounds for this.

The worry to which these observations give rise is that since prescriptive fictionalism and the views of Hilbert and Field are all of basically the same kind, since they all aim to make epistemological mileage out of mathematical instrumentalism, they may also share between them these further connection we have noted between the views of Hilbert and Field. If this is the case, then prescriptive fictionalism will be committed to establishing the consistency of object-committed mathematics (from the first connection) though its epistemological presuppositions will render doubtful that this can be achieved (from the second). This would mean it is objectionably unstable.

In this section, we will address whether this worry can be met. Clearly prescriptive fictionalism could try to meet it either by providing grounds for the consistency of object-committed mathematics or by arguing that it is not obliged to do so. Consider, then, where one might search for grounds for the consistency of a given mathematical theory M. As consistency is usually understood to be a mathematical notion (defined in model theory or proof theory) one might look to mathematics, hoping for a proof of the consistency of M in some other mathematical theory. Such a proof is called a relative consistency proof. Alternatively, one might propose a novel, non-mathematical notion of consistency in the hope of finding non-mathematical grounds for the consistency of M.

We considered an example of this last approach in chapter 6, where we discussed Field's modal notion of consistency (and other meta-logical notions). We argued that it is unclear there are nominalistically acceptable grounds for knowledge of the modal consistency of mathematics (section 6.5.2). Since this explanation of consistency seems to be the only serious alternative to mathematical explanations, this conclusion leaves us with no promising way of showing that there are non-mathematical grounds for the consistency of M. It thus does not seem a good idea for prescriptive fictionalism to appeal to such grounds in order to satisfy the consistency condition.

This suggests that if prescriptive fictionalism is to provide grounds for the consistency of object-committed mathematics, it must do so by appeal to a relative consistency proof. Such an appeal would demand treating the mathematical theory in which the proof is carried out as known. Taking this line would necessarily restrict the scope of prescriptive fictionalism, as the object-committed mathematical claims associated with the mathematics of the proof could no longer be regarded as pretence. However, this is not entirely ruled out by the conclusions of previous chapters. The possibility thus emerges of a two tier account rather like Hilbert's, in which some, comparatively elementary, mathematics would be dealt with from the realist perspective, more advanced mathematics would be treated as pretence, and this would be backed up by a proof of the consistency of the latter in the former.

The two options prescriptive fictionalism has to respond to the threat posed by the consistency condition are thus as follows: (a) to argue that relative consistency proofs are available to support versions of prescriptive fictionalism that are limited in scope but nevertheless worthwhile (b) to argue that prescriptive fictionalism is not committed to satisfying the consistency condition. We will consider each in turn.

7.6.1 Relative consistency proofs

If prescriptive fictionalism is usefully to establish the consistency of object-committed mathematics by appeal to a relative consistency proof, the mathematics in which the proof is carried out must be epistemologically more secure than the mathematics whose consistency is proved. One reason for this demand is that the discussions of previous chapters permit prescriptive fictionalism to assume knowledge only of comparatively weak mathematics, arithmetic, for instance, and not of comparatively stronger theories, like real analysis and set theory. But another reason for this demand is independent of the presuppositions of prescriptive fictionalism: unless the theory in which the consistency proof is carried out is epistemologically more secure than the theory whose consistency is proved, there will be no epistemological advantage to construing the latter as pretence. Our question should thus be the following: What are the prospects of proving the consistency of mathematical theories used in science in mathematical theories that are epistemologically more secure?

7.6.1.1 Relative proofs of model theoretic consistency

Let us first consider this question in connection with the model theoretic notion of consistency. To prove that a mathematical theory is model theoretically consistent one has to prove that it has a model. Informally, this means proving that there exists a set of objects exhibiting the structure described by the theory. To prove the model theoretic consistency of Peano arithmetic, for example, one would have to prove the existence of a set of objects exhibiting the structure of an ω-sequence. This would involve proving the existence of a countably infinite set and so would require assumption of a set theory in which the usual axiom of infinity can be proved. The model theoretic consistency of PA can thus be established only by assumption of the truth of a theory at least as ontologically loaded as PA itself. What about a proof of the consistency of real analysis? Presumably we would not want to foreclose the possibility of second order formulations of the theory, so we would have to be able to establish the existence of uncountable models. This would require assumption of the truth of a set theory in which the existence of uncountable sets could be proved. Thus we would have to assume the truth of a set theory at least as ontologically loaded as real analysis. In general, then, it appears that proofs of model theoretic consistency would not be of use to the kind of view under consideration. They would have to assume knowledge of mathematical theories as ontologically loaded as the mathematical theories that are to be regarded from the pretence theoretic point of view. It is hard to see how such knowledge could be more secure than knowledge of the theories that is to be construed as pretence.

Research on mathematical theories formulated in paraconsistent logic suggests that there are relative consistency proofs for which this condition is not necessary. Meyer (1976) establishes the existence of a finite model for a formalisation of PA in the relevant logic **RQ**.[104] Priest (1991, 1994) establishes the existence of finite models for a formalisation of PA in the logic of paradox **LP**. However, it seems that these results cannot be adduced to help overcome the current difficulty. To begin with, prescriptive fictionalism is committed to a classical reading of the logical vocabulary of mathematical claims because this is presupposed by a Tarskian approach to the semantics of mathematical language, the correctness of which is itself assumed by linguistic realism (see section 1.1). In addition, the proofs of Meyer and Priest address the absolute consistency of PA; they show that there is a sentence in the language of PA such that it is not the case that both it and its negation are true in the finite model described. But what prescriptive fictionalism requires is a proof of the full model theoretic consistency of PA, i.e. that it is not the case that there is a sentence in the language of PA such that both it and its negation are true in the model described; it can be proved that Meyer's and Priest's formalisations of PA are not consistent in this sense.[105] Finally, to suggest that the mathematics used in science is based on a paraconsistent logic of some sort is highly controversial. Paraconsistent logics tolerate inconsistency by allowing the proofs of some sentences to

[104] See also Meyer and Mortensen (1984).

[105] Brady (2006) proves the consistency, in this sense, of a paraconsistent formulation of set theory. However, Brady's proof assumes the model theoretic consistency of a classical analogue of this theory (*op. cit.*, 255). It is thus hard to see what value there would be in appealing to this proof in order to justify a pretence theoretic approach to the paraconsistent theory.

stand together with proofs of their negations. But standard mathematical theories do not tolerate inconsistency, as the history of mathematics shows.[106] For these reasons, it is not plausible to suggest that prescriptive fictionalism can satisfy the consistency condition by appeal to relative proofs of consistency carried out in paraconsistent mathematics.

7.6.1.2 Relative proofs of proof theoretic consistency

Our question was whether prescriptive fictionalism can establish the consistency of the mathematics it views as pretence by appeal to a relative consistency proof. Let us now consider this in connection with the proof theoretic notion of consistency. According to a popular line of thought, Gödel's second theorem shows that the proof theoretic consistency of standard mathematical theories cannot be proved in the theories themselves, that there is no possibility, for example, of real analysis proving its own consistency, or of set theory proving its own consistency. If the theorem does establish this, it suggests that appeal to relative proofs of the proof theoretic consistency of the mathematics used in science will not help prescriptive fictionalism. For if the consistency of a mathematical theory M cannot be proved in itself, it seems unlikely that a proof of its consistency will be forthcoming in a mathematical theory that is epistemologically more manageable than M.

The first question to address here is whether Gödel's result (hereafter "the second theorem") shows that standard mathematical theories do not prove their own proof theoretic consistency (hereafter "consistency"). The second theorem is concerned with formal systems containing primitive recursive arithmetic (PRA) that satisfy certain conditions. These are assumptions about formal systems that hold for the systems that Hilbert

[106] Friedman and Meyer (1992) provide another reason for preferring a classical formulation of arithmetic to Meyer's paraconsistent formalisation; that the former proves interesting arithmetic claims that the latter does not prove. As these claims are true in finite models of the sort Meyer described for his formulation of arithmetic, that formulation is less deductively powerful than we would like.

and others had in mind during their proof theoretic investigations; they are used in Bernays's full proof of Gödel's second theorem.[107] Let F be a formal system containing PRA and satisfying these conditions. By the method of Gödel numbering, sentences of F can be constructed that express claims made in the proof theory of F, in particular, that F is consistent. The second theorem picks one of these sentences, CON_F, and states the following:

If F is consistent, CON_F is not derivable in F.

This is a generalization about a certain kind of formal system. It is not the same as the claim that standard mathematical theories cannot prove their own consistency. So how does one argue from the former to the latter?

To answer this we will consider how the argument runs for the case of set theory (a standard theory such as ZFC). Assume that set theory is consistent and let S be an adequate formalization of set theory. With such a formalization, the consistency of S is stated as follows:

$\forall x \forall y \forall z \neg$(x is a derivation in S of z $\wedge$ y is a derivation of the negation of z)

Picking a system of Gödel numbering, this claim is expressed by a sentence of S, CON_S. S certainly contains PRA, so assuming that S satisfies the derivability conditions, we can apply the second theorem to establish that there is no derivation in S of CON_S. Note that given any system of Gödel numbering, and any properly formulated sentence ϕ about the syntax of S, there is a unique sentence in the language of S there expresses ϕ via that numbering; CON_S is thus the only sentence of S that expresses the consistency of S in canonical form relative to the chosen numbering. There are other sentences of S which express the

[107] It is customary to speak of Gödel's having proved the second theorem. But to the extent that it provides no formulation of the derivability conditions, his (1931) only contains a sketch of the proof,. According to Giaquinto (2002, 258, *n*.4), Bernays later provided a full proof in Hilbert and Bernays (1939).

consistency of S, but these are all formally equivalent to CON_S. So the fact that CON_S cannot be derived in S shows that the consistency of S cannot be derived in S (relative to the system of numbering chosen). In other words, S does not prove its own consistency. To complete the argument we now generalise over adequate formalizations to arrive at our conclusion. This requires a final assumption, that all adequate formalizations of set theory satisfy the conditions necessary for the proof of Gödel's theorems. Making this assumption, we can deduce, from the claim that S does not prove its own consistency, that set theory does not prove its own consistency.

An argument like this could be run for any mathematical theory containing PRA, so the argument suggests that Gödel's second theorem shows that standard mathematical theories do not prove their own consistency. There is, however, a serious challenge to the argument which should be mentioned, to take issue with the assumption that all adequate formalisations of set theory satisfy the derivability conditions. In this regard, Detlefsen (1986) argues on the basis of methods of formalization first described by Rosser (1936) that there may be ways of formalising standard mathematical theories so that the conditions required by the second theorem do not obtain. Let T be a formalisation of a theory using the usual methods of formalisation. To obtain a Rosser formalisation R, it is assumed that Ts proofs have been enumerated so that derivability in R can be defined in terms of derivability in T with an added consistency condition: x is a derivation in R of y if and only x is a derivation in T of y and y is not inconsistent with the conclusions of any derivation z prior to x in the enumeration. Thus we have not only that the second theorem does not apply to Rosser formalized theories, but also that such theories are guaranteed to be consistent. Another kind of formalization for which this situation obtains is described by Feferman (1960). To obtain a Feferman formalization of T, an enumeration of the axioms of T is assumed and derivability in F is defined in a manner parallel to derivability in T. However, axiomhood is defined in such a way as to build consistency into

F: x is an axiom in F if and only if x is an axiom in T and x is consistent with every axiom in T that precedes x in the enumeration.

However, it does not seem likely that the unusual methods of formalisation suggested by Feferman and Rosser could be used to escape the force of the argument from the second theorem. For this would require that we have clear, independent reasons for preferring them to the usual methods. But there do not seem to be such reasons. In fact, the clear and independent reasons available here seem to favour the usual methods, for, as Giaquinto (2002) observes, neither Feferman's nor Rosser's method leads to adequate formalizations of mathematical theories. In Feferman-style formalisations the question whether a given formal object is a derivation is not effectively decidable (*op.cit.*, 187). In Rosser-style formalizations the structural assumptions governing proof differ from those of ordinary mathematical practice, in particular, the cut rule fails for them (*op.cit.*, 188).[108] It is thus not plausible to claim that formal systems arrived at by these methods represent mathematical deductive practice more faithfully than systems arrived at by the usual methods.

What this shows is that, given the methods of formalisation available at present, it is not possible to avoid the second theorem by appeal to unusual formal systems. So for the time being, the answer to our question whether the second theorem shows that standard mathematical theories do not prove their own consistency is that it does. The question we must now address is what significance this has for the project under consideration here, the project of defending a pretence theoretic approach to the mathematics used in science by proving its consistency in epistemologically more manageable mathematical theories.

The result is a major blow. Set theory is both used in science and contains all other standard mathematical theories by translation. It is

[108] This is the rule allowing one to take previously established theorems as premises in proofs. Note that, without it, proofs would always have to take axioms as premises, and so would become unmanageably long.

therefore not possible to step outside such a theory to establish its consistency on proofs that it does not contain. Thus pretence theoretic fictionalism cannot defend a pretence theoretic approach to set theory by appeal to a relative consistency proof in epistemologically weaker mathematics, for there seem to be no such proofs.

This is a bad result for prescriptive fictionalism, and it is already enough to suggest that the project of defending it by appeal to relative proofs of proof theoretic consistency is not a good one. Perhaps, though, the pretence theoretic approach might still be applied to mathematical theories less extensive than set theory, like arithmetic or real analysis. These theories do not contain or translate all standard mathematical methods, so the possibility of relative proofs of consistency for these theories that are useful to the project is not ruled out by the argument from the second theorem.

Without claiming to have a knockdown argument against this possibility, it does seem reasonable to maintain that various developments in proof theory suggest that it, too, will be foreclosed. It is difficult to approach this issue because the conditions under which one theory is epistemologically more secure than another are unclear. But let us assume that a mathematical theory is epistemologically more secure than another only if it is not as proof theoretically strong. Proof theoretic strength is itself an imprecise concept. But when it is made more precise, one finds that formal theories can be thought of as being ranked according to proof theoretic strength, such that theories lower down the ranking do not prove the consistency of theories higher up.

Two themes in proof theory suggest this way of thinking. Gentzen (1936) proved that the consistency of PRA could be established in PRA combined with the principle of transfinite induction up to ε_0. This result gave birth to the field of ordinal analysis, one of the main branches of proof theory, in which the proof theoretic strength of a formal system is measured by its ordinal number, that being the least ordinal, α, such that transfinite induction up to α suffices for a proof of the consistency of the system. This leads naturally to thinking of formal systems as ranked according to proof theoretic strength, such that formal system F_1 appears at a later stage than formal system F_2 if and only if the ordinal of F_1 is greater than the ordinal of F_2.

The second theme in proof theory that suggests this approach concerns the results on proof theoretic reducibility described in Feferman (1993b). F_1 is proof theoretically reducible to F_2 relative to a recursively enumerable class of formulas that they both contain (which includes at least PRA) if F_2 proves every sentence from the shared class which is proved in F_1 and if F_2 proves that this is the case.[109] Feferman discusses many examples of interesting proof theoretic reductions. However, for us the important point is that not every formalised mathematical theory is proof theoretically reducible to every other. As was the case with ordinal analysis, the established results on proof theoretic reduction ~~body~~ suggest thinking of formal systems as ranked according to proof theoretic strength; this time we can think of formal system F_1 as being higher in rank than a formal system F_2 to which it can be proof theoretically compared if and only if F_2 is proof theoretically reducible to F_1.

These avenues of research in proof theory do not reveal one unique method of arranging formal systems in terms of proof theoretic strength, preferable to all others. Rather it makes clear that we have various precise notions of proof theoretic strength according to which we might make such arrangements. But the expectation to which these results give rise is that however one refines the notion of proof theoretic strength, one will always find that the consistency of a formal mathematical theory can only be proved in a formal mathematical theory that is proof theoretically stronger. Given our assumption that in mathematics proof theoretic strength is a guide to the epistemological security of a theory, the current

[109] For a more formal definition of proof theoretic reducibility, see Feferman (1993b, 6).

proposal for prescriptive fictionalism requires consistency proofs in theories that are proof theoretically weaker than those whose consistency is to be proved. It thus looks implausible even for mathematical theories of moderate extensiveness, such as PA or real analysis.

These remarks are somewhat impressionistic and we must stress again that we do not claim to have to have shown that it is impossible for prescriptive fictionalism to ground knowledge of the consistency of the mathematics used in science on relative consistency proofs. But we do claim to have raised very serious doubt about whether this is possible. We claim that Gödel's second theorem does show that standard mathematical theories do not prove their own consistency. We claim that this shows that prescriptive fictionalism cannot establish the consistency of very extensive mathematical theories like set theory on relative proofs of proof theoretic consistency. And we claim that further research in proof theory suggests that this approach will not work for less extensive mathematical theories either. On these grounds, we conclude that the thought that prescriptive fictionalism can satisfy the consistency condition by appeal to relative proofs of proof theoretic consistency is not plausible.

This conclusion should now be placed alongside the conclusion of the previous section, that the consistency condition cannot be met by appeal to relative proofs of proof theoretic consistency. It should also be placed alongside the conclusion of section 6.5.2, which, as we argued above, suggests that there are not adequate non-mathematical grounds for knowledge of the consistency of the mathematical theories used in science. Collectively, these results suggest that pretence theoretic fictionalism cannot satisfy the consistency condition. So if prescriptive fictionalism is successfully to deal with the condition, it must do so in some other way.

7.6.2 Rejecting the consistency condition

In the previous section we argued that prescriptive fictionalism cannot usefully establish knowledge of the consistency of mathematics on relative consistency proofs. In Chapter 6, we argued that it is not clear there are non-mathematical grounds for such knowledge. If these arguments are correct, they leave us with no reason to be confident that pretence theoretic fictionalism can meet the consistency condition. So unless it can be argued that prescriptive fictionalism is not obliged to meet the condition, we will have to conclude that it is not a plausible view. We should thus address the question of whether prescriptive fictionalism is obliged to establish the consistency of the mathematics it treats as pretence.

The suggestion that prescriptive fictionalism might have to satisfy the consistency condition comes from its comparison to the views of Hilbert and Field. But the comparison only raises the possibility that prescriptive fictionalism requires satisfaction of the consistency condition, it does not establish that this is the case. It is therefore a good idea to find out what made Hilbert and Field demand satisfaction of the consistency condition, to see if their grounds provide reason to impose the same requirement on prescriptive fictionalism. We will first consider Hilbert's reasons for imposing the consistency constraint.

Hilbert distinguished between finitary and non-finitary mathematical claims. For our discussion, the nature of the distinction is not important. But it will aid exposition to use the terms as labels for the mathematical claims Hilbert construed instrumentally (non-finitary claims) and the mathematical claims he construed non-instrumentally (finitary claims). For reasons we need not go into here, finitary claims do not obey certain logical principles whose use is common in mathematical deductive practice (such as the law of excluded middle). If mathematics were to avail itself only of finitary claims it would thus have to abandon use of some of its most important logical methods. Hilbert was not prepared to allow this. But how could the use of these logical methods be justified?

> Let us remember that *we are mathematicians* and as such have already often found ourselves in a similar predicament, and let us recall how the method of ideal elements, that creation of genius, then allowed us to find an escape. I presented some shining examples of the use of this method at the beginning of my lecture. Just as $i = \sqrt{-1}$ was introduced so that the laws of algebra, those, for example, concerning the existence and number of the roots of an equation, could be preserved in their simplest form, just as ideal factors were introduced so that the simple laws of divisibility could be maintained even for algebraic integers (for example, we introduce an ideal common divisor for the numbers 2 and $1 + \sqrt{-5}$, while an actual one does not exist), so we must here *adjoin the ideal propositions to the finitary ones* in order to maintain the formally simple rules of ordinary Aristotelian logic. (1925, 379)

Here the "ideal propositions" can be taken to be the non-finitary claims.[110] What the passage shows is that, on Hilbert's view, the introduction and use of non-finitary statements is to be thought of as an application in proof theory of the method of ideal elements. This is taken to be a method of mathematics whose use is exhibited in familiar and important episodes in the history of mathematics.

The reason Hilbert explicitly gives for imposing the consistency condition on his version of instrumentalism is connected to this use of the method of ideal elements:

[110] In fact, Hilbert regarded even some finitary claims as ideal elements introduced to make a simple logic available: "Besides these elementary propositions [numerical equalities etc.], which are of an entirely unproblematic character, we encountered finitary propositions of problematic character, for example, those that were not decomposable [into partial propositions]." (1925, 380). The reason for this is that the range of applicability of the usual rules of classical logic (the rules whose use Hilbert wanted to preserve) is not given precisely by the epistemological distinction between finitary and non-finitary statements. However, on our use of the terms it is perfectly acceptable to take the ideal statements as the non-finitary statements.

> To be sure, one condition, a single but indispensable one, is always attached to the use of the method of ideal elements, and that is the proof of consistency; for, extension by the addition of ideal elements is legitimate only if no contradiction is thereby brought about in the old, narrower domain, that is, if the relations that result for the old objects whenever the ideal objects are eliminated are valid in the old domain. (1927, 471)

It thus appears that Hilbert is arguing as follows: applications of the method of ideal elements must be backed up by proofs of the consistency of the ideal elements introduced, non-finitary claims can be thought of as having been introduced as ideal elements via this method; thus it must be proved that they constitute a consistent body of claims.

Does this argument provide reason to demand a consistency proof of the mathematics pretence theoretic fictionalism construes as pretence? Clearly it does not. Prescriptive fictionalism does not seek to justify the introduction and use of object-committed mathematical claims as an application of the method of ideal elements, or, indeed, as an application of any other mathematical method. Rather it proposes that object-committed mathematical claims are introduced as elements of pretence designed to help express and manipulate information about non-mathematical phenomena. So even if the use of the method of ideal elements must be supported, in each instance of use, with a proof of the consistency of the ideal statements introduced, this does not provide reason for imposing the consistency condition on prescriptive fictionalism.

However, considerations relevant to the method of ideal elements were not Hilbert's only reasons for demanding satisfaction of the consistency condition. Another arose from the aim of his project:

> With this new way of providing a foundation for mathematics, which we may appropriately call a proof theory, I pursue a significant goal, for I should like to eliminate once and for all

> the questions regarding the foundations of mathematics, in the form in which they are now posed, by turning every mathematical proposition into a formula that can be correctly exhibited and strictly derived, thus recasting mathematical definitions and inferences in such a way that they are unshakeable and yet provide an adequate picture of the whole science. I believe that I can attain this goal completely with my proof theory, even if a great deal of work must still be done before it is fully developed. (1927, 464)

For Hilbert, the main question for the foundations of mathematics was how we could be confident of using the methods of classical mathematics without fear of paradox. He could not accept that the lot of mathematics was to lurch from one foundational crisis to another, as had already happened as mathematicians had successfully dealt with contradictions in analysis only for contradictions to appear again in set theory. To stop this from happening Hilbert resolved to establish beyond doubt and in advance that the methods of classical mathematics would not lead to paradox. His method was to formalize those methods and then to prove the consistency of the formal system in which they were formalized. So a further reason Hilbert required a consistency proof for non-finitary mathematics was that he wanted to banish all fears of paradox from the use of classical mathematics.

Prescriptive fictionalism, however, has a more modest aim. It seeks to establish a satisfactory philosophical account of what passes for mathematical knowledge consistent with the basic assumptions of this study and the criticisms of realist views set out in previous chapters of this study. It does not seek to guarantee the future certainty of mathematical reasoning. Therefore prescriptive fictionalism is not required to satisfy the consistency condition as a means to achieving this more ambitious aim. This, of course, does not mean that it can ignore the question of the reliability in applications of the mathematics it construes as pretence, as Hilbert could not ignore the question of why non-finitary mathematical reasoning was reliable in finitary mathematics. But we have already said how prescriptive fictionalism responds to this.

Let us now turn to the reasons for which Field's position is committed to satisfaction of the consistency condition. As we explained above, the consistency condition for Field's view derives from the more stringent conservativeness condition, the demand that the conservativeness of mathematical over nominalistic science be established. What we must find out is therefore whether the reasons for which the conservativeness condition must be met by Field's view lead to reasons for which the consistency condition must be met by prescriptive fictionalism.

We saw in Chapter 6 that Field's instrumentalism uses the dispensability of mathematics to science and the conservativeness of mathematics over nominalistic science to establish the reliability of mathematical reasoning. Here is a simplified reminder of how this was supposed to go. Let P be one of our mathematical scientific theories. Then by the dispensability and conservativeness claims there is a nominalistic theory N and a mathematical theory M such that M is conservative over N and N + M (the conjunction of N and M) has the same nominalistically expressed consequences as P. Let ϕ be a nominalistically expressed claim for which there is a mathematical argument in P. Since P and N + M have the same nominalistically expressed consequences as P, ϕ is a nominalistically expressed consequence of N + M. Then, since M is conservative over N, ϕ is a nominalistically expressed consequence of N alone. Thus the mathematical arguments available in P do not allow us to draw conclusions we could not have drawn without mathematics; so the use of mathematics in P is reliable.

This argument for the reliability of mathematical reasoning is not sound unless mathematical science is conservative over nominalistic science. This is why Field's position is committed to meeting the conservativeness condition (as mathematical science is conservative over nominalistic science only if mathematics is consistent). If this is to lead to a reason why prescriptive fictionalism must meet the consistency condition, then prescriptive fictionalism must rely on a similar argument for which the consistency of mathematics is a premise. But it does not. Prescriptive fictionalism does not presuppose either that mathematics is dispensable to science, or that mathematical science is conservative over nominalistic science. So it does not rely on this argument to establish the reliability of mathematical reasoning. Moreover, it does not rely on a similar argument in which the consistency of mathematics is a premise. As we explained above, prescriptive fictionalism proposes a teleological explanation of the reliability of mathematical reasoning. On this view, the standards of correct mathematical acceptance are themselves thought of as products of a kind of evolutionary testing; if the standards ratify as correct the acceptance of world directedly misleading claims, they will be modified when this comes to light so that it is no longer the case. Conclusions with world directed content that have been shown to be correct according to the standards of correct acceptance are thus reliable because they have been ratified by standards selected in this way. Clearly, the claim that mathematics is consistent is not a premise of this argument.

It would appear, then, that the reasons for which Hilbert's and Field's views are committed to satisfying the consistency condition do not show that it must also be satisfied by prescriptive fictionalism. However, we cannot conclude that this commitment does not arise unless we are also sure that none of the claims made by prescriptive fictionalism presuppose the consistency of object-committed mathematics. There are, perhaps, two reasons for thinking that such a commitment does arise.

The first putative reason is that prescriptive fictionalism attributes to considerations of consistency an important role in its conception of mathematics as an area of enquiry. One might say that it views the production of consistent mathematics as a goal of mathematical enquiry. Doesn't this show that it is necessary to establish the consistency of object-committed mathematics, as the consistency condition requires? It does not. Whilst it is a goal of mathematics to produce consistent mathematics, consistency considerations enter the standards of correct acceptance only in the sense that when some mathematics is known to be inconsistent it cannot be correctly accepted. If we aim to accept only those mathematical claims that may be correctly accepted we will thus resolve to reject mathematics in which inconsistencies are discovered. However, this does not commit one to showing that a piece of mathematics is consistent before one can correctly accept it. So this does not give reason to think that the consistency condition must be met.

The second putative reason for thinking that a commitment to satisfaction of the consistency condition arises from the claims of prescriptive fictionalism has to do with rationality. Prescriptive fictionalism maintains that we should believe in the world directed content of sentences that have been shown to follow from mathematical theory. Suppose, then, that S is an object-committed mathematical claim with world directed content. Then if both S and ¬S are consequences of correct mathematical theory, we should believe the world directed contents of them both. But these contents will be contradictory. Since it is never rational to form contradictory beliefs, prescriptive fictionalism must therefore show that this situation does not arise. Thus, it might be argued, prescriptive fictionalism must establish that the mathematical theories used in applications are consistent.

However the argument contains a mistake. Prescriptive fictionalism does indeed claim that we should believe in the world directed content of sentences that are shown to follow from acceptable mathematics. But it also claims that if a mathematical theory is shown to be inconsistent, it is to be amended so that the proof of at least one of the inconsistent sentences is no longer considered to be correct. In the situation described in which both S and ¬S are shown to follow from a mathematical theory, this very fact establishes that the theory from which they are derived is not acceptable. Thus it is not necessary to rule out the possibility of this situation arising by establishing the consistency of the mathematical theories used in science: if inconsistency emerges, we are required to reject the theory, not to form contradictory beliefs.

A dialectical approach to the consistency of mathematics emerges from this response, according to which the claim that mathematics is consistent is an assumption of mathematical practice. On this approach, we do not, strictly speaking, know that the mathematics used in science is consistent. Rather we assume that it is consistent. If we find that it is not, we revise the mathematics so that the arguments we know about that terminate with inconsistent claims are blocked, taking care to revise in ways that we feel are not likely to lead to new inconsistencies. We then assume that the revised mathematics is consistent.[111] Our undertakings never to accept a piece of mathematics known to be inconsistent, and always to revise inconsistencies away, are what license the assumption of consistency. So on this dialectical approach, the assumption that mathematics is consistent does not need the support of grounds adequate to its being known.

One last concern might be that prescriptive fictionalism cannot even assume the consistency of mathematics, since to do so would commit it to the existence of mathematical objects such as models or proofs. However, it may be possible to deal with this by appealing to the modal notion of consistency we discussed in section 6.5 (Field's notion), according to which a theory is consistent when it is possible that its basic principles are jointly true. In section 6.5, we argued that it is not clear there are nominalistically acceptable grounds for knowledge of the modal consistency of mathematics, but clearly this does not stop us from appealing to a modal notion of consistency in order to assume that mathematics is consistent. This problem can thus be overcome by appeal to Field's modal notion of consistency, provided that notion is meaningful.

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[111] Of course the fact of human error means that there are inconsistencies in mathematics as it is practised. But the assumption of consistency concerns an idealised version of mathematics, purged of human error.

It thus appears that the consistency condition can be dealt with by prescriptive fictionalism. Hilbert's and Field's reasons for thinking that knowledge of the consistency of mathematics must be established do not apply, and it does not seem that this is required by anything prescriptive fictionalism says. The fact that we apparently lack adequate non-mathematical grounds for knowledge of the consistency of mathematics thus does not present a difficulty for prescriptive fictionalism as it can appeal to the modal notion of consistency and take advantage of the dialectic approach to consistency recently described. Accordingly, prescriptive fictionalism takes the modal consistency of mathematics as a defeasible assumption of mathematical practice.

7.7 Conclusion

This chapter addressed the question of whether mathematical fictionalism can meet the challenge from applications without assuming that mathematics is dispensable to science, by arguing that the roles mathematics has in science are played in other contexts by acknowledged forms of pretence. The point of introducing prescriptive fictionalism was that it promised to do just that.

Prescriptive fictionalism puts forward a conception of mathematics as an area of enquiry in which the acceptance of mathematical claims is treated differently from the way it would be treated on realist approaches. This conception states that A accepts an object-committed mathematical claim *p* if and only if (a) A is prepared to use *p* in empirical applications and (b) A is not prepared to assert or endorse the statement that *p* is true (section 7.2.1). It states that a mathematical claim may be correctly accepted if and only if it is known to be an axiom of, or to have been validly deduced from, a mathematical theory that is not known to be inconsistent but is known to be applicable (section 7.2.2). This conception of mathematics as an area of enquiry is intended prescriptively, as an account of how we ought to engage with object-committed mathematical claims.

Prescriptive fictionalism also proposes a (prescriptive) account of the pragmatics of mathematical language. According to this account, sentences expressing object-committed mathematical claims should be used as if they were believed to be true but in the absence of such belief (section 7.3). Prescriptive fictionalism thus arrives at a conception of mathematics as a form of pretence. It then argues that, from the perspective of this conception: (a) the descriptive uses in science of object-committed mathematical sentences can be viewed as world-directed utterances of propositions that are not committed to mathematical objects (section 7.3.1); (b) the reliability of mathematical reasoning in science can be viewed as a consequence of the fact that the standards of correct mathematical acceptance evolve under the constraint of applicability (section 7.3.2). In this way, the challenge from applications is met.

After setting out prescriptive fictionalism, we considered possible objections to it. But having considered Stanley's objections to pretence invoking versions of fictionalism (section 7.4), Burgess's objections to fictionalist approaches to mathematics (section 7.5) and the possibility that prescriptive fictionalism might bear objectionable commitments to the consistency of mathematics (section 7.6), we have found no reason to dismiss it.

Prescriptive fictionalism thus appears to meet the challenge from applications and is strong enough to withstand sustained criticism. This does not mean that there is nothing wrong with the view, nor does it demonstrate that we should adopt it forthwith. But it gives sufficient grounds for a cautious optimism regarding its future prospects. The possibility thus remains open of a fictionalist response to the challenge from applications that argues that the roles mathematics has in science can be played by pretence.

8

Conclusion

We have now reached the end of the programme of study set out in our introduction. Let us take stock of the conclusions we have reached, to assess their significance and to indicate directions for future research.

8.1 Summary

The question with which we began was that of how we should conceive of what passes for mathematical knowledge ("apparent mathematical knowledge"). We made two basic assumptions (section 1.1 and section 1.2). Linguistic realism states that:

> (i) mathematical language should be treated classically and referentially,
> (ii) mathematical language should be taken at face value when used rigorously to express mature mathematical theories.

The amended Quinean criterion of ontological commitment states that:

> A theory is committed to the instantiation of all and only those kinds of thing, to instances of which its bound variables must be capable of referring if it is to be true.

We argued that there are two plausible approaches to apparent mathematical knowledge within these constraints. Mathematical realism states that there are independently existing mathematical objects and treats apparent mathematical knowledge as knowledge of such things (section 1.3). Mathematical fictionalism rejects belief in mathematics, stating that mathematics is, of should be, a form of pretence (section 1.4). Each approach faces its epistemological challenge: mathematical realism must make plausible that we are, or could be, in possession of adequate grounds for belief in mathematical knowledge; mathematical fictionalism must convince us that the roles mathematics has in empirical applications can be performed by pretence.

The project we set ourselves was to find out if these challenges can be met (section 1.5). Chapters 1 and 2 dealt with rationalist approaches to mathematical knowledge, which claim that mathematical knowledge is supported by grounds provided by the intellect, independent of the evidence sense perception. Chapters 3 and 4 dealt with empiricist approaches to mathematical knowledge, according to which its ultimate grounds are provided by sense perception. Chapters 6 and 7 addressed fictionalist strategies for meeting the challenge from applications. In each chapter we described and assessed the chances of success for a particular strategy for meeting one of the challenges. Here is a summary of the main arguments made.

Chapter 2

This dealt with the rationalist strategy of arguing that mathematics is analytic, i.e. knowable on grounds provided by conceptual analysis. We considered first the Neo-Fregeanism of Wright and Hale (section 2.3). Following Boolos, we argued against the analyticity of Frege Arithmetic, the second order classical theory in which Hume's Principle is the only non-logical axiom, on the grounds that it proves the existence of cardinal numbers whose existence conflicts with ZF (section 2.4). We claimed that this shows that Hume's Principle is not true, hence not analytic, and that the conflict with set theory is incompatible with the Neo-Fregean programme for mathematics in general. We then considered Tennant's free logic alternative to Frege Arithmetic (section 2.5). We argued that the rule of 0-introduction and the ratchet principle upon which it is based are neither stipulatively valid nor formulable from conceptual analysis, and thus not analytic, because they bear ontological commitments (section 2.6). Finally, we considered Rumfitt's attempted resuscitation of Tennant's approach by appeal to the general necessary and sufficient conditions for cardinal existence allegedly given by his principle C (section 2.7). As with the ratchet principle and 0-introduction, we argued that its ontological commitments ruled against the analyticity of principle C. Moreover, we argued that the fact that principle C does not allow non-well-orderable sets to have cardinal numbers suggests that it is neither formulable from conceptual analysis of the notion of cardinal number nor stipulatively true.

Chapter 3

This addressed the rationalist strategy of arguing that mathematics can be known on grounds provided by a faculty of intuition. We considered first the possibility of positing a faculty of mathematical intuition, issuing in intuitions *of* mathematical objects. Taking Gödel's account of set theoretic knowledge as an example of this approach (section 3.2), we argued that Gödel's remarks on mathematical intuition are too sketchy to provide us with a clear understanding of how we are supposed to intuit sets (section 3.3). Next we considered the possibility of positing a faculty of rational intuition, issuing in intuitions *that* things are thus and so with mathematical objects. We considered Katz's account of knowledge in the formal sciences as an example of this approach (section 3.5), but argued that, because Katz fails to provide any explanation of the nature and constitution of rational intuition, he provides no convincing reason to think that we possess such a faculty (section 3.6). Thereafter we argued that our central point against Katz's position, that the cognitive sciences do not provide anything like a constitutive account of what the faculty of intuition might be like, applies quite generally to any account of mathematical knowledge that appeals to a faculty of intuition, regardless of the kind of intuition involved (section 3.7). We concluded that this strategy does not promise a satisfactory realist theory according to which mathematical knowledge is ultimately secured on the deliverances of intuition.

Chapter 4

Chapter 4 assessed the empiricist strategy of arguing that mathematical knowledge is ultimately supported by sense perception of mathematical objects. We considered Maddy's account of set theoretic knowledge as an example of this approach (section 4.2). We defended her theory of perception as a collection of sufficient conditions for perception of objects as instances of given kinds (section 4.3.1) and defended her claim that sets are perceivable against objections from Balaguer (section 4.6) and Chihara (section 4.7). But taking our cue from some of Chihara's observations, we rejected Maddy's set theoretic empiricism, on the grounds that its claim that sets of physical objects are located in space and time conflicts conceptually with the axioms of set theory (section 4.8). Moreover, we argued that this conclusion threatens any view that, like Maddy's, takes mathematical objects to be on a metaphysical and epistemological par with ordinary physical objects like chairs and tables (section 4.9). However, we did not claim that it necessarily provides reason to reject views that assimilate the nature and knowledge of mathematical objects to that of properties of ordinary physical objects, like redness. Accordingly our conclusion was that the empiricist strategy of arguing that mathematical knowledge can be grounded on sense perception could not succeed on Maddy's approach, but may yet succeed on this other approach.

Chapter 5

Chapter 5 examined the empiricist strategy of arguing that sense perceptions of ordinary physical objects provide evidence for mathematical beliefs. We considered first Quinean realism, which argues that because standard mathematics is indispensable to science it is confirmed by scientific evidence (section 5.1). Defending this against Maddy's objections from scientific and mathematical practice (section 5.3 and section 5.4), we conceded that they raise valid concerns about the Quinean view; first, whether it can be extended by a methodological theory that satisfactorily explains the facts of practice and, second, whether it can successfully explain the confirmation of dispensable mathematics. We considered Resnik's development of Quinean realism because it promised to meet these concerns (section 5.5). We were impressed by the way it explained the facts of practice (sections 5.6.1 and 5.6.2). However, we argued that considerations from Resnik's local conception of mathematical evidence would either be constrained not to ratify dispensable mathematics as true, or would not be genuinely evidential with respect to dispensable mathematics, and concluded that Resnik's view does not deliver scientific confirmation for dispensable mathematics (section 5.6.3). Consequently, we argued by appeal to results from the foundations of mathematics that it is unclear that either Quinean realism or Resnik's view provides adequate grounds for knowledge of impredicative mathematics, including central mathematical theories like real analysis and impredicative set theory (section 5.6.3). We concluded that we do not have good reason to think that mathematical knowledge quite generally is confirmed by scientific evidence, but conceded that this approach may satisfactorily account for knowledge of mathematics that has widespread indispensable applications (section 5.7).

Chapter 6

This chapter addressed the fictionalist strategy of arguing that mathematics is a convenient but in principle unnecessary instrument in empirical applications. We took Field's programme for the nominalization of science as an example of this approach (section 6.3). We dismissed Hale and Wright's objection that Field's logical notion of possibility should be rejected (section 6.5.1) but argued that it is unclear there are nominalistically acceptable grounds for knowledge of the modal consistency of standard mathematical theories (section 6.5.2). We then defended Shapiro's argument that, when Field's programme is pursued against the background of first order logic, if mathematics is conservative, the representation theorems Field requires cannot be proved (section 6.6). We also considered Resnik's objections to the nominalistic acceptability of Field's reformulations of field theories in flat space-time, arguing that there is not adequate evidence for the assumption that there are uncountably many concrete space-time points (section 6.7). Observing that these points show there are serious difficulties with every aspect of Field's position, we concluded that the prospects are not good for fictionalist accounts of mathematics based on the view that mathematics can be eliminated from science (section 6.8).

Chapter 7

Chapter 7 evaluated the fictionalist strategy of arguing that the roles mathematics has in empirical applications can be played by acknowledged forms of pretence. After setting out prescriptive fictionalism (section 7.1), we argued that it can satisfactorily address the challenge to fictionalism applications by putting forward a conception of mathematics as pretence.

Our argument was that, from the perspective of this conception: (a) the descriptive role of object-committed mathematical sentences in empirical applications can be viewed as world-directed utterances that are not committed to mathematical objects (section 7.3.1); (b) the reliability of mathematical reasoning can be viewed as a consequence of the fact that the standards of correct mathematical acceptance evolve under the constraint of applicability (section 7.3.2). Despite considering Stanley's critique of pretence invoking versions of fictionalism (section 7.4), Burgess's objections to fictionalist approaches to mathematics (section 7.5) and the possibility that prescriptive fictionalism might bear objectionable commitments to the consistency of mathematics (section 7.6), we found no reason to dismiss it. As these results suggest that prescriptive fictionalism adequately meets the challenge from applications, we concluded that there are grounds for cautious optimism regarding its future prospects (section 7.7).

8.2 Discussion

Of what significance are these conclusions for our original question of how we should conceive of apparent mathematical knowledge? To answer this we will first describe the consequences they have for our main questions, those concerning whether the epistemological challenges facing mathematical realism and mathematical fictionalism can be met. Then, before closing, we will describe the bearing our results have on some other areas of concern to the epistemology of mathematics and point out some emerging questions for future consideration.

8.2.1 The epistemological challenges to realism and fictionalism

The epistemological challenge to mathematical realism was that it make plausible that we are, or could be, in possession of adequate grounds for what passes for mathematical knowledge. Our results show that there is no plausible rationalist response to this challenge. They suggest that two out of the three empiricist approaches available are unsatisfactory but they provide no support for thinking that the third will succeed. The third strategy hopes to account for mathematical knowledge by adopting some version of the view that mathematical objects are on a metaphysical and epistemological par with properties of ordinary physical objects. We must therefore draw the following conclusion: it is possible that a satisfactory response to the epistemological challenge to realism may be got from this strategy, but no other approach is adequate.

The epistemological challenge to mathematical fictionalism was that it make plausible that the roles apparent mathematical knowledge has in empirical applications can be performed by pretence. Our results suggest that it is possible, and to a certain extent plausible, that prescriptive fictionalism adequately meets this challenge. So we may tentatively conclude that the epistemological challenge to mathematical fictionalism can be met.

8.2.2 Some consequences for other areas of interest

It is interesting to note some consequences our results have for disputes in the epistemology of mathematics going beyond whether the epistemological challenges to realism and fictionalism can be met. One such dispute concerns whether we possess apriori knowledge of mathematical objects, knowledge that does not depend on the grounds of sense experience for its ultimate justification. Having dismissed all but one empiricist approach to the challenge to mathematical realism, we can conclude that we do not possess such knowledge.

Another dispute of interest concerns whether knowledge of abstract objects is possible. Having conceded that Quinean realism may provide an adequate account of knowledge of claims about mathematical objects that have widespread indispensable applications, we have some reason to think that the indispensability to science of a claim about abstract objects may in certain cases be sufficient ground upon which to claim that it is known. We can thus reject the view that knowledge of abstract objects is not possible.

A third area of interest concerns whether it is possible to explain the applicability of mathematics. From the perspective of prescriptive fictionalism, this can be viewed as a consequence of the fact that mathematics is designed to be applicable, with standards of correct acceptance that evolve under the constraint of applicability. Since prescriptive fictionalism is the best account of mathematics we have considered, we can tentatively conclude that it is possible to explain the applicability of mathematics.

Our investigations also throw up further questions to address: Is a satisfactory response to the epistemological challenge to mathematical realism to be had by treating knowledge of mathematical objects as on a par with properties of physical objects? To what extent may we appeal to pretence theoretic pragmatics in other areas like mathematics, where apparent ontological commitments lead to epistemological difficulties? What are our ultimate grounds for the logical knowledge involved mathematics?

Such questions leave us with plenty to be going on with. But since our concern has been with finding out whether the epistemological challenges to realism and fictionalism can be met, it is worth pointing out a line of enquiry that may attractively integrate the conclusions we have reached on those issues. A presupposition of our discussions has been that it is desirable to find an account of the object-committed fragment of what passes for mathematical knowledge either from the point of view of realism or from that of fictionalism. But if we abandon this presupposition, the possibility arises of a mixed view that takes advantage of both perspectives. Some of our conclusions help to sketch the rough outline of such a view. We have suggested that Quinean realism may provide an adequate account of mathematics with widespread indispensable applications, that predicative mathematics is indispensable to science and that prescriptive fictionalism is an adequate version of mathematical fictionalism. One naturally wonders, then, whether it might be possible to regard predicative object-committed mathematics from the point of view of Quinean realism and impredicative object-committed mathematics from the point of view of prescriptive fictionalism. This is not to say, of course, that a mixed view of this kind will withstand philosophical examination. But it seems to be an appealing possibility, and interesting enough to merit further investigatation.

8.2.3 Close

Giving up the editorial "we", I'll end with an autobiographical note. When I began work on this study, I had a hunch that a realist approach to mathematics could be sustained quite generally. It was going to be a "pluralist Platonism" taking advantage of several realist strategies for accounting for knowledge of mathematical objects and resisting any urge to take knowledge of set theory as an epistemological foundation. The more I thought about this, however, the less likely it seemed that mathematical realism could be sustained in full generality; one by one, the various realist strategies fell short of requirements. Having initially not considered the fictionalist approach, I was thus forced to examine it in more detail and was surprised to find myself impressed with its credentials. Indeed, for a while I expected to defend fictionalism as a general account of what passes for knowledge of mathematical objects. Now as it happens, I have in my discussion of prescriptive fictionalism defended the possibility of such a view, but this might be another staging post on my journey and not its final destination. Just where that will be, I cannot with any certainty predict. Let the arguments lead the way.

References

Azzouni, J.
(1994) *Metaphysical Myths, Mathematical Practice: The Ontology and Epistemology of the Exact Sciences* (Cambridge: Cambridge University Press)
(1998) "On "On What There Is"," *Pacific Philosophical Quarterly* 79: 1-18
(2000) "Stipulation, logic, and ontological independence", *Philosophia Mathematica* (3) 8: 225-243
(2004) *Deflating Existential Consequence: A Case for Nominalism* (Oxford: Oxford University Press)

Bach, K.
(1994) "Conversational implicature", *Mind and Language* 9: 124-162

Balaguer, M.
(1996a) "Towards a nominalization of quantum mechanics", *Mind* 105: 209-226
(1996b) "A fictionalist account of the indispensable applications of mathematics", *Philosophical Studies* 83: 291-314
(1998) Platonism *and* Anti-Platonism *in Mathematics* (New York: Oxford University Press)

Baron-Cohen, S., Leslie, A.M. and Frith, U.
(1985) "Does the autistic child have a "theory of mind"?" *Cognition* 21: 37-46

Benacerraf, P.
(1973) "Mathematical truth," reprinted in Benacerraf and Putnam (1983): 403-420. Originally published in *The Journal of Philosophy* 70: 661-679

Benacerraf, P. and Putnam, H. (eds.)
(1983) *Philosophy of Mathematics: Selected readings,* second edition (Cambridge: Cambridge University Press)

Bernays, P.
(1958) *Axiomatic Set Theory* (Amsterdam: North Holland Publishing Company)

Bigelow, J.
(1988) *The Reality of Numbers: A Physicalist's Philosophy of Mathematics* (Oxford: Clarendon Press)

Boghossian, P
(1996) "Analyticity reconsidered", *NOÛS* 30: 360-391

Boolos, G.
(1971) "The interative conception of set", reprinted in Benacerraf and Putnam (1983): 486-502. Originally published in *The Journal of Philosophy* 68: 215-231
(1984) "To be is to be the value of a variable (or to be some values of some variables)", *The Journal of Philosophy* 81: 430-449
(1985) "Nominalist platonism", *The Philosophical Review* 94: 327-344
(1997) "Is Hume's Principle analytic?", Heck (1997): 245-261

Bostock, D.
(1974) *Logic and Arithmetic: (1) Natural Numbers* (Oxford: Clarendon Press)
(1979) *Logic and Arithmetic: (2) Rational and Irrational Numbers* (Oxford: Clarendon Press)

Brady, R.
(2006) *Universal Logic* (Stanford: CSLI Publications)

Brouwer, L.E.J.
(1949) "Consciousness, philosophy and mathematics", Benacerraf and Putnam (1983): 90-96. Excerpt from E. Beth, H. Pos and J. Pollack (eds.) *Proceedings of the Tenth International Congress of Philosophy, Vol. I, Part 2* (Amsterdam: North-Holland): 1243-1249.

Burgess, J.
(1984) Review of Wright (1983), *The Philosophical Review* 93: 638-640
(2004) "Mathematics and *Bleak House*", *Philosophia Mathematica* (3) 12: 18-36

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

Butterworth, B.
(1999) *The Mathematical Brain* (London: MacMillan)

Cantor, G.
(1932) *Gesammelte Abhandlungen mathematischen und philosophischen Inhalts*, E Zermelo (ed.) (Berlin: Springer)

Carnap, R.
(1952) "Meaning postulates", reprinted in Carnap (1956): 222-229. Originally published in *Philosophical Studies* 3: 65-73
(1956) *Meaning and Necessity* (Chicago: The University of Chicago Press)

Cheyne, C. and Pigden, C.
(1996) "Pythagorean powers or a challenge to Platonism", *Australian Journal of Philosophy* 76: 115-119

Chihara, C.
(1982) "A Gödelian thesis regarding mathematical objects: Do they exist? And can we perceive them?", *The Philosophical Review* 91: 211-227
(1990) *Constructibility and Mathematical Existence* (Oxford: Clarendon Press)

Colyvan, M.
(1998) "In defence of indispensability", *Philosophia Mathematica* (3) 6: 39-62
(1999) "Contrastive empiricism and indispensability", *Erkenntnis* 51: 323-332
(2000) "Conceptual contingency and abstract existence", *The Philosophical Quarterly* 50: 87-91

Detlefsen, M.
(1986) *Hilbert's Program: An Essay on Mathematical Instrumentalism* (Dordrecht: D. Reidel Publishing).

Detlefsen, M. (ed.)
(1992) *Proof and Knowledge in Mathematics* (London: Routledge)

Dirac, P.
(1958) *The Principles of Quantum Mechanics*, 4th edition (Oxford: Clarendon Press)

Duhem, P.
(1954) *The Aim and Structure of Physical Theory* (Princeton, NJ: Princeton University Press); translation by P. Wiener of the second edition of *La Théorie Physique: Son Objet, Sa Structure* (Paris: Marcel Rivière & Cie., 1914)

Edwards, A.
(1972) *Likelihood* (Cambridge: Cambridge University Press)

Feferman, S.
(1960) "Arithmetization of metamathematics in a general setting", *Fundamenta Mathematicae* 49:35-92
(1993a) "Why a little bit goes a long way: logical foundations of scientifically applicable mathematics", *PSA: Proceedings of the 1992 Biennial Meeting of the Philosophy of Science Association* 2:442-455
(1993b) "What rests on what? The proof-theoretic analysis of mathematics", http://math.stanford.edu/~feferman/papers/whatrests.pdf. Also published in S. Feferman *In the Light of Logic* (New York: Oxford University Press, 1998): 187-208. Originally published in J. Czermak (ed.) *Philosophy of Mathematics. Proceedings of the Fifteenth International Wittgenstein-Symposium, Part I* (Vienna: Verlag Hölder-Pichler-Tempsky): 147-171
(1999) "Does mathematics need new axioms?", *The American Mathematical Monthly* 106: 99-111

Feigenson, L. and Halberda, J.
(2004) "Infants chunk object arrays into sets of individuals", *Cognition* 91: 173-190

Feynman, R
(1965) *The Character of Physical Law* (London: Penguin Books Ltd., 1992). Originally published by the British Broadcasting Corporation

Field, H.
(1980) *Science Without Numbers: A Defence of Nominalism* (Oxford: Basil Blackwell)
(1984) "Is mathematical knowledge just logical knowledge", *The Philosophical Review* 93: 509-552
(1982) "Realism and anti-realism about mathematics", reprinted with a postscript in Field (1989a): 53-78. Originally published in *Philosophical Topics* 13: 45-69
(1985a) "Can we dispense with space-time?", reprinted with a postscript in Field (1989a): 171-226. Originally published in P. Asquith and P. Kitcher (eds.) *PSA: Proceedings of the 1984 Biennial Meeting of the Philosophy of Science Association* 2: 33-90.
(1985b) "On conservativeness and incompleteness," reprinted in Field (1989a): 125-146. Originally published in *The Journal of Philosophy* 81: 239-260.
(1989a) *Realism, Mathematics and Modality* (Oxford: Basil Blackwell)
(1989b) "Is mathematical knowledge just logical knowledge", Field (1989a): 79-124. Revised version of Field (1984)
(1991) "Metalogic and modality", *Philosophical Studies* 62: 1-22
(1993) "The conceptual contingency of mathematical objects", *Mind* 102: 285-299

Frege, G
(1953) *The Foundations of Arithmetic*, 2nd edition (Oxford: Basil Blackwell); translation by J.L. Austin of *Die Grundlagen der Arithmetik* (Breslau: W. Koebner, 1884)

Friedman, H. and Meyer, R.
(1992) "Whither relevant arithmetic?", *The Journal of Symbolic Logic* 57: 824-831

Geach, P. and Black, M. (eds.)
(1952) *Translations from the Philosophical Writings of Gottlob Frege* (Oxford: Basil Blackwell)

Gelman, R.
(1977) "How young children reason about small numbers", N. Castellan, D. Pisoni and G. Potts (eds.) *Cognitive Theory*, Volume 2 (Hillsdale, NJ: Lawrence Erlbaum Associates): 219-238

Gentzen, G.
(1936) "The consistency of elementary number theory", M.E. Szabo (ed. and trans.) *The Collected Papers of Gerhard Gentzen* (Amsterdam: North Holland Publishing Company, 1969): 132-201. Originally published as "Die Widerspruchsfreiheit der reinen Zahlentheorie", *Mathematische Annalen* 112: 493-565

Giaquinto, M.
(2001) "Knowing numbers", *The Journal of Philosophy* 98: 5-18
(2002) *The Search for Certainty: a Philosophical Account of Foundations of Mathematics* (Oxford: Clarendon Press).
Gödel, K.
(1931) "On formally undecidable propositions of *Principia Mathematica* and related systems I", reprinted and translated in S. Feferman (ed.) *Collected Papers*: 144-195. Originally published as "Über formal unentscheidbare Sätze der *Principia mathematica* und verwandter Systeme I", *Monatschefte für Mathematik und Physik* 38: 173-198
(1940) *The Consistency of the Axiom of Choice and of the Generalized Continuum-hypothesis with the Axioms of Set Theory. Annals of Mathematics Studies* 3 (Princeton: Princeton University Press)
(1944) "Russell's mathematical logic", Benacerraf and Putnam (1983): 447-469. Originally published in A. Schilp (ed.) *The Philosophy of Bertrand Russell* (Evanston, ILL: Northewestern University): 125-153
(1947) "What is Cantor's continuum problem?", *The American Mathematical Monthly* 54: 515-525
(1963) "What is Cantor's continuum problem?", Benacerraf and Putnam (1983): 470-485. Revised and expanded version of Gödel (1947)

Goldman, A.
(1967) "The causal theory of knowledge", *The Journal of Philosophy* 64: 357-372

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

Grice, H.P.
(1961) "The causal theory of perception", *Proceedings of the Aristotelian Society Supplementary Volume XXXV*: 121-152

Hale, B.
(1987) *Abstract Objects* (Oxford: Basil Blackwell)
(1990) "Nominalism", A. Irvine (ed.) *Physicalism in Mathematics* (Dordrecht: Kluwer Academic Publishers): 121-144
(1996) "Structuralism's Unpaid Epistemological Debts", *Philosophia Mathematica (3)* 4: 124-147

Hale , B. and Wright, C.
(1992) "Nominalism and the contingency of abstract objects", *The Journal of Philosophy* 89: 111-135
(1994) "A reductio ad surdum? Field on the contingency of mathematical objects", *Mind* 103: 169-184

Hallett, M.
(1984) *Cantorian Set Theory and Limitation of Size* (Oxford: Clarendon Press)

Happe, F.
(1994) "An Advanced Test of Theory of Mind: Understanding of Story Characters' Thoughts and Feelings by Able, Autistic, Mentally Handicapped, and Normal Children and Adults," *Journal of Autism and Developmental Disorders* 24: 129-154
(1995) "Understanding Minds and Metaphors: Insights from the Study of Figurative Language in Autism," *Metaphor and Symbolic Activity* 10: 275-295

Hart, W.D.
(1996) *The Philosophy of Mathematics* (Oxford: Oxford University Press)

Hebb, D.
(1949) *The Organization of Behaviour: A Neurophysiological Theory* (New York: Wiley and Sons)

(1980) *Essay on Mind* (Hillsdale, NJ: Lawrence Erlbaum Associates)

Heck Jnr., R.G. (ed.)
(1997) *Language, Thought and Logic* (Oxford: Oxford University Press)
Hellman, G.
(1989) *Mathematics without Numbers* (Oxford: Oxford University Press)
(1999) "Some ins and outs of indispensability: a modal-structural perspective", A Cantini, E. Casari and P. Minari (eds.) *Logic and Foundations of Mathematics. Synthese Library Vol. 280*: 26-39

Hilbert, D.
(1925) "On the infinite", Van Heijenoort (1967): 367-392. Originally published as "Über das Unendliche", *Jahresbericht der Deutschen Mathematiker-Vereinigung* 36: 201-215
(1927) "The foundations of mathematics", Van Heijenoort (1967): 464-479. Originally published as "Der Grundlagen der Mathematik", *Abhandlungen aus dem mathematischen Seminar der Hamburgischen Universität* 6: 65-85

Hilbert, D. and Bernays, P.
(1939) *Grundlagen der Mathematik*, Vol. II (Berlin: Springer-Verlag)

Hodes, H.
(1984) "Logicism and the ontological commitments of arithmetic", *The Journal of Philosophy* 81: 123-149
(1990) "Where do the natural numbers come from?", *Synthèse* 84: 347-407
(1991a) "Where do sets come from?", *The Journal of Symbolic Logic* 56: 150-175
(1991b) "Corrections to "Where do sets come from?"", *The Journal of Symbolic Logic* 56: 1486

Isham, C.
(1989) "Quantum gravity", P. Davies (ed.) *The New Physics* (Cambridge: Cambridge University Press)

Kalderon, M. (ed.)
(2005) *Fictionalism in Metaphysics* (Oxford: Oxford University Press)

Katz, J.
(1981) *Language and Other Abstract Objects* (Totowa, N.J.: Rowman and Littlefield)
(1995) "What mathematical knowledge could be", *Mind* 104: 491-522
(1998) *Realistic Rationalism* (Cambridge, MA: The MIT Press)

Kaufman, E., Lord, M., Reese, T. and Volkmann, J.
(1949) "The discrimination of visual number", *The American Journal of Psychology* 62: 498-525

Kitcher, P.
(1980) "Arithmetic for the Millian", *Philosophical Studies* 37: 215-236
(1984) *The Nature of Mathematical Knowledge* (Oxford: Oxford University Press)

Kline, M.
(1972) *Mathematical Thought from Ancient to Modern Times* (New York: Oxford University Press)

Krantz, D., Luce, R., Suppes, P and Tversky, A.
(1971) *Foundations of Measurement* (New York: Academic Press)

Kyburg, H.
(1984) *Theory and Measurement* (Cambridge: Cambridge University Press)
(1990) *Science and Reason* (Oxford: Oxford University Press)

Lamarque, P. and Olsen, S.H.
(1994) *Truth, Fiction and Literature: A Philosophical Perspective* (Oxford: Clarendon Press).

Leslie, A.M.
(1987) "Pretense and representation: the origins of 'theory of mind'", *Psychological Review* 94: 412-416

Liston, M.
(1993) "Taking mathematical fictions seriously", *Synthèse* 95: 433-458

Lomas, D.
(2002) "What perception is doing, and what it is not doing, in mathematical reasoning", *The British Journal for the Philosophy of Science* 53: 205-223

MacBride, F.
(1999) "Listening to fictions: a study of Fieldian nominalism," *The British Journal for the Philosophy of Science* 50: 431-455

Maddy, P.
(1980) "Perception and mathematical intuition", *The Philosophical Review* 84: 163-196
(1988a) "Believing the axioms. I", *The Journal of Symbolic Logic* 53: 481-511
(1988b) "Believing the axioms. II", *The Journal of Symbolic Logic* 53: 736-764
(1990) *Realism in Mathematics* (Oxford: Clarendon Press)
(1992) "Indispensability and Practice", *The Journal of Philosophy* 89: 275-289
(1993) "Does V = L?", *The Journal of Symbolic Logic* 58: 15-41
(1997) *Naturalism in Mathematics* (Oxford: Clarendon Press)

Melia, J.
(1995) "On what there's not," *Analysis* 55: 223-229
(2000) "Weaseling away the indispensability argument", *Mind* 109: 455-480

Mendelson, E.
(1997) *An Introduction to Mathematical Logic*, 4th edition (London: Chapman & Hall)

Meyer, R.
(1976) "Relevant arithmetic", *Bulletin of the Section of Logic of the Polish Academy of Science*, Vol. 5: 133-137

Meyer, R. and Mortensen, C.
(1984) "Inconsistent models for relevant arithmetics", *The Journal of Symbolic Logic* 49: 917-929

Oliver, A.
(2000) "A realistic rationalism?", *Inquiry* 43: 111-136

Papineau, D.
(1993) *Philosophical Naturalism* (Oxford: Blackwell)

Parsons, C.
(1971) "Ontology and mathematics", *The Philosophical Review* 80: 151-176
(1980) "Mathematical intuition", Hart (1996): 95-113. Originally published in *Proceedings of the Aristotelian Society* LXXX: 145-168
(1986a) "Intuition in constructive mathematics", J. Butterfield (ed.) *Language, Mind and Logic* (Cambridge: Cambridge University Press): 211-229
(1986b) "Quine on the philosophy of mathematics", L.E. Hahn and P.A. Schilpp (eds.) *The Philosophy of W.V. Quine* (La Salle, ILL: Open Court): 369-395
(1993) "On some difficulties concerning intuition and intuitive knowledge", *Mind* 102: 233-246

(1994) "Intuition and number", A. George (ed.) *Mathematics and Mind* (New York: Oxford University Press, 1994): 141-157
(1995) "Platonism and mathematical intuition in Kurt Gödel's thought", *The Bulletin of Symbolic Logic* 1: 44-74

Parsons, T.
(1980) *Nonexistent Objects* (New Haven: Yale University Press)

Peacocke, C.
(1993) "How are a priori truths possible?", *European Journal of Philosophy* 1: 175-199

Piaget, J.
(1937) *The Construction of Reality in the Child* (New York: Basic Books, 1954)

Piaget, J. and Szeminska, A.
(1941) *The Child's Conception of Number* (New York: Humanities Press, 1952)

Phillips, J.
(1975) *The Origins of Intellect: Piaget's Theory*, 2nd edition (San Francisco: W. H. Freeman and Company)

Pitcher, G.
(1971) *A Theory of Perception* (Princeton: Princeton University Press)

Priest, G.
(1979) "The logic of paradox", *Journal of Philosophical Logic* 8: 219-241
(1991) "Minimally inconsistent LP", *Studia Logica* 50: 321-331
(1994) "Is arithmetic consistent?", *Mind* 103: 337-349
(2000) "Objects of thought", *Australian Journal of Philosophy* 78: 494-502

Putnam, H.
(1971) *Philosophy of Logic* (London: George Allen and Unwin, 1972). Originally published in New York by Harper and Row.

Quine, W.V.O.
(1948) "On what there is", reprinted in Quine (1980): 1-19. Originally published in *The Review of Metaphysics* 2: 21-38
(1951) "Two dogmas of empiricism", reprinted in Quine (1980): 20-46. Originally published in *The Philosophical Review* 60: 20-43
(1960) "Carnap and logical truth", reprinted in P. Benacceraf and H. Putnam (1983): 355-376. Originally published in *Synthèse* 12: 350-374
(1969) "Epistemology naturalized", W.V. Quine *Ontological Relativity and Other Essays* (New York: Columbia University Press): 69-90
(1980) *From a Logical Point of View*, 2nd ed. (Cambridge, MA: Harvard University Press)
(1981a) "Things and their place in theories", W.V. Quine *Theories and Things* (Cambridge, MA: Harvard University Press, 1981): 1-23
(1981b) "Five milestones of empiricism", W.V. Quine *Theories and Things* (Cambridge, MA: Harvard University Press, 1981): 67-72
(1984) "Review of *Mathematics in Philosophy: Selected Essays* by Charles Parsons", *The Journal of Philosophy* 81: 783-794
(1992) *Pursuit of Truth*, revised edition (Cambridge, MA: Harvard University Press)

Resnik, M.
(1985a) "How nominalist is Hartry Field's nominalism?", *Philosophical Studies* 47: 163-181
(1985b) "Ontology and logic: remarks on Hartry Field's anti-platonist philosophy of mathematics", *History and Philosophy of Logic* 6: 191-209
(1995) "Scientific *vs.* mathematical realism: the indispensability argument", *Philosophia Mathematica* (3) 3: 166-174
(1997) *Mathematics as a Science of Patterns* (Oxford: Clarendon Press)

Rosser, J.
(1936) "Extensions of some theorems of Gödel and Church", *Journal of Symbolic Logic* 1: 87-91

Rumfitt, I.
(2001) "Hume's Principle and the number of all objects", *NOÛS* 35: 515-541

Shoenfield, J.
(1977) "Axioms of set theory", J. Barwise (ed.) *Handbook of Mathematical Logic* (Amsterdam: North-Holland Publishing Company): 322-344

Searle, J.
(1974) "The logical status of fictional discourse", *Expression and Meaning: Studies in the Theory of Speech Acts* (Cambridge: Cambridge University Press, 1979): 58-75. Originally published in *New Literary History* 6: 319-332

Sellars, W.
(1956) "Empiricism and the philosophy of mind", *Minnesota Studies in the Philosophy of Science* 1: 253-329

Shapiro, S.
(1983) "Conservativeness and incompleteness", W. Hart (ed.) *The Philosophy of Mathematics* (Oxford: Oxford University Press, 1996): 225-234. Originally published in *The Journal of Philosophy* 80: 521-531

Simpson, S.G.
(1985) "Nonprovability of certain combinatorial properties of finite trees", L.A Harrington, M.D. Morley, A Scedrov and S.G. Simpson (eds.) *Harvey Friedman's Research on the Foundations of Mathematics*, Studies in Logic and the Foundations of Mathematics 117 (Amsterdam: North-Holland): 87-117

Sober, E.
(1990) "Contrastive empiricism", E. Sober *From a Biological Point of View* (Cambridge: Cambridge University Press, 1994): 114-135. Originally published in *Minnesota Studies in the Philosophy of Science* 14: 392-412
(1993) "Mathematics and indispensability", *The Philosophical Review* 102: 35-57

Stanley, J.
(2001) "Hermeneutic fictionalism", www.rci.rutgers.edu/%Ejasoncs/Fictionalism.pdf. Also available in P. French and H. Wettstein (eds.) *Midwest Studies in Philosophy* 25, Figurative Language: 36-71

Steiner, M.
(1992) "Mathematical rigor in physics", Detlefsen (1992): 158-170

Tarski, A.
(1933) "The concept of truth in formalized languages", J. Corcoran (ed.) *Logic, Semantics, Metamathematics* (Indianapolis: Hackett Publishing Company, 1983): 152-278. Translation by J. Woodger of *Pojecie prawdy w jezykach nauk dedukcyjnych* (Warsaw, 1933) with a new postscript.
(1944) "The semantic conception of truth and the foundations of semantics", in L. Linsky (ed.) *Semantics and the Philosophy of Language* (Urbana: University of Illinois Press, 1952): 13-47. Originally published in *Philosophy and Phenomenological Research* 4: 341-375

Tait, W.
(1981) "Finitism", *The Journal of Philosophy* 78: 524-546

Tennant, N.
(1978) *Natural Logic* (Edinburgh: Edinburgh University Press)

(1987) *Anti-Realism and Logic* (Oxford: Clarendon Press)
(1997a) "On the necessary existence of numbers", *NOÛS* 31: 307-336
(1997b) *The Taming of the True* (Oxford: Clarendon Press)

Van Fraassen, B.
(1980) *The Scientific Image* (Oxford: Clarendon Press)

Van Heijenoort, J.
(1967) *From Frege to Gödel: A Source Book in Mathematical Logic, 1879-1931* (Cambridge, MA: Harvard University Press)

Van Inwagen, P.
(1977) "Creatures of fiction", *American Philosophical Quarterly* 14: 299-308

Von Neumann, J.
(1925) "An Axiomatisation of Set Theory", translated by S. Bauer-Mengelberg and D. Føllesdal in Van Heijenoort (1967): 393-413. Originally published as "Eine Axiomatisierung der Mengenlehre", *Journal für die reine und angewandte Mathematik* 154: 219-240

Wagner, S.
(1982) "Arithmetical fictionalism", *Pacific Philosophical Quarterly* 63: 255-269

Walton, K.
(1990) *Mimesis as Make-Believe: On the Foundations of the Representational Arts* (Cambridge: Harvard University Press)

Wilson. M.
(1994) "Can we trust logical form", *The Journal of Philosophy* 91: 519-546

Wittgenstein, L.
(1967) *Remarks on the Foundations of Mathematics*, 2nd ed. (Oxford: Basil Blackwell). Translation by G. E. M. Anscombe of L. Wittgenstein's unpublished *Bemerkungen über die Grundlagen der Mathematik*, edited by G. H. von Wright, R. Rhees and G. E. M. Anscombe.

Woolhouse, R. and Francks, R. (editors and translators)
(1998) *G. W. Leibniz: Philosophical Texts* (Oxford: Oxford University Press)

Wright, C.
(1983) *Frege's Conception of Numbers as Objects* (Aberdeen: Aberdeen University Press)
(1988) "Why numbers can believably be: a reply to Hartry Field", *Revue Internationale de Philosophie* 42: 425-473
(1997) "On the philosophical significance of Frege's Theorem", Heck (1997): 201-244

Yablo, S.
(1993) "Is conceivability a guide to possibility?", *Philosophy and Phenomenological Research* 53: 1-42
(1998) "Does ontology rest on a mistake?", *Proceedings of the Aristotelian Society Supplementary Volume LXXII*: 229-262
(2001) "Go figure: a path through fictionalism", *Midwest Studies in Philosophy* 25: 72-102
(2002) "Abstract objects: a case study", A. Bottani, M. Carrara and P. Giaretta (eds.) *Individuals, Essence and Identity: Themes of Analytic Metaphysics* (Dordrecht: Kluwer Academic Publishers): 163-188
(2005) "The myth of the seven", Kalderon (2005): 88-115

Yi, B-U.
(1998) "Numbers and relations", *Erkenntnis* 49: 93-113

Zalta, E.
(1988) *Intensional Logic and the Metaphysics of Intentionality* (Cambridge, MA; MIT Press)