Solving the Proper Problem: Wittgenstein, Fictionalism and the Applicability of Mathematics

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Abstract

This thesis proposes a solution to the problem motivating Albert Einstein's question, 'How can it be that mathematics … is so admirably appropriate to the objects of reality?'; the problem of mathematical applicability. It considers Mark Steiner's anthropocentric non-naturalist attitude to the problem in the light of Hartry Field's fictionalism, contending that Field's proposed solution, though *prima facie* naturalist in character, in fact inherits non-naturalist commitments which were problematic in anthropocentric solutions. Specifically, Field's solution retains a view of the grammar of our mathematical expressions as based on the model of object-and-designation. However, Field finds importance in the notion that what mathematicians *know* is non-propositional in the sense of being concerned with inference rules rather than assertions of existence, and a way of exorcising the rogue grammatical commitment in question from the fictionalist account can develop from a focus on this aspect of our talk about mathematics *tout court* rather than just epistemologically. The view that emerges when we do this, in the first instance enables a naturalistic and wholly adequate solution to the problem of mathematical applicability, and in the second instance is precisely that articulated by Ludwig Wittgenstein. Einstein's question seems pressing only if we understand the applicability of mathematics on the model of a kind of 'fit' between two realities, the mathematical and the physical/empirical. Once we attain an overview of the grammar of our mathematical talk and thereby recognise it for what it is, the question of *fit* between mathematics and the world can be seen to be empty. The importance of the notion of an *overview* of the problem and the general philosophical terrain in which it sits chimes with Wittgenstein's own attitude to the requirement for philosophy of achieving what he calls an 'übersichtliche Darstellung'.

Contents

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Introduction

In an address to the Prussian academy of Sciences in Berlin in 1921, Albert Einstein famously asked the question, 'How can it be that mathematics … is so admirably appropriate to the objects of reality?' (1922, p. 28) This thesis proposes a solution to the problem motivating Einstein's question; the problem of mathematical applicability.

I argue that Einstein's question seems pressing only if we understand the applicability of mathematics on the model of a kind of 'fit' between two realities, the mathematical and the physical/empirical. This model depends on a particular view of the grammar of mathematical sentences, namely that such sentences express propositions or judgements about mathematical reality. However, I will suggest, a grammatical investigation into what mathematicians say and do when practising mathematics (rather than talking *about* mathematics) enables a point of view from which it is possible to see the sentences of mathematics as being, in an important sense (which will need careful clarification) not about anything at all.

Einstein sets the problem in a particular way. Hidden by the ellipsis in the quotation above is the claim that mathematics is '… after all a product of human thought which is independent of experience', something that should tip us off about Einstein's question as a variety of Immanuel Kant's 'proper problem of pure reason' ('*die eigentliche Aufgabe der reinen Vernunft*'):[1]

> … the proper problem of pure reason is contained in the question: How are synthetical judgments *a priori* possible? (1934, p. 35)

Regarding this 'proper problem', Kant goes on to say

> … In the solution of the above problem, we … have therefore to answer the questions: How is pure mathematics possible? How is pure science of nature possible? (*ibid.*)[2]

Nowadays 'pure mathematics' is most often taken in opposition to 'applied mathematics', but I take Kant not to be trading in any such distinction. The question Kant is pointing up is that of how, given that mathematics is a priori, 'a product of human thought independent of all experience' as Einstein had it, it is synthetic and hence can usefully be applied to the world – is 'so admirably appropriate to the objects of reality'.

That is my problem, then: the problem of mathematical applicability, of explaining how mathematics is so *useful* in application to the real world. As we will see, it will

[1] Reading the *Critique of Pure reason* is also what started me thinking about this problem, many years ago, which is another reason for my wanting to reference Kant here at the beginning. (This will be my last autobiographical detail!)

[2] The problem, for Kant, is wider than that of simply accounting for the applicability of mathematics. The latter is perhaps its most important aspect, however.

be important for the account that I want to advance that we have a clear view of how the problem has developed; I will examine some other characterisations and expressions of this problem and situate it historically in **Chapter 1**.

One way of attempting to solve the problem of mathematical applicability is to appeal to what we might call *anthropocentric non-naturalism*. See, for instance Steiner 1998, where he claims in conclusion that there is a

> … true 'correspondence' … between the human brain and the physical world as a whole. The world, in other words, *looks* 'user friendly'. This is a challenge to naturalism. (p. 176)[3]

I do not want to make that move. We should be wary of the implicit appeal to some version of 'God organises it so' that such anthropocentric non-naturalism makes. That this view can even be considered in this context I take to suggest more the difficulty of the problem rather than anything we could call a solution. I will be referencing Steiner's work at several points in the thesis, although I will take it to be a constraint on an adequate solution to the problem of mathematical applicability that the solution be naturalistic in precisely the way that Steiner's is not.

Steiner's anthropocentric non-naturalism can seem the only possible solution to the problem because, as the quotation above makes clear, the problem seems to demand an explanation of the match between mathematical objects of thought and the natural world. A requirement for such a match may itself be seen as deriving from some kind of *realism* about mathematics, given that it may appear that mathematics has to match the real world in order to apply to it.

However, more influential than Steiner's 'non-naturalism' about mathematics in recent times, particularly with regard to mathematical applicability, has been a kind of naturalist *anti-realism* about mathematics, developing within what Michael Friedman described as

> The first (recent) serious study of what we might call the philosophy of *applied* mathematics: the use of mathematics in empirical theories. (1981, p. 505.)

Friedman is referring to Hartry Field's fictionalist philosophy of mathematics as first adumbrated in his seminal monograph *Science Without Numbers*. I will begin seriously to consider Field and fictionalism in **Chapter 2**.

Field's fictionalism shows us a particular way of turning away from realism (so-called 'Platonism' of one sort or another) in the philosophy of mathematics, and hence of avoiding a commitment to anthropocentric non-naturalism. However, I will argue that Field's proposed solution to the problem of mathematical applicability, though *prima facie* naturalist in character, in fact inherits non-naturalist

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[3] In his 2005, Steiner explains his non-naturalism as a genre of 'Pythagoreanism'; see p. 648. The moral is the same as in his earlier work, I take it. 'The world is made of numbers [or matrices of (complex) numbers]' (*ibid.*) seems as much a sidestep – as little a solution of the problem of mathematical applicability – as 'God makes it happen'.

commitments which were problematic in anthropocentric solutions. Specifically, Field's solution retains a view of the grammar of our mathematical expressions as based on the model of object-and-designation. This observation will lead me to ask whether there is a way of rejecting this commitment.

Apart from the turn away from mathematical realism, Field finds an importance in the notion that what mathematicians *know* is non-propositional in the sense of being concerned with inference rules rather than involving assertions of existence. I argue that a way of exorcising the rogue grammatical commitment in question from Field's account can develop from a focus on this aspect of our talk about mathematics *tout court* rather than just epistemologically. The view that emerges when we do this, in the first instance enables a naturalistic and wholly adequate solution to the problem of mathematical applicability, and in the second instance is precisely that which is articulated by Wittgenstein in his later writings. I argue that once we attain an overview of the grammar of our mathematical talk and thereby recognise it for what it is, the question of *fit* between mathematics and the world (or between mind and world) can be seen to be empty.

This notion of 'recognising it for what it is', and of gaining an 'overview' can be misleading. The claim here is not that we should, for instance, adopt a semantic theory about mathematical discourse that takes mathematical assertions as expressing rules rather than existential commitment. It is part of my argument that the debate between realism and anti-realism in the philosophy of mathematics is not to be ended by filling putative gaps in the logical space of available theories in such a manner. Rather, I claim, a perspicuous overview[4] of the philosophical terrain in question is just in itself what is needed. Such an overview resists summary expression by its very nature, but my hope is that this thesis will epitomise a manner of attaining one such.

I am not going to expatiate here in any detail on Wittgenstein's well-known (and still controversial) remarks about the nature of philosophy. The consequences of some of these may – I hope they will – come to light as we proceed. However, there

[4] An 'übersichtliche Darstellung', see *Philosophical Investigations* §122 for instance. The notion Wittgenstein is putting in play here is subtle and somewhat tricky; its translation is controversial, as is its import. Anscombe in her translation has 'perspicuous representation'; Hacker and Schulte in their more recent translation go for 'surveyable presentation'; Kuusela (2008) writes of 'perspicuous presentation'. For myself, I want to accent the 'overview'. It is the lack of such that Wittgenstein stresses in §122 when he says 'wir … nicht *übersehen*'. 'We do not *oversee* [the use of our words]' is connotationally clumsy in English, but the emphasis is plain. I am not going to enter further into translational controversy except to assert that whatever the translation, Kuusela's gloss on the notion and its place in Wittgenstein's philosophy is more or less correct. (See Kuusela 2008 pp. 228-238.) What I aim for in this thesis can be seen, I hope, as an example of what Wittgenstein meant. (There is a fairly thorough investigation of this notion of an overview (*Übersicht*) and its historical genesis in Glock 1996 pp. 278-283.)

is one specific aspect of Wittgenstein's methodology facilitating such an overview I want to highlight and adopt. According to his pupil Maurice O'Connor Drury, Wittgenstein considered as a motto for *Philosophical Investigations* the line from *King Lear*, 'I'll teach you differences' (Fann 1978, p. 69) – emphasising what he saw as a contrast between his own emphasis in philosophy on gaining a clear overview including particularities and the 'craving for generality' he saw as at the root of many of our philosophical problems. (*BB* p. 17 etc.) There are two specific 'differences' of this kind that figure prominently in the thesis as a whole.

First – this is what I investigate and develop in **Chapter 3** – I want to look at the difference between what Wittgenstein refers to as the 'determination of a concept' as opposed to the discovery of 'a fact of nature'. (*RFM* p. 131)[5] This is a distinction that is germane to the development of our number system, and particularly with regard to mathematical notions of *infinity*. The idea of infinity in mathematics is one with perennial interest for the philosopher; it is central for much of the philosophy of mathematics as exemplar and generator of many of the perceived problems therein. A clear overview of the development of ideas of numbers in general and transfinite numbers in particular will help undermine the picture of our mathematical discourse that is responsible for the demand to explain the fit between mathematics and the world.

Chapter 3, then, argues for due importance to be given to the notion of mathematics as proceeding via determination of concepts rather than common-or-garden discovery of facts. However, there is an immediate apparent difficulty with viewing mathematical development in this way. This connects with what I mentioned above as something shared by Field and Wittgenstein – the notion of mathematics as in some sense involved with rules of inference rather than assertions.[6] The difficulty lies in the way in which such a focus away from discovery might seem to lead to some kind of conventionalism about mathematics and logic. The risk is that mathematics and logic might thereby lose their *necessity*. I deal with this in **Chapter 4**, arguing, along with Barry Stroud, that we can, with Wittgenstein, avoid the kind of conventionalism he is saddled with by philosophers such as Michael Dummett through paying careful attention to notions of *sense*. In brief, Stroud explains Wittgenstein as saying that the impossibility of its not being the case that *p* when *p* is necessary is based on the lack of sense we give to its not being the case that *p*.

The idea of sense or lack thereof turns out to be important also for the *second* of the specific 'differences' I mentioned above. In his *Lectures on the Foundations of Mathematics* Wittgenstein distinguishes two different ways in which we might try to make sense of mathematics being 'responsible', as he puts it, to 'a reality'. (*LFM*,

[5] The distinction has aspects worth consideration outwith the philosophy of mathematics, as we will see. I will not develop these in any detail, though.

[6] As we saw, for Field this is an epistemological matter; much less so for Wittgenstein. This will all need careful unpacking.

xxv, *passim*.) Wittgenstein's distinction is close to a distinction associated with Rudolf Carnap, between 'internal' and 'external' questions of existence. (Carnap 1956, pp. 205*ff*.) In **Chapter 5**, I develop and expand on this distinction, with reference to its use in a recent attack on mathematical fictionalism by John Burgess and a defence thereto by Mary Leng. This emphasis on the 'two ways' in which mathematics might be said to be 'responsible' in the light of this recent controversy emphasises for us how the 'perspicuous overview' we have gained amounts to a solution to Kant's (and Einstein's etc.) 'proper problem' of the applicability of mathematics.

The 'overview', and hence the solution I claim to have reached, is further aided by a reference back to Steiner's characterisation of the problem that I mentioned above, using a particular example of his which, he claims, shows how

> Wittgenstein's account of mathematical applicability was seriously lacking. (Steiner 2009, p. 26)

In confounding this claim of Steiner and finding the wherewithal in the overview we have developed to see the problem aright, I claim finally to have solved the problem of mathematical applicability.

(It is my hope that the thesis be understandable without advanced mathematical knowledge on the part of the reader. *Some* grasp of key concepts, however, will be useful for the overview I claim can be reached. So I end with two short mathematical **Appendices** explaining some basic notions for the non-mathematical philosopher.)

Chapter 1 The Problem Posed

1.1 Generalities

The idea that there are philosophical problems of mathematical applicability, in particular problems to do with the relationship between mathematics and the physical world, is not new. Surprise has been often expressed at the fact that mathematics is so fruitful – so *useful* in its applicability to physics. Often, indeed, the applicability of mathematics is seen as some sort of mystery, or ‘enigma’, as Einstein called it, for instance, in the address I mentioned above in the Introduction. Einstein went on to remark that this ‘enigma … in all ages has agitated enquiring minds.’(1922, p. 28)

This is my starting point. To begin to get the flavour of the enigma, it will be useful to consider several specific – and varied – examples of mathematical applicability, beginning with ancient history and continuing through to the present day. Such examples – and although I hope to offer a good selection, there could be many more – may be seen as evidence of the ubiquity of the ‘enigma’ as well as giving us a necessary part of the overview we seek.

Further to these specific examples, soon I am going to be quoting some *cris de coeur* – one at some length – from distinguished (non-philosopher) scientists about the way in which *they* are surprised, even sometimes explicitly amazed, that mathematics applies to physical science. By itself that may not be enough to convince us that an explanation is required. It is at the very least indicative, though.

But we, non-scientists or mathematicians, perhaps: how surprised should we be at the fact of mathematical applicability? It may be that we have become so used to the ubiquity of mathematical methods in the physical sciences, from the outside as it were, that it is difficult to feel any surprise at this. However, if we try to imagine ourselves in a situation in which the connection between mathematics and physics had yet to be made, we might see more clearly that there is something that appears to need explanation.

Consider, by analogy, other human activities or pastimes. Suppose, for instance, it turned out that some kinds of board game – chess, say, or go, or even monopoly – were useful tools for understanding physical theories. Would surprise be an appropriate response? Indeed so: we would require, seek, an explanation. Or suppose a group of intellectuals to work at numerating words in some sacred texts, developing theories about how these numbers could be combined and so on. Suppose further that the resulting theories could then be used successfully to predict the winners of horse-races or movements of the FTSE100, say. Surprise would be an altogether appropriate response to such Cabbalistic applicability. There would also be an imperative to explain the workings of this apparent miracle, or at the very least (more likely, perhaps) to explain them away.

Now imagine a situation in which mathematics was not known to be so applicable to science. And suppose some group of intellectuals to work at investigating the properties of permutations of roots of polynomial equations, developing theories about how these permutations could be abstractly combined and so on. Suppose further that the resulting theories could be used successfully to predict properties of elementary sub-atomic particles, their possibilities of combination and even their very existence. Surprise seems an appropriate response in this case too. Once again, there would be an imperative in such a case to explain the workings of this apparent miracle. There 'would be' an imperative; there *is*, then, such an imperative. In case it is not clear, this is an actual case. Group theory, developed initially in the context of considerations to do with roots of equations in the manner I described, has indeed turned out to be useful in physics in just this way.[7] I will return to some aspects of this in a little more detail below. The history may in the end be not quite as brute as it appears on this first telling; nevertheless, the example exemplifies the *prima facie* requirement for an explanation of the apparently mysterious connection between mathematics and the physical world.

So the very fact of mathematical applicability seems to need explaining. We can see something of a concern for the way in which mathematics applies to the world in earlier times than our own: Johannes Kepler, for instance, writing of how

> Geometry existed before the creation … geometry provided God with a model for the Creation …(Kepler 1619 IV, Ch. 1. Quoted in Koestler 1969 p. 264)

Or, perhaps more familiarly, Galileo with his

> … book, the world … written in the language of mathematics …(Galileo 1623 p. 237)

It may be stretching things to claim that Kepler and Galileo or their contemporaries found the applicability of mathematics particularly *mysterious*, especially given the religious background at the time at which they were writing. Nevertheless a concern with *some* kind of explanation of how mathematics relates to the physical world is evident here at the beginning of modern science. I will have a brief look further back in history shortly, but first I want to make the point that a feeling for the apparent mystery of mathematical applicability is far from unusual amongst Kepler's and Galileo's successors closer to our own time.

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[7] See, for instance, Lagrange's *Réflexions sur la résolution algébrique des equations* in *Nouveaux Mémoires de l'Académie Royale des Sciences et Belles-Lettres de Berlin* (1770, pp. 138-254), sections of which (including, for example, instances of what is now known as 'Lagrange's Theorem') amount to early work in group theory before this latter got its name. That the development of such determinedly *a priori* algebra contains clues about the symmetry of the electron or the existence of the omega-minus particle and the Higgs boson can appear magical, indeed. (I deal with some relevant aspects of electron symmetry and group theory below. … There is a picture of the first omega-minus track to be seen as well as a useful brief explanation of Gell-Man's '*Eight-fold Way*' at http://www.bnl.gov/bnlweb/history/Omega-minus.asp. ... The relation of the elusive Higgs particle to SU(2)xU(1) I leave as an exercise for the interested reader.)

Albert Einstein, in an address to the Prussian Academy of Science in Berlin in 1921, famously asked the question I mentioned in the introduction:

> How can it be that mathematics, being after all a product of human thought independent of all experience, is so admirably adapted to the objects of reality?

(His answer is not quite so well known, perhaps:

> In my opinion the answer to this question is briefly thus: As far as the laws of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality. (*ibid*.)

– But now the same question can be raised about the relationship between Einstein's certain, non-referring laws of mathematics, and the uncertain, referring laws: how can it be that they are so admirably adapted one to the other? So we still have our apparent mystery.)

Nor is Einstein a solitary figure amongst physicists in seeing something here that requires an explanation.

Eugene Wigner claimed in his 'The Unreasonable Effectiveness of Mathematics in the Natural Sciences' (1960), for example,

> … that the enormous usefulness of mathematics in the natural sciences is something bordering on the mysterious and that there is no rational explanation for it. (p. 2)

… And further

> … The miracle of the appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift which we neither understand nor deserve. (p. 14)

Then there is Heinrich Hertz:

> One cannot escape the feeling that these mathematical formulae have an independent existence and intelligence of their own, and that they are wiser than we are, wiser even than their discoverers, that we get more out of them than was originally put into them. (Quoted in Steiner 1998 p. 13)

And Steven Weinberg:

> It is positively spooky how the physicist finds that the mathematician has been there before him or her. (1986; quoted *ibid.*)

Richard Feynman:

> I find it quite amazing that it is possible to predict what will happen by mathematics which is simply following rules which really have nothing to do with the original thing. (1992 p. 171)

The list could be longer. As it is, it is enough of a chorus of surprise – amazement, even – at the fact that mathematics is just *so* well-suited to the task of explaining the workings of the world or helping us to understand physical reality that this fact demands to be taken seriously as a topic for philosophical investigation. However, as Mark Steiner has pointed out,

> … such sentiments have either been ignored or dismissed by contemporary philosophers. (1998 p. 14)

This claim may be too bold; as we will see, *some* contemporary philosophers do involve themselves with issues arising from the applicability of mathematics. Still, it might be suggested, the 'dismissal' Steiner complains of may be well-founded. Steiner himself sees the problem of mathematical applicability as peculiarly central to philosophy *per se*: referencing major philosophers of the past such as Plato, Descartes, Spinoza, Berkeley, Kant and Mill, he goes on to claim that

> This short sample of Western philosophy illustrates that the central philosophical doctrines of these major philosophers were conceived in great measure to explain the applicability of mathematics to Nature. (Steiner 2005 p. 626)[8]

That is a large claim and one we have to admit is not shared by 'contemporary philosophers' in general. *Pace* Steiner, perhaps Einstein, Wigner and the rest of the chorus are just bad philosophers, whatever their credentials in other areas of thought. Maybe we should see their expressed concerns more in the way of a kind of generalised sense of wonder at the fact that the universe has the order and regularity it does rather than as expressing genuine philosophical concerns. Steiner thinks not, so much is plain. But still: does this chorus amount to anything more than a joint expression of a vague sense of wonder that the world is, at bottom, mathematical? I want to look at this idea – of the world being at bottom mathematical – a little more carefully.

1.2 Ancient history

Long ago, Pythagoras and his followers were very impressed by the way musical intervals could be expressed as simple ratios. Here is something *surprising*: pluck a taut string, then pluck the same string held down at exactly half-way along its length. The two notes so produced sound, to our ears, very similar – so similar, in fact that we often call them by the same name: Cmajor … Cmajor, or doh … doh, for instance. Of course, the two notes sound at what we call an *octave* – the most consonant of all harmonies. A ratio of 2:1 on a stretched string will always produce this harmony. Other similar simple ratios of lengths will produce other simple harmonies. For example, hold the string down to divide it in the ration 3:2 to obtain a *fifth* (For the non-musical: a fifth is the second most consonant interval after the octave. Think of the first two notes of the bugler's *Last Post*, the notes doh…soh in tonic solfah.) Of course the names of the harmonious intervals are somewhat arbitrary, being based on our European (twelve-semitone) eight note scale. By whatever name, though, the fifth, together with the octave and the fourth (ratio 4:3,

[8] I mentioned Kant's characterisation of the problem of applicability in the Introduction. Steiner gives a brief summary of how each of the philosophers he mentions sees the problem, and of how each of them claims to solve it. Not all of Steiner's summaries are uncontroversial; I do agree with most of what he says at this point, however.

the next most consonant interval), have been the basis of musical harmony in just about all human cultures. We could, at this point, go on to talk of the next few simple integer ratios – 5:4 (major third); 6:5 (minor third) and so on. The consonance, though noticeable, is noticeably *less* than with the octave with which we began.

This is all *surprising*. What has it to do with the notion of the world being at bottom mathematical, though? Well, Pythagoras, so the story goes, was stimulated by his study of musical intervals and harmony[9] to conclude precisely that the world is, at bottom, composed of *numbers*. What this might mean precisely is perhaps a little obscure, but for our purposes now it is probably enough to gloss it as a claim to the effect that the physical world somehow embodies these abstract objects, the numbers. Further elucidation need not concern us just here: it's rather the *impulse* apparently given by the connection between numbers and an aspect of the experienced world – harmony – that I want to focus on.

Consider knowing nothing about the physics of sound apart from what we can actually *hear* and *see*. The connection between the ratios of the lengths of strings on a musical instrument and the pure harmonies we have seen and heard to be associated with them might well strike us as remarkable. That's not enough by itself, to be sure, to make us think of the whole world as made up in some unspecified way by the numbers or ratios (whatever *they* might turn out to be), but, taken together with other instances – perhaps from applied geometry, say[10] – of mathematics connecting apparently directly to an aspect of the experienced world, might we not be liable to express surprise at the way in which mathematics, a seemingly *a priori* intellectual exercise, manages to fit the physical world of experience so well?

Pythagoras, we are told[11], was the first to use the word *cosmos* of the whole universe: a use that has come down to us from the Greek. What has not survived into our use of the word is a particular connotation it had in Greek in Pythagoras' time, when it was used to express a kind of recognised or fitting *order* in what it referred to, be that a method of organisation of soldiers in ranks (a common use pre-Pythagoras) or the universe itself with the particular order given it by its mathematical nature in Pythagoras' own coinage. The *cosmos* is an *ordered* world, in short – ordered by mathematics.

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[9] See Guthrie 1962 pp. 220 *ff.* for references. Differentiating Pythagoras the individual from his followers is not a simple task. Such differentiation is not important to the points I make, however, so I will use 'Pythagoras' rather than 'the Pythagoreans' etc., and ignore the point.

[10] Or even from an interest in economic theory. Aristoxenus claimed Pythagoras' views about arithmetic to derive from an interest in finance and trade. (Guthrie *op. cit.* p. 221.)

[11] See, *e.g.* Guthrie *op. cit.* p. 208: '[Pythagoras] was traditionally supposed to have been the first to apply the name *kosmos* to the world, in recognition of the order that it displayed.' Guthrie (*ibid.* fn 1) gives a summary of the evidence for thinking this traditional supposition true.

It would be too much to pin the blame for the whole of the mystical/metaphysical tenor of Pythagorean thought on the simple fact that a ratio of 2:1 gives an octave. We might well see this simple fact, though, I am suggesting, as part of – and an exemplification of – the motivation for that particular tenor of thought. Take away any predisposition to see the world metaphysically in a Pythagorean *cosmic* sense, even, and there still seems to be something to explain, namely the fact that mathematics – *numbers* in this case – tell us about some aspect of the world that we had no reason to expect, *prima facie*, to be explicable or describable in such a simple numerical way. Are we surprised? Perhaps not, given our further knowledge of the physics of sound and wave mechanics. Did Pythagoras have a right to be surprised, given his knowledge? Yes. It seems he did.

1.3 A contemporary example

Here is an extract from Eugene Wigner's famous article on the unreasonable effectiveness of mathematics that I mentioned earlier:

> The … example is that of ordinary, elementary quantum mechanics. This originated when Max Born noticed that some rules of computation, given by Heisenberg, were formally identical with the rules of computation with matrices, established a long time before by mathematicians. Born, Jordan, and Heisenberg then proposed to replace by matrices the position and momentum variables of the equations of classical mechanics. They applied the rules of matrix mechanics to a few highly idealized problems and the results were quite satisfactory. However, there was, at that time, no rational evidence that their matrix mechanics would prove correct under more realistic conditions. Indeed, they say "if the mechanics as here proposed should already be correct in its essential traits." As a matter of fact, the first application of their mechanics to a realistic problem, that of the hydrogen atom, was given several months later, by Pauli. This application gave results in agreement with experience. This was satisfactory but still understandable because Heisenberg's rules of calculation were abstracted from problems which included the old theory of the hydrogen atom. The miracle occurred only when matrix mechanics, or a mathematically equivalent theory, was applied to problems for which Heisenberg's calculating rules were meaningless. Heisenberg's rules presupposed that the classical equations of motion had solutions with certain periodicity properties; and the equations of motion of the two electrons of the helium atom, or of the even greater number of electrons of heavier atoms, simply do not have these properties, so that Heisenberg's rules cannot be applied to these cases. Nevertheless, the calculation of the lowest energy level of helium, as carried out a few months ago by Kinoshita at Cornell and by Bazley at the Bureau of Standards, agrees with the experimental data within the accuracy of the observations, which is one part in ten million. Surely in this case we "got something out" of the equations that we did not put in. (Wigner 1960 p. 9)

That is quite a long extract, but it makes the point well. In just the same way as Pythagoras had no right to expect that the simple numerical ratios could be allied to harmonies of music, Wigner is claiming that *he* has no right to expect calculations with matrices to be allied to the energy levels of atoms. It may be that we can explain away Wigner's surprise here in terms of nothing more than an aspect of the problem of induction: matrices work *here* and *here*, but what reason do we have to think they will work *here* as well? Wigner, though, certainly thinks that there's something special about the fact that the inductively projected methodology is *mathematical*.

There is more than a concern with mathematics to unite Pythagoras and Wigner across the millennia. Where Pythagoras refers to the metaphysical thesis that *all things are numbers*, Wigner writes of what he calls *the empirical law of epistemology*, a thesis he puts forward to the effect that the laws of nature are best formulated in terms of *mathematically manipulable* concepts such as those of matrix algebra[12].

We have seen Pythagoras' motivation for his metaphysics to lie at least partly in his reaction to the fact of mathematical applicability as I have described. Wigner's *empirical law of epistemology* has the same kind of roots in a reaction to the fact that matrix algebra and other mathematics is so fruitful in describing and predicting aspects of the empirical world. The underlying notion, in both cases, seems to be that mathematics underlies the physical reality in some way, and that we should be surprised, if not amazed, that this should be so. We do not need to go along with the next step of either Pythagoras or Wigner to notice this similarity in their motivation.

The very fact of mathematical applicability in general, then, is certainly grist for the philosopher's mill. I want to turn to some more examples, now, to make some more specific points and pick up some more specific aspects of the kinds of *surprise* we've seen expressed by our chorus of physicists. First, I want to look at an example of surprise we saw expressed by Steven Weinberg above. Weinberg found it 'positively spooky', we recall, that mathematics somehow seemed to anticipate its own physical applications. I turn now to an example of this kind of 'spookiness'. The example is that of the development of complex numbers and applications thereof.

1.4 Renaissance history

First discussions of the possibility of using square roots of negative numbers in mathematics came in the sixteenth century – long before any genuine physical application of them was in the air. In Gerolamo Cardano's *Ars Magna* (1545), we find, for instance, an investigation into the 'impossible' problem of finding two numbers which sum to 10 yet whose product is 40:

> … Putting aside the mental tortures involved, multiply $5+\sqrt{-15}$ by $5-\sqrt{-15}$ … hence this product is 40 … So progresses arithmetic subtlety the end of which, as is said, is as refined as it is useless. (Cardano 1968 pp. 219-220)

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[12] See Wigner 1960 p. 10: '… the appropriateness and accuracy of the mathematical formulation of the laws of nature in terms of concepts chosen for their manipulability …'

Much of the impetus at this time towards considering complex numbers came from attempts to solve polynomial equations. It is notable that Cardano's own method for solving cubic equations could give rise to complex roots in its inner workings, although Cardano himself ignored these in spite of considering them in other problems, as we have just seen.

Rafael Bombelli, however, a contemporary of Cardano, did make use of complex roots. In Bombelli's *Algebra*, published in the year of his death (1572), we read

> I have found another kind of cube root of a compound expression very different from the other kinds, which results from the case of the cube equal to so many and a number … when the cube of the third of the things is greater than the square of the half of the number, the excess can be called neither plus nor minus. But I shall call it 'plus of minus' when it is to be added, and when it is to be subtracted I shall call it 'minus of minus' … (Bombelli 1966 pp. 133-134)

This refers to the problem of solving the equation $x^3 = cx + d$ for x: 'the cube [of x] equal to so many [of x] plus a number' using Cardano's rule that I mentioned above. Suitably translated into modern algebraic notation, this works as follows. To solve the equation $x^3 = cx + d$, first evaluate

$$u = \sqrt[3]{\tfrac{1}{2}d + \sqrt{(\tfrac{1}{2}d)^2 - (\tfrac{1}{3}c)^3}} \text{ and } v = \sqrt[3]{\tfrac{1}{2}d - \sqrt{(\tfrac{1}{2}d)^2 - (\tfrac{1}{3}c)^3}}$$

– The solution is then given by $x = u + v$.

It is now easy to see that when $(\tfrac{1}{3}c)^3 > (\tfrac{1}{2}d)^2$ ('the cube of the third of the things is greater than the square of the half of the number') the term inside the inner square root in each of the above formulae will be negative. Consider for example the equation $x^3 = 15x + 4$: Cardano's rule gives $u = \sqrt[3]{2 + \sqrt{-121}}$ and $v = \sqrt[3]{2 - \sqrt{-121}}$. Now, it is possible to find these cube roots in complex numbers: they are, respectively, $(2 + i)$ and $(2 - i)$ (where $i = \sqrt{-1}$). Adding, we obtain as solution of our original equation $x = 4$, which is indeed a root of $x^3 = 15x + 4$.

So Bombelli was aware that complex numbers could work in algebra to give correct results. There are few practical uses in this particular area, though, unless we have a decent method for finding cube roots of complex numbers in general. No such method was available to Bombelli or his contemporaries. Nevertheless we can see how striking it must have appeared to Bombelli that his 'plus of minus' and 'minus of minus' could be used in justification of Cardano's rule in cases where the complex cube root was not too difficult to find. Writing of the use of complex numbers[13], he says

[13] Of course Bombelli did not use this terminology. It makes sense to avoid the sixteenth century circumlocutions, though.

> This will seem to many to be more artificial than real, and I held the same opinion myself until I found the geometrical demonstration … (Bombelli 1966 p. 134) [14]

In any case, Bombelli was so struck by 'plus of minus' and so on that he went on to develop the basics of complex number algebra much as we know it today. Here is an extract:

> …first I shall deal with multiplication, setting down the rule of plus and minus.
>
> Plus times plus of minus makes plus of minus
>
> Minus times plus of minus makes minus of minus
>
> … …
>
> Minus of minus times minus of minus makes minus …(*ibid.*)

– That is, $(+1)(i) = +i$, $(-1)(i) = -i$ and so on. It is clear that here Bombelli is developing the structure of complex numbers as a consequence of the structure of mathematics (or simple algebra at least) extant at the time he was working.

Now, given the difficulties with solving equations of the type we have been considering (such the cubic with three real roots, which gives rise to square roots of negatives in Cardano's formula) – the *practical* impossibility, at the time, of finding cube roots of complex numbers – such equations were left to one side pending the discovery of different methods of solution, involving trigonometric methods rather than simple surds. Or, perhaps, we might even see the hiatus as due to a *psychological* impossibility instead of a practical one – 'imaginary' numbers, as we've seen, were considered *unreal* in one way or another. Here is another quotation from some time after Bombelli & co. Leibniz:

> The Divine Spirit found a sublime outlet in that wonder of analysis, that portent of the ideal world, that amphibian between being and not-being, which we call the imaginary root of negative unity. (Quoted in Kline 1972 p. 254)[15]

Complex numbers, then, were available to mathematicians from the sixteenth century onwards at least – their structure had begun to be studied – but the techniques of calculating with them were left on the shelf as far as physical applications are concerned. We might mention briefly some of the developments of theories involving complex numbers within mathematics over succeeding centuries: the proof that every polynomial equation has a root, real or complex (the *Fundamental Theorem of Algebra*, indeed); the link between complex numbers and vectors in the plane (Argand/Gauss, de Moivre); Euler's relation uniting the elementary functions ($e^{i\theta} = \cos\theta + i\sin\theta$). All of this is *intra* mathematics, though. We still do not seem to have any application in *physics*.

---

[14] I will not worry too much about the '*geometrical* demonstration' here: mathematics – even algebra – still needed geometric underpinning in sixteenth century Europe.

[15] Kline adds that 'Though Leibniz worked formally with complex numbers, he had no understanding of their nature.' I do not know what he (Kline) means by 'their nature'.

But, now, it turns out that complex numbers are useful in working theoretically with electric circuits. This is obviously a *different* application from that of solving equations. Think of alternating current, first, as modelled by a *wave*. Think of the wave as itself modelled in the obvious way by a trigonometric expression $I = a\sin\omega t + b\cos\omega t$ (*I:* current, *t*: time, the other letters constant real numbers). Now map *this* expression to the complex number *a* + *bi*. There are certain obvious equivalences: the structures are isomorphic for addition, for instance; the amplitude of the wave maps to the modulus of the complex number; and so on.

What makes the complex number representation most useful, however, is maybe not so apparent. Consider an inductor, though: any induced potential, physical theory tells us, is proportional to the rate of change of the current – to its (time) derivative. Now, given that $I = a\sin\omega t + b\cos\omega t$, $\dfrac{dI}{dt} = a\omega\cos\omega t - b\omega\sin\omega t$. But, mapping as before *I* to the complex number *a* + *bi*, we have that the derivative above maps to $\omega i(a+bi)$. Differentiation of the alternating current – giving its rate of change and hence the *inductance* in a circuit – is equivalent to the simple multiplication of the representing complex number by ωi. (ω is, more or less, the *period* of the current or the *wavelength* of the corresponding wave.)

There is more to it than just this, of course. However, it is easy to see how an easy multiplication in complex numbers might simplify the work involved in dealing with circuits, compared with the requirement to use methods from (differential, integral) calculus. This is a simplifying application, moreover, that Bombelli & co. could have had no inkling of, given the large development of physics and electronics from the sixteenth century to the present day. As such, it is about as good an example as there is of Weinberg's 'spooky'. The mathematician, in the person of Bombelli, was certainly 'there' with complex numbers, for all the 'mental torture' involved in dealing with these 'amphibian[s] between being and no-being' well before the physicist found them useful for simplifying work with alternating currents.

Complex numbers are not just useful for working with alternating electrical currents, of course. Quantum mechanics, our most successful physical theory, requires complex numbers for its very formulation. To quote Wigner's 'Unreasonable Effectiveness' paper again,

> There are two basic concepts in quantum mechanics: states and observables. The states are vectors in Hilbert space, the observables self-adjoint operators on these vectors. …
>
> … the Hilbert space of quantum mechanics is the complex Hilbert space … (*op. cit.* p. 7)

– That is, there is a requirement right at the foundational level of quantum mechanics for complex numbers as elements of the state-vectors. Wigner goes on to remark that

> … to the unpreoccupied mind, complex numbers are far from natural or simple and they cannot be suggested by physical observations. Furthermore, the use of complex

> numbers is in this case not a calculational trick of applied mathematics but comes close to being a necessity in the formulation of quantum mechanics. (*ibid.*)

Whether, or to what extent, the use of mathematics in general, or here complex numbers in particular, is actually 'a necessity' for science – whether mathematics is *indispensable* for modern science – is a large and much discussed question in contemporary philosophy of mathematics. I will return to the question of indispensability soon. For now, let it suffice that it is safe to say we have striking examples justifying Weinberg's notion that it is 'spooky' how particular parts of mathematics become available (often long) before the development of physics puts them to use. Cardano and Bombelli certainly could not have foreseen how the 'refined', 'useless' 'amphibian between being and non-being' square root of negative unity came to be useful in the way I have described. This is a further aspect of surprising mathematical applicability from that which, I suggested, motivated Pythagoras. But, once again, would Cardano, Bombelli & co. have had a right to be surprised at the use to which this part of mathematics was put? Yes. It seems they would. And some of us – Weinberg for one – *remain* surprised.

There is one further feature or genre of surprise expressed at mathematical applicability worth mentioning, something that makes the problem of accounting for applicability even more acute. This feature is exemplified by the 'amazement' expressed by Richard Feynman in the quotation from him above. The kind of 'prediction' to be found in mathematics which has, as Feynman says, 'nothing to do with the original thing', can be seen in an example given by Steiner in his *Applicability of Mathematics as a Philosophical Problem*. This is one of several illustrative and important examples Steiner offers.[16] The example I deal with at this stage, motivating further the status of mathematical applicability as requiring philosophical treatment by considering the *predictive* power of mathematics, is that of Paul Dirac, his eponymous equation, and the *a priori* prediction – in advance of later empirical discovery – of the existence of the positron.

1.5 Dirac's equation and the positron

In 1928, P.A.M. Dirac developed the equation that bears his name, giving a relativistic formulation of the quantum mechanics of the electron. His theory goes on to become the basis for quantum electrodynamics as developed, in particular, by Richard Feynman. I do not want to go that far, though: it will be enough here to see (in very general terms) how the Dirac equation accounts for electron *spin* and – the core of the surprise I am considering just at this point – predicts the existence of the positron. The positron was only observed later – in 1932, in fact, by Anderson. Is it surprising that Dirac's *mathematics* could predict the *existence* of a hitherto

[16] Steiner's examples are unfailingly suggestive and exciting. I deal with another of these in some detail towards the end of Chapter 5.

unthought-of particle? Many people have thought so.[17] To get to grips with the affair, I will go over some of the algebra involved in Dirac's derivation. This will be useful in clarifying the content of Dirac's equation and the surprise at what it predicts.

Dirac's original formulation of his eponymous equation was as follows:

$$\left(c\boldsymbol{\alpha}.\boldsymbol{P}+\beta mc^2\right)\psi=i\hbar\frac{\partial\psi}{\partial t},$$

where ψ is the wave-function for the electron and $\left(c\boldsymbol{\alpha}.\boldsymbol{P}+\beta mc^2\right)$, the relativistic quantum energy operator or *Hamiltonian*[18] (labelled $\boldsymbol{H}$ in what follows), is identified as the total energy of the particle.[19] I want to look at the derivation of this 'Dirac Hamiltonian'. How did Dirac come up with the formulation $\boldsymbol{H}=c\boldsymbol{\alpha}.\boldsymbol{P}+\beta mc^2$? Begin with energy in pre-quantum-mechanical physics:

According to Einstein, the classical (in the sense of relativistic, but non-quantum-mechanical) energy of a free particle of mass m and momentum $\mathbf{p}$ is given by

$$E=\sqrt{\mathbf{p}^2c^2+m^2c^4},$$

where c is the speed of light.[20]

Now, if we want to obtain the equivalent quantum mechanical equation, there is a conventional trick[21]. Given classical dynamical equations, as a standard text has it:

> … Corresponding equations in quantum mechanics are relations between operators, obtained by using the "correspondence principle": dynamical variables in the appropriate classical equations are replaced by the corresponding quantum mechanical operators …(McMurry 1994 p. 53)

Very well, then, applying this principle we might try to get the $\boldsymbol{H}$ in terms of the momentum operator $\boldsymbol{P}$[22], mass and speed of light as follows:

---

[17] Including, of course, Mark Steiner himself.

[18] Not quite the same as the classical Hamiltonian, but obviously related.

[19] $\hbar$ is the (reduced) Planck constant and i is of course the same square root of negative unity that we saw so troubled Bombelli, Leibniz and others.

[20] We might note in passing that if $\mathbf{p}=\mathbf{0}$ this equation reduces to the famous 'E = mc^2'.

[21] The success of this very 'trick' is just about as surprising – and as deserving of philosophical explanation – as the predictions Dirac's equation makes, a point Steiner also makes. It is certainly not easy to find an explanation of just why this 'trick' works. Textbooks tend to put it baldly, without justifications, and physicists of my acquaintance, once the question is raised, remain puzzled as to just why it works as it does. I have not the space here to deal with this in full; it is another example of a surprising feature of mathematical applicability, though.

$$\boldsymbol{H} = \sqrt{\boldsymbol{P}^2c^2 + m^2c^4}$$

That will not do though … we have no simple way of interpreting what we mean by the square root of an operator. (*Squaring* an operator amounts to iterating it; that seems fine, but the inverse process does not invariably – *prima facie* – offer itself.) We might just square through the equation to get

$$\boldsymbol{H}^2 = \boldsymbol{P}^2c^2 + m^2c^4,$$

which has become known as the 'Klein-Gordon' equation. In some ways, that *will* do. It gives the correct empirically-testable result for the energy as one of its solutions. There are problems with it, though. For one thing, it also gives rise to a *negative* energy solution. More about that soon. Another problem with it, in Dirac's eyes at any rate, is that it is second-order. Why is that a problem? Let us just say that he saw it as violating a formal feature of quantum mechanics[23]. At any rate, Dirac wanted to get a Hamiltonian that is linear in $\boldsymbol{P}$, but that was such that its solutions were also solutions of the Klein-Gordon equation above. So: he set out to find an $\boldsymbol{H}$ expressible in terms of $\boldsymbol{P}$ such that $\boldsymbol{H}^2$ is as required by Klein-Gordon. We might think of this as involving a 'factorisation' of the Klein-Gordon equation, which is how Steiner describes it, indeed (Steiner 1998 p. 159). Whether Dirac would have so described it I am not sure: in any case he came up with the idea of taking a Hamiltonian of the form

$$\boldsymbol{H} = c\boldsymbol{\alpha}.\boldsymbol{P} + \beta mc^2,$$

where $\boldsymbol{\alpha} = \begin{pmatrix} \alpha_x \\ \alpha_y \\ \alpha_z \end{pmatrix}$ and β are parameters to be determined by the condition on $\boldsymbol{H}^2$. We can do the algebra here:

$$\boldsymbol{H} = c\boldsymbol{\alpha}.\boldsymbol{P} + \beta mc^2$$

so

---

[22] $\boldsymbol{P} = -i\hbar\nabla$, or $-i\hbar\begin{pmatrix} \dfrac{\partial}{\partial x} \\ \dfrac{\partial}{\partial y} \\ \dfrac{\partial}{\partial z} \end{pmatrix}$. But we will not need *all* the technical details.

[23] It is by no means clear – perhaps was not even clear to Dirac himself – exactly *why* he took the steps he did. What primed his intuition is obviously relevant when we come to ask about how the mathematics he did had the (surprising) applications it had.

$$
\begin{aligned}
\boldsymbol{H}^2 &= c^2(\boldsymbol{\alpha}.\boldsymbol{P})^2 + mc^3\left[\beta(\boldsymbol{\alpha}.\boldsymbol{P}) + (\boldsymbol{\alpha}.\boldsymbol{P})\beta\right] + \beta^2 m^2 c^4 \\
&= c^2(\alpha_x P_x + \alpha_y P_y + \alpha_z P_z)^2 + mc^3\left[\beta(\boldsymbol{\alpha}.\boldsymbol{P}) + (\boldsymbol{\alpha}.\boldsymbol{P})\beta\right] + \beta^2 m^2 c^4 \\
&= c^2[\alpha_x{}^2 P_x{}^2 + \alpha_y{}^2 P_y{}^2 + \alpha_z{}^2 P_z{}^2 \\
&\qquad + (\alpha_x\alpha_y + \alpha_y\alpha_x)P_xP_y + (\alpha_x\alpha_z + \alpha_z\alpha_x)P_xP_z + (\alpha_y\alpha_z + \alpha_z\alpha_y)P_yP_z] \\
&\qquad + mc^3[\beta(\alpha_x P_x + \alpha_y P_y + \alpha_z P_z) + (\alpha_x P_x + \alpha_y P_y + \alpha_z P_z)\beta] + \beta^2 m^2 c^4
\end{aligned}
$$

Now we want

$$\boldsymbol{H}^2 = \boldsymbol{P}^2 c^2 + m^2 c^4$$

which we will have if the α's and β satisfy the following relations:

$$
\begin{aligned}
\alpha_x{}^2 = \alpha_y{}^2 &= \alpha_z{}^2 = 1 \\
\alpha_x\alpha_y + \alpha_y\alpha_x &= 0 \\
\alpha_x\alpha_z + \alpha_z\alpha_x &= 0 \\
\alpha_y\alpha_z + \alpha_z\alpha_y &= 0 \\
\beta\alpha_x + \alpha_x\beta &= 0 \\
\beta\alpha_y + \alpha_y\beta &= 0 \\
\beta\alpha_z + \alpha_z\beta &= 0
\end{aligned}
$$

and $\beta^2 = 1$

These conditions cannot be satisfied if the α's and β are to be numbers, it is plain. Number multiplication is commutative, after all[24]. *Matrix* multiplication is not commutative, however … and Dirac was able to find four *matrices* which satisfy the relations. In fact, it can be shown that the minimum size of matrices which can satisfy all these conditions[25] is 4×4.

Now, if the coordinates of the equation are 4×4 matrices, any *solutions*[26] must have four components. In fact, the solutions are *spinors* – similar to vectors[27]. Details of these spinors need not concern us. For our story what is important is just the fact that they have four components. Four seems to be three too many for just a single free particle. But – here is the surprise – each of the four components of the spinor gives specific physical information. And the three *extra* components give information

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[24] *I.e.* for any numbers *a* and *b*, *ab* = *ba*.

[25] As well as the anti-commutativity, in order to satisfy these conditions, the matrices need to have zero trace (the sum of the elements on their leading diagonals must be zero). The smallest such matrices are 4×4.

[26] The equation being a differential equation, these solutions are functions, of course – *wave* functions, indeed. Each of the four components is a wave function.

[27] But not invariant under rotations of a single turn (2π radians), though invariant under a rotation of 4π. There is a connection here with another example of Steiner's – that of representing the symmetries of the electron. See section 5.7, below.

about previously unknown physical phenomena. Physicists *now* say that the four components of the spinor give the probabilities[28] of the particle being an electron with *spin up*, an electron with *spin down*, a *positron* with *spin up* or a *positron* with *spin down*. The phenomena of *spin* – intrinsic magnetic moment – and the existence of *antimatter* – matter (such as the positron) with all the same properties as ordinary matter (like the electron) but with opposite charge – are unknown to classical mechanics and even to non-relativistic quantum mechanics[29].

Dirac took the notion of *spin* on board more or less straight away, concluding that electron spin results from combining quantum mechanics with relativity. The first two components of the solution to Dirac's equation, we might say, are thus taken care of. The second two took a while longer. It is clear that these components were connected somehow with the negative solutions of the Klein-Gordon equation that I mentioned above. *Negative energy* solutions? The idea that a positively-charged particle might behave sufficiently like a negatively-charged one with negative energy seems attractive. And positively-charged particles – protons – were around to fit that particular niche, perhaps? For a while the conventional view tended to agree with Hermann Weyl, that

> The solution of this difficulty [of accounting for the two *extra* components] would seem to lie in the direction of interpreting our four differential equations as including the proton as well as the electron. (Weyl 1950 p. 255)

That was a dead end, however. The proton's mass is much greater than that of the electron, for one thing, so it could not easily be made to fit. In any case, Dirac soon took seriously what his equation seemed to be (literally) saying, and tried to make sense of the existence of a particle more like a 'positively-charged electron' – a *positron*. He even tried to theorise about why positrons had not been observed, and was vindicated spectacularly by Anderson's discovery of the positron in 1932.

1.6 Steiner and homomorphism

In this latter example we can see particularly clearly how it might seem 'spooky' to Weinberg and 'amazing' to Feynman that 'the mathematician has been there before' the physicist and that mathematics 'makes it possible to predict' what will happen. It *does* look, if not mysterious, at the very least demanding of explanation, that mathematics, an abstract *a priori* discipline, can predict matters of real existence such as that of the positron, particularly in that the aspects of mathematics in

[28] Of course quantum mechanics deals in probabilities – the square magnitude of the wave function that (see above) comprises each component gives the appropriate probability.

[29] This oversimplifies a little. In fact *spin* was known about even pre-Schrödinger's equation. What is new with Dirac's equation (with due development from what I've described) is the interaction of spin with the electromagnetic field. Even the Klein-Gordon equation does not cope with spin in that sense.

question seem to have been developed, in this and so many other instances, without any concern for such particular applications in physics.

Now, we saw above something of Mark Steiner's claim of the pervasive influence of this demand for explanation in philosophy more generally. Steiner claims that

> To an unappreciated degree, the history of Western philosophy is the history of attempts to understand why mathematics is applicable to Nature, despite apparently good reasons to believe that it should not be. A cursory look at the great books of philosophy bears this out. (2005 p. 625)

Steiner instances Plato:

> Plato's *Republic* invokes the theory of "participation" to explain why, for instance, geometry is applicable … (*ibid.*)

Descartes:

> Descartes's *Meditations* invokes no less than God to explain why the ideas of "true and immutable essences" of mathematics (triangle, circle, etc.) that we grasp with our mind must represent existing entities in nature … (*ibid.*)

… and other philosophers, including Spinoza, Berkeley, Kant and Mill. (I mentioned Kant's *proper problem* in the Introduction.) Explanations involving Plato's 'participation' or the intervention of a Cartesian deity strike the contemporary mind as unsatisfactory in their non-naturalism,[30] but it is at least indicative of the seriousness of the problem of mathematical applicability that such appeals to non-naturalism can seem at all plausible.

Steiner himself explicitly draws a non-naturalist moral from the fact of certain kinds of mathematical applicability. His book (1998), he says, 'challenges naturalism' (p. 10 and *passim*), and while he does not go the whole hog with Descartes in laying the blame squarely and uniquely on God's shoulders, his sympathies clearly lie explicitly with those who would give a theological account of mathematical applicability. The challenge to naturalism, he says, '… makes the book consistent with natural theology,' something he certainly takes as positive, even if, as he says, '… there are many positions available that are neither natural nor theological.' (*ibid.*)

I take it as a basic methodological principle of contemporary philosophy that recourse to 'God making it so' ought to be at best a last resort. This being so, that a

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[30] Here as earlier, I leave the idea of 'naturalism' rather vague. As David Papineau has written, 'The term 'naturalism' has no very precise meaning in contemporary philosophy.' (2009; see also his 1993, p. 1, '… there is little consensus on its [*sc.* 'naturalism's] meaning.') We can still use the term, in spite of this imprecision; as Papineau goes on to say, 'The great majority of contemporary philosophers would happily accept naturalism as just characterized [*sc.* as the idea '…that reality is exhausted by nature, containing nothing 'supernatural'…'] (2009) – that is, they would at the very least reject 'supernatural' entities. Steiner's brand of non-naturalism amounts to a challenge to this rejection of the supernatural, as his remarks about theology make clear.

doyen of philosophical work on the problem to hand such as Mark Steiner should conclude with the particular genre of non-natural solution he does is more evidence of the *prima facie* intractability of the problem. In spite of the difference in outlook implicit in Steiner's approach, however, I want to stick with him a little further, firstly for help with a specification of the most important aspects of the problem and secondly for help with characterising its genesis. The genesis of the problem as characterised by Steiner will give us a useful pointer towards an important aspect of its manner of possible solution.

I have written of the problem of mathematical applicability in the singular. In fact Steiner finds several *different* problems of mathematical applicability. In his 1998 he distinguishes the *semantic* and the *descriptive* senses of applicability (*e.g.* p.47), and in his 2005 he moves to a characterisation somewhat overlapping this distinction, in terms of what he calls *canonical* and *non-canonical* applications of mathematics. An application of a mathematical theory is *canonical* if the theory was developed in the first place to describe the application, and *non-canonical* otherwise. This distinction in Steiner's hands further resolves into applications that are empirical and those that are not, making a total, as he says, of four different kinds of mathematical applicability; canonical empirical, canonical non-empirical, non-canonical empirical, and non-canonical non-empirical. It is a moot point whether it is best to talk of several different problems or of several different aspects of one overarching problem. In any case, the most interesting and difficult cases – such as we've seen in our example of Dirac's equation – belong to a single sub-category, that of the non-canonical empirical in Steiner's later taxonomy, or of the descriptive if we adopt his earlier distinction.

I intend to use another of Steiner's examples in a later chapter.[31] This comes from his most recent (2009) paper, which takes on Wittgenstein's philosophy of mathematics. In this paper Steiner allows that Wittgenstein's view of mathematics can cope with cases of his, Steiner's, 'canonical' sort of mathematical applicability. However, the example exemplifies, he claims, the manner in which

> Wittgenstein's account of mathematical application was seriously lacking. (Steiner 2009 p. 26)

– Seriously lacking, that is, in that it fails to account for

> The tendency of mathematics to have non-canonical applications, applications of which the 'inventors' of the mathematics could not have dreamed. (*op. cit.* p. 24)

This 'tendency', it is clear, relates to the kinds of mathematical applicability I have been discussing as examples of Weinberg's 'spooky' and Feynman's 'amazing'; cases in which 'the mathematician has been there before' the physicist or where mathematics 'makes it possible to predict' aspects of physical reality or existence in advance of the development of related physical theory. This – non-canonical empirical applicability of mathematics in Steiner's taxonomy – is clearly where the

[31] See section 5.7 below

hard cases lie. Without belittling the canonical (I will make some brief remarks later about how what Steiner calls Wittgenstein's 'account' of mathematics deals successfully with canonical cases, as Steiner claims), nevertheless we should allow that Steiner's non-canonical empirical requires our full attention.

In the light of this, I will continue to talk of *the* problem of applicability.

Steiner goes on to say,

> To be fair [to Wittgenstein], however, if there is something missing in Wittgenstein's philosophy, it is missing in all the others as well. (*op. cit.* p. 26)

The non-canonical empirical kind of application of mathematics, claims Steiner, remains mysterious, to be explained if at all only in non-natural, possibly theological terms. I am going to claim that we can do better. I will return to the notion of 'something missing' in Wittgenstein's account in chapter 5, after we have garnered sufficient resources to challenge Steiner's claim.

To begin garnering these resources, I want to take something else from Steiner; his summary and original characterisation of the problem of mathematical applicability in his 2005. We have seen above Steiner's claim that the history of Western philosophy is intimately linked with the history of attempts to deal with the problem of mathematical applicability. What all these attempts share, he goes on to say, is a particular view of what that problem amounts to. Following his claim of the ubiquity of the problem as generative of the doctrines of (essentially *all*) major philosophers, he goes on to say

> What is more, we conclude that (though they didn't use the word "apply") all the doctrines presupposed the same *concept* of application: they all assumed that application is a relation (some approximation to a homomorphism) between mathematical theorems and empirical facts, a relation which can be used to "read off" empirical facts from mathematical theorems. The question they ask is: Given the nature of mathematics, why should such a homomorphism exist? (Steiner 2005 p. 627)

This summary characterisation is a useful first step. I take it to be clear how it relates to our examples and the general problem of mathematical applicability. The notion of a *homomorphism* here is key[32]. Furthermore, it is a notion which is also at the centre of what Michael Friedman described as

> The first (recent) serious study of what we might call the philosophy of *applied* mathematics: the use of mathematics in empirical theories. (Friedman 1981 p. 505)

– That is, Hartry Field's much discussed and influential monograph *Science Without Numbers*. It is true that much of the discussion of Field's work has focussed away from particular issues of applicability and on to his fictionalist nominalism – his view that strictly speaking there are no mathematical entities and that mathematical

[32] A homomorphism, roughly speaking, is a structure-preserving map or function. For a little more detail, see Appendix 1.

propositions are but useful fictions. But I want us to take seriously Friedman's injunction that

> Future discussions of this area [of our understanding of applied mathematics] must take up where Field leaves off. (*op. cit.* p. 506)

Friedman describes the use of 'representation theorems' as Field's 'main theoretical device'. A representation theorem asserts the existence of a 'representing' homomorphism connecting physics and mathematics. Agreeing with both Steiner and Friedman, then, concerning the centrality of the notion of homomorphisms in setting up and attempting to deal with the problem of mathematical applicability, I turn in Chapter 2 to a consideration of how Hartry Field wields his 'main theoretical device' to deal with (what he himself describes as) 'the really fundamental' question in the philosophy of mathematics.

Chapter 2 Fictionalism, Applicability, and Face Value

2.1 Nominalism and indispensability

Field argues for a type of *nominalism*, which he takes as the general view that abstract entities do not exist[33]. The particular brand of nominalism he espouses is *fictionalism* about mathematics, roughly the notion that abstract mathematical objects are ontologically similar to, 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)[34]

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 in general and mathematical Platonism in particular,[35] it is up to the Platonist to come up with the good arguments. And, he says,

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[33] There is a clear overlap with traditional *nominalism* denying the independent existence of universals, a usage with a long history. Field's use of the term is not exactly this. His usage is fairly conventional in contemporary philosophy of mathematics, though: I will adopt it here.

[34] There may be some cavil regarding talk of the truth of *sentences* rather than of *what sentences express*, or perhaps of *propositions*. Field makes nothing of this; no more will I.

[35] I discuss one of the most influential contemporary aspects of Platonism's difficulties – characterised by Paul Benacerraf – a little later in this chapter.

> 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)[36]

– 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 that 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)[37]

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[36] This is clearly reminiscent of *TLP* §6.211 as we will see below; there are also some close connections between this discarded view of Putnam's and those of the later Wittgenstein *and*, perhaps surprisingly, (certain aspects of) Field's own.

[37] Elsewhere (Field 1989 p. 228 *fn 2*), Field claims that *this* kind of anti-realism is 'not only too obscure to assert, but also too obscure to deny.' The attitude to idealism he expresses here is interestingly similar to what I will argue is an appropriate attitude to his fictionalism, as we will see, although he does not argue the case in any way similarly.

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 (such as – or perhaps 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)

I intend to look with some care at Field’s notion of ‘taking mathematics at face value’. That comes later in this chapter. For the present I continue with an explanation of Field’s account of applicability.

2.2 Conservativeness

We need to 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 I want to focus on. For my 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 himself implicitly 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.[38] Field claims that mathematics has the possibility of being good so long as it is conservative; conservatism thus is a replacement for mathematical truth.

Why is mathematics conservative? According Field the general answer hinges on the modal and/or epistemological status of mathematics.[39] 'And/or' 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 more detail below in Chapter 4. Here I simply state the argument in modal terms, leaving the epistemology to take care of itself.[40]

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[41] is false. But mathematics (as most of us would agree) is necessarily true[42]. So mathematics must be conservative.

In case the 'necessarily's and 'possible's are difficult to track, here is the argument again in *possible-worldish*[43]: 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

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[38] Field makes this more formal and precise (see Field 1980 P. 11 *ff*), but I see no need to be particularly formal here. The general idea is what we need.

[39] Field outlines two more formal proofs of the conservativeness of mathematics in an appendix to the main development of his (1980), one semantic (model-theoretic), the other proof-theoretic. Again, the main development is what we need, though.

[40] Some have conflated necessity with *a priority*, of course. For my purposes nothing much hangs on the (il)legitimacy of such a move, as I hope will be clear. I will consider the modal status of mathematics in Chapter 4.

[41] Or some relevant part thereof.

[42] When true, or necessarily false when false, of course. I will leave such details tacit from now on.

[43] I am not trying to be formal with possible world semantics for modal logic here. This is just a useful heuristic device.

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*[44] 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

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44 In the Lockean sense; 'to press a man with consequences drawn from his own principles'; see John Locke 1689, IV, xviii 21.

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 that Field cannot supply.[45]

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 mathematicalia, we have no reason to think the actual world is one such.

This brief look at Field's argument for conservativeness, then, 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 the difficulties in accounting for *a priori* knowledge that is at the same time synthetic that I have already canvassed. 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.[46] 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*.

2.3 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 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

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[45] See Hale 1987; Hale and Wright 1992; Field 1989 and 1993; and Hale and Wright 1994 for the beginnings of this controversy and some developments.
[46] Especially in Chapter 4 below.

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[47] 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. (Our reasons for doing this are just what is germane at this point, as well as being more-or-less obvious: 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. 24-25)

'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 *iso*morphism. (An isomorphism is a homomorphism with the added property of being a one-one correspondence. A homomorphism may be many-one in that it may map several elements to the same element.) 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

[47] Talk of 'finding mathematical objects' will raise some hackles of course. I do not want to go into the metaphysics *or* the epistemology just now, though. I am setting out the view here with the intention of raising questions later.

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 – that 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 that are 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*[48], 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

[48] As Friedman does for instance, in his review (1981) of Field 1980. He misses the points about the more general notion, whether deliberately or not I do not know.

least one such theorem: Kranz *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 ≥ ("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 $\succsim$ is a binary relation defined on A. A definition:

The relational structure $< A, \succsim >$ is said to be a ***weak order*** iff for all a, b and c from the set A, the following conditions hold:

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

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 $< A, \succsim >$ 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, $a \succsim b$ iff $\phi(a) \geq \phi(b)$. (That is to say, of course, that ϕ is a representing homomorphism.)[49]

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 $a \succsim b$ iff $\phi(a) \geq \phi(b)$, 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

[49] For a proof of this theorem, see *Appendix 2*.

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.[50] 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?'

2.4 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 mathematicalia 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[51] – does not have its status at all put in question by considerations of the ontology of *groups* or their elements. We might well

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[50] There are other less subjective non-transitive aspects of the world; consider for instance Condorcet's (voting) paradox or the case of non-transitive dice. (Condorcet's paradox is well known. As for dice: we can number three dice A, B and C so that A will probably beat (*i.e.* score more highly when rolled than) B, B will probably beat C, and C will probably beat A. Some people find this surprising.) Investigating further here would take us too far from our main theme.

[51] For some details of this, see *Appendix 1*.

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.[52] 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,[53] the other nothing at all, or at least nothing that exists. So there is 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

[52] This is itself not as clear cut as it might seem. There is a sense, made much of by Wittgenstein, in which the meaning of a mathematical theorem fails to be independent of its manner of proof. Although I do not deal explicitly with this in exactly these terms, my hope is that my next chapter will go some way towards clarifying such matters.

[53] Including such things as bosons and quarks in the category of 'common or garden' objects might seem 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 conservatism: the existence of quarks is *not* conservative over physics in the way the existence of numbers is. For now, though, just let the difference between concrete and abstract carry the weight.

philosophical problem of mathematical applicability in the work of philosophers through the ages. I want now to begin to put this sense in question.[54]

2.5 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. Clearly 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

> Indeed in real life a mathematical proposition is never what we want. Rather, we make use of mathematical propositions only in inferences from propositions that do not belong to mathematics to others that likewise do not belong to mathematics. (*TLP* §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'.[55] 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.[56]

Now, we should contrast the way in which each of Field and Wittgenstein deals with the notion of mathematics as inference-facilitating. Wittgenstein and Field 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

[54] Although the *coup de grâce*, so to speak, will not be administered until Chapter 5.
[55] See, *e.g.*, Wittgenstein 1978, P. 99: 'The mathematical proposition has the dignity of a rule.' Of course this motivates what has become known as Wittgenstein's '*rule-following considerations*' as much in the philosophy of mathematics as in the philosophy of mind. These considerations have generated a plethora of exegetical and consequential controversy that I do not intend to add to explicitly. I will be dealing with some matters arising, though, in Chapter 4.
[56] 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 below.

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.

2.6 Semantic uniformity: taking mathematics at face value

2.6.1 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, I want to 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'[57]. In the current *Oxford Readings* on the philosophy of mathematics, for instance, W.D. Hart claims 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.[58] 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.

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[57] Reprinted, for example, in Hart 1996.

[58] Benacerraf: '… 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*.' (*op. cit.* P. 22)

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. Mark Balaguer, for instance, characterises the importance of Benacerraf's dilemma independently of causal epistemology nevertheless as 'the *epistemological* argument against Platonism.'(Balaguer 2011) Or, one more recent example to make the point; 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 on this latter, 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.[59]

[59] We may be reminded here of the so-called *Frege-Geach problem*. If we are, we may also be reminded of the way in which problems (and their solutions) in philosophy of mathematics often have close analogues in ethical and moral philosophy. It would be interesting to work through a response to Geach's objection to ethical expressivism using the tools I describe Wittgenstein as fashioning here.

2.6.2 Challenging semantic homogeneity 1: 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 I start looking at the mathematical case, I want to prepare such a case by looking at an issue in philosophy of mind, largely 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. Whether it is or is not a formal contradiction is not important.[60] 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*)[61]

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

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[60] 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; 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. See section 2.6.3 and section 2.11.

[61] Or *Philosophy of Psychology: A Fragment xi* in Wittgenstein 2009. Only the title differs.

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.

2.6.3 Challenging semantic homogeneity 2: taking mathematics at face value

I am not going to get into a detailed treatment of Moore's Paradox.[62] But I hope to have sowed some seeds of doubt about the homogeneity requirement. With this in mind, I will 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*[63], he 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)[64]

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* physical object[65], and the abstract – 'the direction of line c' refers to an *abstract* object. This is what I want to challenge.

2.7 Frege, Wright and Field

This takes us back to Frege in *Foundations of Arithmetic* 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. I want 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

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[62] Wittgenstein offers such a – convincing – treatment. Jane Heal gives a good account; see her 1994.

[63] Reprinted as *Platonism for cheap?* in Field 1989 pp. 147*ff*.

[64] The example, as well as the point it illustrates, is Frege's, of course. See *Foundations of Arithmetic*, §64*ff*, especially §66, '… the direction of *a* appears as an object'.

[65] *Are* lines *bona fide* concrete objects? The question is moot, as likewise are related questions about what Field is to be allowed to count as 'nominalistically kosher' within his self-imposed task of 'nominalising' different parts of science. That Field takes lines to be physical entities is clear: see for instance Field 1980 pp. 31-32, where he writes at length of 'physical' lines. Whether he is entitled to such talk within his overall project is not a question that needs answering here, however: surely *some* reduction of direction-talk to the concrete will be possible.

controversy between Wright's 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 *Foundations of Arithmetic*. 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 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[66] 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:

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[66] And Frege's? Doubts have been expressed about Frege's own commitment to Platonism, by Joan Weiner for instance (in her 1990 and elsewhere). It might also be worth mentioning in this regard Erich Reck's characterisation of Frege as '*a contextual, not a metaphysical platonist*' on account of the dependence of his account on the context principle. See Reck 1997. To what extent we should read Frege as an out-and-out realist about mathematics is an interesting and subtle matter that unfortunately I have not the space to go into here.

> 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 (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)[67]

We have seen the beginnings of an attack on this above, 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[68], 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

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[67] See also Wright *op. cit.*p. 50. Field's expression mirrors Wright's except that Field has 'meaning or truth conditions' where Wright has just 'meanings'.

[68] I am not going to make anything here of questions about the appropriateness of ascribing truth to sentences, or concerning the appropriateness of talk of 'truth conditions' in this way. I will try to make something of such questions, in a general way, below, but for now I am just going along with Wright.

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:

> … that 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[69], 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 the familiar device of numerical quantifiers and so avoid reference to a number.[70] 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

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[69] Field is tentative in his attribution of the details of this argument to Wright. He finds the content of (S) unclear, for instance (*op. cit*. P. 155). It is the overall thrust of Field’s critique, though, that is important for us.

[70] In case it is not clear, numerical quantifiers are expressible in first-order logic with identity without any use of number terms. Of course not *every* mathematical statement allows this kind of nominalistic reduction. I will not go into that here.

– 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[71], 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)

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. (Wright 1983 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

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[71] We will recall Field’s nominalism is *contingent*.

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'[72] 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 Benacerraf's dilemma. It is time to begin to consider the other horn of this dilemma.

2.7 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

[72] Note the scare quotes here – but note also that they are there in the original quotation!

in the way Frege was or tried to be (or the Scottish neo-Fregeans are). He 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. This is a point he takes from Kant[73]. 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.)[74]

Of course the idea that logic has no existential commitments – that 'logic cannot yield us any objects' – might be challenged.[75] 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

[73] This is not to say that Field follows Kant entirely in his view of logic and its relationship to mathematics. The important aspect that he *does* follow regards this lack of existential commitment.
[74] It is worth emphasising that this comes late in Frege's life and in particular after he had tried – and failed – to solve the difficulties in his logic illuminated by Russell's Paradox. Earlier, he had thought that logic *could* 'yield us [arithmetical] objects'. Crispin Wright's *Frege's Conception* … tries to rehabilitate Frege's earlier view, or at least whatever of it *can* be rehabilitated in Wright's eyes. We might also qualify the later Frege's apparent blanket denial of logical existential commitment by considering his view of the existence of *concepts* as well as (or, perhaps we should say, apart from) objects.
[75] By Bob Hale, for one. See, *e.g.* his 1987.

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 as '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 to the object language.

Thus, instead of taking the knowledge involved in (i) to be *meta*logical, Field takes it to be *modal*. 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. Thus, what I know is what is expressed by the object language sentence "$\Box(A \supset B)$", where the '$\Box$' 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 (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're 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[76]:

> $\Diamond$B is true in M if and only if B is true in M^* for some model M^*. (Field 1989 p. 118)[77]

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 it by mathematical applicability.[78]

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 without ascending to a metalevel at all. However, the semantics of Field's modality seems to reinstate the requirement for some of those very mathematicalia 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.[79] 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,

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[76] This oversimplifies, but not in any misleading way.
[77] The – slightly longer – statement involving formulae other than sentences is there too. We lose nothing important by keeping to the simplified version.
[78] We have been here before with Field, of course. We might 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.
[79] There is no claim in play here that model theory is only useful for purposes such as Field's, of course. An account of its utility may well be required independently, and Field often writes in such terms. Once again (see previous footnote) it is interesting to mirror the requirement for an account of the applicability of metalogic in Field's own account of the applicability of mathematics.

> 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.

Going into fine detail is not necessary. Some examples will serve to get us the major points. 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 -$\Diamond$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 -$\Diamond$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.*) [where '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.

2.8 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[80] 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 the

[80] Again, recall Field's *contingent* nominalism.

(rule-expressing) mathematical propositions. This is *prima facie* decidedly odd – something that Field himself admits, as we will see shortly. I am going to suggest 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'[81]). 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.

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[81] See, for instance, Putnam 1979 pp. 20*ff*: ' 'If-thenism' as a philosophy of mathematics'. Bertrand Russell held to this for a while.

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 and lines *etc.*, taken in a mathematical sense[82], 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.

2.9 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

[82] Of course Field allows that there are lines and so on in a *physical* sense. It is just their mathematical counterparts that are fictional.

> 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. (*PI* §293)

There are clear differences between the cases of our talk of objects of sensation and of mathematical objects. However, I am claiming a similar 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.[83]

Field's abstract objects, which he claims do not exist, are 'beetles in boxes'. We have seen his involvement with these (self-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 I want to raise.

I want to 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, I suggest 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 to espouse.[84] 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.

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[83] Even if they *did* exist, these abstract objects would still be irrelevant to the *use* we might make of mathematics. Platonic objects themselves 'drop out of consideration as irrelevant' – are 'beetles in boxes' – just as much for the Platonist as for the fictionalist.

[84] And Wright, and, it must be admitted, a large majority of contemporary Anglophone philosophers.

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. (*PI* §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*.

2.10 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, in some sense, 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. 122-123)

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:

> Professor Hardy is comparing mathematical propositions to propositions of physics. This comparison is extremely misleading. (*LFM* p. 240)[85]

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)

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[85] Wittgenstein is responding at this point to another expression by Hardy of his mathematical Platonism. Wittgenstein misquotes Hardy, but the point is anyway clear.

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 that I am suggesting Field misses concerns the force of this 'about': what is it for mathematics to be 'about' numbers and so on? It's at this point that 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 *PI* §10*ff* 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.

2.11 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, in his 2011, 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.

'This sort of challenge' is what I have suggested Wittgenstein makes in *PI* §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.[86]) What we have from Wittgenstein is to be taken as suggesting or reminding us of a way of looking

[86] To pick an example more or less at random, see, for instance, Williamson, 1996.

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.

Nevertheless, 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 *un*importance 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 this I turn to in the next chapter.

Chapter 3 Infinity and Concept-determination

3.1 Face value and concept-determination

We saw in the previous chapter how Hartry Field's characterisation of a 'face value thesis' allied itself with the (Benacerrafian) requirement for a semantics homogeneous over mathematics and physics to generate the notion of mathematics being 'about some postulated realm of mathematical entities'. The implicit parallelism between such a 'mathematical realm' and the physical realm – expressed by G.H. Hardy in terms of two different types of 'reality', the physical and the mathematical, was something we saw Wittgenstein describing as 'extremely misleading'. Now I want to continue the investigation into what it is to 'take mathematics at face value' by looking at another aspect of what Wittgenstein suggests to us as the aetiology of philosophical error regarding the 'reality' of mathematics:

> The dangerous, deceptive thing about the idea: " The real numbers cannot be arranged in a series", or again " The set... is not denumerable" is that it makes the determination of a concept – concept formation – look like a fact of nature. (*RFM* p. 131).

Here Wittgenstein is referring to some specifics of the development of the notion of *infinity* in mathematics.[87] It is going to be useful for us to investigate this notion itself as well as Wittgenstein's particular strictures thereon. Both of these will be of interest as a further pointer towards what we have found important in 'taking at face value' – just what *is* it to take the notion of infinity at face value as part of mathematics? Also, we will find, a consideration of the distinction Wittgenstein makes in the quote above between 'determination of a concept' and 'a fact of nature' will elucidate and emphasise what may mislead us concerning the relationship between mathematics and mathematical reality, physics and physical reality, and mathematics and physics.

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[87] Notions of infinity have always been important for distinguishing different positions in the philosophy of mathematics as well as in generating philosophical problems in their own right. See, for instance, Brown, 2008, p. 70; 'Historically, paradoxes and conceptual problems of mathematics have usually stemmed from the infinite.' Most introductions to the philosophy of mathematics say something similar; I take the point as read. The current chapter considers the notion of infinity both in its own right and as exemplifying more general aspects of the philosophy of mathematics relating to our concerns about mathematical applicability.

3.2 Painless toothache and unconscious desires

I am going to consider some aspects of the historical development of the concept of infinity in mathematics in the light of Wittgenstein's distinction between concept determination and discovery of facts. First, though, to help get a clearer grasp on the distinction itself, here is an example Wittgenstein offers in a *different* context from that of mathematical infinity:

> … examine the following example: It might be found practical to call a certain state of decay in a tooth, not accompanied by what we commonly call toothache, "unconscious toothache" and to use in such a case the expression that we have toothache, but don't know it. It is in just this sense that psychoanalysis talks of unconscious thoughts, acts of volition, etc. Now is it wrong in this sense to say that I have toothache but don't know it? There is nothing wrong about it, as it is just a new terminology and can at any time be retranslated into ordinary language. (*BB* pp. 22-23)

This example occurs in the context of a discussion of intentional states. There is a general moral to take from it. It is not at all implausible that this 'new terminology' should take hold, as it were, and become part of everyday usage. ('For', as Wittgenstein asks, 'What more can you ask of your notation than that it should distinguish between a bad tooth which doesn't give you toothache and one which does?' (*ibid.*) Suppose it does take hold, then. Suppose people to have learned to use such talk unthinkingly, along with the rest of our talk of toothache and pains in general. In such a situation we might well be puzzled by the expression ('the puzzlement of philosophy', as Wittgenstein describes it (*ibid.*), and ask 'How is unconscious toothache possible?' Wittgenstein imagines a scientist explaining, '… like a man who is destroying a common prejudice' (*ibid.*), that of course there is such a thing as unconscious toothache. There may even be a tendency to consider the existence of unconscious toothache as a *discovery*, much as, for instance, Freud felt able to talk of the discovery of unconscious desires and so on.

Now, *did* Freud discover the unconscious, or rather did he offer us a new way of thinking about the world of our desires and other psychological states? The matter is perhaps moot. One thing that seems clear, though, is that we have found the notion of unconscious desire to be a useful one in all sorts of ways. Given such utility, whether we talk in terms of Freud 'discovering' unconscious desire or not is largely irrelevant.

This might seem strange in the light of philosophical issues to do with, say, the *reality* of mental states like desires. 'Are there *really* such things as unconscious mental states?' we might imagine a philosopher asking …, 'Or is talk of the Freudian unconscious just a *façon de parler*?' Consider, though, Wittgenstein's example of unconscious toothache, particularly in the light of the discussion in the previous chapter about how, or whether, numerals refer to numbers and so on. There, we found it immaterial whether we allowed that numerals 'refer' to numbers or not, once we realised that such a locution made no difference to the use of numerals in everyday practice. Such a realisation, we found, could be achieved by a focus on the different uses to which different kinds of words are put. In terms of 'taking at face value', this point arose once we considered, not the question of whether we should take talk of reference and so on at face value, but rather the prior question of what it might *amount to* to take such talk at face value.

Now likewise, consider talk of whether unconscious toothache is really, properly so-called, *toothache* or not in Wittgenstein's example. We might well gloss such a consideration in terms of whether we should take such talk 'at face value'. But by now we have learned to be a little wary of such a way of putting the matter. Indeed, we might say, we *do* want to take talk of unconscious toothache at face value – but our analysis has taught us that to do so is not necessarily to take it that, in the circumstances envisaged, unconscious toothache would have the same status as other scientific discoveries. There is nothing that debars us from claiming, in such circumstances, that unconscious toothache really *is* a kind of toothache – nor indeed even from claiming that it had been *discovered* to be so. Likewise, we might say similar things about Freudian unconscious desires and so on. Such talk might mislead, perhaps; but we will *not* be misled – or at the very least, I am claiming, we are much less likely to be so misled – if we are aware of the differences that there are between the different kinds of toothache (or desire), and so long as we keep in our minds the difference between determining a concept and discovering a matter of fact.

This might seem to have something of an air of paradox. Does talk of unconscious toothache *really* depend on a discovery or is it *really* a case of concept-formation? (Is the distinction between discovery and concept-formation itself a discovery or a determination of a concept?) I may seem to be claiming both. The point is, however, that the distinction, once made, can be seen itself to be irrelevant for our *use* of the concept in question. And further, the distinction allows us to see more clearly how an unthinking reliance on the notion of taking things at face value may mislead. What is in question, once again, it what it *amounts to* to take talk of unconscious toothache at face value. Once this question is raised in this way, we can now see its answer as itself irrelevant to our concerns, even as by contrast we are pushed to acknowledge the importance of the *utility* of our talk – not as a determinant of the reality of the toothache, or even of the distinction itself, but rather just as a determinant of whether it 'catches on' – whether it gets used or not.

More than this, though, we can see the importance of this notion of how or whether a concept is used by contrast to the unimportance of traditional philosophical questions about whether numerals *really* refer to numbers and whether there *really is* a mathematical realm that mathematics is *about* just as there is a physical realm that physics is about … or, by the same stroke, whether, in Wittgenstein's example, unconscious toothache is *really* toothache (or whether unconscious desires are *really* genuine desires and so on). Our considerations are leading us, willy-nilly to an appreciation of how such questions may be empty of real content.[88]

With this in mind, now I move to a consideration of mathematical infinity. What is infinity, *really*? Or, given that we want to take the notion of infinity at face value, what will such taking at face value amount to? We will be wary of any such questions, as of questions about the mathematical realm that infinite numbers, if such there be, inhabit. An investigation of rival characterisations of the notion of infinity in mathematics, though, will help us avoid being misled by such claims as Hardy's about mathematical reality or Field's about a postulated realm.

3.3 Catching-on to infinity (1): ∞

I mentioned above the notion of whether a particular manner in which a concept is determined 'catches on'. I am going to look now at a particular determination that did *not* catch on. It is a notion of infinity developed by John Wallis in his *Arithmetica Infinitorum* of 1655.[89] I will explain his argument using updated notation. It is simple enough.

Consider, first, taking a positive number, $^{+}1$, let us say for definiteness, and dividing it successively by ever decreasing numbers. Begin by dividing by a large number:

…	1 ÷ 1 000 000	=	0.000 001;
	1 ÷ 1 000	=	0.001;
	1 ÷ 10	=	0.1;
	1 ÷ 5	=	0.2;
	1 ÷ 2	=	0.5;
	1 ÷ 1	=	1.0;
	1 ÷ 0.5	=	2.0;
…			

[88] Although, as above with the reference to Wittgenstein's distinction applying to itself, this appreciation iterates on the emptiness. Is it *really* empty? Well …That is the point here, really.

[89] Wallis was Savilian Professor of geometry at Oxford, a contemporary of and influence on Isaac Newton (Newton was Lucasian Professor at Cambridge partly contemporaneously with Wallis at Oxford.) According to Boyer and Merzbach, for example (1989 p. 437), 'Young Newton … studied … perhaps most important of all, Wallis' *Arithmetica infinitorum*.' For further references to the argument I give in the main text, see, for example, Romig 1924, pp. 387-389; Millar 1925 pp. 153*ff*. It is worth making the point that Wallis was far from being an eccentric – he was plumb in the mainstream of mathematical development, no more eccentric than any other mathematician of repute. His proof concerning the relevant size of negative numbers and infinity has largely been forgotten in spite of this.

… and so on, reducing the divisor all the time. Of course the quotients get larger as the divisors gets smaller. Now continue in this vein:

$1 \div 0.2 \quad = 5;$
$1 \div 0.1 \quad = 10;$
$1 \div 0.01 \quad = 100;$
$1 \div 0.001 \quad = 1000;$

…

And so on. It is clear that we can write, in order,

$$\ldots \frac{1}{10} < \frac{1}{5} < \frac{1}{2} < \frac{1}{1} < \frac{1}{0.5} < \frac{1}{0.2} < \frac{1}{0.1} < \frac{1}{0.01} < \frac{1}{0.001} < \ldots$$

Once again, as we decrease the divisor the quotient increases. So, continuing, we will have

$$\ldots \frac{1}{10} < \frac{1}{5} < \frac{1}{2} < \frac{1}{1} < \frac{1}{0.5} < \frac{1}{0.2} < \frac{1}{0.1} < \frac{1}{0.01} < \frac{1}{0.001} < \ldots < \frac{1}{0} < \ldots$$

Zero is less than any positive number, so $\frac{1}{0}$, or $1 \div 0$, must be larger than any positive number. Let us denote '$1 \div 0$' by '∞' ('infinity'). (This familiar symbol was invented by Wallis, and used in this way by him.) Now, continue past zero into the negative numbers[90]: the quotients will increase further:

$$\ldots \frac{1}{0} < \ldots < \frac{1}{-0.1} < \frac{1}{-0.2} < \frac{1}{-0.5} < \frac{1}{-1} < \ldots$$

But $\frac{1}{-1} = -1$. So $\frac{1}{0} < -1$. That is, $\infty < -1$. In words, infinity is less than negative one.

Negative numbers are bigger than infinity! Nobody really thinks that nowadays. As I said above, this involves a notion of infinity that did not *catch on*. It did not *take*. Here is one determination of a concept, to put the matter another way, that was not acceptable to the mathematical community.

[90] Wallis was also the originator of the now-familiar idea of the 'number line', a line containing (we would now say) points representing all the real numbers in order. His 'proof' here of the relative sizes of ∞ and -1 could perhaps better be thought of as working via the numbers (the divisors) as placed on such a number line, from the positives through zero to the negatives. Whether such a view makes the argument more convincing I am not sure.

Nowadays ∞ does not count as a number at all[91], although the notation survives, in talk of limits for instance. '$\lim_{x \to 0}\left(\frac{1}{x}\right) = \infty$', for example, is acceptable contemporary notation: 'the limit as *x* tends to zero of the reciprocal of *x* is infinity,' we can say. In so saying, we might be thought to be giving a meaning to ∞, at least if we follow Hartry Field and Crispin Wright in their semantics. (See Chapter 2 above.) If we follow Wright through to the ontology, we would seem also to be accepting the *existence* of ∞ via the truth of the proposition. For Field, of course, like all mathematical propositions, this one is false. However, any difficulties deriving from talk of infinity in such cases are generally considered to be avoidable by means of standard Cauchy/Weierstrass-style translations of limit statements into 'epsilon-delta' language which avoid any commitment to (or, indeed, use of) 'infinity'. That is what such translations are for. The development here, correctly described, can further bolster the case I am arguing, as I suggest below.

I will eschew analysis of Wallis' 'proof' that negative numbers are larger than infinity at this juncture. But it is worth a brief consideration of present-day attitudes to it. Here is an illustrative example of such attitudes from the distinguished historian of mathematics Morris Kline:

> On the whole not many sixteenth- and seventeenth-century mathematicians felt at ease with or accepted negative numbers as such … There were some curious beliefs about them. Though Wallis was advanced for his times and accepted negative numbers, he thought they were larger than infinity … (Kline 1972 p. 253)

'Curious beliefs': the implicit attitude – Wallis was not just wrong about infinity and negative numbers, his views were a little crazy – is something to bear in mind as we proceed. The 'number' ∞ is in the dustbin of mathematical history. Do other attempts at determining a concept of infinity fare any better?

3.4 Catching-on to infinity (2): $\aleph_0$ (and beyond!)

Georg Cantor's treatment of infinite numbers is much more well known than Wallis'. It behoves us to consider it, particularly in the light of Wittgenstein's own strictures on it.

Some preliminaries: it might be claimed that a major difference between Cantor's theory and that of Wallis is that the details of Cantorian transfinite numbers can sit within a formal axiomatic system. Such a claim would be anachronistic were it to be taken to apply to Cantor's own expression of his theory, but, still, 'proofs' of various

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[91] See, for instance, Speigel 1963, p. 24: 'The symbols $+\infty$ (also written ∞) and $-\infty$ are read *plus infinity* (or *infinity*) and *minus infinity* respectively, but it must be emphasised that they are not numbers.' Likewise the modern-day working mathematician has *no use* for a 'number' represented by $\frac{1}{0}$ (or $1 \div 0$): division by zero is just not given a sense.

aspects of the theory might be claimed to gain a formally correct status once set in the system of, say, standard Zermelo-Fraenkel set theory. Such a claim will not wash, though: ZF set theory has an axiom of infinity. So a particular kind of existence of the 'actual infinite', what we might call a particular way of taking the existence of transfinite numbers at face value, is built in at the ground floor. We are justified, then, in taking a less formal approach in investigating how Cantor determines the concept of infinity (or discovers facts about infinity).

A good place to start is with the quotation I have above:

> The dangerous, deceptive thing about the idea: "The real numbers cannot be arranged in a series", or again "The set … is not denumerable" is that it makes the determination of a concept – concept formation – look like a fact of nature. (*RFM* p. 131)

Begin with the notion of a *denumerable* set. Suppose first of all that I have a finite set of objects. I can 'number' the set by counting the objects it contains. For each object in the set, I pronounce the name of one of the natural numbers, 'one', 'two', 'three', 'four', … and so on. In this way I pair off the objects with the numbers taken in order, stopping when there are no objects left to count. The last number I have pronounced, now, will be the number of objects contained in the set – it will be the answer to the question, 'How many?' applied to the objects in that set[92].

This 'pairing-off' of objects with numbers is key to the notion of denumerability. I know there are *as many* knives as there are forks if there is one knife for every fork and one fork for every knife – if the knives and forks are in one-one correspondence, that is.[93] Very well, then, if the objects in a set are in one-one correspondence with the set of numbers {1, 2, 3, 4, 5}, say, then there are as many objects as there are numbers in that set, namely five.

That is *counting* for finite numbers. To answer the question, 'How many?', we simply find by pairing-off which set of natural numbers is in one-one correspondence with the set of things we wish to count. What Cantor wants to do is to extend this idea into infinite sets.

What *is* an infinite set? In some ways this question is just what is currently at issue; for now, though, let us just take Cantor's own characterisation:

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[92] I should perhaps mention that counting 'in order' gives me an ordinal number, whereas the 'how many?' question asks for a cardinal number. For ordinary (finite) numbers at least, these are the same. (It takes young children a while to realise this. Grown-ups tend to forget the difference.)

[93] The underlying idea here is often nowadays known as 'Hume's Principle'(See Hume (1888) I, *III*, 1), but would better be called the 'Cantor-Hume Principle', as Mike Beaney reminds me; Cantor was the first to use the principle for sets of infinite cardinality. The principle is fundamental, for instance, to (neo-)Fregean developments of arithmetic itself.

> Aggregates with finite cardinal numbers are called "finite aggregates," all others we will call "transfinite aggregates" and their cardinal numbers "transfinite cardinal numbers." (Cantor 1955 p. 104)

That is, finite sets are those with finite cardinality (finite cardinals being 1, 2, 3, and so on) and infinite sets just those that *do not* have finite cardinality.

Are there any infinite sets, though? Cantor assumes their existence, plainly. Focussing on the numbers themselves rather than on sets with that cardinality is indicative of what is going on, and what Wittgenstein wants to suggest we look at more carefully. Cantor:

> The first example of a transfinite aggregate is given by the totality of finite cardinal numbers ν;[94] we call its cardinal number "Aleph-zero" and denote it by $\aleph_0$ …
>
> … The number $\aleph_0$ is greater than any finite number μ:
>
> $$\aleph_0 > \mu. \qquad (ibid.)$$

(Wittgenstein, we should note, denies this, at least by implication: '$\aleph_0$ is not an enormous number', he says. (*LFM* p. 32) We will see something soon of the history of a similar specific denial of what Cantor claims here – in particular by Galileo.)

Now, a set is said to be 'denumerable'[95], following Cantor, if its elements can be put into one-one correspondence with the elements of the set $\boldsymbol{N}$ of natural numbers. It follows that a denumerable set, according to Cantor, has $\aleph_0$ members. Further, we can, it seems, investigate the possibilities of making correspondences[96] between the members of different sets and the natural numbers. It is easy to see, for instance, that the set of even natural numbers, {2, 4, 6, 8, …} can be mapped onto $\boldsymbol{N}$ by the function $x \longrightarrow \frac{1}{2}x$. So, then, using our pairing-off one-one correspondence criterion (the *Cantor-Hume Principle*, recall), there are the same number, $\aleph_0$, of even numbers as there are natural numbers. Likewise, it is easy to see on the basis of this schema that, for instance, the set of squares {1, 4, 9, 16, …} also has cardinality $\aleph_0$.[97]

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[94] This 'ν' is our '$\boldsymbol{N}$', of course.

[95] Sometimes 'countable'. There are different uses. Likewise, sometimes 'denumerable' is taken to apply to finite and infinite sets, sometimes only to infinite sets.

[96] As we might expect, Wittgenstein puts the idea of 'existence' of one-one correlations itself in question. See, for example, *RFM* P. 99, following the remark about 'discovery' *vs* 'invention'. It is worth bearing in mind the ubiquity of this latter aspect of Wittgenstein's thought – the consequent autonomy of mathematics goes all the way down, as it were. I will not develop this just here, though. We need to remain on one level just for the sake of clarity, even while being aware of attendant difficulties such as this that lurk below the surface.

[97] If this is starting to look strange, or debateable (see remarks on *Galileo's paradox* below, for instance), recall that likewise with Wallis's proof of the relative sizes of infinity and negatives, I postponed discussion of apparently fallacious moves. Here too: what was sauce for Wallis's goose is sauce for Cantor's gander.

We can push this notion of denumerability further, too. With a little thought, it seems we can extend the idea to show a one-one correspondence between positive rational numbers and natural numbers:

	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**...**
1	$\frac{1}{1}$	$\frac{1}{2}$	$\frac{1}{3}$	$\frac{1}{4}$	$\frac{1}{5}$	$\frac{1}{6}$	$\frac{1}{7}$	$\frac{1}{8}$	...
2	$\frac{2}{1}$	$\cancel{\frac{2}{2}}$	$\frac{2}{3}$	$\cancel{\frac{2}{4}}$	$\frac{2}{5}$	$\cancel{\frac{2}{6}}$	$\frac{2}{7}$	$\frac{2}{8}$	...
3	$\frac{3}{1}$	$\frac{3}{2}$	$\cancel{\frac{3}{3}}$	$\frac{3}{4}$	$\frac{3}{5}$	$\frac{3}{6}$	$\frac{3}{7}$	$\frac{3}{8}$	...
4	$\frac{4}{1}$	$\cancel{\frac{4}{2}}$	$\frac{4}{3}$	$\cancel{\frac{4}{4}}$	$\frac{4}{5}$	$\frac{4}{6}$	$\frac{4}{7}$	$\frac{4}{8}$	...
5	$\frac{5}{1}$	$\frac{5}{2}$	$\frac{5}{3}$	$\frac{5}{4}$	$\frac{5}{5}$	$\frac{5}{6}$	$\frac{5}{7}$	$\frac{5}{8}$	...
6	$\frac{6}{1}$	$\cancel{\frac{6}{2}}$	$\cancel{\frac{6}{3}}$	$\frac{6}{4}$	$\frac{6}{5}$	$\frac{6}{6}$	$\frac{6}{7}$	$\frac{6}{8}$	...
7	$\frac{7}{1}$	$\frac{7}{2}$	$\frac{7}{3}$	$\frac{7}{4}$	$\frac{7}{5}$	$\frac{7}{6}$	$\frac{7}{7}$	$\frac{7}{8}$	...
8	$\frac{8}{1}$	$\frac{8}{2}$	$\frac{8}{3}$	$\frac{8}{4}$	$\frac{8}{5}$	$\frac{8}{6}$	$\frac{8}{7}$	$\frac{8}{8}$	...
⋮	⋮								

– set out the positive rationals in a grid, eliminate the fractions that 'cancel' (like $\frac{2}{2}$ or $\frac{2}{4}$, for instance), and count the remaining numbers in the order shown by the arrows, so that $\frac{1}{1}$ is the 'first' (corresponds to 1) rational number, $\frac{2}{1}$ is the 'second' (corresponds to 2), $\frac{1}{2}$ is the 'third' (corresponds to 3), … and so on. This way for every fraction we have a corresponding natural number and vice-versa. (We eliminate the 'cancelling' fractions just to avoid double counting.)

Just as many fractions ($\aleph_0$ again, of course) as there are natural numbers! Could this count as a 'curious belief' as we saw above Wallis' conclusion put in question? Even Cantor himself, writing to Dedekind in 1877 of his proof that, in similar fashion, the points in p-dimensional space can be put into one-one correspondence with the points on a finite line-segment, famously had to admit,

> Je le vois, mais je ne le crois pas! (quoted in Ewald, 1996 p. 860)

However, putting such incredulity to one side for the present, we can move on with Cantor to consider whether all sets are denumerable in this sense of being one-one correspondable with $\boldsymbol{N}$. In fact, as we will see shortly, the set of real numbers is non-denumerable, according to Cantor.[98] As with Wallis's proof of the relative sizes of negative numbers and ∞, I will set out Cantor's proof of this latter for the time being uncritically and fairly informally.

Cantor's first proof of the non-denumerability of the reals was published in *Crelle's Journal* in 1874. Later, he developed another proof in a form that has become known as that of a 'diagonal proof', on account of how a picture of the real numbers in a sequential list can be added to by changing the numbers on the leading diagonal to get a number not already on the list. Here is a version of Cantor's diagonal proof:

It goes by *reductio*. Suppose, contrary to what we are trying to prove, that we can put the real numbers between zero and one into one-one correspondence with the natural numbers. Then there will be a first, second, third, … and so on, real number according to this correspondence. Let us suppose we have all the real numbers in this ordered sequence expressed in decimal form[99]. (Every real number is expressible either as a terminating/repeating decimal (if it is rational) or as an infinite non-repeating decimal (if it is irrational)). To effect the *reductio* we characterise a number which differs from the first number on the list in the first decimal place; which differs from the second in the second decimal place; from the third in the third decimal place; … and so on. We do this as follows: if the *n*th digit of the *n*th number in the list is less than 9, add 1 to it (for terminating decimals, fill in with zeros at the end where necessary, before doing this), and if it is 9, replace with 0. Now this ('diagonal') number is a number which cannot be on the list: it is not the first on the list (it differs from it at at least one decimal place, the first); it is not the second (it differs from it at at least the second decimal place); … and so on. So there is a number that is not on the list, contrary to the assumption that the correspondence is one-one. Hence the real numbers *cannot* be put into one-one correspondence with the natural numbers. That is, the set of real numbers is non-denumerable.

If we recall that the set of real numbers contains (strictly speaking, a copy of) the natural numbers, it seems we are forced to admit that there are *more* (in fact, *infinitely* more) real numbers than there are natural numbers or rationals (given that, as we have seen, the rationals are themselves denumerable).

3.5 ∞ and (or versus) $\aleph_0$

Now, recall that towards the end of section *3.3* I pointed out that 'nobody nowadays thinks' along the lines of Wallis's explication of *infinity-as-∞* – a number equal to

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[98] For the time being, I am just assuming we know what the real numbers are: roughly speaking, any number representable on Wallis's number line is a real number, including integers, rational numbers and irrational numbers. For a little more precision, see below regarding Dedekind cuts and related matters.

[99] Cantor, effectively, had the numbers expressed in binary. That is not important.

the reciprocal of zero, but less than any negative number. That conception of infinity did not catch on; in fact, we saw it described as a 'curious belief' – something of an historical oddity. The opposite is true of Cantor's *infinity-as-*$\aleph_0$ – a number answering the 'how-many?' question asked of the totality of real numbers with '*more* than the number of rationals or natural numbers.' Cantor's conception *did* catch on – it is a staple of contemporary philosophy of mathematics, and, arguably, even of mathematics itself.[100] However, as we have seen, it is just this very point at which the Cantorian conception gets going that Wittgenstein points to as being 'dangerous, deceptive' in causing us to mistake concept formation for factual discovery.

In fact Wittgenstein thinks the mistake is also exemplified by how Cantor's argument is taken. Wittgenstein is (in)famously polemical about this. He goes on,

> If it were said: "Consideration of the diagonal procedure shews you that the concept 'real number' has much less analogy with the concept 'cardinal number' than we, being misled by certain analogies, are inclined to believe", that would have a good and honest sense. But just the opposite happens: one pretends to compare the 'set' of real numbers in magnitude with that of cardinal numbers. The difference in kind between the two conceptions is represented, by a skew form of expression, as difference of extension. I believe, and hope, that a future generation will laugh at this hocus pocus. (*RFM* p. 132)

Can we contrast 'hocus pocus' with the 'curious belief' attributed to Wallis? I do not wish to advance the cause of *infinity-as-*∞ over that of *infinity-as-*$\aleph_0$ or anything like that. However, the contrast between the two – indeed the very existence of the two conceptions within the history of mathematics – should give us pause. There is a lesson to be learned from seeing the two conceptions of infinity in the light of Wittgenstein's distinction between concept formation and discovery of facts of nature.

The notion that Cantor discovered once for all facts about the nature of infinite cardinality is put under question by Wittgenstein's distinction, and further under stress by the existence in history of other notions of infinity such as Wallis's ∞. Or, perhaps, the matter is better put obversely: Wittgenstein's distinction is bolstered by the existence of different possible determinations of concepts such as infinity. Hence my talk of one concept rather than another 'catching on'. If, indeed, as I am suggesting, it is the catching on that is important rather than whether one particular way with the concept of infinity connects with the facts, it is to be expected that some different ways with the concept should make themselves available, ultimately to be taken up or not, to catch on or not with the general mathematical community and perhaps beyond.

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[100] It may be debateable whether we should describe studies in the foundations of mathematics that concern themselves with transfinite numbers as mathematics proper or philosophy of mathematics (or a mixture). I do not want to tread on any toes by coming down on either side in any such argument.

Of course it could be that certain ways with the concept of numerical infinity simply fail to do justice to the facts. Perhaps Wallis's development did not catch on because it was not *correct*, in accord with the facts about infinity, by contrast with Cantor's. If true, the correctness of a particular way of developing a concept would explain and justify that development. In short, perhaps Cantor gets infinity right, and Wallis gets infinity wrong. We should investigate this possibility.

There is a clear difficulty in Wallis's proof of the relative size of infinity and negatives. In the move from dividing by a very small positive number to dividing by a negative number, passing through (and assuming a continuity involved in) dividing by zero, the implication is that we move from small to smaller. But we end up with negative numbers *larger* than ∞. It seems that Wallis proves that $1 < \infty < -1$ by using the fact that $-1 < 0 < 1$. So there is a contradiction in his 'proof'. That seems to vitiate that particular way of developing the concept of infinity.

However, does Cantor do any better? Consider what has become known as 'Galileo's paradox': (see Galileo 1954 pp. 31-33) Every perfect square is a natural number, and not every natural number is a perfect square. So the set of perfect squares is a proper subset of the set of natural numbers. In short, and informally, the set of squares is a part of the set of integers. And the whole is greater than the part.[101] So the set of natural numbers is greater than the set of squares. There are *more* natural numbers than there are perfect squares, it seems obvious. However, the set of perfect squares can clearly be put into one-one correspondence with the set of natural numbers, which according to Cantor means that there are the same number of squares as there are integers. This is a point Galileo himself makes:

> *Salviati*: If I should ask further how many squares there are one might reply truly that there are as many as the corresponding number of roots, since every square has its own root and every root its own square, while no square has more than one root and no root more than one square. (*ibid.*)

So we have a contradiction. On the one hand we have more numbers than squares, but on the other hand we have the same number of each. To escape this contradiction, Galileo makes an obvious move. He denies that 'more than', 'less than', 'same number as' are relations that can be applied to infinite sets:

> *Salviati*: … the attributes "equal," "greater," and "less," are not applicable to infinite, but only to finite, quantities. (*ibid.*)

Cantor, however, swallows the contradiction. In fact, one modern day definition of numerical infinity (so-called *Dedekind infinity*) trades on this very contradiction, *defining* an infinite set as one that can be put in one-one correspondence with a proper subset of itself.

[101] Euclid, *Common Notions,* 5.: 'The whole is greater than the part.' See Heath 1956 P. 155.

So we find contradiction in Wallis's development of the notion of infinity, but also in Cantor's. Which contradiction we allow to stand, it seems, determines which particular brand of infinite number we are inclined to buy.

3.6 Conceptual development more generally

This is not a situation that is confined to the development of concepts of (putative) *infinite* numbers. Consider the historical development of our number system. At each step, it seems, from natural ('counting') numbers to rationals, irrationals, negatives, real numbers, complex numbers, mathematicians had to swallow what looked like a contradiction. To allow complex numbers, for instance, we are forced to swallow a number whose square is negative, a manifest apparent contradiction since the square of any (real) number is positive. In fact, Wallis himself dealt explicitly with this latter case, and did so by comparing the development from real to complex with the move from accepting only positive numbers to allowing negatives as well. 'Imaginary quantities,' he claimed, are

> Impossible … as to the first and strict notion of what is proposed. For it is not possible, that any Number (Negative or Affirmative) Multiplied into itself, can produce, for instance, – 4. But it is also Impossible that any Quantity (though not a Supposed Square) can be *Negative*. Since that it is not possible that any *Magnitude* can be *Less than Nothing*, or any *Number Fewer than None*. (Wallis 1685 p. 264)[102]

Helena Pycior describes this as a 'coattails argument':

> … if mathematicians accepted the negatives, they ought to accept numbers involving √–1. (Pycior 1997 p. 129)

For our purposes, though, it is enough to see something of the way developments of our number system – of what *counts* as a number, we might say, or of what the concept *number* includes – share some particular features. Consider the case of negative numbers. On the one hand, we might have thought, as Wallis said, that numbers represent magnitudes, and that a magnitude could not be less than nothing. On the other hand, though, we want our set of numbers to be closed under subtraction: given there is an answer to '5 – 3 = ?', we want there also to be an answer to '3 – 5 = ?'. We are, as it were, pulled two ways – or, to put it another way, we seem to have a conflict of criteria to apply – criteria for what is to count as a number.[103] There is a choice, that is, between whether we apply the criterion that a number represents a magnitude, there being no magnitudes less than nothing, or the

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[102] Wallis took the appellation 'imaginary' from Descartes. Terminology was far from being settled at the time he was writing.

[103] This way of putting the matter was suggested to me by Mike Beaney. It is a neat way of characterising this kind of conceptual change and/or development.

criterion that there must be an answer to a subtraction of numbers which is a number of some sort.[104]

We have a similar conflict of criteria when considering whether to accept complex or imaginary numbers – should we allow that every number (including negatives, supposing us to have allowed them to be numbers) has a square root, or maintain the criterion that every square is positive? For just positive numbers and their roots, these criteria coincide; they are pulled apart, as it were, when the move to negatives and their roots is in the offing.

Likewise, there is a criterial choice to be made when considering possible moves into the transfinite. As we saw just above, comparing Galileo and Cantor, the two criteria (1) that the whole is greater than the part, and (2) that 1-1 correspondence identifies cardinality, come apart when considering putative infinite numbers. Galileo, we saw, explicitly denied (2), whereas Cantor (implicitly) and Dedekind (explicitly) denied (1), in the move from finite to infinite.

This informal characterisation of conceptual development in terms of criterial choice may help see more clearly what is going on once we allow ourselves to consider Wittgenstein's distinction between discovery of matters of fact and determination of mathematical concepts. I am suggesting that seeing matters thus more clearly – moving thereby towards some kind of perspicuous overview – is liable to bolster the plausibility and importance of this distinction.

It is not always easy to see what determines the outcome of such questions of criterial choice. However, one particular feature of Wallis's argument in favour of accepting both negatives and imaginaries is worth a mention as something I will be returning to. As Pycior explains, Wallis wrote within a tradition that emphasised the importance of possibilities of *applying* new aspects of mathematics:

> … stressing applicability (as had Pell and Kesey), [Wallis] emphasised that the "Supposition (of Negative Quantities)" was neither "Unuseful" nor "Absurd". A negative number was, after all, susceptible of "Physical Application … [and] denotes as Real a Quantity as if the Sign were +; but to be interpreted in a contrary sense." (Pycior 1997 pp. 130-131; see Wallis 1685 pp. 265-266)

Nowadays we make no cavil at considering movements along Wallis's 'number line', if positive in one direction, to be negative in the opposite direction. If point *C* is 3 units forward from point *A*, and *D* is 3 units backwards from *A*, then, says Wallis,

> … –3 doth as truly design the Point *D*; as +3 designed the Point *C*. Not Forward, as was supposed; but backward, from A …(*ibid.*)

[104] We might see this in terms of whether to allow negative magnitudes, of course. Nothing turns on whether we see the matter in terms of what to allow as a magnitude or as what we allow as a number.

Further, Wallis tried, perhaps with somewhat less success, to justify the use of imaginary numbers with an argument involving the side of an area of land gained or lost to the sea. A gain of so many acres of land will be considered positive, whereas a loss might be denominated negative. So, suppose we lose 10 acres of land to the sea. In such a case, according to Wallis,

> The Gain is 10 Acres less than nothing. Which is the same as to say, there is a Loss of 10 Acres: or of 1600 Square Perches. … [Suppose] this Negative Plain, –1600 Perches, to be in the form of a Square. … [Concerning the side of the square:] We cannot say it is 40, nor that it is –40 … it is √ –1600 (the Supposed Root of a Negative Square;) or (which is Equivalent thereunto) 10√ –16, or 20√ –4, or 40√ –1. (*ibid*.)[105]

In the absence of profit-and-loss accounts for lost fields and the lengths of their sides, this is not a particularly persuasive application of complex numbers. However, it is notable that Wallis and others, on the historical cusp of the acceptance of 'imaginaries' as numbers in good standing, saw questions about the possibility of application as at least in part determinative of such acceptance.

Of course there are more important applications for complex numbers, as I mentioned above in Chapter 1. The extent to which such applications do determine the manner in which mathematical and other concepts are determined is one Wittgenstein raises; we will see more of this shortly. However, I want to consider, now, another example of how this determination – in both these senses – plays out. It is also an example which will nicely tie together some of our reflections on the historical development of the number system with questions about infinity. The example is of irrational numbers, or more precisely, of real numbers including the irrationals.

3.7 Incommensurables and irrational numbers

Giving an example of a *reductio* proof in *Prior Analytics*, Aristotle refers somewhat cryptically to a proof of incommensurability, or, we might say, of the existence of a particular irrational number:

> For all who effect an argument *per impossibile* deduce what is false, and prove the original conclusion hypothetically when something impossible results from the assumption of its contradictory; e.g. that the diagonal of a square is incommensurate with the side, because odd numbers are equal to evens if it is supposed to be commensurate. One deduces that odd numbers come out equal to evens and one proves hypothetically the incommensurability of the diagonal, since a falsehood results from its contradictory. (*Prior Analytics* I, 23)

[105] An acre is – or was – an area 40 perches by 4 perches, 40 perches (a 'furrow-long' or 'furlong', 220 yards) being held to be the distance a team of oxen could plough ('furrow') in one go. The acre was finally put to death in 2007 by EU directive and is now no longer a legal standard measure. (None of this is of any philosophical import.)

I will fill in some of the details. Supposing the side of a square to be of unit length, we can apply Pythagoras' theorem to deduce that the diagonal must itself have a square of 2. Now, Aristotle claims, this diagonal is 'incommensurate' with the side. That is, effectively, the diagonal of the square is not expressible as a ratio of the side of the square – there is no rational number equal to $\sqrt{2}$. Proof? As Aristotle says, it goes by contradiction.[106] Here it is in present-day terminology:

Suppose $\sqrt{2}$ is a rational number.

That is, $\sqrt{2} = \frac{a}{b}$ for some integers *a* and *b*.

We can assume that *a* and *b* are not both even, for if they are we can divide them both by 2 leaving $\sqrt{2} = \frac{a'}{b'}$, repeating if necessary if the new numerator and denominator are both even. So, let $\sqrt{2} = \frac{a}{b}$ where at most one of *a* and *b* is even, the other being odd.[107]

Now we argue as follows:

$$\sqrt{2} = \frac{a}{b} \Rightarrow a = \sqrt{2}\,b \Rightarrow a^2 = 2b^2$$

That is, a^2 is even. So *a* must be even, since the square of an odd number is odd. Since just one of *a* and *b* is even, it follows that *b* is odd.

But, now, since *a* is even it must be that $a = 2p$ for some integer *p*. So $a^2 = 4p^2$. But from above, $a^2 = 2b^2$. So $2b^2 = 4p^2$, or, dividing by 2, $b^2 = 2p^2$. That is, b^2 is even and so *b* is even.

There is our contradiction. From the supposition that the length of the diagonal of a square ($\sqrt{2}$ units where the side is 1 unit) is commensurate with the side (*i.e.* that $\sqrt{2} = \frac{a}{b}$ for some integers *a* and *b*), we have proved that 'odd numbers come out equal to evens' as Aristotle puts it – we have proved that our integer *b* is both odd and even. So we have 'prove[d] hypothetically the incommensurability of the diagonal [*viz.* that $\sqrt{2}$ is irrational], since a falsehood results from its contradictory [*sc.* that the diagonal *is* commensurable with the side, *viz.* that $\sqrt{2}$ is rational].'

[106] This is essentially the proof to be found in some editions of Euclid, though it seems it is a later addition there. (See Heath 1956 vol. 3 p. 2: 'The proof formerly appeared in the texts of Euclid as x. 117, but is undoubtedly an interpolation ...')

[107] It is easy to see how this generalises for any prime number root, even if we do not have a single word for 'divisible by *p*' or 'not divisible by *p*' except for $p = 2$ ('even'; 'odd'). I keep to the special case to follow Aristotle's text.

Aristotle used this as a paradigmatic example of a proof by contradiction, perhaps indicating the importance of the conclusion for the development of Greek thought. I mentioned Pythagoras in my introductory chapter and suggested the development of the Pythagorean conception of *cosmos* – the world ordered by mathematics – as suggestive of an apparent requirement for *somehow* dealing with the way mathematics engages with the world. The story of how the 'incommensurability' consequence of the Pythagorean theorem disturbed this conception is well known.[108] I will not retell the story here. Suffice for our present concerns that the proof of incommensurability/irrationality faces us, at least in hindsight, with a familiar apparent dilemma involving concept-determination and choice of criteria.

Consider what it is to measure a length. We will need a unit length of some sort, and then any other length, it seems, can be given as a specific (rational) number of such units. Why a *rational* number? – Because, we assume, either the measure of length will be a specific whole number of units, or there will be some whole number of units exactly the same length as some other whole number of the length we are measuring. That is what it is for lengths to be commensurable – one of them can be measured with the other as a unit. In fact, the existence of the rational number itself is inessential – we can always measure a *length* in terms of (so many units) = (such-and-such number of *lengths*), where both 'so many' and 'such-and-such number' are integers. Or, indeed, if we are allowed to pick a smaller length as our unit, decreasing appropriately – in proportion – will always allow a measurement in terms of whole numbers. For instance, if my desk is 1.72 metres in length, we can either say that 172 metre sticks will have the same length as (will *measure*) 100 desks, or, reducing the unit appropriately, that my desk measures 172 centimetres.

So, given that we want lengths to be measured numerically, it seems that this requires that numbers be ratios. Here then are two criteria: (1) lengths have numbers associated with them; (2) numbers are expressible as ratios. The proof of the incommensurability of the diagonal with the side, now, shows us that we cannot keep both criteria. We can *either* allow there are some lengths that have no number associated with them, *or* allow there to be numbers which are not ratios.

We know which way the determination went, of course. We deny the second criterion in order to keep the first. $\sqrt{2}$ is a number in good standing, though irrational, and every length is (still) measurable numerically. Highlighting the importance of the proof of incommensurability in Greek thought serves as a dramatisation of the historical moment at which this determination was first made. At least, I want to suggest, this is a plausible way of expressing the matter. Of course there is no suggestion of the determination being made explicitly. Nor are alternative ways of telling the story ruled out: we might well want to say that the Pythagoreans

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[108] See, *e.g.* Kline 1972 P. 33. Whether or not the tale of Hippasus's assassination by drowning on account of his dealings with incommensurability is apocryphal, the very existence of the story is evidence of such disturbance. See *op. cit.* for further details.

discovered that there are irrational numbers, as we might say that later mathematicians discovered that there are imaginary or complex numbers. That we can tell the tale in terms of criterial choice or the determination of a concept proceeding by negotiation of criteria and adjustment of conceptual interrelations, however, at the very least highlights the difference between this kind of 'discovery' in mathematics and discoveries of facts such as that the boiling point of water changes with air pressure or that there is a southern continent.

Can we imagine the determination going the other way, the first criterion above being denied in order to keep the first? If so, it will further bolster Wittgenstein's way of seeing things. And, indeed, yes, in the light of the incommensurability of diagonal with side, we can certainly imagine holding tight to the idea of numbers-as-ratios and so denying that every length is expressible numerically. We cannot measure any length absolutely precisely anyway, we might say: that certain lengths have no precise number associated with them should not be too difficult to swallow.

So the development of our number system in its movement to include irrational as well as rational numbers, as with other such expansions of what is to count as a number, fits well with Wittgenstein's view of such development as representing concept-determination rather than fact-discovery.[109] Once we adopt this way of thinking, moreover, we are primed to ask – and possibly answer – certain questions about the direction of such development. In terms of the criterial-choice manner I have mooted as an appropriate way of seeing concept-determination, we might ask why one criterion rather than another gets to be chosen once such different criteria pull apart. Such a question asks why the concept in question – largely the concept of *number* – developed the way it did. We saw above that Wallis and others emphasised the *applicability* of mathematical concepts to the physical world as a reason for adopting such concepts, and this emphasis is something I want to look at further.

Before getting to that, though, a brief digression. I want to point out here that answers to such questions about the direction of concept development are hard to come by if we take an opposing view to Wittgenstein's. The opposition takes it that there is some (non-physical) *fact* that is discovered in conceptual development – we discover, that is, that there is a number $\sqrt{2}$ that is irrational, that there is a number $\aleph_0$ that is infinite (and that there is no such number as ∞), and so on. However, if we come to ask *how* we discover such facts, we encounter difficulties. We may think that such discoveries are made mathematically – has not mathematics shown us that $\sqrt{2}$ is irrational and so on, after all? But this will not do. We cannot pin the blame on mathematics itself, as it were. As we have seen, what mathematics has

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[109] I hope it is plain that the dichotomy here is not so clear cut as it might appear from such a bald statement. For example, we might well say that we discover *that* things can be so represented, etc. As ever, we seek, not a definitive theoretical pronouncement but a clear overview of the philosophical terrain, with all its hard cases and differences.

done in the case of incommensurables is show us that two criteria concerning numbers and lengths come apart in certain situations. What mathematics does *not* do is show us which criterion we should keep and which we should reject in such situations. And if it is a *fact* that there are irrational numbers such as $\sqrt{2}$ and that every length is thereby precisely numerically quantifiable, rather than it being a fact that the only numbers properly so-called are rational and that some lengths are not numerically quantifiable with precision, it is certainly mysterious how we might ever gain access to such facts. Mathematics itself, to reiterate, is silent on such questions – the proof of incommensurability of diagonal with side, for instance shows us only that we cannot maintain *both* that numbers are all rational *and* that lengths are all precisely numerically quantifiable.

This is a point I want to flag up for further discussion.[110] There is a distinction to be drawn here between what goes on *inside* mathematics and the manner in which mathematics itself develops – what we might think of rather in terms of mathematics *as a whole* seen as part of our intellectual life in general. In our most recent example, the proof of incommensurability is itself *part* of mathematics, but the manner or direction of development – the acceptance or rejection of irrational numbers as *bona fide* members of the family of numbers – needs determination other than by *intra*-mathematical already-fixed criteria. (Such latter determination might proceed by, but not necessarily be restricted to, questions of utility, as we saw with complex numbers above.)

The distinction I am flagging applies, *mutatis mutandis*, for other developments in our number concepts. For example, mathematics itself has nothing to say about whether we should, with Galileo, deny that one-one correspondence establishes numerical equality and comparisons of magnitude whilst maintaining the criterion that the whole is greater than its parts, or, with Cantor and Dedekind, maintain the criterion of numerical equivalence and comparison of magnitude by one-one correspondence while denying that the whole is always greater than its parts. The two criteria in play here coincide for finite cardinals, of course, and we have good mathematical reasons for seeing how they pull apart when we move to the transfinite. But we do not have any good mathematical reasons for the choice between Galileo's ' … "equal," "greater," and "less," are not applicable to infinite … quantities' and Cantor's assertion that, for instance, '$\aleph_0 < \aleph_1$' makes perfect sense. If there is indeed a matter of fact determining the choice between Galileo and Cantor it is difficult to see how we can gain access to it. We will need some extra-mathematical help, it seems – and it is not at all clear where we are going to find such.

I will return to the difference between questions within mathematics and the kinds of questions I am pointing to here that mathematics itself is, as I claim, silent about. For now, I just want to make the point that a Wittgensteinian 'concept

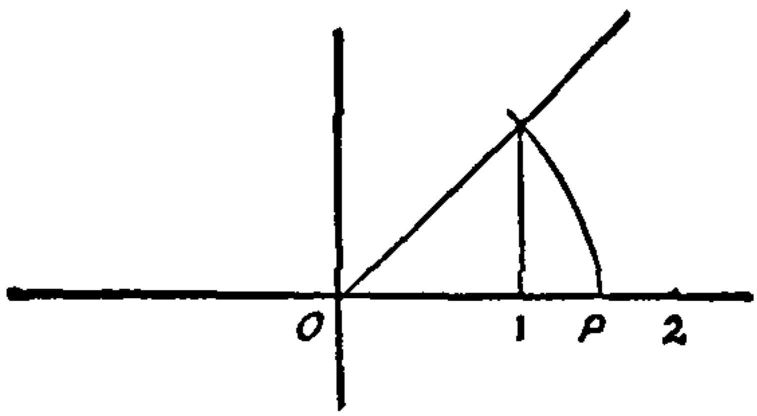
[110] In Chapter 5 below.

determination' view of conceptual development of the kind we have been considering can avoid this aspect of the 'access problem' that a 'factual discovery' view is prone to. I have suggested that one important way of determining the choice – apparent in Wallis and other mathematicians of his time – connected with possibilities of application in the physical world. Wittgenstein also emphasises applicability in such contexts.

3.8 Real numbers and infinity 'in the small'

We saw above something of Wittgenstein's quarrel with Cantor about the concept 'real number', and connected this to questioning the development of Cantor's notion of infinity – $\aleph_0$ and so on. We can link this with the development of incommensurability and the irrationality of √2.

Here is a diagram:



I have copied this from Wittgenstein's *RFM*. (See p. 291) He comments on it thus:

> It is by combining calculation and construction that one gets the idea that there must be a point left out on the straight line, namely P, if one does not admit √2 as a measure of distance from O. 'For, if I were to construct really accurately, then the circle would have to cut the straight line between its points.'

And he goes on to say,

> This is a frightfully confusing picture …
> What is the *application* of the concept of a straight line in which a point is missing?! The application must be 'common or garden'. The expression "straight line with a point missing" is a fearfully misleading picture. The yawning gulf between illustration and application.

The 'straight line with a point missing' is the (Wallis's) number line. The point that has gone missing is that representing the square root of 2, and the diagram shows how we can apply Pythagoras' Theorem to suggest the place where the missing number should go. Draw on the axes, as shown, a right-angled isosceles triangle of unit side, one unit side being placed along the horizontal axis. The hypotenuse of this triangle can then be transferred onto the horizontal … and its length is √2. The ‘number’ is missing from the line precisely because √2 is irrational or incommensurable with the unit length – it is this that Wittgenstein points to in talking of ‘√2 as a measure of distance from O’. There is *no* (rational) number of units that measures the distance from O to the point at which the arc (of radius √2 by Pythagoras’ Theorem) cuts the line. If all the numbers are rational then indeed, it seems, we have a ‘straight line with a point missing’.

Wittgenstein describes this picture as ‘fearfully misleading’. It will help us see what he means if we relate it to our earlier discussion. Recall the choice of criteria involved in deciding whether we should accept √2 as a number in good standing in spite of its irrationality, or alternatively deny that the diagonal of a unit square has a length that is expressible numerically. The picture seems to make the choice for us. Looking at the picture in the knowledge that there is no rational number of units from *O* to *P*, it can seem that nevertheless there *is* a point there – in a gap just where the arc *cuts* the line. The idea that we can find this gap there, as it were waiting to be filled, connects plainly with the notions that the real number √2 can be *discovered* and that there is a *matter of fact* about whether there really is such a number.

One particular standard way of developing the notion of the real numbers involves taking, not the ‘cut’ itself as point *P* in the diagram above, but rather thinking of such a cut as determining the number in terms of the two sets of numbers above and below. I am going to look at some passages where Wittgenstein takes this on. Here he can best be read as tackling the idea as it is expressed by his contemporary G.H. Hardy in what was for a long time the standard textbook for mathematical analysis in English, Hardy’s *A Course of Pure Mathematics*.[111] Thus, for example, here is Hardy considering the example of √2,

> The square of any rational number is either less than or greater than 2. We can therefore divide the positive rational numbers … into two classes, one containing the numbers whose squares are less than two, and the other those whose squares are greater than two. (Hardy 1952 p. 8)

Hardy goes on to describe such a mode of division of the set of rational numbers into a lower and an upper set as an example of a ‘*section*’. (*op. cit.* p. 11) The two properties, $x^2 < 2$ and $x^2 > 2$, are such that every rational number possesses one of them, and no rational number satisfies both. They define a section in Hardy’s terminology, as will any pair of mutually exclusive properties defined on the set of rational numbers such that every rational number has one or other of the properties.

Now some such pairs of properties define lower or upper sets with, respectively, a greatest or least member.[112] (Consider $x \leq 2$ and $x > 2$, for instance.) Such pairs of

[111] I have no contemporary evidence, but it seems at least plausible reading the relevant sections of *RFM* alongside Hardy that Wittgenstein had Hardy’s book to hand as he wrote. I offer some brief references to this below.

[112] See *op. cit.* p. 12 for a proof that these are exclusive options, if that is not obvious.

properties can be associated with, or correspond to, that greatest or least rational number. This means, according to Hardy, that

> … we are almost forced to a generalisation of our number system. For there are sections (such as that [derived from $x^2 < 2$ and $x^2 > 2$]) which do *not* correspond to any rational number. The aggregate of sections is a larger aggregate than that of the positive rational numbers; it includes sections corresponding to all these numbers, and more besides. It is this fact which we make the basis of our generalisation of the idea of number. (Hardy 1952 pp. 13-14)

That is, according to Hardy, and in terms I have used to characterise the conceptual development of our number system, consideration of the notion of a ‘cut’ or ‘section’ (‘almost’) forces the choice of criterion on us. It is clear, however, that this ‘forcing’ is non-mathematical – it derives, as Hardy himself admits, from the ‘geometrical representation’ – from the ‘picture’ Wittgenstein draws, a picture that Hardy also refers to in getting the development up and running:

> The result of our geometrical representation of the rational numbers is therefore to suggest the desirability of enlarging our conception of ‘number’ by the introduction of numbers of a new kind. (*op. cit.* p. 7)

Hardy – and the standard development – goes on to prove ‘Dedekind’s Theorem’, that the real numbers so defined as sections of the rationals (as ‘Dedekind cuts’) form a *continuum*. Dedekind’s Theorem, indeed, is a part of mathematics, as is the completeness of the set of real numbers as Hardy characterises it. It would be tempting to think that Dedekind’s Theorem guarantees that there are no ‘further gaps’ in the (Wallis’s) number line once the ‘gaps’ like that at irrational points like $\sqrt{2}$ have been filled. This would be a mistake, as Hardy himself carefully admits:

> It is convenient to suppose that the straight line … is composed of points corresponding to all the numbers of the arithmetical continuum and no others.* …
>
> * This supposition is merely a hypothesis … we use geometrical language only for purposes of illustration … (*op. cit.* p. 24)

Now, coming back to Wittgenstein, we have seen him to characterise illustrations in this arena as ‘misleading’. And, he says,

> The picture of the number line is an absolutely natural one up to a certain point; that is to say so long as it is not used for a general theory of real numbers. (*RFM* p. 286)

It may be tempting to read Wittgenstein on Cantor and Dedekind/Hardy as somehow challenging the mathematics of real numbers. However, that is not his intent. Remember *Philosophical Investigations* §124,

> Philosophy … also leaves mathematics as it is …

We are *philosophically* misled by the way illustrations work on our understanding here, Wittgenstein suggests. For example.

> The misleading thing about Dedekind’s conception is the idea that the real numbers are there spread out in the number line. They may be known or not; that does not

> matter. And in this way all that one has to do is to cut or divide into classes, and one has dealt with them all.(*RFM* p. 290)

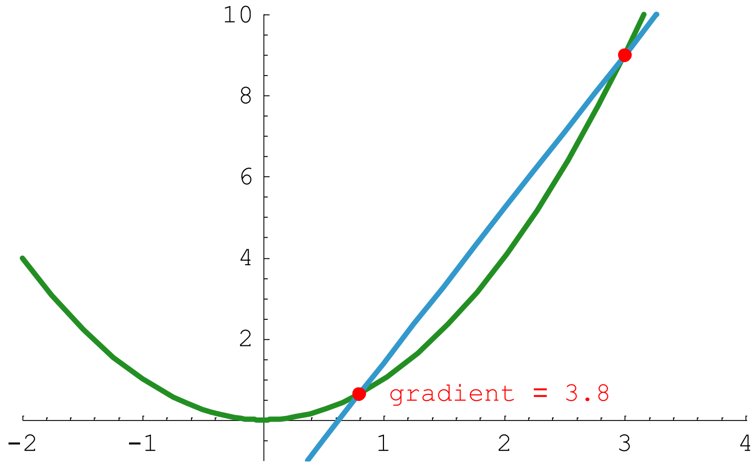
The idea that the numbers – real irrational as well as rational – are in some sense already *there*, waiting our discovery of them, is what Wittgenstein challenges. And *this* idea, of numbers somehow given in extension prior to the development of the mathematics that deals with them, is one that he points out as fostered by the manner of development Hardy espouses.

I mentioned at the end of the last section that Wittgenstein, as we saw Wallis and some of his peers did, emphasised the importance of *applications* of mathematics for determining the direction of determination – what I characterised above in terms of criterial choice – of the concepts of mathematics. Here he is contrasting 'misleading' illustrations of analysis with 'essential' illustrations that are at the same time applications:

> The geometrical illustration of Analysis is indeed inessential[113]; not, however, the geometrical application. Originally the geometrical illustrations were applications of Analysis. Where they cease to be this they can be wholly misleading.
> What we have then is the imaginary application. The fanciful application.
> The idea of a 'cut' is one such dangerous illustration.
> Only in so far as the illustrations are also applications do they avoid producing that special feeling of dizziness which the illustration produces in the moment at which it ceases to be a possible application; when, that is, it becomes stupid. (*RFM* p. 285)

What might 'illustrations [that] are also applications' be? Here is one such. Think of a standard way of illustrating mathematical analysis' way with instantaneous rates of change – via the tangent to a curve at a particular point. An illustration of how we find the gradient of this tangent might well go via a consideration of various secants to the curve drawn at the point of tangency, successive secants approaching closer and closer to the tangent as their points of intersection with the curve approach each other closer and closer. We can see such successively-drawn secants as heuristic in visualising the limiting process that gives us the gradient of the tangent on the curve. Below are some snapshots of an animation illustrating this process as graphed on Cartesian axes for the function $f(x) = x^2$; the point of tangency is (0.8, 0.64):

[113] This is one of those instances where we might usefully think of Wittgenstein as responding directly to Hardy in *A Course of Pure Mathematics*. See, for instance, Hardy 1952: 'It is convenient, in many branches of mathematical analysis, to make a good deal of use of geometrical illustrations. The use of geometric illustrations in this way does not, of course, imply that analysis has any sort of dependence upon geometry: they are illustrations and nothing more …' This is a point Hardy reiterates several times (*e.g.* P. 24, P. 30, …). Wittgenstein looks for a diagnosis of where we might be misled, along with Hardy, by such illustrations; not mathematically, to be sure, but philosophically.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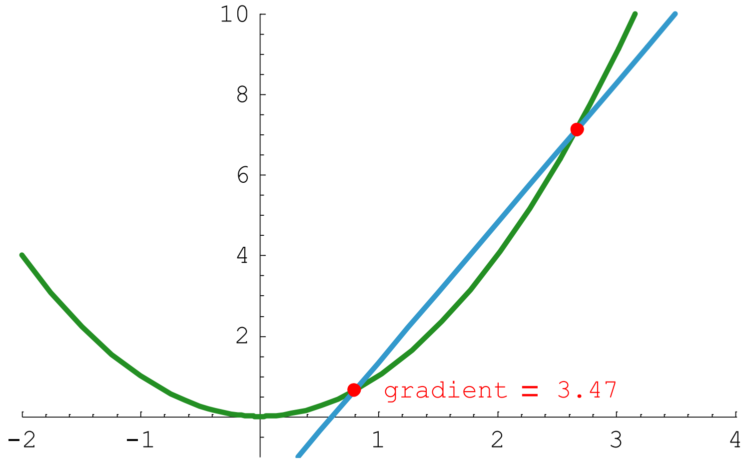
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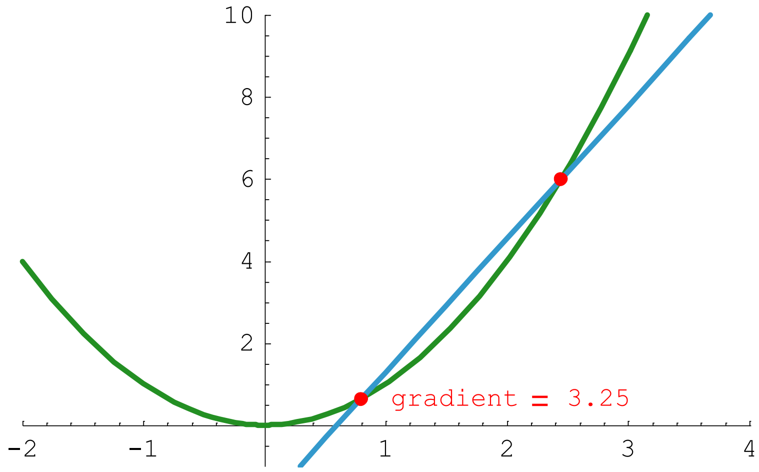
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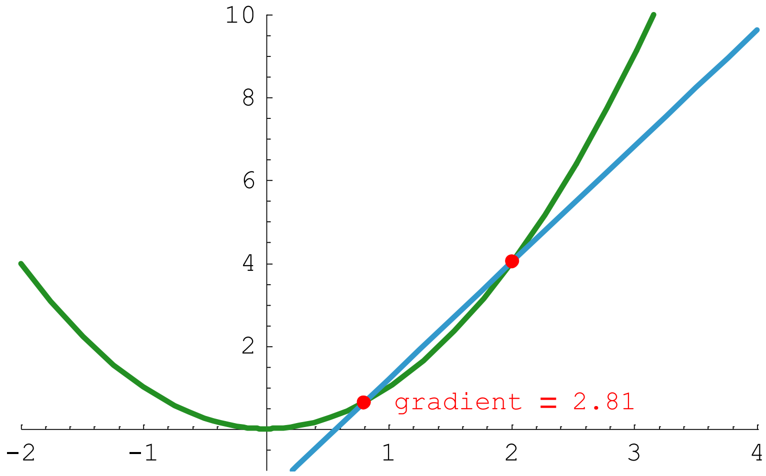
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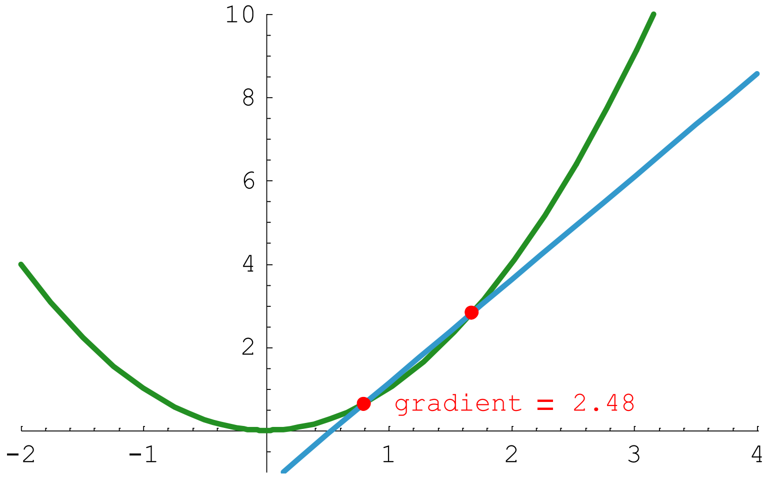
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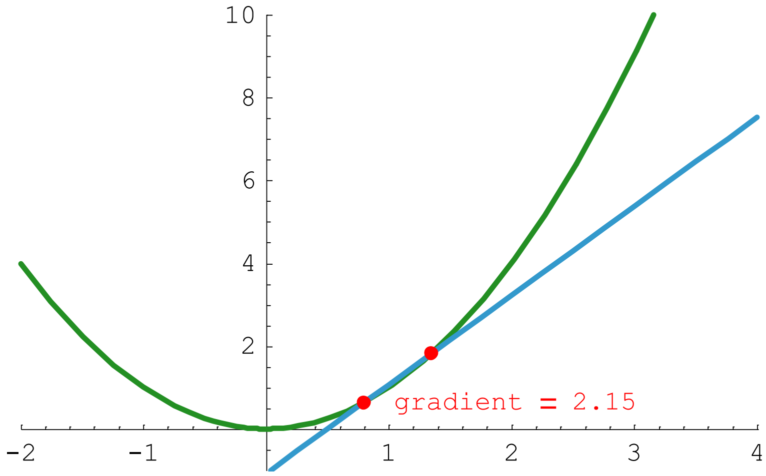
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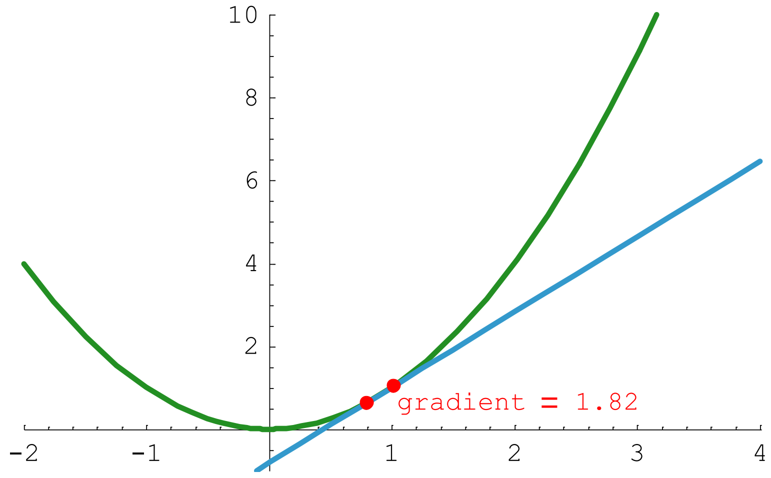

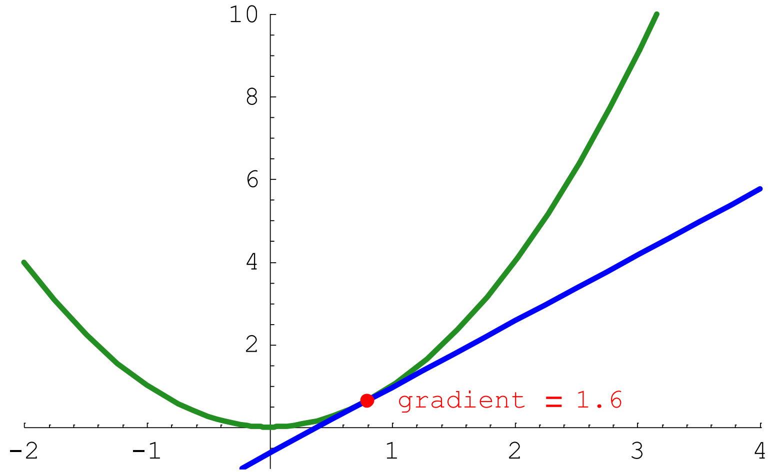

To reiterate, this geometric illustration is inessential to the actual analysis, while the geometric application – of finding the gradient of the tangent – is essential to what is going on. The mathematical illustration – the geometry – at the same time as suggesting the way the mathematics of a rate of change might best be formalised and worked out, does satisfyingly solve the problem of finding a tangent at a point on the curve.

More than that, though, what this *application* of the limit process to the problem of finding a tangent makes clear is the way in which we extend our concept of a rate of change as a ratio of intervals (an *average* rate of change, *e.g.* the average speed over a given time-interval) to an instantaneous rate of change (a rate of change *at* a point, *e.g.* the speed at a given instant of time). It may be possible to see this as a *discovery*, perhaps, of what instantaneous rates of change actually amount to … but seeing the development in action like this certainly predisposes us to see in this case at least the possibility that what counts as an instantaneous rate of change may well depend on the determination – extension in a different sense – of the concept of a rate of change rather than a discovery of the fact of what an instantaneous rate of change *really is*.

That is a good example of how an illustration can be at the same time an application. The 'yawning gulf between illustration and application' is bridged in this case. To spell it out again: finding the gradient of a tangent at a point is at the same time an application of the analytic process of differentiation as well as an illustration of that very process and why we take the process to be what it is. But the idea of a *cut* as determining the concept of a real number, we have seen Wittgenstein to claim, is 'dangerous' in that it has no aspect of possible application built in to it. More, and worse, such free-standing illustrations predispose us to see mathematical concepts – of real numbers in this case – as given somehow in extension *prior* to any application of concepts involving them – a mistaken way of seeing that Wittgenstein convicts Cantor, Dedekind, Hardy – all of us – as being tempted into and as leading to further philosophical error.

With this examination of 'infinity in the small', we have come almost full circle – back to questioning Cantor's *'hocus pocus'*. We go wrong, I am suggesting with Wittgenstein, if we consider the development of mathematical (and other) concepts, including those of *number* and *infinity*, as driven by discovery of facts rather than as determinations of concepts. Further, an important aspect of how such concepts are determined is to be found in their *application*. Lacking application, we may find ourselves at a loss to see how – even implicitly – a particular criterial choice in conceptual development can usefully be made. The choice between factual discovery and conceptual determination as descriptions of mathematical development may itself not be wholly cut-and-dried. (We might consider asking whether 'concept determination' as opposed to 'factual discovery' gets the facts right or is an example of concept determination, should we need convincing of this.) Taken all-in-all, though, the examples I have offered make at least a plausible case for seeing Wittgenstein's distinction as important and illuminating. I have one final example now, to make the point and tie together some of the themes of this chapter.

3.9 Standard and non-standard analysis

The example is that of Non-Standard Analysis (NSA), as developed originally by Abraham Robinson in the early 1960's. I want to set it in the context of what we might call 'standard analysis', particularly the history of differential and integral calculus from Newton and Leibniz up to the present. This is a well-known story, which can be found in any competent history of mathematics.[114] I will not go into all the details; some of the highlights will suit my purposes well enough.

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[114] For instance Kline 1972, Smith 1958, Boyer & Merzbach 1989. See also Fauvel & Gray 1987 for an excellent collection of original sources and references. For a short summary of how the history is conventionally viewed, see Robinson 1996 pp. 260-261.

Robinson himself, at the end of his book in which he explains the technical details of NSA, advances much the same views as those I have been expounding as Wittgenstein's. For example,

> At the moment Cantor's point of view [concerning infinity] is that held by the majority of mathematicians. But … future generations of mathematicians, while accepting the formal results of set theory, may reject the platonistic claims currently associated with it. (Robinson 1996 p. 281)

Likewise regarding the view I have earlier pointed out as shared by Wittgenstein and Fieldian fictionalists, that denial of the *objects* of mathematics might force an emphasis on mathematics as consisting of rules of inference. As Robinson says,

> … we may look at our theory syntactically and may consider that what we have done is to introduce *new deductive procedures* rather than new mathematical entities. (*op. cit.* p. 282)

This introduction of 'new deductive procedures' is, in a sense, a development of relatively new mathematics, as will become plain. However, it may equally well be seen as a *re*-development of some *old* mathematics. Robinson again:

> It is shown in [Robinson (1996)] that Leibniz' ideas [concerning '*infinitesimal* quantities'] can be fully vindicated and that they lead to a novel and fruitful approach to classical Analysis and to many other branches of mathematics. (*op. cit.* p. 2)

One aspect of the history of the development of calculus from Leibniz and Newton to the present day that I want particularly to flag up is the way in which 'the greatest achievement of the seventeenth century'[115], differential and integral calculus, was *used* even though its conceptual grounds were felt to be so shaky for so long. I mentioned above the importance of considering applications in any attempt at a clear overview of conceptual development in mathematics. There is some confirmation in this of my earlier point about the direction of determination of mathematical concepts depending less on intra-mathematical results than on extra-mathematical applications. This is worth keeping in mind as we proceed.

The traditional way of telling the story of the development of the calculus – now denominated part of mathematical 'analysis' – goes as follows. Begun[116] independently in the seventeenth century by Newton and Leibniz, the evident success of the calculus meant it maintained its centrality in spite of dubious foundational aspects, until it was eventually put on a rigorous foundation in the nineteenth century by, especially, Cauchy, Bolzano and Weierstrass. In conceptual

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[115] See Kline 1972 p. 400: ' The greatest achievement of the seventeenth century was the calculus.' Special pleading, perhaps, from a historian of mathematics. Still, that it was a foundational aspect of the scientific revolution and hence of present-day technology is beyond doubt, which surely makes it one of the greatest achievements of the last few hundred years, for good or ill.

[116] 'Discovered'? 'Invented'? The story can be told neutrally, although more often than not the question is thoroughly begged in favour of the former.

terms, the traditional history emphasises Newton's and Leibniz' (and their immediate successors') founding of calculus on notions of the infinitely small; *infinitesimals* (Leibniz) or *fluxions* (Newton). Such foundations having clear infelicities, not to say logical inconsistencies and fallacies such as those pointed out, particularly, by George Berkeley, calculus was only later made rigorous by turning away from such notions and dealing instead very carefully with the idea of a *limit*.

I will give some highlights of this story.

What we might call the first textbook of Differential Calculus was written by Leibniz' disciple the marquis de l'Hôpital; *Analyse des infiniments petits pour l'intelligence des lignes courbes* in 1696. In the preface to the English edition (1730), the translator, Edward Stone, explained how Leibniz followed a path laid out by Newton:

> … upon this latter foundation [i.e. Newton's], is built the Calculus Differentialis, first published by Mr Leibniz in the year 1684 … (Fauvel & Gray 1987 p. 445)

L'Hôpital's (Leibniz') practice, Stone averred, 'does not differ from that of Fluxions', which Newton 'invented … before the year 1669.'(*ibid*)[117] Here is l'Hôpital beginning his '*Analyse*':

> I Demande ou supposition. On demande qu'on puisse prendre indifféremment l'une pour l'autre deux quantités qui ne diffère entr'elles que d'une quantité infiniment petite … (Quoted in Robinson 1996 p. 264)[118]

This notion of 'infinitely small' (*infiniment petite*) is problematic. It is worth looking at part of Berkeley's critique that I mentioned just above to see clearly why. Berkeley's title is indicative of his avowed polemical purpose:

> *THE ANALYST; OR, A DISCOURSE Addressed to an Infidel MATHEMATICIAN. WHEREIN* It is examined whether the Object, Principles, and Inferences of the modern Analysis are more distinctly conceived, or more evidently deduced, than Religious Mysteries and Points of Faith. (Berkeley 1734)[119]

– And here is perhaps the most famous part of this polemic:

> XXXV. I know not whether it be worth while to observe, that possibly some Men may hope to operate by Symbols and Suppositions, in such sort as to avoid the use of Fluxions, Momentums, and Infinitesimals after the following manner. Suppose x to be one Absciss of a Curve, and z another Absciss of the same Curve. Suppose also that the respective Areas are xxx and zzz: and that $z - x$ is the Increment of the Absciss, and $zzz - xxx$ the Increment of the Area, without considering how great, or how small those Increments may be. Divide now $zzz - xxx$ by $z - x$ and the Quotient will be $zz + zx + xx$:

[117] I have no reason to get into the debate about priority here. Nowadays, anyway, it is generally accepted that Newton and Leibniz each independently came up with the calculus.
[118] I translate, 'Requirement or supposition: when two quantities only differ from one another by an infinitely small quantity it is possible to take either one instead of the other.'
[119] The mathematician in question was actually Edmond Halley, disciple of Newton. Berkeley clearly and explicitly intended the strictures to apply equally to Leibnizians, however.

> and, supposing that z and x are equal, this same Quotient will be $3xx$ which in that case is the Ordinate, which therefore may be thus obtained independently of Fluxions and Infinitesimals. But herein is a direct Fallacy: for in the first place, it is supposed that the Abscisses z and x are unequal, without such supposition no one step could have been made; and in the second place, it is supposed they are equal; which is a manifest Inconsistency, and amounts to the same thing that hath been before considered. And there is indeed reason to apprehend, that all Attempts for setting the abstruse and fine Geometry on a right Foundation, and avoiding the Doctrine of Velocities, Momentums, *&c.* will be found impracticable, till such time as the Object and the End of Geometry are better understood, than hitherto they seem to have been. The great Author of the Method of Fluxions felt this Difficulty, and therefore he gave in to those nice Abstractions and Geometrical Metaphysics, without which he saw nothing could be done on the received Principles; and what in the way of Demonstration he hath done with them the Reader will judge. It must, indeed, be acknowledged, that he used Fluxions, like the Scaffold of a building, as things to be laid aside or got rid of, as soon as finite Lines were found proportional to them. But then these finite Exponents are found by the help of Fluxions. Whatever therefore is got by such Exponents and Proportions is to be ascribed to Fluxions: which must therefore be previously understood. And what are these Fluxions? The Velocities of evanescent Increments? And what are these same evanescent Increments? They are neither finite Quantities nor Quantities infinitely small, nor yet nothing. May we not call them the Ghosts of departed Quantities?

That is rather a long quotation, but there is a lot packed into it. There is a hint of the *fundamental theorem of calculus*[120] in Berkeley's consideration of an 'Area' function. It may also be worth noticing his assimilation of 'Fluxions, Momentums and Infinitesimals'; *Fluxions*, Newton's preferred appellation, *Infinitesimals,* that of Leibniz, and *Momentums* (later linked with *Velocities*) pointing up the physical applications of the calculus.[121]

I will eschew further exegetical analysis, apart from dealing with the (in)famous 'ghosts of departed quantities'. It will be worth looking at Berkeley's specific example here to get a clear view of what is going on. I will explain in present-day terms.

Suppose we want to find the instantaneous rate of change or derivative of a function which varies as the cube of its input variable x ; $f(x) = x^3$. Allow x to increase to z, the increment being thereby $(z - x)$. The change of $f(x)$ in this latter interval being thus $f(z) - f(x)$, the *average* rate of change over the interval will therefore be $\dfrac{f(z) - f(x)}{z - x}$, or $\dfrac{z^3 - x^3}{z - x}$. Algebraic division, now, gives us the average rate of change as $z^2 + zx + x^2$ (Berkeley's 'Quotient … zz + zx + xx'.) But we want the *instantaneous* rate of change, for which we need the (fluxion or) infinitesimal

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[120] That integration (finding areas) and differentiation (finding rates of change, or equivalently, as we saw earlier, gradients of tangents) are inverse operations.

[121] Via Newton's Second Law, for instance, linking force with rate of change of momentum.

increment $(z-x)$ to be zero. That is, we should take $z=x$ in our expression for the average rate of change. This gives us the instantaneous rate of change as $3x^2$… *i.e.* $f'(x)=3x^2$, a rule for differentiating that now enables us to find the instantaneous rate of change for any value of *x* without mention of, or ‘independently of Fluxions and Infinitesimals’.

This *works*. We can use such rules for determining ‘Velocities, Momentums, *&c*.’. But, as Berkeley points out, there is a ‘direct fallacy’, a ‘manifest inconsistency’ in the derivation. *z* and *x* are first taken to be unequal, ‘without which supposition no one step could be taken’; we could not divide by $(z-x)$ if *z* and *x* were equal because in that case $(z-x)$ would be zero. Then, later in the derivation, we take *z* and *x* to be equal in order to obtain $3x^2$ from the quotient. So Berkeley is well within his rights to cavil at the derivation; ‘… what are these same evanescent Increments?’ Recall from above l’Hôpital’s ‘requirement’ that in such cases ‘we may take’ it that quantities that differ by an amount ‘infinitely small’ are the same. In practice, this requirement can be seen to be manifestly inconsistent. Either they are the same or they are not the same. In the former case, we may not divide by their difference … but we need to do the division in order to get started.

Here I am going to cut matters short and jump centuries to the present day, or at least to the twentieth century, when, following the ‘rigorisation’ of the calculus by nineteenth century mathematicians, mathematical analysis in its standard form is based, not on infinitesimals or ‘infinitely small quantities’, but on the concept of a limit. Any standard analysis text has near the beginning the definition of a limit, followed by definition of a derivative in terms of a limit.[122]

Using Berkeley’s *x*’s and *z*’s,[123] we will have, in standard analysis, that the derivative or instantaneous rate of change of a function *f* for input *x*, that is $f'(x)$, is defined as follows:

$$f'(x) \stackrel{def}{=} \lim_{x\to z}\left(\frac{f(x)-f(z)}{x-z}\right),$$

where the ‘ $\lim\limits_{x\to z}$ ’ in this is explained as follows:

$$f'(x)=\lim_{x\to z}\left(\frac{f(x)-f(z)}{x-z}\right) \Leftrightarrow \forall\varepsilon>0\exists\delta: 0<|x-z|<\delta \Rightarrow \left|\frac{f(x)-f(z)}{x-z}-f'(x)\right|<\varepsilon .$$

This is commonplace to first-year mathematics undergraduates, while they will be warned that the ‘intuitive’ way of looking at this, namely that when $(x-z)$ is

122 *E.g.* Spiegel 1963. See p. 23 and p. 57, respectively. We will find similar definitions of integrals, *mutatis mutandis*.

123 Berkeley has the *x*’s and *z*’s reversed here and there – that does not make any difference, though.

‘infinitely small’, $\frac{f(x)-f(z)}{x-z}$ is ‘infinitely close’ to $f'(x)$, is to be eschewed, at least as regards any proofs that are required, essentially on grounds identical to those advanced by Berkeley.

Robinson’s Non-Standard Analysis reinstates this ‘infinitely close’ and ‘infinitely small’ talk – talk of ‘infinitesimals’ – thereby vindicating Leibniz in the light of Berkeley’s critique. The existence of infinitesimal quantities, Robinson shows, can consistently be asserted while keeping the rest of standard analysis. That, essentially, is what NSA does.

In very basic terms, here is how NSA works.[124] Start with Gödel’s completeness theorem.[125] That is, if ***K*** is a set of first-order sentences with language ***L***, then ***K*** has a model if and only if it is consistent. Next, note that it follows from this that such a ***K*** is *compact* (or, as Robinson puts it, it satisfies the *Finiteness Principle*) – *viz.* if every finite subset of ***K*** has a model/is consistent, then ***K*** itself has a model/is consistent. (The proof of this latter is straightforward enough. Suppose the contrary, that every finite subset of ***K*** is consistent but ***K*** is inconsistent. Since ***K*** is inconsistent it contains a contradiction; i.e. there must be some sentence S of ***K*** whose negation is provable. But the proof of (not-S) can only have a finite number of sentences, and by hypothesis, this finite set containing (not-S) – call it ***K′*** – must be consistent. Adding S to ***K′*** gives another finite – so consistent – set. But that is contradictory since ***K′*** contains S and its negation.)

Now, suppose we have a theory of real numbers, that is a set of sentences satisfied by the real numbers.[126] Call this set of sentences *R*. And suppose we add a constant symbol *a* to our language ***L***. Consider now the (infinite) collection of sentences ‘ $|a|<1$’, ‘$|a|<\frac{1}{2}$’, ‘$|a|<\frac{1}{3}$’… If we add all these sentences to *R*, it is clear that every finite subset of this enlarged theory has a model, namely the real numbers. So every finite subset is consistent, and thus, by compactness, the whole enlarged theory is consistent. Hence there is a consistent enlargement of *R* that contains an *a* that satisfies every one of the sentences ‘$|a|<1$’, ‘$|a|<\frac{1}{2}$’, ‘$|a|<\frac{1}{3}$’ … that is, there is in this enlargement, **R*, a ‘number’ *a*, non-zero but smaller than any real number.

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[124] There are other, often simpler, derivations, available nowadays. I give a simplified version of Robinson’s own version here, more or less, with the aim of keeping the motivation close to the surface. The important ideas are easy enough to follow.

[125] Proved initially as his doctoral dissertation in 1939. There are, of course, later, more easy-to-follow proofs available now. Any appropriate logic text will have one.

[126] Here we have to be careful not to go higher order. Robinson gives a framework which amounts to keeping to first order by means of ‘type transforms’, essentially mirroring appropriate sentences of higher type with ones of lower type. See Robinson 1996 pp. 19*ff*.

Robinson:

> A number $a \in {}^*R$ will be called *infinitesimal* or *infinitely small* if $|a| < m$ for *all* positive numbers *m* in *R*. (1996 p. 56)

That does it, essentially. We can do calculus in $^*R$, consistently using infinitesimals … and, as Robinson goes on to prove, we will get the results we expect, that is the results that we now prove by means of the translation into talk of ε / δ-type limits as in the explanation of $f'(x)$ just above.

To what extent this vindicates Leibniz and shows Berkeley to have been wrong is certainly debateable. I am not concerned to adjudicate such issues. However, I do want to consider morals to do with what I have been advancing. I have been arguing that we should consider looking at conceptual development, particularly in mathematics, in Wittgensteinian terms as *determination* of concepts rather than as – what we might think of as the Platonist opposition – *discovery* of facts, generally perhaps about a realm of abstract objects, although not tied exactly to such a picture. We have seen that with regard to developments of the notion of infinity, it might appear that Cantor *discovered* that $\aleph_0 < \aleph_1$ and so on, whereas Wallis was simply wrong to think of a number, infinity ∞, as the reciprocal of zero. My suggestion, though, has been rather to consider that whereas Wallis's conception was one that did not catch on – did not quite *take* – the later, Cantor manner of development *did* catch on. The development of Non-standard Analysis, seen against the conventionally-told history of standard mathematical analysis, further bolsters the philosophical utility of considering which concepts catch on and are accepted by mathematicians and scientists.

Mathematics we know, is useful, particularly in science. Non-philosophically-inclined mathematicians and scientists might well just leave the matter there. The philosophically minded, though, may well be inclined to ask further, of mathematical objects, whether they really (or *really*, or even *Really*) exist, or whether mathematical propositions are really (etc.) true. Mathematical realism can be seen as answering the question, 'are there *Really* such things as numbers?' in the affirmative, and likewise, the associated (but different) question as to whether mathematical propositions are *Really* true. Mathematical fictionalists or other anti-realists demur: 'there are no such things as numbers', says the fictionalist, although they may well be useful fictions. Are there, then, *Really* such things as infinitesimals? Traditionally, and in the light of such strictures as Berkeley advanced so forcefully, the answer has been 'no' … but given the utility of apparently assuming otherwise in the calculus, a work-round was required. Hence the attempts to 'add rigour' to the infinitesimal calculus. Robinson's Non-Standard Analysis, now, might appear to give succour to the realist; were there (*Really*) infinitesimals all along? Robinson, we have seen, does not take the view that this is the case, and nor should we. *Whether or not* we allow the existence of infinitesimals, we can use the calculus for the purpose it was intended – and, indeed, for lots of other purposes as well. The history I have outlined, rather than supporting either realism or antirealism about mathematics, suggests a way of seeing the traditional realism/antirealism philosophical debate as empty.

The notion that matters could perhaps have gone differently, that concept-determination might have taken a different turn with Wallis' ∞ and/or Cantor's $\aleph$'s, is bolstered once we are convinced, as we surely should be, that Leibnizian infinitesimals *could have* been accepted all along. We have also seen with this last example how the importance of the application of mathematical concepts can even overcome severe doubts about what looked like sheer inconsistencies in the concepts used in application. All of this, I am suggesting, is indicative of how we may be better off adopting a view of mathematical conceptual development, not as following some somehow predetermined course dictated by who-knows-what form of mathematical reality, but rather as determined, ultimately though often tacitly and inexplicitly, by mathematical utility in application.

There is more to be said about this, and I will consider further such questions as whether or not there are '*Really*' such things as infinitesimals – or numbers and mathematicalia in general – in Chapter 5. Before getting to that, though, I want to raise what might look like a serious objection to the view I am advocating. In brief, if mathematical conceptual development can go in different ways depending on the whim of mathematicians – or even by the way the empirical world is to be best described – then it may start to seem there is no necessity to mathematics. It looks, that is, as though the normative force of mathematics, its apodictic necessity – what Wittgenstein calls 'the hardness of the logical must' as applied to mathematics – is in danger of getting lost if we follow Wittgenstein on this. This is what I want to treat in Chapter 4.

Chapter 4 The Hardness of the Logical 'Must'

4.1 Introduction

The question with which I began the last chapter, 'Just what *is* it to take the notion of infinity at face value as part of mathematics?' has been illuminated by Wittgenstein's emphasis on concept determination as opposed to discovery of facts. However, in recognising differences between 'the kind of referring' involved, say, in talk of numerals referring to numbers compared with how names of concreta refer to physical objects, we are not forced to establish any anti-realist notion of numbers and other mathematicalia once we have turned away from realism in this recognition. Emphasising that this is so – and how it can be so – will be part of the object of the current chapter.

We have seen how we can accept the imperative to take our talk about mathematics 'at face value' as suggested by Field *and* turn away from realism; it did not follow that we needed to follow Field into fictionalism or indeed to accept metaphysical anti-realism of any stripe. And there is also a related point we should consider. We saw that Berkeley, in effect justifiably, convicted Newton and Leibniz and other 'analysts' of 'manifest inconsistency' in their founding development of the calculus. Robinson, with NSA, was able to overturn this verdict and give us a way to allow the consistency of infinitesimals. This example gives us a clear sense in which the 'vindication' of Leibnizean ideas amounts to a change in what is to count as *consistency* in mathematics. For, consider: it is plain that early attempts at conceptual foundations for the calculus foundered on specific inconsistencies; Berkeley, in pointing out these inconsistencies, was pushing at an open door. But, then, as we have seen, following the establishment of Robinson's NSA we can accept that Leibnizean infinitesimals are perfectly consistent after all. This amounts to a change in what is to *count* as consistent in mathematics – and it will be no surprise if I suggest we view such a change more in terms of a conceptual determination than as a factual discovery of whether such-and-such is really consistent or what consistency itself really (or *really* or *Really* ...) is. This fits well with Robinson's own claim that

> … what we have done is to introduce *new deductive procedures* … (Robinson 1996 p. 282. See above section 3.9.)

– Notions of consistency mesh closely with those of logical consequence, after all. (We saw some of the details of this in Chapter 2.)

However, seeing the development in this way emphasises an apparent difficulty. It seems we run the risk of losing the *objectivity* of mathematics once we allow that what is to count as correct in mathematics is determined by ourselves rather than discovered, even if such determination comes about, as I have been suggesting, mostly by tacit agreement rather than express fiat. And this risk seems all the more acute if we allow that what counts as logical consequence or consistency is equally dependent on human agreement. So, to the risk of loss of objectivity, it now further seems, is added the risk of the loss of mathematical *necessity*, what Wittgenstein often calls 'the hardness of the logical must'.

To get to grips with both these points, I will look again at both Platonism and the particular opposition thereto involved in mathematical fictionalism. We have seen how a turn away from Platonism can lead to an emphasis on mathematics as consisting of rules of inference rather than as a collection of assertions about abstract objects in our investigation of Hartry Field's epistemology of mathematics as well as in following Wittgenstein. It will be useful again to set off the view I want to advance against Field-style fictionalism. Also, however, for more general contextualisation of our investigation of objectivity, I want first to turn to Frege again. Recall that above in Chapter 2 Field's own view was illuminated by a contrast with Frege and Crispin Wright's Fregean critique. A similar contrast can serve well at this point too, particularly in the light of our concern with mathematical applicability.

4.2 Fregean objectivity

Frege was concerned with mathematical objectivity in contrast to what can be had from human psychology. For example, as he claims:

> … the number one … can no more be researched by making psychological observations than can the moon. (Frege 1964 p. 16)

He also asserted that mathematical objectivity is on a par with that of physics:

> Whether *y* follows … after *x* has in general nothing to do with our attention … it is a question of fact, just as much as it is a fact that a green leaf reflects light rays of certain wave-lengths …(Frege 1953 p. 37)

Elsewhere, Frege is explicit about the importance of the applicability of mathematics for an assessment of its status:

> … it is applicability alone which raises arithmetic from a game to the rank of a science. So applicability necessarily belongs to it. (Frege 1952 p. 167)

In this latter quotation Frege is arguing explicitly against the *formalist* idea that

> Arithmetic is concerned only with the rules governing the manipulation of the arithmetical signs, not, however, with the meaning of the signs. (Frege 1952 p. 164)

Pulling together these thoughts about applicability and objectivity, one might put the point as follows. If mathematics – as opposed to a game such as chess, for example[127] – is to have any application, it needs to have genuine content. But

[127] Frege's own example. For instance, he says, 'The reason [arithmetic renders services to natural science] can only be that numerical signs have meaning and chess pieces do not.' (*op. cit.* P. 165.) Wittgenstein, of course, draws significantly non-Fregean morals from the

applicability to the real world entails that this genuine content must be *objective* content, which cannot be supplied from human psychology. The only thing that *can* supply such genuine objective content, according to Frege, is the existence of abstract mathematical objects whose objective properties form the subject-matter of mathematical investigation.[128]

A Fregean, then, seeing the need to account for the fact of mathematical applicability, infers that mathematics must be objectively true. And this objective truth, for the Fregean, then further involves a commitment to the existence of abstract objects – to Platonism. This argument from applicability to Platonism should be familiar; it is very similar to what I characterised at the beginning of Chapter 2 as the '*Quine/Putnam* indispensability thesis', the argument from the utility of mathematics in science to the truth of mathematics and hence to mathematical realism of one form or another.

4.3 Objectivity and indispensability

This brings us nicely back to Field. As we saw earlier, Field sees such arguments from applicability[129] as unique in the field; and his fictionalist project proceeds by tackling the argument from applicability to truth. As he goes on to say following the quote I gave above in section 2.1,

> … 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*)

Field's project, as we saw, attempts to account for applicability other than in terms of truth. In his words, 'a mathematical theory needn't be true to be good', and he needs to explain how mathematics *can* be good – applicable – without being true, which, as we have seen, he does by recourse to the notion of *conservatism*, a particular kind of ('strong') consistency.

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same example. See, for instance, *PI* § 563: 'Let us say that the meaning of a piece is its role in the game. …' 'Meaning' has different import in the two cases, it is clear.

[128] Once swallowed, this idea of the dependence of mathematical objectivity on a Platonist ontology can easily cut itself loose from its roots in applicability. I mentioned G.H. Hardy above as a particular foil for Wittgenstein; Hardy praises 'real' mathematics for its very lack of application. See, for instance, his 1992, *passim*. (For instance, 'Real mathematics,' says Hardy, '… must be justified as art if it can be justified at all.' (p. 139)) Wittgenstein sets his face firmly against this, as we might expect. See *RFM* p. 257, for example: 'I want to say: it is essential to mathematics that its signs are also employed in mufti. It is the use outside mathematics, and so the meaning of the signs, that makes the sign-game into mathematics.' Mathematics, that is, according to Wittgenstein, needs application in order to make sense as mathematics.

[129] Field himself writes in terms of '*Quine/Putnam* indispensability'. See, for instance, in his 1980: '… there is one and only one serious argument for the existence of mathematical entities and that is the Quinean …' (p. 5)

If I am worried about the philosophical problem of mathematical applicability, as indeed Field is,[130] I will naturally be concerned, in the same way as we have seen in Frege, with the objectivity of mathematics. Field sometimes talks of the 'non-arbitrariness' of mathematics rather than its objectivity, but we can take this 'non-arbitrariness' as a brand of objectivity. He says, for instance,

> There can be no doubt that the axioms of, say, real numbers … are non-arbitrary; (1980 p. 5)

– and he goes on to claim that this non-arbitrariness is to be explained in terms of the applicability of the relevant parts of mathematics:

> … and an explanation of their non-arbitrariness, based on their applicability to the physical world … will be given …(*ibid.*)

Field's 'non-arbitrariness' amounts to a kind of objectivity-without-truth. His explanation of how mathematics can be non-arbitrary whilst not being true goes via appropriate representation theorems, allied with the conservatism of mathematics. Mathematical applicability, according to Field, can be explained by proving a representation theorem asserting the existence of a homomorphism guaranteeing a 'fit' between the physical world and a mathematical theory applied to it. The non-arbitrariness of the mathematics comes to the surface in that the structure of the mathematics just has to be such as to 'fit' the physical world in the way specified in the statement of any such representation theorem.[131] And Field's claim is that given the conservativeness of mathematics over the physics it thus 'represents', talk of the truth of the mathematics involved is superfluous.

I have already entered a challenge to the coherence of this story in Chapter 2 above.[132] Further details and an extension of the challenge come in Chapter 5. Here I am interested in telling the story again to illustrate how a concern with mathematical applicability pushes considerations of objectivity – and, as we will soon see, necessity – to the fore with Field and his fictionalism as much as with Frege or any Platonist.

4.4 Necessity without truth

So I want to point up the connection between objectivity and necessity in the light of Field's fictionalism and the way it meshes with his account of mathematical applicability. We have seen already the way Field's turn away from truth leads him to see mathematics in terms of inference-rules rather than as descriptive of a world of mathematical objects. We have also seen how his epistemology of mathematics

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[130] Recall Field's view 'that [the] question ['What sort of account is possible of how mathematics is applied to the physical world?'] is the really fundamental one.' (Field 1980 p. *vii*)

[131] We saw this at work in Chapter 2.

[132] See section 2.4.

hinges on questions of logical inference. This being so, it should have come as no surprise to find in Field a strong link between the objectivity of mathematics and its status as *necessary*. Indeed, as we will recall,[133] he goes so far as to describe conservativeness – the property of mathematics that enables it to be applicable without being true, recall – as ‘necessary truth without truth’:

> Conservativeness might loosely be thought of as ‘necessary truth without the truth’. A conservative theory, like a necessarily true one, is compatible with any possible state of the physical world; the only real difference between a conservative theory and a necessarily true one is that the conservative one need not be true at all. (Field 1989 p. 59)

More than this, Field claims, in a later paper specifically dealing with mathematical objectivity,[134] that mathematical objectivity amounts to nothing more than an application of the objectivity of logic. The article, he says, defends the view that

> … logical objectivity is all the objectivity there is. (Field 2001 p. 331)[135]

We should not be surprised at any of this. As we have seen before, once we turn away from the picture of mathematical objectivity as depending on mathematical objects and take account of the requirement that mathematics be applicable to the physical world, we will be primed to see mathematics in the light of something at least very much like *TLP* 6.211,[136] as a collection of inference rules for transforming empirical propositions. And rules are normative: there is a sense of ‘must’ in which if I am to follow a rule I must do what the rule specifies. Whether we gloss it as logical necessity of some sort, or as some other kind of necessity, it is clear that this normative ‘must’ is closely allied to the requirement that mathematics be objective given the possibility – often actualised – of its being applied. Necessity, then, broadly construed, requires serious consideration in the light of this requirement for mathematical objectivity, which in turn gets its major importance from the fact of mathematical applicability.

4.5 Wittgenstein and necessity

For both Frege and Field then – for Platonist and fictionalist alike – the objectivity of mathematics is tied up with logical necessity. For Frege this objectivity is mediated by the existence of abstract objects, but as Field points out, such mediation is unnecessary: mathematical objectivity does not require mathematical objects if we

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[133] From Chapter 2, see section 2.2.
[134] ‘Mathematical Objectivity and Mathematical Objects’, reprinted in Field 2001 pp. 315*ff*.
[135] We should not take Field to be denying the objectivity of empirical statements here. In context, all that is in question is whether there is a kind of mathematical objectivity ‘that transcends logical objectivity’, which Field denies.
[136] See above, section 2.5.

concentrate on the *use* we make of mathematics. However, Field simply *assumes* that logic itself is objective: he says

> From now on I will be assuming that logic, hence mathematical *proof*, is fully objective. (*ibid.*)

It would be unfair simply to charge Field with an unjustified assumption, as this might seem to imply. Elsewhere he does take up the issue of the *apriority* of logic, connecting with issues of objectivity.[137] His 'evaluativism' about *apriority*, he says, in fact, 'does allow for a sort of moderate relativism' (Field 2001 p. 384), which might seem to contradict the assumption of objectivity we have seen him to make above. I do not want to develop this just here, but simply point out, once again, that here as elsewhere Field's analysis – or, perhaps better, the *overview* he develops – comes close to the one I pursue via Wittgenstein's, though without Field being able to take the crucial final step away from theoretical baggage.

Field contrasts what he assumes about the objectivity of logic with something he finds in Wittgenstein – something I want to challenge. Field says,

> … one might hold with Wittgenstein 1956 (see also Kripke 1982) that there is something unobjective about the drawing of consequences in a formal derivation procedure. (Field 2001 p. 316)

This gets us close to the considerations I raised in the introduction to the current chapter. I argued in Chapter 3 that we should take on board Wittgenstein's conception of mathematics developing through concept-determination rather than discovery. And I have agreed, with Wittgenstein, that doing so raises questions about the possible loss of the objectivity of logical inference. However, Field gets Wittgenstein wrong in claiming he holds there is 'something unobjective' about the drawing of consequences in mathematics. Although it is true that Wittgenstein often points out that such a conclusion seems to follow from the considerations he advances, nevertheless the conclusion is one he explicitly denies, often in the same breath. For instance, in *RFM* we find

> What you say seems to amount to this, that logic belongs to the natural history of man. And that is not combinable with the hardness of the logical "must". (*RFM* p. 352)

That is clear enough in our own context. If mathematics – including mathematical inference – develops via concept-determination in the way I have canvassed, it does seem as though there is a kind of human agreement underlying such development. And it does also seem as though we cannot base logic on human agreement without losing the objectivity of that logic. It seems that mathematical and logical necessity – 'the hardness of the logical "must"' – will ineluctably be softened once we allow

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[137] See his 'Apriority as an Evaluative Notion', 2000, reprinted in Field 2001, pp. 361*ff*. (Earlier, at least until 1989, *Realism*, *Mathematics and Modality*, Field wrote of '*a prioricity*'. Even top-notch philosophers occasionally have cloth ears for their mother-tongue.)

ourselves to simply *agree* to take one form of inference rather than another. However, concerning what 'seems' to be the case, Wittgenstein continues:

> But the logical "must" is a component part of the propositions of logic, and these are not propositions of human natural history. (*RFM* p. 353)

This sets out the problem we are considering rather than bypassing it or claiming it simply solved: and there is a reading of Wittgenstein in this regard that contradicts Field's assertion that Wittgenstein simply accepts the unpalatable conclusion of the denial of the objectivity of mathematics and logic that *seems* to follow from the considerations I have advanced regarding the distinction to be drawn between determination of concepts and discovery of facts. To draw out this reading – and, I hope, to convince the reader that Wittgenstein is largely right in its regard – I am going to draw on an extant controversy in the philosophical literature around Wittgenstein exegesis on this point.

4.6 Conventionalism

The controversy centres around whether, or to what extent, Wittgenstein advances a *conventionalist* view both of logical and of mathematical necessity. I should make the point, first, that Wittgenstein does indeed consider both kinds of necessity under the same head:

> "But doesn't it follow with logical necessity that you get two when you add one to one, and three when you add one to two? and isn't this inexorability the same as that of logical inference?" – Yes! it is the same. (*RFM* p. 38)

Further, the way he continues here is relevant for the dialectic I am engaged in and about to outline in more detail:

> – "But isn't there a truth corresponding to logical inference? Isn't it true that this follows from that?" – The proposition: "It is true that this follows from that" means simply: this follows from that. And how do we use this proposition? – What would happen if we made a different inference – how should we get into conflict with truth?
>
> How should we get into conflict with truth, if our footrules were made of very soft rubber instead of wood and steel? – "Well, we shouldn't get to know the correct measurement of the table." – You mean: we should not get, or could not be sure of getting, that measurement which we get with our rigid rulers. So if you had measured the table with the elastic rulers and said it measured five feet by our usual way of measuring, you would be wrong; but if you say that it measured five feet by your way of measuring, that is correct. – "But surely that isn't measuring at all" – It is similar to our measuring and capable, in certain circumstances, of fulfilling 'practical purposes'. (*ibid.*)

The point Wittgenstein makes here can appear, like much of what he writes, allusive, not to say gnomic. I will take the space to illuminate it below. Before doing so, I first note that this allusive aspect of Wittgenstein's methodology might usefully be pinned to something I have already mentioned several times, namely the claim that we are not forced to offer an *alternative* to Platonism once we find it lacking in explanatory force. Making the point here gives us the possibility of a diagnosis of just why it is that, as I argue, much of Wittgenstein exegesis goes wrong: the almost overwhelming desire to systematise, to offer theories in philosophy, lies at the bottom of the error I want to outline.

Wittgenstein is not a Platonist like Frege, that much is evident. He is not, either, a fictionalist in the style of Hartry Field. The strong temptation, then, is to find some other kind of '– ist' to fill the gap in an assumed theory-space of mathematical and logical necessity.[138] Strong though it is, this temptation is to be resisted.

4.7 Dummett vs Stroud

This latter temptation, however, beguiles Michael Dummett, for one. In his review[139] of Wittgenstein's *Remarks on the Foundations of Mathematics*, Dummett claims

> If … one regards [platonism and the various varieties of constructivism] as rivals, there remains the philosophical problem of deciding which of the various accounts is correct. Wittgenstein's book is intended as a contribution to the latter task only. Dummett 1978 p. 167)

Dummett himself, it seems safe to say, wants an *account* of necessity; his reading of Wittgenstein criticises Wittgenstein in trying to arrive at such an account. But Dummett does not allow the possibility that Wittgenstein is not himself attempting an account of necessity that fits the theory-space, 'platonism-and-its-rivals' model of how philosophical investigation might or should proceed. In missing this, Dummett misses a key aspect of Wittgenstein's thought, one that gets its clearest expression in *Philosophical Investigations* §109,

> … we may not advance any kind of theory. …

The denial of the possibility of philosophical theorising is ubiquitous in Wittgenstein's thought and is one of the most radical aspects of his philosophy, as well as one of those most often ignored by exegetes. To give just one more example, Mark Balaguer writes:

> … regardless of what we say about the *details* of Wittgenstein's view [of mathematics], we will surely want to say that it is a version of anti-realism … (Balaguer 1998 p. 197)

It certainly seems difficult for philosophers to accept Wittgenstein's own protestations about the impossibility of theories in philosophy. But at the very least it seems clear that (1) in his most careful exposition of his own views (in *Philosophical Investigations)* he *said* we may not advance any theories; and (2) anti-realism is a theory just as much as Platonism or other forms of realism. Maybe

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[138] Michael Beaney reminded me at this point of Gilbert Ryle's famous rejection of 'isms' in philosophy, which he may well have got from Wittgenstein.
[139] Reprinted, for instance, in Dummett 1978 pp. 166*ff*.

Wittgenstein was inconsistent; but I suggest it is worth looking for a reading of what he says that makes what he says consistent. Stroud, at least, does that.

By contrast to Dummett, Stroud suggests a way of reading that sees Wittgenstein as giving a descriptive critique of the notion of mathematical necessity rather than a theoretical account such as Dummett feels is required. I am going to follow this controversy through, in the hope of gaining a clear overview of mathematical necessity in the light of how I have described conceptual development in mathematics.

It will help keep things clear if we consider also some commentary on the controversy by Hilary Putnam. The focus should be on the debate itself rather than on who says what; the to-and-fro of the dialectic, however, is clarified by Putnam's interjections.

There is also another reason for allowing Putnam a say. These latter 'interjections', in fact, come from the collection of Putnam's papers, *Realism and Reason,* published in 1983 as *Volume 3* of his collected philosophical papers at the time. There he claimed that

> … the Wittgensteinian views that (1) mathematical statements do not express objective facts; and (2) their truth and necessity (or appearance of necessity) arise from and are explained by *our* nature, cannot be right. (Putnam 1983 p. 126)

Characteristically, however (he is famous for changing his mind), Putnam later adopted a view closer to the one I advocate rather than what he expresses here and in what he says in and around the passages I quote from this collection. In his 'Was Wittgenstein *Really* an Anti-realist about Mathematics', written some decades later, for instance, he says, *approvingly* of Wittgenstein (and with a hat-tip to Warren Goldfarb),

> … the Wittgensteinian strategy, I believe, is to argue that while there is such a thing as correctness in ethics, in interpretation, in mathematics, the way to understand that is not by trying to model it on the ways in which we get things right in physics, but by trying to understand the life we lead with our concepts in each of these distinct areas. (Putnam 2001 p. 186)

This is very close to what I argue; and there is much to gained from following through Putnam's expression of his own conversion on this and related matters.[140]

Putnam's earlier views are worth considering for the help they offer in clarifying the (Dummett's) opposition to Wittgenstein and Stroud, however. But in case there should be any doubt, I should say that I support Stroud's view as well as that of this *later* Putnam, both as exegesis of Wittgenstein on necessity and as giving us at least the beginnings of the clear overview we are seeking.

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[140] Although I do not think that Putnam gets Wittgenstein right about mathematical applicability.

4.8 Full-blooded Conventionalism

Now the ground is cleared and the protagonists have their places, let us begin with the promised debate. It is easy to see that one way of denying the objective nature of some practice is to see it as in some way bound by convention rather than determined by something independent of the practitioner. With regard to mathematics and logic,[141] Dummett characterises Wittgenstein as a 'radical' (or 'full-blooded') 'conventionalist'. Why 'radical'? Why not simply 'conventionalist'? – Dummett wants to situate Wittgenstein's view by contrasting it with a different, 'moderate' or 'modified' conventionalism. Dummett claims

> A great many philosophers nowadays subscribe to some form of conventionalist account of logical necessity … The conventionalism that is so widespread is, however, a modified conventionalism. (Dummett 1978 p. 169)

'Modified' conventionalism is the kind of conventionalism, Dummett says, with which we are familiar from logical positivism. The idea is as follows. Some basic mathematical 'truths' (the 'axioms' of some mathematical theory) are held to be true by convention: then mathematicians have the task of finding out the consequences of adopting such conventions or stipulations.

We do not need to consider *just* axiomatic approaches to see moderate conventionalism at work. Here is Putnam explaining what *he* calls a 'first approximation' to Wittgenstein's view:

> when we make a mathematical assertion, say ' 2 + 2 = 4', the 'necessity' of this assertion is accounted for by the fact that we would not *count* anything as a counterexample to the statement. The statement is not a 'description' of any fact, but a 'rule of description' …(Putnam 1983 p. 115)

Putnam finds a problem with this view as so baldly expressed. It is one that, he says, 'Wittgenstein clearly points out' (*ibid.*), and it is also one that Dummett finds. It is this: there are just too many possible mathematical assertions.[142] So most of them can only be *consequences* of the conventions – for Dummett, of the conventionally stipulated axioms of the theory. Why is this a problem? Because of course we can ask how consequences are to be drawn – and, equally of course, we reply that they are to be drawn with logical necessity. But then, as Putnam points out,

> The 'exciting' thesis that logic [and mathematics] is true by convention reduces to the unexciting claim that *logic is true by convention plus logic*. No real advance has been made. (*ibid.*)

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[141] And in other regards too. Dummett rightly sees Wittgenstein's remarks on mathematics as applying more generally if not universally. See, *e.g.* Dummett 1978 p. 185, 'It is clear that considerations of this kind … are of quite general application'. I narrow the focus here, though, to keep matters simple.

[142] Perhaps 'so-called' assertions would be appropriate here – it is not clear that 'rules of description' count as being assertions *simpliciter*. Let that pass for now, as Putnam does.

‘Moderate’ conventionalism gets us nowhere, in short. So, according to Dummett, Wittgenstein adopts a ‘full blooded’ conventionalism according to which each and every true mathematical statement[143] is itself necessarily true directly by convention:

> That a given statement is necessary consists always in our having expressly decided to treat that very statement as unassailable; it cannot rest on our having adopted certain other conventions which are found to involve our treating it so.
> (Dummett 1978 p.170)

Radical conventionalism, in Dummett’s terminology, then, involves that every individual statement that is held to be necessary is so held by direct convention – one (necessary) statement, one convention, we might say. ‘For [Wittgenstein],’ says Dummett,

> The logical necessity of any statement is always the *direct* expression of a linguistic convention. (*ibid.*)

It is true that Wittgenstein does sometimes write like this. *PI* §186, for instance:

> So when you gave the order + 2 you meant that he was to write 1002 after 1000 … It would almost be more correct to say, not that an intuition was needed at every stage, but that a new decision was needed at every stage.

Or, one of many examples from *RFM*,

> “I have a particular concept of the rule. If in this sense one follows it, then from that number one can only arrive at this one”. That is a spontaneous decision. (p. 326)

We should be wary. ‘It would *almost* be more correct to say …’: this is qualified, to say the least. We may have to agree with Dummett that the account he gives of Wittgenstein’s view is ‘very difficult to accept’ (Dummett 1978 p. 170), ‘extremely hard to swallow’ (p. 173) and so on. But I want to suggest that we do not take Dummett’s exegesis as Wittgenstein’s own view. Here, as I said above, I want to call on Barry Stroud’s challenge to Dummett’s reading of Wittgenstein on mathematical necessity.

4.9 Stroud on Dummett on Wittgenstein

Putnam gives us a useful clear summary of Stroud’s counter to Dummett:

> Barry Stroud pointed out that the position Dummett calls ‘radical conventionalism’ cannot possibly be Wittgenstein’s. A convention, in a literal sense, is something we can legislate either way. Wittgenstein does not anywhere say or suggest that the mathematician proving a theorem is *legislating* that it shall be a theorem …

[143] Again, it may not be appropriate to describe mathematical statements as simply ‘true’ if they function as rules of inference rather than as descriptive propositions. Dummett makes nothing of this. Wittgenstein does, and so does Field, in a different way, we have seen. I will not follow this out just here.

> Basing himself on a good deal of textual evidence, Stroud suggested that Wittgenstein's position was that it is not *convention* or *legislation* but our *forms of life* (i.e. our human nature as determined by our biology-plus-cultural-history) that cause us to accept certain proofs *as* proofs. (Putnam 1983 p. 117)

'Legislating either way': this is at the crux of the matter. On the one hand, the very necessity of mathematics and logic makes it impossible that things should be other than they are, mathematically or logically speaking. That is just what necessity amounts to: *p* is necessarily the case just when it is impossible for it not to be the case that *p*.[144] On the other hand, if we are to consider ourselves as legislating necessity – as *deciding* what is to count as necessary – we need there to be the possibility of more than one way of legislating. Hobson's choice, as we might say, is no choice at all.

Stroud explains Wittgenstein as saying that the impossibility of its not being the case that *p* when *p* is necessary is based on the lack of sense we have given to its not being the case that *p*. Wittgenstein works with examples – wood sellers who sell wood by area rather than volume, for instance; or the example we saw above of people with rubber rulers they seem to use for something like measuring; and so on. The point of these examples, according to Stroud, is that while we can entertain the possibility of there being different practices of calculating, measuring, counting, inferring and so on, we do not, at least without further ado, make sense of those different practices themselves. As Stroud says:

> Wittgenstein's examples are intended to oppose Platonism by showing that calculating, counting, inferring, and so forth, might have been done differently. … But we can understand and acknowledge the contingency of this fact … without understanding what those ways might have been. If so, then it does not follow that those rules by which calculating, and so forth, might have been carried out constitute a set of genuine alternatives open to us among which we could choose, or even among which we could have chosen. (Stroud 1965 p. 513)

Wittgenstein's suggestion, according to Stroud, is that as things stand we just do not allow sense to a 'calculation' that involved 2 + 2 = 5, say. Anything like that just would not be a calculation at all.

4.10 Derogating from Stroud? – some asides

The distinction here, we should note, is not always cut and dried. There is no definite boundary of sense, as it were, to cross which would mean we definitively no longer made sense. It is all the more important to realise this given our route up to this point in the dialectic: in Chapter 3 above I made play with the idea that we *can*

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[144] For some special purposes we might try to have a modal logic in which necessity and possibility are not duals in this way. This might be one of those cases, if we try to do a more formal analysis. Such formal analysis certainly does violence to our everyday notion of what it is for something to be necessarily the case, though, at the very least.

consider different ways of proceeding in cases when mathematical criteria of application come apart. For example we allowed that acceptance or non-acceptance of irrational numbers or different notions of infinity could have gone either way, and that Newton's and Berkeley's notion of consistency is one we can understand even once we have accepted Robinson's introduction of 'new deductive procedures' with NSA. Such cases, it appears, then, might seem to reinstate a kind of conventionalism after all.

However, it is important to notice the difference between what I claimed in Chapter 3 – that we *can* sometimes make sense of different ways of proceeding in conceptual determination, for instance in cases when mathematical criteria of application come apart – and what I advanced from Stroud just above as Wittgenstein's suggested way of seeing matters regarding eccentric (so-called) 'calculation': namely that we simply *do not* allow sense to such latter behaviour *as* calculation. We need to be careful here.

In any case if we were to see ourselves as giving any kind of theoretical account of *necessity*, it would be no use talking of what we *can* make sense of, because what we *can* make sense of is what it is *possible* for us to make sense of, which latter is equivalent to whatever it is not *necessary* that we do *not* make sense of. So we would be explaining necessity in terms of necessity.

We are *not* to be seen as giving any kind of theoretical account, though. I have emphasised this point several times already with regard to Wittgenstein; no more does Stroud advance such an account; no more do I. That we *do not* allow the sense of '2 + 2 = 5' as part of calculation is a reminder of a truism – a part of our form of life as humans who calculate; it is not a theoretical statement about the foundations of necessity or an example of the outcome of any such account. There is more that could be said about this, and I will be taking up the thread again in Chapter 5. For the present, I want to suggest that we see the different ways we might have gone in determining mathematical concepts as more of a derogation from Stroud's main point rather than a confutation thereof, as well as taking due notice of the modal status of the claims I have explained him as making on Wittgenstein's behalf.

That sometimes an alternative is comprehensible is something Wittgenstein recognises. Think of the people with their rubber rulers who thereby arrive at different measures: recall from above

> … if you had measured the table with the elastic rulers and said it measured five feet by our usual way of measuring, you would be wrong; but if you say that it measured five feet by your way of measuring, that is correct. (*RFM* p. 38)

Is their way of measuring genuinely a way of measuring? With regard to such questions Wittgenstein goes onto point out that their practice

> … is similar to our measuring and capable, in certain circumstances, of fulfilling 'practical purposes'. (A shopkeeper might use it to treat different customers differently.) (*ibid*.)[145]

We have learned to be wary of such questions as whether there is a 'genuine' measurement going on here. (We might ask whether this is really ('*really*', or '*Really*') measurement.) Again, I will attend to the status of such questions in more detail in the next chapter. For now, it is sufficient to see that the 'boundary of sense' is not exact. Particularly if what the aberrant ruler-users do is sufficiently like *our* measuring, there is no harm in calling it measuring. Likewise with irrational numbers or the consistency of differential calculus based on infinitesimals: *once we have a clear view of the matter* nothing further is to be gained by asking whether an irrational number is a genuine number or whether NSA is genuinely consistent. Is there, though, a resurgent conventionalism here? No. Of course there is conceptual development in mathematics. That we can, with sufficient hindsight, allow that such development might have gone differently, and make sense of such a possibility, does amount in effect to a derogation from rather than an annulment of the major point that Stroud advances on behalf of Wittgenstein. Any practice sufficiently close to, say, our way of inferring, or measuring or calculating, might very well be seen as a kind of inferring, etc. That, in itself, does not mean that we would make sense of a designation in such terms of practices that diverge more radically from our own kind of inferring etc.

The point is general: for example in *RFM* Wittgenstein writes of coming across an alien tribe whose language we do not understand,[146]

> We come to an alien tribe whose language we do not understand. Under what circumstances shall we say that they have a chief? What will occasion us to say that this man is the chief even if he is more poorly clad than others? The one whom the others obey – is he without question the chief? (*RFM* p. 352)[147]

The hypothesis that the person in question is the chief is supported by some criteria but not by others. (We are used to such scenarios in the descriptions I gave of conceptual development in mathematics in Chapter 3.) Is he, then, 'without question the chief'? The lack of a determinate answer, as well as the likely lack of importance of such a lacuna given what we know of the scenario, is what Wittgenstein brings out with this question. And he goes on to point up the same lack of determinacy for the same purpose in cases to do with inference and calculating:

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[145] *Ibid*. The reference to 'use' is important, as we saw above in Chapter 3.

[146] Such examples – and there are several in Wittgenstein's writings – are reminiscent of scenarios examined in a tradition particularly influenced by Quine. Quine takes interestingly different morals from such cases of 'radical translation', however. For a comparison of Quine with Wittgenstein in this context, see H-J Glock, 'On Safari with Wittgenstein, Quine and Davidson' in Arrington & Glock 1996 p. 144*ff*.

[147] (Note that Wittgenstein does not ask, 'Under what circumstances *could* we say …?'; or '… *ought we* to say …?'; or indeed '… *must* we say …?' or '… *should* we say …?'.)

> What is the difference between inferring wrong and not inferring? between adding wrong and not adding? Consider this. (*ibid.*)

We have considered this. Our conclusion should be that our practices allow that certain practices count as inferring, certain others as inferring wrong, and certain others as not inferring at all … but that also there may well be indeterminate cases where we are unsure whether to call a certain practice 'inferring wrong' or 'not inferring' at all.

We should expect such outcomes of our deliberations, given that we are looking, not for a philosophical theory or account of our practice of inferring or imputing necessity, but rather for a clear overview of how we use the notion of necessity in mathematics and logic. Once we have this, it becomes simply otiose to seek definitive answers in uncertain cases.

4.11 Dummett's response to Stroud

I return to the main thread. What of Dummett's response to Stroud? First, a detour via Putnam again. Putnam assimilates Dummett's interpretation and Stroud's in all that is important. He claims

> The real point is that if *either* Dummett *or* Stroud is right, then Wittgenstein is claiming that mathematical truth and necessity *arise in us*, that it is human nature and forms of life that *explain* mathematical truth and necessity. (Putnam 1983 p. 117)

– And, as we saw above, Putnam goes on to claim that this 'cannot be right.' (*op. cit.* p. 126)

Replying in a later paper, Dummett denies the assimilation. He thinks Stroud's interpretation takes a step back:

> I do not think [Putnam] is right to accept Stroud's emendation of my interpretation. For it is really a version of moderate conventionalism in that it acknowledges something – namely human nature or our form of life – that *determines* the consequences of the basic necessary truths, or of the conventions that directly confer necessity upon them. (Dummett 1994 p. 52)

Neither Dummett nor (early) Putnam, though, takes on board clearly what is distinctive about what Stroud brings to our attention, namely the focus on sense asserted and denied in thinking of mathematical and logical necessity. Both Dummett and Putnam take Stroud's point about Wittgenstein's emphasis on forms of life and the way in which the contingencies thereof underlie the necessity of mathematics. What both miss is the way in which this 'underlying' does not justify, explain, or give an account of such necessity.

The point is made. In the end Wittgenstein is no kind of conventionalist, in spite of what might have looked like an ineluctable conclusion to be drawn from his injunction to consider the development of mathematics in terms of determination of concepts rather than discovery of matters of fact. Further, *we* should not accept that we are forced to deny the objectivity of mathematics and logic once we see such matters, as I suggest we should, in the way Wittgenstein suggests. Rather, a careful regard for how, in general, we actually think about and use notions of necessity and our practices of inferring, calculating, measuring and so on, shows us just that *we do not make sense* of deviant ways of inferring, calculating, etc.

4.12 Mathematical responsibility to reality

I want to end this chapter by looking at a another way in which Wittgenstein characterises something akin to what I take to be the major point of Stroud's exegesis, namely the lack of sense to be had regarding deviant practices and the like. In *LFM*, Wittgenstein writes

> If the logical laws do not hold – we don't get the game we want to get, we don't play the game we want to play. (*LFM* p. 243)

This is similar in spirit to Stroud's characterisation of Wittgenstein on 'the hardness of the logical must'. It takes on something of a different emphasis in the context from which this latter quotation is drawn, however.

Wittgenstein is here considering questions to do with what he describes as mathematics' 'responsibility to reality'. He distinguishes two ways mathematics is so responsible:

> With regard to "responsibility to reality": On the one hand you might say, "This conclusion is responsible to certain axioms and certain rules." This responsibility is based on our particular practice of using these rules. But then there is another question: as to whether such a system as a whole is responsible to anything. (*LFM* . 242)

It is in regard to the second question – 'whether such a system as a whole is responsible to anything' – that Wittgenstein makes the point about us not 'play[ing] the game we want to play'. This reflects the point above about aberrant 'calculators': as I said, we just do not allow sense to a 'calculation' that involved 2 + 2 = 5 … that is, '2 + 2 = 5' is just not part of the 'game' of calculating as we play it.

Now recall the point I made above in Chapter 3 (section 3.7) about the distinction to be drawn concerning determination of mathematical concepts between what we might get from mathematics itself – *from the inside*, so to speak – and what we might determine to be a part of mathematics *from the outside*, as it were. Mathematics itself, I pointed out, is silent about such questions as to whether we should maintain that numbers are all rational, or stick with lengths being precisely numerically quantifiable; likewise with the choice between Galileo and Cantor on infinite cardinality; and so on. I want to suggest now that we see such questions in terms of the second kind of 'responsibility to reality' of mathematics that Wittgenstein says we might try to put in question, rather than the other (first) kind of responsibility, such as, for example, the responsibility the proof of incommensurability of $\sqrt{2}$ answers to, 'based on our particular practice of using these rules' *within* mathematics.

I want to investigate with Wittgenstein these two different ways in which mathematics might be thought to be 'responsible to reality'. Here we are directly involved with the overall problem of the applicability of mathematics. I am going to suggest that a clear overview of these kinds of 'responsibility', allied to the way we have approached mathematics as developing by concept-determination as well as to considerations of sense and the lack of the same such as those we have encountered in this chapter, will unite to give us the wherewithal to see the problem in its true colours. This is the task of my next chapter.

Chapter 5 The Problem Solved

5.1 Conventionalism again

In *Philosophical Investigations* Wittgenstein himself writes about the temptation to interpret him as advocating conventionalism of some sort:

> 241. "So you are saying that human agreement decides what is true and what is false?" – It is what human beings say that is true and false; and they agree in the language they use. That is not agreement in opinions but in form of life.
>
> 242. If language is to be a means of communication there must be agreement not only in definitions but also (queer as this may sound) in judgments. This seems to abolish logic, but does not do so. – It is one thing to describe methods of measurement, and another to obtain and state results of measurement. But what we call "measuring" is partly determined by a certain constancy in results of measurement.

And in *LFM*, he is reported as asking us to

> Consider Professor Hardy's article ("Mathematical Proof") and his remark that "to mathematical propositions there corresponds – in some sense, however sophisticated – a reality" (*LFM* p. 239)[148],

… in regard of which 'reality' Wittgenstein goes on to say

> With regard to "responsibility to reality": On the one hand you might say, "This conclusion is responsible to certain axioms and certain rules." This responsibility is based on our peculiar practice of using these rules. But then there is another question: as to whether such a system as a whole is responsible to anything. (*LFM* . 242)

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[148] Hardy does not mention 'correspondence' here, in fact, as Cora Diamond, the editor of *LFM* points out (*ibid.*). It is clear enough what is going on, though. Wittgenstein adds the parenthetical remark (*ibid.*), 'The fact that he said it does not matter; what is important is that it is a thing which lots of people would like to say.' Elsewhere Hardy writes of 'reality',

> '… I shall speak of 'physical reality', and here again I shall be using the word in the ordinary sense. … I hardly suppose that, up to this point, any reader is likely to find trouble with my language, but now I am near to more difficult ground. For me, and I suppose for most mathematicians, there is another reality, which I will call 'mathematical reality' … (Hardy 1992 pp. 122-123)

In this Hardy seems to anticipate Wittgenstein's 'two ways' in which mathematics may be held to be 'responsible' to reality in his (Hardy's) notion of different kinds of reality or different languages in use for physical and mathematical reality. He makes little of it, though, I suppose on account of his Platonism. In spite of protestations about differences, it does seem that Hardy trades on the sort of semantic homogeneity of which we have come to be suspicious.

As a first move towards unpacking here, consider the distinctions Wittgenstein makes in *Philosophical Investigations* §241/2 above: The distinctions are between

what human beings say	and	the language human beings use
opinions	and	forms of life
results of measurement	and	methods of measurement

Wittgenstein is suggesting we notice a similarity between these distinctions. Consider the first pair of conjuncts, for instance. A proposition – *what I say* – may be true or false, and is assessable for its truth. It makes sense without any further ado to ask about something I say – some proposition – whether or not it is true. Contrast with language itself, as a whole: there are many questions I might ask about the language I use to say something, but it does not ordinarily make sense to ask whether the language is *true*. (Note the claim is not that we *cannot* make sense of talk of language being true, but that we *do not*; this is intended as a *description*, once again, rather than part of, or a consequence of, a theory.) Language, we might say, is conventional, but this *contrasts* with what we say in language: we may agree on our language, but what we say in language is not dependent for its truth on our agreement.

For each of the pair of conjuncts above, the first of the two has a similar normative aspect. *What human beings say* may be true or false: likewise *opinions* may be correct or incorrect, as may *results of measurement*. The second of each of the two conjuncts, by contrast, characterises the *frameworks* within which the normative practice of assessing the first takes place. While it makes sense to ask, for instance, whether my opinion on some matter is correct or not (and similarly in the other cases), the question of whether the form of life I share with other humans is correct (and so on) does not arise. Here is one aspect of how Wittgenstein deals with what looks like a drive to relativism of some sort or other via what might *appear* to be – but is not – conventionalism.

There is a clear link here to what I explained, following Barry Stroud, as Wittgenstein's overview of the 'hardness of the logical must' – of logical and mathematical necessity – in the previous chapter. In *Philosophical Investigations* §242 (above) Wittgenstein writes of the agreement in *judgements* that he adverts to as a condition of the possibility of human language as *seeming* 'to abolish logic'. That is, if human judgement determines what is or is not the case, or even more pertinently for our concerns, what is or is not *necessarily* the case, it seems that logic will lose its objectivity with its independence, as we saw Dummett and Putnam claiming. That this seeming abolition of logic is only apparent – a *mere* seeming –, however, as I claimed following Stroud's exegesis, is apparent once we note that the impossibility of its not being the case that *p* when *p* is necessary is based on the *lack of sense* we have given to its not being the case that *p*.

This lack of sense, now, can be seen in a particular light in its association with the move from the first to the second of each of the pairs of conjuncts I have highlighted above; for example the sense we make *within* a system of measurement of considering whether such-and-such result of measurement is correct, contrasts with the *lack* of sense we allow to talk of whether a *method* or system of measurement is correct or incorrect. It is such lack of sense given or allowed which we saw above in chapter 4 to be deeply connected to the root of ascriptions of necessity. With this in mind let us add a line to the table of three conjuncts above taken from the distinction Wittgenstein makes in *LFM* between the two ways of considering mathematics to be 'responsible' to reality.

The distinction is between

mathematical propositions considered individually *intra*-mathematically	and	our practice of using the usual rules and axioms of mathematics

Once again, the first conjunct here has a normative aspect; it makes sense to consider whether a particular mathematical proposition is responsible to 'certain axioms and certain rules' – whether it is in accord with relevant parts of the rest of mathematics, in short. By contrast, again, the second of the two conjuncts characterises the framework within which such accord is assessed. We can mark the difference here by reference to the *intra*-mathematical aspect of talk of whether a certain mathematical conclusion is correct by contrast to *extra*-mathematical questions about whether 'the system as a whole is responsible to anything'.

So, in these terms, now, what is important for us is how the distinction Wittgenstein points up between normative practices and the frameworks within which such practices take place plays out in the case of the distinction between *intra*-mathematical propositions, assessed by our practice of using the usual rules and axioms of mathematics, and *extra*-mathematical questions about mathematics, such as whether the whole of mathematics is responsible to anything at all – or, by the same token, whether our practice of using the usual rules and axioms of mathematics is responsible to, or justified by, anything at all. I am going to argue that this distinction, and consequential differences between the sense we allow to such intra- and extra-mathematical propositions and questions, is key to solving the problem of mathematical applicability.

We do need to be a little careful here. In assimilating some aspects of the distinction Wittgenstein draws between what I am calling *intra*- and *extra*- mathematical questions to the distinctions in play in *Philosophical Investigations* §241-2, we should (once again) beware of thinking that what is on offer is some sort of overall or general theory about the difference between, say, frameworks and normative practices within them. The comparison between the two distinctions is intended, not as a generalisation part way to becoming a theory, but – as usual – in the hope of clarification, of arriving at a perspicuous overview of the philosophical terrain.

5.2 Groups of (very) small order

To make clear the distinction between the two kinds of 'responsibility' Wittgenstein suggests mathematics might be said to have to 'reality', it will help considerably to have an example in view. Basic arithmetic possibly comes with too much philosophical baggage attached. More advanced material risks losing sight of wood for trees. Group theory, at least in its beginnings, has an ideal simplicity and brevity for my purpose here. I will consider – we will *do* – some elementary group theory.

A group is a set together with a binary operation defined thereon.[149] I will take the operation sign as read; '*ab*' will mean the result of combining elements *a* and *b* (in that order) in a group using whatever the operation is.

The four group axioms (in a group *G*):

Closure: $\forall a,b \in G,\ ab \in G$

Associativity: $\forall a,b,c \in G,\ (ab)c = a(bc)$

Identity: $\exists i \in G : \forall a \in G, ia = a = ai$

Inverse: $\forall a \in G\ \exists a^{-1} \in G : a^{-1}a = i = a\,a^{-1}$ (where *i* is the 'identity' as in (3); a^{-1} is the 'inverse' of *a*. I will take *i* as the identity unless otherwise specified.)

Now, for finite groups it is often convenient to display the group in a table (a so-called 'Cayley table'): here is a table for a group of order 3 (the 'order' of a group is the number of elements or members it has):

	i	*a*	*b*
i	*i*	*a*	*b*
a	*a*	*b*	*i*
b	*b*	*i*	*a*

(the middle '*b*' in this table shows that *aa* = *b*; the '*i*' directly to its right, that *ab* = *i*, and so on. This is all conventional.)

Now, in fact, although I said this is 'a' group of order 3, there is also a strong sense in which this is *the* group of order 3. Any group with 3 elements will be isomorphic to this group: and it is conventional to call isomorphic groups the same group.

To see why any group of order 3 will be isomorphic to this group, consider first a property of groups relating to their Cayley table: every element occurs exactly once in every row and column of a group table. For, suppose not: then either there is an element that does not occur in some row or column, or there is an element that occurs more than once in some row or column. Suppose the former. Then, since each row (for definiteness) has the same number of positions as there are elements in the group, there would have to be a position filled by something other than elements

[149] See Appendix 1 for some more basic background.

of the group. But that contradicts Axiom 1. So, suppose the latter; that in a group G there is some element that occurs in two different columns (for definiteness) in the same row of the table. Suppose that element is p:

	x	y
a	p	p

… Thus for some a, x, and y in G (where $x \neq y$), we have that $ax = ay$ (= p).

But a has an inverse by axiom 4, and so

$$\begin{aligned} ax = ay \Rightarrow a^{-1}(ax) &= a^{-1}(ay) \quad \text{(by axiom 4)} \\ \Rightarrow (a^{-1}a)x &= (a^{-1}a)y \quad (2) \\ \Rightarrow ix &= iy \quad (4) \\ \Rightarrow x &= y \quad (3) \end{aligned}$$

… This contradicts the assumption that $x \neq y$. So every element occurs exactly once in every row and column of the table. (A table with such a property is called a 'Latin square'. All group tables are Latin squares, we have proved.[150])

Now consider a group with 3 elements, say i, p, and q. (I will take i to name the identity in any group we consider.) Start to fill in its Cayley table:

	i	p	q
i	i	p	q
p	p		
q	q		

– That much is evident since i is the identity.

Now consider filling in the middle space (row 2 from the top, column 2 from the left, what I will call the space (2, 2)) in the table, pp: the fact that the table is a Latin square tells us that this space must be filled by q, since Axiom 1 entails that only i, p, and q are allowed, and

p is already in row 2 at (2, 1);

if i were in this place, q would have to be at (2, 3) by Latin squared-ness … but q is already in row 3 at (1, 3).

So the table must have

[150] The converse of this is false. Not every Latin square is a group table, though I will not prove that here.

	i	p	q
i	i	p	q
p	p	q	
q	q		

… And now the Latin square property allows us to fill in the rest of the table too:

	i	p	q
i	i	p	q
p	p	q	i
q	q	i	p

Compare this with the table from above:

	i	a	b
i	i	a	b
a	a	b	i
b	b	i	a

Clearly these have the same structure; they are isomorphic. So there is just one group of order 3, according to our isomorphism criterion of identity for groups.

I will continue with this a stage further.[151] How many groups of order 4 are there?

Consider a group with 4 elements *i*, *a*, *b*, *c*. Here is the beginning of its Cayley table:

	i	a	b	c
i	i	a	b	c
a	a			
b	b			
c	c			

… Once again, this is forced by Axiom 3.

Now consider the position (2, 3) in the table (2nd row down, 3rd column from the left, recall – the result of *ab*). This cannot be *a* or *b* since *a* is already in row 2 and *b* is already in column 3. It must be *i* or *c*. Suppose it to be *i* (I will return to this stage (*) below to consider the other possibility):

[151] Written out, what follows probably looks longer and trickier than it actually is. The process is similar to working out a hand of bridge or the odds at poker, essentially by counting. We are seeking an overview, for which it is important to get down to the nitty-gritty and hurly-burly, so we need this brief look at the details of *some* mathematics. (This remark is directed to the mathematical neophyte; balancing apologies to the mathematically-adept philosopher for labouring the point are also in order.)

$$\begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & & i & \\ b & b & & & \\ c & c & & & \end{array}$$

The rest of the table is now forced by the Latin square criterion. For example, now in position (2, 4) we must have *b*, since *a* and *i* are already in row 2 and *c* in column 4:

$$\begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & & i & b \\ b & b & & & \\ c & c & & & \end{array}$$

The rest of the positions are similarly forced (it is easy enough to see each stage …):

$$\ldots \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & c & i & b \\ b & b & & & \\ c & c & & & \end{array} \qquad \ldots \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & c & i & b \\ b & b & i & & \\ c & c & & & \end{array} \qquad \ldots \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & c & i & b \\ b & b & i & & \\ c & c & b & & \end{array}$$

$$\ldots \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & c & i & b \\ b & b & i & & \\ c & c & b & a & \end{array} \qquad \ldots \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & c & i & b \\ b & b & i & & \\ c & c & b & a & i \end{array} \qquad \ldots \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & c & i & b \\ b & b & i & c & \\ c & c & b & a & i \end{array}$$

$$\ldots \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & c & i & b \\ b & b & i & c & a \\ c & c & b & a & i \end{array} \quad = C_1$$

So this is one possibility. Call it C_1.

Now return to the stage (*) above. We filled position (2, 3) with *i*. Suppose the other alternative, that it be filled with *c*:

$$\begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & & c & \\ b & b & & & \\ c & c & & & \end{array}$$

Position (2, 4) can be filled with i or b. Choose i (and return to this stage (**) below for the other choice).

$$\begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & & c & i \\ b & b & & & \\ c & c & & & \end{array}$$

… The rest of the positions are now forced:

$$\ldots \quad \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & b & c & i \\ b & b & & & \\ c & c & & & \end{array} \qquad \ldots \quad \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & b & c & i \\ b & b & & & a \\ c & c & & & \end{array} \qquad \ldots \quad \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & b & c & i \\ b & b & & & a \\ c & c & & & b \end{array}$$

$$\ldots \quad \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & b & c & i \\ b & b & & & a \\ c & c & i & & b \end{array} \qquad \ldots \quad \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & b & c & i \\ b & b & & & a \\ c & c & i & a & b \end{array} \qquad \ldots \quad \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & b & c & i \\ b & b & c & & a \\ c & c & i & a & b \end{array}$$

$$\ldots \quad \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & b & c & i \\ b & b & c & i & a \\ c & c & i & a & b \end{array} \quad = \quad C_2$$

– This, then, is a second possibility. Call it C_2.

Now back to (**) above, for the other choice; b in position (2, 4):

$$\begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & & c & b \\ b & b & & & \\ c & c & & & \end{array}$$

This time not all choices are forced. The next few are, though:

$$\ldots \quad \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & i & c & b \\ b & b & & & \\ c & c & & & \end{array} \qquad \ldots \quad \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & i & c & b \\ b & b & c & & \\ c & c & & & \end{array} \qquad \ldots \quad \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & i & c & b \\ b & b & c & & \\ c & c & b & & \end{array}$$

– But now there are two possibilities again. … First with a at (3, 3):

$$\begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & i & c & b \\ b & b & c & a & \\ c & c & b & & \end{array} \quad \text{… giving} \quad \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & i & c & b \\ b & b & c & a & i \\ c & c & b & i & a \end{array} \quad = \mathrm{C}_3$$

… and next with i at (3, 3):

$$\begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & i & c & b \\ b & b & c & i & \\ c & c & b & & \end{array} \quad \text{… giving} \quad \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & i & c & b \\ b & b & c & i & a \\ c & c & b & a & i \end{array} \quad = V$$

(I have left out a few forced steps in each of these; it is a simple matter to fill them in.) Name these tables C_3 and V respectively.

So, it seems we have four possibilities, C_1, C_2, C_3 and V. However, perhaps surprisingly, all three C_i are isomorphic. We can see this once they have been rearranged (of course nothing is particularly special about the order the elements are given in, although it makes sense to keep the identity in the same place each time).

$$C_1 : \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & c & i & b \\ b & b & i & c & a \\ c & c & b & a & i \end{array} \quad \text{… rearrange as follows:} \quad \begin{array}{c|cccc} & i & a & c & b \\ \hline i & i & a & c & b \\ a & a & c & b & i \\ c & c & b & i & a \\ b & b & i & a & c \end{array}$$

$$C_2 : \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & b & c & i \\ b & b & c & i & a \\ c & c & i & a & b \end{array} \quad \text{… keep the same:} \quad \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & b & c & i \\ b & b & c & i & a \\ c & c & i & a & b \end{array}$$

$$C_3 : \begin{array}{c|cccc} & i & a & b & c \\ \hline i & i & a & b & c \\ a & a & i & c & b \\ b & b & c & a & i \\ c & c & b & i & a \end{array} \quad \text{… rearrange:} \quad \begin{array}{c|cccc} & i & b & a & c \\ \hline i & i & b & a & c \\ b & b & a & c & i \\ a & a & c & i & b \\ c & c & i & b & a \end{array}$$

If we *still* are unsure about the isomorphism, we can exhibit a map to and from each of these to another set, say $\{I, P, Q, R\}$, as follows:

$$C_1: \quad \begin{array}{l} i \longleftrightarrow I \\ a \longleftrightarrow P \\ b \longleftrightarrow R \\ c \longleftrightarrow Q \end{array}$$

$$C_2: \quad \begin{array}{l} i \longleftrightarrow I \\ a \longleftrightarrow P \\ b \longleftrightarrow Q \\ c \longleftrightarrow R \end{array}$$

$$C_3: \quad \begin{array}{l} i \longleftrightarrow I \\ a \longleftrightarrow Q \\ b \longleftrightarrow P \\ c \longleftrightarrow R \end{array}$$

The set {*I, P, Q, R* } now has the imposed structure

$$\begin{array}{c|cccc} & I & P & Q & R \\ \hline I & I & P & Q & R \\ P & P & Q & R & I \\ Q & Q & R & I & P \\ R & R & I & P & Q \end{array} \quad = C$$

whichever of these three maps we take. So it is clear (if it was not before, indeed) that the C_i all share this self-same structure. Call this *C*.

Now, what we have done is show that there can be *at most* two different groups of order 4. We have not yet shown that we have two different *groups* rather than Latin squares with an identity element. In fact, though, each of these tables *does* represent a group – *C*, the cyclic group on four elements, which we might think of as the group of rotational symmetries of a square in Euclidean 2-space[152]; and *V*, often called the *Klein* group[153], which might be characterised as the group of symmetries of a Euclidean rectangle.[154] It is an easy task to check these claims.

So, in conclusion, there are precisely two groups of order four.

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[152] We should be careful; this is not what this group *is* in any essentialist sense. We could equally categorise it as the group of integers modulo 4 under addition modulo 4, for instance. These are, we have said, the *same* group. Groups are *abstract*; not to be identified with any of their specific instantiations. We might think of defining the group as a certain equivalence class under isomorphism, but we do not need anything so formal. ‘Groups are the same when they are isomorphic’ will do.

[153] After Felix Klein. Klein himself called it *V*, for *Vierergruppe* (‘four-group’).

[154] Or, bearing in mind the note just above, as the group { 1, 3, 5, 7 } under multiplication modulo 8, for instance; or, indeed, the symmetry group of the rhombus.

5.3 Responsibility and sense

This short example is intended to clarify Wittgenstein's notion of mathematical 'responsibility to reality'. Recall Wittgenstein's remark,

> On the one hand you might say, "This conclusion is responsible to certain axioms and certain rules." This responsibility is based on our peculiar practice of using these rules. (*LFM* p. 242)

The conclusion that *there are exactly two groups of order four* is evidently responsible to 'certain axioms and certain rules' in just the sense Wittgenstein calls on here. Some of the axioms and rules have been explicitly cited, indeed, although of course there are also axioms and rules that remain tacit, some of which we might bring to the surface in further analysis. For example, in claiming that 'since each row [of the Cayley table] has the same number of positions as there are elements in the group, there would have to be a position filled by something other than elements of the group' were there to be a group element missing, there is an implicit call on the so-called 'pigeonhole principle'.[155] And there are also principles of logic that are called on implicitly. I do not intend to attempt an exhaustive account.

I do want to note two specifics. First, notice that we have concluded with an existence claim: 'There are …' Second, in our counting we have adopted a criterion of identity in the convention that groups are 'the same' when isomorphic. Both of these aspects of our conclusion are 'responsible to' the rules of mathematics, as is the conclusion as a whole. And in being so responsible, it thereby makes sense for us to ask whether our conclusion is true (answer: yes, it *is*, unless I have made a mistake), just as, earlier, in the first of the conjunctions we found in *Philosophical Investigations* §241-2, it made sense to ask whether what we *say* in language is true, and whether our *opinions* or results of measurement are correct.

These expressions of different kinds of normativity were then *contrasted* with their non-normative frameworks; it does not make sense to ask whether our language itself is true or not; and so on. So, now, consider the *other* question Wittgenstein suggests might be asked about mathematical responsibility to reality: the question 'as to whether such a system as a whole is responsible to anything.' Seeing this question in the light of the distinctions advanced in *Philosophical Investigations* §241-2 should make us suspicious of its sense in the same way we are suspicious of the sense of questions as to the truth of our language and other normative questions that we might attempt to pose about the frameworks in which our normative practices reside.

Questions like this, however, are just the sort of question that are raised by fictionalist philosophers like Hartry Field, *and* by their opposition in the Platonist or realist camp. Considering the fictionalist at work enlightens us quite specifically

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[155] Often elsewhere known as 'Dirichlet's drawer principle' or just the 'principe des tiroirs'. If $n < m$ and we try to match n pigeons with m holes, there will be at least one empty hole: a simple but surprisingly productive little principle.

here. We have seen that the question, 'How many groups of order four?', gets to be answered ('exactly two') *intra*-mathematically, responsible to the axioms and rules of group theory and the rest of mathematics. Looking at group theory as we have rather than arithmetic, say, with all its philosophical/historical baggage, helps to isolate and so clarify the important point just here. Because, after all, the fictionalist wants to say, without impugning the mathematics that led us to give that answer, that like all of mathematics, the claim that *there are* exactly two groups of order four is *false*. We are coming to see that the important issue here is not whether the fictionalist is right about this, or what his or her justifications are, but rather *what she might mean.*

We saw in Chapter 2 above that Field's 'meanings' were beetles in boxes (section 2.9); they 'can't be known', he said, *and* they play no part in mathematical practice. They 'drop out of consideration as irrelevant'.[156] Now we are beginning to see the other side of this coin. Even if there were a clear (*extra*-mathematical) meaning for such statements as 'there are groups' or 'numbers exist', such meaning, as we saw in Chapter 2, plays no part in mathematical practice. Indeed there is, as we have just seen, a clear meaning – a *use* – *within* mathematical practice for such statements as 'there are exactly two groups of order four'. But what the fictionalist is talking about in saying that such a statement is false cannot be such use *intra*-mathematically; it would be absurd in that way to assert that the mathematician is *wrong* to say that there are two groups of order four or that groups such as C_1 and C_2 are *not* really the same after all (on metaphysical grounds, no less!). And with Wittgenstein's distinction between *intra*- and *extra*-mathematical responsibility now in clear view, we are beginning to question whether there is any clear *extra*-mathematical meaning to be had for assertions such as this. The fictionalist *requires* such meaning. But it seems there is no such meaning to be had.

We can go *some* way in consideration of Wittgenstein's second genre of mathematical responsibility to reality, 'as to whether there such a system as a whole is responsible to anything'. In effect, this is exactly the task we were engaged on in Chapter 3 above. The kind of responsibility we came up with, however, in terms of giving due weight to determination of concepts rather than focussing exclusively on discovery of facts in the development of mathematics 'as a whole', is not at all such as to allow – what we see to be required by the fictionalist – *extra*-mathematical meaning to such questions as whether there are or not really (or '*really*' or '*Really*') such things as groups.

The qualification 'really' ('*really*', '*Really*') points up this distinction between *intra*- and *extra*-mathematical questions. I take the locution 'italics added *real* or capital-R Real existence' from John Burgess. (See Burgess 2004 p. 35) Consideration of a recent tussle between Burgess and Mary Leng, involving this way of speaking, will

[156] The quote is from Wittgenstein in *Philosophical Investigations* §293. The analogy I am drawing is between Fieldian 'meanings' and private mental (semantic) objects of sensation.

help me drive home this point about fictionalism requiring a sense for mathematical Platonism that is lacking. I intend to parlay this lack of sense into an overall solution to the problem of mathematical applicability by turning it back on to the work of Mark Steiner. It is probably becoming clear how this will play out, in general terms at least. I want to cover this Burgess/Leng controversy first, though, to make the point stick fast.

5.4 Carnap, Burgess, and Leng

Burgess (2004) tackles fictionalism using a distinction due to Rudolf Carnap that is very similar to Wittgenstein's; Leng (2005) replies.

Burgess distinguishes two kinds of fictionalism, only the second of which – what he dubs 'revolutionary fictionalism' – need concern us.[157] A revolutionary fictionalist is, roughly speaking, a fictionalist like Hartry Field. As Leng puts it, revolutionary fictionalism is a philosophical doctrine

> … according to which mathematicians and scientists, when involved in making assertions that imply the existence of mathematical objects, are making assertions that we have no reason to believe to be true. (Leng 2005 p. 284)

This agnostic way of putting the matter – 'no reason to believe to be true' as opposed to 'reason to believe false' is not important for Leng, as she explains in a footnote referring to Field. Field thinks, and Leng agrees, that so long as the 'hypothesis' that mathematical entities exist is dispensable (as he argues it is), then

> … it is natural to go beyond agnosticism and assert that mathematical entities do not exist. (Field 1989 p. 45. Referenced in Leng 2005 at p. 284*fn*.)

Further, as Leng asserts, the move beyond agnosticism is 'a step that many fictionalists would be inclined to take.'(p. 283) And she does, indeed, make this step herself elsewhere in the same paper:

> … according to fictionalism, the correct understanding of mathematical assertions is not as literal assertions of truth. Rather, the revolutionary fictionalist seeks to explain how it is that such literally false theories can nevertheless be useful. (Leng 2005 p. 283)

I will return to this move from 'not literal assertions of truth' to 'literally false' below.

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[157] The first kind, 'hermeneutic' fictionalism, is the view that when we understand what mathematicians are saying we will see they are fictionalists: 'The hermeneutic fictionalist,' Burgess says (*op. cit.* p. 23), 'maintains that *the mathematicians' own understanding* of their talk of mathematical entities is that it is a form of fiction, or akin to fiction: …' Burgess argues that hermeneutic fictionalism is 'implausible' (p. 28), a conclusion with which Leng concurs: 'Burgess's rejection of hermeneutic fictionalism is accepted…' (Leng 2005 P. 277)

Now, Burgess offers two arguments against the revolutionary fictionalist, and it is the second of these which will concern us.[158] The argument in Burgess's hands derives from a distinction made by Rudolf Carnap that is very similar to the Wittgensteinian distinction I have explained above. Carnap's distinction relies on what Burgess describes as 'the infamous 'empiricist criterion of meaningfulness'', something that would be anathema to the Wittgenstein of the *LFM*.[159] Nevertheless, there is much to be gained from looking at Burgess's handling of it, and, even more pertinently, Leng's response thereto.

Carnap's distinction is stated in terms of linguistic 'frameworks' in which we may talk of 'new entities'. He claims:

> … we must distinguish two kinds of questions of existence: first, questions of the existence of certain entities of the new kind *within the framework*; we call them *internal questions*; and second, questions concerning the existence or reality *of the system of entities as a whole*, called *external questions*. (Carnap 1950 p. 206)

This is remarkably similar to the Wittgensteinian distinction I outlined above. Carnap himself makes a 'brief historical remark' in the article from which this quotation is taken, tracing his own ideas from Wittgenstein himself via the Vienna Circle. (See p. 215, 'Influenced by the ideas of Wittgenstein …' etc.) I do not want to get into complications involving relations between Wittgenstein and the Logical Positivists. There is a major difference I want to highlight between Wittgenstein's explanation and Carnap's, though; it is that Carnap situates his *internal*/*external* distinction squarely within Logical Positivist semantic theory, according to which empirically unverifiable propositions are, if not logically analytic, strictly meaningless. Wittgenstein, as we have seen, would have no truck in his later writings with *theory* in philosophy at all; his own expression of the distinction is consequently more nuanced than Carnap's.

Burgess makes something of this, though without mentioning Wittgenstein. Burgess describes the line of thought expressed by Carnap's distinction as one that has been 'consigned to the rubbish bin of philosophical history' because, as I said above,

[158] The first involves the suggestion that it is inappropriate – even 'chronically immodest', see Leng, *op. cit.* P. 283, or 'comically immodest', see Burgess *op. cit.* P. 30 – for philosophy to set itself up as an arbiter for what mathematicians say and do. Think of the philosopher above telling the group theorist she is *wrong* to claim that there are two groups of order four or that the criterion of identity for groups is *mistaken*. Compare the successes of mathematics with those of philosophy – if there *are* any of the latter! Burgess refers to David Lewis's *Credo*: 'Mathematics is an established, going concern. Philosophy is as shaky as can be. To reject mathematics on philosophical grounds would be absurd. … Even if we reject mathematics gently – explaining how it can be a most useful fiction, 'good without being true' – we still reject it, and that's still absurd.' See the longer quote in Burgess and Rosen 1997 p. 34.

[159] And arguably to Wittgenstein *tout court*, although I do not want to get into the fine details of such matters.

Carnap's identification of the case he argues 'was too much identified with the infamous 'empiricist criterion of meaningfulness'.' Burgess is consequently wary of swimming against the tide of philosophical history, and wants to

> … restate the essence of [Carnap's] argument in a way that strips it of most of its most dated formulations and presuppositions. (Burgess 2004 p. 34)

In doing so, Burgess comes very close to what I have set the stage for as an application of the distinction between the *intra*-mathematical aspect of talk of whether a certain mathematical conclusion is correct by contrast to *extra*-mathematical questions about whether 'the system as a whole is responsible to anything' in Wittgenstein's words. Burgess agrees with Carnap that questions like this latter – questions in Carnap's terms that are *external* to the practice of mathematics – are absent 'empirical meaning'. (Burgess, *op. cit.* p. 35) That is, if we think of questions about the 'italics-added *real* or capital-R Real existence of numbers', (*ibid.*) where such questions are *about* mathematics rather than *within* mathematics, then even if we feel we could make sense of such questions, our questions and their answers would make no difference at all to the world.[160] [161]

Burgess now takes this conclusion and compares the situation regarding ('italics-added') *real* existence of numbers and apparently similar *real* existence of certain fictions. Taking one of his examples, if Carlos Castañeda's books were non-fictions as some (strangely) have claimed, so that the characters and events therein described were not just real in the sense of being properly *fictional* characters and events, but (italics-added) *real* characters and events, then the world would be in some particular respects a different place from how it actually is. That, Burgess claims, contrasts with how things stand with regard to the *real* existence of numbers. Suppose, allowing that there are two perfect numbers less than 30, we ask the further (external) question whether there are *really* such things as perfect numbers; what possible difference could any answer to this latter question make to the world? None, answers Burgess; given which, as he says, whatever stance we might take within the philosophy of mathematics, 'we should not call ourselves 'fictionalists' '.

Mary Leng enters the fray on behalf of the revolutionary fictionalist. She agrees with Burgess that *external* questions about what she calls the 'metaphysical Reality of mathematical objects' lack empirical meaning:

> On the one hand, the fictionalist's denial [that mathematicians and scientists ought to believe the claims made within their disciplines] might be based on a denial of the metaphysical Reality of mathematical objects. If so, then as Carnap has shown, we

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[160] Burgess thinks we *can* make sense of such questions by turning them into 'theological' questions about what God did or did not do at the time of creation. That seems a bizarre kind of sense and I confess to being somewhat at a loss about what he means. I will not pursue the matter, though.

[161] Once again, note the similarity here to Field's 'meanings' of mathematical terms, which likewise, as I argued just above and as Field himself at least half-accepts, are semantically idle (see section 2.9 above), like Wittgenstein's beetles in their boxes.

> have good reasons to suspect that the revolutionary fictionalist's thesis is empirically meaningless. (Leng 2005 p. 288)

However, Leng goes on to imply, all the important work goes on *internally*, and it is this that the fictionalist should focus on:

> On the other hand, the fictionalist might be attempting to deny the small-r reality of the mathematical objects posited by our theories, while asserting the reality of (at least some of) the physical objects that our theories posit. (*ibid.*)

This move takes talk of the existence of mathematical objects to be on a par with talk of the existence of tables, mountains, electrons, fields, and so on. Leng wants us to accept that scientific theories posit the existence of abstracta *in just the same way* as they posit the existence of concreta, the only difference being that concreta exist whereas abstracta do not. Or at least, according to Leng's fictionalism we have no reason to believe in abstracta, given that we can account for their utility in our theories other than by accepting their (small-r, recall) real existence. By keeping to talk of small-r real existence, she suggests, we can avoid the conclusion of loss of sense advanced by Burgess.

There are several things that could be challenged in the move Leng makes here. For instance, she accepts the heterogeneity of talk of the 'real' implicit in Carnap's distinction, but at the expense of requiring the homogeneity of talk of matters of 'existence' in considering the *prima facie* different discourses of mathematics and science. Carnap, we saw, puts in question this homogeneity precisely in talk of 'existence'. I dealt with issues about semantic homogeneity above in Chapter 2 in considering what it might be to take certain discourses 'at face value'. Connectedly, the homogeneity Leng leans on also assumes that mathematical propositions are similar in assertive import to propositions involving physical objects, something we also found reason to question earlier considering how mathematical propositions function as rules of inference, not least as an issue connecting with Hartry Field's fictionalist epistemology.

It would take us too far afield at this juncture to rehearse my critical remarks from above or to explore such issues in further detail. However, I do want to make one further particular challenge. I will suggest that the way fictionalism claims to establish the utility of mathematics independently of the existence of mathematical objects – or to put the matter in Hartry Field's words, how mathematics can be good without being true – itself vitiates the point Leng makes just here. I want to take this fairly slowly, as it bears directly on the problem of mathematical applicability – of the utility of mathematics in physical science. So I leave this particular thread hanging for the moment.

5.5 Steiner and Field; homomorphisms (1)

Mark Steiner, we saw in chapter 1, suggests that

> … the central philosophical doctrines of these major philosophers [for example Plato, Descartes, Spinoza, Berkeley, Kant and Mill] were conceived in great measure to explain the applicability of mathematics to Nature. (Steiner 2005 p. 626)

Further, we saw Steiner to go on to say, this motivation of 'central philosophical doctrines' can be characterised universally in terms of the notion of a homomorphism between mathematics and physics, the key question for philosophy about mathematical applicability being, 'Given the nature of mathematics, why should such a homomorphism exist?'

Again, recall the centrality of the notion of homomorphism also to Hartry Field's 'main theoretical device', that of the use of 'representation theorems', theorems which assert the existence of 'representing' homomorphisms connecting physics and mathematics. It was by means of such representation theorems, allied with the idea that mathematics is conservative over science, that Field could claim to be able to show how mathematics can be useful.[162]

In Chapter 2, I gave a particular example of a representation theorem,[163] and, looking at it as an exemplar, suggested in the light of the centrality of its representing homomorphism that we see such theorems as in the business of attempting to answer Steiner's question, 'Given the nature of mathematics, why should such a homomorphism exist?' I also entered a preliminary caveat, what I called there 'an initial worry' about the way in which such theorems are supposed to show us how to bridge a supposed gap between the separate realms of mathematics and physics. Now I want to expand on this initial worry.

Consider the unease I claimed we might feel at the idea of separate 'realms' of physics and mathematics given that for Field, the mathematical realm is one containing non-existent objects.[164] This unease, now, is strengthened by Leng's attempt to situate mathematics as a homogeneous part of the physics to which it applies. For consider: the map at the centre of any representation theorem supposedly connects physical objects to mathematical objects – two different *sorts* of object, it seems. Leng wants them to be considered under the same head as posits of the physical theory; the only difference between the physical objects and the mathematical objects according to Leng is that the physical objects actually exist

[162] I mentioned above in Chapter1that a strong case can be made for Field's main theoretical device being useful in explicating mathematical applicability no matter what the background metaphysical position, something we saw Field himself to be aware of.

[163] See section 2.3, *Theorem 1* from Krantz *et al* 1971. The statement and proof of this theorem is in Appendix 2.

[164] Any difficulty we might have making sense of a 'container' for non-existent objects is grist to the mill of my argument here, of course.

whereas the mathematical ones do not.[165] That is a large difference. Moreover, it is not one that can be taken care of by considering the case with examples of other kinds of non-existent theoretical entities of whatever sort – mathematical objects, according to Field and Leng, are *systematically* non-existent.

My discussion in Chapter 2 of semantic homogeneity in the light of Benacerraf's dilemma and the notion of 'taking at face value' is relevant here. We are uncovering further reasons to be suspicious of claims of homogeneity in the light of difference. The overview we seek will be the more perspicuous if we keep in mind this earlier critique as we proceed.

Pulling up the thread I left hanging at the end of the previous section, now, I want to suggest that Leng is not entitled to press the homogeneity of treatment of talk of abstracta and concreta once we allow that the fictionalist requires they be treated differently, as really existing (physical) objects on the one hand and as systematically non-existent (mathematical) objects on the other to be connected via a homomorphism. The difference between the two – the gap that opens up between the physical and the mathematical – is one that is required for a Fieldian representation theorem with its embedded homomorphism to be considered as explanatory of the applicability of mathematics. But once we disallow Leng's assimilation of physical and mathematical discourse in this way, we find ourselves back with the difficulties raised by talk of extra-mathematical questions; the question of the 'responsibility' of mathematics to something outside mathematics such as the physical world to which it applies is not to be answered by considerations *within* mathematics such as the existence of homomorphisms.

Field and Leng assume we know what it means to say we have a map from physical to mathematical objects or vice versa. *That*, I am suggesting, is something that is particularly called into question by Wittgenstein's distinction between the two ways mathematics can be held 'responsible', or equivalently by Burgess's (non-theory-laden) take on Carnap's inner/outer distinction. We can make straightforward sense of a map from one mathematical structure to another – it is quite another matter when we consider what sense we give to a map from a mathematical structure to a physical structure or vice versa. As I pointed out in Chapter 2, it might seem that a representation theorem does achieve such a map – but that we found to be an illusion. Look again at the statement and proof of Krantz's *Theorem 1*: it is undeniably a part of mathematics, mapping *within* mathematics. To repeat what I said earlier: this theorem is *stated* as a mathematical theorem about mathematical objects; its *proof* consists of mathematical conclusions drawn by deduction from mathematical premises according to conventional mathematical practice. That is, the 'conclusion' – in this case the theorem itself – as well as its establishment,'is

[165] We might recall just here that 'actually' is indeed what Field claims: his fictionalism – of which I take Leng's to be a sub-genre – is *contingent*. There *might have been* numbers, according to Field, although *actually* there are none.

responsible to certain [mathematical] axioms and certain [mathematical] rules', in Wittgenstein's words. It is every bit as much an *intra*-mathematical result as the conclusion we reached just above that there are exactly two groups of order four.

In the end, this is the most valuable insight to be gained from exhibiting an actual such representation theorem rather than simply considering them in the abstract, as it were: it becomes clear that what we might have thought of – are *required* to think of in such manner if we are to accept Field's and Leng's claim to explicate mathematical utility/applicability – as something connecting two realms such as Hardy's physical and mathematical realms, is nothing of the sort. Rather, any such theorem remains – of course, how could it not?, it is a *theorem* – a part of mathematics rather than anything that can link mathematics to something other than itself.

So, to emphasise once again, fictionalism requires sense to be made of external talk about mathematics if it is to explain the utility of mathematics as it sets out to do. The way in which such sense is both absent and yet required becomes plain through our consideration of the homomorphisms at the centre of representation theorems in the light of the distinction between intra- and extra-mathematical discourse as the homomorphism tricks us into thinking that we can link a shadowy fictional realm of non-existent objects with the realm of the real.

I have denominated the sense of fictionalism's talk of a non-existent realm of abstracta as at the same time both absent and required.[166] Recall Leng's and Field's insouciance at the move from the thought that we have no reason to think it true that mathematical objects exist to the thought that such a claim is false.[167] This move itself can only be made supposing that in the context in question we have given sense to the notion that mathematical objects exist, which, as an extra-mathematical supposed proposition, I have suggested it does not. Fictionalism, in short, in attempting to deny the truth of Platonism, illegitimately assumes for Platonism a sense it has yet to be given.

5.6 Steiner; Wittgenstein's 'gap'

I turn away from fictionalism, now, to consider how the distinction Wittgenstein brings to our attention in *LFM* XXV plays out with a specific example of mathematical applicability. The example is Mark Steiner's, from his 2009. It involves group theory; rather more advanced than we encountered in section 5.2, but

[166] Something we saw above to be in some ways explicitly accepted by Field in his discussions of the 'meanings' of mathematical terms.

[167] Recall also Field's claim that the only reason for thinking mathematics true is its utility in science (the *indispensability* argument for mathematical Platonism), denial of which unique purported justification of Platonism lies at the foundation of the fictionalist enterprise.

group theory all the same.[168] Steiner uses the example to locate what he describes as a gap in Wittgenstein's account of mathematical applicability. He claims that 'Wittgenstein's account of mathematical applicability was seriously lacking.' (Steiner 2009 p. 26) I want to show how Wittgenstein gives us the resources to plug the gap Steiner finds.

In fact, Steiner locates this 'gap' in only one of the taxonomic groups of problems he delineates, what he calls 'non-canonical empirical' applications of mathematics. (See section 1.6 above.) Steiner accepts that Wittgenstein deals successfully with 'canonical' applications of mathematics, viz. applications of mathematical propositions that, as Steiner puts it, 'are supervenient on empirical regularities' (*op. cit.* p. 21):

> Did Wittgenstein, then, solve (or, really dissolve) the 'problem' of the applicability of mathematics? For *canonical* applications, yes, in a manner of speaking. Canonical applications are what we call the empirical regularities upon which mathematical theorems are based. Rather than explaining cases of the application of mathematics, Wittgenstein rather explains them away – the applicability of mathematics is an illusion caused by our calling 'mathematics' those very rules founded on what we call their application …(Steiner 2009 p. 23)

This is not wholly wrong as an account of some of what Wittgenstein says. We often find him talking of an empirical regularity being 'hardened' into a rule of inference and thereby becoming a mathematical proposition. For instance

> The justification of the proposition 25 × 25 = 625 is, naturally, that if anyone has been trained in such-and-such a way, then under normal circumstances he gets 625 as the result of multiplying 25 by 25. But the arithmetical proposition does not assert *that*. It is so to speak an empirical proposition hardened into a rule. It stipulates that the rule has been followed only when that is the result of the multiplication. It is thus withdrawn from being checked by experience, but now serves as a paradigm for judging experience. (*RFM* p. 325)[169]

This aspect of Wittgenstein's thought is also apparent in the quotation at the head of this chapter from *Philosophical Investigations* §242, reminding us that ' … what we call "measuring" is partly determined by a certain constancy in results of measurement.' However, although an appreciation of this should be *part* of our overview, we should not take it as – it is far from being – the *whole*.

I am not concerned to challenge Steiner on this aspect of his account of Wittgenstein on such 'canonical applicability'. As I said in Chapter 1, the more interesting cases of mathematical applicability, characterised by Feynman's '*amazing*' or Weinberg's '*positively spooky*', fall into Steiner's *non-canonical* group of problems. It is here that Steiner locates the 'gap' in Wittgenstein's 'account'. This I do wish to

[168] We might recall at this juncture how I used the example of group theory in section 1.1 to begin to motivate an appreciation of the problem of applicability. Here is a payoff for that.
[169] Wittgenstein points out how this 'hardening' is often a to-and-fro affair. (See, *e.g. On Certainty* P. 15). Such matters ramify in many directions, as usual.

challenge, and in so doing, to establish how Wittgenstein offers us the wherewithal to solve the problem of mathematical applicability *tout court*.

Steiner's example of the 'gap' comes via his explication of the use of the group SU(2) in describing the symmetries of the electron. It is an example, he says, of the application of a part of mathematics in a novel situation, an application, as he says, '… of which the 'inventors' of the mathematics could not have dreamed.'

5.7 Steiner; homomorphisms (2)

Steiner summarises his example – 'In order to see how remarkable all this is,' he says:

> The group of 2×2 unitary matrices with determinant +1 was introduced for convenience—to facilitate calculations with rotations needed in the kinematics of rigid bodies. The fact that this representation was not an isomorphism, but only a two-to-one homomorphism onto the group of spatial rotations did not interfere with this application, although it apparently introduced superfluous information about each rotation: its parity. Yet this duplication was hinting at a fundamental topological difference between space and rotations of that very space: though space is simply connected (each closed loop in space can be shrunk to a point), the group of rotations on space is not. Nature makes use of this extra degree of freedom in the electron; its symmetry group is not that of the rotations, but of SU(2) itself. (Steiner 2009 p. 26)

Steiner describes 'this representation' as 'a two-to-one homomorphism'. The 'representation' here is similar, though not identical to, 'representation' in representation theorems or in non-philosophical and non-mathematical English. There are a number of different kinds of 'representation' in play here, and we should be careful about the differences as well as the similarities. Strictly speaking, a representation of a group in a vector space is a homomorphism from the group to the group of invertible linear transformations on the space. Such a representation, essentially, allows for group theory to be done in vector spaces, with elements of the group considered as linear transformations on the vector space. Or, indeed, we can think of the reverse of this, and consider studying linear transformations on a vector space by doing group theory in the group whose representation sits on the space, as it were. In our case of SU(2), we can think of its representation in the vector space $\mathbf{R}^3$ in terms of a map (a homomorphism of course) taking every element in SU(2) to a particular linear transformation – in fact a rotation – of the vector space $\mathbf{R}^3$. So we can study rotational symmetries in $\mathbf{R}^3$ by looking at the group SU(2).[170]

[170] Steiner credits Felix Klein – he of the 'Klein group' *V* (see section 5.2 above) – with 'inventing' the application of (what we now call) SU(2) to rotations in three dimensions. (Steiner 2009 p. 24). Steiner also reminds us of the *other* nineteenth century way with 3-D rotations: (Hamilton's) quaternions. Unit quaternions are isomorphic to SU(2). Their use has fallen into desuetude despite the fame of their inscription into the fabric of the Brougham Bridge in Dublin by the excited Hamilton in 1843.

However, as I mentioned above, and as Steiner emphasises, the map taking elements of SU(2) to rotations on $\mathbf{R}^3$ is a two-to-one homomorphism.[171] An element of SU(2) can be given as $U = \begin{pmatrix} x & y \\ -\bar{y} & \bar{x} \end{pmatrix}$, where x and y are complex numbers such that $|x|^2 + |y|^2 = 1$. ($\bar{x}$ is the complex conjugate of x.) If we parameterise U in terms of (the *Euler angles*) ϕ_1, θ, ϕ_2, writing $x = \cos\frac{\theta}{2} e^{\frac{i}{2}(\phi_2+\phi_1)}$ and $y = i\sin\frac{\theta}{2} e^{\frac{i}{2}(\phi_2-\phi_1)}$, the homomorphism to SO(3), the group of rotations on $\mathbf{R}^3$, is given by the function f, where the matrix $f(U)$ is

$$\begin{pmatrix} \cos\phi_2\cos\phi_1 - \cos\theta\sin\phi_1\sin\phi_2 & -\cos\phi_2\sin\phi_1 - \cos\theta\cos\phi_1\sin\phi_2 & \sin\phi_2\sin\theta \\ \sin\phi_2\cos\phi_1 + \cos\theta\sin\phi_1\cos\phi_2 & -\sin\phi_2\sin\phi_1 + \cos\theta\cos\phi_1\cos\phi_2 & -\cos\phi_2\sin\theta \\ \sin\theta\sin\phi_1 & \sin\theta\cos\phi_1 & \cos\theta \end{pmatrix}$$

– the matrix which effects the rotation defined by the corresponding Euler angles.

In terms just of the original variables x and y, we will have

$$f(U) = \begin{pmatrix} \mathrm{Re}(x^2 - y^2) & \mathrm{Im}(x^2 + y^2) & -2\,\mathrm{Re}(xy) \\ -\mathrm{Im}(x^2 - y^2) & \mathrm{Re}(x^2 + y^2) & 2\,\mathrm{Im}(xy) \\ 2\,\mathrm{Re}(x\bar{y}) & 2\,\mathrm{Im}(x\bar{y}) & |x|^2 - |y|^2 \end{pmatrix},$$ [172]

and it is immediately evident that $-U = \begin{pmatrix} -x & -y \\ \bar{y} & -\bar{x} \end{pmatrix}$ has the same image under f as U.

A little work gives us the kernel of the homomorphism as {+**I**, – **I**} where **I** is the identity 2×2 matrix; every element in the group of rotations on $\mathbf{R}^3$ is mapped to by two similarly related matrices (+/–) in SU(2). So f is indeed two-to-one as Steiner points out.

This should not worry us for, say, combining simple rotations by using SU(2): we can ignore the overall sign (+/–) of the matrix easily enough. What is remarkable, though, as Steiner claims, is that while each simple rotation in $\mathbf{R}^3$ is coded by two (+/–) corresponding elements of SU(2), the rotational symmetries of the electron do not share this redundancy. The symmetry group of the electron requires the *full*

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[171] For an elementary account of what follows here, see, e.g. Carmeli & Malin 1976, chapters 3-4. For an equivalent development to that involving the non-trigonometric expression above see, e.g. Altmann 1986 P. 128ff. The attraction of working within SU(2) rather than directly with the Euler angles is apparent once we see these results. Still, though, it is clear that we remain within mathematics in all this.

[172] 'Re(z)' is the *real part* of the complex number z; 'Im(z)' is the *imaginary part* of z. (If $z = a + bi$, then Re(z) = a and Im(z) = b.)

SU(2) representation. Given the (apparent) pre-existence of the representation of SU(2) in $\mathbf{R}^3$, and even more, given the 'invention' of the group SU(2) for the purpose of simplifying rotations in space by Klein long ago (in 1888 – the electron itself was not discovered until 1897, much less quantum mechanics itself), this looks like an excellent example of what Steven Weinberg finds 'spooky' and Richard Feynman 'amazing'.

So we seem to have a problem of explaining how this piece of mathematics – SU(2), developed to facilitate work on rotations in $\mathbf{R}^3$ – is just the ticket for applying to the symmetries of the electron. Is there a real problem here?

No. To see why, turn to Wittgenstein's distinction between intra- and extra-mathematical responsibility. The suggestion deriving from this distinction is that the apparent problem comes about by taking questions about mathematics ('*Why* should such a homomorphism exist?') and trying to think of them outside their natural habitat. Considered as part of mathematics, to be answered using the usual tenets of mathematical practice, such questions make sense. However, I am suggesting, following Wittgenstein, that the temptation to think of them as relating to the relation between some mythical (or fictional, or 'italics-added *really* or capital-R Really exist[ing]') mathematical realm and the physical world should be avoided – they are at best *inappropriate* in such a setting.

In the light of this, consider the homomorphism (in fact an isomorphism, as it is one-one) connecting SU(2) with the symmetries of the electron. In Steiner's own explanation, this contrasts with the homomorphism from SU(2) to SO(3), the group of rotational symmetries in $\mathbf{R}^3$. There is an almost irresistible temptation now – to which Steiner succumbs – to continue these latter mapping processes *out* of the mathematics, as it were, and onto the real space 'represented by'[173] $\mathbf{R}^3$ or the real electron and its symmetries as 'represented' by its symmetry group. This, I am claiming, is a mistake. But let us try our best to make sense of this move out of the mathematics. It *looks* as though if we can get a representation theorem[174], this will help answer the question, '*why* should such a homomorphism exist?' The axioms of any such theorem will give as clear a statement as anything can of just what needs to be the case for the mathematics to represent the physics, it seems. What more could we want in terms of answering the question, '*why* this homomorphism?'?

[173] This is a different sort of representation again, involving a semantic difference easily missed.

[174] The representation here goes the other way by comparison with the group representation of SU(2) on $\mathbf{R}^3$, a fact that might give us pause for thought, indeed, in this context. Actually the homomorphism in any representation theorem here will be an isomorphism – hence reversible – so we need not be too picky about it anyway.

So let us consider the form of a putative representation theorem here. We will not get a full statement of any such[175], but at least of some relevant specifics it is clear enough what we would need: given that the symmetries of the electron require to be in a *simply connected* space, SO(3) will not do, whereas SU(2) will work fine. The axioms – the 'given' – of our representation theorem will thus include being simply connected.[176] And we can wave our hands for all the rest, because the point of this can now easily be seen. It is as follows: thinking of the connection between physics and mathematics, we consider how we might represent the physics in the maths. But any analysis of how such a representation could take place inevitably pushes us, willy-nilly, back into mathematics. We thought that a representation theorem could bridge the gap between physics and maths – a closer look helps us to see that this *gap* is not what we thought. Any representation taking place remains within mathematics, be it the representation of SU(2) on $\mathbf{R}^3$ or the representation achieved by a representation theorem.

However, it is still worth thinking about what we get from considering the possibility of a representation theorem in this case (and others), as we did above when we looked at Kranz's *Theorem 1*. We may not have an explanation of why the homomorphism relating mathematics and physics exists, where physics and mathematics are considered as two separate realms: there is no such homomorphism relating mathematics to anything outside of mathematics as matters stand regarding the sense we allow via the 'responsibility' of mathematics to its 'reality'. What we do get from looking carefully at the (putative in this latest case) representation theorem, though, is, once again, an indication – within mathematics – of just what is needed in order that this particular piece of mathematics be capable of applying in the case to hand. This is not an explanation of how mathematics relates to physics, we should note, so much as a *description* of a particular part of mathematics applying to physics:

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[175] There are no extant representation theorems for quantum mechanics, although see Balaguer 1996, also 1998 pp. 113-127 for an interesting suggestion involving the representation of *propensities*.

[176] 'Connectedness' is not quite the same thing here as in Kranz's *Theorem 1*. Steiner does explain (see Steiner 2009 P. 25), although possibly he is a little loose about distinguishing different meanings of 'space' as well as relevant notions of connectedness. The required property of being simply connected (that SU(2) has but SO(3) lacks), is a property of the 'space' of rotations in SU(2) – the 'points' of this 'space' are linear transformations – rotations – and a 'closed loop' in this 'space' is a (continuous) subset of these rotations, each of which is 'close' to its 'neighbours' in the subset. The 'shrinking' of such a 'loop' to a 'point' proceeds by continuous deformation; 'points' move to 'neighbouring points' which are not in the subset. This can all seem very mysterious, particularly if we start to wonder how it all connects with fermion spin. A clear view of the mathematics and the physics is what is needed to dispel any apparent mystery here, though. The details are not important for the philosophical issues to be clear.

> We must do away with all *explanation* and description alone must take its place. (*PI* §109)

5.8 Solved

We have our overview, now. And this overview should disarm any temptation we may have felt to ask inappropriate questions about bridging hypothetical gaps between mythical realms. Such temptation disarmed, we find that we no longer have a problem of mathematical applicability. Think back to Steiner's characterisation of the problem of mathematical applicability as grounded in questions concerning homomorphisms between physical facts and mathematical theorems: '*Why* should such a homomorphism exist?' If, as I have argued, the existence of such a homomorphism is something to which we have so far not given any sense, then we have indeed solved the problem of mathematical applicability by seeing the lack of sense of the intended expression of the problem. '*Solved*', I claim, rather than what has been fashionable locution in some quarters, '*dis*solved' or 'shown up as a *pseudo*-problem'. No:–

> The problems arising through a misinterpretation of our forms of language have the character of *depth*. They are deep disquietudes … (*PI* §111)

The problem of mathematical applicability was a deep disquietude. And it has been solved by looking into the workings of our language and getting a clearer description of those workings – of recognising them '*in despite of* an urge to misunderstand them':

> [Philosophical problems] are solved … by looking into the workings of our language …(*PI* §109)

Solved.

Appendix 1 Groups, Transformations and Homomorphisms

What follows is far from an exhaustive account of even the most elementary parts of basic group theory etc. However, it should give enough of a flavour of the topics to help a non-mathematical philosopher engage adequately with the philosophical issues raised by the examples in the thesis.

Groups

A *group* is a mathematical structure (finite or infinite) consisting of a set of mathematical objects ('elements') together with a way of combining pairs of these objects (a 'binary operation'). Typical elements might be numbers, geometrical transformations, mathematical functions like 'square', 'double', and so on – any things that can be combined. Typical binary operations: addition, multiplication, combination (or 'composition') of transformations or functions. The history of group theory dates back to the eighteenth and nineteenth centuries, Evariste Galois being the first to use the term 'group' in 1832. (Though Galois' work was not published until later. The story is romantic and well-known. I will not tell it.)

The structure of a group depends on the group axioms: in a group G with binary operation *, we must have

Closure: $\forall a,b \in G,\ a*b \in G$

Associativity: $\forall a,b,c \in G,\ (a*b)*c = a*(b*c)$

Identity: $\exists i \in G\ \ \forall a \in G:\ i*a = a = a*i$

Inverse: $\forall a \in G\ \exists a^{-1} \in G: a^{-1}*a = i = a*a^{-1}$

Groups are considered the same if they are 'isomorphic':–

Isomorphism and homomorphism

It is sometimes convenient to exhibit finite groups in a table (a 'Cayley table', for Arthur Cayley, 1821-1895). Here are two such Cayley tables for groups **P** and **Q**, with elements and binary operations respectively <{0, 1, 2, 3, 4, 5}, * > and <{**A, B, C, D, E, F**}, *o* >

P:

*	0	1	2	3	4	5
0	0	1	2	3	4	5
1	1	2	3	4	5	0
2	2	3	4	5	0	1
3	3	4	5	0	1	2
4	4	5	0	1	2	3
5	5	0	1	2	3	4

Q:

o	**A**	**B**	**C**	**D**	**E**	**F**
A	**A**	**B**	**C**	**D**	**E**	**F**
B	**B**	**C**	**D**	**E**	**F**	**A**
C	**C**	**D**	**E**	**F**	**A**	**B**
D	**D**	**E**	**F**	**A**	**B**	**C**
E	**E**	**F**	**A**	**B**	**C**	**D**
F	**F**	**A**	**B**	**C**	**D**	**E**

The first table exhibits the results of combining the elements of **P** using the unspecified operation '*'; the second likewise for **Q** with its elements and operation '*o*'. Elements in such tables are conventionally taken by combining the element from the column on the left with the element in the corresponding row at the top, so, for example 4 * 3 =1. (In this case it would make no difference if we took the reverse order, since 3 * 4 = 1 as well; but this is *not* the case with every group, as we will see.)

To be a little more concrete, we might interpret **P** as the addition table for arithmetic modulo 6 (*'Add, then write down the remainders on dividing by 6*') and **Q** as the combination table for applying the rotation symmetries of a regular hexagon – **A**: rotate 0º; **B**: rotate 30º; **C**: rotate 60º etc. A *symmetry* in this context is a transformation which maps a figure – a regular hexagon in this case – onto itself, and for instance we have **B** *o* **C = D** since the result of combing a rotation of 60º with a rotation of 30º is a rotation of 90º. (Again there are wrinkles here about the *order* of application of the transformations, dealt with below.)

A moment's inspection will be enough to convince us that these two tables are structurally similar: '0' in **P** occurs in relatively the same places as '**A'** in **Q**, '1' in the same places as **'B**' and so on. In fact, the two tables exemplify *the same* mathematical structure. The two groups are 'isomorphic'. We can capture this isomorphism – sameness of structure – algebraically by connecting the elements of **P** and **Q** (here in the obvious way: 0↔**A**; 1↔**B**; 2↔**C** and so on) one-to-one via a function or mapping, *f*, say, from **P** to **Q**, and demanding of *f* that it satisfy the condition

$$\forall i, j \in \mathbf{P},\ f(i * j) = f(i)\, o\, f(j)$$

To see that this *does* indeed capture the notion of *sameness of structure,* consider an example. Take *i* to be 3 and *j* to be 4: we note that 3*4=1 in **P**; that 3 corresponds to **D** (*i.e.* *f*(3)=**D**) and 4 corresponds to **E**; that **D**o**E=B** in **Q**; and that 1 corresponds to **B** under the map *f*. Following this through with the two tables, we can see that the same thing happens whichever pair of elements of **P** we take *i* and *j* to be.

The notion of a *homo*morphism is slightly more general, not requiring 1-1 correspondence between the structures being compared.

Here is another example to help see how this goes:

Q (again):

o	**A**	**B**	**C**	**D**	**E**	**F**
A	**A**	**B**	**C**	**D**	**E**	**F**
B	**B**	**C**	**D**	**E**	**F**	**A**
C	**C**	**D**	**E**	**F**	**A**	**B**
D	**D**	**E**	**F**	**A**	**B**	**C**
E	**E**	**F**	**A**	**B**	**C**	**D**
F	**F**	**A**	**B**	**C**	**D**	**E**

R:

$\oplus$	α	β	χ
α	a	β	χ
β	β	χ	α
χ	χ	α	β

It is maybe not clear that we can usefully compare the structure **Q** with that of **R** in the same way as we did **P** with **Q**. A slight rearrangement will make things clearer, however. First of all, a re-ordering of the rows and columns of **Q** will have no effect on the structure itself, so long as we keep the results of combining two elements the same – **C***o***D** must still equal **F**, for instance, to take an example at random, and so on for all other pairs. Here is a suitably rearranged table for **Q**, then:

Q (rearranged):

o	**A**	**D**	**B**	**E**	**C**	**F**
A	**A**	**D**	**B**	**E**	**C**	**F**
D	**D**	**A**	**E**	**B**	**F**	**C**
B	**B**	**E**	**C**	**F**	**D**	**A**
E	**E**	**B**	**F**	**C**	**A**	**D**
C	**C**	**F**	**D**	**A**	**E**	**B**
F	**F**	**C**	**A**	**D**	**B**	**E**

Now we can see how the two structures **Q** and **R** are structurally related. We can map each element of **Q** onto an element of **R** as follows;

$$\left.\begin{matrix}\mathbf{A}\\ \mathbf{D}\end{matrix}\right\}\longrightarrow\alpha\,;\quad \left.\begin{matrix}\mathbf{B}\\ \mathbf{E}\end{matrix}\right\}\longrightarrow\beta\,;\left.\begin{matrix}\mathbf{C}\\ \mathbf{F}\end{matrix}\right\}\longrightarrow\chi\,.$$

And now, once again, we capture the similarity of structure by means of the same condition as previously, namely that (if g denotes this map from **Q** to **R**)

$$\forall i, j \in \mathbf{Q},\ g(i\,o\,j) = g(i)\oplus g(j)$$

Taking i as **B** and j as **C**, for example, we get g(**B**o**C**) = g(**D**) (since **B**o**C** = **D** from the table **Q**); according to the map defined, g(**B**) = β, g(**C**) = χ and g(**D**) = α; and (from table **R**) $\beta\oplus\chi = \alpha$. So, indeed, we have that g(**B**o**C**) = g(**B**)$\oplus g$(**C**) in this case. Likewise for all other pairs. This property of the mapping g is just what we want in order to characterise the essential similarity of the structures of the two groups **Q** and **R**.

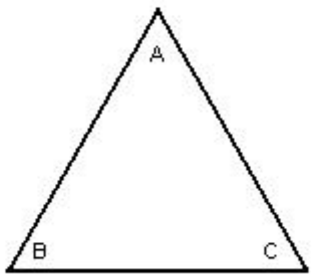
Homomorphisms, cosets and abelian groups

Here is the table for a *different* group with six elements (another group of '*order*' six):

T =

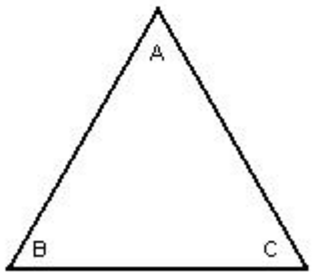
	I	*X*	*Y*	*P*	*Q*	*R*
I	*I*	*X*	*Y*	*P*	*Q*	*R*
X	*X*	*Y*	*I*	*R*	*P*	*Q*
Y	*Y*	*I*	*X*	*Q*	*R*	*P*
P	*P*	*Q*	*R*	*I*	*X*	*Y*
Q	*Q*	*R*	*P*	*Y*	*I*	*X*
R	*R*	*P*	*Q*	*X*	*Y*	*I*

In fact, this is the group of symmetries of an equilateral triangle. (Or, more precisely, the symmetries of an equilateral triangle is one instantiation of this group. We need not be so precise, generally.) Suppose we have an equilateral triangle with vertices labelled A, B and C (to keep track of their positions think of the labels as stuck onto the triangle):

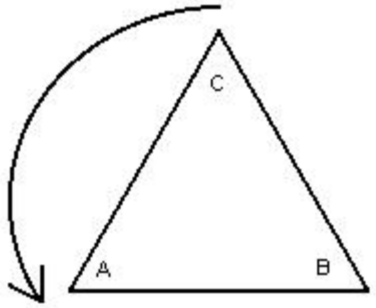


We can 'transform' such a triangle in six different ways so that it ends up in the same place (although with some or all of A, B and C occupying a different position). That is, there are six symmetry transformations of the equilateral triangle –six '*symmetries*' for short. These symmetries are as follows.

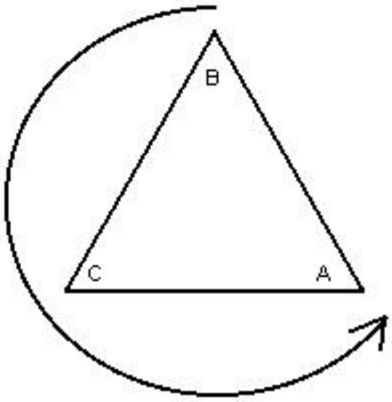
I: leave the triangle where it is (the *identity* transformation).



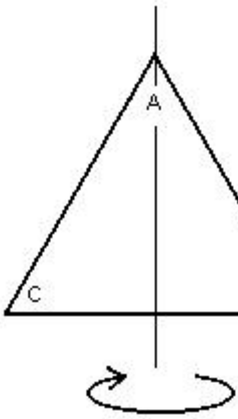
X: Rotate 120° (about the centroid of the triangle – the obvious point at its centre). This transforms the triangle like this:



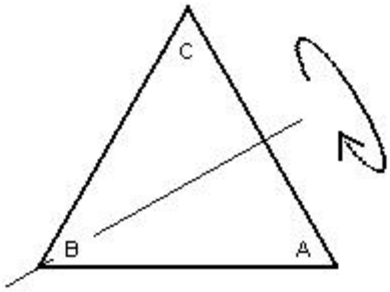
Y: Rotate 240°:



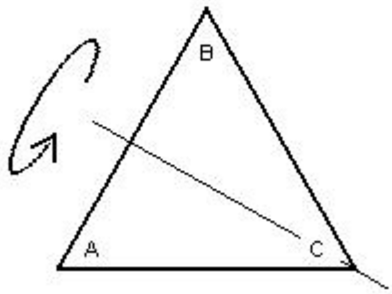
P: Reflect in a (double-sided) mirror placed on the altitude through the top vertex:



Q: Reflect in the altitude through the left lower vertex:



R: Reflect in the altitude through the lower right vertex:



Note that I have not specified in the table above how the group operation is symbolised. This is standard practice whenever, as here, there will be no confusion thereby caused. Concatenate the names of the transformations to indicate the combination: thus we have $X\ P = R$ in the table, for instance. The group operation is so-called 'composition' of transformations, so that, for instance $X\ P$ is to be read as 'perform *P* then perform *X* '. (This probably seems backwards when first encountered. It will become clearer below.) 'Reflect in the vertical altitude, then rotate 120^{o}' is indeed equivalent in effect to the single transformation 'Reflect in the altitude through the lower right vertex'.

Looking at this table **T**, now, it is clear that we do not have the same structure as above with **P** and **Q**. And no rearrangement will do the trick for us either, as a consideration of the symmetry of the different *tables* will make plain. **P** and **Q**, as Cayley tables, are each symmetric in the leading diagonal (upper left to bottom right), whereas **T** is not. This is because, whereas for each pair of elements in **P** we have a*b = b*a, it is not the case that, for instance, $X\ P = P\ X$ in **T**. $X\ P = R$, as we saw, but $P\ X = Q$; 'rotate 120^{o}, then reflect in the vertical altitude' is equivalent to 'reflect in altitude through the lower *left* vertex'.

The group **T** is not *commutative*, whereas our other group of order six is: that is, it is not the case that $\forall a,b \in \mathbf{T}, ab=ba$, whereas it *is* true that $\forall a,b \in \mathbf{Q}, a o b = b o a$. A commutative group such as **Q** is called '*abelian*' (after Niels Henrik Abel, 1802-1829). An abelian group cannot be isomorphic to a non-abelian group.

We might notice that **T** contains a subset {*I*, *X*, *Y* }, which is isomorphic to the group **R** we used above to illustrate the notion of homomorphism. In fact this is a '*subgroup*' of **T**. We might also notice that as well as this subgroup, in the table there is also the subset {*P*, *Q*, *R*} that reappears alternatively with the subgroup {*I*, *X*, *Y* }. {*P*, *Q*, *R*} is called a '*coset*' of the subgroup {*I*, *X*, *Y* } in **T**. (Compare the 'rearrangement' of **Q** above; there it is easy to see the cosets of the subgroup {**A**, **D**} of **Q**.)

Cosets, normal subgroups and quotient groups

Some definitions:

(We had the definition of a homomorphism above.) Given a homomorphism *f* from a group *G* to a group *H*, the set of elements of *G* that map to the identity element of *H* under *f* is called the *kernel* of *f*, written *ker*(*f*).

A *subgroup* of a group is a subset of the group which is itself a group under the group operation.

A (*left*) *coset* of a subgroup *H* in a group *G*, denoted *gH* (where $g \in G$) is the set $\{gh : h \in H\}$. Likewise a (*right*) *coset*, *Hg*, is defined as $\{hg : h \in H\}$.

(Of course if the group is abelian, left cosets and right cosets coincide; not so for non-abelian groups. However, *some* non-abelian groups may have subgroups whose left- and right- cosets *do* coincide. These are the '*normal*' subgroups:–)

A subgroup *N* of a group *G* is *normal* in *G* $\overset{def}{\Leftrightarrow} \forall g \in G \; gN = Ng$, where *gN* and *Ng* are cosets as defined above.

Now consider the set $G / N \overset{def}{=} \{aN : \mathrm{a} \in \mathrm{G}\}$, where *N* is a normal subgroup of *G*. That is, *G / N* is the set of all cosets of *N* in *G* (*left* or *right* being immaterial because *N* is normal in *G*). *G / N* is a group with operation defined as follows:

$\forall aN, bN \in G / N, \; aNbN \overset{def}{=} abN$. With this operation, *G / N* is called the *quotient* or *factor* group of *G* by *N*.

There is a natural (or *canonical*) homomorphism *p* from *G* to G/N : $\forall g \in G, p(g) = gN$. That is, any element *g* of *G* maps to the coset of *N* in *G* that contains *g*. Now, the identity of the quotient group is *iN*, where *i* is the identity in *G*; and the kernel of the homomorphism *p* is just the normal subgroup *N* itself.

In fact, for *any* homomorphism *f* from one group *G* to another *H*, the kernel of *f* is a normal subgroup of *G*.

– Suppose now that *G* and *H* are groups and *f* is a homomorphism from *G* to *H*. Let *N* be a normal subgroup of *G* and *p* the canonical homomorphism from *G* to the quotient G / N as above. Then if N is a subgroup of the kernel of f there exists a unique homomorphism h mapping G / N to H such that $f = h\ p$. This is the 'fundamental homomorphism theorem'. (And if N is *all* of the kernel of f, we have the 'first isomorphism theorem', that H is isomorphic to N, the kernel of f.)

A group with no normal subgroups is called *simple*. By factoring groups with normal subgroups/kernels of homomorphisms, we can reduce groups to simple groups, much as factoring composite numbers gives us primes. Or we can build in the other direction. We get more *structure* doing this in groups than with numbers of course. There is a long story here to tell. Suffice for now that homomorphisms, their kernels, and normal subgroups are tightly bound together. We might say that this is where homomorphisms are most at home, connecting groups with their quotients by normal subgroups.

Linear algebra in the plane

We might consider other mathematical structures defined axiomatically rather in the way I started above with group theory; amongst such structures might be that of a *vector space*, a generalisation of geometry as pursued by Euclid, though with many more applications than the strictly geometrical. I do not wish to take the axiomatic route here, though. We can be much more rough-and-ready given that we need just to get the flavour.

Consider the group I called **T** above – the group of symmetries of an equilateral triangle, of certain *transformations* in a (two-dimensional) plane. We may think of doing geometry by considering transformations more generally. One way of doing the computation is to use *matrices* and *vectors.*

Suppose I have a point in the (Euclidean) plane, furnished with Cartesian axes in the usual way, so that it is specified as $P(x,\ y)$ relative to such (rectangular, '*orthonormal*') axes. Any transformation of this point P that moves it to another point depending on where exactly P is – on the values of x and y, that is – might usefully be expressed as a transformation (call it $\boldsymbol{A}$) that maps $(x,\ y)$ to the point $(a_{1,1}x + a_{1,2}y,\ a_{2,1}x + a_{2,2}y)$. Any such transformation will be a so-called *linear* transformation.

It is convenient to characterise such a transformation in terms of matrices and vectors as follows. Consider each point in terms of a directed line segment from the origin of coordinates, so that the instead of the *point* $(x,\ y)$ we use the *vector* $\begin{pmatrix} x \\ y \end{pmatrix}$.

The point that $P(x,\ y)$ moves to (called the *image* of P under a given transformation) can be represented by $P'(x', y')$, with corresponding vector $\begin{pmatrix} x' \\ y' \end{pmatrix}$. So, for our general linear transformation, we will have, in vector terms, that $\begin{pmatrix} x' \\ y' \end{pmatrix} = \begin{pmatrix} a_{1,1}x + a_{1,2}y \\ a_{2,1}x + a_{2,2}y \end{pmatrix}$. We can pull this apart a little, now, notationally, and write $\begin{pmatrix} x' \\ y' \end{pmatrix} = \begin{pmatrix} a_{1,1} & a_{1,2} \\ a_{2,1} & a_{2,2} \end{pmatrix}\begin{pmatrix} x \\ y \end{pmatrix}$, where the *matrix* $\begin{pmatrix} a_{1,1} & a_{1,2} \\ a_{2,1} & a_{2,2} \end{pmatrix}$ can also be called $\boldsymbol{A}$. Think of the matrix *or* the transformation it encodes as operating on the vector $\begin{pmatrix} x \\ y \end{pmatrix}$ to get the vector $\begin{pmatrix} x' \\ y' \end{pmatrix}$, its image under the transformation.

Now consider combining transformations as we did above to get the table **T**. The 'multiplication' of the vector $\begin{pmatrix} x \\ y \end{pmatrix}$ by the matrix $\boldsymbol{A}$ to get the vector $\begin{pmatrix} x' \\ y' \end{pmatrix}$ has a similar form when we multiply (I will leave off the scare-quotes) matrices themselves. Call $\boldsymbol{B}$ the matrix (and the transformation it encodes) $\begin{pmatrix} b_{1,1} & b_{1,2} \\ b_{2,1} & b_{2,2} \end{pmatrix}$. Multiplying $\boldsymbol{B}$ by $\boldsymbol{A}$ according to the schema gives us

$$\begin{pmatrix} a_{1,1} & a_{1,2} \\ a_{2,1} & a_{2,2} \end{pmatrix}\begin{pmatrix} b_{1,1} & b_{1,2} \\ b_{2,1} & b_{2,2} \end{pmatrix} = \begin{pmatrix} a_{1,1}b_{1,1} + a_{1,2}b_{2,1} & a_{1,1}b_{1,2} + a_{1,2}b_{2,2} \\ a_{2,1}b_{1,1} + a_{2,2}b_{2,1} & a_{2,1}b_{1,2} + a_{2,2}b_{2,2} \end{pmatrix}$$

... and the matrix $\begin{pmatrix} a_{1,1}b_{1,1} + a_{1,2}b_{2,1} & a_{1,1}b_{1,2} + a_{1,2}b_{2,2} \\ a_{2,1}b_{1,1} + a_{2,2}b_{2,1} & a_{2,1}b_{1,2} + a_{2,2}b_{2,2} \end{pmatrix}$ does correctly encode the transformation 'perform $\boldsymbol{B}$ than perform $\boldsymbol{A}$'. (Think of this as 'multiply rows by columns, then add.) Note the order here: we want that $\left(\begin{pmatrix} a_{1,1} & a_{1,2} \\ a_{2,1} & a_{2,2} \end{pmatrix}\begin{pmatrix} b_{1,1} & b_{1,2} \\ b_{2,1} & b_{2,2} \end{pmatrix}\right)\begin{pmatrix} x \\ y \end{pmatrix}$, that is, the result of applying the composite transformation to *P*, to be the same as $\begin{pmatrix} a_{1,1} & a_{1,2} \\ a_{2,1} & a_{2,2} \end{pmatrix}\left(\begin{pmatrix} b_{1,1} & b_{1,2} \\ b_{2,1} & b_{2,2} \end{pmatrix}\begin{pmatrix} x \\ y \end{pmatrix}\right)$, which is the result of applying $\boldsymbol{A}$ to the result of applying $\boldsymbol{B}$ to *P*. (It should be clear now why the convention here is *not* 'backwards', as it might have appeared.)

What we have here is a statement that matrix multiplication is associative. And the rest works equally well. In fact, the group of linear transformations in the plane under composition is isomorphic to the group of matrices under matrix multiplication as so defined. So we can *do* transformations in the plane by multiplying matrices.

Linear transformations in 3-dimensions; Euler angles.

A little trigonometry shows us that a *rotation* in the plane with angle θ is represented by the matrix $\begin{pmatrix} \cos\theta & -\sin\theta \\ \sin\theta & \cos\theta \end{pmatrix}$. This can be obtained by checking the images of the *base vectors* in the plane under the transformation: $\begin{pmatrix} 1 \\ 0 \end{pmatrix} \longrightarrow \begin{pmatrix} \cos\theta \\ \sin\theta \end{pmatrix}$, and $\begin{pmatrix} 0 \\ 1 \end{pmatrix} \longrightarrow \begin{pmatrix} -\sin\theta \\ \cos\theta \end{pmatrix}$ (This works so nicely because the identity matrix is $\begin{pmatrix} 1 & 0 \\ 0 & 1 \end{pmatrix}$: consider multiplying this by the transformation matrix.)

Moving to 3-dimensions is simple enough, once we have this. Matrices become three-by-three, and for any transformation, to obtain its matrix we need to consider its columns as the images under the transformation of the base vectors in the directions of the *x*, *y*, and *z* axes respectively. Matrix multiplication extends in the obvious way into 3 dimensions – 'multiply rows by columns and add', just with an extra entry in each row and column.

Rotations in 3-D are conveniently represented in terms of so-called *Euler* angles (after Leonhard Euler, 1707-1783). There are several different conventions here. I adopt the following convention: any rotation can be expressed in terms of three (Euler) angles ϕ_1, θ, and ϕ_2 as follows:

Rotate about the *z*-axis through an angle ϕ_1. (Call this $\boldsymbol{R_1}$)

Rotate about the *new position of* the *x*-axis through an angle θ. ($\boldsymbol{R_2}$)

Rotate about the *new position of* the *z*-axis through an angle of ϕ_2. ($\boldsymbol{R_3}$)

Each of these rotations can be thought of as a rotation in a plane: we can see how we get the matrices for each of the rotations $\boldsymbol{R_1}$, $\boldsymbol{R_2}$ and $\boldsymbol{R_3}$ by embedding appropriate 2-D matrices in our 3-D encodings:

1. $\begin{pmatrix} \cos\phi_1 & -\sin\phi_1 & 0 \\ \sin\phi_1 & \cos\phi_1 & 0 \\ 0 & 0 & 1 \end{pmatrix}$ (Note the *z*-axis $\begin{pmatrix} 0 \\ 0 \\ 1 \end{pmatrix}$ is fixed.)

2. $\begin{pmatrix} 1 & 0 & 0 \\ 0 & \cos\theta & -\sin\theta \\ 0 & \sin\theta & \cos\theta \end{pmatrix}$ (*New x*-axis fixed.)

3. $\begin{pmatrix} \cos\phi_2 & -\sin\phi_2 & 0 \\ \sin\phi_2 & \cos\phi_2 & 0 \\ 0 & 0 & 1 \end{pmatrix}$ (New *z*-axis fixed.)

Now the matrix for the whole rotation can be obtained by finding the product $\boldsymbol{R_3 R_2 R_1}$ (in that order, remember – and note that the product as written is well-defined because the operation is associative: $(\boldsymbol{R_3 R_2})\boldsymbol{R_1} = \boldsymbol{R_3}(\boldsymbol{R_2 R_1})$). I leave it as an exercise that this gives the right answer (see Chapter 5).

The group of rotations in 3-dimensional Euclidean space is denoted SO(3) (the *special orthogonal group* of 3-by-3 real matrices). In Chapter 5 above, I describe the homomorphism from SU(2) (the *special unitary group* of 2-by-2 *complex* matrices) to SO(3). I will not go over that again here. These are *continuous* infinite groups. Once again, much more can be said, but there is enough here to give an idea of what is going on.

Appendix 2 A Representation Theorem

This is *Theorem 1* from Krantz *et al* 1971. (See P. 15.) I follow the proof outlined there, more or less, although I fill in much of the detail.

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

The relational structure $< A, \succsim >$ is said to be a ***weak order*** iff for all a, b and c from the set A, the following conditions hold:

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

Armed with this definition, we are ready to state the representation theorem: Suppose that A is a finite non-empty set. If $< A, \succsim >$ 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, $a \succsim b$ iff $\phi(a) \geq \phi(b)$. (That is to say, of course, that ϕ is a representing homomorphism.)

Proof:

Define a new relation on A, " $\sim$ " , as follows: $a \sim b$ iff $a \succsim b$ and $b \succsim a$.

This relation, now, is

Reflexive (i.e. $a \sim a$ for every a), since

$$a \succsim a \text{ for every } a$$

(by Condition 1. above)

Symmetric (i.e. if $a \sim b$, then $b \sim a$), since

$$\begin{aligned} a \sim b \quad &\Rightarrow \quad a \succsim b \text{ and } b \succsim a \\ &\Rightarrow \quad b \succsim a \text{ and } a \succsim b \\ &\Rightarrow \quad b \sim a \end{aligned}$$

and

Transitive (i.e. if $a \sim b$ and $b \sim c$, then $a \sim c$), since

$$\begin{aligned} a \sim b \text{ and } b \sim c \quad &\Rightarrow \quad (a \succsim b \text{ and } b \succsim a) \text{ and } (b \succsim c \text{ and } c \succsim b) \\ &\Rightarrow \quad (a \succsim b \text{ and } b \succsim c) \text{ and } (c \succsim b \text{ and } b \succsim a) \\ &\Rightarrow \quad a \succsim c \text{ and } c \succsim a \quad \text{(by Condition 2. above)} \\ &\Rightarrow \quad a \sim c \end{aligned}$$

Thus the relation " ~ " is an *equivalence relation* on A, and so partitions A into disjoint equivalence classes.

We can name these equivalence classes: $\tilde{\mathbf{a}} = \{x : x \in A, x \sim a\}$, etc.

That is, $\tilde{\mathbf{a}}$ is the set of all those members of A that are related to a by the relation " ~ ", and likewise for $\tilde{\mathbf{b}}$, the set of members of A related to b, and so on. Since " ~ " is an equivalence relation, as we have seen, $\tilde{\mathbf{a}}$ and $\tilde{\mathbf{b}}$ have no members in common unless they have all their members in common, and every member of A is contained in one of the equivalence classes, $\tilde{\mathbf{a}}$, $\tilde{\mathbf{b}}$, ….

We can give the set of these equivalence classes a name, now:

$$\tilde{\mathbf{A}} = \{\ \tilde{\mathbf{a}},\ \ \tilde{\mathbf{b}},\ \ \tilde{\mathbf{c}},\ \ ...\ \}$$

And we can define a relation on $\tilde{\mathbf{A}}$: $\tilde{\mathbf{a}} \ \tilde{\succ} \ \tilde{\mathbf{b}}$ iff $a \gtrsim b$.

Now, this relation " $\tilde{\succ}$ " has the following properties:

1. *Connectedness* ($\tilde{\mathbf{a}} \ \tilde{\succ} \ \tilde{\mathbf{b}}$ or $\tilde{\mathbf{b}} \ \tilde{\succ} \ \tilde{\mathbf{a}}$ for every $\tilde{\mathbf{a}}$, $\tilde{\mathbf{b}}$, recall)

   – this follows directly from the connectedness of " $\gtrsim$ ".

2. *Transitivity* ($\tilde{\mathbf{a}} \ \tilde{\succ} \ \tilde{\mathbf{b}}$ and $\tilde{\mathbf{b}} \ \tilde{\succ} \ \tilde{\mathbf{c}} \ \Rightarrow \ \tilde{\mathbf{a}} \ \tilde{\succ} \ \tilde{\mathbf{c}}$)

   – again, this follows from the transitivity of " $\gtrsim$ ":

$$\begin{aligned} \tilde{\mathbf{a}} \ \tilde{\succ} \ \tilde{\mathbf{b}} \text{ and } \tilde{\mathbf{b}} \ \tilde{\succ} \ \tilde{\mathbf{c}} \quad &\Rightarrow \quad a \gtrsim b \text{ and } b \gtrsim c \\ &\Rightarrow \quad a \gtrsim c \\ &\Rightarrow \quad \tilde{\mathbf{a}} \ \tilde{\succ} \ \tilde{\mathbf{c}} \end{aligned}$$

3. *Antisymmetry* ($\tilde{\mathbf{a}} \ \tilde{\succ} \ \tilde{\mathbf{b}}$ and $\tilde{\mathbf{b}} \ \tilde{\succ} \ \tilde{\mathbf{a}} \ \Rightarrow \ \tilde{\mathbf{a}} = \tilde{\mathbf{b}}$):

$$\begin{aligned} \text{since} \quad \tilde{\mathbf{a}} \ \tilde{\succ} \ \tilde{\mathbf{b}} \text{ and } \tilde{\mathbf{b}} \ \tilde{\succ} \ \tilde{\mathbf{a}} \quad &\Rightarrow \quad a \gtrsim b \text{ and } b \gtrsim a \\ &\Rightarrow \quad a \sim b \\ &\Rightarrow \quad a, b \in \tilde{\mathbf{a}} \\ &\Rightarrow \quad \tilde{\mathbf{a}} = \tilde{\mathbf{b}} \end{aligned}$$

(The relation " $\tilde{\succ}$ " is said to define a *simple order* on $\tilde{\mathbf{A}}$. What we have done is use the weak order defined by " $\gtrsim$ " on A to get to this simple order on the set of equivalence classes. It is probably clear what to do now:)

Define a function $\tilde{\phi}$ on $\tilde{\mathbf{A}}$ as follows:

$$\tilde{\phi}(\tilde{\mathbf{a}}) = \text{the number of equivalence classes } \tilde{\mathbf{b}} \text{ such that } \tilde{\mathbf{a}} \tilde{\succ} \tilde{\mathbf{b}} \;.$$

I claim that $\tilde{\mathbf{a}} \tilde{\succ} \tilde{\mathbf{b}}$ iff $\tilde{\phi}(\tilde{\mathbf{a}}) \geq \tilde{\phi}(\tilde{\mathbf{b}})$ (this " $\geq$ ", of course, is the usual relation 'greater than or equal to' on the set of natural numbers):

For suppose that $\tilde{\mathbf{a}} \tilde{\succ} \tilde{\mathbf{b}}$: then, for any $\tilde{\mathbf{c}}$, $\tilde{\mathbf{b}} \tilde{\succ} \tilde{\mathbf{c}} \Rightarrow \tilde{\mathbf{a}} \tilde{\succ} \tilde{\mathbf{c}}$, by transitivity. So if $\tilde{\mathbf{c}}$ is counted for $\tilde{\phi}(\tilde{\mathbf{b}})$, it is also counted for $\tilde{\phi}(\tilde{\mathbf{a}})$. Hence $\tilde{\phi}(\tilde{\mathbf{a}}) \geq \tilde{\phi}(\tilde{\mathbf{b}})$.

Conversely, suppose that it is not the case that $\tilde{\mathbf{a}} \tilde{\succ} \tilde{\mathbf{b}}$: then $\tilde{\mathbf{b}} \tilde{\succ} \tilde{\mathbf{a}}$ by connectedness: it follows that it is not the case that a $\gtrsim$ b, but that b $\gtrsim$ a. So a and b are not related by " ~ ". Hence there is at least one element of $\tilde{\mathbf{A}}$, namely $\tilde{\mathbf{b}}$, which is counted in $\tilde{\phi}(\tilde{\mathbf{b}})$ but not in $\tilde{\phi}(\tilde{\mathbf{a}})$. So $\tilde{\phi}(\tilde{\mathbf{b}}) > \tilde{\phi}(\tilde{\mathbf{a}})$ (where, of course, once again, " > " is the usual 'greater than' relation on the set of natural numbers.)

We have proved, then, to recap, that $\tilde{\mathbf{a}} \tilde{\succ} \tilde{\mathbf{b}}$ iff $\tilde{\phi}(\tilde{\mathbf{a}}) \geq \tilde{\phi}(\tilde{\mathbf{b}})$. All that remains to do is to define the function ϕ on A by $\phi(\mathrm{a}) = \tilde{\phi}(\tilde{\mathbf{a}})$ for every a in A.

So we have a $\gtrsim$ b iff $\tilde{\mathbf{a}} \tilde{\succ} \tilde{\mathbf{b}}$

iff $\tilde{\phi}(\tilde{\mathbf{a}}) \geq \tilde{\phi}(\tilde{\mathbf{b}})$

iff $\phi(\mathrm{a}) \geq \phi(\mathrm{b})$.

That completes the proof of the representation theorem.

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