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Fictionalism

I suggested earlier that the overview we seek will be assisted if we can manage to set our problem—and Wittgenstein’s relevant views—within the context of contemporary philosophy of mathematics. This is what this chapter begins. I keep technical details to a minimum; the overall view—Wittgenstein’s *übersichtliche Darstellung*—is what we seek, and such a presentation will not be helped by undue focus on minutiae on first exposure. *Some* technical details are inescapable, however, if we are to deal seriously with the problem we are tackling.

I start with a focus on the work of Hartry Field, for reasons I explained at the end of the last chapter. This focus will naturally broaden to include other present-day philosophers of mathematics. There is no attempt at an exhaustive survey; I concentrate on the *problem* of mathematical applicability. It is worth saying, too, that our engagement with contemporary philosophy of mathematics is far from wholly negative. We will find much to agree with in the work of Field and others, even as we see reasons to disagree with *some* fundamental aspects of contemporary philosophy of mathematics.

Let us begin by setting out a stall. The meaning and status of ‘Platonism’ (sometimes ‘realism’) in the philosophy of mathematics is

currently far from settled. Some take it as a metaphysical/ontological position about the status of mathematical objects such as numbers, others as a semantic claim about the truth of mathematical statements, ... and so on. Øystein Linnebo, author of the *Stanford Encyclopedia* article on *Platonism in Mathematics* offers *eight* separate definitions as well as one of his own; there are certainly others. For our purposes, we need not be too precise—nor need we engage in controversy about which definition, if any, Plato himself might have accepted. It will be clear overall, as we proceed, what general aspects of Platonism (or *platonism*, depending on which author I am considering) are germane to our discussion. This being so, Hartry Field's own definition will serve, so long as we are clear we need not be bound to its specifics:

A mathematical realist, or platonist, (as I will use these terms) is a person who (a) believes in the existence of mathematical entities (numbers, functions, sets and so forth), and (b) believes them to be mind-independent and language-independent. (Field 1989, p. 1.)

Nominalism and Indispensability

Field is no Platonist. In fact, he argues for a type of *nominalism*, which he takes as the general view that abstract entities do not exist. The particular brand of nominalism he espouses is *fictionalism* about mathematics, roughly the notion that abstract mathematical objects are ontologically similar to (*sc.* have the same status regarding their existence as), say, Jane Eyre or Gormenghast in that they exist only in stories. Field expresses the doctrine he wishes to defend thus:

A fictionalist about mathematics-taken-at-face-value is someone who does not literally believe mathematical sentences, at least when they are taken at face value. (Or ... a fictionalist is someone who does not regard such sentences, taken at face value, as literally true.) (Field 1989, p. 2)

To take a sentence at face value, for Field here, is just to assume it makes a claim independently of whatever story it happens to be in. ‘The second perfect number is less than thirty’ may in some sense be true *within mathematics*, but, Field claims, it is literally false because numbers—and *a fortiori* perfect numbers—do not literally exist. So, taken-at-face-value, the claim is simply false, according to fictionalism, just as the claim ‘Jane Eyre ended her life as Mrs. Rochester’ is simply false taken-at-face-value. Though true *within the novel* it is literally false because Miss Eyre and Mr. Rochester do (or did) not literally exist. According to the fictionalist, as Field himself puts the matter,

... the sense in which $2 + 2 = 4$ is true is pretty much the same as the sense in which ‘Oliver Twist lived in London’ is true: the latter is true only in the sense that it is true *according to a certain well-known story*, and the former is true only in that it is true *according to standard mathematics*. (*op. cit.*, p. 3)

Now, although I began by saying that Field argues for a type of nominalism, in fact he does not advance any positive arguments for nominalism in general, taking the line that all he needs to do is show what he sees as the major—possibly *unique*—anti-nominalist argument to be unsound. He takes the view that given well-known problems with Platonism, it is up to the Platonist to come up with the good arguments. And, he says,

The only non-question-begging arguments that I have ever heard for the view that mathematics is a body of truths all rest ultimately on the applicability of mathematics to the physical world; so if applicability to the physical world isn’t a good argument either, then there is no reason to regard any part of mathematics as true. (Field 1980, p. viii)

The argument to Platonism from the applicability of mathematics is part of what has become known as the (Quine/Putnam) indispensability thesis. Here is how Hilary Putnam summarises it:

... quantification over mathematical entities is indispensable for science, both formal and physical; therefore we should accept such quantification; but this commits us to accepting the existence of the mathematical entities in question. This type of argument stems, of course, from Quine, who has for years stressed both the indispensability of quantification over mathematical entities and the intellectual dishonesty of denying the existence of what one daily presupposes. (1979, p. 347)

At one time, indeed, Putnam held what we might call a 'non-propositional' attitude to mathematical sentences, contrary to the view he expresses here. He claimed

... the role of the formula 'two plus two equals four' is not an added *premiss* ... it is rather the *principle* by which the conclusion [concerning the number of physical objects] is derived ... (1979, p. 28)

—But he later changed his mind, on account of what he saw as the ineliminable way in which mathematics occurs within science, so that

... mathematics and physics are integrated in such a way that it is not possible to be a realist with respect to physical theory and a nominalist with respect to mathematical theory. (1979, p. 74)

This is precisely what Field denies. Of course he is not going to deny mathematics is *useful* for science. He does think it is possible to be a realist about science at the same time as a nominalist about mathematics, though. So he denies the indispensability thesis.

Field's adoption of fictionalism as the preferred brand of nominalism or anti-realism about mathematics is closely linked to his wish to 'take mathematics at face value', as we saw above. He wishes simply to ignore what he describes as 'the idealist form of anti-platonism' (e.g. Field 1989, p. 2), according to which

Mathematical entities exist but only as some sort of 'mental construction' or 'construction out of our linguistic practices'. (*op. cit.*, p. 1)

Field thinks it ‘not immediately obvious’ that such idealism differs in any important way from his own manner of denial of ‘face-value’ construals of mathematical propositions. Furthermore, he thinks it apparent that investigations of non-face-value construals (similar to the ‘non-propositional’ attitude we saw expressed by Putnam just above) are not worth the candle. He says,

What my anti-realism involves is a disbelief in mathematics. Or at least, it involves a disbelief in mathematics if mathematics is taken at face value; coupled with a lack of commitment to (and lack of much interest in) the program of finding a non-face-value interpretation of mathematics on which the mathematics becomes more believable. (1989, p. 227)

Field’s notion of ‘taking mathematics at face value’ needs to be looked at carefully. That comes later in this chapter. For the present let us continue with an examination of Field’s account of applicability.

Conservativeness

We should be clear about what the dialectical situation forces on Field. If he wants to adopt a fictionalist stance, he needs to deny that mathematics is indispensable for science. (If mathematics is a requirement for science and mathematics is false, after all, then so much the worse for science.) But given the usefulness of mathematics for science, he needs to do two things in opposition to the believer in indispensability. Field needs to show both how to disentangle mathematics from scientific theory, and how to explain the usefulness of (*false*, recall, in his terms, as well as dispensable) mathematics for science. That is, he needs to show how to *nominalise* specific scientific theories, and he needs to explain how, in his own phraseology, mathematics ‘*need not be true to be good*’. (See for instance Field 1989, p. 96: ‘My conclusion was that a mathematical theory needn’t be true to be good.’)

A large part of Field’s project—and much subsequent critique thereof—involves the first of these two imperatives, that of nominalising scientific theories. In his 1980, for instance, he formulates nominalistic

versions of Newtonian space-time and gravitational theories as examples. The overall success of this project is moot, indeed, but anyway it is not the aspect we need to focus on. For our purposes, what is important is the second of the two imperatives, that of explaining how mathematics can be ‘good without being true’.

The two parts of this second imperative are independent; the ‘good’ part of ‘good without being true’ is independent of the (absence of) ‘truth’ as Field recognises. He points out, for instance, that

... a number of reviewers of my book [1980] have rejected its anti-platonism while endorsing its account of the application of mathematics. (Field 1989, p. 191)

The ‘account of the application of mathematics’ comes soon, and as we will see it is indeed independent of platonism or its denial. For now we are focused on what Field sees as replacing *truth* so that mathematics can nevertheless be good.

The property that mathematics has which, Field claims, enables it to be good without being true—to be a useful fiction in applications—is ‘conservativeness’. Mathematics is *conservative* with respect to physics-without-mathematics in the sense that adding mathematics to a physical theory will not licence any consequences of the theory that are not already consequences of the theory alone. He makes this more formal and precise (see Field 1980, p. 11 *ff*), but there is no need to be particularly formal here. The general idea is what we need. Field claims that mathematics has the possibility of being good so long as it is conservative; conservativeness thus is a replacement for mathematical truth.

Why is mathematics conservative? According to Field the general answer hinges on the modal and/or epistemological status of mathematics. This needs something of a gloss; essentially the same argument applies with the status of mathematics as *necessary* as with its status as *a priori*, and Field simply runs the two together. The problem of applicability as I originally posed it was mainly in terms of an epistemological clash, and I will have more to say about mathematical necessity in Chap. 4. Here I simply state the argument in modal terms, leaving the epistemology to take care of itself.

Here is how Field's argument goes. Suppose that mathematics is not conservative with regard to some scientific theory *S*, say. Recall that this means that some result, *R*, say, is entailed by (*S*+mathematics), but that *R* is not entailed by *S* alone. Now, it follows from the fact that *R* is not entailed by *S* alone that it is possible that *S* be true but *R* false. But if it is possible for *R* to be false, then it follows from the fact that *R* is entailed by (*S*+mathematics), that if *S* is true, it is possible that mathematics (or some relevant part thereof) is false. But mathematics (as most of us would agree) is necessarily true (when true, or necessarily false when false, of course—I will leave such details tacit from now on). So mathematics must be conservative.

The 'necessarily's and 'possible's may be difficult to track; without trying to be formal with possible world semantics for modal logic, we can apply the useful heuristic device of '*possible-worldish*'. Here is the same argument again in this philosophical dialect: Take *S* and *R* as before and again assume that (*S*+mathematics) entails *R* but that *S* (alone) does not. Since *S* does not entail *R*, there is some possible world in which *S* is true but *R* false. But in that world, since (*S*+mathematics) does entail *R*, it must be the case that (*S*+mathematics) is not true. Since *S* is true in that world, mathematics must be the guilty party. That is, there is some possible world in which mathematics is not true. That contradicts the feeling that most people have that mathematics is true in all possible worlds. So it cannot be the case that (*S*+mathematics) entails *R* but that *S* (alone) does not. That is, mathematics is conservative with regard to the scientific theory *S*.

The argument as it stands is strictly unsound, of course. A 'feeling that most people have' is not good enough: we cannot be sure that some parts of mathematics might not turn out to be false. Assuming that mathematics is necessarily true, though, seems not to be too controversial a move. What may seem strange, though, is to have Field, as part of an argument for *fictionalism*—a doctrine denying the truth of mathematics—using an argument that relies on the necessary *truth* of mathematics. This is not wholly illegitimate: Field can be read as arguing *ad hominem* (in the Lockean sense '*to press a man with consequences drawn from his own principles*', see Locke 1689, IV, xvii 21; 1997, p. 606), against the Platonist, using Platonist premises (including at least one involving

necessity of some appropriate sort) to argue against Platonism. And, indeed, this is the way he wishes to be read:

‘Although ... espousing nominalism, I am going to be using platonistic methods of argument ...’ (Field 1980, p. 5)

If Field can, as he claims, prove from Platonist assumptions that the existence of abstracta is not a requirement for the use of mathematics in physical science, he will indeed have shown that what he claims as the *only* justification for believing in the existence of mathematical abstracta—their indispensability for science—is no justification at all. In short, Field argues, Platonism ‘entails its own unjustifiability’ (*op. cit.*, p. 6).

Furthermore, however, Field also sidesteps the implied objection at this point by denying that ‘the feeling that most people have’ of the necessity of mathematics actually requires truth. Field’s claim here is that *conservativeness* captures the important part of that ‘feeling’ without requiring the truth of mathematics:

Truth isn’t required for goodness (so necessary truth isn’t required either); what is required instead is ... conservativeness, which embodies some of the features of necessary truth without involving truth.’ (*op. cit.*, p. 5)

In fact, conservativeness, Field claims, is—more or less—what we will be left with if we subtract truth from necessary truth:

Conservativeness might loosely be thought of as ‘necessary truth without the truth’. (Field 1989, p. 241)

So it is *necessity* rather than *truth* that does the work in the argument for conservativeness.

Now, Field claims that actually—in *this* world—mathematics is false. And it turns out to be important for him to make the claim in this way, taking it that mathematics is contingently false, and not what looks at first glance the more natural claim, that mathematics is *necessarily* false. That Field’s fictionalism can only be a *contingent* nominalism has been cited by Bob Hale and Crispin Wright as a major flaw in Field’s position.

Briefly, their argument goes as follows. Given conservativeness, it is evident that mathematics is consistent. So there *could have been* mathematical entities, even if, as Field claims, there are none. And, claim Hale and Wright, such contingent non-existence requires an explanation Field cannot supply. (See Hale 1987; Hale and Wright 1992, 1994; Field 1989, 1993 for the beginnings of this controversy and some developments.)

In answering Hale and Wright, Field accepts the label of ‘contingent nominalist’—he does not think that mathematics is necessarily false, but only that although there are possible worlds containing mathematical entities, we have no reason to think the actual world is one such.

This brief look at Field’s argument for conservativeness gives an insight into just what conservativeness amounts to. And we should note in passing that a possible motivation for the sort of surprise we found expressed by our chorus of scientists might well be located in the apparent contrast between the *contingency* of physical science and the perceived *necessity* of mathematics, as much as in epistemological difficulties. Seen in this light, Field’s view of conservativeness, giving us a kind of ‘necessary truth without the truth’, might help explain away the contrast and mitigate the surprise. At the very least this aspect of the problem of applicability motivates further work in consideration of mathematical necessity. I will take this up later, in Chap. 4. For now, having looked at this aspect of Field’s project, I turn to his use of his ‘main theoretical device’; having given us a substitute for *true*, he needs to show us how mathematics can be *good*.

Representation Theorems

Suppose we agree with Field that mathematics is not indispensable. His way to the conclusion seems to make it too strong, since if we cannot get any new scientific results by adding mathematics to science, it starts to seem as though mathematics is simply otiose. Why not just do science nominalistically, without mathematics? Apparently we could, since mathematics is conservative. However, we know that mathematics is *not* otiose in this way. So our problem is still acute. We might agree with

Field that mathematics need not be true to be good, but we know that mathematics *is* good—it is *useful* in science. Accounting for this usefulness is where we began with the problem of mathematical applicability. This is where, in Field's account, we start to need representation theorems.

A representation theorem is an assertion that a certain physical system is structurally the same as some mathematical system, so that the physical system can be mirrored in—*represented by*, that is—the mathematical system. It will be worth bearing in mind in a general way as we work through some specifics, just *how* this might look like it can help us to understand mathematical applicability.

Consider some physical system—a collection of physical objects, say, to take a very simple case—to which we want to apply mathematics. (We might want to compare their sizes in some structured way, for example.) The idea is that if we can find a collection of mathematical objects that are related amongst themselves in the same way as are the physical objects in question, we may be able to work within the mathematical system rather than directly with the physical objects themselves. (It may well just be *easier* to work with mathematics.) What we require is that the results of working with the mathematics can be *applied* back in the physical world (applied to the physical objects, that is). The possibility of such application will be guaranteed, so the claim goes, so long as these mathematical objects are indeed *structured*—related among themselves, as I said—in just the same way as the physical objects. This guarantee of structural identity is what a representation theorem supposedly supplies.

Here is Field's own characterisation of how this goes:

Suppose that using some mathematical theory *S* ... we can prove the existence of some mathematical structure *B* with certain specified properties. If we can then ... prove the existence of one or more homomorphisms (structure-preserving mappings) from concrete objects ... into that mathematical structure *B*, then such a homomorphism will serve as a "bridge" by which we can find abstract counterparts of concrete statements. Consequently, premises about the concreta can be 'translated into' abstract counterparts; then, by reasoning within *S*, we can prove abstract counterparts of further concrete statements, and then use the homomorphism to

descend to the concrete statements of which they are abstract counterparts.
(Field 1980, pp. 24–5)

‘Ascend’ from the concrete to the abstract via a homomorphism—make some deductions concerning the abstracta—then ‘descend’ back to apply those same results about the concreta. That is the general way in which representation theorems are intended to help us understand mathematical applicability.

There are a couple of questions that might be raised at this point. First, we might ask why the structure-preserving map is taken to be a homomorphism rather than an *isomorphism*. (See *Appendix 1* for more about this mathematical terminology.) The answer is simple enough. Suppose, for example, that we are looking at the way in which the *lengths* of physical objects can be dealt with mathematically. We certainly do not want to deny the possibility of several objects having the same length—that is, of several physical objects mapping to the same (abstract) number that *represents* their length. So we allow the structure-preserving map to be many-one rather than specifying that it be one-one. The same goes for other physical attributes as well as length, too, of course, and in general it makes sense to keep with the more inclusive notion of homomorphism.

The second question that might arise is linked to this. The representation theorem story of applicability asks us to consider moving in two different ways: *from* the concrete (via our homomorphism) to the abstract, then (once the work is done, so to speak) back *to* the concrete from the mathematics. Now, if the link from the concrete to the abstract is one-one, we will of course end up back where we started if we undertake this two-way journey. Once we allow the connection to be many-one, though, we might ask whether we will be guaranteed to end up back in the same place with our application of mathematics in the physical system.

Technically, we can express this concern in terms of the fact that isomorphisms have (specific) inverses, whereas homomorphisms may be ambiguous as regards their inverse-images. Should this be a worry? Not for long. Think of the example of application we considered above, that of representing *length*. Several objects may have the same length. And suppose that we derive some mathematical result about numbers

representing lengths—concerning their additivity, say—which we wish to apply to concrete objects themselves. Might we be misled because when we want to apply such a result there are several objects that count as the inverse-image of any particular length—several objects with the same length, that is? No: all we need do is demand that any such results apply to *all* objects with the same length. In general, it seems, any result gained mathematically, if we are to apply it concretely, needs to be capable of applying to *any* of the concrete objects represented by the mathematical object they each map to under the representing homomorphism. That, indeed, is a *requirement* of any mode of application we are trying to characterise in this way, rather than a *difficulty* with such a characterisation.

So, although we may well often talk of representing *isomorphisms*, indeed, there is nothing to be lost, and sometimes something to be gained, by sticking with the more general *homomorphism* when characterising the structure-preserving map at the core of representation theorems.

Now for some of the details. Field's characterisation of a representation theorem (1980, p. 58) generalises the notion from Krantz, Luce, Suppes and Tversky:

A representation theorem asserts that if a given relational structure satisfies certain axioms, then a homomorphism into a certain numerical relational structure can be constructed. (Krantz *et al.* 1971, p. 9)

Field himself is explicit as to the debt he owes in his work to the representation theorems he takes—more or less whole—from measurement theory and the survey thereof supplied by Krantz *et al.* It is going to be worth our while here to consider at least one such theorem: Krantz *et al.*'s *Theorem 1*. What is important in what follows is the use made of the notion of homomorphism, and in particular the way in which the statement and the proof of the theorem seem to bring to the surface just what it is that is required in order that a part of the physical world be capable of being represented mathematically—and in order that the mathematics in question then be applicable to the physical world in a specific manner.

Suppose we want to apply the kind of ordering relation epitomised by the way real numbers can be ordered by the relation $\geq$ (“greater than or equal to ...”). We might have, that is, some set of objects whose magnitudes we wish to compare in some more-or-less theoretical kind of way. What do we require of such a set as regards the structural aspects of the way the magnitudes can be compared in order to be able to apply real-number ordering? What must be true, that is, of the physical structure, the set of objects, in order that we can apply this particular bit of mathematics to it? Let us go a little formal:

Suppose A is a set and $\gtrsim$ is a binary relation defined on A . A definition:

The relational structure $\langle A, \gtrsim \rangle$ is said to be a **weak order** if and only if for all a, b and c from the set A , the following conditions hold:

1. *Connectedness*: either $a \gtrsim b$ or $b \gtrsim a$.
2. *Transitivity*: if $a \gtrsim b$ and $b \gtrsim c$, then $a \gtrsim c$.

Armed with this definition, we are ready to state the representation theorem (*Theorem 1* from Krantz *et al.* 1971):

Suppose that A is a finite non-empty set. If $\langle A, \gtrsim \rangle$ is a weak order, then there exists a function ϕ mapping A into the real numbers such that for all a and b in the set A , $a \gtrsim b$ if and only if $\phi(a) \geq \phi(b)$. (That is to say, of course, that ϕ is a representing homomorphism; for a proof of this theorem, see *Appendix 2*.)

What are we to take forward from this example? We might consider, first, how the importance of the structure-preserving homomorphism property seems here so well exemplified. It seems clear how the property of the representing function ϕ —that $a \gtrsim b$ if and only if $\phi(a) \geq \phi(b)$, so that ϕ preserves the working of the relevant relation (here of a basic type of *order*) in the move from the physical structure to the representing mathematical structure—is precisely what we need in order that the mathematics correctly represent the physical structure.

Second, it is also worth a look at the specifics of what is important for the theorem to go through—the axioms or *given* of the theorem that enable the proof to work. In the example we needed elements of the physical structure to be capable of being related by means of a ‘weak

order’—a relation that is both connected and transitive. We would be wasting our time trying to compare and measure an attribute that was not transitive, for instance, using the order properties of the real numbers. Well, but we always knew that, I suppose it might be said here: there is no point trying to achieve a consistent ordering or measure of, say, desirability of prospective partners if it could be that A were more desirable than B, B more desirable than C and C more desirable than A. This may seem a trivial point. However, exhibiting the relevant representation theorem does manifest just what seems to be required in order that relevant parts of mathematics can be applied in situations where it *is* possible so to apply them.

Field’s ‘main theoretical device’, then, we see, is indeed in the business of answering Steiner’s question ‘Given the nature of mathematics, why should such a homomorphism exist?’

An Initial Worry

However, now we have an idea of Field’s account of mathematical applicability, I want to enter a preliminary caveat. It certainly seems that there is *something* to be got from considering how a representation theorem might be thought to explain how mathematics is applicable to the physical world. However, there may be some unease at this thought that a representation theorem gives an *explanation* of how mathematics applies, an unease that seems all the heavier in the context of Field’s overall project. For consider: the map at the centre of any representation theorem supposedly connects physical objects to mathematical objects—two admittedly different *sorts* of things under *any* philosophical view of mathematics and particularly under Field’s. The sort of things mathematical objects are, in Field’s view, is about as different as it is possible to be from the sort of things physical objects are. Mathematical objects, according to Field, are non-existent, whereas physical objects paradigmatically exist, whatever else we may choose to say about them. Can we have a connection between non-existent objects and existing things, one that moreover bears the weight of explanation we are trying to give it here? It seems problematic, to say the least.

Of course we do not consider the ontological status of mathematical objects when we are mapping *within* mathematics: the homomorphism from a group to its factor group by a normal subgroup, for instance—perhaps a paradigm case for the notion of homomorphism within mathematics (see *Appendix 1*)—does not have its status at all put in question by considerations of the ontology of *groups* or their elements. We might well consider the exhibiting of such a homomorphism to be indeed explanatory of the relation between object and image groups or of the structure of the group thereby factored; we may be said not really to understand this latter unless we can see how and why this homomorphism exists. That seems a different case, though, from the one under consideration where we are supposedly mapping between such different sorts of objects as (non-existent) abstract mathematical objects and real physical things.

We might put the case as follows. It is clear what we mean when we exhibit a homomorphism *within* mathematics: we have already extant criteria for whether such a homomorphism exists as part of mathematics. In the light of this, the unease I have gestured at when we start to think of connecting different sorts of objects in maps from physics to mathematics might be explained in terms of questioning whether we really know what it means to say we have a map from physical to mathematical objects. It does seem that a representation theorem makes sense of such a map. On reflection, though, it is clear that it only does so without further ado so long as it is considered as itself a part of mathematics. We may have missed the point because we were thinking so much about linking mathematics to physics, but Krantz's *Theorem 1* above is itself part of mathematics. It is stated, precisely, as a mathematical theorem about mathematical objects, and its proof consists, precisely, of mathematical conclusions drawn by deductions from mathematical premises according to the usual tenets of mathematical practice.

In the light of this, it is worth beginning to question the way in which a representation theorem is supposed to show us how to bridge a supposed gap between the separate realms of physics and mathematics. According to Field, as I have emphasised, these 'realms' are very different: one of them contains ordinary, common or garden concrete objects, the

other nothing at all, or at least nothing that exists. It may be objected that including such things as bosons and quarks in the category of ‘common or garden’ objects is unwise. There are surely questions to ask about the ontology of such theoretical entities. Such questions, though, are very different from apparently similar ones about abstracta. The main difference regards the playing-out of Field’s conservativeness: the existence of quarks is *not* conservative over physics in the way the existence of numbers is. So we can allow the difference between concrete and abstract to carry the weight here.

There is, then, a sense in which, for a fictionalist like Field, the mathematical realm does not exist. At the same time, though, there is another sense in which, even for a fictionalist like Field, it appears the mathematical realm *must* exist in order for it to be mapped onto by the homomorphism at the core of a representation theorem. This latter sense, now, it is also becoming clear, is the sense which generates the search for ‘something like a homomorphism’ that Steiner found at the heart of the philosophical problem of mathematical applicability in the work of philosophers through the ages. I want now to begin to put this sense in question.

Field and Wittgenstein; Mathematics as Rules

The sense in which the mathematical realm does (or does not) exist so as to figure as the co-domain of a representing homomorphism will be illuminated by comparing Field’s way of turning away from mathematical Platonism with Wittgenstein’s way of so turning. There will be contrasts in this comparison; however, I want to focus first on a similarity. Both Wittgenstein and Field see mathematical propositions (or ‘so-called propositions’, we might say to avoid any accusations of question-begging) as particularly related in some important way to rules of inference, a view that might be seen as forced once we make the turn away from Platonism. Forced or not, each of these two philosophers makes the implied move. Field writes of the importance of mathematics being

... useful in *facilitating inferences* (between nominalistic premises and nominalistic conclusions) ... (1989, p. 59)

—Just as Wittgenstein, in his *Tractatus Logico-Philosophicus* says

In life it is never a mathematical proposition which we need, but we use mathematical propositions *only* in order to infer from propositions which do not belong to mathematics to others that equally do not belong to mathematics. (Wittgenstein 1933, §6.211)

Whilst there are differences in emphasis, this *Tractatus* view of mathematics survives into Wittgenstein's later writings. In *Remarks on the Foundations of Mathematics*, for instance, we find him often writing of mathematical propositions explicitly as 'rules'. Field makes no such explicit move, although as we will soon see, he does take mathematicians' knowledge of relations of consequence—of 'what follows'—as *the* uniquely important feature of mathematical epistemology. See, e.g., Field 1989, p. 83:

'... the only knowledge that differentiates a person who knows lots of mathematics ... is (i) knowledge that certain mathematical claims follow from certain other mathematical claims ...'

—I deal with this in more detail later.

Having made the point that Field and Wittgenstein are on common ground here, we should contrast the way in which each of Field and Wittgenstein deals with this notion of mathematics as inference-facilitating. They both have the notion as an important part of the use of mathematics in practice. But Field has also, what Wittgenstein does not, the notion of a *link* between two realms—a representing *homomorphism* between inferences in mathematics and inferences in (nominalised) science. Wittgenstein's move towards something like a closer identification of mathematics with the rules of inference that Field suggests are 'facilitated' by mathematics is blocked by Field's insistence on 'taking mathematics at face value'. Or, rather, as I shall argue, Wittgenstein's move here is blocked by Field taking 'taking mathematics

at face value' a certain way. It is going to be important to get this clear; I will take the trouble to unpack carefully.

Benacerraf's Dilemma

As well as situating Wittgenstein's thought in regard to what I am claiming as relevantly similar aspects of Field's account, we can situate the views of both these philosophers in the light of what has become a canonical framework for much contemporary philosophy of mathematics, namely the dilemma set out in Paul Benacerraf's 1973 article 'Mathematical Truth'. In the current *Oxford Readings* on the philosophy of mathematics, which contains a reprint of this seminal article, the editor W.D. Hart suggests that

Benacerraf's dilemma ... gives us a perspective from which to organise many, especially contemporary, philosophical discussions of mathematics. (Hart 1996, p. 5)

Benacerraf claims there are two concerns or requirements that must be satisfied if we are to have what he describes as 'a coherent over-all philosophic account of truth and knowledge' in our philosophy of mathematics. (p. 18) These requirements are as follows:

(1) the concern for having a homogeneous semantical theory in which semantics for the propositions of mathematics parallel the semantics for the rest of the language, and (2) the concern that the account of mathematical truth mesh with a reasonable epistemology. (p. 14)

Unfortunately, Benacerraf claims, these two concerns are such that they cannot be simultaneously satisfied by any extant philosophical accounts of relevant aspects of mathematics. This is because the only *semantic* game in town, so to speak, is Tarski's, according to which, as Hart reminds us, '... truth requires reference ... to objects' (1996, p. 2); and *knowledge* requires some causal connection between knower and thing known: '...for *X* to know that *S* is true requires some causal relation to

obtain between X and the referents of the names, predicates and quantifiers of S .' (Benacerraf, *op. cit.*, p. 22) But mathematical objects, supposing them to exist at all, are abstract and hence *ipso facto* non-causal. So, the argument goes, we need there to be mathematical objects for there to be mathematical truth, but we cannot get to know anything about such objects because they do not participate in causal relations.

Benacerraf's reliance on a causal epistemology might give us pause, but in fact most contemporary philosophers of mathematics see the crux of the dilemma independently of specific causal theories of knowledge. Knowledge of non-spatio-temporal entities seems itself *prima facie* difficult to account for, as we saw in the previous chapter, not least in considering Plato's *Parmenides*. There I mentioned Mark Balaguer and his characterisation of the importance of Benacerraf's dilemma independently of causal epistemology nevertheless as 'the epistemological argument against Platonism' (Balaguer 2011). As I have emphasised, the problem has remained acute over succeeding millennia. Just one more example (of possibly many) makes the point again; Stewart Shapiro:

Here is our dilemma: the desired continuity between mathematical language and everyday and scientific language suggests realism, and this leaves us with seemingly intractable epistemic problems. (Shapiro 1997, p. 4)

So, the dilemma arises from the difficulty—impossibility, says Benacerraf—of squaring the demand for uniformity of semantics with an appropriate epistemology of mathematics. I will be looking at the horn of the dilemma concerning mathematical knowledge, particularly in the light of Hartry Field's stance, below. In the end, though, my major concern in this and succeeding sections is to blunt the horn involving the concern for a homogeneous semantic theory.

Why should we want uniformity of semantic theory? One reason is that we might want to mix propositions from different regions of discourse. Hart mentions

... one banal fact which Tarski used ... the conjunction ... A and B is true if and only if A is true and B is true ... (Hart 1996, pp. 2–3)

Taking different varieties of truth for A and B seems to make the conjunction hopelessly equivocal, as Hart points out. (Suppose A to be a historical, and B a mathematical, proposition, for instance.) We want a uniform semantics because it is at best *inconvenient* if we do not have one, as well as to satisfy the desire for simplification to be had from a general theory. Along the same lines, think how complicated our lives as semantic theorists would be if we had to gloss some semantic value for a proposition *p* differently for *p* asserted *simpliciter* compared with an occurrence of *p* as part of a conditional ‘if *p* then *q*’. We want—it seems we *need*—*p* to be the same as simple assertion as it is in a hypothesis or conditional, where ‘the same’ refers to whatever semantic value is in question: *meaning*, *truth conditions* ... or whatever. The requirement for semantic homogeneity stands or falls with this aspect of the compositional requirement on philosophical semantics.

Moore’s Paradox

I am going to challenge this everyday notion of semantic homogeneity in the hope that we can get a clearer view of what is going on that might make us less likely to get hung up on the likes of Benacerraf’s dilemma.

Before we start looking at the mathematical case, however, I want to prepare such a case by considering an issue in philosophy of mind, broadly construed, in order to sow some seeds of doubt about the general applicability of the requirement for semantic homogeneity.

As I said just above, it seems we need that ‘*p*’ as a simple assertion *means* the same (*has the same truth conditions*, whatever ...) as ‘*p*’ when it occurs as the antecedent in a conditional ‘if *p* then *q*’. But look: suppose we take ‘*p*’ as ‘*I think it’s raining and it isn’t raining*’ and ‘*q*’, say, as ‘*I’ll carry an umbrella unnecessarily*’. ‘If *I think it’s raining and it isn’t raining*, then *I’ll carry an umbrella unnecessarily*.’ ... That is unexceptional. However, *p* as a simple assertion, ‘*I think it’s raining and it isn’t raining*’ is—at the very least—an odd thing to say. There is lots of literature about such cases—aspects of ‘Moore’s paradox’, so named by Wittgenstein. For general accounts, see for example Roy Sorenson, *Blindspots*, 1988; or Mitchell Green and John N. Williams (eds.), *Moore’s paradox: new essays*

on belief, rationality, and the first person, 2007. I mention some other relevant articles and studies below.

Whether this ‘odd thing to say’—‘*I think it’s raining and it isn’t raining*’—is or is not a formal contradiction is not important. I want to focus just on the feature pointed out by Wittgenstein, that

... it *looks* as though the assertion “I believe” [*I believe it’s raining...*] were not the assertion of what is supposed in the hypothesis “I believe”! [*If I believe it’s raining...*] (*Philosophical Investigations Part II xi*)

The assertion ‘*I believe it’s raining and it isn’t raining*’ seems not to make clear sense *as* an assertion or utterance. It is not something anyone could sensibly *say*. But embed it as the antecedent of a hypothetical or conditional, ‘*If I believe it’s raining and it isn’t raining, then ...*’ (*I’ll end up carrying an umbrella I don’t need*) ... *That* is just fine.

We are forced to conclude that here we have a case where, whatever else is going on, we have a proposition *p* that actually disallows homogeneity of semantic treatment over (i) simple assertion and (ii) as embedded in a molecular proposition, contrary to the requirement we thought necessary. And further, we should remark that this is not a problem that necessarily needs a theoretical fix; it is simply a feature of our language that this is the case. There may be important morals for the way we view ascription of so-called intentional attitudes, particularly in the first-person singular present tense indicative, but this need not detain us. We should note, however, that a solution to the apparent paradox might well lie not in any change in the way we talk about belief or other aspects of our mental life but rather in seeing clearly that what we thought to be a theoretical requirement is in fact nothing of the sort. The fault, we might say, lies in the theory rather than the language, in short.

Taking Mathematics at Face Value

I am not going to get into a detailed treatment of Moore’s paradox. (Wittgenstein offers such a—convincing—treatment, of which Jane Heal gives a good account; see her 1994.) But I hope at least to have sowed

some seeds of doubt about the homogeneity requirement. With this in mind, let us turn back to mathematics and Hartry Field.

The requirement for semantic homogeneity in Field's writings is apparent in his talk of taking mathematics 'at face value'. We have already seen the importance of 'taking mathematics at face value' for Field. It is worth trying to get his meaning as precisely as possible. Field obliges us: in his critical study of Crispin Wright's *Frege's Conception of Numbers as Objects* (see Field 1989, p. 147ff), he takes up an example Frege himself offers. (See Frege 1953, §64ff). Field writes of

... what might be called 'the face value thesis', according to which both 'line c' and 'the direction of line c' function semantically as singular terms. (*op. cit.*, p. 151)

Field recommends this thesis to us:

In my view, the face value thesis should be accepted. (*ibid.*)

That is, Field wants us to accept the homogeneity of such things as the semantic function of singular terms in discourse concerning both the physical/concrete—'line c' refers to a *concrete* object (a physical line), and the abstract—'the direction of line c' refers to an *abstract* object (a direction). This is what I want to challenge.

Frege, Wright and Field

This takes us back to Frege in *Foundations of Arithmetic* (Frege 1953) and Crispin Wright's exegesis thereof. Field's study of Wright's book does not engage with Frege directly. Nor does it cover all of Wright's exegesis and comments in detail. What Field particularly challenges is Wright's development of Fregean Platonism, and it is this that should interest us. We need to get clear about just what is involved in this 'taking at face value', and for this it will be useful to look at what is going on in this latter challenge. There is no need here for anything like a thorough assessment of the controversy between Wright's (*neo*-Fregean) Platonism

and Field's fictionalist anti-Platonism. My concern is not with differences between Field and Wright, but rather with what, overall, they share, as exemplified by Field's talk of 'taking at face value'. This will be clearer in the light of a brief description of the dialectic in which they are engaged.

First, let us go back to Crispin Wright for a run-through of some important aspects of the argument for Platonism he finds in Frege. Wright is at pains to point out that Fregean Platonism is, as he remarks,

... associated with no Gödelian epistemology. (Wright 1983, p. 51)

This is anachronistic in a sense, since Gödel's epistemological ruminations post-dated Frege's *Foundations*. However, it is a natural point to make: the reference is to a view that has become particularly associated with Gödel, but that must strike any Platonist—any philosopher of mathematics indeed—as a possible answer to an apparently pressing problem, one we have visited beginning with Plato's *Parmenides* and more recently seen sharpened as one of the horns of Benacerraf's dilemma. The problem is that of accounting for knowledge of abstract entities, supposing they exist, given such entities' apparent lack of interaction, causal or otherwise, with us as putative knowers. Here is an expression of Gödel's epistemology of mathematics:

But, despite their remoteness from sense experience, we do have something like a perception also of the objects of set theory, as is seen from the fact that the axioms force themselves on us as being true. I don't see any reason why we should have less confidence in this kind of perception, i.e., in mathematical intuition, than in sense perception. (Gödel 1983, p. 271)

This kind of Gödelian 'perception', we might think, is as much a desperate move as Plato's theory of recollection or Descartes' 'God is no deceiver' in attempting to account for mathematical knowledge. Be that as it may, we have seen Wright (above)—in exegesis of Frege, perhaps we should remind ourselves—setting out to eschew any such Gödelian epistemology.

Field explains how this relates to Wright's metaphysics. Difficulties with the problem of accounting for knowledge of abstracta may well be

taken as good reasons for denying Platonism, so that if a convincing epistemology can be supplied from elsewhere, Platonist metaphysics becomes all the more plausible. (It is easy to see this in the light of Benacerraf's dilemma.) However, Gödelian 'perception' is implausible: we can do better in accounting for our knowledge of abstracta, Field explains on Wright's behalf:

A central tenet of Wright's approach to ontology is that one can and should be a platonist without endorsing Gödelian views about the 'perception' of abstract objects. If we understand the Context Principle correctly, Wright thinks, we see that there is no need for such a faculty of 'perception'. For we can have knowledge of abstract objects if we can have knowledge of states of affairs in which those abstract objects figure. (Field 1989, p. 162)

The 'Context Principle' is one of three 'fundamental principles' Frege sets out in the introduction to *The Foundations of Arithmetic*:

never to ask for the meaning of a word in isolation, but only in the context of a proposition. (1953, p. *x*)

What is important for us is Wright's gloss on it and Field's reaction to that. Field describes Wright's exegesis as 'quite plausible' (Field 1989, p. 152). It consists of two components. The first is 'pretty uncontroversial' (Field, *ibid*) and 'fundamental to modern philosophical semantics' (Wright 1983, p. 50): it is a thesis of compositionality, according to which

... a satisfactory account of the meaning or reference of a subsentential expression must make clear its contribution to the meaning or truth conditions of sentences that contain it. (Field 1989, p. 152)

We have just seen the beginnings of an attack on this, with particular reference to Moore's paradox. However, the second component is more germane just now. Wright describes it as follows:

... the thesis of the priority of syntactic over ontological categories.
(*op. cit.*, p. 51)

—More fully:

... the question whether a particular expression is a candidate to refer to an object is entirely a matter of the sort of syntactic role which it plays in whole sentences. If it plays that sort of role, then the truth of appropriate sentences in which it so features will be sufficient to confer on it an objectual reference; and questions concerning the character of its reference should then be addressed by philosophical reflection on the truth-conditions of sentences of the appropriate kind. (*ibid.*)

Of course it does not follow from 'The maiden captured the unicorn' that there are unicorns. However, if we accept that syntactically 'The unicorn' plays the role of a singular term, *and* that the sentence is true, Wright will claim that it does so follow. There may still be caveats, of course. For instance, the truth of the sentence has to be of the right sort, or to put it another way, the truth conditions for such sentences must be 'of the appropriate kind'. It will not do if the sentence is only true in a fairy-tale, for instance. The appropriateness of allowing different kinds of truth is largely what is at issue, however, we should remind ourselves.

Field wants to split this 'priority thesis' apart. He wants to take Wright's thesis (the 'Strong Priority Thesis') as the conjunction of two claims, only the first of which he accepts. The 'Weak Priority Thesis', which Field agrees with, he expresses as follows:

...any expression which by syntactic criteria counts as a singular term also functions semantically as a singular term. (Field 1989, p. 153)

The second part concerns the criteria for distinguishing the truth of sentences under question. It is this:

(S) What is true according to ordinary criteria really is true, and any doubts that this is so are vacuous. (Field 1989, p. 155)

Briefly, Field's explanation of this is as follows. Suppose, for example, there are at least three books in the room. According to Wright, this will count as an 'ordinary criterion' for the truth of the statement, 'The number of books in the room is greater than or equal to three.' Since 'the number of books' counts as a singular term by syntactic criteria, according to the Weak Priority Thesis it also functions semantically as a singular term: that is, it refers to a number. But, according to Wright's argument as explained by Field, a true statement referring to a number entails the existence of that number.

Arguing against Wright, Field denies that 'The number of books in the room is greater than or equal to three' is made true by there being at least three books in the room, whether this counts as an ordinary criterion for truth or not, since a sentence expressing that there are three books in the room can be glossed nominalistically using a device familiar to logicians—numerical quantification—and so avoid reference to a number. (Numerical quantifiers such as 'there are at least three ...' (sometimes called 'cardinality quantifiers') are expressible in logic without any use of number terms.) According to Field, then, this is an example of how we can accept the Weak Priority Thesis, deny the principle (S) and in doing so keep the semantic part of the context principle without accepting any ontological entailment.

Schematically, then, Wright's priority thesis, Field claims, gets us from syntax to ontology:

Syntax----- > Ontology

For Field, this move is best seen when analysed:

Syntax—(1) → Semantics—(2) → Ontology

—and the link from syntax to semantics—(1)—is acceptable to Field, whereas the further link to the claim of existence—(2)—is not, because it relies on this 'truth by ordinary criteria' principle which Field has argued to fail. So overall the link breaks, according to Field, because although the move to semantics means that mathematical propositions do involve

reference to abstract entities, we have no reason—he does not think Wright’s Frege gives us any reason, nor can he find reasons elsewhere—to think there are any entities corresponding to the singular terms. There just are not, as a matter of fact, Field claims, any abstract objects for the terms in such propositions—singular (referring) terms in good standing though they be—to refer to. So syntax gets us semantics, according to both Wright and Field. Where they differ is in the further step from semantics to existence.

We saw above the first component of Wright’s exegesis of Frege’s Context Principle, what I called a thesis of compositionality, and to which Field in his expression added the gloss of ‘truth conditions’ to Wright’s ‘meanings’. We have also seen Field writing of the link between ‘knowledge of abstract objects’ and ‘knowledge of states of affairs’. Here is that quotation again:

... If we understand the Context Principle correctly, Wright thinks, we see that there is no need for such a faculty of ‘perception’. For we can have knowledge of abstract objects if we can have knowledge of states of affairs in which those abstract objects figure. (Field 1989, p. 162)

For Field there is no knowledge of states of affairs in which abstract objects figure, just because there are no abstract objects. But, we have seen, for him the *semantics* remains, so that although terms which purport to refer to abstract objects fail to do so, nevertheless their meaning involves this reference to non-existent abstracta. Wright thinks, what Field denies, that the states of affairs *do* pertain. And it is not possible, Wright claims,

... to determine that a class of expressions have no genuine reference when, by the best syntactic criteria, these expressions function as singular terms in a range of statements ... which we have every reason to suppose to be true; and thus that the states of affairs that make these statements true have no objects ‘in’ them of the appropriate sort, bear no structural affinity, as it were, to the overt syntactic structure of the relevant statements. (1983, p. 25)

This is important. To labour the point slightly, here is a little more of the same:

If, therefore, certain expressions in a branch of our language function syntactically as singular terms, and descriptive and identity contexts containing them are true by ordinary criteria, there is no room for any ulterior failure of 'fit' between these contexts and the structure of the states of affairs which make them true. (*op. cit.*, p. 52)

I want to emphasise that Field's disagreement with Wright hinges not on any denial of the appropriateness of such talk of 'fit' between linguistic contexts and states of affairs, but only on the further move of denying the existence of the states of affairs involving abstracta which 'fit' the linguistic contexts. We have seen Field to hold on to the semantic theory while denying it any ontological entailments even in the mathematical case. It is this that I want to claim as his blind spot—the commitment to a particular semantic theory that makes him unable to take on board the full consequences of his movement away from abstract entities.

It is clear how the notion of semantic homogeneity plays out in Field—and it is also clear from these latter remarks exactly what kind of theoretical account of semantics underlies the homogeneity claim; it is an account involving an imprecisely defined 'fit' (note the scare quotes—but note also that they are there in the original quotation!) between 'states of affairs' in which objects 'figure' and the contexts of use of linguistic expressions describing or talking about such states of affairs and the objects (*abstracta* in the mathematical case, *concreta* in talk of *physical* objects) which figure in them.

This commitment of Field's to uniformity is what I want to challenge: it is a uniformity of semantic theory that emphasises the *similarity* of how different linguistic terms function. On the surface, it looks as though 'the number of books' in a sentence such as 'The number of books is greater than three' functions in much the same way as 'the sofa' in a sentence like 'The sofa is larger than the table.' It seems a small step from there to the thought that, just as 'the sofa' signifies a physical object or refers to an item of furniture, so 'the number of books' signifies a non-physical object or refers to a number. And at this point, it seems, philosophy has

questions to answer. What is the ontological status of non-physical objects? How can we know about them if they are non-causal? This is familiar: we are back with Parmenides' 'greatest of all difficulties', Descartes' 'objection of objections' and Benacerraf's dilemma. It is time to begin to consider the other horn of this dilemma.

Knowing Non-existence

I am going to continue with Field; the difficulties he encounters will be suggestive. Recall that Field's turn away from Platonism issues in fictionalism about mathematics; he wants to convince us that mathematics is uniformly *false*. If we consider mathematics to be false, however, then it seems there can be no mathematical knowledge. We cannot be said to know anything that is false, after all. We cannot just leave matters there, though. Clearly there is *something* that mathematicians can claim to know ... and something that some mathematicians know *more of* than others. (There are many mathematicians who know more than me, for instance.) What can we make of such knowledge, though, once we allow that mathematics is uniformly false, as does Hartry Field?

Field sees the problem, and offers us an epistemology for mathematics that sorts things out on his own terms. '[T]he idea that mathematical knowledge is just logical knowledge,' Field says, 'is largely correct.' (1989, p. 81) [*Logic* for Field is conceptual necessity. (1989, p. 80)] Despite this claim, though, Field is not a logicist in the way Frege was or tried to be. Field denies that mathematics is analytic—'that is, true by logic and definitions alone'. (1989, p. 79) Logic, says Field, should be taken as eschewing existential commitments, a point he takes from Kant. According to Field, mathematics ('taken at face value') does, whereas logic does not, involve existential commitment. Or, at least, we should so construe logic: there are, he claims, distinct advantages to be gained by taking such a Kantian (Field claims 'normal') view of logic:

... there is good reason not to depart from the normal sense of 'logic' by counting existence assertions as part of logic: doing so would tend to mask the fact that there is a substantive epistemological question as to how it is

possible to have knowledge of the entities in question (God, numbers, etc.). (Field 1989 p. 80)

Field thinks that there is a moral here that Frege himself (eventually) saw. If we cannot get existence from logic, and mathematics involves claims of existence, we cannot reduce mathematics to logic. Field quotes Frege appositely in this regard:

...it seems that [logic] alone cannot yield us any objects ... [So] probably on its own the logical source of knowledge cannot yield numbers. (Frege, *A new attempt at a foundation of arithmetic*, quoted in Field 1989, p. 81.)

Of course the idea that logic has no existential commitments—that ‘logic cannot yield us any objects’—might be challenged. (See, e.g. Hale 1987.) But this is not the place for such considerations.

Field, we have seen, wants to claim that mathematics is not, strictly speaking—‘taken at face value’—true, although of course it *is* useful, the utility being in terms of mathematics as a kind of inference-tool. Now, suppose we take this to be *all* that mathematics amounts to in the way Field does. We may nevertheless want to give an account of mathematical knowledge that will allow us to distinguish someone who knows a lot of mathematics from someone who only knows a little. What is this knowledge that the former has more of? Given that mathematics in use is exclusively inferential at the same time as being uniformly false, we are going to have to gloss mathematical knowledge as knowledge about what follows from what. This is what Field does, more or less. Only ‘more or less’, indeed, for there is a kind of mathematical knowledge, Field points out, which is just empirical. Someone who knows lots of mathematics will know more than average about which theorems the mathematical community accepts as proved, what are the accepted axioms of a group or Hilbert Space, and so on. This kind of knowledge, empirically gained, is not logical. According to Field, though, knowledge of ‘what follows’ *is* a species of logical knowledge. It is in this sense, he claims, that mathematical knowledge is logical knowledge.

Field labels ‘deflationism’ the view that, apart from the kinds of empirical knowledge such as those considered above, knowledge of ‘what

follows', supplemented with knowledge of *consistency* within mathematics, exhausts the possibilities for mathematical knowledge. He delineates his preferred kind of deflationism about mathematical knowledge in a basic way as follows:

... the only knowledge that differentiates a person who knows lots of mathematics from a person who knows only a little (aside from empirical knowledge of various sorts ...) is

- i. knowledge that certain mathematical claims follow from certain other mathematical claims or bodies of claims,
- ii. knowledge of the consistency of certain mathematical claims or bodies of claims, and other knowledge of a basically similar sort; and that all this knowledge is logical. (Field 1989, p. 83)

There is an immediate problem with this for Field, now, if he is to maintain his fictionalist nominalism. There are two standard ways of glossing claims of 'what follows' and consistency, the semantic and the syntactic (or the model-theoretic and the proof-theoretic). Each of these, however, involves commitment to some kind of abstract entity—*models* in the case of the semantic, *formal derivations* in the case of the syntactic. The problem, then, is that a nominalist seems unable to deal with logical knowledge if that knowledge is, as it appears Field claims it to be, of models or derivations, since to a nominalist these latter exist no more than do any other abstracta. Field attempts to solve the problem by moving back from the metalanguage (in which we talk *about* a language) to the object language (in which language does its everyday work).

Thus, instead of taking the knowledge involved in (i) to be *metalogical*, Field takes it to be *modal*, as follows. Suppose I know that *B* follows from *A*. Instead of glossing this as the claim that I know that in all models in which *A* is true, *B* is true, or that there exists a formal derivation of *B* from *A*, he suggests we take the claim as being that I know that it is necessary that *A* implies *B*. So what I know is what is expressed symbolically by the object language sentence " $\Box(A \supset B)$ " where ' $\Box$ ' is an *operator on* rather than a predicate of sentences. (See Field 1989 p. 85) Likewise, my knowledge that a conjunction *C* of mathematical statements is consistent

should be glossed as my knowledge that C is possible (In symbols, I know that " $\Diamond C$ ".)

Now there seems to be a new problem for Field. How is he to gloss this modal talk as part of the object language? Again, it looks as though we are going to be forced to accept abstracta, insofar as the standard, Kripkean, semantics for modal logic makes reference to models, in this case sets of possible worlds. Field responds by adopting a semantics for modality that is different from Kripke's.

Field takes a semantics using ordinary model theory, but without possible worlds. Sentences asserting possibility in a particular model, then, will be true if and only if what is claimed to be possible in that model is actual in some other model (this oversimplifies, but not in any misleading way):

$\Diamond B$ is true in M if and only if B is true in M^* for some model M^* . (Field 1989, p. 118)

This is fine so far as it goes. But there is an immediate apparent *further* difficulty, as follows. Field's semantics for modal logic uses the very model theory he has tried to gloss away. He is explicit about this:

The [alternative] model theory [for modal logic] will be, like Kripke's, platonistic, for it will presuppose a large body of pure set theory. (p. 116)

The question is, how can a fictionalist avail himself so apparently insouciantly of an avowedly platonistic model theory? Field attacks the problem head on, situating it rather nicely with regard to his fictionalism and the apparent problem raised for that by mathematical applicability. We have been in a similar situation before with Field; recall him in *Science Without Numbers*, arguing against the Platonist using Platonist premises. There he claimed that 'Platonism entails its own falsity'. What follows here is very similar.

In accounting for mathematical knowledge, as we have seen, Field claims that all we need to deal with (apart from empirical knowledge that mathematicians have about how other mathematicians work and so on) is knowledge of notions such as logical consequence and consistency.

Such notions, although standardly held to require knowledge of mathematical (metalogical) entities of some sort (models, formal derivations), can be dealt with, according to Field, by using modal analogues—*necessity*, *possibility*—without moving to a metalevel at all. However, the semantics of Field's modality seems to reinstate the requirement for some of those very mathematical entities that the whole enterprise of 'modalising away' metalogic in favour of a use of modality at the level of the object-language was intended to enable us to eschew. Field needs ordinary model theory just as a scientist needs ordinary mathematics. And just as Field claims, as we have seen, to have given an account of how ordinary mathematics 'need not be true to be good', so now he sees the need to explain how, likewise, ordinary model theory can be good without being true. As he says,

Traditionally, proof-theoretic concepts are defined in terms of mathematical entities, with the result that proof-theoretic reasoning becomes reasoning about mathematical entities. If we accept the usual definitions of proof-theoretic concepts, then a deflationist cannot regard proof theory as a subject of which we can have any knowledge. So how can a deflationist account for its utility? ... [This] is a problem that arises for semantics as well as for proof theory. (Field 1989, pp. 100–101)

Field explains how model theory and proof theory can be good without being true by, again, 'modalising away' commitment to abstract entities. This is a little more complicated, but, as we will see, in essence it involves the same sort of move as he made earlier.

Field claims that

... the central uses [to which model-theoretic semantics and proof theory are put] are as devices for finding out about logical possibility. (p. 104)

How can we find out, that is come to know, whether something is logically possible? The standard Platonist, model theoretic, answer depends upon the existence of models. Here are two of the schemata Field pinpoints in this regard (the '*model-theoretic possibility schema*' and the '*model existence schema*'):

(MTP) If there is a model for ' A ' then $\Diamond A$

(ME) If there is no model for ' A ' then $\neg\Diamond A$ (*ibid.*)

That is, we come to know that A is possible or impossible on account of the existence or non-existence of a model for A . But if there are no models, then, as Field points out, MTP is just '*vacuously true*' (p. 108) by *ex falso quodlibet*, and ME is invalid since its antecedent is true independently of whether $\Diamond A$ or $\neg\Diamond A$. Schemata using proof theory rather than model theory for finding out about possibility and impossibility are similarly vacuous or invalid, *mutatis mutandis*.

So it looks as though Field's deflationism is in trouble. However, Field claims that 'the problem is easily solved.' (*ibid.*) The modal equivalent of (MTP), for instance, allows us to specify how a deflationist can find out about possibility without taking on any commitment to the existence of models:

(MTP[#]) If $\Box$ (NBG $\supset$ there is a model for ' A ') then $\Diamond A$ (*ibid.*) ['NBG' is von Neumann-Bernays-Gödel set theory, which Field takes as his standard base for mathematics.]

This looks similar to what he did above, taking talk of possibility to the object level to avoid metalevel commitment to the existence of possible worlds. Here the object level/metalevel distinction is not in play—what is in question is the form epistemological schemata for modality ['devices for finding out about logical possibility' (*ibid.*)] should take—but the move looks familiar.

Too familiar, it might be thought: there is an apparent whiff of circularity about Field's development here. He modalises away commitment to the existence of models, first, by expressing the notions of consequence and consistency in modal terms in the object language. But, then, the semantics for these modal terms—avowedly Platonistic—seems itself to require commitment to the existence of models. Now in turn *this* commitment is modalised away by an expression of conditions for knowledge of modality in terms of (in our example) " $\Box$ (NBG $\supset$ there is a model for ' A ')". It looks as though in order to know that something is

possible we have to know that something else is necessary, where the meaning of this latter necessity is explained in terms, not of models, but of modality. In short, in Field's scheme it looks as though modality is explaining itself.

Is there a way out of this circle? Yes. We are interested in what we can know about, and how we find out about, logical possibility and necessity. What Field has managed to do is to explain how we might find out about what is possible, not in terms of what things (models, formal derivations) exist, but in terms of what we might be able to *do*. If, using model theory (that is, a branch of mathematics), we can derive a statement (remaining within mathematics) saying that a certain conjunction of statements A has a model, then we know that A is logically possible. That is how we need to gloss the statement " $\Box$ (NBG $\supset$ there is a model for ' A ').". We are bootstrapping modality, in a way, but we are beginning the process by thinking about what we can *do*, and not about what exists. As Field puts it

The deflationist can use [MTP[#] and similar schema] in much the same way the platonist uses [MTP likewise]: to find out that A is, or is not, logically consistent, it suffices to derive a model-theoretic or proof-theoretic statement from standard mathematics. (*ibid.*)

Once we have taken this on board, we are free to use standard model theory just as the platonist does—and indeed we have a description of what is going on that does not depend upon existential commitment.

Deductivism and Implausibility

Now, this exegesis of Field shows his turn away from Platonism, as well as involving a commitment to a view of mathematics in use as expressing rules rather than truths about some abstract realm, to be equally tied to a view of mathematics as based ultimately on what we *do*—on particular *practices* that we (as mathematicians) share. Both of these aspects of Field's view, as I hope will become clear, are going to be worth salvaging—and both are shared by Wittgenstein. However, I want to look more

closely at the way in which Field's denial that mathematics expresses truths about an abstract realm plays out. He wants to say, in effect, that mathematical propositions do actually make claims on truths about an abstract realm in spite of this ultimate basis of mathematics in *practice*. Such claims, according to Field, are unfulfilled only because of the (contingent) non-existence of the mathematical objects that, given his commitment to semantic homogeneity, he nevertheless feels forced to maintain as referents for the mathematical terms in (rule-expressing) mathematical propositions. This is *prima facie* decidedly odd—something that Field himself admits, as we will see shortly. We can tidy matters up considerably by engaging with some of Wittgenstein's remarks about what it might be to take mathematics 'at face value'—an engagement which will involve considering what aspects of semantic homogeneity we should take seriously other than the commitment to an over-arching semantic theory such as that we have seen Field to support.

Field takes the trouble to distance his development from a philosophical theory he designates 'deductivism', (often elsewhere known as 'if-thenism'). A deductivist about mathematics holds that any mathematical statement *M* should be interpreted as stating not baldly that *M*, but rather only that *if* (the axioms of some mathematical theory), *then M*. Field characterises the difference between his view and deductivism as follows:

... deflationism, unlike deductivism, does not claim that mathematical claims mean other than what they appear to mean. (Field 1989, p. 114)

This is important for Field. He does not want to reinterpret mathematical statements. He wants to take them at face value. On his account, mathematical objects play no role in our understanding of mathematical practice—but he holds that this is not reflected in the analysis of the meanings of mathematical propositions themselves, where it remains important as an aspect of this very 'taking at face value'. We have seen that for Field, the only alternative to either his fictionalism or basic Platonism is some other theory, usually idealism of one sort or another. This turn to other theories does not exhaust the philosophical possibilities, though.

In fact, Field's commitment to taking mathematics 'at face value' surfaces in the paper we have just been considering at the end of his discussion of mathematical knowledge. Thus, continuing the characterisation of the difference between deductivism and deflationism, he says

Instead of saying that mathematical assertions don't mean what they appear to mean, the deflationist says that what they literally mean can't be known: the knowledge that underlies a mathematician's assertions is not what those assertions literally say. (*ibid.*)

It is important to be clear about what Field is claiming here. It is not that mathematicians cannot know what they (literally) mean when they make a mathematical assertion. According to Field, they know very well what they mean, but given what they mean, what they assert is unknowable since the entities they refer to do not exist. Thus, what these mathematical assertions literally say is *false*, according to Field, and so indeed what they say cannot be known. Field has given us a plausible characterisation of what mathematical knowledge might amount to if we take all this on board—mathematical knowledge, he claims, is nothing to do with knowledge of entities, rather it is to do with knowledge of what follows from what.

But what of these 'entities', now? It is clear that for Field the meaning of any mathematical assertion is closely connected to the existence of mathematical objects. When I say that 28 is a perfect number or that the square on the hypotenuse of any Euclidean right-angled triangle is equal to the sum of the squares on the other two sides, what I *mean*, according to Field, includes an assertion that numbers, triangles, squares etc., taken in a mathematical sense, all exist. Since they do not exist, according to Field, I cannot know what I assert, even though I can know what I mean when I make the assertion. According to Field I cannot know that 28 is a perfect number or that the square on the hypotenuse (etc.), because there are no numbers or hypotenuses. What I can know, however, he claims, is what *follows* from such assertions, and it is this that is uniquely important in doing mathematics as well as forming the basis for its application.

Given the ultimate importance—recognised by Field, I have emphasised—of what is involved in *doing* mathematics as opposed to

what, or whether, mathematical entities actually exist, it might seem otiose to press the connection between meaning and existence. This is not something that Field can allow, however, in the light of the way in which he takes the commitment to what he describes as ‘taking mathematics at face value’. It is something I do want to press, though.

That what mathematical statements mean cannot be known, Field admits, takes a bit of swallowing:

I’m afraid many readers will still find this implausible. (*ibid.*)

But, then, characteristically, he goes on to say that it is *less* implausible than what the deductivist claims. There is no better theory in play, Field seems to be saying, and Platonism is itself so very implausible, so we should swallow the implausible consequences of his own theory.

Meanings and Beetles: Wittgenstein on Semantic Homogeneity

It is clear that Field’s ‘literal meanings’ play no part in how mathematical assertions are used, nor in how we get to know mathematics in order to be able to use it. Wittgenstein finds a similar dissociation of meaning and use in talk about private mental ‘objects’ of sensation. He has a striking and well-known figure to emphasise this point in *Philosophical Investigations*:

... Suppose everyone had a box with something in it: we call it a “beetle”. No one can look into anyone else’s box, and everyone says he knows what a beetle is only by looking at his beetle. – Here it would be quite possible for everyone to have something different in his box. One might even imagine such a thing constantly changing. – But suppose the word “beetle” had a use in these people’s language? – If so it would not be used as the name of a thing. The thing in the box has no place in the language-game at all; not even as a something; for the box might even be empty – No, one can ‘divide through’ by the thing in the box; it cancels out, whatever it is.

That is to say: if we construe the grammar of the expression of sensation on the model of ‘object and designation’ the object drops out of consideration as irrelevant. (Wittgenstein 1958, §293)

There are clear differences between our talk of objects of sensation and of mathematical objects. However, I am claiming a similar possible dissociation of meaning from use in both cases, and I want to suggest we make use of Wittgenstein’s figure in our case. We have seen, specifically, how Field makes the move away from commitment to the existence of abstract entities towards a conception of what we *do* in the sense of deriving mathematical assertions in ‘bootstrapping’ knowledge of modality into his theory. The abstracta, which he claims not to exist, clearly play no part in this. And ‘what mathematical assertions literally mean’ likewise plays no part in the account of mathematical knowledge he advances. That is to say, if we construe the grammar of the expression of mathematical assertion on the model of ‘object and designation’ the object drops out of consideration as irrelevant. Moreover, we should note that even if they *did* exist, these abstract objects would still be irrelevant to the *use* we might make of mathematics, as Field’s analysis underlines. Platonic objects themselves ‘drop out of consideration as irrelevant’—are ‘beetles in boxes’—just as much for the Platonist as for the fictionalist.

Field’s abstract objects, which he claims do not exist, are ‘beetles in boxes’. We have seen his involvement with these (admittedly implausible) non-existent semantic coleoptera to be generated by his commitment to semantic homogeneity—to his determination to take mathematics at face value. Or, rather, it is generated by the *manner* in which he feels he needs to take mathematics at face value, involving as it does a prior commitment to a particular semantic theory. What might it *be* to take mathematics at face value; what should *count* as taking mathematics at face value? These are questions we should raise.

Let us focus on the point about differences in use contrasting with apparent similarity of surface linguistic form. We have seen that Field’s commitment to a particular semantic theory—and so to a particular way of glossing what ‘taking at face value’ amounts to—makes it impossible for him to deal with this contrast satisfactorily. We just have to swallow the implausible consequences of this analysis, Field claims, including that

‘what [mathematical propositions] literally mean can’t be known’ and so on. However, we *can* escape from these implausible consequences if we attend carefully to just what ‘taking at face value’ might amount to *without* shackling ourselves to a semantic theory such as we have seen Field (and many other present-day philosophers, we must admit) to espouse. We need to re-establish a connection between meaning and use—a connection that should have all the more importance for one such as Field whose epistemology of mathematics finds the use of mathematics so fundamental.

Of course Wittgenstein reminds us of the importance of the relation of use to meaning. This is more than a slogan: In *Philosophical Investigations* §8, Wittgenstein asks us to consider a pared-down use of mathematics. Imagine some builders using a language with just a few names, ‘slab’, ‘block’, and so on, words for ‘there’ and ‘this’, and some letters of the alphabet, ‘a’, ‘b’ and so on, these latter learned in order, so as to function as numerals. A builder might say to an assistant, ‘d—slab—there’, for example, and it is clear enough how such an order might be carried out. He goes on to consider how the words of this language *signify*, *refer to*, or what they may be said to *mean*:

Now what do the words of this language signify? – What is supposed to shew what they signify, if not the kind of use they have? And we have already described that. So we are asking for the expression “This word signifies this” to be made a part of the description. In other words the description ought to take the form: “The word.... signifies....”.

Of course, one can reduce the description of the use of the word “slab” to the statement that this word signifies this object. This will be done when, for example, it is merely a matter of removing the mistaken idea that the word “slab” refers to the shape of building-stone that we in fact call a “block” – but the kind of ‘referring’ this is, that is to say the use of these words for the rest, is already known.

Equally one can say that the signs “a”, “b”, etc. signify numbers; when for example this removes the mistaken idea that “a”, “b”, “c”, play the part actually played in language by “block”, “slab”, “pillar”. And one can also say that “c” means this number and not that one; when for example this

serves to explain that the letters are to be used in the order a, b, c, d, etc. and not in the order a, b, d, c.

But assimilating the descriptions of the uses of words in this way cannot make the uses themselves any more like one another. For, as we see, they are absolutely unlike. (Wittgenstein 1958, §10)

We should note that Wittgenstein explicitly allows that we say ‘The word ... signifies ...’ in different cases. As he points out, too, we can also talk of words ‘referring’ to something, or of them ‘meaning’ this or that. There is no hint of a denial of face-value interpretation here: rather what is at issue is what ‘face value’ might amount to. When Wittgenstein explicitly mentions ‘that “c” means this number’, he does not wish to deny either the utility or, in context, the truth of such an assertion. Wittgenstein’s point is just that surface similarity does not make “c” like “slab” or “here”. ‘Signifies’ does not express a relation that is always the same, and no more do its cognates such as ‘refers’ or ‘means’. To know how a word signifies, refers to or means something, we have to look at the use we make of the word in question.

We have in train an examination of what Field meant by ‘taking mathematics at face value’. As we have seen, in Field’s analysis this seemingly innocuous phrase stands proxy for a substantial commitment to an underlying semantic theory that seems to vitiate his conclusions pointing towards the use of mathematical propositions in inference rather than involving the existence of referents for mathematical terms in his account of mathematical knowledge. Recall what Field feared many readers would find ‘implausible’:

Instead of saying that mathematical assertions don’t mean what they appear to mean, the deflationist says that what they literally mean can’t be known: the knowledge that underlies a mathematician’s assertions is not what those assertions literally say. (Field 1989, p. 114)

This expression of a radical dissociation of meaning and use is just what I am calling into question. Can we bring ‘taking at face value’ back to earth in terms of how we use mathematical discourse while eschewing

the commitment to semantic theory which seems to engender such disconnection? We can if we see ‘taking at face value’ not just as a matter of the syntax or surface grammar of a linguistic item, but rather as a matter of looking at how the item in question is *used*.

The Mathematical Realm

Field is explicit about what his ‘taking at face value’ entails. We saw above in his characterisation of the ‘face value thesis’ how it connects with the requirement for homogeneity of semantics: reference to concrete objects in physical object language is supposedly mirrored by reference to abstract objects in mathematical language. This mirroring, in its turn, according to Field, entails taking a stance on a particular metaphysical issue, that of the existence or not of mathematical entities, where talk of the existence of such entities has the same import as talk of existence of concreta. There is a move made here that might go unnoticed, and again it is connected with an unexamined commitment to homogeneous semantic theory. From the homogeneity of the semantics, we move to the homogeneity of what we mean by the ‘existence’ of the entities we’re talking about. We need to look carefully at how this homogeneity of different kinds of existence plays out.

G.H. Hardy expresses the underlying idea clearly

By physical reality I mean the material world ... [but] there is another reality, which I will call ‘mathematical reality’ ... (Hardy 1992, pp. 122–3)

The parallelism between the material world and ‘mathematical reality’—the idea that we can think of mathematical existence as simply on a par with real existence—is something Wittgenstein queries (Wittgenstein is responding here to another expression by Hardy of his mathematical Platonism; Wittgenstein misquotes Hardy, but the point is clear):

Professor Hardy is comparing mathematical propositions to propositions of physics. This comparison is extremely misleading. (Wittgenstein 1976, p. 240)

Field's way with 'face value' is equally misleading. Here is Field again:

... a mathematical theory, taken at face value, is a theory that is primarily about some postulated realm of mathematical entities ... (Field 1989, p. 2)

Taken with the assimilation of talk of abstracta to talk of concreta we saw in his characterisation of the 'face value thesis', it is easy to see that Field is building his own notion of 'face value' on what Wittgenstein describes as an extremely misleading comparison. Let us consider further how we might be misled.

Recall Field's emphasis on a particular way with Frege's context principle. We saw above that for Field syntax determined semantics but not ontology, so that he was able to specify a semantic theory homogeneous over abstracta and concreta whilst denying the existence of the former though accepting the existence of the latter. Thus, for Field, mathematics is *about* mathematical objects even though there are no such objects. To engage in mathematical discourse, then, for Field, is to talk about some realm of mathematical objects—although since these objects do not exist, he claims, it turns out that mathematical discourse is universally *false*. However, we are not forced to follow Field here. To take mathematics at face value is not particularly to take mathematics as being about anything at all like Field's (or Hardy's) 'postulated realm', at least if taking things this way conceals the different ways mathematics and physics might be about their own realm. This difference will become apparent once we focus on the *use* we make of the different kinds of discourse. Thus, even if we decide to continue to talk in this way of mathematics being about the realm of mathematics, we should take the trouble to avoid being misled by the comparison with the physical realm that physical-object talk is in its own way *about*. We need to have an overview, that is, of the *differences* between the way in which we use mathematical propositions and propositions of physics even supposing we assimilate the descriptions of these uses under the banner of each being 'about' its own realm.

Let us agree with Field that to take mathematics at face value is to take mathematics as being about what it appears to be about—numbers,

geometrical shapes, groups, Hilbert Spaces, and so on. An important issue I am suggesting Field misses concerns the force of this ‘about’: what is it for mathematics to be ‘about’ numbers and so on? It is at this point the homogeneity of semantic theory raises itself. Taking mathematics to be about numbers and so on does not entail that the way in which mathematics is about numbers is just the same as the way in which physics is about *its* objects.

This is the moral Wittgenstein suggests to us in his consideration of how different words or signs *signify* in *Philosophical Investigations* §10ff that I quoted earlier. As he concludes §10,

... assimilating the descriptions of the uses of words in this way cannot make the uses themselves any more like one another. For, as we see, they are absolutely unlike.

The language-game in question is to be taken as a point of comparison—as illustrating a particular aspect of our own use of language. Like the users of the ‘block, pillar; ... a, b; ... here, there’ language Wittgenstein posits, we use names of physical-object types to refer to physical objects, as likewise we use numerals to refer to numbers, and indexicals of place to refer to particular positions. As Wittgenstein suggests, talk of each of these kinds of signs or words signifying or referring is philosophically harmless ... but only so long as we remain aware of the distinctively different uses each kind has in the language while we assimilate the descriptions of their uses under the banner of ‘signifying’, ‘referring’ or ‘being about’ some particular realm. We can allow that a mathematical proposition is about abstract objects in the same breath as saying that a physical proposition is about physical objects, so long as we keep in mind the difference in the way we use propositions of mathematics compared with propositions of physics.

Summary and *Envoi*

I have taken issue with Benacerraf’s dilemma concerning the difficulty of maintaining a semantics that is homogeneous over mathematical and physical discourse contemporaneously with a suitable mathematical

epistemology. Specifically, I have challenged the first horn of this dilemma. Øystein Linnebo sets out the challenge:

Let me just note that a lot of work is needed to substantiate this sort of challenge [*sc.* to what I've called homogeneity of semantic theory as applied to mathematics]. The challenger will have to argue that the apparent semantic similarities between mathematical and non-mathematical language are deceptive. And these arguments will have to be of the sort that linguists and semanticists—with no vested interest in the philosophy of mathematics—could come to recognize as significant. (2011)

'This sort of challenge' is what I have suggested Wittgenstein makes in *Philosophical Investigations* §10 and elsewhere. It is not that Wittgenstein wants to deny that we should take mathematics at face value, we have seen. Rather, he wants us to see that what 'taking at face value' amounts to is itself not homogeneous. This is a delicate matter, but it is at least plain we should notice the *differences* between the way in which we allow that numerals refer to numbers and the way in which names of physical objects (or more precisely types thereof) refer to physical objects, even while we appreciate the similarities. An appreciation of these differences enables us to see the way in which talk of a Hardyesque 'mathematical reality' or a Fieldian 'postulated realm' is, as Wittgenstein claims, 'highly misleading'. Furthermore, an appreciation of these differences has enabled us to blunt a horn of Benacerraf's dilemma.

What we have arrived at need not be seen as a knock-down argument against the philosophical utility of semantic theory or even of the Benacerrafian imperative for semantic homogeneity. There may be other reasons for sticking to a theoretical approach and attempting to accommodate what looks like recalcitrant data; Field himself may be read as taking such a view in so readily admitting the 'implausibility' of his results whilst nevertheless sticking to them. (Likewise, there are many accounts of Moore's paradox that attempt to fix the difficulty it brings to light by appropriate adjustment to the semantic theory in play; To pick an example more or less at random, see, for instance, Williamson 1996.) What we have from Wittgenstein is to be taken as suggesting or

reminding us of a way of looking at the matter to hand rather than a particular refutation of a Fieldian view of what ‘taking at face value’ amounts to. Nor, it should be emphasised, does such a way of looking at ‘face value’ or requirements for semantic homogeneity itself associate itself naturally with a different *theory* about how numerals ‘refer’ to numbers and so on.

In spite of such caveats we have made substantial progress. We can see now a way of blunting at least one of the horns of Benacerraf’s dilemma by getting clear about different possibilities of taking ‘taking at face value’ and accompanying perceived requirements for semantic homogeneity. Furthermore, this chapter’s foray into Field’s fictionalism has shown us the way in which Field’s own turn away from mathematical Platonism leads to an emphasis on the importance of the *use* made of mathematical propositions as rules of inference as well as the *unimportance* of the notion of a mathematical realm separate from the physical in considering what we do when we so use mathematics.

Further than this, though, we are beginning to see that this notion of a mathematical realm that mirrors the physical realm in some way is not just unimportant for our view of mathematics in use; it is very likely to be positively misleading. For example, thinking of a mathematical realm in this way makes plausible the idea of mathematical discovery as analogous to discovery in science; but we have seen the difficulties inherent in assimilating mathematical to physical epistemology, which should be enough in itself to give us pause. Is there another way of looking at mathematics that avoids such difficulties? Indeed there is, and it will come as no surprise to find it in Wittgenstein. It is to this I turn in the next chapter.



<http://www.springer.com/978-3-319-63990-1>

Wittgenstein, Mathematics and World

Clark, B.

2017, XIII, 215 p. 9 illus., Hardcover

ISBN: 978-3-319-63990-1