Chapter 6 Mathematics and Other Non-Natural Subjects

6.1 Introduction

The arguments of the last chapter, together with those in chapter 3, amount to a realist package. In chapter 3 I argued for a realist theory of content, a theory which implies, as I observed in section 3.13, that there is no conceptual guarantee that our procedures for forming beliefs should yield beliefs which are true. Such a theory establishes a sense in which our beliefs answer to an independent world: the claims made by our beliefs conceptually outstrip the basis on which we form those beliefs.

This gap between the basis for judgements and their content creates room for sceptical threats. The theory of knowledge developed in the last chapter, however, shows how such threats can be dealt with. For even if there is no conceptual guarantee, issuing from the theory of content, that judgement and truth should co-vary, they might still co-vary as a matter of a posteriori fact. The arguments of the last chapter showed how this might work for the general run of our beliefs about the natural world: there may be good empirical evidence that our belief-forming procedures are reliable producers of truth, even if there is no conceptual guarantee of this. So the last chapter in effect added a realist theory of knowledge to the realist theory of content developed in chapter 3, by showing how, even though truth conceptually transcends evidence, they may still co-vary as a matter of empirical fact.

So realism, as I shall use the term henceforth, has two components. First, it requires a realist theory of content, according to which our beliefs answer to an independent world. And then it deals with the resulting sceptical threat by means of a realist theory of knowledge, according to which truth and judgement co-vary as matter of a posteriori fact, even if not of conceptual necessity.

So far, however, I have focused exclusively on "natural beliefs", in the sense of beliefs about the natural world. What about non-natural beliefs, such as beliefs about mathematics, or morality, or about modal questions of necessity and possibility? In chapter 3 I explicitly excluded such non-natural beliefs from the scope of the teleological theory of content, on the grounds that the teleological theory requires beliefs that are relevant to the success of actions, and it is debatable whether non-natural beliefs satisfy this requirement.

So it is unclear whether our realist theory of content, in the form of the teleological theory, applies to non-natural beliefs. Moreover, it is equally unclear whether a realist theory of knowledge has any grip on the non-natural realm: it seems odd, to say the least, to suppose that mathematical or other non-natural beliefs should derive their warrant from a posteriori reliable methods of belief-formation, from procedures whose reliability is testified by empirical evidence.

Non-natural beliefs thus seem to fall outside the scope both of our realist theory of content, and of our realist epistemology. Given this, it seems appropriate to explore a different route to the vindication of non-natural beliefs, and to see whether they can be vindicated in an anti-realist manner instead.

I am here using "anti-realist" in the sense made popular by Michael Dummett, to refer to analyses of content which imply that truth does not transcend evidence, that there is no gap between a judgement being properly arrived at and its being true. Even if such an anti-realist view of content is inappropriate to beliefs about the natural world, it may still be the right account of mathematical and other non-natural beliefs. And this would then deal with any epistemological difficulties that may threaten such non-natural judgements: for if there is no conceptual gap between truth and evidence, then there is no need for any further explanation of why ou r practices for making such judgements should be thought to yield truths.[1]

6.2 Content and Knowledge

Before proceding to the detailed consideration of mathematical and other non-natural judgements, it will be helpful to digress briefly and make some observations about anti-realist theories of content and knowledge in general, as applied to natural as well as non-natural beliefs. As I have just observed, anti-realist theories of content have the epistemological attraction of promising to dissolve sceptical threats: if your theory of content tells you that there is no conceptual room for properly arrived at judgements to be false, then you can stop worrying about the possibility of error.&nb sp; This is not of course an argument for anti-realism: that certain philosophical problems would disappear, if judgements about trees were really just judgements about sensations, is not in itself a good reason for thinking that trees are sensation s. Accordingly, anti-realist philosophers, from Berkeley on, have offered independent arguments for thinking that the content cannot outstrip evidence. Still, there seems no doubt that the epistemological implications have always operated as a strong motive for adopting anti-realist theories of content. (After all, the arguments for idealist, verificationist, and other anti-realist theories of content are scarcely conclusive; and the conclusions, certainly as applied to non-natural beliefs, are pre-theoretically highly implausible; it follows, I think, that something other than the arguments is needed to account for the widespread acceptance of anti-realist theories of content.)

On this question of motive, th e epistemological attractions of anti-realism of content are all the greater if you aspire to certainty as a requirement for knowledge. For this strong traditional demand on knowledge adds weight to scepticism, and so to the desirability of a theory of content that promises to block scepticism at source. This link between certainty and anti-realism isn't inescapable: Descartes, for instance, managed to uphold certainty as a requirement on knowledge without abandoning a realist theory of content. He did, however, need God as a guarantor of certain knowledge. Without God, it is difficult to uphold certainty except by appeal to anti-realism. For the only plausible strategy for achieving certainty without God is the anti-re alist tactic of collapsing the world into the mind by arguing that the contents of claims about the world don't extend beyond what what introspection and logical analysis guarantees. (Note how this anti-realist move then aims to satisfy the demand f or certainty, the demand that we arrive at beliefs in ways that necessarily deliver truths, by arguing that the truth of our beliefs is conceptually guaranteed by the ways we arrive at them.)

Of course, anti-realism of content often has d ifficulty delivering on its anti-sceptical promises. In order for anti-realism of content to be all plausible, it needs to allow at least some distance between appearance and reality, needs to allow that there is something more to a judgement being true than that it is taken to be true on some specific occasion. So anti-realism will quickly move away from the claim that the presence of a table, say, is just a matter of your current sense impressions, and allow that it also depends on what impressions you will have in a moment, and on those that other people will have, and in the end on enough evidence to ensure that the relevant judgement won't be overturned by further discoveries. But of course the more anti-realism moves along this dimension, the less effective it becomes as a response to scepticism: it is one thing to be certain about what sensations you are currently having; it is quite another to be certain that some current judgement will never be overturned by future discoveries.

But we can leave these problems to proponents of the anti-realist programme.[2] For it matters little whether or not they can fulfil their epistemological promises, given that we have good reason, as was shown in 3.13 above, for rejecting the anti-realist theory of content in the first place.

Moreover, once we abandon the demand for certainty, and therewith the requirement that our methods of thought should necessarily produce truths, there is then much less initial pressure for a theory of content which makes truth a conceptual upshot of our belief-forming procedures. The reliabilist alternative to the demand for certainty asks only for methods that are contigently reliable for truth. So from the reliabilist perspective there is no need to find a conceptual link between our doxastic practice and truth. It is quite enough if we can defend a realist theory of knowledge, a theory according to which the reliability of our methods is an empirical matter.

6.3 Anti-Realism versus Fictionalism for Mathematics

Let me now return to the issue of non-natural beliefs. As I said, it seems unlikely that we will be able to develop a realist theory of knowledge for the non-natural realm which will defend the reliability of our belief-forming procedures on a posteriori grounds. So perhaps here at least we should vindicate our beliefs by showing, in anti-realist spirit, that truth does not conceptually outstrip the basis on which we make such judgements.

In what follows I shall concentrate on mathematical judgements. It would be unreasonably ambitious to aim for detailed accounts of moral and modal judgements as well. But at the end of the chapter I shall return briefly to morality and modality, both for purposes of comparison, and to offer a few promissory thoughts.

My procedure, in connection with mathematics, will not be to aim for some general semantic theory, akin to the teleological theory I developed for natural judgements, which will explain "aboutness" for mathematical judgements. The welter of existing controversy which surrounds any philosophical discussion of mathematical judgements effectively precludes any such direct approach. Instead I shall proceed indirectly, by asking about the epistemological consequences of mathematical meaning, rather than about mathematical meaning itself. In particular, I shall ask directly whether an anti-realist epistemology is defensible for mathematics: is truth, for mathematical judgements, nothing more than evidence, nothing more than being warranted by proper mathematical procedures?

In due course I shall conclude that this anti-realist view of mathematics is unacceptable. This will implicitly establish that mathematical judgements have a realist semantics, in the sense that truth, for mathematical claims, conceptually transcends the basis on which we make such claims.

This then threatens scepticism. I have argued that, in the case of claims about the natural world, the corresponding sceptical threat is blocked because our judgemental practices are reliable for truth as a ma tter of a posteriori fact, even if not by conceptual necessity. I shall briefly consider whether any analogous strategy will work for mathematics. But this line of thought will come to nothing.

So we will be left with a scepti cal -- or, more familiarly, "fictionalist" -- attitude to mathematics. Hartry Field (1980, 1989) has done much to explain how such a position can work. A detailed explanation is best left till later. But in outlin e the fictionalist attitude will combine:

(a) a literal understanding of mathematical claims, as referring to abstract objects like numbers, sets, and so on, with

(b) a rejection of belief in such claims, and

(c) an acceptanc e of such claims as fictions which are useful for various pragmatic purposes.

The resulting position is closely analogous to the instrumentalist attitude to scientific theories adopted by Bas van Fraassen (1980). Van Fraassen's scep tical instrumentalism avoids the contortions of earlier anti-realist brands of scientific instrumentalism, in that he takes scientific theories at face value, as literally referring to unobservables like atoms and electrons, and abandons any attempt to re construe scientific theories as merely making claims about observables. He then combines this literal understanding of scientific claims with a refusal to uphold those claims as true. Van Fraassen's view is that we shouldn't believe scientific claims about unobservables, but should simply "accept" them as useful instruments for making predictions, summarizing data , and so on.

I don't agree with Van Fraassen about scientific theories about unobservables, as the arguments at th e end of the last chapter will have made clear. But I do think the analogous position is right for mathematics: we should understand mathematical claims at face value, but should only accept them as useful instruments, not believe them.
&n bsp;

6.4 If-Thenism

I shall start, as I said, by asking whether an anti-realist theory of mathematical knowledge is defensible. Is there an intrinsic link between evidence and truth for mathematical claims? At f irst sight such a view may seem highly plausible. After all, it is a familiar thought that, where judgements about the natural world answer to independent facts, there isn't anything more to mathematical truth but provability: that what makes it correct to say that there is no greatest prime number, say, or that the real numbers are non-denumerable, is not that there is some independent world in which these facts obtain, but simply that these claims can be established by recognized methods of proof. (My initial concern in this chapter is with pure mathematics, like arithmetic and analysis; the application of mathematics to the empirical world will be discussed in due course.)

However, familiar as it is, this anti-realist view of mathematics faces difficulties. Let me focus on the notion of proof. What exactly are the "recognized methods of proof" in any given mathematical subject area? An initial answer might be that in any such area mathematicians start with certain basic assumptions (which might or might not be formally recognized as "axioms") about the natural numbers, or the reals, or groups, or non-Euclidean spaces, or whatever, and then use logic to derive further conclusions as theorems from those basic assumptions. Let us grant, for the time being, the appropriateness of logic for this purpose. (I shall return to the epistemological status of logic at the end of this chapter.) This leaves us with the axioms. And here there is an obvious difficulty, namely, that the axioms haven't themselves been proved. Rather they are the point at which mathematicians start proofs. So, on the face of it, it seems that mathematical proofs only establish that if certain assumptions are true, then certain other claims, the theorems, are also true.

There is a view in the philosophy of mathematics according to which mathematical assertions should be understood as expressing precisely such hypothetical claims. This view is called "if-thenism". Now, if "if-thenism" were true, then the existence of a mathematical proof would indeed conceptually guarantee the truth of the corresponding mathematical claim, and mathematical anti-realism would be vindicated. However, it seems clear that "if-thenism" is simply wrong about what mathematical statements actually mean (cf Resnik, 1980, ch 3). Number theorists don't just hold that if there is a number 0, and if every number has a successor, and . . . so on for the rest of Peano's postulates, then there is no greatest prime number. On the contrary, they hold that 0 does exist, and that every number does have a successor, . . . and consequently that there definitely isn't a greatest prime number.

We could of course understand "if-thenism" not as an account of what mathmatical statements do mean, but rather as an account of what they should mean. On this interpretation, "if-thenism" would be recommending that we should revise our understanding of mathematical statements, precisely so as to ensure that our methods of mathematical proof suffice to establish those statements. But this then makes my point clear: namely, that, as currently meant, mathematical statements lay claim to more than mathematical proofs establish, thus undermining the anti-realist equation of mathematical truth with mathematical proof.[3]

6.5 Postulationism

The difficulty raised for mathematical anti-realism in the last section was in effect that mathematical practice seems just to assume the axioms from which it starts proofs, and does nothing to establish those axioms. But perhaps anti-realists could respond that the peculiarity of mathematics is precisely that its basic assumptions don't need any further proof, on the grounds that the requisite mathematical objects will automatically be available to satisfy any consistent set of mathematical assumptions.

I shall call this attitude towards mathematics "postulationism", since it implies that no further justification is needed for a mathematical theory than the consistency of its postulates. At first sight postulationism might seem to make mathematical existence unacceptably dependent on human activity, with mathematical objects somehow springing into existence as mathematicians formulate assumptions. But we needn't understand the postulationist theory in such a strongly anthropocentric way. Rather the idea could be that there is a timeless Platonist realm in which there are abstract objects satisfying any possible set of consistent mathematcial axioms, whether or not anybody has yet thought of those axioms.[4]

This would of course mean that there are an awful lot of mathematical objects -- as well the familar objects of standard mathematics, there will also be such non-standard objects as all the different kinds of numbers modulo n, and all the shapes in all possible geom etries, and all the operators in all possible vector spaces, and indeed all kinds of things that have never been thought of and never will be. But perhaps there's nothing wrong with that. Large universes are scarcely alien to mathematics.

A more substantial objection to postulationism might be that mathematicians themselves make a distinction between those branches of mathematics whose existence claims they take seriously and those whose they don't. The complaint here wo uld in a sense be the mirror image of the claim levelled against "if-thenism". Where "if-thenism" says that all mathematics is meant hypothetically, "postulationism" seems to imply that all mathematics can be asserted unconditionally. But this then means that "postulationism" is open to a mirror image of the objection made to "if-thenism": since there are branches of mathematics in which mathematicians do restrict themselves to hypothetical attitudes, considering the axioms as assumption s whose consequences are worth exploring, rather than as claims to be believed, it is wrong to read all mathematics as unconditionally assertible.

However, I think that postulationism has a reasonable answer to this complaint. For t here is a natural way for postulationism to distinguish between those branches of mathematics which it appropriate to understand hypothetically and those which which it appropriate to understand unconditionally, a way which seems to line up accurate ly with the way practising mathematicians make this distinction. Postulationists can appeal to the distinction between sets of axioms which are categorical, in the technical sense of determining a unique model, up to isomorphism, and those which are not so categorical. The axioms of group theory, for instance, are not categorical, in that quite disparate sets of objects, of different cardinalities, can form groups. Peano's postulates, by contrast, are categorical (in second-order logic), in that any set of objects and relations which satisfy them can be placed in a structure-preserving one-to-one correspondence. So the natural move for postulationism is to argue that mathematical objects are available to satisfy every consistent an d categorical set of axioms; non-categorical axiom sets, by contrast, do not guarantee the existence of any mathematical objects, and so should be read hypothetically, as saying merely that if there are any objects which . . ., then . . .

This suggestion accords well with actual mathematical practice. Mathematicians certainly seem to be committed, as I have already observed, to the numbers 0, 1, and all their successors, as existing objects. By contrast, it makes little mathematical sense to talk about the identity element mentioned in the axioms of group theory. Yet, even within group theory, once we add enough special assumptions to the general axioms of group theory to give us categoricity, then, in line with th e current suggestion, we do find mathematicians talking unconditionally about the simple group of order 68, the elliptic modular group, and so on, as if these special groups, at least, were as real as the number one.

So postulationism can answer the charge that it makes all mathematics unconditionally assertible. It simply restrict its ontological commitment to those abstract objects required to satisfy categorical mathematical theories. This then implies, in accord with existing ma thematical practice, that only such categorical theories should be unconditionally asserted, and that non-categorical theories should merely be embraced hypothetically.

There is, however, another rather more telling objection to postulati onism. According to postulationism, as now understood, all objects that can consistently and categorically be postulated thereby exist. But how then does postulationism differ from a fictionalist attitude to mathematical objects? If ever ything that can consistently and categorically be thought to exist thereby does exist, then won't Sherlock Holmes exist, and Santa Claus, and the Wicked Witch of the North?[5] Perhaps it is natural to slip into unreflective acceptance of ca tegorical truths about numbers and sets and simple groups. But if all this really amounts to is accepting what follows from assumptions agreed by mathematicians, why is it any different from accepting what follows from our agreed assumptions about S anta Claus?

The postulationist might object that the comparison is not fair. If we take claims about Sherlock Holmes and Santa Claus literally, then these claims are about people who inhabit the same spatiotemporal world as we do.&n bsp; And on this literal reading these claims can be shown to be false. Maybe the stories are internally consistent, but they aren't consistent with the totality of our beliefs about the world. Which is why we don't in fact accept these claims literally, and why we don't accord Shylock and Santa Claus real existence, but only fictional "existence", that is, non-existence. By contrast, claims about mathematical objects aren't about spatiotemporal objects. So nothing forces us to reg ard them as literally false, nor to regard the objects they mention as mere fictions.

But I don't think that this is good enough. It still seems to me that the postulationist story as told so far gives us no reason to view mathemati cal existence as anything more than fictional non-existence. Maybe the definite reasons which force us to adopt fictional attitudes to explicit fictions don't carry over to the mathematical case. But, even so, the postulationist hasn't told us anything more about what's involved in mathematical existence, other than that a consistent and categorical story can be told about the objects in question. Such a story guarantees fictional "existence". But if mathematical existence is to am ount to more than the non-existence of fictional "existence", then there must be something more at issue than an internally consistent story, for abstract objects just as for concrete ones.

6.6 Reductionism

The l ast two sections have presented mathematical practice as an entirely "internal" business, in which assumptions are accepted and consequences drawn therefrom. As long as we stick with this internalist picture, it will be difficult to avoid fictionali sm, for lack of any account of what makes the acceptance of basic assumptions anything but arbitrary. But should we accept this internalist picture in the first place? After all, isn't there an essential relationship, at least for the centralb ranches of mathematics like set theory and arithmetic, between abstract mathematics and activities like classifying and counting ordinary non-abstract objects?

Consider these two sentences:

(1) John is tall

(2) John is a member of the set of tall people.

Again, consider these two:

(3) (Ex)(Ey)(Rx & Ry & x*y & (z)(Rz -> z=x v z=y))

(4) The number of rhinoceroses in England = 2,

where "R" abbreviates "is a rhinoceros i n England".

Clearly there is an initimate relationship between the first and second members of these pairs. On the surface at least, the second sentence in each case mentions an abstract object -- a set, a number -- whereas the firs t member is free of any such reference. But, despite these surface differences, there is no doubt in each case that any non-philosopher who understands both sentences and accepts one will automatically accept the other.

The relation ships illustrated by these pairs hold out the promise of grounding our knowledge of abstract mathematical objects. Given the close affinity between the two sentences in each pair, it is hard to see how there can be epistemological problems about the latter platonist claims, given that there obviously aren't any about the former nominalist ones.

However, to develop this idea we need a more precise analysis of the relationships illustrated by the above pairs. In this section and the next I shall consider two possible such analyses. I shall argue that both analyses run into difficulties, and that in both cases we are in the end forced back to fictionalism.

The first analysis -- let's call it the "reductioni st" analysis -- is that the quantificational sentence (3) gives the real meaning of the apparently platonist sentence (4). (It will be convenient to focus on the second, arithmetic example, as it brings out the issues more clearly.) On this vi ew, there isn't any real reference to the abstract object 2 in sentence (4) in the first place. (4) is just a stylistic variant of (3), and no more commits us to abstract numbers that talk of doing things "for the sake of such-and-such" commits us t o sakes.

If we restrict our attention to pairs like (3) and (4), then this argument has a high degree of plausibility. That there's one rhinoceros in England, and another one, and no more, indeed seems to be just what is meant by sa ying that "the number of rhinoceroses in England equals two". So in this kind of case the apparent reference to an numerical object, two, can quite happily be viewed as a mere figure of speech. (4) commits us to rhinoceroses, but not numbers.& nbsp; What is more, if (4) is equivalent to (3), it is easy to see how we could establish that it was true, by counting or some equivalent procedure.

So far this deals with statements that attach a number to a non-numerical concept like " rhinoceros in England". But what about statements of pure arithmetic, like

(5) 2 + 3 = 5?

Here too the reductionist has a plausible line. To simplify our notation, let us abbreviate (3) above as (E2x)(Rx), and u nderstand further numerical quantifiers (Enx) analogously. Then the natural reductionist move is to read (5) as really saying

(6) (V)(W)[(E2x)(Vx) & (E3x)(Wx) & -(Ex)(Vx & Wx) -> (E5x)(Vx v Wx)].

That is, we can read (5) as saying merely that if there are two Vs and three different Ws, then there will be five things which are V or W. The numerals here just indicate the kind of quantification involved, and don't refer to numbers.

It is by no mean s implausible that (6) gives the real content of (5). What is more, (6) is a logical truth, and so, assuming still that we can give a satisfactory account of logical knowledge, the rendering of (5) as (6) accounts for our ability to know such truth of simple arithmetic as 2 + 3 = 5.[6]

This then offers the model for a reductionist account of arithmetic in general. First of all reductionists parse away apparent references to numbers as abstract objects in favour of qua ntificational constructions. And then they aim to show that the truths of arithmetic reduce to truths of logic.[7]

However, this programme runs into difficulties when we come to more complicated arithmetical statements, suc h as "there is no greatest prime number". There is, it is true, a quantificational version of even this statement, which once more is free of any commitment to numbers as such. But now the quantificational version is extremely complex, involvi ng not just second-order, but third-, fourth- and fifth-order quantifiers, and it becomes much harder to see in exactly what sense the quantificational version is equivalent to the orginal number-theoretic claim.

Perhaps the reductionist need not not be unduly worried by such complexity. Why shouldn't the surface structure of arithmetical statements conceal hidden logical articulation? But there are further problems. Consider again the simple arithmetic truth 2 + 3 = 5. A couple of paragraphs ago I allowed that this was plausibly equivalent to a second-order logical truth. But suppose that what we're counting is not rhinoceroses, say, but numbers themselves, as in "If there are two numbers which are F, and another three numbers which are G, then there are five numbers which are F or G". In line with the reductionist programme, this will come out as a fourth-order logical truth. And similarly there will be yet higher-order "versions" of 2 + 3 = 5. T he reductionist seems to be forced to say that "2 + 3 = 5" is ambiguous, hiding a number of distinct "real" contents behind its surface structure. But this is surely unacceptable. It's one thing to say that surface structure is misleading as t o the hidden content of arithemtical statements. It's another to maintain that straightforward arithmeticial statements don't have an unequivocal real content at all.[8]

To leave the example of arithmetic for a moment, it i s worth pointing out that similar problems of ambiguity will arise if we attempt to apply the reductionist programme to mathematics in general. The natural strategy here would be (a) to reduce other branches of mathematics to set theory, (b) appeal to the affinity illustrated by (1) and (2) above to argue that the apparent reference to sets conceals the real quantificational content of the reduced mathematical theories, and (c) to aim to show that the reduced theories all are logical truths.

&nb sp; Two problems of ambiguity face this programme. To start with, there is the point that the simple notion of set will correspond to different types at different levels of logic, analogously to the above way in which numbers come out differen tly at different logical levels. And there is also an additional difficulty, because of the familiar point that there are in general many alternative ways of reducing braches of mathematics like real analysis, say, to set theory, all of which preser ve the relevant logical structure, but which give different set theoretical surrogates for given statements of analysis. For both these reasons the thought that logical reduction gives the "real" content seems to lead to the unattractive conclusion that straightforward mathematical claims conceal hidden ambiguities.

In the face of such problems, defenders of the reductionist programme tend to shift position, and allow that in the end that mathematical statements, as meant by mathema ticians, do after all essay reference to simple mathematical objects like numbers, and that because of this such statements are both free of ambiguity and psychologically manageable.[9]   They thereby limit their reductionism to the mod erate position that the legitimacy of mathematical statements derives from the availability of logically true quantificational surrogates which don't refer to abstract objects.

However, this move takes away the distinctive claims of reduc tionism. You can't have it both ways. Either mathematical statements really do mean the same as their quantificational surrogates, or they do not. If they do, you are stuck with the problems of ambiguity mentioned above. If they do not, then the fact that we should believe the quantificational surrogates doesn't establish that we should believe the mathematical statements.

Of course there is still room to argue that the affinities between mathematical and quantific ational statements show why it is harmless, and useful, to accept mathematical claims. But this is different from showing that it is right to believe those claims. Indeed this position is not significantly different from fictionalism. Th e reductionist is now arguing that it is legitimate to "accept" mathematical claims because they can in principle always be replaced by logically true quantificational surrogates. But, as we shall see, fictionalists hold a very similar view, though for somewhat different reasons, in that they hold that our "acceptance" of mathematical claims about abstract objects is all right because in principle mathematics doesn't allow us to do anything that we couldn't do by logic alone.[10]   ; The fictionalist, however, goes on to insist that since these mathematical claims, which commit us to abstract objects, are not equivalent to logical claims, which do not, and since we have no epistemological warrant for this extra commitment to abstrac t mathematical objects, we ought to stick to the "acceptance" of mathematical claims, and eschew belief. Similarly, once somebody of reductionist sympathies admits that mathematical claims do refer to abstract objects, and so are not equivalent to q uantificational surrogates, however significant those surrogates may be for understanding why references to abstract objects are useful, then the reductionsist has ceased to offer an argument for believing mathematics.[11]

< H3> 6.7 Neo-Fregeanism

I have just argued that, once we allow that mathematical claims commit us to abstract objects, then we cannot continue to view them as equivalent to quantificational claims, on the grounds that the latter do not comm it us to abstract objects. Crispin Wright, in

Frege's Conception of Numbers as Objects (1983), disagrees, for arithmetic at least, on the grounds that quantificational claims do commit us to abstract objects like numbers.

Let us ret urn to the equivalence:

(3) (Ex)(Ey)(Rx & Ry & x*y & (z)(Rz -> z=x v z=y))

(4) The number of rhinoceroses in England = 2.

Wright agrees with the reductionist that (3) and (4) mean the same. But he thinks that (3) gives the real meaning of (4), rather than the other way around. That is, he thinks that the surface form of the quantificational (3) is misleading, and that we ought to recognize that underneath its surface it commits us not just to rhinoceroses, b ut to the number two as well.

Since he holds that arithmetic does commit us to abstract objects, Wright needs a non-reductionist epistemology for arithmetic. To this end, he introduces an equivalence between the following two schemas (whi ch is in effect a generalization of the equivalence of (3) and (4)):

(7) The Fs can be put into a one-to-one correspondence
with the Gs.

(8) The number of Fs = the number of Gs.

Wright calls this eqival ence "N=", and he proceeds to show that it implies all of Peano's postulates, and hence all of arithmetic, in the context of second-order logic.[12] The resulting system, which is closely modelled on Frege's Grundlagen, treats the num bers themselves as objects in the range of first-order variables. It uses second-order quantification, but, unlike the reductionist programme, nothing higher. However, where the reductionist programme promises to account for all arithmetical k nowledge as purely logical knowledge, Wright needs to add N= to logic, since there is no question of justifying statements which commit us to numbers as objects by pure logic alone.

The success of Wright's programme thus hinges cruciallyo n the status of N= itself. Wright takes this to be a conceptual truth, despite the fact that (8) refers explicitly to numbers but (7) does not. His line here is the same here as with (3) and (4). He thinks that the reference to numbers a s objects in (8) is indeed to be taken at face value. But he doesn't think that this undermines the conceptual equivalence of (8) with (7), because he thinks that (7) itself commits us to numbers as objects, even if its surface structure conceals th is fact.

In support of Wright's view that the reference to numbers in (8) should be taken at face value, we can observe that the numerical expressions appearing in (8) certainly seem to function like genuine singular terms in these, and o ther, contexts. They can flank identities, they allow existential generalization, and so on. This creates a strong prima facie case for reading these terms referentially,[13] and provides a serious challenge to anybody who wants to de fend a non-referential interpretation (a challenge which, as the last section showed, the reductionist, for one, is unable to meet).

Yet, once we accept this referential reading of (8), then, given the conceptually equivalence of (8) with (7), it immmediately follows that somebody who asserts (7) is already committed to numerical objects, even if it doesn't look like it.

Or so at least Wright argues. The difficulty with this line, however, is that by urging the genu ineness of the numerical singular terms in (8), Wright thereby undermines the analytic equivalence between (7) and (8).

Recall that the schemas at issue are:

(7) The Fs can be put into a one-to-one correspondence
; with the Gs.

(8) The number of Fs = the number of Gs.

I am entirely happy to agree with Wright that instances of (8) are genuine identity statements which commit us to numbers as objects. However, this claim surely takes away a ny original reason we had for accepting the analytic equivalence of (7) and (8). For on the face of it, where (8) commits us to numbers as objects, (7) does not.

In Frege's Conception of Numbers as Objects, Wright does not really ad dress this objection. This is because he takes his main opponent to be the reductionist, and accordingly takes N= to be agreed as an analytic truth on all sides. Wright's concern is then to merely to show that, once it is agreed that N= is ana lytically true, his reading of N= is superior to the reductionist reading. I agree that the his reading is superior to the reductionist reading, if we assume (7) and (8) are analytically equivalent. But my point is that, once we move to Wright 's reading, then we ought to question whether (7) and (8) are equivalent, as asserted by N=, in the first place.

After all, the most natural way to read (8) is as increasing our ontological commitments, beyond what is required by (7).&nbs p; If we adopt this reading, then we will agree with Wright that (8) involves genuine commitment to numbers as objects. But we will deny, precisely for this reason, that (7) is analytically equivalent to (8). After all, it certainly doesn't lo ok as if (7) requires us to believe in numbers as well as everyday objects. We can certainly imagine a community, for instance, who understood statements like (7), but who had no notion of a numerical object.

Wright would object tha t the possibility of such a community isn't conclusive: the crucial issue is whether, once the community has acquired the notion of a numerical object, it is then in a position to recognize that N= is analytically guaranteed. However, we can c an agree that this is the crucial issue, yet still insist that the onus is on a Fregean like Wright to produce some argument for the analytic equivalence of (7) and (8). For, as before, at first sight the relevant statements certainly seem to differ markedly in ontological commitment.

Wright holds, plausibly enough, that N= will play a central part in any adequate introduction to the concept of number. But this doesn't suffice to make N= an analytic truth. Consider an an alogy. Some such thought as that electrons are negatively charged objects orbiting the nuclei of atoms is no doubt essential to any adequate introduction to the concept of an electron. But that doesn't make it an analytic truth that, if there are atoms, then there are electrons. For a commitment to electrons is an extra ontological commitment, over and above any commitment to atoms, as is shown by the example of late nineteenth-century chemists, who believed in atoms, but not in electron s. What is analytically true is this: if there are any small, negatively charged entities orbitting the nuclei of atoms, then those objects are electrons. But this is not enough to derive, from claims about atoms, claims about electrons.   For that we need extra evidence that there actually are small, negatively charged entities orbiting the nuclei of atoms.

Yet this is what Wright seems to think we can do for numbers. Let us grant Wright that any adequate intr oduction to the concept of number will contain the information that the same number attaches to equinumerous concepts. Still, it doesn't follow that N= is an analytic truth. For, just as with the example of electrons and atoms, numbers may inv olve an extra ontological commitment, over and above that required by equinumerous concepts. What is certainly analytically true is that, if there are any numbers, then the same number will attach to equinumerous concepts. But this in itself d oesn't suffice to take us from premises about equinumerous concepts to conclusions about numbers. In order to make that move, we need some independent argument for supposing that numbers actually exist.

6.8 Mathemati cal Anti-Realism Rejected

In the absence of any such argument, I think we ought to reject Wright's neo-Freagean account of arithmetic. And, more generally, I think that we ought also now to reject the overall anti-realist approach to mathe matics. The initial objection to this anti-realist approach, made in section 6.4, was that mathematical evidence, in the form of proofs, only seems to establish hypothetical claims, while mathematics itself consists of unconditional assertions. ; In answer to this objection, I offered the anti-realist various ways of discharging the axioms assumed in mathematical proofs: first, we considered if-thenism, which read all mathematical claims hypothetically; then I discussed postulationis m, which argued that, for any consistent and categorical set of assumptions, the abstract objects exist which make them true; after that came reductionism, which claimed that mathematical assumptions could all be construed as truths of pure logic;&n bsp; and finally there was Wright's neo-Fregeanism, which claimed that the crucial assumptions necessary to introduce abstract objects are simply analytic truths. None of these strategies has proved defensible, and it is difficult to think of any ot her a priori argument for the view that the assumptions standardly made by mathematicians about abstract objects are automatically true. It seems to me that is now time to conclude that no such anti-realist defence of mathematics is available, and t o accept that the content of mathematical claims does indeed conceptually outstrip the grounds on which mathematicians make them.

I realise that some readers will find this difficult to stomach. Surely, they will say, the truth cond itions of mathematical judgements cannot possibly transcend our grounds for asserting those judgements. Are not the contents of such judgements fixed by the grounds which our practice authorizes as sufficient for their assertion? So what could possibly make it the case that a mathematical statement stands for something that goes beyond such grounds?

But my thesis is precisely that the contents of mathematical judgements are not fixed by the grounds our practice recognizes as s ufficient. I am denying that such an anti-realist model of meaning is acceptable for mathematics. It may be helpful, in this connection, to consider the fate of anti-realist thinking in the analogous context of the interpretation of scientific theories. In the first half of this century many philosophers were attracted to the view that theoretical terms in science were a disguised shorthand for describing complexes of observational circumstances. This was, of course, an absurdly co unter-intuitive view. It is scarcely credible to suppose that scientists who talk about "electrons" are in fact talking about the behaviour of oil drops, tracks in cloud chambers, and so on, and not about the small negatively charged objects which o rbit the nuclei of atoms. But philosophers had difficulty seeing how scientists could be talking about small negatively charged objects. Since the authorized grounds for applying the term "electron" are always observable circumstances, what co uld possibly justify us in interpreting the scientists as making some further insecure reference to invisible entities?

Frank Ramsey (1931) explained how scientists manage to refer to unobservables. "Electron" does not have its mean ing fixed just by association with the observable symptoms of electron behaviour. It also gets its meaning from its role in a theory which postulates the existence of small particles which orbit atomic nuclei and are responsible for those observable symptoms. Ramsey showed how statements about electrons can be read as existentially quantified statements, which say that there exist particles which are small, negatively charged, orbit atomic nuclei, have certain observable symptoms, . . .

&n bsp; In effect, Ramsey shows that talk about scientific unobservables derives from our ability to make existential claims about object which are not immediately accessible. I suggest that this same ability makes it possible for mathematical cl aims to answer to proof-transcendent states of affairs. In the case of arithmetic, say, we have a theory which postulates the existence of objects with certain properties, namely, just those properties which flow by logic from N=. We call thes e putative objects numbers. But the basis of our ability to make claims about numbers, namely, our power of existential generalization, is independent of any further abilities we may have to prove such claims.

It must be allowed, of course, that we have an established discursive practice of making arithmetical claims, and that a central role in this practice is played by N=. But the existence of this practice does not justify N=, nor the arithmetical claims which follow from i t. For, as we have seen, N= is not analytic, but a synthetic claim, which inflates our ontology by postulating entities we are not otherwise committed to. As such it cannot be justified just on the grounds that it is part of an established dis cursive practice. Analytic truths are justified by facts about linguistic usage. But synthetic claims require some other warrant.

One last throw is available to mathematical anti-realists. They can deny that "existence", in the context of mathematical discourse, is to be understood in the same way as in other areas of discourse, and thereby hope to argue, for example, that the "existence" of numbers is analytically guaranteed by facts of equinumerosity. But this mo ve is not only unattractively ad hoc -- since there is no other reason, apart from the threat of scepticism, for suspecting mathematical existence claims of equivocation -- but it is also likely to prove a two-edged sword -- since the anti-realist will st ill have to explain what "existence" means for mathematical objects, and why it is different from fictional non-existence.

The argument of this section has in effect shown that mathematical discourse falls within the scope of the teleological theory of content after all. For I have now argued that mathematical terminology can be introduced, a la Ramsey, by existential quantification into theoretical contexts. This means that mathematical discourse rests on no special vocabulary, but simply on the existential quantifier we use in general discourse. A corollary is that the semantic realism of mathematical discourse is just a special case of the general semantic realism which emerges from the teleological theory.

6.9 The Putnam-Quine Defence of Mathematics

If mathematical claims have a realist semantics, they face a threat of scepticism. Still, perhaps this threat can be met. After all, scientific theories are able to cope with the threat of scepticism. Perhaps mathematics can be defended against scepticism in the same way as scientific theories are.

This might seem a faint hope. However, there is a well-known line of argument, propounded by Hilary Putnam (1971), and originally due to Quine, which seeks to vindicate claims about abstract mathematical objects by arguing that such claims play an ineliminable role in scientific theories about the natural world. So far I have been taking mathematics to consist of pure mathematics, such as arithmetic and analysis. But references to abstract mathematical objects, and in particular to the natural and real numbers, are also regularly made in the applied sciences, as when we say, "the number of planets = 9" , or "the distance-in-metres between two particles = 5.77". The Quine-Putnam point is that if scientific claims of the latter kind are epistemologically warranted, as surely they often are, and if those claims commit us to real numbers as objects, as they certainly seem to, then claims about real numbers must be epistemologically warranted too.

An extension of this line of argument promises to vindicate, not just mixed statements of applied mathematics, as in the above examples, but also the axioms on which pure arithmetic and analysis are based, such as that every number has a successor, or that every set of reals has a least upper bound. For these assumptions are presupposed in the mathematical calculations which we use to derive predictions from scientific theories, as when we add together the numbers of stars in different galaxies, say, or divide forces by masses, and so can be argued to be confirmed, along with the rest of such theories, when such predictions prove successful.

From the point of view of my general epistemological framework, this argument amounts to a realist defence of mathematics. Quine and Putnam would no doubt not think of it in this way, given their generally pragmatist attitudes to scientific truth. But, on my account, scientific theories have realist contents, yet qualify as knowledge because the methods by which we choose them are reliable for truth as a matter of empirical fact. So if mathematical theories qualify as knowledge as part and parcel of scientific theories, then they will share this realist epistemological status with scientific theories.

One difficulty facing the Quine-Putnam argument is that it seems unable to account for the difference between the research methods of pure mathematicians and natural scientists. Where scientists actively seek to vindicate their theories by experimental means, pure mathematicians seek a priori proofs. The idea of a pure m athematician trying to estbalish some mathematical principle by experiment seems silly. Yet the Quine-Putnam argument seems to imply that pure mathematics and natural science share the same epistemological status.

This objection, ho wever, is less conclusive than it looks. It is certainly true that the Quine-Putnam argument implies that basic mathematical principles are supported by observations. But this doesn't imply that there will be some specific experiment that bear s on each such principle. Rather, as with the basic laws of motion, large numbers of observations will contribute to the support of mathematical principles in a holistic manner, by confirming the overall theory in which they play a part. As to the r ole of a priori proof in mathematics, we can accept that basic mathematical principles depend on observational support, without denying the importance of exploring the purely logical consequences of those principles. In line with this, we might view the overall scientific enterprise as containing a division of labour: the scientists conduct experiments which will shape the overall tree of scientific theory; while the mathematicians explore the purely logical consequences of the assumptio ns that lie at the root.

Perhaps there remains something counterintuitive in the idea that the axioms of Peano arithmetic have the same epistemological status as Newton's laws of motion. I shall not pursue this issue any further, ho wever, since there is a rather more telling objection to the Quine-Putnam argument, elaborated in Hartry Field's Science without Numbers (1980).

6.10 Field's Fictionalism

Field argues that the crucial premise of the Quine-Putnam argument, that pure mathematics is an inextricable part of natural science, is unwarranted. There are two parts to Field's claim here. First, he argues that we can say everything we want to say about the natural world in "nomi nalist" terms, that is, without mentioning abstract objects. When it comes to arithmetic, for example, his claim is that we can always describe the natural world using quantificational statements like "(E2x)(Fx)" or "F and G are equinumerous", and c an thus avoid any commitment to numbers as objects. And in the case of geometry, to take another example, he argues that talk which commits us to distances as real numbers can always be dispensed with in favour of claims about relations of congruenc e between different spatial intervals. Field argues that similar procedures will allow all claims about the natural world to be understood as free of commitment to abstract objects.

Second, Field argues that whenever we use abstract mathe matics to facilitate inferences between such "nominalist claims" -- and Field admits that abstract mathematics often enables us to find a simple route through inferences that would otherwise be impossibly complex -- we could in pri nciple always make the inferential step by logic alone. As Field puts it, mathematics is conservative with respect to inferences from nominalist premises.

In line with these arguments, Field concludes that there is no good agument f or believing the claims of abstract mathematics, and that we should therefore reject these claims. This doesn't mean we should simply away throw all mathematical claims as complete rubbish. As I have just observed, Field accepts that mathemati cs is often immensely convenient for making inferences, and accordingly recommends that we should adopt the fictionalist view that mathematics is a useful pretence. But the point remains that this kind of usefulness provides no basis for belief, but only for the kind of attitude that we have towards the statements in a fictional narrative, or -- perhaps a better analogy -- the kind of attitude that we have towards the Coriolis force, or towards the Newtonian theory of gravita tional forces in Euclidean space. We don't believe in these theories or the entities they postulate, but we know they will work satisfactorily enough when certain purposes are at hand, and then we find it convenient temporarily to pretend that theya re true.

In a sense Field's motivations are similar to those of the reductionist discussed earlier -- he too wants to do without abstract objects. But instead of arguing, implausibly, that our existing mathematical disco urse is free of commitment to abstract objects, his strategy is instead to admit that mathematics does commit us to abstract objects, but show how we could in principle manage without mathematics.

There are a number of technical difficult ies facing Field's programme, both in respect of the first claim, that natural science can be "nominalized", and especially in respect of the second claim, that, within such a nominalized science, mathematics won't ever underpin any inferences that logic can't (cf Malament, 1982; Shapiro, 1983; Chihara, 1990). But for the most part I propose to skip these technicalities here (though I shall touch on some of them in section 6.15 below). After all, there are strong prima facie reasons for expect ing them to be surmountable: it would surely be surprising if descriptions of the natural world of space and time required essential reference to abstract objects outside space and time; and it would be almost as surprising if logically possib le combinations of natural facts were incompatible with standard assumptions about abstract mathematical objects, as would be the case if mathematics were not conservative with respect to nominalist premises.

Suppose then that we agree th at abstract mathematics can be successfully extricated from the rest of science, in the way Field has in mind. It is worth considering a bit more carefully exactly why the rejection of mathematical beliefs is supposed to follow. After all, wha t Field has shown is that we can do without mathematical beliefs, in the sense that our scientific beliefs do not require mathematical beliefs. But this is scarcely the same as showing that we ought to do without mathematical beliefs. Why shou ldn't we still retain mathematical beliefs, in addition to scientific beliefs?

Consider the analogous issue as to whether we ought to have beliefs in scientific unobservables, in addition to beliefs about observables. Craig's theore m shows that such unobservable claims are extricable from the observational claims, analogously to the way that Field shows mathematical claims are extricable from "nominalist" claims. In this sense, Craig's theorem shows how we can do without belie fs about scientific unobservables.[14] However, few philosophers nowadays would want to infer from Craig's theorem that we ought to do without such beliefs, that our beliefs about scientific unobservables are all unwarranted.

&nbs p; To conclude that we ought not to adopt a given belief, just because we don't have to, is to betray an overdeveloped taste for desert landscapes. A better principle would be that we ought to adopt as many beliefs as we can, on matters that a re of interest to us, as long as these beliefs can be reached by reliable methods, and so can be expected to be true.

In line with this principle, and following the points made at the end of the last chapter, I would offer the following e xplanation of why we are entitled to believe theories about scientific unobservables: the procedures that lead us to believe such theories are reliable routes to the truth, since nature generally prefers the kind of simplicity recognized by scientis ts to the kind of complexity that would obtain if reality were exhausted by the observable phenomena.

However, if this is our reason for upholding scientific theories, then why can't we defend mathematical theories in the same way? W hy shouldn't we argue that scientific theories which incorporate mathematics display a greater degree of basic simplicity than nominalized theories which don't incorporate mathematics?

This wouldn't be the same as the Quine-Putnam argumen t that mathematics is inextricable from scientific theories. The idea would rather be that the addition of mathematics to scientific theories is justified in the same way as the addition of unobservable claims to observable claims. This is a d ifferent kind of realist defence of mathematical theories. Instead of arguing that the leap to mathematical knowledge is part of the leap to scientific knowledge, we are now accepting that mathematical knowledge requires an extra leap, but suggestin g that it might be achieved by the same technique as the leap to scientific knowledge.

However, if we examine this idea more closely, it doesn't really work. We can all agree, fictionalists included, that the incorporation of pure m athematics into scientific theories adds to the ease with which they are manipulated by human beings. But this is not the kind of simplicity at issue. What guides us in our choice of scientific theories is not computational simplicity but phys ical simplicity, the kind of simplicity manifested by the atomic theory of matter, or the relativistic theory of gravitation. These theories show that the world contains fewer independent phenomena than we might initially suppose, and as such accord with the general pattern of physical simplicity which allows us to gain knowledge of the natural world. But the incorporation of pure mathematics into scientific theories does not add to this kind of simplicity, but subtracts from it: it requ ires that we should recognize, in addition to the nominalized world of distances, forces and other interrelated natural quantities, a world containing real numbers, and sets, and other purely mathematical objects. This might make it easier to do calculati ons, but it receives no backing from principles of scientific theory choice.

Field (1980) makes this point in terms of a contrast between "intrinsic" and "extrinsic" scientific explanations. Mathematized scientific theories commit u s to "extrinsic" explanations, to the kinds of explanation which explain a body's acceleration, say, as depending inter alia on the body's connection (via a mass-in-some-units relation) with a real number outside space and time. A nominalized versio n of this theory, by contrast, will explain accelerations in terms of "intrinsic" masses which do not commit us to real numbers. It seems obvious that a theory which needs to invoke relations to real numbers to explain accelerations has less physica l simplicity that one which does not.

So, to sum up, the arguments in this section and the last show that pure mathematics cannot be be given a realist defence, either on the Quine-Putnam grounds that they are inextricable from scientific theor ies, or on the grounds that their addition to scientific theories adds to physical simplicity. Since I can think of no other prospects for a realist defence, and since we have already decided against anti-realist defences, I conclude that we ought t o adopt a sceptical fictionalism about mathematics.

Bob Hale (1987, ch 5.II) has argued, against Field, that mathematical fictionalists cannot coherently maintain

(a) that mathematics is false

(b) that the truth of mathemati cs is possible, and

(c) that mathematics, if it were true, would have no consequences in the nominalist world that do not already follow from nominalist truths, as is required by Field's conservativeness claim.

Hale's thought is that if mathematic s is only contingently false, then its falsity ought to make some nominalist difference, ought to manifest itself by the failure of certain nominalist consequences.

However, I don't see why fictionalists cannot maintain the conjunction of (a), (b) and (c). Fictionalists are certainly committed to (a) and (b). By definition they think mathematics is false. And, since they take mathematics to be making meaningful existence claims, they do not think that its truth is ruled out by logic or concepts alone. But there seems no obvious reason why they should not believe (c) too, and deny that the contingent falsity of mathematics need show up in the nominalist world. After all, the fictionalist objection to mathemati cs is not that we can detect definite symptoms of its falsity in the natural world, but rather that neither this world nor anything else provide us with any grounds for believing it.

If there is an objection here, it is that lack of groun ds for belief might seem only to warrant suspension of belief, rather than active disbelief. In the face of this objection, the fictionalist could simply acquiesce, and agree that we ought to combine our fictionalist "acceptance" with a neutrality o f belief, rather than an outright rejection of mathematics. But this seems unnecessarily weak-kneed. On our current understanding, mathematical theories invite us to inflate our ontology by adopting synthetic bridge principles. If there are no positive grounds for these principles, other than that the entities they posit are possible, then surely the appropriate attitude is disbelief rather than neutrality. If someone urges that there are little green men on the first planet of Proxima C entauri, but by way of evidence offers us only that these men are possible, then surely we ought to reject this claim outright, rather than afford it the courtesy of agnosticism.

6.11 Morality and Modality

Many o f the above arguments about mathematics are parallelled for morality and modality. In both these areas anti-realism seems initially promising: that is, it seems initially plausible that truth, for either moral or modal judgements, might be con ceptually guaranteed by the availability of some discursively authorized way of establishing these judgements.

However, when we turn to the details of our agreed discursive practices, they seem to hinge on certain crucial assumptions, ana logous to the arithmetical N=, which provide a bridge between non-moral or non-modal claims, on the one hand, and distinctively moral and modal claims, on the other. For example, in morality we have bridge principles such as that killing people is b ad, and that causing happiness is good; in modality we have bridge principles such as that anything provable from no premises is necessary.[15]

But, now, when we ask for the warrant for these claims, epistemological difficu lties arise. Reductionist readings, which say that "good" means nothing more than "increased happiness", or that "necessary" means nothing more than "provable from zero premises", seem clearly not to do justice to the intended meaning of the moral a nd modal terms. Yet once we allow that moral and modal claims take us beyond claims about happiness and provability, then we can no longer assume that the crucial bridge principles are simply analytic truths guaranteed by the meanings of the terms i nvolved. And this undermines the prospects for an anti-realist defence of moral and modal claims. For if the bridge principles are substantial synthetic claims, we cannot simply maintain that they are somehow automatically guaranteed by the st ructure of our discursive practice.

On the other hand, there seems little likelihood of a realist vindication of such judgements, a vindication which accepts that the content of moral and modal claims take us beyond what our discursive pr actices automatically guarantee, but which nevertheless argues on a posteriori grounds that these practices reliably generate truths. So the only option left seems to be some version of sceptical disbelief.

These moves are perhaps m ost familiar in the case of morality. Thus Hume long ago pointed to the non-analyticity of moral bridge principles, by observing that you can't infer an "ought" from an "is"; and G.E. Moore's analogous objection to the "naturalistic fallacy" i s that it always makes meaningful sense, given any natural description of some situation, to query whether it satisfies any further moral description (Moore, 1903). These doubts about the analyticity of moral axioms make it difficult to provide any anti-realist epistemology for morality. On the other hand, once we do allow that the content of moral claims transcends the moral evidence, there seems little hope of a realist demonstration that this evidence is nevertheless a reliable guide to the moral truth. (Moore's intuitionism can be seen as a desperate attempt to provide such a realist epistemology.) So we seem pushed towards scepticism, and indeed we find this position explicitly defended in, for example, J.L. Mackie's error the ory of ethics (Mackie, 1977).

The same moves are also discernible, if somewhat less familiar, in the philosophy of modality. Thus Simon Blackburn (1986, pp 120-1) argues that any attempt to build an analytic bridge between non-modal premises and modal conclusions must fail to do justice to the distinctively modal character of those conclusions: how can the fact that our language happens to contain certain proof procedures guarantee that certain things are necessary? Yet, if such bridge principles aren't analytic, it is difficult to see how modal anti-realism can work. On the other hand, a realist epistemology for modality seems even less likely than for morality. So again we seem pushed towards some kind of fiction alist scepticism.

6.12 The Non-Doxastic Alternative

However, there is an alternative to scepticism about morality and modality. So far I have been taking it for granted that moral and modal claims are expressions of belief. But perhaps we shouldn't read them in this way in the first place: instead of reading them as beliefs about some distinctive kind of non-natural fact, perhaps we should read them as expressing a distinctive non-doxastic attitude towards natural facts. Thus, as a first shot, there is the option of reading moral judgements as expressing some kind of impartial approval.[16] And, similarly, there is the option of reading modal judgements of necessity as expressing our unqualified commitment to certain forms of argument.[17]

If such a non-doxastic account of moral and modal claims is right, then the arguments for scepticism fall away. It will no longer be an i mmediate objection to the relevant bridge principles, for instance, that they are synthetic assertions that lack empirical evidence. For, on the construal now being considered, the bridge principles will not be assertions at all, but rather prescrip tions or permissions about adopting certain non-doxastic attitudes when certain natural facts obtain. That "killing people is bad" will no longer license inferences to moral beliefs about acts of killing, but simply prescribe a negative moral attitu de to such acts. Similarly, that "propositions provable from zero premises are necessary" won't entitle us to any beliefs about necessity, but just endorse our unqualified commitment to syntactically valid arguments. There will of course remain room to debate the appropriateness of these prescriptions and permissions. But the lack of empirical evidence for the corresponding assertions will cease to be an obvious objection.

This line of thought thus points to the possibility of a position about morality and modality, which would be manifestly different from scepticism, in that it would recommend upholding our existing moral and modal judgements, rather than rejecting them. But this would not be because it recommended beli eving them, on either realist or anti-realist grounds, but rather because, on a proper understanding of their significance, questions of belief would simply not be at issue.

This non-doxastic suggestion about morality and modality raises some obvious questions. What justification is there for denying that moral and modal claims are expressions of belief, especially given that they share many features of normal belief-expressing assertions? And, if is right to read moral and mo dal claims in this way, then why isn't it right to read mathematics similarly non-doxastically, and thereby avoid scepticism about mathematics?

In the next two sections of this chapter I shall try to answer these questions. A final section will then say a bit more about how a non-doxastic interpretation might work for modality. This issue, like the analogous issue about morality, about which I shall say little further, is far too large a topic to treat properly here. But since the fictionalist account of mathematics needs various modal claims, it will be appropriate to finish this chapter with some brief suggestions on the topic.

6.13 Why are Morality and Modality Non-Doxastic?

My suggestion is that moral and modal utterances be understood as expressing some attitude other than belief. However, I can imagine that some readers will be dubious about the force of this idea. What exactly is the significance of denying mo ral and modal judgements the title of "belief"? In particular, what real difference is there then between my position, and that of an anti-realist who says that truth, for a moral or modal belief, is something conceptually guaranteed by the availabi lity of a authorized derivation within our existing practice? After all, the point of the non-doxastic option is supposed to be that it allows us to continue upholding moral and modal judgements. But if I thus allow that our existing practice for making such judgements is entirely appropriate to the content of those judgements, and that we are therefore quite entitled to the moral and modal judgements that we make, then what makes me different from the anti-realist who argues on just these gro unds that we should uphold moral and modal beliefs?

This query can be made even more pressing if it can be shown, as arguably it can, that the structure of moral and modal discourse closely matches the structure of discourses which do exp ress beliefs, in respect of conforming to the canons of logic, allowing conditional constructions, accepting that currently accepted views may come to be overturned, and so on.[18]

However, I think that there is a good reason why it would be wrong, despite all these similarities, to assimilate moral and modal judgements to normal beliefs. Namely that, on our present assumptions, moral and modal judgements behave differently from beliefs in epistemological contexts. I argued in section 6.11 that, if moral and modal judgements were beliefs, then the bridge principles which take us from natural premises to moral or modal conclusions would be unwarranted. It is only when we view moral and modal conclusions as non-do xastic that we are free to read those bridge principles as acceptable prescriptions, rather than unevidenced beliefs. So we need to deny the title of belief to moral and modal claims, if we want to continue upholding them.

It might seem as if this reason for denying the title of belief to moral and modal claims is purely a philosopher's reason, which doesn't reflect anything in the cognitive workings of ordinary people, but only philosophical anxieties about epistemological "justifi cation". However, I don't think that questions about justification can be sliced off from the first-order reality of psychological states in this way.

By way of analogy, consider the case of a scientist who uses a theory about unobs ervables for making observational predictions, accounting for observable results, designing experiments, and so on. According to Bas van Fraassen (1980), somebody can do all this, and yet be an instrumentalist in not believing what the theory says a bout unobservables. Paul Horwich (1991) has queried the cogency of Van Fraassen's position here, on the grounds that, once scientists "think with" a theory to the extent that Van Fraassen allows, then there remains no substance to the thought that t hey do not believe the theory.

However, I think that substance can be given to this thought, contra Horwich, by taking into account the scientists' epistemological dispositions. Imagine that it is pointed out to certain scientists t hat, despite its usefulness, a given theory does not satisfy the epistemological requirements normally imposed on beliefs about things we are not directly acquainted with. The scientists' subsequent response will disclose whether or not they believe the theory: if they are unperturbed by the theory's admitted epistemological shortcomings, then we can infer that their attitude is not belief, but merely instrumentalist acceptance; on the other hand, if they are unable to view the shortcomi ngs with equanimity, and accept that they require a change of attitude, then this shows that their original attitude was belief.[19]

The point of these last remarks about scientific theories has been to show how belief is distinc tive among psychological attitudes in answering to certain epistemological obligations. And so, to return to the main thread of argument, if moral and modal attitudes do not answer to these obligations, then this is itself a good reason for placing these attitudes outside the category of beliefs. This now explains why the non-sceptical position about morality and modality I have outlined in this section is genuinely different from anti-realism. Anti-realism needs to show that our practic e is adequate for generating moral and modal beliefs. I agree that our practice is adequate, but say that this is precisely because moral and modal claims aren't beliefs. (If they were beliefs, they would then be subject to certain damaging ep istemological requirements, which would then leave us with scepticism, not anti-realism.)[20]

It is perhaps worth emphasizing that in denying the title of belief to moral and modal judgements, I do not want to suggest that they a re unimportant, or that the difference between correct and incorrect such judgements is somehow arbitrary. On the contrary, I fully accept that both moral and modal judgements play a central role in the thinking and action of human beings, and that their doing so requires that they conform to fully impartial standards of correctness. I would also like to emphasize that I not want want to disqualify moral and modal judgements from the category of beliefs just because they report on matter which are non-natural, or abstract, or outside the causal world of space and time. I have been happy to concede the possibility of such non-natural beliefs from the beginning of this chapter. Indeed this is precisely my view of mathematical judgeme nts: I take these to be beliefs about non-natural states of affairs (albeit beliefs that we have no warrant for adopting).

6.14 Why isn't Mathematics Non-Doxastic?

This brings us to the next question: namely, if moral and modal attitudes can be saved from scepticism by the non-doxastic option, why isn't the same true of mathematics? Why has my analysis of mathematics led to scepticism, rather than the endorsement of some non-doxastic species of mathematical judgement?

After all, isn't the fictionalist attitude to mathematics itself an instance of such a non-doxastic attitude? Once we embrace fictionalism, then we uphold mathematical judgements -- not as beliefs, it is true , but nevertheless as claims that can properly be accepted-in-the-mathematical-fiction. So why isn't this just another range of judgements that can be upheld because they don't express beliefs?

Well, once we do embrace fictionalism, then the resulting attitude to mathematical judgements does evade scepticism. But the difference between mathematics, on the one hand, and morality and modality, on the other, is that, prior to philosophical argument, most people adopt an attitude of belief to mathematics, whereas I take it that moral and modal attitudes are already different from belief. Fictionalism, as a philosophy of mathematics, is thus advocating a revision of everyday thinking, where the non-doxastic account of moralit y and modality is happy to leave everything as it is. This is why fictionalism in the philosophy of mathematics a sceptical doctrine: it is sceptical about the beliefs that most people have, whereas the non-doxastic account of morality and mod ality has no corresponding objection to everyday thought, since it doesn't take everyday thought to involve modal and moral beliefs.

Is this contrast justified? Is it true that most people believe mathematical claims, but express di fferent attitudes in their moral and modal judgements? Well, this seems plausible to me, but I do not need to defend the thesis here. For it is an empirical matter, a thesis about the psychology of actual individuals, not a philosophical issue . In the last section I showed that there is a real difference between believing moral, or modal, or mathematical claims, and having a non-doxastic attitude to them. But, given this, the further question of how many people actually hold belief s, and how many hold non-doxastic attitudes instead, is a question for sociologists, not philosophers. The essential philosophical point can be put hypothetically, in a way that abstracts from the actual psychology of individuals, and so applies equ ally to mathematics, morality and modality: if people adopt beliefs, then they are wrong, and they should reject those beliefs, in a sceptical spirit, and switch to some less demanding non-doxastic attitude instead.

Still, as I said , it seems plausible that mathematics ordinarily involves beliefs in a way that morality and modality does not, and I shall continue to speak accordingly. It may be helpful to offer a possible empirical explanation for this conjectured empirical con trast. Note that it is entirely natural to view mathematical judgements, whatever their doxastic status, as essaying reference to a distinctive range of objects, like numbers, sets, vector spaces, and so on. With moral and moral judgements, by contrast, it is by no means so obvious there is any intended reference to distinctive objects. So there is a sense in which mathematical thought doesn't need any attitude other than belief to constitute itself as a distinctive mode of discourse:&nb sp; its range of distinctive objects already ensures that mathematical claims have a distinctive content. By contrast, if moral and modal judgements do not refer to any distinctive range of objects, then there remains a question about what gives tho se judgements their distinctive contents; and a natural answer to the question is that they express attitudes other than beliefs.

This story isn't incontestable. For a start, you might question whether a lack of intended refer ence to distinctive objects does distinguish moral and modal claims from mathematical claims. And, second, even if you do accept this, it doesn't automatically follow that mathematical claims must be understood as expressing beliefs, and moral and m odal claims other attitudes.

Let me take the latter question first. Even if, as I have been suggesting, moral and modal claims don't have any objects of their own, it might still be possible to understand them as expressing distinct ive kinds of belief: for nothing I have said so far rules out the possibility that such operators as "it is right that", and "necessarily", yield beliefs when applied to contents, rather than non-doxastic attitudes. (Though the arguments of 6.11 wou ld then still indicate a sceptical attitude towards these beliefs.) Nor, conversely, does the intended reference to distinctive mathematical objects force us to view mathematical claims as beliefs: after all, in a community of self-professed f ictionalists, claims about numbers, sets, and so on, would express fictional acceptance, rather than belief.[21] Nevertheless, even if there is no logical tie, in either direction, between distinctive objects and the adoption of belie fs, the differing involvement of objects still seems to me to offer a likely empirical explanation of why people should have mathematical beliefs, but non-doxastic moral and modal attitudes. For even if it is logically possible to combine objects wi th the absence of belief, and vice versa, it still seems psychologically plausible that people will adopt the attitude of belief to the objects they are introduced to in mathematics, but non-doxastic attitudes to non-object-involving moral and modal claim s.

There is also the former question, about the object-involving contrast: am I right to hold that mathematics involves intended reference to objects, while moral and modal claims do not?

The first part of this clai m has already been established: we have already examined accounts of mathematics which represent it as free from commitment to numbers, sets, and other mathematical objects -- namely, if-thenism and reductionism -- and reject ed them, precisely on the grounds that such non-objectual readings are not faithful to the standard content of mathematical claims.[22]

The converse issue, however, is less clear-cut. Thus there is the well-known "possible worlds" interpretation of modal discourse, which takes modal judgements to commit us to a range of non-actual universes. And similarly it is possible to construe moral claims as essaying reference such distinctive entities as rights and wrongs, virt ues and vices. I do not myself think these objectual readings of everyday modal and moral claims are compelling, but I shall not argue the point here, for little of philosophical substance hangs on it. The question of whether moral and modal c laims involve reference to distinctive objects is once more an empirical issue, a matter of the actual contents of the thoughts of actual individuals. And I have put forward the empirical hypothesis that they do not so refer only in order to explain another empirical conjecture, namely, the conjecture that such claims do not express beliefs, but some other non-doxastic attitude.

My only substantial philosophical contention remains the hypothetical thesis that, if anybody were to ado pt the attitude of belief to moral and modal claims, then the resulting beliefs would be epistemologically unjustified. And to this thesis the possibility of object-involving interpretations of moral and modal discourse poses little threat. Fo r if modal beliefs commit us to possible worlds, or moral beliefs to rights and wrongs, it will surely be harder, not easier, to justify modal and moral beliefs.

6.15 Fictionalism and Modality

In this final secti on I want to say a bit more about modality, and in particular about the use that fictionalism makes of this notion.

Note first that fictionalism needs to make assumptions about logical consequence, in at least two places. First, and most obviously, the claim that mathematics is dispensable in science rests on the premise that it is conservative with respect to nominalist truths -- that is, that no nominalist conclusions follow logically from nominalist-premises-plus-mathematics that don't follow from nominalist premises alone.

Second, fictionalists need the notion of logical consequence to identify what they mean by "mathematics" in the first place. For the kind of mathematics that fictionalists think is dispe nsable, though nevertheless useful as a fiction, isn't just any old set of claims formulated in mathematical vocabulary (for that wouldn't be conservative, or useful), but rather standard, or accepted, or good mathematics. So fictionalists owe us so me account of what standard mathematics is, some account of what exactly it is that they are recommending we accept as a fiction. And the obvious account is that standard mathematics consists of all the claims that follow logically from standard mat hematical assumptions.

I shall now show that we fictionalists need to understand such claims about logical consequence in modal terms: that is, we need to understand the claim that B follows logically from {A} as equivalent to it is not possible that {A} all be true and B false.

This may not be immediately obvious. Why can't the fictionalist avoid modality by simply appealing to the standard characterizations of logical consequence in metalogic? For exam ple, why not explain consequence semantically, saying B is a logical consequence of {A} if and only if B is true in all models in which all members of {A} are true? Alternatively, why not offer a syntactic analysis, saying that B is a logical conseq uence of {A} if and only if B can be proved from {A} using a specified set of rules of inference?

The characterizations are unquestionably of great mathematical significance. However, there are reasons why we fictionalists cannot re st with either of them as a philosophical account of logical consequence. Let me take the semantic characterization first. The obvious problem is that models are themselves abstract mathematical objects, and therefore, given the overall argume nt of this chapter, not something we can have beliefs about.[23] (In the end, of course, we should be able to regard such claims about models, along with other mathematical claims, as useful parts of the mathematical fiction. Bu t at this stage we still face the task of explaining what exactly the mathematical fiction comprises.)

The alternative metalogical characterization of logical consequence, in terms of syntax, specifies a set of rules of inference for movi ng between sentences with certain syntactic forms. The normal objection to fictionalists accepting this characterization is that rules of inferences and syntactic forms are themselves abstract objects, and so inadmissible from our fictionalist persp ective.[24] But this is less than compelling; there seems plenty of room to view such syntactic entities as features of certain physical systems, namely, languages. However, there are worse problems facing a fictionalist w ho appeals to the syntactic explanation of logical consequence. These arise from the fact that nothing stronger than first-order logic can be completely characterized in syntactic terms. So, for a start, there is a problem about the fictionali st delineation of standard mathematics as whatever follows from the standard axioms. If "follows from" simply means whatever follows by first-order logic, then Godel's theorem tells us that there will be mathematical truths which do not so follow fr om the standard axioms, contrary to the fictionalist's delineation of standard mathematics.

A related Godelian difficulty faces the fictionalist's claim that standard mathematics is conservative with respect to bodies of nominalist assert ions. For consider, as the relevant body of nominalist assertions, the claim that there exists a geometrical point, and another point, and then another as far away again in the same direction, . . . By such means we can construct a nominalist version of Peano's axioms, which refers to geometrical points instead of the natural numbers. But now there will be a "nominalized version" of a Godel sentence which does not follow logically from these axioms, if "follow from" means by first-order logic. However, this nominalized Godel sentence will follow in first-order logic if we are allowed to add pure arithmetic plus appropriate bridge principles to the nominalist Peano axioms, since pure arithmetic will include the pure version of this Godel sentence. The upshot is that pure mathematics, and in particular pure arithmetic, is not conservative with respect to bodies of nominalist assertions, if by "conservative" we mean that the addition of mathematics to nominalist assumptions gene rates no new conclusions within first-order logic.[25]

So a fictionalist cannot happily rest either with the model-theoretic or with the syntactic characterization of logical consequence. Suppose, however, that we take the modal notions of necessity and possibility as primitive, and define consequence in modal terms in the way suggested above, as the impossibility of the premises being true and the conclusion being false. This then evades the difficulties facing the s tandard semantic and syntactic characterizations. There is no obvious commitment to abstract objects like models in this definition. And since there is no reason to regard the consequence relation thus defined as restricted to first-order cons equence, the Godelian difficulties need no longer apply.

If the arguments of the last few sections are right, it follows that the appropriate attitude to claims of logical consequence will not be belief, but rather a non-doxastic attitude of unqualified commitment to the corresponding forms of argument. Of course, there is nothing to stop us believing that, whenever the premises of such arguments are true, then the conclusions will be true too, that is, that such arguments are relia ble. But the further thought, that these arguments are necessarily reliable, will not itself be a belief.[26]

As I observed in passing earlier, a non-doxastic view of necessity raises the question of whether our normal crit eria for making judgements of necessity provide appropriate grounds for the relevant non-doxastic attitude. In the present context, where we are using necessity to explain logical consequence, there are also a number of further technical issues, whi ch I cannot pursue here, about whether these criteria are adequate to the mathematical structure of logical consequence.[27] However let me make just one point. A standard soundness proof for some form of argument, such as is gi ven by the truth table for modus ponens, say, provides an obvious vindication of an unqualified commitment to the reliability of that form of argument. Such a proof can simply be thought of as arguing in the alternative, for all the possible alterna tive arrangements of semantic values for non-logical expressions which would make the premises true, that the conclusion would be true too.[28] So such a soundness proof provides an immediate basis for belief in the reliability of the form of argument in question. And since the proof hinges on no assumptions save those about the meaning of logical expressions, it also provides obvious grounds for an unqualified commitment to that form of argument.

1. There is a pr oblem of terminology here. In some circles, especially American ones, philosophers like Bas van Fraassen are called "anti-realists", not because they hold that there is no substantial possibility of erroroneous belief, but, on the contrary, because they fear that this possibility is actual (cf Van Fraassen, 1980). However, in my terminology, and in contemporary British usage, Van Fraassen is not an anti-

realist, but rather a pessimistic realist, that is, a sceptic. To get things straight , we need to distinguish three positions: anti-realism, in the British sense, which denies the conceptual possibility of error; optimistic realism, which admits the conceptual possibility of error, but disputes its actuality; and pessimi stic realism, or scepticism, which fears that error is not only possible but actual. In what follows, unqualified uses of the terms "anti-realism", "realism", and "scepticism" should be understood to stand for these three positions respectively.&nbs p; That is, I shall reserve the term "anti-realism" for philosophers who uphold beliefs on the grounds they can't be false; philosophers who reject beliefs because they fear they are false will be called "sceptics".

2. For more on the different varieties of anti-realism and their problems, see Papineau (1987, ch 1).

3. I would make the same point about contemporary "structuralist" or "modal-structuralist" views, which read mathematics as saying only that there exist, or poss ibly exist, some objects satisfying the axioms, and that therefore the theorems are true of those objects, or of those possible objects, whatever they might be. (Cf Lewis, 199x, pp xx-xx; Hellman 1989, 1990.) Whatever other virtues these views may have (but cf footnote 22 below), they are unquestionably revisionary proposals. The same goes for Michael Resnik's (1981, 1982) more platonist species of structuralism; this also faces extra problems, because of its reification of "struct ures" (cf Chihara, 1990, ch 7).

4 I have found this position defended more often in conversation than in writing.

5. Are these stories categorical? The plethora of Santa Clauses who appear around Christmas might make us wonder. B ut such stories can easily be made categorical, by including the explicit provisos that there is only one genuine Santa Claus, only one genuine Sherlock Holmes, and so on.

6. It is true that (6) is a truth of second-order logic, and some philosophersw ill feel that this commits (6) to sets, thus reintroducing the epistemological difficulties of abstract objects. But this is by no means uncontentious: in a series of recent papers George Boolos (1975, 1984, 1985) has defended higher-order qua ntification against the charge of implicit reference to sets. See also Wright (1983) pp 132-3.

7. For an interesting recent version of this generally Russellian approach, see Hodes (1984). Cf also Lear (1982). It is worth distinguish ing this "reductionist" approach from the "if-thenism" mentioned in section 2. Both approaches claim that there is nothing more to mathematical knowledge than logical knowledge. But "if-thenism" does so by arguing that mathematical knowledge i s always knowledge that if such-and-such axioms hold, then certain theorems follow. The reductionist approach, by contrast, needs to show that the axioms (and so the theorems) are themselves logical truths.

8. Hodes (1984) holds that the constru ction "the number of . . ." is systematically ambiguous, but gets disambiguated when the gap is filled in. This is reasonably plausible. What is not so plausible is the claim that "2" is ambiguous in "2 + 3 = 5".

9. Cf Hodes op cit p 144-6 ; Lear op cit p 188-91.

10. Note however that reductionism, as I have characterized it, is technically more demanding than Field's fictionalism: the reductionist needs to find, for every mathematical claim, some (family of) quantificational surr ogate(s) which yields the same inferential power; while Field is only committed to holding that all inferences underpinned by mathematical claims can be made by logic alone, and not to a case-by-case pairing of mathematical claims with quantificatio nal equivalents.

11. It is perhaps unfair to accuse Hodes (op cit) of wanting to have it both ways, since he explicitly embraces a kind of fictionalism (p 146), and to that extent explicitly abandons his reductionist ambitions. Lear (op cit), on the other hand, does seem to want to have it both ways. He shows how the possibility of holding reduced beliefs which do not involve abstract objects makes it both harmless but useful to work with mathematical propositions that do. But he the n claims that this legitimates belief in the mathematical propositions.

12. This provides a route to knowledge of arithmetic. But what about the rest of mathematics? Well, it is arguable that the rest of mathematics is reducible to set the ory. Moreover, there is a plausible set-theoretical analogue to N=, namely, the conceptual equivalence of:

(9)   (x)(Fx <-> Gx), and

(10)   The set of Fs = the set of Gs.

But of course, as it stands, this is too st rong: without some restrictions on what can be substituted for F and G, Russell's paradox will follow. Still, there remain weaker eqivalences which are both pre-theoretically plausible and powerful enough to yield set theory. I shall not pursue this line of thought, however, since the criticisms I am about to make of Wright's account of arithmetic will carry over to any analogous account of set theory.

13. For details of this line of argument, see Hale (1987, ch 2).

14. Of course , theoretical claims aren't conservative with respect to observational claims in the strong sense that extra theoretical premises never augment the consequences of any set of observational premises. But Craig's theorem does show that adding the clai ms of some theory to the observational consequences of that theory does not augment observational consequences. So to that extent theories are dispensable for drawing observational conclusions.

15. In what follows "necessary" should be understoo d in the narrow sense of logically necessary. I take it that other kinds of necessity (physical, conceptual, legal, and so on) can be defined in terms of logically necessity (as necesary consequences of physical laws, conceptual laws, legal laws, an d so on). In the case of physical necessity, there is of course the extra problem of distinguishing physical laws from accidentally true generalizations. In my (1986a) I advocated a fictionalist view of this distinction. I now think this was a mistake. My current view is that we can distinguish physical laws as consquences of those true generalizations which have sufficient robustness to qualify as causal. But that is another story.

16. As, for example, in Ayer (1936, ch 6).

17. Cf McFetridge (1990, essay VIII).

18. Simon Blackburn (1984, 1986) has coined the term "quasi-realism" to emphasize the structural affinities between normal discourse and moral and modal discourse. As it happens, Blackburn also upholds the "projectivist" view that moral and modal judgements express attitudes other than belief. The question I am currently asking (though Blackburn does not) is why his "quasi-realism" doesn't undermine his "projectivism".   Fo r this point, see Wright (1987). See also Hale (1986) for further discussion of Blackburn's position.

19. So I agree with Van Fraassen, and disagree with Horwich, that it is psychologically possible to "think with" a theory, and yet not believei t. But I certainly don't agree with Van Fraassen's further sceptical claim that this is the appropriate attitude to all scientific theories. Cf my remarks about the epistemology of theory-choice in 6.10 above.

20. Why exactly should belief be subject to epistemological requirements not imposed on other attitudes? A short answer is that belief is that attitude which is supposed to represent how things are, as opposed to how they are taken to be. This is why beliefs require evide nce, and cannot be made true just by being part of some established intellectual practice.

21. Note that the intended reference to mathematical objects is the reason why fictionalism is the appropriate non-doxastic attitude in mathematics. I hav e already explained why this non-doxastic attitude will involve a sceptical element: it requires us to reject the mathematical beliefs I conjecture most people to hold. But such a sceptical attitude needn't be fictionalist. To see this, imagin e a community who did have moral beliefs whose content derived from a non-object-introducing moral operator of the kind mentioned above. Then, I say, we ought to reject those beliefs in favour of a non-doxastic moral attitude. This attitude wo uld thus be sceptical about those people's moral beliefs. But it wouldn't be fictionalist, for lack of any moral objects to populate the fiction. In mathematics, by contrast, we have intended mathematical objects to provide our fiction.
&n bsp;
22. Even if such non-objectual views are wrong about the meaning of mathematical claims, might they not be defended as revisionary proposals? This is possible, but there is an obvious respect in which the revision proposed by fictionalism i s preferable. Fictionalism only requires a revision in attitude, where these alternatives require a revision in content. Apart from this, the explanations of the applicability of mathematics offered by if-thenism and reductionism are technical ly more demanding than that offered by fictionalism. The extra requirements facing reductionism are those noted in footnote 10 above. The problem facing if-thenism is that it can only account for the applicability of mathematical theories to s ets of natural objects which provide models of those theories; yet we often apply mathematical theories to setsc of natural objects which are too sparse to yield such models.

23. For other objections to the semantic characterization of logical c onsequences, which apply even if you don't mind abstract objects, see Field (19xx) and McGee (1991).

24. Cf Putnam, 1971, ch 2; Field, 1984, p 514.

25. This Godelian argument is due to Shapiro (1983). At first sight Shapiro's argument might seem inconsistent with the proof of the conservativeness of

mathematics given by Field in Science Without Numbers. However, what Field proves is that mathematics is conservative with respect to first-order nominalized theories. The kind of geo metrical theory needed to mimic Peano's postulates, by contrast, requires more than first-order quantification, which is why it escapes Field's proof. This leaves Field with a problem, however, since he himself requires just this kind of nonfirstord erizable geometrical theory to nominalize physics. (Cf Chihara 1990, ch 8 and Appendix.)

26. This answers a point raised by Hale (1987, p 120).

27. But see Field (19xx) for an investigation of a primitively modal interpretation of logical consequence. I should make it clear that Field does not himself advocate a non-doxastic approach. But it seems to me that many of his points could be adopted by someone who does.

28. As a fictionalist, I don't want to read such so undness proofs as showing that the conclusion is true in all models in which the premises are true. My idea is rather that they assume that (in actuality) either X or Y or . . ., and conclude that (in actuality) either the premises are false or the conclusion true.