Existence, knowledge & truth in mathematics

Stephen Charles Ion Jones

Thesis Abstract:
Stephen Charles Ion Jones,
Existence, Knowledge, & Truth in Mathematics,
MLitt – 1997.

This thesis offers an overview of some current work in the philosophy of mathematics, in particular of work on the metaphysical, epistemological, and semantic problems associated with mathematics, and it also offers a theory about what type of entities numbers are.

Starting with a brief look at the historical and philosophical background to the problems of knowledge of mathematical facts and entities, the thesis then tackles in depth, and ultimately rejects as flawed, the work in this area of Hartry Field, Penelope Maddy, Jonathan Lowe, John Bigelow, and also some aspects of the work of Philip Kitcher and David Armstrong. Rejecting both nominalism and physicalism, but accepting accounts from Bigelow and Armstrong that numbers can be construed as relations, the view taken in this work is that mathematical objects, numbers in particular, are universals, and as such are mind dependent entities. It is important to the arguments leading to this conception of mathematical objects, that there is a notion of aspectual seeing involved in mathematical conception. Another important feature incorporated is the notion, derived from Anscombe, of an intentional object.

This study finishes by sketching what appears to be a fruitful line of enquiry with some significant advantages over the other accounts discussed. The line taken is that the natural numbers are mind dependent intentional relations holding between intentional individuals, and that other classes of number – the rationals, the reals, and so on – are mind dependent intentional relations holding between other intentional relations. The distinction in type between the natural numbers and the rest, is the intuitive one that is drawn naturally in language between the objects referred to by the so-called count nouns, and the objects referred to by the so-called mass nouns.

EXISTENCE, KNOWLEDGE, & TRUTH in MATHEMATICS.

Stephen Charles Ion Jones

MLitt Thesis

University of Durham
Department of Philosophy

1997

CONTENTS

1. INTRODUCTION.

> It has been sufficiently pointed out that the objects of mathematics are not substances in a higher degree than bodies are, and that they are not prior to sensibles in being, but only in definition, and that they cannot exist somewhere apart. But since it was not possible for them to exist *in* sensibles either, it is plain that they either do not exist at all or exist in a special sense and therefore do not "exist" without qualification. For "exist" has many senses.[1]

In this work I propose to examine the nest of problems involved with mathematical existence, and also the closely related problems of mathematical knowledge and mathematical truth. Starting with a brief historical introduction to the problems, I endeavour to trace the deep roots back from Greek mathematics through to the "three schools" of philosophy of mathematics – logicism, formalism, and intuitionism – in the early twentieth century. I move on to consider the basic challenges that must be met by any adequate account of mathematics, and then I survey the (mostly) current work on issues of realism and anti-realism in the philosophy of mathematics. What is striking about this is how the small field of philosophy of mathematics acts as a microcosm of larger philosophical issues; we can see played out on this small stage a set of arguments that are structurally similar to those aimed at broader debates over realism and anti-realism. For example we can see that Penelope Maddy's arguments, that we perceive a set by perceiving its individual members, are in some way structurally similar to Russell's (et al) arguments that we perceive a physical object by perceiving "its" sense data. It is also interesting to note in this summary of some of the current philosophical positions, just how many of them have their roots in ancient and medieval philosophy – of some of those that I later deal with in detail, it turns out that I can describe them as nominalist, Platonist, Aristotelian, and Pythagorean.

So, after these first background considerations, I turn to consider two quite contrasting positions in detail – these are the nominalism of Hartry Field, and the Platonism of Penelope Maddy. Field's nominalistic

---

1. Aristotle, *Metaphysics M*, 1077b, 12-17, quoted from Ross, W.D. (Ed) [1908], *Aristotle's Works Vol VIII*, (Oxford, 1908).

stance, in which he accepts the reality of scientific theoretical entities yet rejects as fictions mathematical entities, I believe founders, not only because it seems that he cannot carry out his programme to nominalize the whole of science, but also because of what appear to be strong technical objections. I hope to undermine as technically flawed his principal pillar – that mathematics is conservative rather than true – so that it cannot any longer bear the weight that he requires it to bear. Also, I hope to show that his attempt to nominalize science, by removing just the mathematical entities, is incoherent, in the sense that it is arbitrary – he wants to reject mathematical entities, and yet is happy to accept a whole host of other equally puzzling and possibly abstract objects.

To contrast with Field's account, I look next at Maddy's Platonistic position, within which she wishes to maintain a strong naturalistic line based on Quine. She justifies our belief in the real existence of mathematical entities by an argument from Quine which suggests that, since they are indispensable for our best naturalistic theory of the world, then they must exist, and also she endeavours to show that at least some mathematical entities, sets, are in fact perceptible. The Quinian argument proves hard to undermine, but I hope to point out the full cost of its acceptance, that cost being a collateral acceptance of the contingency of mathematics; also I reject her claim that sets are in fact perceptible by using a technical argument from Chihara.

Following on from Maddy, for whom numbers are properties of sets and thus universals, I consider two accounts which are along the 'numbers as universals' line. Jonathan Lowe's account, relying as Maddy's does on the perceptibility of sets, puts forward the thesis that numbers are sorts instantiated (in an Aristotelian sense) by individual sets. I argue that this account is unsatisfactory for a number of reasons: principally because of its reliance on the perception of sets (a problem which Lowe may be able to avoid, for he weakens the requirement compared with Maddy); because of the failure of the account to deal with simple mathematical operations such as addition and multiplication; and also because of its failure to comply with Lowe's own ideas about sorts. An alternative 'numbers as universals' story is told by John Bigelow, whose claim is that (natural) numbers are *relations among* objects – but not *properties of* sets of objects – and that these relations are wholly physical. This account avoids several of the problems that cropped up for Maddy and Lowe which stemmed from their need for sets to be the (perceptible) individuals instantiating the number properties and so on; sets for Bigelow are on a par with all other mathematical objects. The price that he pays is in having to reconsider the roles of both spatial and temporal permanence as criteria for physical existence; he even endeavours to undermine our basic preconceptions about locality. A set of examples which I use to illustrate the strength of Bigelow's position, compared with that of Lowe, rely on the possibility of seeing a mathematical statement in several ways (or as several "things"); because of this, I conclude this chapter with some psychological and philosophical comments (in particular Barrie Falk's comments on what he calls the "s-phenomenal") on the phenomenon of "seeing-as" (or aspectual seeing), which I consider to be an important fact in the mathematical story.

With Bigelow we seem to be close to a plausible story, but two alternative approaches are considered at this point to see if further light can be shed on the issues raised thus far – in particular the problem of knowledge in mathematics. Philip Kitcher's idealizing theory stresses the importance of mathematics arising out of, and possibly even reducible to, our possible actions and operations in the world – something which finds an echo from Falk's s-phenomenal notion. A major part of Kitcher's interest is knowledge, and his analysis of this entails that mathematical knowledge cannot be a priori. This seems to be a high price to pay, and in discussing whether he is in fact justified in this position, we end up looking at David Armstrong's view of the a priori and of mathematics – here not only do we find some good reasons to restore the a priori in mathematics, but also we end up with another very plausible conception of numbers as relations, an account that is very similar to Bigelow's.

I finish by suggesting a possible way out of the maze of problems, by putting forward a hopefully fruitful line of enquiry with the potential to circumvent many of the problems that beset the accounts so far discussed. The principle idea is that mathematical objects are – as Bigelow and Armstrong argue – relations among individuals, but that these are not in fact part of an independent physical reality as Bigelow would insist, rather that they are mind dependent constructs which, in perhaps a Quinian sense, we ourselves bring to our perceptions in order to make (scientific) sense of the world. To do this I adduce some arguments from examples of simple mathematics and the notion of "seeing as"; I refer to this as *intentional* perception. My conception of numbers as relations owes more to Bigelow than to Armstrong, for in Bigelow there is a distinction to be drawn between the natural numbers, and the rest (rationals, reals, etc.); I believe that this is in fact a natural distinction which is captured in our language by the distinction between count nouns (associated with the natural numbers) and mass nouns (associated with rationals and reals).

Clearly we will end up with some difficulties ourselves by taking this line, and the problems of zero and infinity, and the difficulty with a possible loss of objectivity in mathematical truth, are ultimately hard to dislodge.

The scope of the debate opened up in this study is very broad; much broader in fact than I would have chosen with hindsight. It is a consequence of this breadth that some of the points and arguments which I would like to have dealt with in more depth, have been cut down somewhat due to the pressures of both time and space available for them. In these cases (or at least in as many of them as possible) I have tried to take up the relevant points in the footnotes, and to indicate there where those lines of enquiry might be going. Thus the footnotes are especially important here.

2. PRELIMINARIES 1: HISTORICAL BACKGROUND.

In this chapter I would first like to trace out two separate strands in the history of the ideas that have led to some of our preconceptions about the nature of mathematics. It is perhaps not surprising that both of these strands start with Euclid[2], for it is with him that mathematics, in this case Greek Geometry, was first systematised.

I First strand.

Firstly we need to consider Euclid's *Elements of Geometry*, written in about 300 BC, which encapsulated and built upon the work of his predecessors, most notably the Pythagoreans, and more importantly, Eudoxus. Geometry had come to the forefront of Greek mathematics, partly because of the pressures put upon the then current systems of arithmetic jointly by the discovery of incommensurability (probably) by the Pythagoreans, and by the problems thrown up by the paradoxes of the Eleatic, Zeno. For example, Pythagorean arithmetic was concerned deeply, mystically even, with whole numbers and their ratios; that the diagonal of a square was in fact incommensurate with the side of that square was a heavy blow to Pythagorean ideals[3]. The Pythagorean system of rational numbers clearly could not cope with such incommensurable quantities, but the fact that such a diagonal could be *constructed* (in the Greek sense by using only straight edge and compasses) meant that the problem of incommensurability did not arise in a geometrical setting where lengths and their ratios were considered fundamental. Further impetus was given to mathematics – now predominantly geometry – by Plato who, despite

---

2. There has in the past been some misunderstanding as to which Euclid we mean. Early editions of Euclid's *Elements* – for example the first English edition of 1570, translated by Sir Henry Billingsley – credit the author as Euclid of Megara. The real Euclid of Megara however predates the author of the *Elements* by several generations, for he was a pupil of Socrates; Euclid of Alexandria is the correct author of the *Elements*, and the Euclid to whom I refer here.

3. In point of fact it is most likely that the Pythagoreans found out about incommensurability by considering the infinite sequence of inscribed regular pentagons within a given regular pentagon. See for example Boyer's [1968], p.80. We can however note that the Pythagoreans were anxious enough about the discovery of incommensurability to drown one of their members – Hippasus of Metapontum – who had the audacity to tell those outside the Pythagorean order of the discovery. See Boyer [1968], p78 ff.

himself not being a mathematician, and perhaps despite also the anti-mathematical influence of his teacher Socrates[4], was a tremendous champion of geometry. We can note that over the doors of the Academy were the words 'Let no one ignorant of geometry enter here', and that institution became very much the mathematical centre of the world. Eudoxus of Cnidus came to the Academy and was a pupil of Plato, and it is his pioneering work in geometry that Euclid could synthesize into his great textbook.

The style of Euclid's great work was, at the time and for many years afterwards, a model of logical rigour: theorems followed on from definitions and propositions (axioms) – which were given as self-evidently true[5] – with incorrigible logic. (Many of the proofs employed a technique known as the method of exhaustion – often called the 'method of the ancients' in post Renaissance times – which has as its basis the law of excluded middle[6].) The geometrical system thus produced was then: (i) logical, since theorems followed deductively from earlier propositions; and (ii) believed to be true, for the fundamental propositions were taken as self-evident. It is a consequence of (i) and (ii) that until the 19th century, Euclidean geometry (known then as just "geometry" of course) was accepted as a body of known statements, whose truth was taken to be incorrigible, which in fact described the physical world.

(It is interesting to note that the structure of Euclid's *Elements* became a model for other attempts to produce systems of flawless fact – perhaps the most notable attempt in philosophy is Spinoza's *Ethics*. This great work was also an inspiration to the logical bent of Thomas Hobbes who, after discovering Pythagoras' Theorem and working through its proof whilst sitting 'in a gentleman's library', was instantly 'made in love with Geometry'[7]. By today's standards, however, the Elements does not

---

4. In his early life Socrates was interested in certain questions of mathematics – why the sum 2+2 was the same as the product 2x2 etc. – but he gave up these interests when he realised that mathematics would not satisfy his desire to understand the essence of things.

5. The fifth postulate – the 'parallel postulate' – is a notable exception to the notion of Euclid's postulates being self-evidently true, for there is a long history of attempts to derive this postulate from the others, resulting, as discussed later, in the discovery of non-Euclidean geometries.

6. This method was considered at the time and, to a lesser extent later in post renaissance mathematics, to be the only acceptable method of proof; it may be seen as ironic then that the intuitionist school of Brouwer, earlier this century, were happy to dispense with the law of excluded middle which lies at its heart.

7. From John Aubrey's *Brief Lives*, quoted in Davis and Hersch [1981], p.149.

quite come up to scratch in terms of strict logic, being 'replete with concealed assumptions, meaningless definitions, and logical inadequacies'[8].)

Attempts to derive the fifth of Euclid's postulates – the rather non-self-evident parallel postulate – from the other postulates, led ultimately to the discovery of non-Euclidean geometries. The first of these was discovered early in the 19th century, by both Bolyai and Lobachevsky separately[9], and later on the work of Reimann, and Klein (who famously generalised the idea of a hierarchy of different geometries in his Erlarger Programm of 1872), knocked down the great edifice of Euclidean geometry as being the one and only (and true) geometric description of the world. These new geometries opened up the possibility of other logical systems with different, possibly "self-evident" axioms, and the fact that these new geometries were just as good as the old Euclidean one meant that either none of them were true, or only one was true, but we could not tell which[10].

Other pressures on mathematics in the 19th century included a general feeling that the calculus, in use by this time for nearly 200 years, needed a better foundation. Dedekind and Weierstrass were forced by the crisis in geometry, produced by the discovery of non-Euclidean geometries, to found both analysis (calculus) and geometry on arithmetic, and we need to note that this foundation required the use of sets, in particular infinite sets, for its achievement[11].

This then is the first of my historical strands which I will leave here with the failure of Euclidean geometry to be a paradigm of truth, and with the foundation of the new developing analysis moving towards a basis in arithmetic and set theory.

---

8. Boyer [1968], p.658.

9. The great mathematician Gauss also claimed to have done some of the work on non-Euclidean geometry, but never published any of it.

10. It could in fact be possible to decide which of several competing geometries was true by (e.g.) measuring the angle sum of triangles; unfortunately, and rather importantly, a negative result proves nothing. Even if triangles of intergalactic proportions were chosen, a negative result could still mean that a genuine deviation from the angle sum of 180°, which characterises euclidean geometry, was lost in the experimental error.

11. An ironic point of historical interest here is that it was, as we have seen, the crisis in arithmetic, brought about by both the discovery of incommensurability by the Pythagoreans, and also the problems highlighted by Zeno's paradoxes, which forced greek mathematics away from an arithmetical base and towards a geometrical one.

II Second strand.

We need now to look at the second, and rather more philosophical strand. Euclid's *Elements* was considered for many years to be a body of a priori truths describing the structure of the world, or universe – thus, in Kant's terminology, geometry was a body of a priori *synthetic* truths[12]. With the rationalist philosophers there was no problem with the idea of such a priori synthetic truths[13]; DesCartes, for example, famously built up a body of "facts" about the whole world (including religion) from pure thought in the *Meditations*. For the empiricists, however, all synthetic statements had to be empirically based, and thus a posteriori. Kant stands in between the rationalists and the empiricists, and his views on geometry and arithmetic, although they are a clear digression in this narrative, are worth noting here now. For Kant, the inaccessible noumenal world cannot be perceived, only the phenomenal world, the world of appearances, can, but this perception must necessarily be through the forms of perception – space and time. Space and time are necessary a priori particulars that we bring to the world (we do not abstract them from it) and mathematical judgements are those made about the structure of space and time. Geometry represents statements about space, and arithmetic represents statements about time; because space and time are a priori and mathematics gives us synthetic truths about space and time, then mathematics furnishes us with synthetic a priori truths.

For the empiricist, however, there is a problem with Euclidean geometry, for he is faced with a choice between accepting: (i) that the body of geometric facts is not to be considered synthetic but rather analytic; (ii) that the body of geometrical facts must be considered to be synthetic but a posteriori, or contingent in some way; or (iii) that geometry must be a special case of the a priori synthetic which is to be allowed. If he chooses (i), then the empiricist would be challenged to account for how it is that Euclidean geometry in particular, and mathematics in general, gives us

---

12. We need to note that this view is a very powerful one: statements of geometry certainly appear to be a priori since they are proved, without any obvious empirical input, from the basic propositions etc.; but these statements also appear to be obviously synthetic – take, for example Pythagoras' Theorem, and try to argue that the sense or meaning of $a^2 + b^2$ is analytically included in c^2, when a, b, and c are sides of a right angled triangle.

13. Note carefully however that Leibniz held the statements of mathematics to be analytic, not synthetic.

such surprising and genuinely "new" facts which seem to be true of the world (in as much as they seem to be generally applicable and useful in Science); in short, how to account for the huge gap between mathematics and logic. If he chooses (ii) then the empiricist has to account for the apparently a priori nature of Euclidean geometry in particular, and mathematics in general; and if he chooses (iii) then the empiricist has to live with the embarrassment of a special case.

Now if we recall the famous words of Hume:

> If we take in our hand any volume; of divinity or school metaphysics, for instance; let us ask, Does it contain any abstract reasoning concerning quantity or number? No. Does it contain any experimental reasoning concerning matter of fact and existence? No. Commit it then to the flames: for it can contain nothing but sophistry and illusion.

we can see that he clearly separated the empirical work from the mathematical, indicating that he considered mathematical knowledge to be different from empirical knowledge, but also there is an implication that mathematics does have something useful to say. Hume is in a sense then stuck with (i), and thus he owes us an account of the usefulness of mathematics. Mill on the other hand tackles the problem head on, and although his influence is slight[14], he puts forward a thoroughgoing empiricist view of arithmetic, choosing option (ii); Mill then owes us an account of the apparently a priori nature of mathematics. Empiricists in general would have chosen, with Hume, path (i), but as time went on the gulf between the "genuine" a priori analytic truths of logic and the truths of mathematics must have become increasingly clear[15], and so the problem of mathematics as a special case – choice (iii) – would have started to become apparent.

---

14. Mill's influence was slight at the time, but more recently the suggestion that mathematics is in some sense contingent has had fair currency; for example Quine, as we shall see later, for quite different reasons to do with his desire to blur the analytic/synthetic distinction, holds a view which implies that mathematics is part of the web of our (contingent) empirical scientific beliefs. Also Philip Kitcher develops an account of Mill arithmetic in his [1984].

15. It is just the very applicability of Maths in Science – leading to genuine (synthetic) knowledge – that makes maths different from logic; and it is of course just this area that Hartry Field tackles in his [1980], but which we will not deal with until later.

This then is my second historical strand, which ends with the suspicion for the empiricist that mathematical knowledge is, in some sense, a special case of a priori (possibly synthetic) truth.

III The early 20th century.

At the end of the nineteenth century, the collapse of geometry as a paradigm had caused a new foundational movement (Dedekind and Weierstrass) that was trying to establish geometry and calculus in arithmetic, requiring for this effort set theory, including infinite sets, to achieve its aims. Also the ambiguous position of mathematical knowledge not really fitting very comfortably into the empiricist pattern of a priori analytic or a posteriori synthetic, was in a sense responsible for another foundational trend – a programme that would somehow remove the "anomaly" of mathematics within the empiricists' epistemological scheme.

Frege in Germany, and Russell and Whitehead in England, embarked upon the logicist programme – the attempt to found mathematics on logic alone. The central concept that was to be employed in this programme was again that of the *set* – an idea first introduced by Cantor in his *Mengenlehre*. This concept was seen as being so simple and so self-evident that its use as an adjunct to logic was not considered to alter the fundamental thrust of the task; but it was just this concept that proved to be the ultimate downfall of the logicist programme, for it is the concept of set that is at the heart of the famous antinomy known as Russell's Paradox.

Thus by the beginning of the twentieth century problems with long historical roots had started to cause attempted solutions employing the concepts of set and infinite set; these concepts were at the heart of the issues of the foundations and philosophy of mathematics, and it is the various attitudes towards these concepts that colour and characterize the three schools of thought, concerning the nature of mathematics, that held sway in the first half of this century; it is with the notion of an infinite set that the schools divide: for the intuitionists the actual infinite was an unacceptable idea; for the formalists the notion of a completed infinity would be acceptable, so long as its use could be defended by strictly finitist argument; and for the logicists the axiom of infinity was a cornerstone. Clearly it is worth briefly discussing these three basic positions.

Logicism, propounded most completely by Russell and Whitehead in the *Principia Mathematica,* held that mathematics was reducible to logic; in this attempt to reduce mathematics to logic they hoped to ensure that the foundations of mathematics were then completely self-evident – as self-evident as the axioms of logic and the notion of set[16]. The logicists can also be seen as taking on Kant directly, attempting to deny that there was any extra-logical subject matter in mathematics.

> Logicists hoped to show, as against Kant, that mathematics did not have any "subject matter", but dealt with pure relations among concepts, and that these relations were "analytic", that is, of the same character as the principle of noncontradiction, or the rule of *modus ponens.*[17]

The necessary link that was used to bridge the gap between logic and mathematics, as mentioned above, was that of the set; to define the integers and ensure that there were enough of them, Russell needed the axiom of infinity:

> . . . to put the matter very crudely, if there are only a finite number of propositional functions, only a finite number of integers will exist. Thus an 'axiom of infinity' is required, postulating the existence of infinitely many propositional functions.[18]

With these two steps however – the use of sets and the introduction of the axiom of infinity – Russell caused his system to have some serious problems. Firstly, the notion of set was seen to be the root problem of Russell's paradox; to avoid this uncomfortable antinomy, the theory of types was eventually born, but unfortunately this extra theoretical baggage now meant that the notion of mathematics being derivable from "self-evident" foundations was compromised, for the theory of types is anything but self-evident. Secondly, there was opposition to the axiom of infinity for it was also not clearly self-evident, and the other two schools of thought differed from logicism in their respective attitudes to the use of non-finite methods.

---

16. This is, as we shall see shortly, exactly where they failed.

17. Benacerraf and Putnam [1983], p.11.

18. Black [1933], p.112.

Logicism is not Platonism, in the sense that logicism alone – defined as the reduction of mathematics to logic – is not Platonism; but logicism implies Platonism in the sense that the existence claims of mathematics are accepted by logicists (remember that Russell's programme is descriptive rather than normative). The early Russell seems to have held a sort of set-theoretic Platonism[19]; the Russellian logicists *defined* the numbers as various sets – e.g. zero is the class of all empty classes – so they were taken as saying that such and such a number *is* such and such a set. (That numbers can be assigned to sets in more than one way did not matter for the logicists, but it did open the door for Benacerraf's famous critique of Platonism, in his [1965], which will be met in the next chapter.) We may note also that this type of Platonism is still very much implicit in mathematics books today – most texts on analysis, for example, will start with a chapter on sets and functions defining numbers as sets in a fairly straightforwardly Russellian way.

The logicists did not all approach the existence problem in the same way; Frege's Platonism is quite different from that of the early Russell. Frege's logicist view was that mathematics is about pure (analytic) relations between concepts; Frege's concepts are a little strange – they are not objects themselves, but they do have "extensions" which are objects, and these extensions are the numbers. So here is another place where Platonism creeps into the logicist account.

What did logicism achieve? It set out to derive all mathematics from logic, plus some other self-evident notions such as set. We have seen that this programme founders because to achieve such a derivation, non-self evident concepts were needed – the theory of types to remove the antinomies, and the axiom of infinity to ensure that enough of "accepted mathematics" was derivable. (Note that the logicists did not aim to be prescriptive about how mathematics ought to be, rather they set out to produce foundations for what mathematics actually was; in other words this was a descriptive project.) But even if logicism failed in its principal aims, it did succeed in closing the embarrassingly wide gap between mathematics and logic which I claimed was the motivation for the programme in the first place.

> . . . if today it seems somewhat arbitrary just where one draws the line between logic and mathematics, this is itself a victory for Frege, Russell,

---

19. c.f. Penelope Maddy's 'set-theoretic realism' in chapter 6.

and Whitehead: Before their work, the gulf between the two subjects seemed absolute.[20]

In contrast to the logicist claim that mathematics contained no extra-logical subject matter, the formalists claimed that mathematics did have such a subject matter – but merely the signs etc. in which it was written. For the formalist this was all – it did not need any interpretations, in the logical sense of interpretations as models in which the statements could be true or false. In other words, mathematical formalism is the claim that mathematics is not *true* of anything. Also in contrast to the logicists, the axiom of infinity was a problem: whereas for Russell the *possibility* of the truth of the axiom of infinity was self-evident, Hilbert (the father of the formalists) demanded a proof of the consistency of that axiom before its use could be sanctioned; moreover that proof must be entirely by finitist[21] methods. So Hilbert's programme[22] was to find a finitist proof that infinitistic methods could form a consistent whole; he wanted to found mathematics securely by reducing it to axioms (i.e. signs etc.) and transformation rules for those axioms (i.e. ways of manipulating those signs). Unfortunately Gödel's incompleteness theorem effectively showed that this could not be done.

Gödel's famous paper[23] from 1931 put forward two important theorems which in effect limit the possibilities of what can be proved within any logical system: these are called Gödel's first and second incompleteness theorems, and revolve around the possibility of writing within a system of arithmetic, using a special coding called Gödel numbering, a self-referential sentence – the 'Gödel sentence' – which says (very roughly) 'I am not provable'. The paper splits into four sections, the

---

20. Benacerraf and Putnam [1983], p.13.

21. For the proof to be 'finitist', it must not use or refer to in any (even possibly disguised) way, the axiom of infinity. As Benacerraf and Putnam point out (in [1983], p.7): 'Hilbert did not think it very likely that the system of [*Principia Mathematica*] was, in fact, inconsistent; he simply felt that to take its consistency . . . without proof, was to adopt too low a standard of mathematical exactness . . .'

22. The question as to whether or not the axioms of arithmetic were consistent, was first posed by Hilbert in his address to the Paris Congress of 1900; in this address he posed twenty-three questions which he felt would be the important ones in the mathematics of the new century. Hilbert announced his ideas on mathematical formalism in the 3rd International Congress on Mathematics, in Heidelberg, 1904.

23. Gödel, K. [1931], 'Über formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme I', *Monatchefte für Mathematik und Physik*, Vol.38 (1931), p.173-198.

first of which is fundamentally philosophical, and the last three mathematical. In the first section, Gödel explains in a general, non-mathematical way, that the Gödel sentence is not provable within the system in which it is written; he then shows metamathematically that it must in fact be true. Thus there are two routes by which Gödel's paper leads to Platonism: firstly by the destruction of Hilbert's programme (and the assumption that we do not turn to intuitionism – see below); and secondly by this metamathematical argument from section 1 of the paper[24].

A completely different, and much more strongly normative account of mathematics was produced by Brouwer and the intuitionist school. Brouwer attacked the logical foundationism of both the logicists and the formalists. His view was idealist in flavour, for he claimed that mathematics had its roots in intuition, and made its concepts immediately clear to us through our intuition[25]. Any statement for Brouwer could only be considered true if it could be shown, or constructed, in a finite number of steps. Truth is thus linked to some notion of provability, and falsehood is linked to provability of the denial. Thus intuitionism famously denies the law of excluded middle – if we cannot prove a statement, nor prove its denial, then it is neither true nor false. Although many have found this aspect of the intuitionist position uncomfortable, the largest problem with it as an account of mathematics is that, since infinitist and non-constructive proofs are not allowed, a large amount of modern analysis is left out as unacceptable. Here the prescriptive contrast with Russell's descriptive approach is most clear: for Russell, Brouwer's epistemology must be at fault since it leaves out large amounts of important and good mathematics; for Brouwer, Russell's mathematics cannot be good since it does not measure up to his exacting epistemological standards.

Wittgenstein is sometimes linked both to Russell's logicist programme, and also to the intuitionism of Brouwer, but in fact he really occupies an entirely unique position in the 20th century debate on mathematics. He had no interest in Russell's ambitions to found mathematics on logic, nor was he particularly sympathetic towards Brouwer's Kantian approach to mathematical intuition. It is true that in the *Tractatus* he set out a theory of propositions – distinguishing between

---

24. These comments were motivated by a lecture given by Ray Monk, at Rewley House in Oxford on 15th October 1995.

25. c.f. Gödel's views on the perception of mathematical objects in the chapter 4.

logical propositions (tautologies) and mathematical propositions ('equations') – which Ramsey later used in an attempt to rescue the logicist programme by trying to show that all 'equations' are tautologies. But Wittgenstein strongly disagreed with Ramsey's approach, in which a central pillar was the introduction of

> . . . a Definition of Identity which, using a specially defined logical function Q(x,y) as a substitute for the expression x=y, tries, in effect, to assert that x=y is either a tautology (if x and y have the same value) or a contradiction (if x and y have different values).[26]

His reply to Ramsey's paper shows that he felt the whole attempt to found mathematics in this logicist way was just an incorrect approach.

> The way out of all these troubles is to see that neither "Q(x,y)", although it is a very interesting function, nor any propositional function whatever, can be substituted for "x=y".[27]

Indeed Wittgenstein ultimately rejected the need at all for foundations in mathematics: each foundational attempt – logicism, formalism, intuitionism – is for him merely more mathematics *itself*. Wittgenstein might however have been sympathetic towards Brouwer's attack on the notion of the actual infinite, for he felt that such a picture was a symptom that something had gone wrong:

> If you want the right image for aleph-0, you mustn't form it from mathematics. If you say 'How terrific!', if your head reels – you can be sure it is the wrong image. It is not terrific at all.[28]

Ultimately it was the "charm" of mathematics with its attendant metaphysical baggage that Wittgenstein wanted to attack, and to replace it with a view in which mathematics is seen just as "techniques". Thus Wittgenstein can be seen as taking a divergent path from the rest of those

---

26. Monk [1990], p.245.

27. A letter from Wittgenstein to Frank Ramsey, dated 2.vii.27, and quoted from Monk [1990], p.246.

28. Diamond [1989], p.253.

who were interested in philosophy of mathematics, and so he will not feature very much in the current project.

I have tried to show above that what led to logicism was the problem of how to account for mathematical knowledge. The logicists, strapped into their empiricist straight-jackets, were faced with the problem of mathematics being a "special case" of *a priori*, possibly synthetic knowledge, needed to put it firmly in its place by proving that maths could be founded entirely in logic. They wanted to provide foundations for the mathematics that would allow an adequate description of the then current mathematical knowledge; as a result, they placed existing mathematics above epistemology in terms of priority – any epistemology would have to be able to deal with mathematics as it is for it to be acceptable. The other two schools were more prescriptive in flavour, and felt that epistemological principles were prior to the mathematical; in these two schools epistemology becomes a critical tool for mathematics.

As we have seen, none of these three schools of philosophy of mathematics really succeeded in achieving their goals: the logicists did not really manage to reduce mathematics to a set of self evident logical principles; the formalist programme of Hilbert foundered on Gödel's incompleteness theorem; and the intuitionists threw away a large and important part of classical mathematics[29]. Yet the wake of these three approaches to the problems associated with how to account for our mathematical knowledge (which stick with us now) still wash over aspects of current work; and it is these problems of mathematical knowledge, along with the related problems of mathematical truth and mathematical existence, which are the subject of the current work.

---

29. Indeed – as Wittgenstein would have pointed out – they really only started a new branch of mathematics – the mathematics that can be derived from only finitist arguments.

3. PRELIMINARIES 2: PHILOSOPHICAL BACKGROUND.

This thesis is concerned with the debate on the problem of how we can account for our mathematical knowledge, and this problem is deeply interlinked with other problems of mathematical truth and mathematical existence.

> If it is correct to suppose that we have some mathematical knowledge then some mathematical statements must be true. So, for example, if almost everyone knows that 2+2 = 4 and if the *cognoscenti* know that $\int_{-\infty}^{+\infty} e^{-x^2}\,dx = \sqrt{\pi}$, then the statements "2+2 = 4" and "$\int_{-\infty}^{+\infty} e^{-x^2}\,dx = \sqrt{\pi}$" are both true. Yet to advance that conclusion is immediately to raise the question of what makes those statements true.[30]

It is my contention that these problems are indeed inextricably linked, and that by pursuing one, we will necessarily pursue all three. My course then will be initially to pursue one in particular – the problem of mathematical existence – and thus to approach the nexus of problems from that particular direction. There will obviously be some loss of generality by approaching from this direction, but the same criticism is surely as applicable – *mutatis mutandis* – to approaches from either of the other directions, so I have no misgivings about this particular tack.

I Realism and anti-realism.

Firstly the idea of mathematical existence: I want to talk about the "objects of mathematics" and must then define what I mean; but if the definition is too prescriptive, then surely the question as to the nature and existence of such objects will be begged[31]. I do not want the phrase "the objects of mathematics" to refer to those things such as signs and symbols

---

30. Kitcher [1984], p.101.

31. I cannot, for example and rather trivially, define the objects of mathematics to be existing objects, for then there would be no debate – this is one of the problems with the ontological argument!

that are the only "objects" for the mathematical formalists (who, so we have seen, consider mathematics to be about nothing at all); rather I intend that phrase to refer (if referring does not itself beg the existential question) to those (possibly transcendental) objects of mathematics such as the numbers, functions, sets, bounded linear functionals, etc. with which the average mathematician (the mathematician on the Clapham omnibus) considers himself to be dealing every day. Bearing in mind the hope that we are not begging the question as to the nature of these objects, then the phrase "the objects of mathematics" will not mean the signs and symbols used in mathematics, but rather the numbers, functions, and sets etc.[32]

Secondly, we need some idea of what we mean by realism and anti-realism before turning to specific problems in the debate about mathematical objects. The realist/anti-realist debate in general is one of the most all-pervading in philosophy, and also one that has traditionally been able to cut across other "party lines". For example, Berkeley, unlike most other empiricist philosophers, was an idealist; DesCartes on the other hand, by virtue of his evident dualism, displays a clear realist standpoint on at least some issues that is not shared by some other rationalist philosophers like Leibniz and Spinoza, who can both be seen as idealists. Crispin Wright, in the introduction to his [1993] places the realist/anti-realist debate at the very centre of philosophy:

> If anything is distinctive of philosophical enquiry, it is the attempt to understand the relation between human thought and the world. The project is constitutive of metaphysics. While undergoing shifts of interpretation, it nevertheless supplies a dominant motif in the writings of all the great philosophers of the past. Realism simply supplies by far the most natural, pre-philosophically agreeable conclusion which this project could have. If our successors come to reject not the details but the very issue of the contemporary debate concerning realism, it will be because they have rejected philosophy itself.[33]

---

32. I am tempted to call the objects of mathematics the transcendental objects of mathematics, but it is clear that here there would be a problem of question begging; this problem can be overcome however with careful definition, but it is hoped that we have done enough here to draw the distinction between the objects of the formalist and the "proper" objects of mathematics. If we look forward to Penelope Maddy's work – in her [1990] – then there is a similar difficulty with describing sets and numbers as transcendental within her system, for she will want sets in particular to be perceptible, whereas she defines transcendental objects as 'outside the causal nexus'; for Maddy we will need to be careful about describing sets as mathematical objects in the same way as numbers.

33. Wright [1993], p.1; but note that the point of attack here is in fact Rorty.

It will be instructive then, before turning our attention to specific mathematical realism or anti-realism, to look briefly at some of the points in that modern realist/anti-realist debate. We may be struck first by the wide range of positions that may broadly be classified as realist; for example, mathematical realism (in several flavours: ontological Platonism, epistemological Platonism[34], physicalism, etc.) scientific realism, Cartesian mental realism, causal realism, moral objectivism, . . . the list goes on; it may be as well then to restrict ourselves broadly to realism about the perceived world – that is realism about the physical world – and not to worry overmuch about such things as moral objectivism.

It would be reasonable to suggest that realism is the usual pre-philosophical, or naive, position, and so we can start with this. What, if anything, could be said to characterise those positions that we would call realist? For Dummett the crucial thesis is that of bivalence – that a statement is determinately either true or false (irrespective of whether we can discover this) so long as it is not too vague[35]. For example, in the preface to his [1978], Dummett (writing about the distinction between Platonism and intuitionism in mathematics, but generalising to any statements) states:

> To establish the correctness of a realistic interpretation of any range of statements, it could not be enough to observe that those statements were about objects not of our making whose properties it was for us to discover rather than to decide. This could not be enough, because the assumption that mathematical objects exist independently of our thinking of them was not enough to guarantee the correctness of a platonistic view of mathematical statements, while no intuitionist maintained that we are free to allot to such statements what truth-values we please. Rather, to justify a realistic interpretation of statements of any kind, it was necessary to demonstrate that we had conferred on those statements meanings such as to yield a notion of truth, as applied to them, with respect to which each

---

34. The distinction between these will be drawn below.

35. There is a large problem however with vagueness, in as much as there are many concepts in language – for example Dummett's own of "hill" in his [1978] p.182 – which are just ineliminably vague; so a question can be raised as to whether or not we can take a realist standpoint with respect to "vague objects".

> statement could be taken to be determinately either true or false, that is, under which the principle of bivalence held good.[36]

Dummett thus involves the notions of truth and meaning in the debate. Wright on the other hand suggests that:

> Realism is a mixture of modesty and presumption. It modestly allows that humankind confronts an objective world, something almost entirely not of our making, possessing a host of occasional features which may pass altogether unnoticed by human consciousness and whose innermost nomological secrets may remain forever hidden from us. However, it presumes that we are, by and large and in favourable circumstances, capable of acquiring knowledge of the world and of understanding it.[37]

Furthermore, just as Dummett involves truth and meaning into the debate, so Wright suggests that we need to consider three types of objectivity if we are to become involved in this debate; these are objectivity of truth, objectivity of meaning, and objectivity of judgement. The objectivity of truth is, for Wright, not only the notion that truth is in some sense 'not of our making', but also the notion that truth may, in fact and in some circumstances, 'defy our powers of rational appraisal'[38]. The objectivity of meaning is a constraint on the meaning of a statement; statements must have some objective meaning which will be a necessary standard against which the objective truth of the statement may be measured[39]. The objectivity of judgements is that objectivity that statements have when they deal with real, factual (contingent) subject matter.

Wright goes on to characterise two general types of anti-realist position – the sceptical and the idealist:

---

36. From Dummett [1978], p.xxix.

37. Wright [1993], p.1.

38. I suppose then that traditional Intuitionists would not subscribe to the objectivity of truth, for they would not want a truth value in cases where our rational appraisal falls short of the task.

39. Here we can see a sharp contrast with all modern deconstructional philosophies, such as Jacques Derrida's.

> Scepticism represents a slide towards the pole of modesty. The sceptic agrees with the realist that our investigative efforts confront an autonomous world, that there are truths not of our making. But he disputes that there is ultimately any adequate warrant for regarding our routine investigative practices as apt to issue in knowledge of, or reasonable belief about the world. . . . The distinguishing mark of the idealist, by contrast, is a more thoroughgoing presumption. In his view realism is founded on a misunderstanding of the nature of truth. . . . No truth is altogether 'not of our making'. Rather reality is – on one version – a reification of our own conceptual and cognitive nature, with no more claim to autonomy than a mirror image.[40]

So for Wright, the broad picture is this:

Realism
MODESTY
PRESUMPTION
Sceptical anti-realism   Idealist anti-realism

(We might like to note, as it will be important later when we consider Field's radical views on mathematics in chapter 5, that traditional nominalism is a sceptical form of anti-realism, for the nominalist disputes whether we have any – or at least any adequate – warrant to believe that we can obtain knowledge about any putative external reality. This is certainly a more conservative or cautious view than a radical idealist one.)

II Truth

As noted above, the problem of truth is already deeply embedded in the realist/anti-realist debate, and it is as well to recall some of the possibilities for theories of truth. Theories of truth include disquotational theories, provability theories, coherence theories, and so forth, but for many philosophers writing about mathematics, some sort of referential account of truth is often given as the "best" theory, or at least the one that is accepted for the sake of the discussion. Referential theories include the correspondence theory, which requires some sort of non-trivial reference

40. Wright [1993], p.2.

between the referring expressions in a true statement, and the fact to which it corresponds and which makes it true. A most popular referential theory is Tarski's, which is specifically accepted as the only acceptable one by both Benacerraf and Field. Benacerraf claims:

> I take it that we have only one such account: Tarski's, and that its essential feature is to define truth in terms of reference . . .[41]

And Field suggests that:

> . . . the only known account of mathematical truth not subject to obvious difficulties is the same account of truth that works outside mathematics. (Tarski's account essentially.)[42]

Now, even if we are to agree that *some* sort of referential account of truth is what we need in our semantics, then there are still some questions over whether specifically Tarski's theory is the one we want. For as W.W. Tait has pointed out in his [1986], Tarski's theory is essentially a mathematical theory:

> It is difficult to understand how Tarski's 'account' of truth can have any significant bearing on any issue in the philosophy of mathematics. For it consists of a definition *in* mathematics of the concept of truth for a model in a formal language . . . But Benacerraf is concerned with mathematical truth, not with truth of a formal sentence in a model. How can Tarski's account apply here?[43]

Tarski's theory is concerned with the truth of a formal sentence in a model and, thinks Tait, this is just not appropriate to any useful conception of mathematical truth. The key idea in Tait's critique is that of interpretation. He summarizes Tarski's account of truth thus:

> . . . with each formula ø of L, we define by induction on ø a formula I(ø) (in the same variables) of the metalanguage, that is, of some part of the

---

41. Benacerraf, [1973], p.408.
42. Field [1989], p.53.
43. Tait [1986], p.148.

> ordinary language of mathematics in which we defined the model. The truth definition is now just the 'material condition for truth': a sentence ø of L is true iff I(ø).[44]

Now the anti-Platonists can say that in Platonist mathematics, the interpretation of the mathematical object language is via a model which is an abstract, non-causal realm. Thus Tait suggests that these anti-Platonists[45], by employing Tarski's theory of truth, are ultimately presupposing this ('model-in-the-sky') picture of Platonism. The use of Tarski's theory implies that mathematics has two layers:

> . . . the layer of ordinary mathematical practice in which we prove propositions such as (1) ['There is a prime number greater than 10'] and the layer of the model at which (1) asserts the 'real existence' of a number.[46]

The suggestion is then, that if we adopt a different theory of truth here (the notion of truth for Tait is not univocal) then we can have a different Platonism which is more robust when it comes to the Benacerrafian challenge C2 below. (To be fair to Field here, he does in fact adapt Tarski's theory in his [1972][47], removing some of the drawbacks to which Tait's criticisms apply.)

Nevertheless, it turns out that referential accounts of truth seem to hold sway in the literature. In any case, it is worth looking at the range of (supposedly) true sentences that may occur in mathematics. I offer the following small selection of examples to illustrate just some aspects of that range:

S1. $1.999... = 2$

S2. $2 + 3 = 5$

---

44. Tait [1986], p.149.

45. His point of attack here is in fact both Dummett and Benacerraf.

46. Tait [1986], p.150. Tait reflects that this distinction lies behind Chihara's distinction between 'mythological' and 'ontological' Platonism: 'The former simply does mathematics while refraining from commitment to the interpretation. The latter accepts the interpretation, and so is committed to the 'real' existence of a prime number greater than 10 . . .' (From Tait [1986], p.150. – this distinction appears again with Van Fraassen in chapter 5.)

47. Field, H. [1972], 'Tarski's Theory of Truth', *Journal of Philosophy* 69 (1972), 347-375. A good account of Field's adaption of Tarski is in Schmitt [1995], p.180-195.

S3. $\int_0^1 x \, dx = \frac{1}{2}$

S4. If ABC is a triangle with the angle at B a right angle, then $AC^2 = AB^2 + BC^2$.

Now, no matter what theory of truth we are using, it strikes me that if we wish to accept all of these, S1 – S4, as true, then we will be in fact quite hard pushed to do it at the same time as keeping the notion of truth univocal. For example, the truth of S1 seems to depend only on some convention for the use of the symbols; if we accept some referential account of truth, then the truth of S2 will imply the existence of the "entities" (in some sense) denoted by "2", "3", and "5"; the truth of S3 will imply the existence of more, higher level entities; and S4 is trivially true if there are no right angled triangles, and true otherwise (in some sense of the word "true"). Our philosophical position vis-a-vis Platonism or anti-Platonism might even be revealed if we try to describe such sentences as 'analytically true', 'synthetically true', or even 'contingently true'. Clearly S1 is analytically true, but what about S2, S3, and S4?

III Some important distinctions.

We need now some definitions made, and distinctions drawn, for this will help put the issues involved in this study in a clearer light.

D1. Epistemological versus metaphysical, or semantic realism. (Van Fraassen.)

Epistemological realism is the notion that our empirical evidence is enough (epistemological) warrant to infer the existence of unobserved or unobservable entities[48] in science. Metaphysical (or semantic) realism on the other hand is the belief that unobserved or unobservable entities do in fact exist. This seems like a hard distinction to draw, but Van Fraassen in his [1980] puts forward the suggestion that science only gives us theories that are empirically adequate, i.e. ones whose observable consequences are

48. We again run across the problem of reference by talking about "entities" which may not exist, and as above, I use the token "entities" hopefully without prejudice as to their existence or otherwise. A second point also: the distinction between unobserved and unobservable entities will be dealt with later in chapter 5.

true, rather than ones whose terms refer to existing but unobserved – or unobservable – entities. In other words Van Fraassen is claiming that scientific theory does not ever give us enough warrant to infer the existence of theoretical entities. This anti-realism in Wright's terminology is a sceptical form of anti-realism. Van Fraassen also claims however that other arguments (in his [1980] he puts forward some argument parallel to Aquinas' Five Ways) may give us enough warrant in fact to believe in the existence of theoretical entities; thus he is a metaphysical realist but an epistemological anti-realist[49].

D2. Epistemological versus ontological Platonism. (Steiner.)

In his [1973], Steiner distinguishes between 'the doctrine that we come to know facts about mathematical entities through a faculty akin to sense perception' – epistemological Platonism, and the doctrine that 'the truths of mathematics describe infinitely many real mathematical objects' – ontological Platonism. Two comments are due here: (i) This is not the same distinction as Van Fraassen's. Ontological Platonism differs from metaphysical realism in that it seems to require an *infinitude* of mathematical objects which are distinct from the physical world (at the very least by virtue of their infinitude). If the axioms of mathematics are true, then Steiner claims that this is equivalent to suggesting that ontological Platonism is (if not correct then at least) tenable. (ii) If, however, ontological Platonism is indeed true (thus entailing non-physical mathematical entities), and also if we want at the same time to hold onto a causal theory of knowledge, then epistemological Platonism has little chance of being correct. This distinction is crucial to the challenge C2 below.

D3. Transcendent versus immanent mathematical realism. (Irvine.)

Irvine, in the introduction to his (ed) [1990], distinguishes between transcendent mathematical realism (Platonism) and immanent mathematical realism (physicalism). The crucial thesis upon which these differ is the Platonists' contention that mathematical entities are non-physical, outside space and time and thus outside the causal nexus; immanent mathematical realism on the other hand requires that mathematical entities be 'naturalistically construed'.[50]

---

49. We will return to Van Fraassen's views in more detail in chapter 5, section VII.

50. As noted in footnote 32 above, this distinction is very sharp within Penelope Maddy's work, in that sets for her are immanent objects – her set-theoretic realism is a

D4. Intentional and material objects of perception. (Anscombe.)

In her [1965], G.E.M. Anscombe cuts a useful distinction between what she terms material objects on the one hand, and intentional objects on the other. We need to note firstly that she explicitly uses the word "object" in what she calls the old sense:

> That word "object" which comes in the phrase "object of sight" has suffered a certain reversal of meaning in the history of philosophy, and so has the connected word "subject", though the two reversals aren't historically connected. The subject used to be what the proposition, say, is about, the thing itself as it is in reality – unprocessed by being conceived, as we might say (in case there is some sort of processing there); objects on the other hand were formerly always objects *of* –. Objects of desire, objects of thought, are not objects in one common modern sense, not individual things, such as *the objects found in the accused man's pockets.*[51]

In the case of perception of some kind, or some activity that requires perception, Anscombe distinguishes the material object from the intentional.

> An intentional object is given by a word or phrase which gives a *description under which.*[52]

She also gives an example, which we will look at, to make this idea clear: 'It will help if we consider shooting at, aiming. A man aims at a stag'[53]. Suppose for a moment that this happens, and that the man shoots at, and kills a stag. Then in this case the given intentional object is the stag ('What did you aim at?' 'I aimed at the stag'); the material object is also in this case the stag. So much for my preamble; Anscombe is more interested in the case where the given intentional object is not really there – where it doesn't exist.

---

physicalist account of (perceptible) sets – whereas numbers, as properties of sets, may be transcendental. The details of her work are in chapter 6 below.

51. Anscombe [1965], p.158.

52. Anscombe [1965], p.166.

53. Anscombe [1965], p.166. The first part of this example which follows is, however, not in Anscombe; rather it is my preamble to her example proper which follows.

> A man aims at a stag; but the thing he took for a stag was his father, and he shoots his father. . . . Let us introduce the term "material object": "his father" gives, we shall say, the *material* object of the verb in the sentence "He aimed at his father" in the sense in which this was true.[54]

Now in this example the given intentional object was a stag, but because the stag in this instance is non-existent, then we need to find another intentional object – something which the man would accept as a description of the thing at which he was aiming – that will identify the material object. (But note that he would not accept that he was aiming at his father.)

> E.g. he was aiming at that dark patch against the foliage. The dark patch against the foliage was in fact his father's hat with his father's head in it.[55]

So the (given) intentional object is the stag; but this doesn't exist so we find another intentional object (i.e. an alternative description of the thing at which he was aiming, acceptable to him), i.e. the dark patch; and the dark patch is also a description of the material object, i.e. (part of) his father. (And this final description is one that he would not accept).

Now there need not even be a material object (of aiming). Anscombe goes on to say that if the man were hallucinating a stag, and just happened to hit his father, then this would not make his father the material object of his aim. (Note however that his father was the material object – but not the intentional object – of his aim when he did in fact aim at his father's head, which he took to be a stag.)

> While there must be an intentional object of seeing, there need not always be a material object. That is to say "X saw A" where "saw" is used materially, implies some proposition "X saw --" where "saw" is used intentionally; but the converse does not hold.[56]

We might be moved to wonder which use of "object" – the intentional or the material – is epistemologically prior. I would like to say

---

54. Anscombe [1965], p.166/7.

55. Anscombe [1965], p.167.

56. Anscombe [1965], p.176.

that this example suggests very strongly that the intentional use is in fact epistemologically prior to the material use, even though Anscombe denies that this need be the case.

Finally here, it is clearly the case that the material objects are real, physical objects, whereas the intentional objects are mind dependent. This distinction will be important to us later on.

IV Some challenges.

There have been some important landmarks in the recent work on the problems thrown up by mathematics, and some of these have posed important challenges to any account that is put forward; these challenges must be met by any adequate account of mathematics.

C1. Benacerraf.

The famous challenge to the Platonism inherent in mathematical textbooks, the Platonism that ultimately stems from Russell and Whitehead's *Principia*[57] i.e. that the fundamental objects of mathematics – in this case numbers – are in fact sets, is expressed by Benacerraf in his [1965]. Here Benacerraf shows firstly that, since there is both no unique way of identifying numbers with sets, and also no adequate way of deciding which of the given identifications (if any) is the correct one, then there is no question of there being a "correct" account (or identification of numbers as sets) at all. In the final part of his article, by extending the argument that numbers could not be sets, he goes on further to suggest that numbers could not be any type of object at all. He argues that a requirement for numbers to be objects, is that it would have to be possible to individuate them from the role that they play in the system of number relations – the structure.

> "Objects" do not do the job of numbers singly; the whole system performs the job or nothing does. I therefore argue, extending the argument that led to the conclusion that numbers could not be sets, that numbers could not be objects at all; for there is no more reason to identify any individual number with any

---

57. Even if we do not have to assume that the *Principia* implies Platonism, its broad acceptance in the (largely Platonist) mathematical community tends to the view as defining numbers as sets; almost every work on (mathematical) analysis starts with a chapter on sets and numbers which, in effect, defines numbers as sets.

one particular object than with any other (not already known to be a number).[58]

C2. Benacerraf, and Steiner.

Another major challenge to the philosopher working in this area, is that of trying to reconcile a "standard"[59] semantics with a plausible epistemology. Now Irvine in his [1990] has pointed out that, whilst broadly maintaining both a referential account of mathematical truth and a causal theory of knowledge, much of the recent philosophy of mathematics has been argued by holding on to one "reasonable" theory of knowledge at the expense of trying to keep the semantics for mathematics in line with the semantics for the rest of language – or vice versa. In his own words he sees the current debate as one of 'reconciling semantics with epistemology' as have, so he points out, both Benacerraf and Steiner. For Benacerraf:

> two quite distinct kinds of concerns have separately motivated accounts of mathematical truth: (1) the concern for having a homogeneous semantical theory in which semantics for the propositions of mathematics parallel the semantics for the rest of language, and (2) the concern that the account of mathematical truth mesh with a reasonable epistemology. . . . almost all accounts of the concept of mathematical truth can be identified with serving one or another of these masters *at the expense of the other*.[60]

And for Steiner:

> The objection is that, if mathematical entities really exist, they are unknowable – hence mathematical truths are unknowable. There cannot be a science treating of objects that make no causal impression on daily affairs. . . . Since numbers, *et al.* are outside all causal chains, outside time and space, they are inscrutable. Thus the mathematician faces a dilemma:

58. Benacerraf [1965], p.290/291.

59. In what follows it is generally considered to be a requirement – unless some good reason is supplied – for the semantics of our mathematical language to be the same (or approximately the same) as that of our scientific language. Obviously some philosophers would want to oppose this.

60. Benacerraf [1973], p.661; reprinted in Benacerraf & Putnam [1983], p.403; and quoted from Irvine [1990], p.x.

> either his axioms are not true (supposing mathematical entities not to exist), or they are unknowable[61].

A similar thought rather more snappily expressed[62]. Note however that in stating this problem in this way, both Benacerraf and Steiner are clearly accepting a causal theory of knowledge – quite explicitly in the latter's case[63], and Steiner is also clearly committing himself to a referential account of mathematical truth. (In all that follows it is possible to discern this thread – the tension between semantics and epistemology – particularly in those cases where a referential account of truth is taken.)

There are three possible ways out of this problem that I would like to consider (and one other perhaps which will be outside the scope of this thesis[64]); the first two are for the realist:

(i) to deny the causal theory of knowledge;

(ii) to accept the causal theory of knowledge, but to deny the transcendental nature of mathematical entities, i.e. to deny Platonism (transcendental mathematical realism) and to hold onto some form of immanent mathematical realism;

and the third is for the anti-realist:

(iii) to accept that mathematics is in fact not true and that its objects are (for example) fictions.

C3. Quine/Putnam Indispensability.

It is claimed that most statements taken *on their own* from some empirical (scientific) theory have no empirical content; they are however (a non-redundant) part of a larger scientific theory that has explanatory and predictive value, and should thus be counted as true (or probably

---

61. Steiner [1973], p.58, quoted from Irvine [1990], p.x.

62. It might be worth remarking here that for some, for Burgess for example, there already has to be a mistake somewhere here, for what he would want to say is that it is just brute that (i) mathematics is true, and (ii) we do in fact know the (known) facts of mathematics.

63. Steiner also then uses the defence of denying the causal theory of knowledge in his [1973] to enable him to hold on to a Platonist position. Steiner in fact wants to escape the dilemma, and he chooses a path of denying the causal theory of knowledge to enable him to hold onto some form of Platonism.

64. Irvine ([1990], introduction p.xxi) suggests that an alternative route out of the problem is 'to alter in essential respects the terms of the above debate, for example either by introducing the kind of modal-structuralism advocated . . . by Hellman or by defending, as does Gauthier, the kind of constructivist views which may quite naturally (at least in certain respects) appeal to the physicalist.'

true?) if that theory is an acceptable one[65]. Now, it is clear that mathematical statements are non-redundant statements forming part of modern (accepted and acceptable) scientific, particularly physical, theory, and thus they must be taken as true even though they have no empirical content taken on their own. If true then, at least on a Tarskian theory of truth, the referring expressions in existential statements must have objects to which they refer. Crudely then, this is the Quine/Putnam indispensability argument – mathematics is an indispensable part of our best theory of the world; taking a naturalistic standpoint, our best theory is the one that we take to be true, thus mathematics must be true and its referring expressions must refer to mathematical entities[66].

C4. Kitcher and Field.

Both Kitcher and Field highlight the problem of how to account for the utility of mathematics as a major challenge. Field is interested in how to accomplish this from a nominalistic standpoint, but Kitcher makes the challenge far broader; it is, he claims, a challenge not only for the nominalist, but also for the Platonist. The nominalist must account for the utility of mathematics which is, in the extreme case of Field, not even to be considered as true. Against this the Platonist's position seems more suitable given that mathematics under this theory is at least considered to be true, and so this truth should account for the utility of mathematics; but the picture is not really any better, for Kitcher asks the question as to why the Platonist's knowledge of an *abstract* reality should help account for the usefulness of that knowledge in scientific questions about the nature of *physical* reality.

It seems then that the debate over the existence or otherwise of mathematical objects will dominate our standpoints on several key issues. We can trace out those issues by considering the following four problems which Hartry Field, in the introduction to his [1980], suggests are the three central questions that are usually tackled in contemporary works on the

---

65. What might count as an acceptable scientific theory here would no doubt depend on one's point of view, but it is enough that we can take a (for example) Popperian stance on the "best" scientific theory amongst competing theories.

66. The Quinian argument will be dealt with in more detail itself in chapter 6 (on Maddy).

philosophy of mathematics, plus the fourth which is the one that he is most interested in:

1. How much of standard mathematics is true?
2. What entities do we have to postulate to account for the truth of (this part of) mathematics?
3. What sort of account can we give of our knowledge of these truths?
4. What sort of account is possible of how mathematics is applied to the physical world?

4. THE CURRENT DEBATE: A BRIEF SURVEY.

Having settled the basic framework for the debate in the last chapter, we can now turn to look at the philosophical positions some of the current players in this debate with respect to their attitudes towards mathematical realism. This will be set out along the lines of the pattern suggested by Blackburn[67] for investigations into any debate between realists and anti-realists. The specific area of commitments in which we are interested here, is the discourse that gives rise to existence claims about (transcendental) mathematical objects; for example the existential theorems in mathematics such as 'there is a prime number greater then 1000', and so on. The diagram will be:

Area of commitments - Mathematics.

Accept.	*Reject:* Brouwer.
On its own terms.	*Reduce it:* Chihara, Kitcher, (Field?), Weir.
With genuine truth conditions.	*Offerings with some other point:* Field, Papineau.
Mind independent: Gödel, Hunter, Tait, Quine, Maddy, Lowe, Bigelow, Resnik & Shapiro.	*Mind dependent:* Kant, Armstrong.

In the discussion below, I have described each of these broad positions (working down the right hand side of the diagram) as rejectionist, reductionist, instrumentalist, mentalist, and finally realist.

---

67. See Blackburn [1984], p.145 ff, and especially the diagram, figure 4 on p.147.

I Rejectionists.

I start with the suggestion that no one (clearly) wants to reject outright the propositions of mathematics, except perhaps the Intuitionists, whose radical views (as discussed above in Chapter 2) entail a rejection of a fair amount of current mathematics. That we may take their work as being tantamount to the creation of a "new" mathematics[68], we can see them as, in this case, rejecting mathematical discourse. We can see also, that in terms of Wright's criterion of the objectivity of truth, as already mentioned in a footnote, they can be considered as anti-realists – idealists sliding towards the pole of presumption; in a similar way, their rejection of the principle of bivalence marks them out as anti-realists in Dummett's terms.

II Reductionists.

An alternative to rejection as a means of escaping the problems of mathematical existence, is to attempt to reduce the discourse of mathematics into different terms – terms which seem less problematic and in which the problems do not perhaps arise. Several contemporary approaches might be considered to be doing this.

(i) Chihara's constructibility.

In his [1990], Chihara sets out the detailed axiomatic structure of a system that, in principle, would rewrite the existential statements of mathematics in terms of statements about what could in principle be constructed[69].

> The basic idea . . . is to develop a mathematical system in which the existential theorems of traditional mathematics have been replaced by constructibility theorems: where, in traditional mathematics, it is asserted

---

68. Not to be confused with the 'New Math' of the 1960's and Tom Lehrer fame.

69. The inspiration for this seems to be Euclid (again!): 'It may also be mentioned, in defence of using the constructibility quantifiers in mathematics, that geometry was practiced for over two thousand years within the framework of Euclid's system – a system that . . . is constructive in nature.' From Chihara [1990], p.52.

> that such and such exists, in this system it will be asserted that such and such can be constructed.[70]

His approach is an attempt to cut a path between intuitionism and realism. By using special constructibility quantifiers to replace the current existential quantifiers in mathematics, he aims to provide a stronger set of constructibility theorems (replacing the traditional existential theorems of the traditional mathematician) than the intuitionists, and thus he hopes to avoid the problem that beset them, of creating a new, different (and smaller) mathematics. (In the terms of the current chapter, he hopes to avoid the actual rejection of mathematics as it stands.) But on the other hand he does not wish to replace the totality of mathematics with constructibility theorems (why not? why should he limit his project?) – rather his target is the mathematics of the Quine/Putnam indispensability argument: the mathematics applicable to science.

> The constructibility theory is not a theory about how to analyse actual mathematics. I have not been claiming that the existential quantifier in ordinary mathematics should be treated as constructibility quantifier. However, I have been arguing that the kind of mathematics studied in this work could provide an adequate mathematical framework for science. In other words, I have in effect been providing a response to what I have called Putnam's neo-Quinian argument – a response that implies that we do not have to believe in the existence of mathematical objects simply because present-day science seems to involve reference to such objects in the way it does.[71]

Despite these protestations from Chihara, it is my contention that, within the Blackburn format which we are employing here, his avoidance of realism is one which is fundamentally reductionist.

There are two clear concern's with Chihara's constructibility theory. Firstly the limited scope of the project (the point of his attack being only the mathematicas applicable to, or indispensible for, science) strikes one as being either ad hoc, or suggestive that the project might not succeed across the whole spectrum of mathematical endeavour. Secondly, Chihara's system as developed is one of open-sentences, along the lines of Frege's he

---

70. Chihara [1990], p.25.

71. Chihara [1990], p.174.

claims, but without the ontological baggage and the paradoxes. His motivation for the development of this system is to avoid the ontological problems that beset realist accounts, but as James has pointed out in his review ([1992]) of Chihara, he does not necessarily himself avoid ontological problems with his (possibly) epistemologically unclear open-sentence tokens. It is therefore open to doubt on at least two counts as to whether or not Chihara ultimately achieves his stated aims.

(ii) Kitcher.

Philip Kitcher, in his [1984], wants to adopt an anti-realist position with respect to mathematical entities, but he does not want mathematics to come out as not being true, nor does he want to reduce knowledge of mathematics to knowledge of logic. His [1984] actually starts from the premise that we do in fact know the truths of mathematics – he accepts the 'obvious and uncontroversial thesis that most people know some mathematics and some people know a large amount of mathematics'[72]. Indeed Kitcher's book is intended 'to understand how this mathematical knowledge has been obtained'[73].

Now to achieve this position of anti-realism with respect to mathematical objects, but still keeping mathematics as a body of truths he faces the problems of producing an alternative account of truth (as Weir attempts to do – see below), an alternative account of reference, or a different account of the nature of mathematical knowledge. What Kitcher in effect does is to produce an alternative account of our mathematical language, in which statements of arithmetic (for example) are to be understood unproblematically as reducible to statements about what manipulations (of physical objects by physical subjects) are possible in the real world. To avoid the problem of this view becoming too narrow, he goes further and makes this account one about the possible manipulations by *ideal* subjects:

> I propose that the view that mathematics describes the structure of reality should be articulated as the claim that mathematics describes the operational activity of an ideal subject. In other words, to say that

---

72. Kitcher [1984], p.3.

73. Kitcher [1984], p.3.

> mathematics is true in virtue of ideal operations is to explicate the thesis that mathematics describes the structure of the world.[74]

Thus Kitcher avoids mathematical realism, in the sense of realism about a realm of transcendental objects, by reducing discourse about that realm into discourse about possible operations within the (real) world. The cost of this approach however is his reliance on ideal subjects knowing ideal facts; as Papineau puts it:

> I think that Kitcher ought to be rather more worried than he is about the epistemological accessibility of such ideal facts.[75]

We will return to Kitcher (in Chapter 8 above) for a more sustained look at his radically different epistemological stance.

(iii) Field's nominalism.

I will deal in detail with Field's famous views, expressed in his [1980], in the next chapter. For the purposes of this discussion however, I think it worth putting him into the general philosophical landscape. In fact this small task is not such a simple one, for as a nominalist there are elements of the reductionist tendency in Field, but as an instrumentalist, he may more properly fit into the next category – i.e. those who think that mathematical discourse is acceptable, but has some other point than genuine truth conditions, as Blackburn would say. I will consider both of these aspects of Field briefly. Typically, nominalistic accounts of mathematics attempt to reduce, or reformulate, statements with (for example) cardinality claims in them into statements with no such claims. For example, in chapter 2 of Field's [1980], he produces a nominalistic theory N 'that contains the identity symbol and the usual axioms of identity, but does not contain any terms or quantifiers for abstract objects'[76]. What this boils down to is that Field's view of arithmetic would ultimately want to say that statements such as 'The number of toads is at least 3' – clearly a statement which, if true would refer to some mathematical objects, could be translated (or reduced) to the nominalistic assertion 'Ex Ey Ez (x≠y & x≠z & y≠z & Tx & Ty & Tz)'. Whether or not a

74. Kitcher [1984], p.111.

75. Papineau [1987], p.159, footnote 2.

76. Field [1980], p.21.

nominalistic programme on these lines is possible or not is debatable[77], but Field may actually escape criticism from this type of reduction (although I doubt it) and my suspicion is that his nominalistic enterprise belongs more in the next section.

(iv) Weir's projectivism.

Weir, in his [1993], puts forward some epistemological objections to realism, and he does this from the standpoint of naturalized epistemology since he realizes that this is just where the strongest realist arguments originate. He describes naturalized epistemology as the programme of firstly supposing that a given set of objects exist, and then secondly trying to explain how we could form beliefs about such objects. He rejects the Quine/Putnam argument, essentially because it fails this test; it postulates a given set of objects, but does not adequately explain how we could possibly form beliefs about such objects, because it does not give us an explanation of how we could ever individuate such objects – i.e. how we could form 'definite individuating conceptions'[78] of them:

> This, however, is what I find most implausible about mathematical realism: its assumption that we can form definite individuating conceptions which pick out particular, individual objects and properties such as the number 7, the set of real numbers, the addition function and so on, even though we have no spatio-temporal or causal relation to them whatsoever. The Putnam indispensability argument . . . gives no explanation whatsoever of how one might arrive at individuating conceptions of mathematical entities.[79]

Weir does however espouse a position where he claims that mathematics (despite the above argument against Putnam) is in fact indispensable for physics, and thus he also rejects Field's nominalistic project. The reason he employs has to do with the need to use

---

77. For example, see Irvine's comments on the possibility or otherwise of such nominalist linguistic reductions, in the introduction to his [1990].

78. There is a strong echo here of Benacerraf in his [1965] – i.e. challenge C1 above – where there is an argument to suggest that if numbers are objects, then we must be able to individuate them from their role within the structure; there is also a slight echo of the notion of a 'criterion of identity' which will be seen to be important to E.J. Lowe's conception of the natural numbers in a later chapter.

79. Weir [1993], p.257.

mathematics to enable us to extrapolate to the very large and very small objects of physical theory – without mathematics he thinks that this would not be possible.

> . . . I still think the truth of mathematics is indispensable for physics for essentially the same reason as I think Putnam's indispensability argument fails the naturalized epistemology test. This time, though, it is the theoretical entities in science which are the problem: how does Field think we acquire individuating concepts of particles and forces microscopically small or "mega-scopically" large in size? . . . It is here, it seems to me, that mathematics comes in. The only naturalistic route to explaining how our conceptual grasp reaches beyond the observable is via the application of mathematics.[80]

Weir is left then with a requirement to account for the truth of mathematics that will be consistent with his fictionalism with respect to mathematical entities. He cannot rely on a referential account of truth since, as a fictionalist, any such account would render mathematics as false (as in Field[81]) since there is nothing to which the mathematical referring expressions can refer. He finds his account of truth in the notion of proof, tied in with a variant of a projectivist semantics. He has of course to be careful that his notion of proof is a non-Platonic one, and just as importantly one which does not lay him open to problems with Gödel's incompleteness theorem. He does this by taking the idea that 'being provable is like being legal – it is something grounded in human practices and institutions – . . . '; I am not completely convinced by this, not least because he does not work out fully the details of a theory of proof along these lines. His brand of projectivism is one where:

> In the mathematical case we get, for mathematical *p*:

p	*is keyed to*	'*p*' is provable
7+5 = 12	is keyed to	7+5 = 12 is provable[82]

---

80. Weir [1993], p.260.

81. See the quotation from Field immediately below in section III.

82. Weir [1993], p.264.

For Weir mathematics is a fiction, but it is true because each statement is provable[83]. So Weir in many respects is using a reductionist argument, reducing truth to provability, to generate his brand of anti-realism. Now despite my claim that Weir's position is that of the reductionist, reducing the notion of truth to that of provability, it must be pointed out that he specifically denies this claim:

> Of course '7+5 = 12' does not *mean* '7+5 = 12' is provable – if asked to give its meaning I cannot think of anything better than '7+5 = 12'. But what makes it true is simply the fact that it is provable and what competent speakers implicitly understand, when they grasp such a sentence, is that it is correct to assert it just when it is provable.[84]

But despite his protestations here to the contrary, I would judge his enterprise to be fundamentally a reductionist one.

III Instrumentalists.

(i) Field's nominalism (again).

If we are not to consider Field's programme as fundamentally a reductionist one, then it may be better to imagine that mathematical discourse has some point other than being a language with genuine truth conditions. For Field explicitly takes a referential account of truth.

> . . . the only known account of mathematical truth not subject to obvious difficulties is the same account of truth that works outside mathematics. (Tarski's account essentially.) According to this account of truth, the sentence 'There are prime numbers greater than seventeen' is true only if there is at least one entity with the properties of being a number, being prime, and being greater than seventeen; . . . I have heard it argued that unless we can come up with an alternative account of mathematical truth,

---

83. Weir has a problem here with Gödel's First Incompleteness Theorem, wherein there can be statements of mathematics that are true but not provable within the system. Now Weir acknowledges this but points out that Gödel's result is in fact parasitic on a particular notion of proof, which he himself is not using. Also, the position of being "fictional, but true" sounds paradoxical, and may not be even coherent; a problem for fictionalism – from Nolan and O'Leary-Hawthorne [1996] – which may entrap Weir is discussed in section IV of chapter 5 on Field.

84. Weir [1993], p.264.

> this consequence of the standard account of mathematical truth forces us to believe in mathematical entities[85].

He then goes on to conclude (famously) that since there is no other account of mathematical truth, and since mathematical entities do not exist, then mathematics cannot be true. Under our supposition of the non-existence of mathematical entities, Field denies the truths of the theorems of mathematics. It is worth noting straight away that he never considers any other possible account of truth, and yet Tait (see chapter 3, section II) has cast some considerable doubt on the suitability of Tarski's theory for the job required of it in mathematical discourse.

Field is of course rather unusual in his denial of the truth of mathematics, and it comes about just because he wants both to hang on to his referential account of truth, and to his rejection of the existence of mathematical objects – this is in direct contrast to Weir's approach. His position rests on the fundamental idea of conservativeness rather than that of truth[86], so that he is, in effect, an instrumentalist as regards mathematical entities;

> . . . mathematics is *conservative*: any inference from nominalistic premises to a nominalistic conclusion that can be made with the help of mathematics could be made (usually more long-windedly) without it.[87]

Because mathematics is conservative but not true, then the edifice of mathematics is a useful fiction – an instrument – which we can employ to make nominalist scientific theories easier to use. The details of Field's project will be discussed in the next chapter.

(ii) Papineau.

David Papineau adopts a strongly normative approach to epistemology in his [1987]. He develops a reliabilist doctrine for his

---

85. Field [1989], p.53.

86. This distinction is essential to Field, and will be dealt with in detail in the next chapter. For now, put briefly, mathematics is conservative because any nominalistic conclusions that can be deduced from a nominalistic theory along with mathematics, could be deduced from the nominalistic theory alone. In other words and put rather crudely, the addition of mathematics does not make a nominalistic theory any more "powerful" even if it might make it simpler to use.

87. Field [1980], p.x.

naturalised realistic view of the (independent) world; but note that his naturalism is one which accepts the independent reality only of the physical (scientific) world, and he does not accept the reality of mathematical entities. His reliabilist approach to epistemology is built around his interest in the avoidance of error.

> . . . part of what we believe about the natural world is that certain belief-forming methods allow us to respond reliably to natural facts. Natural science contains its own epistemology, so to speak, in that it explains how humans find out about natural facts. But there is nothing corresponding in mathematics. Mathematics certainly doesn't itself explain how humans discover mathematical facts. And even philosophers can't come up with any satisfactory story of how natural beings might respond to non-natural facts.[88]

With specific reference to mathematics, Papineau first counters the idea that mathematical statements can be reduced to statements about the natural world[89], by suggesting that such reduction would remove the contrast between mathematical and natural beliefs. Accepting then that the mathematical realm is one of non-natural facts, he goes on to oppose the idea that this realm has an independent reality, for such non-natural facts would (by the definition of non-natural) be outside the physical world, and thus we as natural beings would not be able to gain access to such facts. (Essentially, this is one of the problems highlighted in challenge C2 from chapter 3 above.)

Papineau tries to find a route between relativism and absolutism in mathematical judgements:

> . . . we should see the worth of mathematical judgements as in effect self-sustaining. We do want to accept that in general competent mathematicians only believe that p when that claim is correct. But we can't explain this generalization by saying that mathematical beliefs are caused by independent mathematical facts. Instead we should account for the success of competent mathematicians as due to the fact that there really isn't

---

88. Papineau [1987], p.160-161.

89. This is an important point, for his approach here does not sit happily with his acceptance of Field's nominalistic views, which he adopts (as does Field) to deal with the problem of applying mathematics to the world.

> anything more to 'p obtaining' than that competent mathematicians can be moved to the belief that p.[90]

This chosen path of Papineau's is one that I suspect is extremely difficult to tread, and I am not at all convinced that he succeeds in so doing. Eventually he falls in behind Field:

> . . . I would like to appeal to some ideas developed by Hartry Field (1981). Field indicates how applied mathematical statements . . . can be given an alternative 'nominalist' reading, according to which refer solely to structural features of the natural world, and are free of any reference to such 'platonist' entities as the number three itself.[91]

But isn't this nominalist reading just the reduction of mathematical statements to statements about the natural world, which it is Papineau's contention that we cannot do!

There are indeed several tensions in Papineau's brief analysis of mathematics which he does not adequately resolve. Firstly, as already noted, there is the tension between his rejection of reductionism and his acceptance of Field – and hence some form of reductionism. Also, he wants to maintain some notion of the truth in mathematics (truth-in-a-fiction, as in a novel), whereas Field is happy to reject truth for mathematics. And finally there is the tension between the objectivity that he appears to require for mathematics, and the fictionalist status that he gives it – we will deal with this specific problem in section VI of the next chapter, when we explore the nature of mathematical knowledge for Field (in particular), and for fictionalism (in general).

IV Mentalists.

(i) Kant.

As mentioned in the historical discussion of chapter 1, Kant views mathematics, specifically geometry and arithmetic, as the forms of space and time respectively. Although Kant is not part of the current debate, his

---

90. Papineau [1987], p.161.

91. Papineau [1987], p.177.

influence is strong, and mentioning him here is intended to be helpful in terms of keeping a well known landmark visible in a difficult landscape.

(ii) Armstrong.

David Armstrong is in general a thoroughgoing realist, yet his views on mathematics seem slightly at odds with his general, strongly physicalist position. These views on mathematics are put forward in his [1989] – a work that argues strongly for his position of 'combinatorial naturalism' – and are dealt with in more detail later, in chapter 8 when some ideas from Philip Kitcher are also discussed again; I sketch his account without detail here however for the purposes of the current brief survey.

Armstrong puts forward a view of mathematics as a set of a priori[92], analytic and necessary truths; these truths are about a mind dependent realm of possibilities – possible entities such as numbers etc. I suggest here that Armstrong is a mentalist because of the distinction that he himself draws between mathematical entities and other entities in his [1989].

> A mathematical entity 'exists' if and only if there is no contradiction in the idea that things having this mathematical property should exist, that is, if and only if it is *possible* that things having this property exist.[93]

His conception of number is that of a particular relation – the ratio between one property of an object and the unit property. This conception of number has much in common with John Bigelow's ideas (below, and in chapter 7), and is unusual in as much as it takes account of all types of numbers (for example: natural, rational, and real) without any special distinctions.

> According to this view, numbers are in the first place *internal relations between possible universals*. . . . I believe that these internal relations between universals are the logically central cases of the numbers.[94]

---

92. It is important to note that he 'purges' the a priori concept of its Cartesian overtones – he does not expect a priori knowledge to be certain (or incorrigible). This is the basis of an important argument against Kitcher – see chapter 8 below.

93. Armstrong [1989], p.125.

94. Armstrong [1989], p.127.

Because of Armstrong's naturalism universals in general must be viewed as mind independent entities, yet there seems to be some tension here with his conception of numbers as *possible* relations (and mathematical entities as possible entities in general), if necessary made up from 'expansions beyond' the actual, which strikes me as very much a mind dependent notion.

V Realists.

(i) Gödel.

Again, for the sake of completeness and as a landmark, Gödel's radical platonistic views are included here. The "standard" view of Gödel is of a Platonism in which the objects are transcendental, non spatio-temporal, and in which knowledge of these objects comes from some sort of obscure "intuition":

> . . . the objects of transfinite set theory . . . clearly do not belong to the physical world and even their indirect connection with physical experience is very loose (owing primarily to the fact that set-theoretical concepts play only a minor role in the physical theories of today).
>
> 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. mathematical intuition, than in sense perception . . . [95]

Typically Gödel is seen as rather philosophically naïve, for example by Chihara in his [1990], who, having quoted the above passage himself, claims that he doesn't 'find Gödel's reasoning on this matter very convincing'[96].

> First of all, the appeal to a kind of perception of mathematical objects does not seem to yield anything like a satisfactory explanation of the phenomenon under consideration. After all, there is supplied no description of a causal mechanism by which we humans are able to "perceive" objects

95. Gödel [1947], p.483/484.

96. Chihara [1990], p.17.

> that do not exist in the physical world. The appeal to mathematical intuition does not explain how we are able to "perceive" sets – it, essentially, only asserts that we do.[97]

But this intrinsic imputation of a naïve view of perception in general is perhaps a little unfair to Gödel, whose clarification (below) of his views, given in the paragraph immediately following that quoted above, is ignored by Chihara.

> It should be noted that mathematical intuition need not be conceived as a faculty giving an *immediate* knowledge of the objects concerned. Rather it seems that, as in the case of physical experience, we *form* our ideas also of those objects on the basis of something else which *is* immediately given. Only this something else here is *not*, or not primarily, the sensations. That something else besides the sensations actually is immediately given follows (independently of mathematics) from the fact that even our ideas referring to physical objects contain constituents qualitatively different from sensations or mere combinations of sensations, e.g., the idea of object itself . . . Evidently the 'given' underlying mathematics is closely related to the abstract elements contained in our empirical ideas.[98]

This is much less naïve than Chihara would have us believe, and in many respects it is rather Kantian in overtone – in fact explicitly so in Gödel's own text[99]. Gödel in fact goes further with his realism, and he justifies a realist position with respect to mathematical objects not only by their intrinsic properties (that make them accessible to at least some mathematicians), but he also justifies his realism by arguments that appeal to the extrinsic properties of mathematical objects – their theoretical usefulness.

> . . . even disregarding the intrinsic necessity of of some new axiom, and even in case it it has no intrinsic necessity at all, a probable decision about its truth is possible also in another way, namely, inductively by studying its

---

97. Chihara [1990], p.19.

98. Gödel [1947], p.484.

99. In footnote 26 on p.484 of his [1947], Gödel states: 'Note that there is a close relationship between the concept of set . . . and the categories of pure understanding in Kant's sense. Namely, the function of both is "synthesis," i.e., the generating of unities out of manifolds . . .'

> "success". Success here means fruitfulness in consequences, in particular in "verifiable" consequences, i.e., consequences demonstrable without the new axiom . . .[100]

Thus we may want to argue, certainly contra Chihara, that Gödel is best understood not as holding a sort of naïve Platonist position, but rather to suggest (conventionally) that Gödel's is in fact a well-considered Platonism, (and certainly not any sort of physicalism).

(ii) Hunter.

In his very forthright statement affirming a strong Platonist position ([1994]), Hunter sets out, and attempts to defend a very "traditional" Platonism:

> There exist things that are not the objects of any actual or possible sense-experience, and that are not themselves experiences of any sort. These things can only be grasped by the intellect: they can't be seen, touched, tasted, heard or smelt but they can be known.
> Further, these things are a main concern of philosophy.[101]

And in particular, with reference to mathematics, he goes out to meet the several challenges that specifically dog Platonism.

C1 – Benacerraf's challenge about what numbers cannot be: this first challenge rests on, amongst others, the argument that for numbers to be objects, then we must be able to individuate them from their role within the structure – since we cannot do this, then they cannot be objects[102]. Hunter, however, denies the claim that such individuation is necessary for numbers to be objects:

> I think Benacerraf fails to establish that numbers are not objects. Of course numbers necessarily stand in relations to each other, and of course number theory is concerned with those relations; and it is not possible to individuate numbers independently of the role they play in the system of

---

100. Gödel [1947], p.477.

101. Hunter [1994], p.151.

102. As noted above in footnote 78, we see a clear link here between Benacerraf's notion of individuation and Weir's 'definite individuating conceptions', and another strong link between Benacerraf's notion of structures, and the structuralist account of mathematics mentioned below.

relations studied by number theory[103]. It is not possible for something to be a natural number without other things being natural numbers. So numbers are not Aristotelian substances. But it does not follow that numbers are not objects, in the sense of 'object' introduced by Frege, viz.

> x is an object if and only if (i) some word for x functions logically as a singular term and (ii) there is a criterion of identity for x.

By that test the number 2 is an object.[104]

C2 – Benacerraf and Steiner's challenge about knowledge of abstract entities: Hunter faces down this challenge by casting doubt on the causal theory of knowledge (as Steiner himself does); he claims, in effect, that our knowledge of abstract entities (not only numbers, but such things as meanings) is much more certain than our knowledge of the causal theory of knowledge. In a similar way he attacks causal theories of reference:

> . . . it is a great deal more certain that we do refer to numbers than that any philosopher has produced an adequate account of reference.[105]

Hunter also goes and tackles Field's nominalism – he does this on three counts. Firstly he criticizes Field for not giving a reason as to *why* mathematics is conservative when it is only a set of fictions – Hunter himself subscribes to the view that a good explanation of the conservativeness of mathematics would be that it is a 'body of necessary truths' – this is a fair criticism of Field I think. Secondly he attacks Field for suggesting that the existential claims in (Platonic) mathematics are only "assumptions" and "unjustifiable dogma" – but I think his line of argument here is at best only suggestive, and at worst a mere rhetorical contradiction of the nominalist's claims:

---

103. This particular claim is common to the structuralists (dealt with below), to Benacerraf (arguably himself with structuralist tendencies) and also to Hunter; I am unhappy with this claim however, as is Maddy – see her [1990] p.174/175, for, following an example in Shapiro, I would argue (below) that the primeness of 13 is in fact nothing to do with its role in the structure, but rather something else. This example and others will surface again in my consideration of the structuralist account below.

104. Hunter [1994], p.159.

105. Hunter [1994], p.156.

> . . . it is not at all obvious that those propositions are assumptions, and it needs argument to show that they are. Nominalists take too much for granted in assuming that the Platonist is making assumptions.[106]

Finally, he makes a good point against Field's nominalist position in general, if not actually against Field's [1980] in particular, when he points out that the bulk of mathematics is pure mathematics – 'the stronghold of Platonism' – and that Field has not tackled this at all, since he has only set out to nominalize applied mathematics, the mathematics which is indispensable for science.

Overall, Hunter produces a robust defence of traditional Platonism, even though I suspect that he is rather too quick in his dismissal of those who have suggested problems with this position.

(iii) Tait's Platonism.

W.W. Tait's [1986] puts forward a view which he describes as Platonism, but which in many respects is hard to categorize here since his ultimate objects are mathematical propositions:

> . . . a mathematical proposition A may be regarded as a type of object and that proving A amounts to constructing such an object . . .[107]

On his way to putting forward this view of Platonism, he does produce some rather important arguments.

Firstly he reformulates the Benacerrafian challenge C2 into what he calls the 'truth/proof problem'. On the one hand it seems that mathematical propositions (in for example arithmetic) are about the system of numbers, and statements of arithmetic are true if they obtain in that system; on the other hand we discover the truth of an arithmetical statement by proving it. 'But what has what we have learned or agreed to count as a proof got to do with what obtains in the system of numbers?'[108]. He points out that this style of attack on Platonism is in fact by comparison, or analogy with physical objects. The evidence of our senses is a warrant for our knowledge of facts about the real world; a proof cannot be a warrant for our knowledge of facts in the system of numbers, for there

---

106. Hunter [1994], p.158.

107. Tait [1986], p.159.

108. Tait [1986], p.142.

is no (causal) interaction between the (human) proof and the (Platonist, abstract) system of numbers. Tait thus restates the challenge C2 above in a very similar way to both Benacerraf and Steiner, in order to counter it.

> . . . if a proof is a warrant for [an arithmetical proposition] A, then A cannot be about the system of numbers. If, on the other hand, a proof is not a warrant, then we have no mathematical knowledge at all.[109]

We have seen that at the heart of this challenge is a causal theory of knowledge (and ultimately, perhaps, a causal theory of reference). Now Tait's manoeuvre is, in effect, to face down the challenge by rejecting both a causal theory of knowledge for mathematical facts (thus having perhaps, in Benacerraf's terms, an implausible epistemology), and also as we have seen above in chapter 3, section II above, by rejecting the Tarskian account of truth which underpins the challenge (or is at least tacitly accepted as *the* referential account of truth).

How does Tait manage to do without the causal theory of knowledge? Firstly he compares the statement

(1) There is a prime number greater than 10.

with

(2) There is a chair in the room.

Now the anti-Platonist will claim that, since the warrant for the truth of (1) is a proof, then it can't be about the system of numbers etc. Whereas the warrant for the truth of (2) is the sense-experience of seeing a chair in the room. Tait then asks the sceptical question equivalent to the anti-Platonist's 'What has proof got to do with the system of numbers?', that is, he asks 'What has my sense experience got to do with the existence of a chair in the room?' This he calls the truth/verification problem.

> So, if A is a proposition about the sensible world of rooms and chairs, then it is true if and only if it holds in that world. But we sometimes count what we experience as verification for A. And why should these two things, what holds in the world of rooms and chairs and what we experience, have anything to do with each other? Note that it is not sufficient to point out that verification is not conclusive in the way that the existence of a proof

---

109. Tait [1986], p.143. See challenge C2 in chapter 3, section IV for Benacerraf and Steiner's equivalent statements of the problem.

> is, since the question is why verification should have anything to do with what holds in the world of rooms and chairs.[110]

It is important to note that Tait does not hold such sceptical positions – he thinks that they are muddled in fact – all he is doing is showing that the scepticism of the anti-Platonist is no different in type from scepticism about whether our sense experiences are warrants for our knowledge of facts about the world.

> Of course, scepticism about either is misplaced, and both the truth/proof and the truth/verification problems are consequences of confusion, and are not real problems.[111]

He defends both our canons of mathematical warrant for truth by proof, and also our canons of physical warrant for truth by sense-experience by suggesting that any challenge to them would also be a challenge to our canons of meaning.

> Why should the structure of reality be what is presupposed by the grammatical structure of our language as we have learned it? For example, the meaningfulness of a sentence involving '+' presupposes the truth of the sentence which expresses that '+' is well defined in the numbers. So scepticism about truth will already imply scepticism about meaning.[112]

Thus concluding that scepticism in both truth/proof and truth/verification terms is ultimately meaningless.

Of course it is the causal theory of knowledge that underpins the fact that our sense experiences are warrants for our knowledge of the world, and Tait notes that it is also central to Benacerraf's position from which the epistemological challenge C2 was issued. (More than this in fact, for Benacerraf binds in a causal theory of reference as well.) Yet Tait opposes this, countering Benacerraf's arguments that our account of mathematical objects must be extendable to knowledge of physical things. In particular he tackles Benacerraf's argument from the interdependence of knowledge of different types of things in different areas: our knowledge of (say)

---

110. Tait [1986], p.146.

111. Tait [1986], p.146.

112. Tait [1986], p.146.

mathematics helps us to predict the motions of physical objects, and so these pieces of knowledge – mathematical and physical – are interdependent.

> Consider a case of interdependence: a mathematical prediction of the motion of a physical object. First, we read the appropriate equations off the data – that is we choose the appropriate idealization of the phenomenon. Second, we solve the equations. Third, we interpret the solution empirically.[113]

How could this be if there were different accounts of knowledge for the material and the mathematical, asks the Benacerrafian argument. But Tait is unimpressed, and suggests that Benacerraf is confusing 'knowing how' with 'knowing that'; in the example, 'knowing that' some mathematics is the case is involved only at step two, whereas steps one and three involve 'knowing how' (to abstract or apply mathematics). Tait concludes (in a style reminiscent of Hunter here):

> The fact is we do know how to apply mathematics, and we do not causally interact with mathematical objects. Why doesn't this fact simply refute a theory of knowing how that implies otherwise?[114]

(iv) Quine, Maddy, Lowe, and Bigelow.

All of these are dealt with in detail in later chapters of the this work, and so will only receive a brief outline here.

Quine we have characterised above as a realist, fundamentally because he sees no difference in kind between physical objects (medium sized dry goods), theoretical entities, and mathematical entities. They are all for him convenient fictions that fill out and inform our sense experiences. Yet Quine cannot be considered a fictionalist, for he insists that such objects are to be taken as real; since we quantify over such objects – in the sense that they are possible values of variables in the language – then they must really exist; to be is to be the value of a variable.

Maddy approaches her set theoretical realism in a two-tiered way similar to Gödel's; she justifies her belief in mathematical objects (for her

---

113. Tait [1986], p.153.

114. Tait [1986], p.154.

these are specifically sets at this level, and numbers are then seen to be properties of sets) by arguing that they are in fact directly perceived, and she justifies her belief in other, more complicated, mathematical objects by an appeal to Quine's indispensability argument.

Lowe is more of an Aristotelian than a Platonist; his general ontological commitments involve a belief in not only particulars, but also in sorts. For him particulars are real physical objects, and sorts are Aristotelian (rather than Platonic) forms, which also exist in some real physical sense[115]. With this in mind, he sets out to show that numbers do indeed exist and faces down the first of the Benacerrafian challenges (C1) by arguing that they are sorts, rather than particulars: in fact he claims that they are sorts whose instances are sets. Also, he endeavours to escape the second challenge (C2) by modifying Maddy's suggestion that we do in fact directly perceive sets.

Bigelow declares himself to be a Pythagorean as well as a Platonist; this is because of his confidence that the 'physical world is laden with mathematical properties and relations'[116]. He identifies mathematical objects with universals – in particular with recurrences (the 'one over many' universals) rather than with what he calls truthmakers. He endeavours to loosen the grip of the idea that ultimately real things can only exist in one place at one time (point-instants) – and tries to make enough room for himself to allow universals which wholly exist at many disjoint places and times. Since numbers are universals – relations among objects in the simplest case of natural numbers, and relations between relations for all other types of numbers – then there is no problem for Bigelow in accepting that three can be wholly present in any, and every, triplet of objects.

(v) Resnik & Shapiro.

Resnik and Shapiro are structuralists (and we might also like to consider – as mentioned before – Benacerraf in this group), and even though their outlooks are different in detail, I will consider them here together. For the structuralists mathematics is the study of structures, or patterns. What is a structure? An example will help. In number theory, a

---

115. This is in fact rather difficult in Lowe (as I hope to show), for although he claims that both individuals and sorts exist, and that "exist" is univocal, he does claim that sorts and individuals have different *modes* of existence.

116. Bigelow [1988], p.2.

mathematician will study the natural numbers, but for Shapiro this will be the study of:

> . . . the structure or pattern of any system that has an infinite number of objects with an initial object and a successor relation (or operation).[117]

In a sense there is nothing new in structures, since mathematicians quite consciously study them – groups, rings, integral domains, and fields are part and parcel of abstract algebra. For the mathematician these structures are all defined set theoretically[118], but for the structuralist this is not possible since set theory is to be seen as just another structure, on a par with all of the others in mathematics. It becomes a problem then just how to define structures without using set theory. Shapiro suggests (vaguely) that a structure is

> . . . the form of a possible system of related objects, ignoring the features of the objects that are not relevant to the interrelations . . .[119]

Resnik is no more definite, telling us that a structure is some sort of complex entity which consists of one or more 'positions'. So this is the first nest of problems facing the structuralist – the problem of how to define any structure non set theoretically; the problem of defining what type of objects structures actually are; and the problem of explaining what type of objects the positions within those structures are. That Chihara considers the ontological status of structures and positions to be 'very strange'[120], and that Maddy can suggest that the structuralists consider numbers to be universals[121], is enough evidence to allow me to claim that the structuralists have not made their position here clear enough.

Another problem arises for the structuralists, for the claim being made by them is that numbers and their properties are totally determined

---

117. Shapiro [1983b], p.534.

118. For example, a Group is defined to be a set A, equiped with an operation *, satisfying the group axioms G1 – (closure under *), G2 – (the existence of an identity element in A), G3 – (each element must have a unique inverse), and G4 – (the operation * must be associative).

119. Shapiro [1983b], p.535.

120. Chihara [1990], p.133.

121. See Maddy [1990], p.173.

by their positions and roles within the structure or system. I am unhappy with this however, for, taking an example from Shapiro himself – the primeness of 13 – I would suggest that 13 is not prime for any reasons dependent on the structure of the natural numbers (as defined above by Shapiro), but rather because of extra-structural features of the properties of 13 objects (e.g. dots) i.e. 13 is prime just because thirteen dots cannot be arranged into the form of a complete rectangle, and it is hard to see how this feature is connected with any property that springs from the structure as such of the natural numbers[122]. In other words, Shapiro's definition of natural numbers seems to stress the ordinality of numbers, but seems to miss out certain cardinality features. Maddy makes a similar point against the structuralists in general terms:

> . . . some of the 'positions' . . . have properties beyond those they have solely by virtue of their relations with other positions within the structure.[123]

Are the structuralists Platonists? Are we to consider the structures as having their own existence as abstract objects? Shapiro is vague on the ontological status of structures, choosing to describe himself as a 'methodological Platonist' since he uses the word "structure" as a substantive noun and quantifies over structures – so in Quine's sense (of to be is to be the value of a variable) the structures exist.

---

122. It is hard to see, for example, how we can graft the notion of primeness onto the integral domain structure, without construing the objects of the integral domain as specifically numbers, and this would be to accept that there are indeed properties of numbers that go beyond their mere position within the structure.

123. Maddy [1990], p.175.

5. FIELD'S NOMINALISM.

I Field and Maddy.

Hartry Field, in particular his [1980], and Penelope Maddy, in particular her [1990], represent the opposite ends of the spectrum of thought about mathematical realism. Field as we have already seen briefly, maintains a fictionalist/instrumentalist account of mathematics with his attempt to nominalize science, whereas Maddy's 'compromise Platonism' proposes a 'set-theoretic realist' (or even, at a pinch or so she claims, a physicalist) account of the objects of mathematics; i.e. of sets – this is the point of set-theoretic realism. In considering these two very different philosophers and the difficulties that each of their approaches to the problems of mathematics throws up, I hope to show in this chapter the power of Benacerraf's challenge in his [1973]; in fact I hope to show that despite each of these philosophers' best efforts, and whilst they both explicitly cite Benacerraf's *double* challenge (i.e. both his ontological challenge – C1 – as expressed in his [1968] and his epistemological challenge – C2 – from his [1973] described in detail in chapter 3) as being taken up in their work, they both fail adequately to meet those challenges.

In starting to describe Field and Maddy as poles apart, I am rather jumping the gun: it would be useful first of all to look at what common ground exists between them. Maddy claims:

> Field and I agree that the indispensability arguments provide the best evidence for mathematics as a whole. Moving beyond Quine/Putnamism into Gödelian territory, we also agree that there are other possible forms of mathematical evidence . . . Finally, we agree that the Benacerraf-style worry is a real one, that the Platonist owes a descriptive and explanatory account of our knowledge (or reliability about) mathematical facts, . . .[124]

In other words she is suggesting that both she and Field:

(i) accept that the Quine/Putnam indispensability argument is the best evidence for 'mathematics as a whole';

(ii) accept that there are other forms of evidence available for mathematics; and

124. Maddy [1990], p.159.

(iii) accept Benacerraf's problem (from [1973]) as a real one.
Can we find any evidence in Field to support Maddy's claims here?
In support of (i) he has:

> . . . I believe it becomes clear that there is one and only one serious argument for the existence of mathematical entities, and that is the Quinean argument that we need to postulate such entities in order to carry out ordinary inferences about the physical world, and in order to do science.[125]

In support of (ii) he has:

> . . . it is then plausible to argue that considerations other than application to the physical world, for example, considerations of simplicity and coherence within mathematics, are grounds for accepting some proposed mathematical axioms as true and rejecting others as false.[126]

And in support of (iii) we can find for example:

> Perhaps the most widely discussed challenge to the platonist position is epistemological. Here the *locus classicus* is again a paper by Benacerraf (1973). Benacerraf's formulation of the challenge relied on a causal theory of knowledge which almost no one believes any more; but I think that he was on to a much deeper difficulty for platonism.[127]

Maddy then sets the tone for this and the next chapter, in which we will be able to compare Field's nominalism and, in the next chapter, her own Platonism, facing each other in just the way Benacerraf describes in his challenge (C2).[128]

> Against this shared backdrop, two possible strategies stand out: take mathematical statements to be (mostly) true and meet the epistemological challenge head on, or take mathematical statements (at least the

---

125. Field [1980], p.5.

126. Field [1980], p.4. (also quoted by Maddy – loc cit.)

127. Field [1989], p.25.

128. See Benacerraf [1973], p.661, quoted above in Chapter 3.

> existential ones) to be false, and explain why those falsehoods are so useful in applications.[129]

Clearly Maddy herself chooses the first path, and Field the second.

II Field's programme.

Now we can turn to consider Field's nominalism more closely; first some general comments. As we have seen above in the previous chapter, it is Field's stated aim to consider, and give an account of, how mathematics is applied to the physical world. He does this against a background of his (both famous and startling) conclusion that, in applying mathematics to science, the mathematics does not need to be *true*, but merely *conservative*. I will deal with the notion of the conservativeness of mathematics (with respect to science) later, but it is worth considering firstly whether or not we should even begin to regard this central claim of Field's as at all reasonable – Chihara in his [1990] puts the following case against Field:

> . . . mathematics has been regarded for hundreds of years as a developing body of truths; mathematicians and scientists have acted on this way of regarding mathematics: they have reasoned and constructed their theories with the tacit belief that the accepted statements and principles of mathematics are, for the most part true. Many of these mathematical beliefs have been checked and rechecked countless times, and in countless ways, by both sophisticated and elementary methods. Furthermore, this way of proceeding has yielded remarkable results and has been tremendously successful. We thus have some strong reasons supporting the belief that mathematics is a body of truths.[130]

I am not sure that this is so much as an argument by Chihara as a piece of rhetoric, but, however we view it, there is clearly a suggestion that there is a case to answer. As regards the truth of mathematics, we may consider Field's position in two ways: on the one hand Field is not necessarily

---

129. Maddy [1990], p.160.

130. Chihara [1990], p.172-3. We note here that this argument is a species of "success" or "usefulness" argument, common in scientific realism, and I will deal with this type of argument in more detail later in the next chapter.

claiming that the theorems of mathematics are simply false, but rather that they *need* not be true (using a Tarskian notion of truth, or perhaps more accurately for Field, his own correspondence theory of truth[131]) and thus that we need not be committed to an ontology of (transcendental) mathematical referents for the terms within those theorems – i.e. we can take a nominalist stance with respect to mathematics. But on the other hand Field does take considerable pains explicitly to deny the truth of mathematics:

> . . . acceptance of a standard conception of truth for mathematics doesn't itself require the existence of mathematical entities: it requires this only given the additional assumption that something like the usual mathematics is true (*an assumption I deny*).[132] [My italics.]

Now to be fair to Field here, his denial of the truth of mathematics is not the same thing as asserting its falsity. If we understand the distinction between truth and conservativeness aright, then we should find that a Field-like position – affirming the conservativeness of mathematics but remaining agnostic on its truth – might prove more defensible[133]. (It may be the case that this is in fact Field's position, but I am doubtful about this – he is not particularly clear on the point, but he does seem to want, as noted above, to take mathematics as actually false.) A possible analogy with scientific realism suggests itself here: Van Fraassen in his [1980] attempts to defend a position which affirms the true observable consequences of a scientific theory, but which remains at least agnostic, if not actually denying, the truth (again in the sense of Tarski, with implications for the reality of the theoretical entities referred to by the theory) of that theory. I will consider Van Fraassen's work in more detail towards the end of this chapter, and I shall also consider the status of mathematical knowledge – in particular Field's deflationary theory –

---

131. As mentioned above in chapter 3, section II, this is well evaluated by Schmitt [1995], p.180-195.

132. Postscript to the essay Realism and anti-realism about mathematics, Field [1989], p.77.

133. David Papineau puts forward such a position in his [1987], but as we have seen above in chapter 4, he does appear to have some difficulties with this. It seems that in attempting to maintain a sort of "pseudo-truth" for fictions (c.f. Papineau's "pseudo-beliefs"; p.164 in [1987]) he fails to be able to maintain the essential objectivity of mathematical facts.

which results from the Field/Papineau stance on the truth status of mathematical statements.

Leaving aside then the reasonableness, or otherwise, of Field's claim about the non-truth of mathematics, another preliminary worry with the general structure of Field's approach is his employment of Platonistic arguments – he, for example, proves the conservation theorem[134] using classical i.e. Platonistic set-theory. Now there is no problem with Field using Platonistic reasoning in arguments and proofs, but there is a potential problem of circularity in using such reasoning in the *explanations* of the utility of mathematics in science. The distinction I have in mind is this: it seems perfectly reasonable (even good practice?) on the one hand for Field to employ Platonistic reasoning in arguments that are designed to undermine the Platonist position – to establish the conservation theorem for example. He states clearly:

> . . . if I am successful in proving *platonistically* that abstract entities are not needed for ordinary inferences about the physical world or for science, then anyone who wants to *argue* for platonism will be unable to rely on the Quinean argument that the existence of abstract entities is an indispensable assumption.[135]

This is of course fine, but now on the other hand, Chihara in his [1990] has argued that it seems circular then to use this theorem – the conservation theorem thus proved Platonistically – to *explain* the utility of mathematics, for we are expected to believe (and Field must believe) the explanation, and how can we do so when we know that it contains false (for the nominalist) reasoning? Field argues for example:

> . . . I argue that mathematical entities are not theoretically indispensable, and that the entire utility of mathematics can be accounted for by its conservativeness, without assuming its truth.[136]

Why should we be swayed by, or accept, such an explanation asks Chihara; but is Chihara assuming that explanation implies truth? Truth need not be

---

134. He does this in his appendix to Chapter 1, and I will deal with conservation in detail below.

135. Field [1980], p.5-6.

136. Field [1980], p.xi.

the only basis for explanation (c.f. Van Fraassen again, for whom empirical adequacy rather than truth is the basis for explanation). It may be the case that Field escapes censure by Chihara's argument here just because he has in fact drawn the distinction between truth and conservativeness strongly enough.[137]

One last general worry about Field's programme that I would bring out before looking in detail at other aspects of his work, is a lingering doubt – rather more than this perhaps – about his ability successfully to nominalize other aspects of modern physics, for example quantum theory. Field seems to acknowledge this difficulty:

> At present of course we do not know in detail how to eliminate mathematical entities from every scientific explanation we accept; consequently, I think that our inductive methodology does at present give us some justification for believing in mathematical entities.[138]

But this does not seem to worry him overmuch[139], whereas I would suggest that any such examples of the failure of his programme would fatally undermine it – for if we can't nominalize even one area of our 'best theory', then the Quine/Putnam argument still remains intact. Field disputes this line of argument however, and in the introduction to his [1989] he argues against the idea

> . . . that our observations of the empirical consequences of physical law are enough to explain the reliability of our mathematical beliefs.[140]

by noting that

> the amount of mathematics that gets applied in empirical science (or indeed, in metalogic and in other areas where mathematics gets applied) is

---

137. There is some debate about whether or not explanation requires truth (or conservativeness, or empirical adequacy) as a basis: see section IV of chapter 6 below where Peter Lipton clearly thinks that explanations require truth.

138. Field [1989], p.17.

139. For example he still sticks to his strongly nominalist position all through the articles collected in his [1989]. Clearly he is sure (himself) that the nominalist programme can be achieved even if we do not know how to do this; I, however, remain fairly sceptical here.

140 . Field [1989], p.28/29.

> relatively small. This means that only the reliability of a small part of our mathematical beliefs could be directly explained by the proposal of the previous paragraph [above].[141]

He describes the idea that one can transfer reliability from a small part of (empirically essential) mathematics to the rest, the majority, of (empirically inessential) mathematics as 'bootstrapping up'. Field wants to argue that even the partial success of the nominalisation programme ought to undermine our belief in the reliability of mathematics.

> Suppose for instance that we could nominalize everything but quantum theory. If this were so . . . then the entire weight of our belief in mathematical entities would rest on quantum theory. Is it really believable that an adequate account of the reliability of our mathematical beliefs could be made on this basis?[142]

I, however, am unconvinced by Field's arguments here for two reasons. Firstly it does not attack Quine, in particular his subscription to what we might call the Quine/Duhem thesis, on his own terms; for mathematics is a large edifice, and this thesis suggests that our belief in the reliability of mathematics is due to the mutual support of all of that edifice taken as a whole. Secondly, I think that Field is being disingenuous in starting his nominalisation programme with the intent of undermining the indispensability argument, and then changing his position, when it becomes clear that the programme can't nominalize all of science, to one where *any* successful nominalisation is taken to undermine both the whole of (Platonistic) mathematics and the indispensability argument; the boot, I think, is still on the other foot, and it is still for Field to show that mathematics is in fact dispensable in science.

---

141. Field [1989], p.29.

142. Field [1989], p.30.

III Some technical worries about conservativeness.

Leaving aside now these initial grumbles, we can look in detail at some aspects of Field's technical apparatus. He opens his [1980] with an explication of what he suggests is a disanalogy between theoretical entities – entities that are postulated within some scientific theory, such as quarks, genes, etc. – and mathematical entities i.e. the transcendental objects of mathematics. His supposed disanalogy runs, at a first approximation, something like this: we cannot provide (he claims) an "attractive" reformulation of a physical theory[143] without its theoretical entities, but we can provide an attractive reformulation of a physical theory without mathematical entities[144]. His argument to support this disanalogy is tied to the notion of mathematics being *conservative* over a nominalistic theory, and this idea needs some detailed explanation. First we need some preliminaries; Field tells us that:

> A nominalistic assertion is an assertion whose variables are all explicitly restricted to nonmathematical entities.[145]

We will let N be a nominalistic theory – one in which all of the assertions are nominalistic as described above[146]. We also need a mathematical theory S. Now Field's suggestion is that:

> . . . for any mathematical theory S and any body of nominalistic assertions N, N+S is a conservative extension of N.[147]

---

143. Field is not too clear here on quite what makes a theory more or less "attractive" in any particular case, beyond giving us the strong implication, on p.8 of his [1980], that "reasonably attractive theories" should be ones which explain phenomena "in terms of a small number of basic principles". We can clearly see in this idea the traditional nominalist wielding of Occam's Razor!

144. I'm not sure to what extent this is a proper disanalogy; for if theoretical entities are being given as the analogues of mathematical entities, then there will surely be no disanalogy between the suggestion that there can be no attractive reformulations of physical theory without its theoretical entities, and the analogous idea that there can be no attractive reformulations of mathematical theory without its mathematical entities. I will take up this point at the end of the discussion of conservativeness in this section.

145. Field [1989], p.125.

146. At a later stage it will become very important to be clear about whether or not N is a first- or second-order nominalistic theory, but for this initial exposition here we can leave out this complication.

147. Field [1980], p.11.

What this means, at first approximation, is that any nominalistic conclusions which could be drawn from N+S, could also be drawn (possibly less easily, but that is not at issue here[148]) from N alone. (This is not too surprising a suggestion, for if the mathematical theory has no empirical content then it could never add anything that could ultimately become a nominalistic conclusion! – does Field's definition of conservation beg the question?) A worry here is, even if we grant that the question is not begged by Field's empirical-content-free mathematical theory S which ensures that all nominalistic consequences of N+S are consequences of N alone, there may still be a question about non-nominalistic consequences of N+S which are not consequences of N alone. And a second worry is that pointed out by Hunter, in his article [1994] mentioned in the last chapter, that Field never attempts to tell us just why mathematics is conservative; in other words, if he hasn't begged the question of conservation in his definition, then he still owes us an account of the reason why mathematics, as a set of fictions, is in fact conservative. (It may be suggested that his proof of the conservation theorem does just this, but I am not sure that a proof is the same thing as a reason, and in any case he would also have to deal with Hunter's suggestion that the best explanation of the conservativeness of mathematics is its truth.)

The initial definition of conservation that I have given is only really a rough first approximation, and it needs cleaning up a little; for N, being a body of nominalistic assertions according to the definition given above, may contain assertions that make N+S inconsistent – by, for example explicitly ruling out mathematical objects. To obviate this possible problem, Field creates what he has called an agnostic version of N, termed N*, by explicitly restricting every quantifier in each assertion in N to non-mathematical entities only. (For any assertion A in N, then A* is the resulting restricted assertion in N*.)

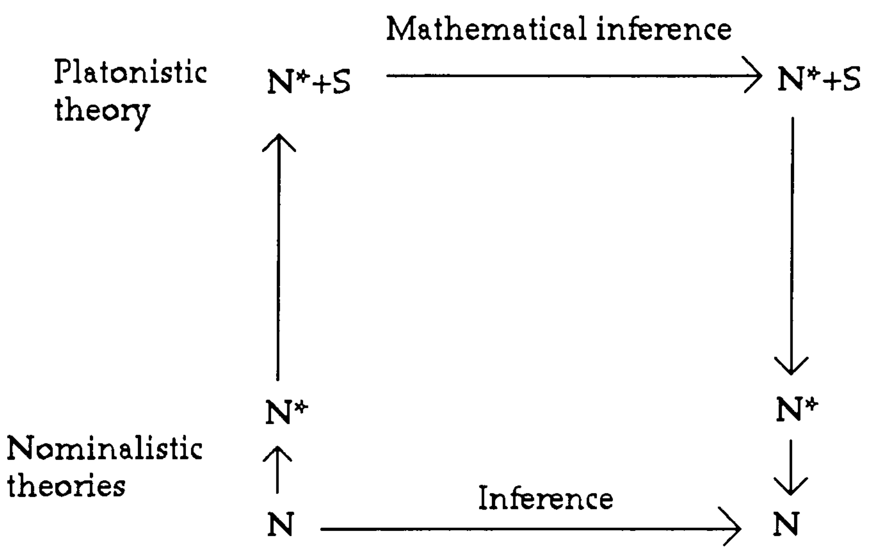
---

148. Field is at pains to point out the utility of mathematics, but he strongly stresses the difference between the usefulness which he claims is due to its conservativeness, and the idea of the theoretical indispensability of mathematics which, he claims, could only be due to its truth. Field is really putting forward two senses of the word "use" – one tied to conservativeness, and the other tied to truth. Mathematics is useful in the first sense if it helps in the deduction of nominalistic conclusions because it is conservative; mathematics is useful in the second sense – i.e. it is theoretically indispensable – if there cannot even be a nominalistic theory: in this case mathematics must be true. See Field [1989] p.64.

> N* is then the agnostic version of N: for instance, if N says that all objects obey Newton's laws, then N* says that all non-mathematical objects obey Newton's laws, but it allows for the possibility that there are mathematical objects that don't.[149]

Thus N*+S will not be inconsistent in the way that N+S could have been.

So Field is claiming that any Platonistic theory P (a theory that contains terms which purport to refer to some Platonic entities) is just a conservative extension of the nominalistic theory N*; whatever can be shown in P (=N*+S) could in principle be shown in N* alone. A picture will help here:



We will call the direct inference N –> N route **a**. We will call the route of indirect inference N –> N* –> N*+S –> N*+S –> N* –> N route **b**.

We can see clearly in this diagram that the *utility* of mathematics is to do with the fact that if the direct route of inference from nominalistic assertion to nominalistic conclusion (route a) is either too difficult or too inconvenient[150], then we can instead choose route b. Field's conservation theorem allows us to do this – we say that the diagram *commutes* – and, in particular, at no point do we require any statement of theory S to be true.

---

149. Field [1980], p.11.

150. It is important to note that conservativeness would guarantee route a as always being possible, for the impossibility of route a would be the same thing as the denial of conservativeness, and the affirmation of the indispensability of mathematics.

> Then if we want to determine the validity of an inference in N* (or equivalently of N), it is unnecessary to proceed directly; instead we can if it is convenient ascend from one or more statements in N* to abstract counterparts[151] of them, then to use S to prove from these abstract counterparts an abstract counterpart of some other statement in N*, and descend back to that statement in N*. . . . I must emphasise that the only thing required for the procedure to be legitimate is not that S be true but merely that N*+S be a conservative extension of N*, . . .[152]

This seems to be a *prima facie* plausible suggestion, but the case is not quite so simple. We need to distinguish between what Shapiro (in his [1983]) has called "deductive" conservativeness and "semantic" conservativeness. Shapiro defines these two thus:

> S is *semantically conservative* over N if and only if, for each nominalistically statable assertion A, if A is true in all models of N+S, then A is true in all models of N.

> S is *deductively conservative* over N if and only if, for each nominalistically statable assertion A, if A is a theorem of N+S, then A is a theorem of N.[153]

He notes that the distinction between these two depends on whether N and S are first- or second-order theories: Gödel's completeness theorem for first-order languages entails the equivalence of truth and theoremhood; however his incompleteness theorem for second-order languages entails a distinction between these two notions.

At this point we need to be quite clear as to what exactly Field has proved in the appendix to chapter 1 of [1980]. Here he proves firstly *semantic* conservativeness for first-order theories, and secondly he proves

---

151. This needs a little more flesh to be comprehensible. Field has in mind something like a statement in nominalized Newtonian physics (which might refer to such entities as space-time points which he condones within a nominalistic theory) as the statement in N*, being translated, using 'representing homomorphisms' into a non-nominalistic statement (in say the geometry of R^4; real space-time points from N* are being represented by fictional 'abstract counterparts' – points in R^4) in the theory S. I will have more to say about the status of space-time points and representing homomorphism below.

152. Field [1980], p.20-21.

153. Shapiro [1983], p.525.

deductive conservativeness (via the completeness theorem for first-order theories) for first-order theories only. Finally he indicates that similar arguments establish only *semantic* conservativeness for second-order theories.

Now Shapiro goes on to show, using an argument analogous to the one used by Gödel in the proof of the incompleteness theorem for second-order languages, that Field's given (second-order) set-theory-plus-urelements (i.e. second order set theory equiped with individuals – first order particulars) is not deductively conservative over his second-order nominalistic physics. Another line of argument in Shapiro aims to show that, if Field's theory is limited to being a first order theory (which he tries to do later in [1980]), so that he can have deductive conservativeness, then he cannot – irrespective of whether S is first- or second-order – form the relevant 'representing homomorphisms' from his space-time points[154] to their abstract counterparts in R^4. Thus Shapiro has caught Field in the horns of a dilemma: if his theory is only a first-order nominalistic physics, then he cannot form his necessary representing homomorphisms from the (nominalistically acceptable) concrete objects – his space-time points – to their (mathematical) abstract counterparts in R^4; but on the other hand, if his theory is a second-order nominalistic physics, then he can only have his mathematical theory S as *semantically* conservative over his physical theory, and this, I contend, will not do the work required of it.[155]

Field has pointed out (in his [1989][156] for example) that he only ever intended that semantic conservativeness; indeed he can cite an explicit footnote (no. 30) in his [1980] suggesting exactly that:

> . . . recall that conservativeness as I defined it initially is a *semantic* notion, one involving *consequence* rather than *provability*.[157]

---

154. We will discuss Field's use of space-time points, and the idea of representing homomorphisms for them below; suffice it to say here that these are parts of his technical apparatus that are required for use to make inferences around route b in the diagram above.

155. Interestingly, Shapiro also offers Field another possible route for escape by suggesting that with Henkin semantics he can avoid the loss of deductive conservativeness because of the Henkin completeness theorem; but the door here is quickly shut on Field, for in the resulting theory the mathematics, we are told, could also not be applied to the physics in the way Field indicates.

156. Field [1989], p.127.

157. Field [1980], p.115, footnote 30.

This sits however rather uncomfortably with the general thrust of Field's project. It seems to me that Field probably both needs and (ultimately) wants deductive conservativeness; this is hinted at in the text on many occasions[158]:

> . . . premises about the concrete can be translated into abstract counterparts; then, by reasoning with S, we can *prove* [my italics] abstract counterparts of further concrete statements, . . .[159]

Moreover, what is the content of the bulk of mathematics if it is not deductive proof?

Now we can briefly return to Field's original contention about the disanalogy between theoretical entities and mathematical entities. Having discussed the details of what is involved in reformulating science without mathematical entities, I have two significant concerns. Firstly, whilst noting how difficult it is to produce any scientific theory without mathematics, and hence mathematical entities, I remain unconvinced that the resulting nominalized theory is any more "attractive" (in Field's rather vague sense, where the word "attractive" is concerned with a 'small number of basic principles') than a *fully* nominalized theory without theoretical entities, such as a recursively reaxiomatised Craigian theory. On the contrary, if Field's nominalized science without mathematics is more "attractive" than Platonistic science (with mathematics), because it uses a smaller number of 'basic principles' (even if this makes it hard to use) then surely a fully nominalized science without even the theoretical entities will be even more "attractive" by the same token, for it will use even fewer 'basic principles'. Of course neither of these nominalized theories are attractive in any genuine sense, for neither have more explanatory power than ordinary science, and both are impossibly wieldy in use; thus Field's alleged disanalogy breaks down here. Secondly, if there is any analogy (between theoretical entities in science, and mathematical entities in mathematics) for Field to attack, then the "fact" (a fact, that is, for Field if not for others) that science can do without mathematical entities can only be analogous to the more obvious claim that

---

158. In fact I am not sure that Field is not being a little disingenuous by writing claims in the main text that imply deductive conservativeness, whilst at the same time relegating his knowledge that he can only show semantic conservativeness to a footnote.

159. Field [1980], p.25.

mathematics can do without theoretical entities; to put it another way, if there is an analogy here, then it must be that mathematical entities are indispensible for mathematics in just the same way that theoretical entities are indispensible for science, and Field's arguments do not touch this analogy[160].

IV Field's account of cardinality.

Moving on now to other concerns in detail, the second chapter of [1980] contains an example aimed to show, at the same time, both (i) the utility of arithmetic, and (ii) the dispensability of arithmetic. By doing this he displays his strategy for dealing with finite cardinality. Firstly he allows into his nominalistic theory N various extra quantifiers (all nominalistically acceptable, as they are all recursively axiomatizable and hence contain no references to abstract entities) – such extra quantifiers are things like 'E_{87}' for 'there are exactly 87'. Next he describes how, by translating cardinality statements in N (in his example statements about the number of bugs on a number of aardvarks) into their abstract counterparts in number theory, the conclusion that could have been drawn with difficulty using recursive definitions within the nominalistic theory now becomes relatively straightforward to draw – thus the mathematics is seen to be useful, and also at no point do we require it to be true[161].

There are perhaps two criticisms of this account; firstly the given example is not at all convincing, in that the suggested deduction by way of number theory would be in fact no easier than the nominalistic deduction itself, the problem lying in the difficulty of proving the equivalence of the nominalistic statements and their abstract counterparts – thus the utility claim must be doubtful here[162]. Secondly, Charles Chihara (again in his

---

160. Of course Field does not want to touch this analogy, for it is not quite where the thrust of the Quine/Putnam argument is. Ironically, if (as in the Quine/Putnam argument) mathematics actually is indispensible for science, then there will be a loss of symmetry between theoretical and mathematical entities – the mathematical ones being indispensible in a greater range of theory than the theoretical ones – but this is not the sort of disanalogy that Field wants.

161. Here, for example, we see as suggested in the last chapter, that Field does employ the translation (or reduction) of cardinality statements into other nominalistic statements that could ultimately be recursively axiomatized; this supports my suggestion that Field can be considered, in some sense at least, as a reductionist.

162. Field does acknowledge this problem later in his [1989] – postscript no.2 on p.78 – where he explains that, as well as number theory, he would also need some form of

[1990]) has criticized Field's account of cardinality in general, first of all by asking the question as to how we are, on Field's account, supposed to understand everyday statements of cardinality. The reply – in Field [1980] Chapter 2 – is implicitly given: everyday statements of cardinality are to be understood in terms of inductively defined numerical quantifiers – in other words by recursive definition. This, thinks Chihara, is too limited for it does not give us, at this point, enough to know how to analyse the truth conditions of statements like, for example, 'there are more dogs than cats'. But Field does eventually produce extra quantifiers later, in chapter 9 of [1980], to deal with this:

> Let us introduce the symbol F for the binary quantifier "fewer than": that is, let '[Apple(x)] Fx [Orange(x)]' mean 'there are fewer apples than oranges'. . . . It will turn out actually in Newtonian gravitational theory, if we add a new predicate to the theory then the 'fewer than' quantifier can very easily be dispensed with in favour of a still simpler quantifier 'E_{fin}' meaning 'there are only finitely many' (i.e. 'E_{fin} x Apple(x)' means 'there are only finitely many apples').

Now there appears to be a slight circularity in this, in that Field is using the notion of "finiteness" – a cardinality concept – to deal with comparative cardinality examples. It seems to me as if the notion of cardinality is itself getting tied up in his ultimate explication of cardinality.

Finally, another challenge to arithmetic fictionalism in general, if not to Field's fictionalism in particular, comes from Nolan and O'Leary-Hawthorne's [1996], and stems ultimately from recent discussion of the problems thrown up by modal fictionalism[163]. The challenge takes the form of a paradoxical problem for the fictionalist who wishes to reduce arithmetical discourse to discourse within some fiction. The sort of reduction Nolan and O'Leary-Hawthorne have in mind, taking their own illustrative example, is from statements like

(1) There are two moons of Mars.

to the logically equivalent (for the fictionalist) statement

---

metalogic which itself would postulate mathematical entities; fortunately for Field metalogic would also (so he reasonably claims, but does not prove) be conservative. This defence however still does not remove the doubt about Field's utility claim for mathematics in his given example.

163. See Hale, B. [1995], 'Modal fictionalism: a simple dilemma', *Analysis* 55 (1995), 63-67; Rosen, G. [1993], 'A problem for fictionalism about possible worlds', *Analysis* 53 (1993), 71-81; and Rosen, G. [1995], 'Modal fictionalism fixed', *Analysis* 55 (1995), 67-73.

(2) According to the mathematical fiction, the number Two *numbers* the moons of Mars.

Note that this is considered to be a logical reduction, (1) iff (2); and note also that the notion of *numbering* is taken as some sort of fictional relation.

Now the problem for the arithmetical fictionalist occurs when such statements are made reflexively about arithmetical objects. Again, taking Nolan and O'Leary-Hawthorne's example, we can consider the statement:

(3) There are (at least) three numbers.

The fictionalist does not want to accept this statement (3) to be true, but if he reduces it to its logically equivalent:

(4) According to the mathematical fiction, the number Three *numbers* the numbers (or at least *numbers* some subset of the numbers).

The problem comes with the truth of (4). If the fictionalists, such as Field, or Papineau (whose fictionalist position was briefly characterised in chapter 4), wish to use "standard" Platonic mathematics as the fiction (which they seem to want) the it is indeed true that 'the number Three *numbers* the numbers (or at least *numbers* some subset of the numbers)', i.e. (4) is true; and if (4) is true then so is (3). So the fictionalist finds that by accepting (Platonic) mathematics as a fiction, he must also accept (Platonic) mathematics itself!

Whether or not Field himself gets caught by this problem depends very much on whether we should consider him to be what Nolan and O'Leary-Hawthorne term a 'strong', or a 'timid' fictionalist. Strong fictionalism is the thesis that:

> . . . says that the literal content of, e.g., number operators is to be explained by (or reduced to) propositions about what's true according to a fiction.[164]

Timid fictionalism on the other hand is the thesis which:

> . . . says that claims involving number operators are often strictly and literally true but that they are not true in virtue of any facts about what is true according to a story that involves an ontology of numbers.[165]

---

164. Nolan and O'Leary-Hawthorne, [1996], p.27.

165. Nolan and O'Leary-Hawthorne, [1996], p.27.

Despite the fact that Nolan and O'Leary-Hawthorne claim that Field is in fact a timid rather than strong fictionalist, I suspect that he is neither; Field seems to fit a third position – 'broad fictionalism' – defined in a footnote:

> There is another style of fictionalism that is relatively untouched by the paradox, one which says that all use of the discourse . . . is strictly speaking false or truthvalueless . . .[166]

Thus I suspect that Field can in fact escape this particular problem for mathematical fictionalism, but on the other hand it may be that related positions such as Papineau's, which is rather more cavalier about truth (accepting some sort of fictional truth), are in fact strong fictionalisms and thus cannot so easily escape the problem.

V Field's underlying metaphysics.

The final set of objections to Field are aimed principally at his claims in chapter 4 of [1980] about the structure of space and, in particular, his ontological commitment to 'space-time points' and 'regions of space-time points'. The point at issue here is whether it is reasonable to regard these space-time points and regions of space-time points as *physical* entities on a par with electrons, quarks, and so on. We need to note that Field *needs* these entities for his nominalistic programme to work, for in chapter 3 of his [1980] he uses what is in effect Hilbert's axiomatization of geometry to show that we can translate traditionally *analytic* work involving metrical (and hence mathematical or Platonic) relations, usually in terms of real numbers or real-valued functions, into completely synthetic (nominalized) terms at the level of geometrical relations such as betweeness, segment-congruence, and angle-congruence; and Field's version of Hilbert's axiomatization explicitly commits him to a rich structure of space-time points and regions of space-time points. The problem is that these space-time points and regions of space-time points are just as transcendental as real numbers[167].

---

166. Nolan and O'Leary-Hawthorne, [1996], footnote 3, p.29.

167. A.P. Hazen's [1993] casts some doubt on the plausibility of regarding 'space-time points or other limiting abstractions as physical objects'. He argues that 'a physics would have to be a good deal more bizarre . . . in order for planes without thickness, lines without breadth, or dimensionless points, to play the roles of distinguishable physical objects . . .' (p.192.)

(At this point, as an aside, it is interesting to note a parallel between Field's programme – to nominalize science, and Hilbert's programme[168] – to formalise mathematics. This parallel is in fact more than interesting, since it may be of use as an analogy to help understand Field's notions of abstract counterparts etc. I set the parallel out as a table:

FIELD	HILBERT
Nominalistic theory (N).	"Contentual" part.
Mathematical theory (S).	"Ideal" part.
Conservativeness	Consistency (+ conservativeness, but these are coextensive in Hilbert's programme)
Breakdown by Shapiro's arguments.	Breakdown by Gödel's incompleteness theorem.

Indeed this parallel may be seen to be deeper than just this, for as we have just seen, Field explicitly makes use of Hilberts axiomatization of geometry.)

These strange objects – the space-time points etc. – are not the only seemingly transcendental entities which Field needs for his programme. Field makes use of 'representing homomorphisms' which must themselves have a questionable status – homomorphisms in general are part of the subject matter of mathematics which Field has set out to undermine quite explicitly in his definition of nominalism.

> Nominalism is the doctrine that there are no abstract entities. . . . In defending nominalism therefore I am denying that numbers, functions, sets, or any similar entities exist.[169]

and yet he needs just these entities – homomorphisms – to link his nominalistic assertions to their 'abstract counterparts' from the non-nominalistic theory. Another example: Linda Wetzel, after showing that

---

168. Referred to in chapter 2.

169. Field [1980], p.1.

expressions are also abstract entities, has pointed out in her [1989] that Field

> . . . gives no indication of trying to make do with the finite (and in fact very small) number of actual physical expressions there are, as Quine and Goodman do. He helps himself to an unlimited number of expressions . . .[170]

I would claim that Field's nominalism is not nominalism at all; he wants to purge science of the need for mathematical realism – a need expressed by the Quine/Putnam indispensability argument – yet all the while he seems happy with (for example) expressions, homomorphisms, space-time points and regions of space-time points, each of which has a questionable metaphysical status. Moreover, he cannot even begin to attempt his programme without them. Thus there seems little point in chasing a programme that can, at best, only remove some (perhaps numbers, but I hope to have undermined Field's programme even in this case) abstract entities from our scientific discourse, whilst leaving behind equally puzzling entities of similar metaphysical status.

VI Mathematical knowledge.

In this section I will deal with the important questions as to the status of mathematical knowledge which are posed by Field's denial of the truth of statements in mathematics, and Papineau's corresponding, if slightly weaker position on fictional truth for mathematical discourse. Clearly a mathematician has something which someone unskilled in mathematics does not have; ordinarily we would say that the mathematician *knows* more mathematics than the lay person, but this "ordinary" mode of discourse is not readily available to either Field or Papineau. Thus both Field and Papineau owe us something of an account as to what the difference is between the mathematician and non-mathematician.

Initially, a large part of the problem can be analysed away by noticing that a great deal of what the mathematician "knows" (compared with the non-mathematician) is indeed genuine knowledge of contingent fact – facts about other mathematicians, their biographies, lists of theorems

---

170. Wetzel [1989], footnote 1 on p.194.

derived by them and accepted by them, methods used and accepted by them, and so on. Also we could include as contingent fact all the mathematics that is generally accepted, the mathematics which we might describe as being part of a mathematical canon. Unfortunately for the nominalists, this does not cover everything that a mathematician "knows", and the question will still remain as to what exactly constitutes the difference between what a skilled mathematician has, and what a non-mathematician has.

Papineau, whose interest is epistemology (in the sense that he is interested in a normative programme for the production of reliable beliefs) rather than specifically in mathematics, is very direct about this problem. He is happy that the non-existence of mathematical entities implies that mathematical statements are false[171]; and he is happy that since mathematical statements are false, then we should not believe them.

> If mathematical objects don't exist, then any mathematical statements which imply the existence of such objects (that is, nearly all normal mathematics) will be false. Moreover we can't believe things we take to be false. So it follows from my position that we ought to stop believing the claims of mathematics.[172]

Papineau goes on to suggest that, by adopting a specifically fictionalist attitude towards mathematics, then we can have what he terms 'pseudo-beliefs' about mathematical propositions, akin, he claims, to the pseudo-beliefs of fiction, such as our "belief" that 'Sherlock Holmes lived at 221B Baker Street'. I remain deeply sceptical about the supposed parallel here, and for the very same reason that he gives as being the point of breakdown of the analogy between fiction and mathematics – that is the objectivity of mathematics.

> Even if the reader of a fiction is constrained by the text as to which pseudo-beliefs to adopt, the author of a fiction isn't constrained by anything but the conventions of narrative and the limits of his or her imagination. The mathematical author, on the other hand, seems to have no freedom at all in choosing mathematical pseudo-beliefs.[173]

---

171. But note that Papineau still wants mathematical statements to be true-in-a-fiction.

172. Papineau [1987], p.163-164.

173. Papineau [1987], p.165.

In trying to untangle the problem with which he has presented himself, Papineau tries to allow an objectivity in mathematical judgements (which most commentators would indeed admit was there), which comes from

> . . . their having an authorized provenance, in their having been derived in an authorized way . . .[174]

And these "authorized" judgements are not purely conventional and arbitrary, rather their objectivity is further, and ultimately, underpinned by their applicability in the real world. It strikes me that Papineau has two cases to answer: firstly I suspect that he cannot both have a thoroughgoing fictionalism along the lines he describes without there being more freedom and relativism in mathematical judgements; secondly, I am not convinced that he can effectively tie the objectivity of mathematics to its applicability in the real world, for not all (not even most) of mathematics is thus applicable, and yet we still would want some objectivity for (non-applicable) judgements. Of course, the restriction of Papineau's discussion ultimately to applicable mathematics is a strong echo of Field's approach, and he does in fact rely on Field at this point to resolve some of his problems.

Field approaches the problem of mathematical knowledge by putting forward a position which he calls 'deflationism'[175]. In this account we can see overtones of both logicism and the early Putnam's if-thenism, and it tries to cut a line between them. Field's deflationism is similar to logicism in that it sees specifically mathematical knowledge as logical knowledge, but it differs in not actually reducing mathematics to logic.

> . . . mathematics, taken at face value, can not be reduced to anything reasonably called logic.
>
> Still, I think that the idea that mathematical knowledge is just logical knowledge is largely correct, . . .[176]

---

174. Papineau [1987], p.162.

175. His account is given in Field [1989], p.79-115.

176. Both from Field [1989], p.81.

On the other hand, Field's deflationism is similar to if-thenism in that his claim is that mathematical knowledge is just the knowledge of which statements in mathematics follow logically from which other statements, yet it differs by allowing mathematics to contain a *greater* knowledge than just which statements follow from which.

> . . . in addition to knowing that certain claims follow from certain bodies of other mathematical claims, don't we also know the consistency of some of those bodies of mathematical claims?[177]

So what ultimately does Field's deflationism actually amount to? Deflationism here is the triple claim:

(i) that there is no mathematical knowledge of the claims of mathematics;

(ii) that there is (a great deal) of empirical knowledge about mathematics and mathematicians; and

(iii) that any knowledge that isn't of the empirical sort, is knowledge of a logical sort.

In response to the problem of adequately distinguishing what a mathematician has and what a non-mathematician has, is that whatever it is, it must be either empirical or logical.

> . . . some of the knowledge that separates those who know lots of mathematics from those who know only a little is straightforwardly empirical. . . . If one were to attempt a realistic account of all of the knowledge differences that separate a typical mathematician from a typical non-mathematician, I think that such differences in empirical knowledge would play a large role. . . . The interesting question, however, concerns the mathematical knowledge that remains when this straightforward empirical knowledge is ignored. The deflationist claim that I have defended is that the only such knowledge there is is purely logical . . .[178]

Despite Field's claims to the contrary, I remain strongly sceptical that his account squares with the brute facts of mathematical knowledge; the activity of mathematicians does not seen like the activity of logic –

177. Field [1989], p.82.

178. Field [1989], p.113.

indeed it cannot be so because of Gödel's incompleteness theorems: more can be proved in mathematics than in any particular logic – and I doubt very much that Field has adequately explained the 'differences that separate a typical mathematician from a typical non-mathematician'.

VII Van Fraassen's constructive empiricism.

In section I of this present chapter, I quoted Maddy as suggesting that there are two possible strategies to deal with the problems of mathematical knowledge etc., starting from her shared common ground with Field. These were Field's nominalistic approach, denying the truth of mathematics, which is a radical form of anti-realism, and her own set theoretic realism. Before we turn to Maddy's work in the next chapter, it is worth seeing now if there is a middle way between these strategies: can we construct a more modest form of anti-realism than Field's, along the lines perhaps of Van Fraassen's constructive empiricism?

Van Fraassen's approach restricts inductive inferences to observables only, and he allows only inferences about the existence of observable phenomena. The key notion here is that of 'empirical adequacy'; a theory is empirically adequate if its observable consequences are true – in particular this notion does not require us to believe that any unobservable consequences be true.

> Science, apparently, is required to explain its own success. There is this regularity in the world, that scientific predictions are regularly fulfilled; and this regularity, too, needs an explanation. . . .
>
> The explanation provided is a very traditional one – adequatio ad rem, the 'adequacy' of the theory to its objects, a kind of mirroring of the structure of things by the structure of ideas – . . . [179]

> *Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate.* This is the statement of the anti-realist position I advocate; I shall call it *constructive empiricism.*[180]

---

179. Van Fraassen [1980], p.39.

180. Van Fraassen [1980], p.12.

What Van Fraassen is doing is trying to drive a wedge between metaphysical (or semantic) realism on the one hand – which he accepts – and epistemological realism on the other – which he does not accept. He does not think that it is epistemologically acceptable to infer beyond the empirical evidence to possible unobservable entities; he does on the other hand argue for a belief in theoretical entities from a completely different standpoint, using "cosmological" arguments parallel to those in Aquinas's *Five Ways*.

There appears to be no *prima facie* reason why we could not defend a position of epistemological anti-realism with respect to mathematical objects, in which mathematics would be indispensable for science in the sense only that scientific theories plus mathematics were then empirically adequate. At the same time we could perhaps find arguments to accept a metaphysical realist position with respect to the objects of mathematics as well – perhaps again along the lines of the *Five Ways*; I shall not attempt this however, for I find Van Fraassen's general approach implausible. The main area which seems to me to make Van Fraassen's position unattractive is his reliance on drawing the distinction between observable entities, and unobservable entities.

The standard argument against the possibility of drawing the observable/unobservable distinction, due ultimately to Grover Maxwell[181], relies on some claim about there being a continuum of cases of "observation" running from direct observations (with the naked eye), through to inferences made with the help of scientific apparatus (for example bubble chambers for detecting neutrinos). Van Fraassen disputes that there is such a continuum for observations, whilst conceding that there may be such a continuum for what he calls detections.

> This continuous series of supposed acts of observation does not correspond directly to a continuum in what is supposed observable. . . . the moons of Jupiter can be seen through a telescope; but they can also be seen without a telescope if you are close enough. That something is observable does not automatically imply that the conditions are right for observing it now. The principle is:
>
> X is observable if there are circumstances which are such that, if X is present to us under those circumstances, then we observe it.

---

181. See Maxwell, G. [1962], 'The Ontological Status of Theoretical Entities', *Minnesota Studies in Philosophy of Science*, III (1962), p.7.

> This is not meant as a definition, but only as a rough guide to the avoidance of fallacies.
>
> We may still be able to find a continuum in what is supposed detectable.[182]

Van Fraassen seems to push us towards the idea that the observable is literally observable with the naked eye, rather than merely 'detectable in some more roundabout way'. But I suspect that he does not want to make the distinction here, for he would lose as physical entities all such things as cells and protozoa, which are only "observable" under a microscope. (On the other hand he may be happy with this: many entities would then come under the epistemological realist's banned list; but in this case, we could possibly use this as a *reductio ad absurdam* argument against him!) He does not, in fact, want to draw the line in any particular place, for he argues, not only that the above principle is not a definition, but also that "observable" is a vague predicate. Now a vague predicate suffers from many problems, not least from heap paradoxes, and Van Fraassen seems to be implying that there is indeed something like a heap paradox in the 'continuum of what is supposed observed'. He claims however that we can still use the distinction (between observable and unobservable entities) despite the vagueness, so long as we can give clear examples of the observable and the unobservable. (This would be like giving clear indications of things that definitely are heaps, and things which definitely are not heaps.)

> A vague predicate is usable provided it has clear cases and clear counter-cases. Seeing with the naked eye is a clear case of observation. . . .
> A look through a telescope at the moons of Jupiter seems to me a clear case of observation, since astronauts will no doubt be able to see them as well from close up. But the purported observation of micro-particles in a cloud chamber seems to me a clearly different case . . .[183]

Now Van Fraassen either has, or has not done enough to retain the distinction between the observable and the unobservable. I suspect that he has not, for I doubt that his defence even gets off the ground – I am inclined to say that ordinary observation with the naked eye is as theory

---

182. Van Fraassen [1980], p.16.

183. Van Fraassen [1980], p.16.

laden as sight through an optical telescope, a radio telescope, an optical microscope, an electron microscope, or even a bubble chamber. i.e. there is no line to be drawn at all! But if we grant for a moment that such a distinction is indeed tenable, then we can try to counter his constructive empiricism by other means. We may want to consider distinguishing between three groups of possible entities: the observed, the observable-but-unobserved, and the unobservable. Drawing the line in this tri-partite distinction on the grounds of observability, i.e. grouping observed entities with observable-but-unobserved entities, and splitting off unobservable entities, is a *metaphysical* distinction; drawing the line on the grounds of actual observation, i.e. grouping observable-but-unobserved entities with unobservable entities, and splitting off observed entities is an *epistemological* distinction[184]. Van Fraassen's is the metaphysical distinction, grouping the observable-but-unobserved entities in with the observed entities, but there are good reasons to suggest that, if a distinction is to be made, then this is the wrong one, so I am inclined to suggest that the epistemological distinction is a better one, and perhaps even a more natural one for Van Fraassen.

Lipton, in his [1991], produces several arguments for drawing the line between the observed entities on the one hand, and the observable-but-unobserved entities with the unobservable entities on the other. The first argument he calls the 'same path, no divide' argument. In this the suggestion is that, since there is no epistemic difference in the paths of inference from theories employing observable-but-unobserved entities, and theories employing unobservable entities, then it is disingenuous to suggest that there is any warrant to consider these entities as being different in type. A second argument, again used by Lipton and called by him the 'transfer of support argument', suggests that the epistemological anti-realist (and in fact any sort of instrumentalist) has less warrant for his conclusions since he cannot transfer support for them from observable cases to unobservable ones. These arguments of course, will apply equally well to any scientific anti-realist, and even to the mathematical anti-realist like Field.

---

184. It strikes me that this characterization of the two possible ways of distinguishing between observed, unobserved but observable, and unobservable entities strikes at the heart of Van Fraassen's programme; for if he wishes to drive a wedge between metaphysical realism and epistemological realism, this would appear not to be possible, as they do not split the tri-partite distinction in the same place.

The question we now face is whether anything from Van Fraassen's approach can be rescued to cut a middle way between Field's radical anti-realism, and Maddy's set-theoretic realism; can we accept scientific theory as true (with Field) but consider mathematical theory to be empirically adequate rather than false (unlike Field). It strikes me that we cannot, for if we accept Van Fraassen's approach we will not be able to be scientific realists about unobservables at all; thus if we wish to consider mathematics as empirically adequate, then we will also have to group much of our scientific theory as (merely empirically adequate) rather than true.

6. MADDY'S PLATONISM.

I Maddy and Gödel; Maddy and Quine.

Now we can turn to Penelope Maddy and review her realist account of the nature of some of the objects of mathematics given in her [1990]. The central tactic employed by Maddy – bearing in mind Benacerraf's challenge that any realist account of the objects of mathematics that is to preserve successfully the semantic face of the language, will have to give us a good account of how it is to overcome the epistemological problems of gaining knowledge of abstracta which are outside the causal nexus – is to face down the epistemological problem directly by claiming that sets at least are in fact directly perceived by us. In a sense this position is the most natural contender to oppose Field's position, for Maddy's claim seems, at first blush, to be just as extraordinary as Field's own claim as to the falsehood of the theorems of mathematics, and it is certainly in direct philosophical opposition to it.

Maddy is a thoroughgoing realist; she believes that when we produce a true cardinality statement, for example when we truthfully say 'there are three eggs in this box', then *something* must have, in some sense, the number property that is being predicated of it[185]. The somethings turn out to be sets (hence she describes her position as a *set theoretic realist* one) and numbers turn out to be properties of sets; thus she is doubly a realist in the sense that she not only believes in sets as real, perceptible (non-abstract) objects, but also she believes in the reality of universals, existing in all of those places where they are instantiated[186]:

> Assuming that numbers aren't sets, the set theoretic realist faces the prospect of adding a new type of entity to her ontology and an extra epicycle to her epistemology. . .
>
> Let's begin then with the suggestion that the epistemology for numbers should be as similar as possible to that given for sets. In that case, numbers must also be located in space-time. Where, then, is the number ten?

---

185. Several questions are clearly raised by this: (i) what is the nature of the somethings? (ii) what is the nature of the number property? (iii) and what is the nature of the having of a number property?

186. Later we will see that this position is strongly, and perhaps rather successfully argued for by John Bigelow in his [1988].

> The easy answer is: ten is located where the set of my fingers is located, in motion over the keys of my word processor.[187]

The first aspect of Maddy's position that we should examine is the notion, central to her argument, of a two-tiered epistemology. To understand this, we first need to see how she explicates Gödel's epistemological position, which Maddy refers to as 'Gödelian Platonism'. This, she tells us is a brand of Platonism in which we "perceive" the objects of mathematics by some mechanism (only hinted at rather than explicated clearly) called mathematical intuition. This intuition is supposed to do a job analogous to sense perception in the physical sciences[188], but Gödel goes beyond this in suggesting that, just as in the physical sciences there are facts about objects which cannot be perceived, then so in mathematics there are facts about objects that cannot be mathematically intuited; in the physical sciences such facts are accounted for and justified by their success within the theory – i.e. by their theoretical efficacy[189] – so in mathematics these further facts are accounted for and justified by their equivalent place within the mathematical theory – i.e. by their mathematical efficacy[190]. So Maddy is putting forward this general account of Gödelian Platonism:

> Thus Gödel's Platonistic Epistemology is two-tiered: the simpler concepts and axioms are justified intrinsically by their intuitiveness; the more theoretical hypotheses are justified extrinsically, by their consequences.[191]

It is this intrinsic/extrinsic contrast that is central for Maddy, and we need to take account of it if we are to understand her own epistemological position.

---

187. Maddy [1990], p.86/87.

188. This position has been mentioned above in chapter 3 where we saw that it was severely criticised – ridiculed even, although possibly a little unfairly – by, among others, Chihara in his [1990].

189 . Maddy suggests that the concept of theoretical efficacy should include '. . . their role in our theory, by their explanatory power, their predictive success, their fruitful connections with other well-confirmed theories, and so on.' – Maddy [1990], p.32.

190. Despite the definition given in footnote 188 immediately above, I think that the concept of 'mathematical efficacy' is too opaque and would need more clarification before it could become useful here.

191. Maddy [1990], p.33.

For her own part, Maddy takes this two-tiered approach and gives arguments that are similar to Gödel's, but altered in relevant ways, for each of those tiers. At the lower, intrinsic level, the objects of mathematics – in this case sets – are taken to be directly perceptible, replacing the rather problematic Gödelian intuition with ordinary sense perception. At the higher, extrinsic level, she takes what amounts to a Quinian indispensability argument to justify the existence of higher order mathematical objects. In part II below I propose to examine her arguments for the first, or lower of her two tiers, and in part III her defence of the second, or higher tier by looking in detail at the Quinian argument itself.

II The perception of sets.

In this section we will look in detail at Maddy's defence of the first tier of her justification of mathematical realism. Firstly it would be as well to rehearse the Benacerrafian problem (C2): we have been presented with a choice between semantics and epistemology – we either have to develop a separate semantic apparatus (including a theory of reference, a theory of truth etc.) to deal with specifically mathematical statements, or else we have to cope with the epistemological problem of producing an account of how we have (*prima facie*) knowledge about (again *prima facie*) abstract objects which are transcendent, outside the causal nexus. In Maddy's words, the second course of action runs like this:

> The Benacerrafian Syllogism rests on two premisses. The second is a traditional Platonistic account of the nature of mathematical entities as abstract, in particular, as non-spatio-temporal. The first premiss concerns the nature of human knowledge: what is it for me to know something? It was originally suggested, again by Plato, that it is enough that I believe it, that my belief be justified, and that the belief be true. . . .
>
> . . . For a justified true belief to count as knowledge, what makes the belief true must be appropriately causally responsible for that belief. This idea, in its many versions, is called the 'causal theory of knowledge'.
>
> The two premisses, then, of our Benacerrafian argument are the causal theory of knowledge and the abstractness of mathematical objects.[192]

---

192. Maddy [1990], p.37.

Maddy accepts a standard (i.e. a naturalistic and empiricist) semantic position for scientific language, and accepts that this position will do for mathematics as well. She accepts a causal/historical theory of reference[193] which seems to be based solidly in Kripke and Putnam – i.e. that we successfully refer, to things or types say, when we stand in an unbroken chain of borrowed usage (of the word) leading back to some initial dubbing of the thing or stuff.[194]

> Thus my use of 'Einstein' refers to Einstein . . . because it is part of a network of borrowed usage that extends me back to a somewhat imaginary event called an 'initial baptism'.[195]

Now Maddy's acceptance of such a causal/historical theory of reference as sketched above, along with her insistence that, for a true cardinality statement, something must have the number property, together imply that the referents of simple mathematical statements cannot be abstracta. Maddy's claim is that these simplest (lowest order) mathematical objects – sets – can in fact be perceived[196]. Maddy is then not so much a Platonist, as a physicalist – her account at this point seems to bend away from transcendental mathematical realism and towards immanent mathematical realism[197].

To start then we need to consider Maddy's account of perception, and in particular her account of the perception of sets. She starts with an account which, she claims, is due to Pitcher:

---

193. This stands in sharp contrast to some other mathematical realists – for example Hunter in his [1994] – where he explicitly doubts this type of theory of reference.

194. Obviously opponents of realism just state that we do not so stand, as there could never be such an initial act of dubbing for an abstract object. See for example Lear [1977].

195. Maddy [1990], p.38.

196. In fact we might like to defend a slightly weaker position here, that direct perception itself of sets is not required as Maddy proposes, but rather that we can get away with allowing the sets to cause only perceptual beliefs about their existence; this point will be taken up in the next chapter.

197. Although we must not conflate at this point the notion of a set (for Maddy a spatio-temporal and therefore perceptible object) and the notion of a number, which is a property of sets; my suggestion here is that properties of real objects could be considered just as immanent as the objects themselves, as the quotation from Maddy above (Maddy [1990], p.86/87) in section I implies.

> . . . for Steve to perceive a tree before him is for there to be a tree before him, for him to gain perceptual beliefs, in particular that there is a tree before him, and for the tree before him to play an appropriate causal role in the generation of these perceptual beliefs.[198]

Translating this specifically into the language of set perception, we have the following three conditions for Maddy's account of person S perceiving a set at location l:

P1 there is a set at location l;

P2 S gains perceptual beliefs, in particular the belief that there is a set at location l;

P3 the set at location l plays an appropriate causal role in the generation of these perceptual beliefs.

Now Maddy's assertion is that these three conditions are in fact met, and thus that sets – the bearers of the number property – are perceptible objects; but is it reasonable to accept all three of these conditions?

P1 – this is surely not unreasonable if we are already realists about sets, but Maddy will have to argue that sets exist (at specific locations) separately for this; she cannot employ the idea that we do indeed perceive sets in this manner – with the inherent implication of this condition P1 obtaining – as itself part of an argument for the existence of sets, as this would be circular.

P2 – this condition is certainly plausible – particularly if we accept all of the psychological evidence[199] which Maddy evinces for the gaining of perceptual beliefs through the operation of neuron cell assemblies acting as 'set detectors' along the lines of triangle detectors etc.

P3 – in a sense we cannot accept this condition unless we already accept that a set is just the sort of thing which can play such a causal role, and again, as with condition P1, this needs its own argument to convince the anti-realist.

Thus it seems to me that Maddy's arguments here for the actual perception of sets are not necessarily convincing for the anti-realist, or even for the realist who does not believe in the "special position" which such an account accords to sets. But leaving aside whatever doubts we may have about the satisfaction or otherwise of the perception conditions, and ignoring also any possible objection to Pitcher's theory of perception which

---

198. Maddy [1990], p.51.

199. This evidence is mostly from Hebb; see chapter 2, §2, in Maddy [1990], p.50ff.

is at root here, Maddy's assertion that sets are perceived faces a far greater problem. The objection comes from Chihara's [1990][200], and in many respects it resembles Benacerraf's objection (C1) to the identification of numbers with sets which Maddy is at pains to avoid. In short, whereas Benacerraf tells us that numbers cannot be sets because we have no principled way to decide exactly which set (from among several competitors) any particular number is to be associated with, so in a similar manner Chihara suggests that we cannot perceive sets for we have no principled way of deciding exactly which set (from among several competitors – possibly an infinity of them) we are in fact perceiving.

Chihara argues by contrasting what might be perceived when we look at a set containing a single apple, with what might be perceived when we look at a different set containing the same apple and the empty set – he calls this a set*. He goes on and asks what difference it would make if we looked at a set** or set***[201], or even the apple on its own as a singleton. Now Maddy argues (not necessarily convincingly) that there is a difference in perception between attending to the apple as a singleton, and attending to the set containing the apple. But even if we grant this distinction, then there still cannot be a principled way of deciding whether or not we are attending to a set, a set*, a set**, and so on. I would claim, with Chihara, that this objection fatally undermines Maddy's contention that we do in fact perceive sets[202], and thus I doubt her justification for the first, or lower tier of her Gödelian epistemology.

---

200. See Chihara [1990], p.201ff.

201. These are left undefined in Chihara, but it is clear that what he has in mind is a comparison of a set {a}, where a is the apple, as the set which Maddy claims she perceives, with a set* {{a}, {}}, and perhaps a set** is {{a}}, and a set*** is {{{a}}, {}}, and so on. Chihara is certainly right that we could in principle produce any number of these objects.

202. It may be possible that Maddy can escape Chihara's argument here, using a Quinian argument from part III below – I set this out briefly in footnote 210 below.

III The legacy of Quine.

As mentioned above, Maddy justifies mathematics at a higher level by an appeal to the Quine/Putnam indispensability argument. The purpose of this short section is to attempt to make clear the structure of this argument and other similar "usefulness arguments" that seem to be common in the literature, and which seem to spring from the current tendency towards naturalized epistemology. The scope of this section, and the range of examples employed, will focus largely on problems from the philosophies of science and of mathematics, for that is clearly our area of interest. We can start by looking at Maddy's definitions of naturalized epistemology:

> Our best understanding of the world, after all, is our current scientific theory, so by what better canons can we hope to judge our epistemological claims than by scientific ones? The study of knowledge, then, becomes part of our scientific study of the world, . . . Standing within our own best theory of the world – what better perspective could we have? – we ask how human subjects like ourselves are able to form reliable beliefs about the world as our theory tells us it is. This descriptive and explanatory project is called 'epistemology naturalized'.[203]

> On the naturalized approach, we judge what entities there are by seeing what entities we need to produce the most effective theory of the world.[204]

There is clearly a lot to unpack in here, not least of all the implicit reference to some sort of 'inference to the best explanation' within this general epistemological scheme, and as Maddy's self confessed inspiration is Quine, it would be as well to start with him.

Quine, as we have seen above, is very much the progenitor of the indispensability argument that seems to spring from this naturalized account of epistemology, and it is time that we looked in more detail at this strand of the story. Quine's views embody Duhem's thesis – that theories are confirmed only as totalities rather than as individual terms or

---

203. Maddy [1990], p.9.

204. Maddy [1990], p.29; c.f. Weir's characterization of naturalized epistemology in his [1993], p.255: 'One supposes a given type of object exists . . . and then set out to explain how we could form beliefs about objects of that type'.

statements. On this view, terms or statements from a theory are individually devoid of empirical content; empirical significance is carried by the theory as a whole; in fact for Quine this means that it is carried by the whole of science. I will quote him in full here:

> The idea of defining a symbol in use was . . . an advance over the impossible term-by-term empiricism of Locke and Hume. The statement, rather than the term, came with Bentham to be recognized as the unit accountable to an empiricist critique. But what I am now urging is that even in taking the statement as unit we have drawn our grid too finely. The unit of empirical significance is the whole of science.
>
> The totality of our so-called knowledge or beliefs, from the most casual matters of geography and history to the profoundest laws of atomic physics or even of pure mathematics and logic, is a man-made fabric which impinges on experience only along the edges. . . . A conflict with experience at the periphery occasions readjustments in the interior of the field. Truth values have to be redistributed over some of our statements. Reëvaluations of some statements entails reëvaluation of others, because of their logical interconnections – the logical laws being in turn simply certain further statements of the system, certain further elements of the field. . . But the total field is so underdetermined by its boundary conditions, experience, that there is much latitude of choice as to what statements to reëvaluate in the light of any single contrary experience.[205]

It is important to take stock of Quine's position here; he is involved in arguing (in [1951]) for a blurring of the (dogma of the) analytic/synthetic distinction, and also against the dogma of reducibility. (Reductionism in this context is the 'belief that each meaningful statement is equivalent to some logical construct upon terms which refer to immediate experience.'[206] i.e. the reducibility which Quine is against is the idea that ultimately all meaningful statements can be reduced to statements about 'immediate experience' – or perhaps even sense data.) The blurring of the analytic/synthetic distinction occurs because on Quine's view we see traditionally synthetic statements near that edge of the "fabric" which impinges on experience, and the traditionally analytic statements as being embedded much more deeply in the body of the fabric. So for Quine there

---

205. Quine [1951], p.42/43.

206. Quine [1951], p.20.

is a difference of degree between the analytic and the synthetic, but not a difference in kind[207]. He avoids reductionism by explaining that the postulated entities of experience are *more than* the experiences themselves – this is often referred to as Quine's holism:

> Here we have two competing conceptual schemes, a phenomenalistic one and a physicalistic one. Which should prevail? Each has its advantages; each has its special simplicity in its own way. . .
>
> The physical conceptual scheme simplifies our account of experience because of the way myriad scattered sense events come to be associated with single so-called objects; still there is no likelihood that each sentence about physical objects can actually be translated, however deviously and complexly, into the phenomenalistic language. Physical objects are postulated entities which round out and simplify our account of the flux of experience . . .[208]

This holistic view needs a little more fleshing out. Quine claims that our conceptual scheme is in fact underdetermined by the evidence available to us. In his [1948] he compares a physicalist account of sense experience with a phenomenalist one. To overcome the problem of underdetermination of these competing conceptual schemes by the (phenomenological) evidence available, he suggests that simplicity should be a criterion for helping to decide between them, but even so the "best" conceptual scheme is still underdetermined, for simplicity 'is not a clear and unambiguous idea'[209], and, more importantly, it is not evidence. But Quine wants to say more than this; he wants to be more definite. He argues that we infer a physicalist conceptual scheme because of the benefits that are to be gained by so doing.

> Physical objects are conceptually imported into the situation as convenient intermediaries – not by definition in terms of experience, but simply as irreducible posits comparable, epistemologically to the gods of Homer. . . .

---

207. Before we start imagining Quine to be propounding too strong an empiricism with respect to maths as Mill's, there is evidence to suggest that children do in fact learn mathematical facts from empirical evidence – for example middle ability children will only accept Pythagoras theorem after measuring triangles, whereas top ability children require a proof.

208. Quine [1948], p.17.

209. Quine [1948], p.17.

> But in point of epistemological footing the physical objects and the gods differ only in degree and not in kind. Both sorts of entities enter our conception only as cultural posits. The myth of physical objects is epistemologically superior to most in that it has proved more efficacious than other myths as a device for working a manageable structure into the flux of experience.[210]

Quine is here appealing to efficacy – he is in fact putting forward a type of usefulness argument here – to decide which myths are superior[211]. So he has used notions of simplicity and efficacy to help argue which myths are used to make sense of the world.

We can now see three levels of argument in Quine, which he would want to be different only in degree and not in kind, and these are:

(i) arguments for the existence of observed physical objects;

(ii) arguments for the existence of unobserved objects; and

(iii) arguments for the existence of unobservable objects[212].

We argue for the existence of theoretical (i.e. unobserved, but possibly observable) entities because they (are myths that) simplify and are useful in our best theory of the world; this argument is identical to one that we could employ to justify our beliefs in ordinary physical objects[213]; and finally, because there is no difference in kind between observable and unobservable objects for Quine – as myths that are useful and simplify – we can argue that mathematical entities exist as well.

---

210. Quine [1951], p.44.

211. It may be possible for Maddy to escape Chihara's argument from the end of section II above by using a Quinian argument like this one; for there is a structural similarity between her argument for the perception of sets from the perception of their members, and Russell's (et al) argument, taken up by Quine, for the perception of physical objects from the perception of sense data. Our perception of the world is underdetermined by the available evidence – the sense data – so the postulation of physical objects rounds out and simplifies what we perceive. In a similar manner, Maddy could argue that our mathematical perception (intuition?) is underdetermined by the available evidence (the objects which make up the set), so the postulation of sets rounds out and simplifies our mathematical perceptions.

212. This three-way distinction was seen in the previous chapter in our discussion of Van Fraassen.

213. This notion of considering the argument as a justification for our ordinary beliefs comes in Maddy [1990], p.8: 'What actually happens is a developing neurological mediation between purely sensory inputs and our primitive beliefs about physical objects. The justificationary inference comes later, when we argue that the best explanation of our stubborn belief in physical objects is that they do exist and that our beliefs about them are brought about in various dependable ways, . . .'

All of these strands in Quine lead to the indispensability argument which has been mentioned many times in previous chapters. Our scientific theories are empirically successful in that they are well confirmed by empirical scientific evidence (our theory is our "best" theory of the world); the evidence confirms not individual parts of the theory, but the theory as a whole (the Quine/Duhem thesis); the mathematics is part of the (fabric of the) theory (blurring the analytic/synthetic distinction); thus the existence of not only medium sized dry goods, but also theoretical entities and mathematical entities is confirmed by the evidence. The theoretical and mathematical entities are indispensable in two ways: firstly the theory could not be formulated without them (it is this strand that Field attempts to deny in the case of mathematical entities, even though he allows it for theoretical entities), and secondly Quine's denial of reductionism – his holism – suggests that they are more than convenient fictions that can be substituted out of our scientific discourse; science and indeed ordinary perception goes further, for Quine, than mere empiricism. Putnam sums this all up nicely:

> . . . 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.[214]

Naturalized epistemology, as captured in the above indispensability arguments seems then to be pointing us towards inferences to the best explanation; but how exactly do such inferences work, and are they reliable? Peter Lipton's [1991] is an attempt to explicate such a model of inference, and to question its reliability. I will briefly sketch out his analysis, and then see if it sheds any light on our current undertaking. I aim to argue that Lipton succeeds in showing that the Quine/Putnam use of some sort of inference to the best explanation is, like induction in Hume, not so easily justifiable, for any justification tends to be circular. (Not that this will worry those who favour this form of argument, for, as he also shows, the justification might have some weight for the already

214. Putnam [1975], p.347.

converted.) Lipton also argues that science in fact is more than empirical (echoing Quine again here, in that Quine employs principles – of simplicity etc. – that go beyond mere empirical fact to make sense of the world and of experience) and shows how science tends to go beyond the empirical adequacy of Van Fraassen by inferring the existence of unobserved and even unobservables in the construction of its "best" theories. I will also consider Elliott Sober who, in his [1993], argues against the indispensability of mathematics by showing just how difficult it is actually to accept the consequences of that indispensability argument; by framing science in terms of discrimination problems, his examples show how hard it would be to accept the contingency of mathematical statements inherent in the Quine/Duhem view. I do not think that Sober quite achieves any crushing argument against Quine, but what he does achieve is to show that those who accept the Quinian argument (and similar) often do not seem really to know what it is costing them in terms of the contingency of mathematics.

IV Inferences to the best explanation.

We start then with Lipton, who's self-confessed aim is to attempt both to explicate and also to justify the actual inductive practices of people and scientists. Starting from the problem that our inductive practices are in fact underdetermined by premises (of course they are, for they would be deductions otherwise!) and also that they are unclear – he talks about 'black box inference' – he goes on to describe and defend a model of empirical, inductive 'inference to the best explanation'.

In building this model he endeavours to make clear the notions of "best" and "explanation".

> Let us begin to flesh out the account by developing two signal distinctions that do not depend on the details of explanation: the distinction between actual and potential explanations, and the distinction between the explanation best supported by the evidence, and the explanation that would provide the most understanding or, in short, between the likeliest and the loveliest explanation.[215]

---

215. Lipton [1991], p.58/59.

An actual explanation is one that is exactly as described, an *actual* (thus true) explanation. This is not what we want in our inductive practice of inference to the best explanation we are told, for what we are trying to do here is to make some sort of inductive inference in order to find out the truth, or at least the approximate truth about something; clearly if our inference relies on an actual explanation, then we would already know the truth and our inference would then be empty[216].

> We are trying to describe the way we go from evidence to inference, but Inference to the Best Actual Explanation would require us already to have arrived in order to get there. In short, the model would not be epistemically effective.[217]

Potential explanation on the other hand allow us to have competing explanations, one (or even none) of which might be an *actual* explanation; thus a non-trivial account of inference to the best explanation must be an account of inference to the best *potential* explanation.

Secondly Lipton distinguishes between two notions of what it is to be the *best* of competing potential explanations – the likeliest[218] and the loveliest.

> . . . the distinction between the member of the pool of candidate explanations most warranted by the evidence – the likeliest explanation – and the member which would, if true, provide the most understanding - the loveliest explanation.[219]

---

216. Note here that Lipton clearly supposes that explanations require truth. We saw this assumption before in Chihara's criticism of Field (in §2, chapter 4); but an explanation may rest on conservativeness, or empirical adequacy, rather than truth.

217. Lipton [1991], p.59.

218. In what follows, Lipton never make it clear what he considers likeliness to be, for there are two candidates which are very different. P(O/E) – the probability that the observations (O), or facts, are as they are, given the hypothesis that the explanation (E) is true; and P(E/O) – which is the probability that the explanation is true given that the observations are as they are. My suspicion is that for Lipton the "likeliest" explanation E is the one for which P(E/O) is the greatest – the explanation which is most likely given the observational evidence O. This is equivalent to the most inductively strong explanation. Further comments on this distinction will be made in the next section (section V) with Elliott Sober.

219. Lipton [1991], p.185/186.

A couple of examples help to fill out this distinction: Lipton suggests that a (very) likely explanation of why the smoking of opium puts one to sleep, is because of its dormative powers; but this is not a very lovely explanation – i.e. it does not give us much understanding. He also suggests that certain conspiracy theories may be quite lovely without being likely – I am put in mind of the enormous web of conspiracy built up in Umberto Eco's novel *Foucault's Pendulum* which, whilst being highly unlikely, certainly assembles the evidence in a way which, if true, would give tremendous insight into the reasons why the events in the novel happened in the way they do. Of course Lipton ultimately wants these two conceptions of "best" to pick out the same explanation; more precisely, he wants to say that explanatory loveliness is in fact just what we use as a guide to explanatory likeliness. He rejects likeliness as a criterion for the best explanation for, he claims, the likeliness criterion would make the inference to the best explanation slogan trivial; to be useful, we need our criterion to describe how we actually judge explanatory likeliness – and this criterion is explanatory loveliness.

> We want a model of inductive inference to describe what principles we use to judge one inference more likely than another, so to say that we infer the likeliest explanation is not helpful. To put the point another way, we want our account of inference to give the *symptoms* of likeliness, the features an argument has that leads us to say that the premises make the conclusion likely. . . .
>
> So the version of Inference to the Best Explanation we should consider is Inference to the Loveliest Potential Explanation. . . . This version claims that the explanation that would, if true, provide the deepest understanding is the explanation that is likeliest to be true.[220]

Having built this model of inductive inference, Lipton subjects it to a battery of objections, and it is not really relevant here to describe or evaluate these. What is relevant however is the use he makes of his model for the justification of scientific inferences[221]. Firstly he puts forward the argument – which he calls the 'truth argument' – that scientific theories (arrived at themselves by inferences to the best explanation) are at least approximately true, since their truth is the best

---

220. Lipton [1991], p.62/63.

221. Chapter 9 in Lipton [1991].

explanation of their success; the scientific inferences (to the best explanation) that have been used to build the scientific theory are thus seen to be "truth-tropic". Science is thus seen to be an activity that produces true, or at least approximately true conclusions. On examination this form of argument is seen to be circular just as Induction in general is seen to be circular. Imagine a scientific theory, in the discovery of which inferences have been made using the best (loveliest) available of potential explanations. Imagine also that this theory is not only successful in in accommodating the available data, but also proves successful in prediction. How are we to account for the success of this theory? Well the best explanation of the success of the theory is that it is true. Thus the truth argument begs the question. This, then is the circularity objection:

> According to that objection, the truth argument is illegitimate precisely because it would be an inference to the best explanation, and so would beg the question, since only a realist could accept it.[222]

Accepting that the truth argument is circular (and despite his own defence of this argument, he also accepts its weakness in this context), Lipton also subjects it to a further objection. He asks the question as to whether the truth is an adequate (lovely) explanation of predictive success at all. Lipton considers what he terms the 'bad explanation' objection. This, he claims, is the reverse of the circularity objection. He asks the question as to how lovely the truth is as an explanation of the success of a theory, and the answer according to the bad explanation objection, is that it is not a good explanation, or even any sort of explanation at all.

> According to the bad explanation objection, the truth argument is not warranted by Inference to the Best Explanation, so even a realist ought not to accept it.[223]

But how could it be that the truth of a theory is not a good explanation of its success? One answer comes from Van Fraassen's work[224] which suggests a better explanation; and another stems from the idea that any

---

222. Lipton [1991], p.169.

223. Lipton [1991], p.169.

224. From Van Fraassen [1980]; some of these ideas were mentioned in the previous chapter on Field.

amount of actual evidence will always underdetermine any theory that seeks to describe it.

Van Fraassen draws a parallel between the Darwinian theory of natural selection, and scientific method which, he claims, is the method of selecting the most successful amongst competing theories. Taking an Augustinian example, he explains that mice run away from cats just because only mice that have behaved like this in the past have been successful (i.e. have survived), and thus this behavioural trait is inherited in mice now. Similarly, only theories that are successful (in terms of accommodating the available data, and with good predictive records perhaps: i.e. ones which are empirically adequate rather than true) are selected as part of our current scientific view at any point. This selection explanation pre-empts the truth explanation as a "better" explanation of the success of our current theories. I'm not sure that we should be too impressed by this type of explanation propounded by Van Fraassen, and nor does Lipton. The selection explanation does not explain as much as the truth explanation; in fact Lipton enumerates two facts that are explained by truth but not by selection:

> First, it explains why a particular theory that was selected is one that has true consequences. Second, it explains why theories that were selected on empirical grounds then went on to more predictive facts. The selection explanation accounts for neither of these facts.[225]

I'm not convinced that the first of Lipton's facts here really is a fact, for I'm not at all convinced that the truth of an theory can be said to *explain* why it has true consequences. His second fact however does seem to be one that Van Fraassen's account fails to tackle – that a theory is true is certainly a good explanation as to why it leads to more successful predictions; that a theory has been empirically adequate is not as good an explanation of any future predictive success of that theory. Thus the idea that Van Fraassen's selection explanation pre-empts the truth explanation is indeed undermined.

The other way of answering the question as to whether or not the truth explanation is the best explanation for the success of a scientific theory, is to say that it is no explanation at all. Given a necessarily finite amount of data, then this information underdetermines the theory that

---

225. Lipton [1991], p.172.

describes the observations – the same observations could support many, possibly incompatible theories, all of which could be equally successful. And how could we account for the success of these theories? Well the truth explanation, that the truth of the theory is the best explanation of its success, could now be applied to all of these theories, thus this explanation does not help us to infer one theory over any other[226]. In other words, if all theories are underdetermined by the available evidence, then there is no principled way of choosing between equally successful theories – we cannot use the truth argument with any of them, for they cannot all be true (they may all in fact be false!), and so we cannot explain their success by means of their truth: truth is thus no explanation at all of success here.

So we see that, by employing the 'circularity' objection, and the pair of 'bad explanation' objections, Lipton has successfully exposed some key weaknesses in any inferences to the best explanation. Yet if Lipton's discussion of of these does not wholly undermine Quine's indispensability argument, it does at least put it into some context.

V How indispensable is mathematics?

An alternative type of empiricism that is worth consideration here by way of contrast, springs from a strong criticism of the Quine/Putnam argument by Elliott Sober in his [1993]; he introduces introduces his own empirical view – 'contrastive empiricism' – against both the (Quinian) realist, and also Van Fraassen's constructive empiricist. Sober's view rests on two major premisses.

Firstly, Sober tells us that his contrastive empiricism requires science to be analysed as the attempt to *discriminate* between contrasting, competing hypotheses.

> According to contrastive empiricism, science attempts to solve discrimination problems. Consider the following triplet of hypotheses:
>
> (X_1) Moriarty (not Jones) committed the murder.
>
> (X_2) Jones (not Moriarty) committed the murder.
>
> (X_3) Moriarty did not commit the murder, although all the evidence will make it appear that he did.

---

226. This objection would of course also apply to Van Fraassen's explanation as well.

> The clues gathered by Holmes may discriminate between X_1 and X_2. But no evidence, gathered by Holmes or anyone else, will discriminate between X_1 and X_3. Contrastive empiricism views the first discrimination problem, but not the second, as scientifically soluble.[227]

Secondly, another cornerstone of Sober's position is the Likelihood Principle[228] which rests on the conditional probabilistic notion of the likelihood of an hypothesis (contrasted with the notion of the probability of an hypothesis):

> The likelihood of a hypothesis, H, relative to a set of observations O, is the probability the hypothesis confers on the observations. Don't confuse the likelihood of the hypothesis with its probability; P(O/H) is quite different from P(H/O). The *Likelihood Principle* . . . says that this mathematical idea can be used to characterize the idea of differential support:
>
> Observation O favors H_1 over H_2 if and only if $P(O/H_1) > P(O/H_2)$.[229]

So an hypothesis enjoys confirmational support by observations over any rival just when the probability of those observations are highest under that hypothesis. This likelihood principle for confirmation implies (and is implied by) an equivalent one for disconfirmation, making the relationship between confirmation and disconfirmation symmetrical.

> $P(O/H_1) > P(O/H_2)$ if and only if $P(-O/H_1) < P(-O/H_2)$.
>
> If the observations (O) favour H_1 over H_2, then, if the observational outcome had failed to occur, the opposite verdict about the hypotheses would be required.[230]

---

227. Sober [1993], p.39/40.

228. Sober takes the Likelihood Principle from A. Edward's *Likelihood*, (C.U.P., 1972).

229. Sober [1993], p.38. Note that favouring the notion of likelihood over probability, Sober is using quite a different empirical test compared with the inductivist (or, as I suggest above, Lipton). There is some question as to which is the most "natural" conception: Inductivists tend to be seen as trying to maximise the conditional probability of the hypothesis relative to the observations – P(H/O), whereas Sober is maximising the likelihood P(O/H). The only scientific use of likelihood seems to be in statistical hypothesis testing where the test is always on P(O/H).

230. Sober [1993], p.44.

This, Sober tells us, implies that observations can only confirm what can also be disconfirmed; so only dispensable statements within an hypothesis are confirmable. Sober now hopes that he can use this apparatus to disarm the Quine/Putnam indispensability argument: since mathematical statements within competing scientific theories are considered to be indispensable[231], then they are not then confirmable by observation.

Has Sober succeeded? Well Sober's main strands – both the idea that science is always contrastive, and also the likelihood principle – are not necessarily as easy to accept as he might like. Firstly he puts forward defences against possible objections to the idea that science is always contrastive – are theories always in competition? Sober says we can always find competitors, even if the contrast for a theory is with its own negation. This defence may work, even though I doubt that it is true to scientific practice. A tougher problem is to ask whether contrasting a theory equiped with a stronger mathematics against one with a weaker mathematics, where the one equiped with a stronger mathematics has predictive successes that the alternative, rather than producing false predictions, is completely silent upon, provides an undermining counterexample to contrastive empiricism?

> I now want to consider the suggestion that stronger mathematics may allow empirical predictions that weaker mathematics cannot produce. What I question is that the predictive success of the stronger theory is evidence that the mathematical assumptions are true. Suppose S embeds stronger mathematics than W does and that S makes true predictions about matters on which W is silent. Is this evidence that the mathematics in S is true? I suggest that if the mathematics in S is given *credit* for these predictive successes, we should be prepared to *blame* those mathematical statements when they occur in theories that make false predictions. . . .
>
> . . . It is less often noticed that mathematics allows us to construct theories that make false predictions and that we could not construct such predictively unsuccessful theories without mathematics.[232]

Secondly, as remarked above, there is a distinction between the likelihood and the probability of an hypothesis vis: P(O/H) – the

---

231. Sober introduces here two notions of indispensability: a priori, and a posteriori indispensability, and mathematics is supposed to be indispensable a priori.

232. Sober [1993], p.53.

likelihood of H relative to observations O, and P(H/O) – the probability of H given observations O. "Traditional" induction describes as inductively strong any argument whose premisses (observations) make the conclusion probable i.e. P(H/O) – the probability of H – is high in an inductively strong argument, and the observations are said to confirm the hypothesis. On the other hand, in statistical hypothesis testing (often used in science) experiments tend to use a low value of the likelihood – P(O/H) – as a *rejection* criterion for the hypothesis H; on the other hand, a high value of the likelihood is not said to confirm the hypothesis here, rather the hypothesis is merely not rejected in this circumstance. Thus our traditional view of empirical science would tend to employ the notions of probability and confirmational support, whereas perhaps Sober's contrastive empiricism is better seen as a more Popperian account – success is not in strong confirmational support, but rather in the non-rejection of the theory. Furthermore, Sober believes that our inductive (scientific) practices use the likelihood principle in discriminating between rival hypotheses. He believes that likelihoods can be well defined for hypotheses, but probabilities cannot be:

> Here Van Fraassen seems to join forces with the Bayesians, who think that the probabilities of hypotheses, and not just their likelihoods, are well defined and epistemically relevant.[233]

What are we to make of this? – I am not at all convinced that he is right here.

Finally, Sober claims that the symmetrical relationship between confirmation and disconfirmation – 'a hypothesis is supportable by observations if and only if there are observations that would count against it.' – conforms, he says to Popperian ideas except for the asymmetrical relationship between verification and falsification in Popper; but confirmation is not necessarily verification, and disconfirmation is certainly not falsification.

So what has Sober succeeded in showing? I don't agree that science is either necessarily contrastive, or that it is completely empirical, and I do not think that he has shown this either. Nor does he ever really attack Quine on his own ground or in his own terms; at all times it is reasonably clear that he is presupposing a difference in kind rather than in degree

---

233. Sober [1993], p.41.

between mathematical hypotheses and scientific ones – his article is an attempt to argue for this difference, but he seems to presuppose it:

> . . . do we really have alternative hypotheses to the hypotheses of arithmetic? If we could make sense of such alternatives, could they be said to confer probabilities on observations that differ from the probabilities entailed by the propositions of arithmetic themselves? I suggest that both these questions deserve negative answers.
>
> None of these critical comments apply to abductive arguments concerning genes or quarks.[234]

Nevertheless, by endeavouring to show that there are no observations that would make us reject our mathematical statements[235], he does undermine our possible belief in the Quine/Putnam indispensability argument, or at the very least his work indicates what we are forced to accept if we are to take wholeheartedly the Quine/Duhem standpoint.

> . . . my goal is to undermine a particular line of argument that purports to show that our justification of accepting '2 + 2 = 4' is purely empirical.[236]

> Perhaps the indispensability of mathematical statements in empirical science is some sort of reason to regard those statements as true. Nothing I have said here shows that this vague statement is wrong. What I have criticized is the idea that a mathematical statement inherits the observational support that accrues to the empirically successful scientific theories in which it occurs.[237]

VI Return to Maddy.

To conclude then, Maddy's defence of her neo-Gödelian lower tier founders on problems brought about by the introduction of the notion of *set* – we do not perceive sets because we do not have a principled way of

---

234. Sober [1993], p.46.

235. c.f. Gasking [1940].

236. Sober [1993], p.50.

237. Sober [1993], p.53.

deciding which set we are in fact perceiving (Chihara's argument). The higher tier argument is harder to dislodge, but aspects of the naturalized approach have been questioned in our discussion of Lipton's work, and the notion of indispensability itself is possibly undermined by Sober's arguments, at least for those who might find the full ramifications of the Quine/Duhem thesis rather hard to stomach. In any case, Quine's argument on its own does not do enough work for Maddy.

Beyond this, Maddy seems to suggest that numbers are properties of sets, that is universals of some sort. In her [1990] she does not fully work out this aspect of her account, and in the next chapter I turn to consider two possible solution to the 'numbers as universals' suggestion, the first from Lowe, and the second from Bigelow. Lowe explicitly follows Maddy's initial epistemology (at least it seems to be the possible choice amongst competitors that he favours), and he can be seen as trying to fill in this further gap in Maddy's account. Bigelow on the other hand does not follow Maddy in the perception of sets – for him numbers are not universals that are instantiated by sets; on the contrary, sets themselves are types of universal.

7. LOWE'S ARISTOTELIANISM AND BIGELOW'S PYTHAGOREANISM.

I Numbers as sorts.

In his [1993], E.J. Lowe puts forward the idea that the natural numbers are kinds (or sorts) rather than individuals. His position obviously entails a realism about universals, but his metaphysical position vis-a-vis these is to be understood, so he claims, as more Aristotelian than Platonic. Briefly put, and using the terminology from his [1989a], Lowe's thesis runs something like this – individuals are necessarily perceived as instances of some sort, or kind, of object; there are natural kinds like *horse*, and artefactual kinds like *chair* etc. Sorts are instantiated by individuals – thus Dobbin (say) might be a particular horse which is an instance of the natural kind *horse*; this particular chair in which I am sitting would be an instance of the artefactual kind *chair*. At this point it is worth noting carefully that it is essential for Lowe's argument in [1989a] that a sortal term must have a definite criterion of identity associated with it. (It seems to turn out that it has been, and remains, fairly hard to give such criteria of identity for ordinary objects – it has even proved quite difficult to decide what a criterion of identity actually is – see Lowe's own [1989b], but he does work hard to elucidate the concept in [1989a].)

Now, Lowe's claim is that the natural numbers are sorts, and as such they are instantiated by individual sets. (We might like to note that the sortal term "set" is one of the only ones upon which most philosophers can agree an adequate criterion of identity[238].) Thus three is a sort, and its instances are the various triplets of things: {a, b, c} is a three, as is {Peter, Paul, Mary}. Lowe's realism thus conceived enables him straightaway to escape Benacerraf's critique of mathematical realism in general – his challenge C1 – since he does not fall into the trap of insisting that the natural numbers are individuals (of some sort), but rather that they are themselves sorts (instantiated by certain individuals – in this case sets).

Lowe's general position here is metaphysically quite puzzling – I am not sure what type of existence the sorts are supposed to have (although

---

238. Two sets are identical if and only if they have the same members, in other words, when they are extensionally equal.

we are supposed to believe that they are as real as the individuals that instantiate them):

> . . . an important point to appreciate here is that the notions of *individual* (or particular) and *sort* (or kind) are, very arguably, interdependent and mutually irreducible. Individuals are only recognizable as *individuals of a sort*, while sorts are only intelligible as *sorts of individuals*.[239]

He also describes this mutual relationship as both necessary, and quite symmetric. He is also at pains to point out that he does not wish to suggest that individuals and sorts have different kinds of existence – one type for individuals and another for sorts.

> One thing which I should particularly stress . . . is that I most emphatically do *not* wish the title of this study [Kinds of Being] to convey the impression that I postulate different *kinds of existence*, as opposed merely to different *kinds of thing that exist*. 'Exist' is univocal.[240]

But he also wishes to have different modes of this univocal existence – he means to

> . . . acknowledge . . . such relatively uncontroversial facts as that (concrete) individuals exist at specific times and places whereas kinds, being universals, are not spatio-temporally localized in their existence.[241]

One thing that I find quite puzzling here is this idea that only individuals are spatio-temporal, yet only sorts (universals) are not[242]; yet "existence" is univocal! – I suppose that what this puzzlement boils down to is that the notion of 'different modes of existence' is hard to fathom, for the only

---

239. Lowe [1989a], p.11.

240. Lowe [1989a], p.4, and note that this is in sharp contrast to the Aristotelian view, expressed in the Metaphysics, and with which I opened the introduction to this study, where '"exist" has many senses'.

241. Lowe [1989a], p.4.

242. This contrasts strongly with Bigelow's views about the existence of universals. This account is similar in some respects – certainly "existence" is univocal in Bigelow – yet he is happy to accept, what I imagine to be an obvious consequence of this view, that universals are also spatio-temporal. The price he pays (and there is always a price) is that their existence is fractured in both space and time. Bigelow's views form the subject matter of sections V and VI in this chapter.

examples that I have of different modes of existence are that individuals have one mode of existence, and sorts another. This may be what is puzzling in Aristotle as well!

A related worry is that it is never completely clear quite how the instantiation process works – Aristotle again. It seems as if he has in mind that sorts are much the same thing as Aristotle's forms – but it is not obviously the case that Aristotelian forms had a 'symmetrical relationship' with the substances which they shaped. Ultimately then, I am not at all convinced that Lowe gives adequate reasons to believe in his (admittedly non-Platonic) sortal universe; to be sure he solves some problems (of identity etc.) but the cost seems to be high in terms of an extravagant, and not necessarily very clear ontology. But these considerations are not my main concern here, and we will accept for the sake of argument that Lowe's general account of the real existence of individuals and their duals – sorts – can be plausibly defended.

II The perception of sets again.

A more pressing problem with Lowe's account concerns the possibility of perception of the individuals (sets) which are the instances of the sorts "natural numbers". Sets are abstract objects, and thus – in the usual conception – by definition they cannot causally interact with us. It is with this epistemological problem that Lowe's account becomes a little woolly. He gives no less than four possible stories – or at least the beginnings of these stories – to account for the possibility of our perception of sets, but he clearly favours a reworking of Maddy's position (given in her [1990] – and dealt with in chapter 5 above) by Shaughan Lavine in his review ([1992]) of Maddy; so, for this reason, and the fact that this is the most conservative, and thus most robust account, I will focus on it.

Lavine takes Maddy's example of 'Steve opening a carton of eggs and seeing that the set of eggs has three members.' Maddy's account of this perception of the set of eggs is taken as literally seeing the set, and thus she grants that we can causally interact with sets – she is forced into the (not necessarily happy) conclusion that sets (or perhaps just certain sets) are not abstract objects at all. Now Lavine recognises that Maddy's position, vis-a-vis the non-abstractness of sets, may be hard to defend, but he also notes that such a strong line of argument is not really necessary. He puts forward what seems to be a more plausible account along the lines that, although we do not actually perceive the set itself, our perception of the eggs causes us to acquire a perceptual belief about the set which, he claims, is all that Maddy's general account requires, and of course, all that Lowe's requires as well.

I would like to trace in detail just what is being suggested here. Steve, on opening the carton perceives the eggs; also, if the conditions are right (i.e. if the eggs are suitably distinct from their background, and that they are simple similar shapes, all of which we may grant) he perceives that there are three of them. This much is not in contention, for empirical psychology seems to have gone some long way to showing how the perception of the number of objects there are (on a table, in a box, drawn on a piece of paper etc.) works, and in this case the perception – that there are three eggs – is generally not obtained by counting, but rather by subitizing[243]; for larger sets however it seems that both counting and estimating are both used to obtain some sort of perception of the number of objects in a collection. Thus, so runs Lavine's argument, Steve obtains a perceptual belief that the set of eggs in the carton has cardinality 3. The set of eggs is never itself perceived; and that the set of eggs exists (in addition to the eggs themselves) is to be accepted because of the indispensability of sets to our best theory about the world – the Quine/Putnam indispensability argument again.

Now, when Lavine presents his argument he notes almost as an aside that Maddy has a difficulty with the empty set here (how could we possibly have a causal interaction with that?) and suggests that his own account avoids the problem. Consider the case of Steve opening an egg carton and finding it empty: I do not find Lavine's rather glib suggestion that the empty set 'can be handled in the same way as other small sets can' to be at all plausible – it is all just too fast. Granted (for the moment) that the perception of the three eggs in the carton causes me to have a perceptual belief about the set of eggs in the carton (i.e. that Lavine's account is alright so far), it does not follow that when I perceive that an egg carton is empty I must acquire perceptual beliefs (or any other sort of beliefs for that matter) about the empty set. A null perception is not a perception of anything – certainly not of the empty set. Thus not only Maddy, but Lowe also has a serious problem dealing with zero.

Now Lavine's general account relies, as does Maddy's, on what we have described as the Quine/Putnam indispensability argument –

243. See Stanislas Dehaene's [1991] – "subitizing" is the term that is used for the perceptual process of recognizing the number of objects in a small collection, and it is characterised by its speed which is considerably faster than counting.

epistemology naturalized – and Lavine goes on to suggest a parallel between giving a naturalized account of seeing sets, and giving a naturalized account of seeing medium-sized physical objects:

> Her argument can be fruitfully recast in contrapositive form: skepticism about the possibility of giving a naturalistic account of seeing sets leads to a parallel skepticism about the possibility of giving a naturalistic account of seeing medium-sized physical objects.[244]

With this Lavine is suggesting that Maddy has a tool for explaining perceptual beliefs in a whole host of abstract items – statements, melodies, causes etc. (Lavine's list!). I am unhappy with Lavine's supposed parallel here, because it relies on the notion that perceptual beliefs about medium-sized physical objects are of the same strength (or type, or quality) as perceptual beliefs about sets. But this seems to me to misunderstand the Quine/Putnam argument. As I explained in the last chapter, the Quine/Putnam argument justifies our belief in the existence of theoretical (unobserved physical) entities because they are indispensable for (or are our best explanation of) our best theory of the world; by analogy we could justify our beliefs in the existence of physical objects by a similar argument; and I am unconvinced that sets play an equivalent role in our mathematical intuition as medium-sized physical objects do in our intuition about the world; sets are certainly not the most obvious elements of our (pre-philosophical) mathematical perception, whereas medium-sized physical objects are very much the most obvious elements of our perception of the world as naive realists. Numbers, rather than sets, seem to me to be more the obvious initial pre-philosophical elements of our mathematical perception, or intuition, the elements that should be seen by Lavine as equivalent to physical objects in Quine's scheme. Thus I would hope that we could use Lavine's contrapositive recasting of Maddy's argument to jutify our perceptual beliefs in *numbers*, rather than *sets*.[245]

---

244. Lavine [1992], p.323.

245. In section IV below I point out other problems which arise ultimately just because Maddy (and thus Lowe) is employing sets (rather than numbers) in a central position in the theory.

III Are numbers only dummy sortals?

Now, if we side-step the foregoing epistemological issues for a moment and grant that Lowe, with his several suggestions for circumventing the problems, can actually so do, let us then move on to the central part of his account, that the natural number are sorts, and that sets of objects are the individual instances of those sorts.

Firstly we note that Lowe's account at least affords us a reasonable account of counting. We count (a set) by forming a one-one correspondence between the members of the set in question, perhaps the set of bottles in a case of wine, and the members of some standard set. (Typically the standard set is a learned-by-heart sequence which has a structure that enables it to be continued indefinitely – the English verbal counting sequence 'one, two, three, ...' is an example, as is the modern Hindu/Arabic numeral system '1, 2, 3, ...') On Lowe's account both sets would be instances of the sort twelve, but neither would actually itself *be* the natural number twelve (nor would any other set, or set of sets, and it is thus that he escapes Benacerraf's famous critique) – the number itself exists as a sortal, and the standard set would only be a paradigm instance of that number. (Lowe does not mention paradigms in his [1989a], but I suspect that they have a role to play in, at the very least, language learning, and thus for the grasping of sortal concepts.)

Lowe claims that his account fits smoothly with the arithmetical operation of addition.

> The proposal, then, is that we take the ordinary language of addition pretty much at its face value and render '2 + 2 = 4' in words as '(A) two plus (another) two make (a) four'. This requires us to treat 'two' and 'four' as sortals and hence to recognize the legitimacy of such predicates as '... is (a) two' and '... is (a) four'.[246]

Lowe considers the teaching of this arithmetical fact – two plus two equals four – to a child, by showing the child firstly two objects (a two), and then bringing up another two objects (another two), and finally counting the resulting set of objects. But this will not do; the example does not run as smoothly as Lowe suggests, just because of his own insistence that the particulars that instantiate the sortal "two" are (two membered) sets.

---

246. Lowe [1992], p.143.

Consider Lowe's example again. Here's a two: {a, b}; here's another two: {c, d}. If we put them together (i.e. if we juxtapose the first two and the second two) we have the set: {{a, b}, {c, d}}. Unfortunately this is a two, and not a four, since as a set it has two members which are themselves sets. For Lowe's example to work, we would have to lose sight of the fact that the original twos were sets – {a, b} and {c, d} – and suddenly consider them only as individuals. But Lowe gives us no good reason to do this[247].

Not only does addition suffer such a problem under Lowe's account, but so also does multiplication. Consider the set {a, b, c, d, e, f} which is a "six": now if we arrange the members of the set as two "threes" (vis: {{a, b, c}, {d, e, f}}) then this is *not* a "six" but a "two". The problem seems to arise because Lowe wants us to perceive, or form a perceptual belief about, a set, and the criterion of identity for a set is very strict – as noted above, two sets are identical only if they have the same members (i.e. if they are extensionally identical), and under this criterion it would never be possible for two threes to be a six. What would be better here, but which would not fit into Lowe's general metaphysical position, would be the suggestion that we just perceive the (for example) six objects as a group of six objects (as mentioned above, there is good empirical evidence – see Dehaene's [1992] – that we do just this for small collections); the notion of 'two threes are six' would then just be the "seeing as" of these six objects as two threes. I will take up this idea again below with Bigelow's account[248].

It is hard to see how the forgoing problem – the multiplication problem – could be circumvented, but, leaving it to one side for a moment, we can see that Lowe also falls foul of a larger objection. In his [1989a] he argues:

> A sufficient, but not necessary, condition for a general term's being a sortal is that there should exist some principle for *counting* or *enumerating* individual instances falling under it. Thus there are ways of counting the number of *men* or *tables* or *books* in a given room, but no way of counting the number of *red things* there are: and this is not because there *is* such a number but one beyond our powers of determining (as in the case of the number of *atoms* in the room), but because it apparently does not even make sense to

---

247. My contention here is that he can't give us a good reason here; for if he could, then it would be a good reason not to consider sets as the perceptible things which instantiate the numbers at all.

248. The ideas of "seeing as", or aspectual seeing, is taken up in section VII at the end of this chapter.

> speak of such a number until the *sort(s)* of red thing one is to count have been specified. . . . It rapidly becomes apparent that there is no principled way of deciding these matters, until we are told what *sorts* of red things we are supposed to be counting.[249]

What I want to argue is that Lowe's suggestion (that the natural numbers are sortal terms) fails his own condition for being a sortal – it clearly makes no sense to ask how many threes there are in a room, until one specifies threes of what sort. Lowe later calls such apparently sortal terms "dummy" sortals – sortals which function grammatically as count nouns, but which do not function logically as count nouns. The examples he gives are *things* and *objects;* they do not convey any criterion of identity, and although they are superficially (grammatically) count nouns, they have no principle for counting.

Now it may be objected that, in the above quotation, Lowe has stated that the condition given for a general term's being a sortal – that we should be able to count the number of instances of its individuation – is only sufficient and not necessary. But Lowe's only given class of exceptions are the mass nouns:

> But, to repeat, the countability of instances falling under it is not a necessary condition for a general term's being a sortal, since the so-called *mass nouns* like "gold" and "water" apparently have criteria of identity associated with their use despite the fact that it makes no sense to ask *how many* instances of gold or water exist in a certain place.[250]

Well the natural numbers are not mass nouns, so it seems that terms like "three" and "twelve" are only dummy sortals like "thing". Real sortals have, or convey, a criterion of identity for the individuals instantiating them; we cannot count individuals unless we can individuate them, and we cannot individuate (for example) sixes unless we know what in particular is to count as a six. We can also see that this objection is related to the multiplication problem where there was a difficulty with saying 'two threes are six' since two threes are a two, but a six is a six – we do not know how to count how many twos there are in a room, for how do we

---

249. Lowe [1989a], p.10.

250. Lowe [1989a], p.11.

know whether or not to count the sixes (as two threes, i.e. as a two!); i.e. we need to know what *kinds* of twos to count.

IV Problems with sets.

Underlying Lowe's thesis are the assumptions (i) that sets exist (relying perhaps on some type of naturalized epistemology) and (ii) that we can either perceive (Hale, Maddy), or at least form perceptual beliefs (Lavine) about, these sets. But it seems to me that it is the set concept itself that causes the epistemological problems from which Maddy's argument (for the perception of sets), and even Lavine's modification of Maddy's argument, cannot extract us. At the very least, in the extreme case of the empty set, neither really produce a convincing or plausible account. Lowe's account, seen as an attempt to clean up the notion of 'numbers as universals' or 'numbers as sorts' which is implicit in Maddy's account, is clearly parasitic on either a Maddy-like account, or some other variant[251]. If he accepts Maddy's initial arguments, then I hope that I have undermined these in the last chapter, and if we grant that he can at least get started with the perception of sets in at least some sense, then I hope to have shown above that his account of numbers as sorts is flawed – he has to avoid the multiplication problem above, and more importantly the problem of numbers as dummy sortals which is perhaps its root cause[252].

What is needed then is a different conception of natural numbers that avoids the notion of set completely, at least in a central position within the theory, but clearly this suggestion will need some expansion. Consider again the example of Steve and his carton of eggs; most obviously he perceives the eggs; less obviously, but with some scientific justification (given in Dehaene's [1992]) he perceives the number of eggs. (Note that for lots of eggs, he does not immediately perceive the number

251. As mentioned above, Lowe himself suggests as many as four possibilities.

252. In fact I might also suggest that Lowe's account of numbers as sorts is flawed for another reason. It strikes me that the distinction in Lowe between count nouns and mass nouns is very suggestive of the distinction between natural number (counting) and real number (amount). There appears to be a germ in Lowe's work to suggest that the notion of number (both natural and real) needs to be treated differently from the notion of a sortal term, because it seems to be in some sense logically prior: the existence of count nouns in the language seems to imply in some sense the notion of counting, and thus the idea of natural numbers; and the existence of mass nouns in the language seems to imply the notion of amount, and thus perhaps the idea of real numbers. It would be beyond the scope of this present work to explore this more fully.

of eggs without some further activity but ultimately he comes to believe that the number of eggs is x, say, by counting – i.e. by comparison with some standard set – we should note then that this gives him no exact perceptual information, only information of a conditional type[253].) Now, sets do not yet come into the picture so there is no problem with our seeing six eggs as three lots of two eggs, or two lots of three eggs, and these still being the same number of eggs; for us there is no multiplication problem. It is surely just a matter of empirical psychology that we can see several objects as one pattern or another. It is a matter of our individual perceptive ability how well we can do this "seeing as" – the ability of a subject to see the famous "duck-rabbit" of Kohler[254] (for example, but any other of the pictures used to explain the ideas of the gestalt in psychology would do) varies considerably; some may only see it as a duck, others only as a rabbit; some may take a long time to see the "other" aspect (the one that they did not initially see it as) whereas others can change from duck to rabbit and back again in a flash. In the same way (and again as an example only) the idea of commutativity may be shown to children by showing them six dots – $\begin{matrix} o & o & o \\ o & o & o \end{matrix}$ – and getting them to see that this is both three lots of two, and also two lots of three. Lowe's account cannot do this, because it is so tied to the notion of set.

V Numbers as universals.

John Bigelow, in his [1988], also tries to identify numbers (in fact all mathematical objects) as universals – in particular as recurrences:

> The universals we study in mathematics, I claim, should be conceived as *recurrences*: their distinguishing feature is simply that they play fast and loose with space and time; many of them, for instance, can be found in two places at once.[255]

---

253. The sort of thing I have in mind her is a conditional like: 'If I put this collection of eggs in a one-one correspondence with the notches on this stick, then there will be no remainder.' or 'If I put this collection of eggs in a one-one correspondence with the terms in the learned sequence "1, 2, 3, . . . 22", then there will be no remainder.'

254. Wittgenstein attributes the duck-rabbit to Jastrow, and Chihara attributes it to Kohler. I go along with Chihara as he is almost certainly better read than Wittgenstein; also I supply this figure in section VI below.

255. Bigelow [1988], p.4.

He contrasts this with other (possible) universals which he calls truthmakers.

> The core idea is that of some way of inferring the *existence* of certain things from the *truth* of certain claims: a way of calling things into existence by linguistic magic – *defining* things into existence. I call the guiding idea behind this sort of linguistic magic the Truthmaker principle: when something is true then there must exist certain things which constitute its 'truthmaker'.[256]

This strong attack on linguistically generated truthmakers (Bigelow spends a significant portion of the book in the effort to show that mathematical objects cannot be truthmakers) can be seen as an attack on indispensability arguments. (Indispensability arguments of course are just such arguments which generate entities as conclusions from the premiss that the theory is true.) Bigelow's attack could thus also be seen as a general attack on the project of naturalized epistemology – i.e. the programme of supposing that a given set of objects exists, perhaps because they are (linguistically generated) truthmakers, and then trying to explain how we could form beliefs about such objects.

Bigelow, in contrast with Lowe (and explicitly Russell), specifically rejects the idea that sets are the individuals that instantiate the universals which are numbers.

> For Russell, 3 is a property of the collection, or set, containing Brown, Jones, and Robinson: for me it is a relation among Brown, Jones, and Robinson. They are three: the number three is not instantiated by any one thing, but rather, by any three distinct things.[257]

In fact, for Bigelow, with a strong echo of the structuralists, sets are also to be construed themselves as universals, but of a special sort,

> . . . akin to the universals with which I identify the numbers. I construe a set as specific sort of relation holding among its members.[258]

---

256. Bigelow [1988], p.7.

257. Bigelow [1988], p.5.

258. Bigelow [1988], p.6.

Now, before we go too far here, it is important to point out that Bigelow explicitly does not concern himself with any possible epistemological problems with his account[259], but he does pay lip service to possible ways of solving problems of epistemology for mathematics. Interestingly he claims to have sympathy with 'Quine's epistemology for mathematics'[260], but, as hinted at above, this is strongly at odds with his attack on linguistically generated truthmakers. He favours most of all Kitcher's epistemology (from Kitcher, [1983]) discussed briefly in chapter 3 above.

These preliminaries over, we can now turn to the central contentions of Bigelow's account. Numbers are divided into two groups – natural numbers, and the rest, i.e. rational, real, and complex numbers[261]. The natural numbers are to be construed as relations holding between objects, and the other types of number (rational, real, complex etc.) are to be construed as relations holding between relations; i.e. all of the mathematical edifice is to be built, as it were, out of the natural numbers (just as in the best traditions of mathematical analysis texts). He gives as an example the number three, leading to a definition of a (natural) number n.

> I claim there is indeed something, a universal, which is instantiated by each triple of numerically distinct things. We may call it the relation of *threefold mutual distinctness*. Or, call it the number three.
>
> I claim: any number n is the n-place relation of n-fold mutual distinctness.[262]

We need to understand that for Bigelow all of these things are entirely physical: recurrences are universals which exist both spatially and temporally. Universals may be scattered – they may 'play fast and loose

---

259. See p.3 in Bigelow [1988].

260. On p.4 of Bigelow [1988]; I assume that he is referring to the epistemological edifice built from the foundations of the Quine/Duhem thesis, and the indispensability argument.

261. If we construe for a moment the natural numbers as extending to the integers (it is a large step to include negative numbers and zero, I admit) then there is some mathematical support for making the distinction just here between integers and rational numbers; for the integers only have the structure of an integral domain, but all of the others, the rationals, reals, and complex numbers, all have the richer structure of a field.

262. Bigelow [1988], p.52.

with space and time' – but they are physical. He arrives at this by arguing, initially that properties must exist physically, and then that relations too must also so exist, at the places where they are instantiated. It sounds odd, but he insists that each universal is wholly present everywhere it is instantiated, and also that several universals could be in the same place at the same time.

> When a universal is instantiated by located individuals, then such a universal can, in some sense, be located within regions of space. Of course, we must allow that several different universals may be within the same region at the same time; and that the same universal may be within several different, disconnected regions at the same time.[263]

To justify these strange assertions, Bigelow takes a brief quantum-mechanical diversion through a consideration of the Copenhagen interpretation in an attempt to undermine our preconceptions about locality in general. We are not to worry about the problems that this conception of (real, physical) universals throws up, since the problems with (real, physical) objects are no better.

> . . . the best attitude to take is one of extreme suspicion about all assumptions concerning locality. Among the things there are, it may or may not be possible to draw a sharp line between spatially restrained 'particulars', and spatially promiscuous 'universals'. . . . The question, 'Where is it?' is thus much less straightforward than you might have thought.[264]

If we accept for a moment Bigelow's account of the physicality of universals, and ignore any epistemological problems that we have with this, then another metaphysical problem stands out. What we are being asked to accept is (i) that universals exist physically at the places where they are instantiated; thus (ii) if the physical objects cease to be in some locality (or at the least, since 'ceasing to be' is a tough thing to imagine for a physical object, are radically altered), then the universals instantiated

---

263. Bigelow [1988], p.23.

264. Bigelow [1988], p.25/26.

also cease to be in that locality[265]. We might go along with this so far, but there is another consequence which I will explore with an example – Kohler's "duck-rabbit" again[266]. Imagine seeing the duck-rabbit for the first time, and seeing it as a duck; then there is a picture of a duck in front of you. On Bigelow's account this means not only is there a physical piece of paper with physical lines on it, but also that there is a physical property of 'being a picture of a duck'. Now you suddenly see the duck-rabbit as a rabbit; nothing in the paper has changed, only the intention of your perception, yet on Bigelow's account the universal 'being a picture of a duck' has ceased to be for a while at that location, and the universal 'being a picture of a rabbit' has come into being. What is worrying about this is that this shifting ontology seems to be under the control[267] of your intentional perception.

VI Bigelow, and Lowe's problems.

Does Bigelow's account solve any of the problems that arise with Lowe's account? In some ways their approaches are similar, construing numbers as universals; as we have seen Lowe's Aristotelian universals are sorts, whereas Bigelow's are recurrences – in particular relations. The chief problems encountered by Lowe's approach were:

(i) the difficulty with "exist" being univocal, yet having different modes – i.e. the metaphysical problem with Lowe's Aristotelianism;

(ii) the epistemological problem – inherited from Maddy – of perceiving sets;

(iii) the problem that numbers are only dummy sortals;

(iv) the related "multiplication problem"; and

(v) the problem of zero.

The crucial difference between Lowe's account and Bigelow's is over the place allocated to sets. With Lowe sets are particulars which instantiate the numbers (sorts); for Bigelow sets are on a par with numbers and other

---

265. This is like Aristotle again. Imagine here, for example, a jug which is whole one moment, and smashed the next; then at least some of the universals instantiated in that jug – the property of 'being a jug' for instance – ceases to be in that locality.

266. I come back to this example again, for I claim that it is in fact an important one if we are to understand the problems of perception in mathematics.

267. This assumes, of course, that you are capable of seeing the picture as a duck or a rabbit at will.

mathematical objects – all are relations. Bigelow avoids problems like (i) by being a strict physicalist; in his account "exist" is also univocal, but the price is that existence seems not to be a continuous and smooth thing – his universals are forever coming into, and going out of existence. Bigelow also explicitly denies that numbers are universals instantiated by sets, rather that they are relations among objects, or among other relations; so here he may escape the epistemological problems (ii) faced by Maddy and Lowe, but it is not yet clear whether his physicalist account doesn't generate its own epistemological problems: for example it is not altogether clear, even if a relation is physically in the same place etc. as the objects of that relation, how we come to know about it. (Might we never see the rabbit part of the duck-rabbit?) In fact both Lowe and Bigelow seem to have some difficulties with giving us a clear account of the instantiation process, which for me still remains rather mysterious.

Problem (iii) clearly does not arise for Bigelow, and the crucial advantage of Bigelow's account over Lowe's is in their respective abilities to deal with the multiplication problem (iv). The problems for Lowe stem here from his requirement for the objects of our perception to be sets. Bigelow's account will deal quite happily with both addition and multiplication, and also other parts of mathematics.

Let us return to Lowe's original addition: 2 + 2 = 4; (a) two plus (another) two make a four. For Bigelow we do not need Lowe's inclusion of the bracketed indefinite article, since for Bigelow, the universal is wholly present in each place that it is instantiated; two plus two make four. Imagine then if we have the pair of objects A and B, and we then bring up the pair of objects C and D, then we will have the objects: A B C D. The relation holding between these objects is four; as Bigelow would say, these objects are four. Because Bigelow has not clouded this basic numerical issue with sets, he has avoided, on his own terms, Lowe's problem.

And what about multiplication? Again if we try 'two three are six', taking the objects o o o as three, then two threes would be $\begin{matrix} o & o & o \\ o & o & o \end{matrix}$ and the relation holding between these objects is six; they are six. Again Lowe's problem of identifying the array of two threes as six does not arise, because Bigelow is not trapped into having the threes as *sets* of three objects.

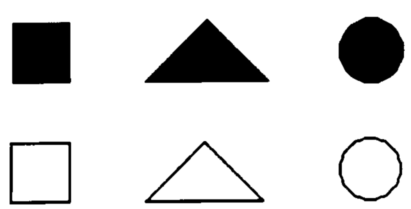
Now it may be objected in these two examples that there is a flaw – for example the array $\begin{matrix} o & o & o \\ o & o & o \end{matrix}$ is not just six, it is also two. In other words the objection is that, on Bigelow's account, the relation "six" holds between the objects within the array, and also at the same time, the relation "two" hold between the objects; how can the objects be both two and six? But this is exactly what Bigelow says is a characteristic of universals (playing fast and loose with space and time), and also it is exactly why his account works! For if we want two threes to *be* six, then we must accept that the same objects can be both two and six. The duck-rabbit can be seen as both a duck, and a rabbit. The following examples are intended to show the strengths of Bigelow's position over Lowe's.

(i) As we have already noticed, the ideas of commutativity and multiplication may be shown to children by showing them six dots

o o o
o o o

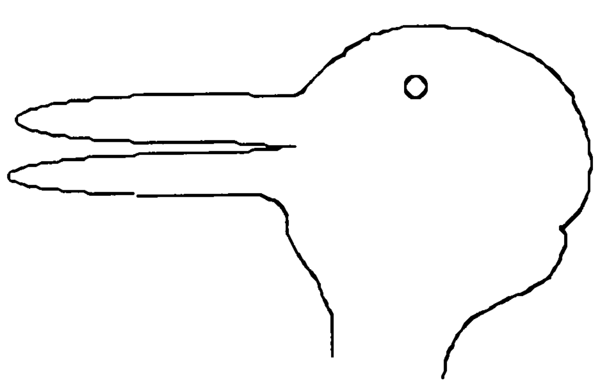
and getting them to see that this is both three lots of two, two lots of three, and also just six. Accounts that rely on the perception of sets foundered on this type of example, simply because the criterion of identity for sets was so strict, and two threes – as sets – could never be (identical with) either a six or three twos.

(ii) The following array is a little like that in (i)



in as much as we can see it as a six, two threes, or three twos. But also we can see it as a two (two colours) or a three (three shapes). If we had no interest in mathematics, we might not register anything at all about this array.

(iii) Kohler's duck-rabbit



provides another example of "seeing-as" – we can see it as a duck or as a rabbit. I claim that looking at the following equation (with a view to its solution for example) is similar in that it can be seen as more than one different type of equation:

$$\frac{12x + 9y}{3x^2} = \frac{6x}{2x - 4y}$$

can be seen as

$$\frac{A}{B} = \frac{C}{D}$$

if we want to cross multiply, or as

$$\frac{ab + ac}{ad} = \frac{ef}{eh + ei}$$

if we wish to cancel. Like the duck-rabbit, it is not guaranteed that everyone will be able to see both of these aspects (or indeed either of them, for the form that it is in in the first place is also an aspect of the equation – the most obvious one).

Finally, returning to the problems with Lowe's account, how does Bigelow's account deal with zero? (Problem (v).) We have seen that Lowe, through Maddy, has a serious problem here – even if we can perceive sets, then surely when we perceive (say) and empty room, then this cannot be taken as a perception of the empty set – it is just no perception at all. Unfortunately Bigelow's account also seems to have a problem here; if numbers are (real physical) relations between objects as he claims, the zero cannot exist – there does not seem to be any way of having a relation between nothing. Indeed he is fairly silent on the whole matter of zero, the best he does is the definition:

> . . . the assertion that there are zero F's is logically equivalent to the *denial* that there is at least one F:
>
> $\sim$ E z.Fz.
>
> And this yields the obvious logical equivalent for 'The number of F's is zero'.[268]

268. Bigelow [1988], p.49.

I remain unconvinced that this does the work that is required of it within his scheme.

VII Some comments on aspectual seeing.

Following the hints in the last sections concerning numbers and the phenomenon of "seeing as"[269], in this section we will look at some background work on perception and aspectual seeing. Firstly we will tackle some background ideas from psychology, and secondly we will look at Barrie Falk's analysis of the phenomenon of aspectual seeing.

One important area of concern for the Gestalt psychologists[270] was the way in which we organise the information received (in our brains) into patterns that help us understand the world; the way in which we impose order and form on our perceptions. These psychologists emphasised that we perceive organized units because of characteristic properties of wholes which cannot be reduced to the properties of their constituent parts. They proposed a set of laws of organisation which are supposed to specify – or at least codify – how we perceive form. Amongst these principles are proximity, similarity, figure-ground, continuity, closure, common fate, set, direction, habit etc. Some examples[271] will suffice:

(i) Proximity. Objects physically close together will be grouped together as units.

e.g. oooooo
oooooo
oooooo
oooooo is seen as four horizontal lines.

(ii) Similarity. Similar objects are perceived to belong together.

---

269. Although it is clearly relevant to my task to consider this problem (of the perception of aspect shifts), I certainly do not propose to tackle this area in any great detail for it is so difficult a problem that to do it any justice would itself be a task greater than the whole of the current project.

270. The term "Gestalt" is from the German meaning form, or pattern. The name Gestalt is applied to those Psychologists from the early 20th century, such as Koffka, Köhler, and Wertheimer, who were interested in this area of psychology. See e.g. Koffka, K. *Principles of Gestalt psychology*, (N.Y.: Harcourt 1935).

271. The examples which follow are based on Miller [1962].

```
e.g.   x  o  x  o  x  o
       x  o  x  o  x  o
       x  o  x  o  x  o
       x  o  x  o  x  o   is seen as six vertical lines.
```

Now, by making one principle of organisation work against another, we can create examples of aspectual seeing.

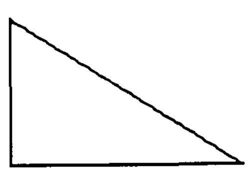
```
e.g.   x o x o x o

       x o x o x o

       x o x o x o

       x o x o x o      is seen as four horizontal lines (by
```

proximity) and as six vertical lines (by similarity).

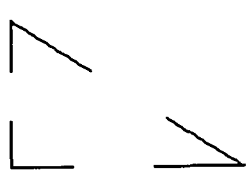
The Gestaltists argued from the *fact* of pattern recognition and the transformation of form – a triangle:

```
x
  x
x    x
x       x
x          x
             x
x  x  x  x  x  x  x
```

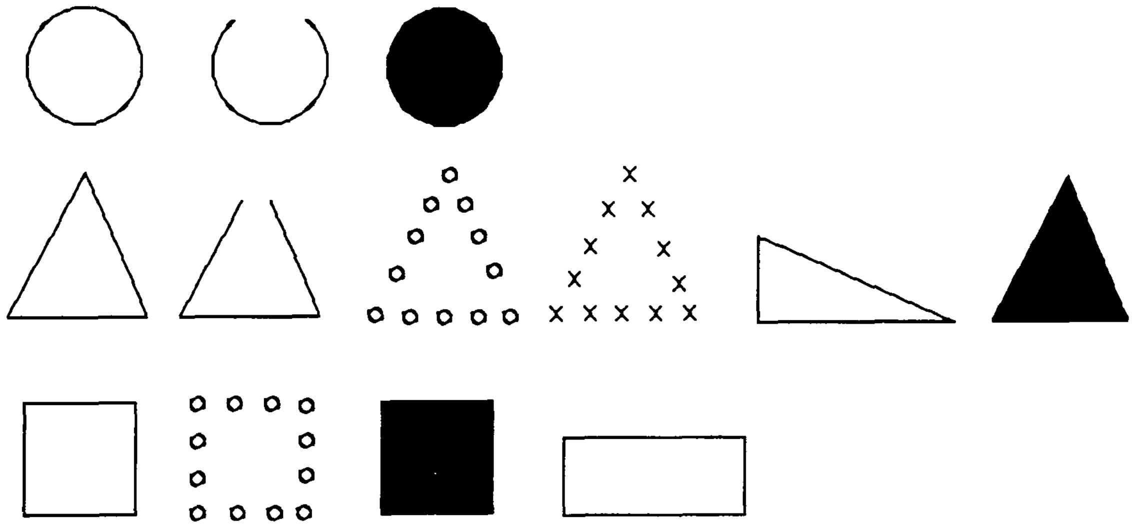
is seen as a triangle:



whatever elements compose it, even:



plus the Gestalt principle of *closure*. But this is far from the whole picture; the Gestaltists certainly had a point in that the whole is important, but it is not just form transposition that is at work here. In 1976 the psychologist A.R. Luria published the results of an experiment conducted across a culturally diverse range of subjects from the USSR[272] – from illiterate Ichkari women from remote regions, through semi-illiterate workers on collective farms, to educated women student teachers - in which the subjects were shown figures such as these:



They were asked to name the shapes and to group them. The literate women students were the only ones to categorize the figures by name, and they could collect together the pictures into correct geometric groups. The illiterate subjects tended to give names to shapes according to whatever everyday (for them) objects they resembled – a watch, plate, or moon for circular shapes etc. – and grouped the shapes according to their similarity with such objects[273].

> They perceived the figures as similar to objects in their environment and classified them accordingly. 'No, they cannot be alike,' one peasant said, 'because the first is a coin and the second a moon'.[274]

Thus the educated subjects, those schooled in simple geometric concepts, tended to apply the Gestalt laws of organisation, whereas those ignorant of geometrical categories tended to group the given figures according to some

---

272. See Luria, A.R. [1976], *Cognitive Development: Its Cultural and Social Foundations*, (Harvard, 1976). Also, Luria, A.R. [1979], *The Making of the Mind: A Personal Account of Soviet Psychology*, (Harvard, 1979).

273. For example the square with dots might be classified as a watch along with some of the circles, since it resembles a watch with hour dots, but the solid square would be classified differently.

274. Luria [1979], p.63.

other rules, but still using some higher conceptual apparatus than simple sense perception. So a triangle is more than just the sum of its parts, in the sense that a triangle can only be in some way an intentional object for us if we have some geometric education. What is interesting here is that it is clear that the perception of the figures is being processed not only in a bottom-up fashion, i.e. from the basic sensory experience, but also in a top-down manner, i.e. organized by using concepts already held by the subjects[275]. It is not that the Gestalt laws are in fact universal (for surely Luria's experiment suggests that they are not) but rather that perception involves some type of top-down processing of which the Gestalt laws are only a type.

(We might like to note that Wittgenstein criticized the Gestalt psychologists for they believed that the organisation of a perception was an original feature – a phenomenal feature like colour – of what is perceived, rather than something that has to be learned in some way.

> If you put the 'organization' of a visual impression on a level with colours and shapes, you are proceeding from the idea of the visual impressions as an inner object. Of course this makes this object into a chimera; a queerly shifting construction.[276]

More than this however, it strikes me that Luria's results strongly favour the idea that organization is a top-down cognitive process, rather than a bottom-up one.)

Barrie Falk in his [1993] introduces us to what he argues is a 'sophisticated kind of phenomenal experience', and he coins the term 's-phenomenal' for this. The s-phenomenal state is something which is produced jointly by the bottom-up processing of our perceptual input, along with the top-down processing of that input using higher level cognition. What is different in Falk's s-phenomenal state, as compared with ordinary phenomenal states such experiencing colours etc. – which only employ bottom-up processing – is that they require higher level, top-down, cognitive processing. What is different in Falk's s-phenomenal state, as compared with other high level judgements about how the world

---

275. This distinction between bottom-up and top-down processing, in connection with the "seeing as" phenomenon, is discussed below in connection with Barrie Falk's recent work.

276. Wittgenstein [1953], p.196.

is – which are caused by top-down cognitive processes – is that they are phenomenal, or phenomenal-like, experiences.

> These concepts have the distinctive mark of being concept of *phenomenal* properties. Like colour concepts, they give expression to a way things are with us experientially when we survey the object which is not the inferential product of, and cannot be justified in terms of, any further experiential component of the scene. On the other hand they are unlike colour concepts in being dependent on high-level processing. Figures will look poised, precarious and so on only *because* one has given them a particular orientation or grouped and assigned a 'common fate' to their parts in a particular way.[277]

Falk reaches his notion of the s-phenomenal by considering at first 'a traditional way of thinking about our cognitive relation to the world'[278], stemming ultimately from Locke and Hume, which he terms the *processing paradigm*. In this model,

> The world, acting upon the sense organs, produces states which are 'present' to the mind . . . The presence of these sensory states consists in this. First, they are something *of* which we are conscious. Their being so enables us to perform various operations upon them. . . In virtue of this capacity, we are in a position to make higher level judgements about how the world more generally must be, given that it presents this immediate appearance.[279]

Falk argues that there is an important distinction to be made

> between, on the one hand, our judgements about how the world is and, on the other hand, encountering or being presented with the world which is the topic of those judgements.[280]

What Falk means by 'encountering . . . the world' in this paradigm, is not what we now might call having sense data, rather:

---

277. Falk [1993], p.67, and note the use of the Gestaltist notion of 'common fate' here.

278. Falk [1993], p.55.

279. Falk [1993], p.55.

280. Falk [1993], p.56.

> . . . this is what our first encounter with the world is like – it is perceptually given before conceptualization and provide the material on which the conceptualizing works.[281]

So the processing paradigm rests on an acceptance of the bottom-up processing, and top-down processing distinction. The mistake made in the paradigm, so argues Falk, lies in the conflation of the idea of our encounters with the world about which we make judgements, with the idea of our actual sensory experience. How does this work? It appears at first blush as if Falk is merely accepting the same distinction that is implicit in his formulation of the processing paradigm given above; but this assessment would be to fall into the very trap about which he is warning us. For Falk is forcing a wedge between our sensory experience, and our encounters with the world – when we encounter the world, we are not 'being presented with the world as a kind of low-level cognition'[282]. So Falk's s-phenomenal conception stands between the basic (bottom-up processed) experience of the world, and the judgements (top-down processed) that we infer about the world.[283]

If Falk is not to be merely begging the question as to how we do in fact encounter the world, then he must produce some arguments or examples to convince us that such encounters are in some sense more than 'low-level cognition'. Falk uses examples of aspectual seeing to argue this case – he claims that although the processing paradigm is compatible with many of the features of aspectual seeing – "seeing as" – it is not compatible with all features. (In many respects he has some fairly good backing from current empirical psychology, as the discussion of the Gestalt principles and Luria's investigations, above, demonstrate.) This area – the seeing of aspects – is of course the one in which we are interested, but for Falk the s-phenomenal conception illuminates more than just the cases of aspectual seeing.

---

281. Falk [1993], p.56.

282. Falk [1993], p.57.

283. See footnote 291 below for a possible counter-example to Falk's suggestion here that his s-phenomenal state lies neatly between the basic phenomenal experience, and the higher level judgements.

Now, a crucial point for Falk is that aspectual seeing – the changing of the aspect – cannot be due either to what is seen, or what thought accompanies what is seen:

> Suppose, to take one of the standard examples, that I see the array of lines before me as a drawing of a duck; and suppose that, although I know that it can also be seen as a drawing of a rabbit, I cannot at present manage to do so. Now when I do manage it, the change that occurs will not be in what is sensorily presented to me, since that has remained the same; nor will it be in what thought accompanies the sensory input, since the thought that this could be seen as a rabbit was present earlier and was not sufficient for the change.[284]

In effect Falk is suggesting that there must be something else: Falk's suggestion is that there is something in the way that the concept, or thought, 'attaches itself' to the sensory input. Now he argues that it could not be a phenomenal experience like seeing a colour, for the aspect change depends on thought – 'on one's being master of and now deploying a technique'. Nor, he claims, could it 'consist in nothing but the fact that one deploys the technique', since it is a consequence of such a deployment of a technique.

What Falk wants to suggest is that to see an aspect of a picture – for example seeing the duck-rabbit as a duck – is to understand not only the whole, but also the inter-relations of the parts (beak, neck, . . .), thus both the top-down and bottom-up processing of the sensory data must be acting together.

> . . . it is reasonable to suppose that when an aspect shift occurs it will be a mixture of top-down and bottom-up processing, as between response to the whole figure and to its components. . . This process is sufficient, I submit, to explain the difference between merely thinking that the figure can be seen in some other way, and actually being able to do so.[285]

So seeing an aspect – or experiencing and aspect shift – is caused by the complex interaction of the bottom-up processing of the parts and their

---

284. Falk [1993], p.57.

285. Falk [1993], p.60.

relation with the top-down processed whole, so that the thought becomes 'attached' to the figure, i.e. to the sensory input, in just the right way.

This much, Falk suggests is compatible with the processing paradigm; he wants to claim that there is in fact more, and also that Wittgenstein thought that there was more. Thus if he is right, then he successfully argues against the paradigm – which is one of the points of his attack. For us, the attack on the so-called processing paradigm is less important, rather it is the discussion of the aspect shift phenomenon. Falk's suggestion for the extra component in the aspect shift phenomenon is that there is a physical part played – a 'bodily contribution'. One looks right or left with the duck-rabbit depending on which aspect one is attending to; the bodily contribution is a high level process, part of our intentional perception, and Falk wants to claim that it may have affective properties which are not a consequence of the interaction with the perceived object, rather part of the perception.

> Locating an object and its parts involves a bodily contribution. Therefore, being visually impressed by it (i.e. internally relating it to, among other things, a set of possible movements) involves imaginatively anticipating the bodily adjustments correlated with the internally related movements. This bodily constituent of the impression may have certain affective properties – there may be reactions to the scene thus imaginatively interacted with, the imagined movement may be difficult or easy and so on.[286]

Falk offers no mathematical examples, but a brief discussion of such in the light of his comments on s-phenomenal states would clearly be of benefit. Firstly consider the array of dots[287] $\begin{smallmatrix} o & o & o \\ o & o & o \end{smallmatrix}$ which we may see as a six, a three, a two, or a one. Seeing it (for example) as a three – three lots of $\begin{smallmatrix} o \\ o \end{smallmatrix}$ involves the top-down processing of the $\begin{smallmatrix} o \\ o \end{smallmatrix}$ into a single intentional object (an *intentional* object because I might say: 'I mean it is three of *these*'), and then seeing it as a three involves the top-down processing of noticing that these objects – $\begin{smallmatrix} o \\ o \end{smallmatrix}$ – are in one of the "standard" forms or patterns of a three (vis: **x x x**; another example might be $\begin{smallmatrix} & \mathbf{x} & \\ \mathbf{x} & & \mathbf{x} \end{smallmatrix}$).

---

286. Falk [1993], p.66.

287. From section VI above.

Here we can note that for small numbers, the perception of the array as (for example) a three is due to subitizing[288], and tends to be dependent on the form of the array, as in the examples above. What is interesting with numbers is that the array, for odd "formless" arrays or for large arrays, can be counted, thus classifying the array as a twenty-seven, or a three-hundred-and-nineteen. Some further discussion on this is relevant here.

It is reasonably uncontentious that we do obtain the cardinality information from a collection by employing more than one technique, in particular, we can subitize, or count the collection. In subitizing, one mechanism (though not necessarily the only one) employed is the recognition of various canonical forms for various numbers, akin to the recognition of geometric shapes. For example the triangle

```
 o
o o
```

for three; the square

```
o o
o o
```

for four; the pentacle

```
o   o
  o
o   o
```

for five, and so on. (Think about how you know the result of a roll of an ordinary die – you subitize!) At some point however counting must take over, and possibly other mechanisms too[289].

> Infant, adult and animal evidence suggests the existence of several quantification processes specific to the apprehension of numerosity. Children and adults rely mostly on verbal counting. However, small numerosities can also be rapidly subitized, and large numerosities can be approximately estimated. . . . The intuitive symbolic-processing model of human numerical cognition must therefore be supplemented with a number of dedicated non-verbal quantification processes.[290]

Consider the complex collection:

288. See above in chapter 6, and also Dehaene [1991]. The recognition of (for example) a three by recognising a pattern is part of what is termed "subitizing".

289. A full discussion can be found in Dehaene [1991], p.1-19.

290. Dehaene [1991], p.19.

```
o o           o o
o o           o o
       o o
       o o
o o           o o
o o           o o
```

which pentacular form we recognize immediately (if we are schooled in the right way – see the comments on A.R. Luria above) as a five; as we also recognize the collection individuals as squares – i.e. as fours – then we can also come to see the whole collection as a twenty. But note the following however: (i) it is hard to see this collection as anything other than a one, a five, or a twenty – in particular we do not so obviously see it as a four, although this can be done by seeing it as four large pentacles not quite superimposed; (ii) the seeing of the collection as a twenty, having first and most obviously seen it as a five, is not s-phenomenal, for it is not a phenomenal experience at all, rather it is like Falk's 'judgements about the world' – it is an inference from the (phenomenal) apprehension of the forms of the five and the four.[291]

A final point here, which we can take from Falk, is the idea of the 'bodily contribution' to the perception, for when we see a collection as instantiating a number, then we know how to do something – how to divide it up in some way, how to correlate it with another collection, and so on. But this is not restricted to just numerical examples, and the relationship between "seeing as" and some sort of bodily contribution can be noticed in other areas of mathematics – consider the algebraic examples from section VI above, vis:

$$\frac{12x + 9y}{3x^2} = \frac{6x}{2x - 4y}$$

can be seen as

$$\frac{A}{B} = \frac{C}{D}$$

if we want to cross multiply, or as

291. We see here at least one candidate for a counter-example to Falk's claim that his s-phenomenal concept will explain aspectual changes, for there is no clear delineation between this example, and any smaller cardinality examples such as the six seen as a three or a two discussed above, which does seem to be s-phenomenal in Falk's terms. In any case nothing of importance to this current project turns on whether the seeing of the collection as a twenty is or is not a phenomenal experience.

$$\frac{ab + ac}{ad} = \frac{ef}{eh + ei}$$

if we wish to cancel. So the bodily contribution in the seeing-as concept in mathematics seems to me to be very much related to some notion of 'knowing how to do something'.

The idea that mathematics somehow fundamentally, or ultimately even, relates to being able to do something (in the world; in mathematics) is found in Philip Kitcher's novel approach to mathematical epistemology, and this is where we turn to next.

8. KITCHER AND ARMSTRONG: NEW WAYS FORWARD ?

I Kitcher's idealizing theory.

As we saw briefly in Chapter 4, Philip Kitcher in his [1984] presents us with a radically different approach to the problems of mathematical knowledge. Statements of mathematics are to be understood as statements which tell us something about the possible actions of some ideal subject. But this is not all of his concern: having explained what he believes mathematics to be fundamentally about, Kitcher goes on to develop the other main strand in his book: an account of how it grows – his historical account of mathematical knowledge.

> . . . the knowledge of an individual is grounded in the knowledge of community authorities. The knowledge of the authorities of later communities is grounded in the knowledge of the authorities of earlier communities. . . . we can envisage the mathematical knowledge of someone at the present day to be explained by reference to a chain of prior knowers.[292]

Kitcher calls the theory of mathematical knowledge that he develops an 'evolutionary theory of mathematical knowledge'[293], because it describes how mathematical knowledge has evolved and passed down to us, from the basic knowledge of how to do things - essentially mathematical activities - in the world. (We can include here such activities as individuation; forming simple collections; comparing collections; etc.)

My purpose in this section is to look at how, starting from his position that 'most people know some mathematics and some people know a large amount of mathematics'[294], Kitcher arrives at his evolutionary theory of mathematical knowledge and his thesis about 'ideal operations performed by ideal agents'[295]. His account has something in common with Falk's thesis on the s-phenomenal and aspectual seeing,

---

292. Kitcher [1984], p.5.

293. Kitcher [1984], p.92.

294. Kitcher [1984], p.3.

295. Kitcher [1984], p.109.

in the sense that both stress the idea of the possibility of physical action in some sense: for Falk, the s-phenomenal state requires the possibility of some physical involvement, or 'bodily contribution'; for Kitcher, fundamental mathematical statements tell us about the possibility of certain actions. The relation between aspectual seeing and mathematics, plus the insights of Kitcher and Falk as to the role of possible actions in these areas, are, I shall claim, amongst the most important insights.

Kitcher starts with epistemology, arguing that any adequate account of knowledge must be a psychologistic account, rather than an apsychologistic account. Psychologistic accounts of knowledge take knowledge as true belief which has been produced by some appropriate process that includes some psychological element or event – something like psychological causation:

> The difference between an item of knowledge and mere true belief turns on the factors which produced the belief – thus the issue revolves around the way in which a particular mental state was generated. . . . the approach is appropriately called 'psychologistic' in that it focuses on processes which produce belief, processes which will always contain, at their latter end, psychological events.[296]

Most causal theories of knowledge will thus be psychologistic. Apsychologistic accounts of knowledge also take knowledge as true belief, but the feature here which distinguishes knowledge from true belief is that the belief must be warranted, or supported, by other beliefs of the subject; the warrant or support here is taken to be a logical relation between the beliefs rather than a causal relation.

> . . . an apsychologistic approach to knowledge [is] an approach which proposes that knowledge is differentiated from true belief in ways which are independent of the causal antecedents of a subject's states. . . . proponents of this view of knowledge will emphasize that the criterion is to be given in *logical* terms. We are concerned with logical relations among propositions, not with psychological relations among mental states.[297]

---

296. Kitcher [1984], p.13.

297. Kitcher [1984], p.14.

Kitcher rejects this apsychologistic approach to knowledge, and his reasoning is closely tied to his subsequent attack on the a priori in mathematics. His contention is that (at least some) apriorism comes from an 'acceptance of an apsychologistic epistemology'[298], thus he endeavours to undermine such epistemologies. His tactic is to provide counterexamples to the apsychologistic epistemologists, in which cases their conditions for knowledge are met (but crucially the psychologistic conditions are *not* met) but which we intuitively would claim not to be cases of knowledge[299].

Chihara, in his [1990], has produced two criticisms of Kitcher's adoption of his psychologistic account of knowledge. The first hinges on Kitcher's tacit acceptance that any account worth its salt must define knowledge as true belief:

> . . . Kitcher is willing to consider only analyses of knowledge according to which X knows that P iff X believes that P, P is true, and . . . , where the '. . .' is to be filled in with some condition or conditions. I am not convinced that we should restrict ourselves to analyses of this sort.[300]

But here, unless Chihara has other more definite suggestions to make (other than the sketches which he does in fact give), then I think that this rather negative criticism gets us nowhere further along. His second criticism is more useful however; he attacks the idea that, for (specifically) mathematical knowledge, the true belief has to be produced in the "correct" (i.e. psychologistic way). For example, he asks us to consider the (science fictional) case of Tom, who undergoes surgery in the year AD 2500, after which he believes some mathematical theorem, can prove it and explain its significance etc. Chihara suggests that this would surely be a case of apsychologistic mathematical knowledge. Now I don't think that Chihara needs such an outlandish example to make his point here, for he could quite easily make a similar point with cases such as that of Ramanujan which are well documented. In any case the point at issue is clear: Kitcher wants to reject the notion that mathematics is a priori, and hence he requires a strongly psychologistic account of knowledge; on the

---

298. Kitcher [1984], p.14.

299. The details are given in Kitcher [1984], p.15-17.

300. Chihara [1990], p.225.

other hand, Chihara's criticism shows us that, if we are to accept that mathematics is in fact a case of a priori knowledge, then we could more easily accept an apsychologistic account of knowledge – at least of specifically mathematical knowledge[301].

A more difficult ad hominem argument against Kitcher's overall programme, would be to query the coherence of his attack on apsychologistic epistemology (and his championing of psychologistic epistemology), whilst at the same time not dealing the the inherent epistemological problems of having mathematical knowledge as being something ideal, known by ideal agents.

We leave aside these concerns from Chihara, and accept, perhaps reasonably, that in fact the debate between (Kitcher's characterisation of) the psychologistic theorists and the apsychologistic theorists (at least with respect to specifically mathematical knowledge) is irretrievably linked with the problem of the apriorism, or otherwise, of mathematics. Kitcher's discussion in fact ends up with him defining knowledge thus:

> There is a simple normal form for a psychologistic account of knowledge. We may introduce the term 'warrant' to refer to those processes which produce belief "in the right way" proposing the equivalence
>
> (1) X knows that p if and only if p and X believes that p and X's belief that p was produced by a process which is a warrant for it.[302]

Kitcher warns that the conditions on warrants need specification, and also that the process which makes up a warrant is a token process, rather than a process type.

Now, as mentioned above, Kitcher wants to oppose the idea that mathematical knowledge is a priori knowledge, which he defines in a similar way:

> (2) X knows a priori that p if and only if X knows that p and X's belief that p was produced by a process which is an a priori warrant for it.[303]

---

301. Note that Chihara misses the opportunity to approach his critique of Kitcher in these terms, since he specifically states that Kitcher's views on the status of the a priori are 'not directly relevant to my concerns in this chapter.' – Chihara [1990], p.219.

302. Kitcher [1984], p.17.

303. Kitcher [1984], p.24.

Now whereas in his definition of knowledge Kitcher explicitly shied away from any commitment to specific conditions on warrants, here he puts forward a clear analysis of a priori warrants:

> (3) α is an a priori warrant for X's belief that p if and only if α is a process such that, given any life *e*, sufficient for X for p,
>
> (a) some process of the same type could produce in X a belief that p
>
> (b) if a process of the same type were to produce in X a belief that p, then it would warrant X in believing that p
>
> (c) if a process of the same type were to produce in X a belief that p, then p.[304]

Kitcher goes on to argue that mathematical knowledge cannot be a priori knowledge, and I summarise his argument now.

Kitcher starts by setting up what he takes to be the essential structure of any a priorist programme, which, he claims, is tied to the notion of proof.

> The a priorist typically claims that all known statements of mathematics can be known a priori by following proofs. To defend this claim, one must defend some thesis of the following form:
>
> (4) There is a class of statements A and a class of rules of inference R such that
>
> (a) each member of A is a basic a priori statement
>
> (b) each member of R is an apriority-preserving rule
>
> (c) each statement of standard mathematics occurs as the last member of a sequence, all of whose members either belong to A or come from previous members in accordance with some rule in R.[305]

Using this apparatus, Kitcher launches two attacks against mathematical a priorism, one against the whole sense of (4) above, and another against claims of the form (4a) in particular.

The first of these criticisms is based on the fact that many mathematical proofs are long and difficult; that they sometimes need

---

304. Kitcher [1984], p.24.

305. Kitcher [1984], p.39.

revision. Thus Kitcher can claim (and perhaps rightly so) that such long proofs do not yield the certainty of the mathematical result – mathematical knowledge is not then certain knowledge. But Kitcher here seems to be employing far too strong a notion of knowledge[306] and is expecting far too much of the a priori; Armstrong has called this type of a priorism the "Cartesian" a priori[307].

The second attack Kitcher claims to be the more important; it is a direct attack on the possibility of our having basic a priori knowledge of mathematical truths (claim (4a) above):

> . . . I shall try to expose a more serious weakness. Mathematical apriorism is committed to the thesis that we have (or can have) basic a priori knowledge of mathematical truths, and I shall charge that there is no prospect of an explanation of how this is possible.[308]

Kitcher's approach here is to show that any possible apriorist will not be able to supply adequate warrant for their claimed a priori mathematical knowledge; for those warrants will never be able to satisfy the standards required of a priori warrants given his definition (3) above. Kitcher does this by dividing the mathematical a priorists into three groups. Firstly he splits off those whom he calls the *conceptualists* – this group take mathematical knowledge as ultimately deriving from our understanding of mathematical statements. The rest (who clearly accept mathematical knowledge as deriving from knowledge of some realm of mathematical entities) are further divided into what he terms the *constructivists*, and the *realists*. The constructivists class mathematical entities as mind dependent, and thus mathematical knowledge as being a construct of our minds. The realists on the other hand class mathematical knowledge to be knowledge about some mind-independent reality.

Now Kitcher spends a full two chapters of his [1984] in attacking these three positions for their mathematical apriorism, and I do not

---

306. One is put in mind here of Descartes' method of doubt and his attempt to achieve certain, or even incorrigible, knowledge.

307. '. . . the notion of the a priori carries a theoretical loading derived from past centuries, a loading that is objectionable. But this loading can be removed without any great difficulty, leaving a workable concept of the a priori. . . . The loadings that need to be removed . . . are the notions of certainty (a fortiori, the Cartesian notion of indubitable or incorrigible certainty) and knowledge.' – Armstrong [1989], p.119.

308. Kitcher [1984], p.46.

propose to explore the details of his arguments. Rather, I would point out again (with Armstrong – see below) that Kitcher is employing too strong a notion of the a priori – a notion (as Armstrong would say) with too great a 'theoretical loading derived from past centuries'. To paraphrase Hunter's terse comment (quoted from his [1994] in chapter 4 above), it is a great deal more certain that we do have a priori mathematical knowledge than that Kitcher has an adequate and acceptable account of the a priori.

So where in Kitcher's (seemingly most reasonable) definition of a priori warrants ((3) above) do thing's go wrong? I do not hope to provide a full analysis here, but looking at (3), it seems to me that condition (3b) is rather too strong: what it appears to be implying is some sort of certainty condition, and this is just the theoretical loading which we don't want. Evidence for this comes from Kitcher himself; he explicitly claims that all the positions for apriorism fail this particular condition (3b) on a priori warrants: long proofs and their associated problems fail (p.43)[309]; constuctivists fail (p.52, p.54); and conceptualists fail (p.85). Thus the argument between Kitcher and the apriorists is now seen to turn on the notion of the a priori, Kitcher[310] taking a strong view of a priori warrants, and others (such as Armstrong as we have mentioned, but also Kripke for example) taking an alternative conception.

Now, leaving aside the problem of the apriorism of mathematical knowledge, and accepting that we do in fact have mathematical knowledge (Kitcher's own starting point, but remembering that for him, such knowledge cannot be a priori knowledge), Kitcher introduces his evolutionary theory to account for this knowledge. He is, however, left with three crucial questions:

A From where did the first mathematical knowledge get its warrant? (i.e. how did the first mathematical knowledge start?)

B How does the warrant get transfered from generation to generation? and

C How do we add to our inherited mathematical knowledge?

Kitcher employs two "historical" models – the Kripke-Putnam causal chain theory of reference, and the Kuhnian notion of revolutionary scientific change – to help him tackle questions B and C; it is not really

---

309. All page references here to Kitcher [1984].

310. Patrick Suppes also takes such a strong view; see his *Probabilistic Metaphysics*, (Oxford, Blackwell, 1984).

within the scope of the current work, nor relevant to it, to tackle these issues here.[311]

In answer to A, Kitcher suggests that the initial mathematical knowledge was rudimentary mathematical knowledge obtained from perception. We need to remember that Kitcher is an anti-realist, and thus he cannot mean that we perceive mathematical objects forming, as it were, mathematical facts in some sense, as can Gödel et al. Rather, he maintains that mathematics has its roots in the bare and basic human activities of collecting and correlating. His claim is that mathematics springs from the basic idea that we can collect physical objects (all of the red objects here, and all of the blue ones there, say) – and notice that this activity presupposes that we can individuate objects – and can correlate them in various ways (we may correlate the collected sheep in a pen with the notches on a wolf-bone). Kitcher's thesis is that from these basic physical operations, we learn to do such collecting and correlating in our heads, and ultimately, symbolically.

Thus ultimately Kitcher's account must square with a basic fact of mathematics, which any adequate account must fit – simple counting. Counting, having collected some objects (physically, or just "in the head") is forming a one-one correspondence between – i.e. correlating – that collection, and a learned by heart sequence that can be continued indefinitely. This sequence is itself a mental collection. I claim that counting in this sophisticated manner (comparing with a learned sequence) is a higher level activity than just the comparison of two simple collections, but is ultimately no different in type.

II Armstrong's combinatorial naturalism.

D.M. Armstrong's [1989] is an attempt to give an account of possibility within a strongly naturalistic framework. For Armstrong:

> The term 'Naturalism' is often used rather vaguely, but I shall understand by it the doctrine that nothing at all exists except the single world of space and time. So my objective is to give an account of possibility which is in no way other-worldly.[312]

---

311. These issues are, however, well dealt with in Chihara [1990], chapter 11.

312. Armstrong [1989], p.3.

His target is just those other-worldly theories of possibility that are non-naturalistic, in particular the possible world semantics of David Lewis, and he champions in their place 'a new version of the metaphysic of *Logical Atomism*'[313]. The background to Armstrong's position with respect to mathematics is then a strongly physicalist one, bound into a version of logical atomism which, though similar to Russell's (and others), does not carry the burden of semantic and epistemic atomism.

With this background, and almost as an aside from his main task in his book, Armstrong's chapter on mathematics (chapter 9) attacks firstly Kitcher's attempt to show the non-apriorism of mathematics (and similarly Patrick Suppes's), and secondly any subsequent mathematical empiricism. The attack on Kitcher is based on the argument – as argued above – that Kitcher' notion of the a priori is too strong, or too "Cartesian" (see footnote 307 above): Kitcher wants a priori knowledge to be certain; Armstrong is happy to allow a priori knowledge to be as corrigible as empirical knowledge. Following on from this, Armstrong characterizes modern empiricists (who is he thinking of here?) who claim that once the Cartesian a priori is abandoned, then there is no distinction left between science and mathematics. But Armstrong does not himself want to follow this path; the argument which leads to the acceptance of mathematics as an empirical science is, he claims, flawed.

If we first distinguish between mathematical results and mathematical premisses, and to consider the types of premisses that are used in mathematics. Armstrong's characterization of the argument claims that premisses split into two classes which do no more than overlap.

> First, there are the epistemically original premisses, those propositions from which mathematical investigation begins. Second, there are those axioms which mathematicians use as axioms for their deductive systems.[314]

Now, noticing that the second class of premisses here are normally chosen because of their consequences – their ability to produce the right (known) results, including the epistemically original premisses – rather than

313. Armstrong [1989], p.ix.

314. Armstrong [1989], p.120.

because of any inherent "obviousness", the empiricists can claim that the first class of premisses are just truths derived from observation, and that axioms are like the laws of science. So just as science deduces results from the laws of its theories, then so mathematics deduces results from its axioms. The flaw in this is clear, claims Armstrong, and due to a serious disanalogy between the cases.

> . . . to draw out this comparison is to see that it fails. When deductions are made from postulations in natural science, the process is not ended but has only begun. The essential further step is to check by observation (interlaced with theory) whether the matters deduced are or are not so. . . Nothing like this occurs in mathematics. There it is the deductions that need to be checked, not what is deduced nor what it is deduced from.[315]

Armstrong finally asks how it is that we have so much a priori mathematical knowledge, and, brushing aside both Field's solution (that mathematics is not true, so that the question disappears) as 'desperate', and the possibility that mathematical knowledge is innate, he concludes that the best explanation for our having a priori mathematical knowledge is just that it is analytic. So for Armstrong, with his logical atomism and naturalistic stance, mathematics is both a priori (with a suitably altered and purged sense of the a priori) and analytic.

Having tackled the initial problems of mathematical knowledge, Armstrong goes on to the problems of mathematical entities. He needs still to describe what sort of things the objects of mathematics are, about which we have this a priori knowledge; also, he must (given that he is a naturalist in the sense that he believes the physical world is all that there is) tackle the particular problem that some mathematical entities might exceed the natural world – for example the infinite sequence of infinite cardinals. He solves this second problem with the technical apparatus of his combinatorial theory.

> It seems to me that the Combinatorial theory, as developed in this essay, contains within itself the resources to solve this difficulty. The theory allows not merely for the recombination of wholly distinct elements of the actual world, but also for the contraction of, and expansion beyond the

---

315. Armstrong [1989], p.121.

> actual. The expansion does not extend to alien universals, but it does allow for the idefinite multiplication of individuals in a single possible world.[316]

The details here of Armstrong's combinatorial theory, and thus his consequent success in the evasion of this problem, are not important for us, but the details of his conception of numbers, as mathematical entities, is of prime importance.

As mentioned briefly in chapter 3, Armstrong's conception of number is that of a relation, rather like Bigelow's. For Armstrong, once some 'unit-property' (a universal) is defined, then the number of such units in a collection (another universal, the n-object aggregate) is the ratio, or proportion, that the n-object aggregate bears to the unit-property.

> For the case of natural numbers, unit-properties are universals of the same sort as their corresponding aggregate-universal, but the aggregate-universal will bear a positive whole number ratio to its unit-property. For the universal *being a nineteen-electron aggregate* the (salient) unit-property is *being an electron*, and for *being a nineteen-proton aggregate* the (salient) unit-property is *being a proton*. Each of these pairs of universals, and innumerable other pairs of universals, bears the same (internal) relation to the others. The relation is a *ratio* or *proportion*. It is a plausible candidate, I suggest, for the natural number 19.[317]

Now what is different from Bigelow's account here is, not only that the relation of proportion bears between universals (n-object aggregate; unit-property) rather than between individuals[318], but also that the same account holds for natural numbers as well as for rationals and reals etc.[319] and Armstrong contrasts this with neo-Fregean accounts of natural numbers which will not generalize to the rationals and the reals. To see that Armstrong's own account does so generalize, he invites us to

---

316. Armstrong [1989], p.125.

317. Armstrong [1989], p.128.

318. Remember that for Bigelow, the natural numbers are relations between individuals, and the rationals and reals etc. are relations between relations.

319. It would be of some interest to know if it extends in an easy way to complex numbers, as I suspect that Bigelow's does, since it seems, prima facie at least, to draw a line here. Yet as I have claimed before, if there is a line to be drawn anywhere, the mathematician would draw it between the integers and the rest – as Bigelow does. (At least, Bigelow draws the line between the natural numbers and the rest. which, I claim, is much the same thing.)

consider, as an example, that *being a one kilogram mass* as being a unit property; then 2.5 kilograms of some substance will stand in the 2.5 ratio to this unit property in exactly the same way as in the example above, or even, so he claims, π kilograms of some substance will stand in the π ratio to this unit property.

Despite the claim that his account works for all types of numbers, Armstrong does spend some effort in trying to show why natural numbers are different – that the unit property divides up the aggregate which instantiates the aggregating property without remainder. What is good about this account here is that he can deal with the cases we have considered before[320], such as how to account for seeing six objects as a six, a three, a two, or even a one.

> . . . the unit-property may operate on the aggregate that instantiates the aggregating property to produce (without remainder) *more* than one set whose members each instantiate the unit-property. If the aggregating property is *made up of eighteen electrons* (= *made up of just nine two-electron aggregates*) and the unit-property and the unit-property is *made up of just two electrons*, then it is easy to see that the unit-property as it were carves up the aggregating property to give a large number of different sets.[321]

It strikes me however that, despite his protestations to the contrary, Armstrong has not shown why the (specifically) natural numbers are different from the rest (as Bigelow claims, and with which I agree). For all he has shown is that in 'carving up without remainder' is what separates whole real numbers from other real numbers: i.e. he gives us a way of characterizing whole numbers within the set of reals. I contend (with Bigelow) that there is more to the natural numbers taken as themselves (and not embeded in the reals) as counting numbers, than Armstrong's characterization allows. For example we could consider prime numbers – primeness being just one of the properties that is specific to some of the natural numbers. Now, Armstrong's characterization might allow him to define prime numbers (which he does not specifically do), for he could suggest that a number is prime when there are exactly two possible "carve-ups" of the aggregating property by any unit-property which leave no

---

320. See, for example, section IV in the last chapter.

321. Armstrong [1989], p.131.

remainder. For example a seven-electron aggregate would be carved up without remainder by the unit-property *being an electron*, and also by the unit-property *being just seven electrons*; hence seven would be a prime number. But why should we stick to these unit properties? Could we not say that a seven-electron aggregate could be carved up without remainder by the unit-property *being just two-and-one-third electrons*, or by the unit-property *being just seven-tenths of an electron*, and so on. The very thing which Armstrong claims is good about his account – that the rationals and reals can be dealt with in exactly the same way as the natural numbers – is the very thing which I would claim is a weakness, for the natural numbers are fundamentally different from the rationals and reals in a more important way than Armstrong allows.[322]

Armstrong does not leave mathematical objects with just the numbers – including specifically zero and one, but also (like Bigelow) he goes on to consider sets, and he also specifically deals with higher order sets, ordered sets, and the null set. Now as we have seen above in chapters VI and VII, the zero concept was something of a problem for first Maddy, and then also Lowe and Bigelow, so what does Armstrong actually suggest?

> Given a unit-property and given an aggregating property to which the unit property *lacks* numerical relation of any sort, then the two properties stand in the 0 relation.[323]

Unfortunately, Armstrong's own ontology does not admit of such 'negative universals' – a point which he concedes – so that the zero relation does not have the same ontological status as the other numbers. He claims that 'since the relation is internal . . . we do not have to treat it with too much ontological seriousness'[324], however I am sceptical of an account in which the number zero is treated differently from other numbers[325].

---

322. As Bigelow would say: '. . . the natural numbers can be construed at a deeper level than the rational or real or imaginary numbers ever could be. . . there is a greater difference between one pie and three-quarters of a pie, than between one pie and three pies.' (Bigelow [1988], p.5.)

323. Armstrong [1989], p.132.

324. Armstrong [1989], p.133.

325. I think that there may be *some* mathematical grounds for treating zero in some way differently from other numbers, since zero has certain unique numerical properties – e.g. $ab = 0 \Rightarrow a = 0$ or $b = 0$. On the other hand there are arithmetics in which the zero

9. TOWARDS A NEW ACCOUNT.

I Review.

My approach to the philosophy of mathematics in general, and in particular the problems with which I have been dealing in this study, is ultimately descriptive. Mathematics is a large and powerful subject area: some of it is clearly very useful when applied to real world problems; some of it, even if it is not yet applied, will be useful; and some of it may never be applied, but still fills important roles and is useful within mathematics itself. I don't think that it is for the philosopher fundamentally to challenge such an edifice, but rather to account for our knowledge of that edifice. The historical perspective that I tried to sketch at the very start of this work was intended to show that it is the problem of knowledge in mathematics that has the pedigree. In my opinion (and that of others, Kitcher for example, for whom as we have seen 'most people know some mathematics and some people know a large amount of mathematics'[326]) there is no question about whether or not we have mathematical knowledge, rather the important question is how we in fact do have such knowledge in the light of the problems that I set up as challenges in chapter 3.

Fundamental disagreements with the basis of mathematics, such as the normative approach of the intuitionists, which would ultimately reject large amounts of mathematics as it is, seem to me to miss the important point here. Fundamental disagreements with the idea that most of mathematics is known and thus true (or approximately true), such as Field's also seem to me to be misplaced, even if we do allow that 'there is so much about the concept of knowledge that is unclear or controversial'[327]. Thus my approach is with Kitcher to assume that there is a great deal of known mathematics, and to attempt to account for it in an epistemologically and metaphysically acceptable way. We know what good mathematics is, and we must ensure that our epistemological and

---

term does not have such special properties, and is set apart only in the sense that the identity is set apart – e.g. some of the modular arithmetics.

326. Kitcher [1984], p.3.

327. Chihara [1990], p.216. This is the reason that he gives for not himself starting from the premiss that a lot of mathematics is in fact known.

metaphysical positions can take take account of it – if they cannot, then so much the worse for our epistemological and metaphysical positions!

In this work I have discussed a number of philosophical approaches – how have those approaches fared? Field, I suggest, has not been successful. His inability to nominalize all of science, the technical problems with conservativeness highlighted by Shapiro, and his generally rather arbitrary acceptance of some abstract objects (space-time points, homomorphisms, expressions etc.) against his rejection of others (mathematical ones), all go to undermine his entire project in, I suspect, an irredeemable way. Maddy does a little better, but her account does seem to founder on the problem of the perception of sets as highlighted by Chihara. Lowe's account too holds sets in the special position of being the objects that instantiate the numbers. I hope that I have managed to cast enough doubt on this approach; it seems to me to be in error to attempt to give an account which gives a special position and importance to sets, for not only does there seem to be a problem with the perception of sets, but as I showed in discussing Lowe's account, there also seems to be a problem that sets, when taken in this special position, as entities which are epistemologically prior to other mathematical objects, do not do the work required of them and cannot account for even the most basic of mathematical operations. Bigelow's claim that mathematical objects are in fact relations between things (and relations between relations), seems to be a more adequate account than the Maddy/Lowe account, taking over the 'numbers as universals' idea, and yet avoiding the difficulty of over-emphasising the importance of sets. The strength of Bigelow's approach is that it can adequately account for our basic mathematical operations of addition and multiplication; the weaknesses in Bigelow are firstly the metaphysical and epistemological difficulties with 'fast and loose' universals, and secondly the problem of zero (which we may remember was also a failing of Maddy's and Lowe's account). Kitcher's bold moves to tackle the problems in a completely different way throw up many useful ideas, but unfortunately his reliance on the non-apriorism of mathematics is, I have argued, unsustainable. Armstrong on the other hand produces a much more acceptable account of the a priori in general, and thus he can escape Kitcher's arguments against mathematical apriorism; he generates an account of mathematical entities, acceptable I think not only within the technicalities of his combinatorial naturalism, but also for other naturalistic approaches, which is well in tune also with Bigelow's successful contention that numbers are some sort of relations. Armstrong's main weakness, I have contended, is that his account does not adequately draw out the distinction between the natural numbers and other types of number, and also we note that he too has some difficulty with zero.

Thus it appears that the most successful approaches so far – Bigelow's and Armstrong's – both treat of numbers as relations. My contention is that this is fundamentally correct, and that if we can deal adequately with the weaknesses in these particular accounts, then we should have a very robust account of the objects of mathematics which will not only be acceptable to mathematics itself, but also will square with our theories of both epistemology and metaphysics.

II A new account.

I now put forward some synthesis of the discussions so far, and in doing so I will produce a hopefully coherent view to be seen as a potentially fruitful line of enquiry from which we can make some progress. The line of enquiry which I propose should have some significant advantages over those accounts discussed so far in this study; in particular the view I suggest should be able to cope with the major challenges set out in chapter 3, and ought to avoid the weaknesses which, I have argued, undermine the other accounts that we have considered. Obviously there will be some weaknesses in the account I offer – principally these are the problems of zero and infinity, and also a possible loss of objectivity in mathematical truth – and these difficulties are pointed out below.

The foundations of my position should be clear, but I state them here to be both precise and definite.

(i) I am impressed by the Quine/Putnam indispensability argument, and by Quine's arguments for a realism about physical objects; this is central in my own account.

(ii) I am strongly influenced by the occurrence of aspectual seeing (in mathematical statements in particular[328]), and thus by the facts (a) that certain sorts of perception are intentional, and (b), if Falk is right, that the aspect-shift phenomenon implies some possibility of a physical contribution. This leads on to:

---

328. See the examples given in chapter 7, section VI.

(iii) Another fundamentally important insight is Kitcher's, that mathematics has grown from the basic (physical) operations of collecting and correlating, and thus that all mathematics presupposes not only these operations, but also other cognitive abilities, such as the ability to individuate objects.

(iv) I find those accounts, such as Bigelow's and Armstrong's, of numbers as relations (between objects, or between relations) convincing, and I favour Bigelow's account – which gives a special status to natural numbers – over Armstrong's.[329]

(v) I think that Armstrong has created strong reasons to accept a defence of an altered ('purged') a priorism in mathematics.

(vi) Lowe has noted the distinction between count nouns and mass nouns, and this implies, I believe, that there is a fundamental source of mathematical intuition in us.

I want to argue that relations in general (and numbers in particular) are intentional objects, mind dependent entities which are indispensable for our scientific[330] conception, and manipulation, of the world; I also want to argue that mathematics is a priori, and is also capable of describing different possible structures for the world. I shall put forward this position initially with a Quinian style argument for these mind dependent entities, and I will set it out after prefacing it with a Quinian style argument for physical objects; for it is structurally similar just up to the point of the mind dependence, or independence, of the objects. So my suggested realism is based on a sort of double-decker Quinian argument.

At the first level then, we perceive physical objects because these (are myths that) round out our experiential flux. It is conceivable that objects on this model are merely mental, or 'logical constructs out of sense data'[331]; but Ayer's logical constructs were only linguistic, and Quine, as we have seen, is a realist with regard to what we presuppose about the physical world; I am with Quine here. At the second level, I am suggesting that relations (and hence numbers) are myths (in Quine's realist sense) that round out our perceptions of objects; they structure, or give extra structure to, our experience of the physical world in a way analogous to the

---

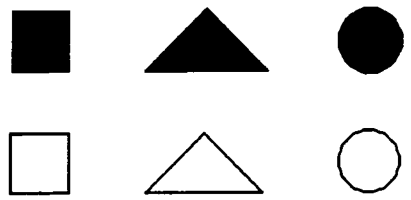
329. But I am less convinced by Bigelow's contention that mathematics is the science of (all) universals.

330. Here I use the word "scientific" in a sense which may contain such primitive activities as counting sheep, through things such as measuring, to the more complex notions of quantum mechanics etc.

331. c.f. A.J. Ayer, Language, Truth and Logic.

way objects give structure to our experience of the flux of sense data. And yet I will argue below that in this case relations (and hence numbers) are in fact mind *dependent* entities. (This argument will turn on the fact of aspectual seeing in the case of the application of relations to objects).

An example will help. Imagine that we enter a room, empty except for six objects, and for the sake of argument imagine the objects to be the coloured cards[332]:



At the first level, Quine's argument tells us that we do indeed perceive some individual physical things. If this is the totality of our interest, then this is all – we can imagine a tribe (or a stage of human development) for whom this is indeed all[333]. If we have a primitive interest in counting, as early herdsmen must have had, then we might compare the individuals with marks on a stick – are there the same number of individuals here today as there were yesterday? An advance on this would be if we could recognize the pattern of the six objects as a six, subitizing rather than counting. The world of the room is slowly gaining more structure which the perceiving agents are themselves bringing to the room, until the stage where we can see lots of structure in that array – we can see it as: a six (six objects); as two threes; as three twos; as two (two colours); as three (three shapes); and so on. And note that being able to do all these different "seeings as" requires us to have learned how to do them; so the structure comes from us – in terms of top-down processing rather than reflective inference – and gives more sense to what we perceive. In each case, when we change the aspect here, we are changing the focus of our attention from one set of intentional objects to another: for example, if we see the array of

---

332. As in chapter 7, §VI, example (ii).

333. There are countless stories of tribes who count 'one, two, many'. Assuming this is true, then such tribesmen have a mathematically very unstructured world. Also, in his [1993], p.58, Falk cites some research in animal psychology from Dretske – the monkeys in the study could be taught to respond to the larger of two rectangles ("abstracting" as it were the "larger than" relation) but could not respond to the middle sized of three given rectangles (thus not being able to abstract the "intermediate size" relation). My claim is that the amount of structure that we perceive in the world depends on what conceptual apparatus we bring to bear ourselves; in particular my claim is about mathematical structure.

objects as three twos ('I see three') then the given intentional objects might be the columns, or possibly the shapes; but if we see it as two threes ('I see two') then the given intentional objects might be the rows, or even the colours[334].

Now, whereas at the first level of the neo-Quinian argument I opposed the suggestion that the objects were mental constructs, I want to argue at the second level that the numbers are *in fact* mental entities. Bigelow's account has these numbers (relations) as real physical things, but I argue (contra Bigelow) that numbers (relations) are in fact mental constructs – intentional relations – and not real physical things. We might either construe the numbers as real things (analogous to Quine's construal of objects as real things) or as mental (or logical) constructs, but my claim is that there is a difference in the argument here at the second level. I have two arguments for this claim, the first is based on the idea that the mind-dependence is implicit in the fact of being able to see several aspects in number relations, and the second based on the idea that numbers are intentional objects.

Firstly then, the argument from aspectual seeing will be made by means of an example. At the first level of the neo-Quinian argument, within a particular community[335], whatever is perceived as (for example) a bottle, is either a bottle or is not. Let us assume that it is a bottle, and if I perceive it as such then all well and good; on the other hand, if I do not perceive it as a bottle, then I have made a mistake (for whatever reason). We could imagine this conversation, where A and B are looking at a bottle:

A I see a bottle.

B So do I.

or this:

A I see a bottle.

B I see a stick.

In the second case, B is mistaken.

At the second level however, if I perceive a collection of objects, then this could be (as in the example above) construed as a six, or a three, or a two, or even as a one, and I would not have had to make a mistake to

---

334. I shall claim below that the numbers are also intentional objects.

335. I am certainly thinking here of some sort of Wittgensteinian 'form of life' with its own 'language game'; for example people in Great Britain speaking English.

contradict. In other words, neither of the conversations, where A and B are looking at the objects from the example above:

A I see a six.
B So do I.

and

A I see a three.
B I see a two.

need imply that anyone has made an error (even though they might have so done!) Thus at this level, the numbers (as myths that round out our perceptions of objects) are not as concrete as the physical objects at the first level; rather they are different ways of seeing the collection, under our intentional (mental) control. Thus I claim that the numbers are mind dependent entities, for they do not have the permanence of the physical objects at the first level.[336]

(It may be objected here that there is in fact no difference in type between my two examples, but only a difference in degree. I suppose that such an objection would be based on some idea that a bottle may be a stick in some sense ('but it's a stick to *me*!'), but I hope that by grounding both A and B in the same community, or 'form of life', then this objection is circumvented; my suggestion is that, within this form of life, a bottle cannot be a stick in any way.)

Now the argument from the intentionality of numbers. I have argued above that numbers are intentional objects – intentional relations holding between intentional individuals. Intentional objects are mind dependent, for although they (usually) are derived from our real perceptions of real objects (as in Anscombe's Stag example where the intentional object – the stag – is derived from the perception of the real object – the father) this is not a necessary derivation. For intentional objects can be (again as in Anscombe) derived from entirely mental causes such as hallucination. Thus intentional objects are mind dependent just because they are intentional. Hence numbers are mind dependent.

So numbers are mental entities, imposed in a Quinian way upon the world to give it structure, and the mental entities are relations (for Bigelow's or Armstrong's reasons). As an example, consider again the array of dots from chapter 7, section IV: $\begin{smallmatrix} \mathrm{o} & \mathrm{o} & \mathrm{o} \\ \mathrm{o} & \mathrm{o} & \mathrm{o} \end{smallmatrix}$. This is a *six* (it

---

336. Further, it is interesting to note that the 's-phenomenal' cases of aspectual seeing do not occur for real objects. Aspectual seeing seems to be in the province of what Wittgenstein called 'picture-objects' – Wittgenstein [1953], p.194.

exemplifies the six-place relation of six-fold mutual distinctness[337]), or a *three* (it exemplifies the three-place relation of three-fold mutual distinctness etc.), or a *two*, or a *one*. If we see it as a six, then it is made of six of these: "o". Both *six* and "o" are intentional objects – the intentional individuals are "o", and the intentional relation which holds between them is the six-place relation of six-fold mutual distinctness (i.e. it is the number *six*). On the other hand, if we see it as (for example) a two, then it is made of two of these: "o o o". Both *two* and "o o o" are intentional objects – the intentional individuals are "o o o", and the intentional relation which holds between them is the two-place relation of two-fold mutual distinctness (i.e. it is the number *two*). And so on.

It is important to understand here that the fact of the collection
o o o
o o o
being a *one, two, three,* or *six,* is a mathematical fact; our being able to see this collection as a *one, two, three,* or *six* depends on our mathematical knowledge – in this case our knowledge of certain forms[338]. It is in just this way that the numbers and other mathematical objects are mind dependent.

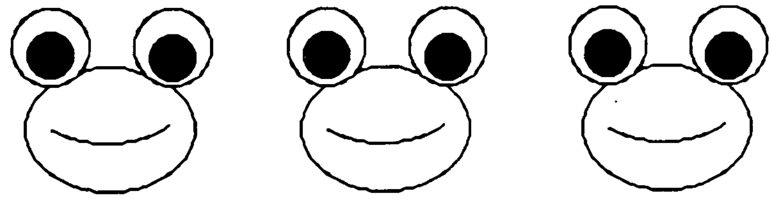
Note that here I am committed to being able to point both at intentional individuals ('Look, there's a "o o o" '; 'Look, there's a triple') and also to intentional relations ('Look, there's a *two*'). We need to ensure that we distinguish between the individuals and the relations here: a triple "o o o" is not a number, rather it is a (real, physical) individual; a *two* is a number, it is a (mind dependent) relation holding between individuals. It seems as if it might be easy to confuse, for example, a triple with a *three*, but even if I pointed to the collection – o o o – then when I say 'Look, there's a triple', my intentional object (to which I refer) is the (real, physical) form made up of paper and ink, which itself makes up the 'material object' in Anscombe's terminology; on the other hand when I say 'Look, there's a *three*', my intentional object is the relation that holds between the (also intentional) individuals "o", which are in this case singles (as opposed to *ones*).

It may be possible to draw out the distinction here between the numbers and the individuals more clearly, by changing the example. Consider these:

---

337. c.f. Bigelow [1988], p.52.

338. c.f. the discussion above in section VII of chapter 7 of the Gestalt, and A.R. Luria's work.



and ask how many eyes? how many frogs? Seeing the collection as a *three* involves us in identifying the intentional individuals as frogs, and the intentional relation amongst the frogs as *three;* seeing the collection as a *six* involves us in identifying the intentional individuals as eyes, and the intentional relation amongst the eyes as *six*.

It is clear that I have been in close sympathy with Bigelow's account of numbers as relations, and have sided with his view over Armstrong's similar line because of the distinction which Bigelow draws, and Armstrong does not, between the natural numbers and other types of number. (Yet note that I have argued against Bigelow that the numbers, as relations, are not physical entities but are rather mind dependent intentional entities, using the Quinian style argument above.) So like Bigelow, I would argue that rational, real, complex numbers and so on are all relations among relations; as in standard analysis we can build the more complicated number structures out of the integers. Of course, we do need some sort of axiom of infinity to enable us to do this – and this is still something of a stumbling block for this account. But on the other hand it strikes me that relations among relations seem more naturally mind dependent entities than (as for Bigelow) physical ones, and if the infinity concept is to have make any sense in a finite physical universe, it has a much better chance of being a mind dependent possibility that a physical relation[339].

Now there are two further reasons to prefer the account which I have sketched to others – notably Bigelow's physicalist approach. Firstly, we have seen that Bigelow (and Maddy, Lowe, Armstrong etc.) have all stumbled to a greater or lesser degree on the zero concept, but I would suggest that such a concept (just as the infinity concept) has far more chance of being an intentional, mental entity than a physical one. As we have seen the zero concept poses a thorny problem, and I would suggest

339. The infinity concept seems to me to be fundamentally based upon a possibility – the possibility of continuing the learned number sequence "1, 2, 3, . . ." indefinitely. The difficult problem is that of going from this *potential* infinite to the *actual* infinite which is required by any axiom of infinity in analysis.

that the difficulty over zero is mirrored by (perhaps even caused by) its very peculiar place in the number system; its unique mathematical properties[340] set it apart from other numbers and warn us that we should handle it with care. (On the other hand, the fact that zero is so obviously located, as every other number, on the number line suggests that its "existence" should not be radically different from the other numbers.) I would suggest at first blush that a potentially fruitful way to handle zero would be to consider it *only* as a real number – i.e. as a relation among relations – rather than as a natural number. In any case, it still remains for us a problem for the present suggested account of numbers.

A second reason for preferring numbers as mental entities rather than physical is in the explanation of the apriorism of mathematics, for it seems more than plausible that knowledge of mental entities should be a priori knowledge. On the other hand, a potential problem for the account which I am putting forward, is that if numbers are in fact mind dependent entities, then the truth of mathematical statements which refer to those entities will lose its objectivity[341]. I do not suppose that this problem is easy to solve, and clearly I have neither the time nor the space for it here in this work; rather I leave unanswered at this point the tension between, on the one hand the claimed apriorism of mathematics, and on the other the possible loss of objectivity of truth under this account.

III Putting it to the test.

How does this new account of numbers fare when put to the test of the challenges which I set up in chapter 3? Firstly, the Benacerrafian challenge C1, that numbers can't be objects. This challenge is met by any account that assigns numbers to be universals rather than particulars. The force of this Benacerrafian challenge is that, if numbers are objects, then they must be capable of being individuated separately from the role that they fulfil within the structure of numbers; but universals do not stand in need of individuation in this way, so any account which says that numbers

---

340. e.g. zero has a special place in the structure of the integers taken as an integral domain; zero has a special place in the structure of the rationals or reals taken as fields; zero has unique properties with respect to multiplication and division which stem from its position as the additive "zero" in the real number field; and so on.

341. Remember here that the objectivity of truth was, for Wright, one of the defining characteristics of realism in the realist/anti-realist debate. See above in chapter 3, section I, and in Wright [1993], p.5.

are properties (such as Lowe's or Maddy's) or relations (such as Bigelow's, Armstrong's, or mine) escapes this challenge entirely.

Secondly, a conception of numbers as mind dependent entities escapes the second Benacerrafian challenge (the Benacerraf/Steiner challenge C2 – trying to reconcile semantics with epistemology), for we can have no epistemological problems with entities which are mind dependent and come *from* us, as it were; on the other hand, we can employ a straightforward theory of truth in our semantics – in line with science at least – so that a statement ('I see three'; 'I see two') may be true or false depending on the intentional objects of the subject's perception. (And note that this is entirely public, in a Wittgensteinian sense.)

Thirdly, my account must already square with the Quine/Putnam indispensability challenge C3, for this challenge is part of the argument that led us to the account.

Finally, challenge C4: the challenge by both Field and Kitcher of accounting for the utility of mathematics in its application to the real (physical) world. If mathematical reality consists of a set of mind dependent objects, then this is the real challenge; for it seems, at first blush, fairly improbable that anything dependent on the mind could have anything at all to do with the real (physical) world. Yet this is to make a mistake, for if all relations are in fact mind dependent, and if science ultimately studies relations in the (real, physical) world, then it would seem not only probable, but entirely suitable that mind dependent relations – mathematical objects – should be used to describe such physical relations. We place relational structures (top-down processing) upon our immediate (bottom-up processed) perceptions, and this is called "abstraction" – we abstract the number concept from collections this way. But the mind dependent mathematical objects that we employ are subject to certain logical constraints and have their own inter-relations. Mathematics is the activity of exploring all of the possible inter-relations, which is itself a necessarily infinite, or unending, process, since the relation between any two relations is obviously itself a relation. The body of mathematics produced is thus a priori, and can in some cases be used to describe relations between real physical objects, but need not always do so. What I want to say is that mathematics at this point – the point where it is re-applied to the physical world – has become the 'science of possibility'[342] – the science that describes different possibilities for the way things might

---

342. Note that I am not referring to probability theory here.

in fact be. By another neo-Quinian argument, just as we bring mathematical to our perceptions in order to give them more structure, so at a more theoretical level we can bring mathematics to our theories of the world to give them different structures – to see how things might be. An example here might be illustrative.

What sort of thing is a length? A metal bar (at a certain temperature – but what is a temerature?) may be measured with a ruler calibrated in millimetres (say). The bar perhaps turns out to be is 345mm long, but this only means that its length is in the interval [344.5, 345.5]mm. Suppose we use a set of Vernier calipers to measure it at 344.8mm, yet now all we can say is that it is in the interval [344.75, 344.85]mm. No matter how accurate our measuring device, we ultimately can only ever end up with a range of values for the length, an interval of the real numbers in which, we claim, the actual length lies. Now if reality – the length of the bar is in fact continuous (a metaphysical claim), then the length of the bar can be described by some real number; on the other hand if reality is ultimately discrete in some way (another metaphysical claim), then the length of the bar can be described only by some rational number. Whatever the truth as to the metaphysics, the mathematics is available to accommodate both possibilities.

A further test of this account of mathematics is to see how works in practice – how we can use it as an account of our (initially at least) basic mathematical activities. Let us take counting – the natural numbers – as an example, and imagine that we have a case of 12 bottles of wine.

(i) Individuation: it is just brute that we individuate objects of our perception, and also it is clear that our vocabulary of count nouns presupposes that we can so do.

(ii) Collection: we can collect groups of objects into a set, either physically (like rounding up sheep into a pen) or "in our heads".

(iii) Comparison: we can compare one collected set with another, setting up a correspondence between the members; at the end of this process, one set may have more than, less than, or the same number of objects[343].

(iv) Comparison with a learned sequence: we learn an indefinitely extensible sequence – either the word sequence 'one, two, three, . . .' or the written sequence '1, 2, 3, . . .' in some language – and use it to lay off the

---

343. Note that we don't need to use the word "number" here, for we could easily rephrase any statement here in terms of coextension.

objects in our collection; the resulting "number" is just the name of the term in the learned sequence, so that we can compare our collection with (e.g.) another at some physical distance.

N.B.

(a) the number is a relation amongst the objects in our collection.

(b) If we are interested in the number of individuals, then we can in fact perceive that relation.

(c) If we change the individuals (from say the bottles to the rows of bottles – the individuals are the intentional objects) then we change to relation to which we attend.

(d) As we change the focus of our attention from one (type of) intentional object to another, then this is an aspect shift. We note (with Falk) that this has a physical attribute in that we notice here a way in which we could arrange the bottles.

(e) We note also (as with Luria's psychological experiment, or Dretske's comments on monkeys) that we are limited by our culturally-dependent conceptual apparatus in what ways we can change the individuals.

Another example: imagine that we have a 12 kg lump of lead.

(i) We have an appreciation that we have an amount of stuff that can be divided[344]. This is implied in our grammatical use of mass nouns.

(ii) We can compare the amount of stuff we have with another amount. For example we can compare the volumes of stuff (how much space they occupy) by displacing water, or we can compare the masses using a balance.

N.B.

(a) We can use a convention (a predefined volume or mass, e.g. 1 cubic cm; 1 Kg) so that two amounts of stuff can be compared at a distance. For this we use conventional "standard" masses or lengths etc. (such as the standard metre in Paris).

(b) With real numbers and measurement come a different type of error: the implication of measurement against a conventional scale is that we can measure with more, or less accurately[345]. The sense of

---

344. That is it is finitely, indefinitely, or infinitely divisible – this brings up the atomist debate and Zeno's paradoxes. But NB mathematical models can be built in all (possible) cases – see Hazen [1994] – and this is the point of the claim that mathematics is the 'science of possibility'.

345. An interesting study would be the possible types of error that can be made in mathematics in comparison to science; this, as is hinted at several times (e.g. in

"indefinitely accurate measurement" is fundamentally the real number concept.

IV Concluding remarks.

So, I am suggesting a potentially fruitful direction in which to take the debate opened up in this study, is to consider numbers as universals – intentional relations among objects (intentional individuals) for the natural numbers, and intentional relations among intentional relations for the other types of numbers. But these intentional relations are not physical, rather they are mind dependent objects brought to our perceptual experience to give it more structure.

We can them summarize this overall position vis-a-vis the numbers in two parts: Firstly with the natural numbers, and secondly with the real numbers, following the distinction in language between count nouns and mass nouns.

Natural numbers are (intentional and hence mind dependent) relations between individuals – as in Bigelow. The individuals are intentional objects in Anscombe's sense. Our ability to individuate objects of our perception is just brute, and is implicit in the grammar of count nouns in our language. Natural numbers have their roots in the possibility of doing things with individuals – collecting them, collating them, comparing collections of them – as noted by Kitcher.

The set of integers is an extrapolation from the natural numbers, including both zero and negative numbers. Negative numbers are abstractions from the possibility of certain (physical) actions – for example the action of removing, or the state of owing – thus we can see even these numbers grounded in physical actions in a Kitcher-like manner.

(Two problems still exist for the account so far: (a) the problem of accounting for zero; and (b) the problem of how to account for infinities. If zero is to be an entity within the system integers, then it has a far better chance of being a mental entity than a physical one. The notion of infinity for natural numbers and integers is just one that is embedded in physical reality – we imagine the counting just continuing for ever (whatever that may mean; it may even mean different things to different people, showing that this is not necessarily a well defined concept at this point), but we also

---

Armstrong's defence of the a priori in mathematics), would surely produce important insights into the nature of mathematics.

note that this is not really enough for us if we wish to build the real numbers etc. – as relations among relations – out of the natural numbers.)

The real numbers are (again intentional and hence mind dependent) relations between relations as in Bigelow. The unit-properties involved in the relations between relations idea are also intentional objects in Anscombe's sense. Our ability to notice that some stuff is capable of division is just brute and is implicate in the grammar of mass nouns in our language. Real numbers have their roots in the possibility of doing things with quantities of stuff – amassing it, dividing it, comparing amounts of it – again this is Kitcher's insight. Because we cannot tell "at a glance" if the world can be subdivided infinitely or not – the old atomist debate – then the real numbers contain both possibilities. The rational numbers are the relations between amounts of finitely divisible stuff; the irrational numbers are the relations between infinitely divisible stuff. (Note that if the world were ultimately atomic, then there could never be a real square with a diagonal since $\sqrt{2}$ is irrational.) Thus mathematics exceeds the world in that it can account for all possibilities.

(A problem remains about accounting for how something mental can exceed the physical (finite) universe – the fact that mathematics can describe ways in which the world might be other than how it actually is, is puzzling if mathematical entities are ultimately mental entities; and another problem remains as to how to account for the prima facie loss of objectivity in mathematical truth that results from the mind-dependence of the numbers.)

10. BIBLIOGRAPHY

Anscombe, G.E.M. **[1965]**, 'The intentionality of sensation: a grammatical feature', published in Butler R.J. (ed), *Analytic Philosophy* (second series), (Oxford: Basil Blackwell, 1965), 158-180.

Armstrong, D.M. **[1989]**, *A combinatorial theory of possibility*, (Cambridge, 1989).

Benacerraf, P. & Putnam, H. (Eds) **[1983]**, *Philosophy of Mathematics* (Cambridge,1983).

Benacerraf, P. **[1965]**, 'What numbers could not be', in Benacerraf and Putnam [1983], 272-294.

Benacerraf, P. **[1973]**, 'Mathematical truth', in Benacerraf and Putnam [1983], 403-420.

Bigelow, J. **[1988]**, *The Reality of Numbers* (Clarendon, 1988).

Black, M. **[1933]**, *The Nature of Mathematics* (Routledge, 1933).

Blackburn, Simon **[1984]**, *Spreading the Word* (Oxford, 1984).

Boyer, C.B. **[1968]**, *A History of Mathematics* (Wiley, 1968).

Burgess, John P. **[1990]**, 'Epistemology and nominalism', published in Irvine [1990], 1-15.

Chihara, C.S. **[1990]**, *Constructibility and Mathematical Existence* (Clarendon, 1990).

Clark, P. **[1993]**, 'Logicism, the continuum and anti-realism', *Analysis* 53, (1993).

Davis, P.J. & Hersh, R. **[1981]**, *The Mathematical Experience* (Harvester, 1981).

Dehaene, S. (Ed) **[1991]**, *Numerical Cognition* (Blackwell, 1991).

Diamond, Cora (Ed) **[1989]**, *Wittgenstein's lectures on the foundations of mathematics – Cambridge 1939*, (Chicago, 1989).

Dummett, M. **[1978]**, *Truth and Other Enigmas* (Duckworth, 1978).

Falk, B. & Mulhall, S. **[1993]**, 'Consciousness, cognition, and the phenomenal', *Aristotelian Society Supplement* Vol LXVII, (1993).

Field, H. **[1980]**, *Science without Numbers* (Blackwell, 1980).

Field, H. **[1989]**, *Realism, Mathematics, and Modality* (Blackwell, 1989).

Frege, G. **[1884]**, *The Foundations of Arithmetic* 2nd ed., trans. J.L. Austin (Blackwell, 1959).

Gasking, D. **[1940]**, 'Mathematics and the World' , *Australasian Journal of Psychology and Philosophy*, (1940); reprinted in Flew, A.G.N. *Logic and Language* (2nd series), (Blackwell, 1953), 204-221.

Gödel, K. **[1947]**, 'What is Cantor's continuum problem?', in Benacerraf and Putnam [1983], 470-485.

Hale, Bob **[1990]**, 'Nominalism', published in Irvine [1990], 121-144.

Hale, Bob, & Wright, Crispin **[1994]**, 'A Reductio ad Surdum? Field on the Contingency of Mathematical Objects', *Mind* 103, (1994).

Hart W.D. (Ed.) **[1996]**, *The Philosophy of Mathematics* (Oxford, 1996).

Hazen, A.P. **[1993]**, 'Slicing it thin', *Analysis* 53, (1993).

Hellman, G. **[1989]**, *Mathematics Without Numbers* (Clarendon, 1989).

Hookway, Christopher **[1988]**, *Quine: language, experience and reality* (Polity, 1988).

Hunter, G. **[1994]**, 'Platonist Manifesto', *Philosophy* 268, (April 1994).

Irvine, A.D. **[1990]**, *Physicalism in Mathematics* (Kluwer Academic, 1990).

James, E.P. **[1992]**, Review of Chihara's [1990], *Philosophical Books* XXXIII, (1992).

Kitcher, P. **[1984]**, *The Nature of Mathematical Knowledge* (Oxford, 1984).

Lakatos, I. **[1976]**, *Proofs and Refutations* (1976).

Lavine, S. **[1992]**, Review of Maddy' [1990], *Journal of Philosophy* (1992).

Lear, Jonathan **[1977]**, 'Sets and Semantics', *Journal of Philosophy* 74 (1977), 86-102.

Lipton, Peter **[1991]**, *Inference to the Best Explanation* (Routledge, 1991).

Lowe, E.J. **[1993]**, 'Are the natural numbers individuals or sorts?', *Analysis* 53, (1993), 142-146.

Lowe, E.J. **[1989a]**, *Kinds of Being* (Blackwell, 1989).

Lowe, E.J. **[1989b]**, 'What is a Criterion of Identity', *The Philosophical Quarterly* 39 (1989), 1-21.

Maddy, P. **[1990]**, *Realism in Mathematics* (Clarendon, 1990).

Miller, G.A. **[1962]**, *Psychology*, (Penguin, 1966).

Monk, Ray **[1990]**, *Ludwig Wittgenstein: The Duty of Genius.* (Cape, 1990)

Nagel, E. & Newman, J.R. **[1958]**, *Gödel's Proof*, (Routledge, 1989).

Nolan, D. & O'Leary-Hawthorne, J. **[1996]**, 'Reflexive fictionalisms', *Analysis* 56 (1996), 23-32.

Papineau, D. **[1987]**, *Reality and representation*, (Oxford: Basil Blackwell, 1987).

Popper, K.R. **[1972]**, *Objective Knowledge* (Clarendon, 1972).

Putnam, H. **[1975]**, *Mathematics, Matter and Method* (1975).

Quine, W.V.O. **[1948]**, 'On what there is', reprinted in his [1961], 1-19.

Quine, W.V.O. **[1951]**, 'Two dogmas of empiricism', reprinted in his [1961], 20-46.

Quine, W.V.O. **[1961]**, *From a logical point of view* (2nd edition, Harper, 1961).