IS MADDY'S NATURALISM A DEFENSIBLE VIEW OF MATHEMATICS?

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ABSTRACT

The thesis is an examination of Penelope Maddy's book *Naturalism in Mathematics* and of the defensibility of the arguments she presents in support of her version of naturalism, with emphasis on the philosophical significance of her work.

In Part A I first give an overview of the aim of the book and the methods used to achieve this aim.

I then set out and appraise the arguments Maddy advances showing how she supports these arguments with her appeals to historical and current scientific and mathematical practice. I discuss and appraise her comments on the work of the authors she cites and give examples of the way in which she presents her case. I examine the extent to which the work of these authors can be seen to give support to Maddy's arguments. I also examine the validity of her appeals to the analogy between naturalism in science and naturalism in mathematics with reference to her descriptions of scientific practice.

In Part B I discuss the objections advanced against Maddy's version of naturalism by contemporary critics of her book, with reference to seven authors in particular, considering the similarities and differences of the approach taken by each to Maddy. I show how their objections fall under two specific heads and appraise the persuasiveness of their criticisms.

Finaly, I assess the effect of the objections which can be raised against Maddy's naturalism and examine the question of how compelling they are and whether Maddy's naturalism is still a convincing approach to mathematics in the light of these criticisms.

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PART A

1. Maddy's *Naturalism in Mathematics*

1.1 An overview

In *Naturalism in Mathematics*, Maddy sets out a version of naturalism which, she argues, provides the answer to one of the fundamental philosophical questions: what justifies the axioms of set theory? Her aim rests on the view that set theory, along with the canons of logical inference, provides the foundation for classical mathematics: any justification of a mathematical statement will rest eventually on the axioms of set theory and the rules of deduction which enable mathematical proofs to be derived from the axioms. In particular, it is the axioms of 'ZFC' (Zermelo-Fraenkel set theory with the axiom of Choice) with which the book deals. In finding a justification for the set theoretic axioms, Maddy looks for an exemplar to the methods of justifying statements in science and in particular to Quine's naturalistic approach to the problem. She also looks to Gödel's version of realism and, identifying problems with both this and Quine's view, she arrives at a version of naturalism which, she seeks to demonstrate, provides an answer to the question of the justification of the axioms.

Her version of naturalism starts with Quine's assertion that science is justified by appeal to its own standards (as Quine puts it [p180] science is 'not answerable to any supra-scientific tribunal, and not in need of any justification beyond observation and the hypothetico-deductive method') and applies this to mathematics, arriving at the view that the justification for mathematical statements lies within mathematics itself and the practices of mathematicians. Mathematics, like science, is to be answerable to itself alone. Following on from this view, Maddy concludes that traditional (or 'First') philosophy has no role in justifying mathematical statements - that is a matter for the practitioners of mathematics. *Naturalised* philosophy however, does have a role, as it is the perspective from which Maddy's naturalism itself arises. The job of the philosopher on this reading is to act as an 'anthropologist' to the mathematical culture, identifying its goals and the methods used to achieve them, by studying what mathematicians themselves do. What philosophy cannot do is question those goals themselves and it is to Wittgenstein that Maddy turns for insight in this aspect of philosophy. Maddy ends by working through an example of what she sees as the naturalistic approach in action: she considers the case of a new axiom candidate, 'V = L' (Gödel's axiom of Constructibility) and concludes that it should not be accepted.

Maddy's book is divided into three parts: Part I deals with a statement of the problem, showing in what sense Maddy considers set theory as the foundation for mathematics and how the notion of justification applies to mathematical statements. Maddy examines the nature of justification with reference to the standard axioms and distinguishes two sorts: intrinsic justification and extrinsic justification. She then refers to several potential new axiom candidates and chooses V = L as one about whose acceptance debate still takes place. In Part II Maddy sets out the historical precursors to her naturalism: in particular she relies on Quine and Gödel's versions of realism as a starting point, setting out these authors' views and showing how they relate to historical developments in mathematics and science. She sees problems in both authors' viewpoints however, and argues that they do not accurately reflect the actual working practices of scientists and mathematicians. Specifically, she argues that reliance on the indispensability argument - that mathematics is justified by its indispensability to science - is misconceived and that consideration of Wittgenstein's views points the way to the adjustments needed to Quine and Gödel's views. Part III sets out the version of naturalism which Maddy arrives at by this process of adjustment and argues that the problems with Quine and Gödel's realism disappear when the naturalistic approach she advocates is utilised. Maddy's conclusion is that justification for the set theoretic axioms is to be found within mathematics itself: just as science does not look outside itself to justify scientific statements, so mathematics itself provides the justification for its statements. This justification is to be found by considering the goals mathematicians set for themselves, goals which are entirely a matter for them to formulate. An axiom will then be justified just when its adoption is best suited to achieve those goals. In particular, Maddy identifies two maxims which she says reflect the central goals of mathematics: 'Unify' and 'Maximise'. The first relates to the aim of arriving at one single theory which will unify all branches of mathematics, the second relates to what Maddy sees as the mathematician's desire to introduce and explore as many new mathematical concepts and objects as possible. Pointing to the potential tension between the two goals, Maddy argues that 'Maximise' prevails over 'Unify' and that when V = L is approached in this light, it is found wanting and should be rejected as too 'restrictive' of the 'Maximise' goal.

1.2 Justifying the axioms

Given that the central purpose of Maddy's book is to arrive at a 'justification' for the axioms of set theory, she devotes some time to an exploration of what a justification amounts to and what it means to say that set theory is a 'foundation' for mathematics. As far as the latter is concerned, Maddy foreswears any claim to demonstrate that set theory provides an *ontological* foundation for mathematics, because of the philosophical difficulties that arise from such an attempt. She gives Benacerraf's problem as an example: if sets are *identified* with natural numbers, how is it that {1,2,3...} can be equally well represented by the *different* sets {∅, {∅}, {∅,{∅}}...} and {∅, {∅}, {{∅}}...}? In any case, she claims that such attempts are irrelevant because set theory's true value is to provide 'surrogates' on which mathematical objects can be modelled, so that either of the two series mentioned can represent the natural numbers, ordered pairs of integers can represent the rational numbers and so on. Furthermore, Maddy claims that since all mathematical objects and structures are thereby brought into 'one arena', set theory serves as a 'court of final appeal' for questions of mathematical existence and proof: whether a certain mathematical object exists is answered by asking whether there is a set theoretical surrogate for it, and whether a given statement is provable or disprovable is answered by asking whether it it provable or disprovable from the axioms of set theory [p26]. A justification of mathematics thus becomes for Maddy a justification of the set theoretic axioms, and it is to the justification of these that Maddy turns to illustrate what is involved in a 'justification'.

As Maddy is taking ZFC as her standard set theory, she begins with the historical observation that Zermelo, whose 1908 paper 'Investigations in the foundations of set theory' she identifies as the earliest source for ZFC, saw Russell's Antimony as making Cantor's original definition of set inadmissible and therefore decided to start with set theory as a given corpus of knowledge. He chose as axioms those principles from this corpus of knowledge which were justified as 'necessary for science' rather than as being 'self-evident'. This leads Maddy to distinguish 'extrinsic' from 'intrinsic' justification [p37]: the former appeals to that which is external (the *consequences* it produces), the latter appeals to the internal (that which relies on notions of 'self-evidence' or 'intuition'). Maddy gives examples of how the axioms can be justified, both intrinsically and extrinsically. In doing so, she departs from Tiles, who objects that that it would be circular to justify the set theoretic axioms by appealing to their consequences, if the former were to provide the foundation for the latter. Maddy meets this objection by distinguishing two senses of what it means for an axiom to be a 'foundation' for a theorem. She starts from Russell's distinction between two senses of the 'premise' of a proposition [p30]: the 'empirical premise' which is some other proposition or propositions which actually lead us to believe the proposition in question, and the 'logical premise', which is some other, logically simpler, proposition or propositions from which the proposition in question can be validly deduced. So for the proposition '2 + 2 = 4', the empirical premise might be the observation that 2 sheep + 2 sheep = 4 sheep, whereas the logical premise would be axioms of pure logic from which the equation can be derived. Maddy cites Russell's conclusion: that this demonstrates the difference between obviousness and certainty on the one hand and logical simplicity on the other. So the proposition '2 + 2 = 4' is more obvious or certain than '2 + 2 = 4' but less logically simple than it, and '2 + 2 = 4' is in turn less simple than the axioms of pure logic. She thus sees the search for foundations of mathematics as a process of deriving more certain premises from less certain ones and by so doing, justifying the less certain ones. She claims this is an answer to Tiles's objection because that applies only to 'epistemic' foundations, that is those supposed to be more *certain* than their consequences. The point made by Tiles about circularity can then be seen in these terms as saying that only intrinsic justification is defensible, because extrinsic justification seeks to affirm theories by appeal to their consequences which in turn are founded upon those very theories.

Maddy's case is thus that extrinsic justification would be circular if it purported to base the more certain on the less certain, but this claim for set theory as an *epistemic* foundation for mathematics is one she is content to set aside [pp30 and 32]. What is *not* circular, she claims, is to *derive* the logically more complex from the logically simpler. So while '2 + 2 = 4' is more *certain* than the set theoretic axoms, the axioms are logically *simpler*. By deriving the equation from the axioms, Maddy says we are thereby 'boosting our confidence' in the latter [p32]. Maddy says that she follows Russell in seeking by this notion to overcome the apparent futility of using very complex propositions to arrive at what we know already. The 'pay-off' that Maddy sees in the set theoretic enterprise is one of increased fruitfulness of the theorems that can be proved from a few logically simple axioms. The justification for those axioms is thus a purely *intra-mathematical* one: not that they are more certain in themselves, but that they bring mathematical benefits such as 'uncovering inconsistencies, organising knowledge, leading to theories of greater power and fruitfulness' [p31]. This turns out to be the end result of Maddy's naturalism: that mathematical justification should be a matter entirely for mathematicians, and not subject to extra-mathematical judgement. Maddy goes on to apply the notions of extrinsic and intrinsic justification to the standard axioms. Three examples illustrate how this works.

Maddy specifies her first axiom as that of *Extensionality*: 'two sets are equal if and only if they have exactly the same members'. The 'extensional' nature of sets here stipulated is contrasted with the 'intensional' nature of entities such as properties, which can belong to exactly the same things but be different (the most famous example being Quine's 'creature with a heart' and 'creature with kidneys'). Maddy notes that some set-theorists (e.g. Shoenfield and Wang) consider extensionality to be part of the definition of 'set'. She quotes Boolos as saying that this axiom, unlike the others, might be called 'analytic' because to say 'there are distinct sets with the same members' would be a 'nonstandard usage' to a greater extent than denying other axioms would be. Maddy emphasises that Boolos uses 'analytic' in the sense of 'true by virtue of the meanings of the words involved' but says only that 'one might be tempted' to call the axiom analytic, rather than describing it as actually analytic. This reservation on Boolos's part stems from Quine's attack on the notion of analyticity and Maddy draws on this to observe that although we might say that the speaker who denies the axiom is using the word 'set' in a different way from us, this would follow as easily from the assumption that 'reasonable people do not miss obvious facts, like the axiom' as from the assumption that the axiom is analytic. In other words, it could just be that the axiom is simply 'more obvious' than the others [p39].

Boolos's response to this problem is to say that the justification for the axiom, whatever it might be, is more likely to resemble the justification for sentences such as 'all bachelors are married' than for the other axioms and Maddy says she suspects that this justification would not be extrinsic (so, for example, would not be based on the consequences of the axiom). Maddy contrasts this view with what she sees as an opposing one: that the axiom is not analytic at all because there could be different intensional notions of 'set' with the same extension. She quote examples given by Fraenkel, Bar-Hillel and Levy such as 'the set of things which are red or blue' having the same extension (but different intensional sense) as 'the set of things which are blue or red'. On this reading then, the axiom is not analytic, so its justification is found by asking 'why should we study extensional rather than intensional sets?' [p39].

The three part answer given by Fraenkel et al is noted here: (1) because the extensional notion of set is simpler and clearer than any possible intensional notion of set; (2) because there is just one extensional notion of set but many intensional ones, depending on the purpose for which those sets are needed, so that choosing an intensional notion as the basis for set theory is bound to be arbitrary and (3) since it might nevertheless be useful to retain the availability of the different purposes performed by the various intensional notions, intensional notions of set can be constructed within a system based upon the extensional notion of set. Here, (1) is explained as saying that the notion of co-extensionality (having the same members) is easier than that of co-intensionality (being determined by the same property) because our understanding of 'membership' is firmer than our understanding of 'what it is to be the same property'. For Maddy, all three of these arguments are 'broadly extrinsic' because they state that since the extensional notion of set generates a simpler theory, it is thus easier to use, and also is powerful enough to replicate its rivals, and this is so even though one might think intrinsic justification for extensionality is more attractive [p40].

For the second axiom, the *Empty Set* ('there exists a set with no members') Maddy suggests an extrinsic justification as being the obvious one, because as Zermelo and Russell point out, the concept of a set (a collection) of nothing is self-contradictory from a philosophical point of view [p41]. Zermelo called the empty set 'fictitious' but, like Russell and Gödel, included it in his set theory because of its usefulness - the fact that it leads to a more workable theory being its (extrinsic) justification. Maddy notes however [p42] that there also exists an *intrinsic* justification for this axiom, if one adopts the iterative conception of set. On this reading, which underlies Zermelo's cumulative hierarchy, sets are formed in stages, each set having as members every possible collection of members formed at an earlier stage. The first stage must then be, as the set of all previously formed sets, the empty set, as *ex hypothesi* there are at this stage no previously formed sets. Maddy sees this intrinsic justification as additional to Zermelo's extrinsic one.

Maddy observes that Zermelo's use of two types of justification for the axiom of *Choice* neatly illustrates her categories of intrinsic and extrinsic justification [p55]. She states the axiom by citing Fraenkel et al's comment on Russell's example. Russell said that it is easy to prove the existence of a choice set for an infinite collection of pairs of shoes (e.g. the set of all the left shoes) but what of an infinite collection of pairs of socks? Fraenkel et al observed that since socks in a pair are indistinguishable, there is no condition which simultaneously distinguishes one sock in each of the infinite number of pairs, so that such a set exists only by virtue of the axiom. Zermelo's first argument to justify this axiom is that it is 'intuitively self-evident' which is the only explanation for why so many set-theorists have made use of it, and this Maddy considers as an intrinsic justification. His second argument is clearly extrinsic in Maddy's terms, as Zermelo presents a number of theorems and problems which he says 'could not be dealt with at all' without the axiom [p56]. Examples are Cantor's Well-Ordering Theorem, the Aleph Theorem and fields such as analysis, topology and abstract algebra which all depend on the axiom. Maddy counters the notion of simply rejecting such areas of mathematics by reference to the story of van der Waerden's 1930 *Modern Algebra*, which dealt with areas of algebra founded on the axiom, areas which had to be deleted when the author was prevailed upon to delete the axiom (in his 1937 2nd edition) by opponents of it. It had to be re-instated for the next edition, as algebraists found it indispensable for their work. Maddy quotes a significant passage from

Zermelo where he says that no-one has the right to prevent 'scientists' from continuing to use the axiom and develop its consequences because 'principles must be judged from the point of view of science, and not science from the point of view of principles fixed once and for all' [p57].

Having thus demonstrated justifications for the existing axioms, Maddy then considers what kind of justification would be required for proposed new axioms. She uses three new candidates [pp73-81]: V = L (the axiom of Constructibility, where V is Zermelo's cumulative hierarchy and L an alternative class in which ZFC is true and so is the Continuum Hypothesis); the Large Cardinal axioms, each of which asserts the existence of more and more stages in the cumulative hierarchy, and the axiom of Determinacy (which comes from studies of infinite games). Of these, it is the first which she chooses as a model for consideration. The axiom says that only constructible sets exist, constructible in the sense that at stage $\alpha + 1$ of the construction of L, only those subsets of L_α that are definable by a first-order formula whose quantifiers range over, and whose parameters are drawn from, L_α are added. L is then the union of the L_αs. The constructible set is contrasted with the notion of V as composed of all combinatorially-determined subsets of the V_αs [p65]. This latter, non-constructible set is the set envisaged in Gödel's explication of 'set': 'a set is something obtainable from the integers (or some other well-defined objects) by iterated application of the operation "set of"'[p84].

The attraction for Maddy in considering V=L is that there is no universal agreement among set theorists as to whether it is true, Gödel, Drake and Scott, for example, being opposed to its acceptance while Devlin and Fraenkel et al are in favour [p84]. On the other hand, it is not one of those candidates which are too new and controversial for dependable analysis, nor one which is so well entrenched that its justification 'is inevitably coloured by custom' [p82].

1.3 <u>Maddy's arguments against Gödel's realism</u>

Having explained what she means by saying that set theory is the foundation of mathematics and how 'justification' is to be understood in relation to axioms, Maddy devotes Part II of her book to an argument against taking the realist point of view in reaching a decision as to how specifically one justifies accepting an axiom and in particular how one answers the question of whether we should accept V = L (as will be seen, her use of 'accept' is arguable ambiguous: does it mean simply 'adopt' or does it mean 'accept as *true*' or 'believe'?). The two versions of realism she considers, and rejects, are Quine's and Gödel's. Though she finds much of interest in both versions, Maddy argues that Gödel's Platonic realism is epistemologically unworkable and that Quine's holistic realism does not adequately reflect the actual practices of scientists and mathematicians. In particular, Quine's position relies on the indispensability argument, which Maddy argues is invalid. In consideration of realism in general, Maddy says that on this view, sets exist and have properties independently of ourselves so that 'good evidence for (or against) an axiom candidate is good evidence that it is true (or false) in the real world of sets' [p87]. She argues that an additional virtue of this approach is that it accords with the 'phenomenology of mathematical experience' which is, according to Moschovakis, that 'most everybody' who does mathematics has an 'instinctive certainty' that he is thinking about 'real objects'. Significantly, Maddy notes here that 'realism gives literal backing to these sentiments' because Moschovakis himself claims, in the passage quoted, that the 'instinctive certainty' in question is 'the main point in favor of the realistic approach'. Maddy does not make clear at this point whether the 'instinctive certainty' is the justification for realism or the other way round. The first justification is analogous with those used to justify the axioms intrinsically: if an axiom is to be accepted as true because those proposing it are sure of its truth, so realism would be accepted if there were 'instinctive certainties' supporting it. The second justification is a quasi-emprical one: realism is the favoured position because it also explains the psychological phenomenon of 'instinctive certainty' that mathematical objects exist.

Maddy calls Gödel's version of realism 'staunch realism'. It states that 'the objects and theorems of mathematics are as objective and independent of our free choice and our creative acts as is the physical world' [p89]. She observes [p90] that Gödel developed this analogy between mathematical objects and the physical world firstly by his consideration of Russell's claim that axioms 'need not necessarily be evident in themselves, but rather their justification lies (exactly as in physics) in the fact that they make it possible for these "sense perceptions" to be deduced'. In relation to this claim, she quotes Gödel's later view that there might exist some axioms that would have to be accepted just as physical theories are, regardless of whether they were 'intrinsically necessary' or not, simply because of their 'abundance' of verifiable consequences and their ability to yield new methods for solving problems.

Secondly, Gödel drew a parallel between sense perception and mathematical intuition, claiming that 'despite their remoteness from sense perception, we do have something like a perception also of the objects of set theory, as is seen from the fact that the axioms force themselves upon us as being true. I don't see any reason why we should have less confidence in this kind of perception, i.e., in mathematical intuition, than in sense perception...' [p91]. Gödel also rejected the notion that the 'givens' of mathematical intuition are purely subjective: he concedes they cannot be causally ascribed to actions of external bodies on our senses but claims they 'may represent an aspect of objective reality' whose presence in us is due to 'another kind of relationship between ourselves and reality'.

Finally, Gödel's view was also that there is as much reason to believe in sets as in physical objects because the assumption of the former is as legitimate as the assumption of the latter, both being necessary to obtain a satisfactory system of mathematics or empirical science respectively. Maddy criticises Gödel's analogy between the positing of physical objects to explain sensory data and the positing of mathematical objects to explain mathematical intuition. She remarks [p93] that whereas natural science itself has an account of how physical objects come to stimulate our beliefs about the world via sense perception, mathematics has no such account of how we come to form mathematical beliefs, particularly given Gödel's claim that 'the objects of transfinite set theory...clearly do not belong to the physical world'. She observes that if such objects are acausal, it is difficult to see how one could arrive at an explanation for their having an effect on us at all. Maddy backs up her criticism by positing some alternative analogies and explanations: just as religious experiences can be explained by natural means rather than by appeal to supernatural beings, so mathematical experiences could be explained by reference to social phenomena such as mathematical teaching and innate capacities. Where Gödel appeals to the apparent 'intuitive obviousness' of the axioms as evidence that what we perceive are actual 'axiom entities', Maddy cites novelists saying their characters develop minds of their own without it being thought that the characters actually exist. This literally suggests the Fictionalist position but Maddy's point is not to advocate this view but rather to show that nothing in Gödel's arguments rules it out, that there is nothing in those arguments to show that mathematical claims are literally true [p94].

In contrast, she moves on to a type of realism, espoused by Quine, which is designed to do just this. She first observes that Carnap's notion of a 'linguistic framework' is central to this type of realism. Carnap denied that questions like 'do numbers exist' are deep philosophical questions: instead they are simply part of what constitutes the linguistic framework of mathematics, so that if '5 is a number' is part of this framework, 'there are numbers' follows trivially. It can of course be objected that philosophers might want to ask if there are numbers, independent of any framework, on the basis that the answer has to be known before one could know which framework to choose. Carnap's answer was that such questions are 'pseudo-questions', lacking in cognitive content: they are 'external questions', asked externally to any linguistic framework, whereas legitimate questions are always 'internal ones', posed within a linguistic framework that provides the evidential rules characterising what counts as an answer [p96].

That leaves open the question whether or not to adopt a given framework, e.g. thing language', 'number language', 'set language' and so on but Carnap's answer was that such questions do not take place within a linguistic framework and to consider one answer or another to them is pointless because again, without being within a framework, we have no evidential rules to determine what counts as an answer. Carnap argued that whether to accept a particular framework such as 'thing language' is a *pragmatic* question rather than an *epistemic* one. In accepting the 'thing language', we accept the world of things but this does not entail that we have 'a belief in the reality of the thing world' because 'the thesis of the reality of the thing world...cannot be formulated in the thing language, or it seems, in any other theoretical language'. All one is left with is the acceptance of certain statements which follow *within* the 'thing language', so one could ask 'are there apples on Mars' but not 'do apples really exist'. The former is an epistemic question, an affirmative answer to which leads us to certain beliefs about extra-terrestrial fruits, whereas the latter is not asked within our thing language or within any other theory. Carnap's criterion for accepting one framework as against another is therefore not whether it is 'true', because that is an external question, but whether it is 'more or less expedient, fruitful, conducive to the aim for which the language is intended'.

Maddy cites as an example of this the immense usefulness of mathematical language in the pursuit of natural science: this provides a good reason for accepting the mathematical language rather than, say, a 'fictionalised' thing language, but does not provide evidence for the existence of sets [p97]. Quine adduces another indispensability argument - one which Maddy rejects - that the indispensability of mathematics to science is a good reason for saying *mathematical objects exist*. This point is a key one: Maddy is saying here that the indispensability of mathematics to science is a good reason for *studying* mathematics (for 'accepting the mathematical language'). But Maddy also holds that justification of mathematical statements is a matter entirely for mathematicians: there is no appeal to any outside field such as science. But if a reason for accepting mathematical language amounts to the (or a) justification for mathematics, then it is precisely scientists and not mathematicians who are best placed to justify it because only they can assess its usefulness (this recalls Socrates' question as to who is better placed to judge the utility of a chair, the carpenter who made it or the customer who sits in it). If, on the other hand, one argues that a 'justification' is not provided by appeals to *utility* but to something else, what is that something else to be? It cannot, on Maddy's reading, be 'truth' because again, the justification for mathematics would then be found *outside* mathematics as far as the foundational axioms are concerned (once their truth is guaranteed, the truth of any resultant theorems can then be ascertained by appeal to mathematical proof). Gödel's answer was of course that the 'truth' of a mathematical statement was underwritten by the existence of the relevant mathematical objects in an abstract realm of such entities, but Maddy rejects this for the reasons given above. Another alternative is to say that mathematical truth is guaranteed by the state of the physical world, so that mathematics and science are co-terminous - this is Quine's holistic realism. But this entails that mathematical statements are revisable in the light of empirical experience and as we shall see, this is one of the aspects of Quine's realism that Maddy objects to, as being contrary to mathematical practice. The question is whether any remaining meaning of 'justification' is so artificial and narrow as to be devoid of any real content. Just such a notion gives rise to the 'astrology' objection - that on Maddy's reading, astrology too is 'justified' so her naturalism must be wrong.

1.4 Maddy's arguments against Quine's realism

Quine began from Carnap's point that efficiency and fruitfulness are the criteria for choosing between frameworks, but went further and claimed that all scientific hypotheses are tested in this way. Hence, there is no clear distinction between the internal evidential hypothesis-testing of the scientist and the pragmatic standards that Carnap argues are the criteria for deciding which framework to adopt. Maddy illustrates this disagreement between Quine and Carnap with reference to an imaginary scenario where we have adopted the standard scientific framework prior to the introduction of atomic theory and are wondering whether to add atoms to it. In Carnap's terms, we are wondering whether to move to a new framework, which includes the old one as a proper part, and we should use the power/fruitfulness and so on of the new framework as against the old to decide. Quine on the other hand would say that these criteria are already part of our scientific framework so that the distinction between internal and external questions is non-existent. But for Maddy, this difference between Quine and Carnap is not that great because they both agree that the adoption of a framework depends on pragmatic criteria [p98].

She argues that there is another difference, however, which involves the analogy between mathematical justification and scientific evidence. She gives as an example of the latter the 'rule' that experiences of such-and-such a sort provide good evidence for the statement that there is a tree outside my window. This 'rule' is part of the 'thing-language' framework, the acceptance of which entails, as Carnap puts it, that one accepts 'rules for forming statements and for testing, accepting or rejecting them'. But Carnap contrasted this approach with that entailed by the mathematical framework or 'number language' [p99]. In this framework, one accepts rules for forming statements and asks internal questions such as 'is there a prime number greater than 100' but the answers are found, in Carnap's words, 'not by empirical investigation based on observations, but by logical analysis based on the rules for the new expressions'. For Carnap therefore, mathematical statements are 'analytic' whereas scientific ones are 'synthetic' so that what Maddy calls 'the full language of scientific enquiry' is composed of two parts, the mathematical (analytic) part and the physical (synthetic) part. Carnap also put this view in terms of truth-conditions: the mathematical part he considered true by virtue of the meaning of the words involved, the physical part being true 'by virtue of the way the world is'. Maddy puts the difference between Quine and Carnap briefly: Quine denies that ontological questions are external and he denies that the linguistic and factual parts of scientific statements can be separated in any principled way [p100].

This leads Quine to his 'holistic' view of the language of science, according to which mathematical statements are revisable in the light of experience but lie towards the centre of a 'man-made fabric' of knowledge which touches experience at its edges. So if experience leads us to revise our current knowledge, it is our physical statements that are revised first, before our mathematical statements are allowed to be changed. Were we to change those mathematical statements, it would have a ripple effect on many other statements, so intimately is mathematics woven into the fabric of scientific discourse [p102]. Quine's ontological view - his mathematical realism - is summed up here with reference to his 'criterion of ontological commitment' [p103]. This is applied by consideration of what the discourse in question says 'there is': for Quine, we adopt our best scientific theory of the world on pragmatic grounds (simplicity, comprehensiveness, efficiency and so on) and by doing so commit ourselves to an ontology including mathematical things; we become mathematical realists. She contrasts this view with that of

Putnam and sums up the respective views of Quine, Putnam and Carnap by saying that 'Carnap imagines a distinction between the pragmatically justified and the factual; Quine and Putnam agree that there is no such distinction; Quine sees all as pragmatic, while Putnam sees all as factual' [p103 n9]. Maddy illustrates this with a passage from Putnam in which he argues that mathematical objects are *indispensable* to scientific discourse and that this can be seen from consideration of the Law of Universal Gravitation. He says the law makes an objective statement about bodies, namely that they behave in such a way that the quotient of two numbers *associated* with the bodies is equal to a third number *associated* with the bodies. If numbers and 'associations' were merely fictions, how could the law have any objective content? Therefore, one cannot be a realist with respect to physical objects but an anti-realist with respect to mathematical theory [p104].

Maddy observes that this, the indispensability argument for mathematical realism, has been as 'influential a factor in support of realism as Benacerraf's problem has been against it'. However, she draws attention to some of the 'peculiarities' of this realist view. Firstly, she notes that those persuaded by Ayer's insistence on the impossibility of denying mathematical statements without experiencing self-contradiction will not be satisfied by Quine's attempt to explain such feelings away [p105]. Secondly, she draws attention to the distinction made by Putnam himself between pure and applied mathematics, the indispensability argument applying to the latter only. So Putnam argues for the existence of sets and functions insofar as they are needed for scientific discourse, but says that sets of very high type (higher than the continuum) are only *potentially* indispensable at some time in the future. Maddy contrasts this view with Quine's, when he says that talk of such higher sets is not meaningful. The peculiarity of the indispensability argument, according to Maddy [p106], is that its proponents justify mathematics by appeal to its role in physical theory whereas most mathematicians would appeal to proof, intuition, plausibility and 'intra-theoretic pragmatic considerations'. Maddy claims that this divergence is marked enough to cause Quinean realists to arrive at different conclusions from those reached by most mathematicians. She cites as an example Quine's support for the truth of 'V=L' as a means of keeping scientific discourse free from what he calls 'the more gratuitous flights of higher set theory', a position which Maddy says is 'precisely opposite to that of the set theoretic community'. The objection being put here is that since Quine argues that only those mathematical objects which are indispensable for science exist, he can consistently deny that, for example, non-constructible sets exist (he says they are 'without ontological rights') even though they are used in contemporary set theory. Finally, Maddy argues that Quine's view is inconsistent with the 'phenomenological matter' that, as Gödel said, some mathematical truths 'force themselves upon as being true'. Quine's view of such truths was on the contrary that we should, in the words of Parsons, 'maintain reserve, keeping in mind that experience might not bear them out'.

Gödel's realism gives us no reason to believe that mathematical claims are true, on Maddy's reading, while Quine's realism, although it gives us such a reason via the indispensability argument, conflicts with mathematical practice. She therefore sees the task for set theoretic realism as that of arriving at a view which has the strengths of Gödel and Quine's versions of realism but not their weaknesses [p108]. She sees this compromise position as one which utilises the indispensability argument to provide grounds for supposing some mathematical objects exist (e.g. the continuum), but utilises mathematical methods rather than physical scientific ones to verify mathematical claims. She says that the starting point for this approach is to give a 'down to earth neurophysiological' account of mathematical intuition, rather than Gödel's 'rather mystical faculty', then to show the rationality of extrinsic justification and to use these tools to discover what are now taken to be the determinate answers to questions about new axiom candidates. Maddy's argument is that if Quinean holistic realism is correct, there should be an analogy between the circumstances in which scientific theories are accepted or rejected and those in which mathematical theories are accepted or rejected.

She takes as an example the history of the principle of Mechanism, which states that the force between any two given particles depends only on the distance between them and acts along the line connecting them [p111]. Maddy observes, with quotations from Einstein and Infeld's *The Evolution of Physics*, that Mechanism arose as a generalisation from scientific practice and gave rise to striking experimental success in such diverse fields as astronomy and kinetics - it was thus a classic candidate for a true theory. Problems arose with it, however, when scientists applied it to explain electrostatic and magnetic phenomena, such as the need to introduce new substances within the theory and the contradictory experimental findings that a moving electric charge will deflect a magnetic needle along a line *perpendicular* to that connecting charge and magnet, with the force it exerts depending not on distance but on the velocity of the charge.

Thus far, the Mechanist could still have retained his theory, in Quinean terms by revising parts of the theory at the periphery while leaving the core untouched, but Maddy points out that the difficulty of explaining light waves as transverse in the Mechanists' 'ether' was unsurmountable, because transverse waves require an elastic, solid medium but the ether had to be more like a gas to allow planets to move through unimpeded [p113]. The result was the evolution of a new theory - field theory - first fully set out by Maxwell in his description of the electromagnetic field. The theory, laying emphasis as it does on the description of the field between two charges rather than the charges themselves, bears a physical analogy to Gödel's 'axioms so abundant in their verifiable consequences', as it 'led to the identification of light waves as a species of electromagnetic waves, that is, to the unification of optics with electricity and magnetism'. Such was the power of field theory, and its avoidance of the problems besetting Mechanism, that it superseded the latter.

In sum: (1) generalisation from successful scientific practice leads to the formulation of a theory; (2) anomalies arise within the theory which are intially dealt with by modifying it; (3) an alternative theory arises, again from generalisation, which can account for the ground already covered by the existing theory but which is more 'fruitful' in its consequences. Eventually, the old theory is abandoned, not because of any simple anomaly, but because 'the accumulated effects of a number of anomalies, combined with an attractive alternative maxim, do the job together' [p115].

Maddy in fact doubts the validity of drawing an analogy between this model of scientific practice and mathematical practice and the next chapter of her book is headed *Hints of trouble*. The first matter she draws attention to (and one which causes her to question the realist viewpoint) is a diametric difference between mathematical and scientific practice:

whereas scientists posit only those entities necessary to account for their observations, mathematicians posit as many as they wish 'short of inconsistency' [p131]. This recalls the preceding discussion where Maddy noted that a reason for deposing mechanism was that it required the positing of *too many* entities (she quotes Einstein and Infield: 'New kinds of substances had to be invented...The wealth of substances begins to be overwhelming!' [p112]). Another problem is the difference between mathematical and scientific practice in 'tracking' the truth: whereas the scientist has to give up an otherwise consistent and useful theory because it simply is not *true*, the set theorist would be less likely to say that a theory is consistent and mathematically useful but is nevertheless simply *false*. But from a realist viewpoint, the realist *should* be willing to say this, as whether a mathematical theory is true or not is as much a question of fact as whether light is a transverse or longitudinal wave (the example quoted here [p 132] being Einstein and Infeld's disappointment that nature was not 'merciful to the physicists' seeking to explain light mechanically, which is easier if light is modelled as a longitudinal wave). Yet another cause for concern for Maddy is the role of continuum mathematics in science, an issue she develops in the next chapter.

1.5 Indispensability and actual scientific practice

Having found these causes for concern, Maddy is led to re-examine the indispensability arguments. She summarises these arguments as follows: (i) our best scientific theory of the world makes indispensable use of mathematical entities; (ii) confirmation from successful tests adheres not just to individual statements but to 'large bodies of theory' (this is Quinean holism) and (iii) following Quine's criterion of ontlogical commitment, we are committed to those things our best accepted theories say there are. It follows that 'our theory, and we who adopt it, are committed to the existence of mathematical things' [p133]. Maddy's course of action is to test Quinean realism as applied to atomic theory against the actual practice of scientists. She draws attention to the fact that Quine's position depends on a number of theses, including those listed at (i)-(iii) above and that Quine talks of 'a convergence of indirect evidence' for molecular theory: *simplicity* (laws concerning apparently dissimilar phenomena are integrated into a compact and unitary theory); *familiarity of principle* (one can use already familiar laws of motion in the theory, rather than having to invent new ones); *scope* (the theory implies a 'wider array of testable consequences' than would an accumulation of separate laws); *fecundity* ('successful further extensions of theory are expedited') and *successful outcomes* of tests of the theory.

Maddy claims that scientists generally look for something *more* than satisfaction of these 'five virtues'. As an example, she traces the history of modern atomic theory from Dalton, in the first decade of the 19th Century. Dalton hypothesised that 'a sample of an elementary susbstance is made up of many tiny identical atoms which remain unchanged through chemical reactions and that a sample of a compound is made of many identical molecules, each composed of an identical combination of atoms from constituent substances' [p135]. This theory had numerous benefits: it explained other laws (e.g. Dalton's own Law of Multiple Proportions and Proust's Law of Definite Proportions); it was consistent with later experimental results (e.g. those of Gay-Lussac); it enabled extensions of the theory (e.g. it enabled Avogadro to explain Boyle's Law of 1662 ('pressure varies inversely with volume') and Charles's Law ('gases expand equally when heated equally') and led to Dumas' discovery that substitution atom-for-atom could account for the fact that a compound losing hydrogen and gaining chlorine did so in equal volumes). There was however a problem with atomic theory [p136]: there was no agreed method of calculating atomic weights (because the molecular formulas of the substances involved were not known) and scientists produced several different tables of atomic weights. This led Dumas to say he would 'erase the word "atom" from science, persuaded that it goes beyond experience; and never in chemistry ought we to go beyond experience', even though, Maddy notes, he apparently believed in atoms. She relates [p137] how the problem was overcome in 1858 by Cannizzaro who was the first to 'distinguish carefully between molecule and atom' and was able to calculate a consistent atomic table, supported by using vapour densities and then further supported by results from using specific heats. With all the laws relevant to atomic theory now being seen as consistent, and with the successful use of the theory in areas of physics such as the kinetic theory of heat, the atomic theory, by 1900, 'enjoyed all five theoretical virtues in abundance' [p138].

However, she says that 'At this point, history deals a blow to [Quine's] theory of confirmation; despite the virtues of atomic theory, scientists did not agree on the reality of atoms'. She quotes Ostwald as an example. He held that atomic theory had 'proved to be an exceedingly useful aid to instruction and investigation' but cautioned against being 'led astray by this agreement between picture and reality'. Indeed, Maddy notes that even the proponents of atoms considered their opponents' views reasonable, mainly because it was agreed that not only were atoms invisible, but their existence was not directly verifiable by experiment. Although atomic theory had Quine's fifth virtue in abundance (there were many successful tests of the theory), what it lacked was 'something more "direct", something that more conclusively "verifies" it'. It fell to Perrin to provide this proof, in accordance with results predicted by Einstein [p139]. Einstein had reasoned that Brownian movement in a liquid might be a visible manifestation of atoms moving in accordance with kinetic theory. Perrin used 'experimental techniques of unprecedented accuracy' to measure the rate of rarefaction of particles subject to Brownian motion in an emulsion. This rate (based on the fact that a gas in a vertical column is more compressed lower down owing to gravity) was already known for oxygen. So, bearing in mind that when the forces of gravity (pulling down on the particles) and Brownian motion (scattering the particles) reach equilibrium in the column of liquid, equal elevations of the liquid come with equal rarefactions, Perrin reasoned that if a column was filled with different particles from those of oxygen, any difference in 'equal elevations' would be due to exactly proportionate differences in the weight of the particles as compared with that of oxygen particles. He then showed with a series of experiments using variously sized particles that the observed atomic weights of the various substances confirmed the figures already given for them, as well as providing proof for the existence of molecules [p141]. Following publication of Perrin's results in 1908, scientists previously sceptical about the existence of atoms accepted that they did exist. Ostwald now spoke of Perrin's 'experimental proof for the atomistic constitution of space-filled matter', while Poincaré said that atoms were 'no longer [merely] a useful fiction' [p142].

This history, claims Maddy, shows the inadequacy of Quine's account of confirmation because 'scientists do not, in practice, view the overall empirical success of a theory as confirming all its parts' and would happily make use of a 'useful fiction' as a central hypothesis of a successful theory. Maddy describes the missing confirmatory test variously as 'seeing', 'observing', 'direct testing' or 'experimentally verifying' and notes that there is a gap (in the case of atomic theory, literally so, from 1860 to 1913) between confirmation whether according to the Quinean 5-fold route or to other methods such as 'inference to the best explanation' and confirmation by actual scientific standards. She concludes that there is therefore something wrong with Quine's premises, perhaps that his notion of holism is flawed or oversimplified in that a theory is not confirmed as a whole, or perhaps that his notion of ontological commitment is wrong, in that we might not be committed to *everything* our theory says 'there is' [p143].

This leads to a second cause for concern about Quine's arguments for realism: it shows that for X to be 'indispensable' for a theory does not in fact imply that practitioners accept X's existence. Where does this leave Quine and Putnam's indispensability argument for realism in relation to the 'mathematics of the continuum' as actually used by scientists? Our first reaction on opening any physics text book, Maddy claims, should be to find the argument 'laughable' as the instances where continuous quantities are used are in fact idealised fictions. She cites as examples the assumption, when studying waves, that the ocean is infinitely deep, the use of continuous functions to represent quantities such as energy and charge when we know these are quantised, and the assumption in fluid dynamics that liquids are continuous despite their atomic makeup. She considers these as instances of the more general technique of *scientific idealisation*, another instance being the 'famous frictionless plane' [p144]. Another example mentioned is the 'simplification', used for instance when studying trajectories, in the assumption that the surface of the Earth in the locale is flat. Quine accounts for idealisation in science in terms of 'paraphrasing', so that statements about the frictionless plane can be paraphrased as claims about 'the behaviour of actual planes as friction is gradually reduced to a minimum' and these paraphrases provide the 'literally true theory behind the surface idealization' [p145]. She argues however that this does not account for cases like the 'continous fluid' notion, for example, where it would lead one to say that such an ideal fluid would be what a real fluid is like as it approximates ever more closely to an ideal fluid, presumably as its molecules become ever more closely packed together. But she objects that 'this is all wrong' because such a fluid would cease to be a fluid and even if it did not, fluid dynamics applies to all fluids equally, not more to those which are more closely packed together.

What of continuous mathematics in non-generalised scientific contexts? Maddy here turns to what she calls 'one of the most common and most powerful mathematisations in all of science: the representation of phenomena by differential equations'. For such equations to be 'literally true', the physical phenomena they describe 'must be truly continuous, which would be enough to give truth-values to the independent questions of descriptive set theory, if not the CH itself' [p146]. Maddy's argument for this is confined to a brief footnote: 'Presumably the definable subsets of the continuum exist if the continuum does, so there should be a fact of the matter about their Lebesgue measurability etc. CH is a more delicate matter; if there is a physical continuum in the full second-order sense of Dedekind, then CH is also either true or false'.

There follows an examination in some detail of the 'continuum' issue beginning with an observation from Feynman that the differential equation developed for electrostatics occurs in several other fields: the analysis of heat-flow, the diffusion of neutrons, irrotational fluid flow and uniform illumination of a plane. The equation used for neutron diffusion is in fact an idealisation, fairly close on a large scale, but 'if we look more closely, we would see the individual neutrons running around'. The differential equation is thus a simplification based on the assumption that neutrons are smoothly distributed in space which leads Feynman to ask if both electrostatic and electrodynamic equations are merely 'smoothed-out', simplified versions of what is in fact a more complicated, discontinuous, sub-microscopic world. This leads Feynman on to discussion of the failure of classical electromagnetic theory when applied to electrons or other charged particles conceived as points, in that energy and mass go to infinity in the field surrounding the particle [p147]. He notes that even the introduction of quantum mechanical considerations does not save the theory, which still gives an infinite mass and energy for the electron. He concludes that scientists simply do not know how to make a self-consistent theory out of the classical Maxwell equations with their point-charges, or how to devise a theory with non point-charges.

Maddy's argument against the indispensability argument depends on the observation that there is a gap between the *utility* of a theory and the commitment of a scientist who uses the theory to its *truth.* She illustrates this by noting that Feynman himself helped to develop one solution to the problem raised above - quantum electrodynamics - which in Feynman's words makes it possible to 'sweep the infinities under the rug, by a certain crude skill' while still being able to achieve experimental accuracy to one part in a billion. Feynman's pessimism about alternative theories is explained by his acknowledgement that QED involves what he calls the 'dippy process' of 'renormalisation' [p149]. Feynman goes further and says he suspects renormalisation is not 'mathematically legitimate' and that there is in fact no 'good mathematical way to describe the theory of quantum electrodynamics', a situation which, Maddy tells us, has not improved since Feynman wrote these words in 1985 [p149]. On the other hand, Feynman records that QED does *work* 'one hundred per cent' and he therefore looks to 'the inner guts of the composition of the world' for locating the 'wrong ideas' leading to the mathematical inconsistencies in QED. He believes that 'the theory that space is continuous is wrong' and suspects that 'the simple ideas of geometry, extended down into infinitely small space, are wrong'. Maddy describes how, like electromagnetism, the weak and strong forces were renormalised to harmonise with quantum theory but that gravity has resisted such treatment, and she quotes Davies: 'so long as gravity remains an unquantised force there exists a devastating inconsistency at the heart of physics' [p150].

Maddy considers Isham's analysis of the problem [p151]: he says the problem lies with our notion of spacetime as 'based on the idea of a continuum' - a model used by special and general relativity - when in fact 'the construction of a "real" number from integers and fractions is a very abstract mathematical procedure, and there is no *a priori* reason why it should be reflected in the empirical world'. This false assumption about spacetime, by virtue of the Heisenberg Uncertainty Principle, then requires an infinite amount of energy to localise a particle at a true point, which leads Isham to speculate that below the 'natural Planck length' of quantum gravity, spacetime may be discontinuous. Maddy recalls Einstein's thought here that 'adhering to the continuum originates with me, not in a prejudice, but arises out of the fact that I have been unable to think up anything organic to take its place', leading Maddy to conclude that 'even the inventor of gravitational theory sees no compelling evidence for the continuity of its underlying spacetime' and she sees the accumulated evidence on this issue as 'enough to cast serious doubt on the existence of any physical phenomena that are literally continuous' [p152].

In summary, the argument so far presented is that firstly, scientists do not in practice have an ontological commitment to an ostensible entity which is indispensable to a scientific theory: until it is 'experimentally verified', they may be content to view it as a 'useful fiction'. And secondly, that the state of physics at the present time does not warrant any ontological commitment to continuum mathematics, which could be merely an idealisation of the real, discontinuous world. For Maddy, everyday mathematics is unproblematic, non-idealistic and finite: our current theories suggest that the number of particles in the universe has an upper bound, there are no perfect geometric figures or points in real life and it is even possible to dispense with numbers in locutions such as 'there are 3 balls on this table', which becomes 'there are x,y,z, all distinct, all balls on this table and every such ball is either x,y or z'. The conclusion is therefore that the indispensability argument 'seems unlikely to support the existence of more than a few (if any) mathematical entities', too few to guarantee any determinate truth-value to the independent questions of set theory. This gives cause for doubt that 'the interaction of mathematics with natural science is the proper arbiter of mathematical ontology', and in particular, 'science seems not to be done as it would be done if it were, in fact, the arbiter of mathematical ontology' [p154]. On the one hand Feynman speaks of the 'open question' whether time is continuous, but on the other, he represents the motion of a car as a graph of positions and times 'and finally, by a function from real numbers to real numbers' a representation which 'presupposes that time is infinitely divisible' i.e. continuous [p155]. Maddy claims that this demonstrates a double standard in use in scientific practice: on the one hand, we have mathematical assumptions and their corresponding physical structure assumptions; on the other we have 'ordinary physical assumptions' [p156]. The former 'are not treated on an epistemological par ' with the latter: 'the standards for their introduction are weaker, and their role in successful theory lacks confirmatory force', an 'epistemic disanalogy' which Maddy sees as undermining another of Quine's arguments - that the evidence for atoms is of a kind with that for everyday objects. As Quine argued that scientific evidence is simply an extension of 'commonsense' evidence, he *defined* 'evidence' in these terms and by analogy, defined evidence for mathematical objects in the same terms. But as Maddy has argued, scientific evidence imposes *stronger* standards than Quine's 'five virtues' standard, whereas evidence for mathematical objects is not like this, so Quine's argument that the existence of mathematical objects is evidenced by their role in scientific practice breaks down.

Insofar as she argues that mathematical objects (or at least, not many of them) are *not* indispensable to science, and that therefore the indispensability argument is not evidence for their existence, Maddy's claim is a straightforward one. But she then appears to conflate two arguments here: first, we have the claim that not all mathematical objects are indispensable for science and second we have the claim that the *evidence* for the existence of mathematical objects is different from that for the existence of physical objects. The difficulty is that according to Maddy, it is merely *empirically* true that the first claim is correct: she leaves it open that in fact spacetime might be continuous and therefore that the whole realm of mathematical objects might exist. But her second claim is an *epistemic* one, that the grounds for believing in the existence (as scientists understand the term) of mathematical objects are different from those for believing in the existence of physical objects: we believe a mathematical object exists merely if Quine's five virtues test is satisfied, whereas there is the additional hurdle of the 'direct verification' or 'observability' test to be jumped before one can say a physical object exists. Given this epistemic difference between 'exist' as applied to mathematical objects and as applied to physical objects, can we be sure that the meaning of the word 'exist' in each context is the same? Or ought we to understand there to be two senses of 'exist', one (the "weak" sense) applying to mathematical objects, the other (the "strong" sense) applying to physical objects?

The case for two meanings of 'exist' seems good, as the grounds for its successful use appear to be consistently different, the strong sense always and only requiring the 'extra step' of observability when physical objects are involved. We have reached this position because Maddy has rejected Quine's claim that the evidence for mathematical objects is the same as that for physical objects. (On Quine's reading, the notion of their being two senses of 'exist' arises from the mistaken idea that physical observation is relevant only to deciding whether physical objects exist. He claims that this notion is 'readily refuted' by considering questions such as 'what is the ratio of centaurs to unicorns': a ratio is clearly an abstract entity, but it is only by studying nature that we conclude the numbers in question are both zero and that ratio therefore does not exist [Quine p3].) Any semantic confusion is not, therefore, like that of a child learning (to use a well known example) the meaning of 'win'. There, the team that scores most is the winner in football but the loser in golf, so the semantic difficulty is fixing 'win' with any meaning at all. Here, we are presented with clear criteria for judging when objects exist strongly or weakly, but given no means of knowing whether the strong and weak uses of 'exist' are uses of the same word.

There appears to be an inconsistency here however: Maddy seems to be saying that if it turns out that spacetime *is* continuous, she would then concede that all mathematical objects exist, but would they exist in the strong or the weak sense? For Maddy argues that we already know they exist in the weak sense, as the useful fictions of Feynman's time and motion graph or real number function, for example. On the other hand, if as a result of the discovery that spacetime were continuous, real numbers and so on were taken to exist 'strongly', Maddy's second claim - of an epistemic difference between strong and weak existence - would fall.

1.6 Wittgenstein and naturalism

Does indispensabilty play a role in *mathematical* (as opposed to *scientific*) practice? Maddy, as already discussed, points to the difficulties the mathematician has with Quine's indispensability argument, particularly the acceptance of V = L and the use of appeals to physics rather than proofs as a justification for mathematical statements. She believes that set theorists are in fact 'indifferent' to such matters as renormalisation and their implications [p159]. This is because mathematicians, like scientists, reject the notion that applications of mathematics are the 'arbiters of mathematical ontology' and the position she advocates is that this jointly held belief should not be altered (as a philosopher 'wedded to realism' might urge). If there is a conflict between mathematical practice and philosophy, 'it is the philosophy that must give'. Naturalism is for Maddy therefore not a philosophy of mathematics so much as 'a position on the proper relations between the philosophy of mathematics and the practice of mathematics' and finds its roots in the 'anti-philosophy' of Wittgenstein's late period [p161]. She believes that a Wittgensteinian note of caution intrudes at this point because, accepting her previous arguments, we are now left with no ontological criteria for the existence of mathematical objects - realism having been found wanting - so we may be tempted to look at some form of conceptualism or fictionalism to fill the gap [p163]. But what Maddy takes Wittgenstein to be arguing for is the 'anti-philosophy', in his words, that 'philosophy may in no way interfere with the actual use of language....It leaves everything as it is. It also leaves mathematics as it is.... philosophy simply puts everything before us, and neither explains nor deduces anything'. To the obvious objection that this is itself a philosophical claim, Maddy answers that all Wittgenstein is doing is giving 'a description of an activity he proposes to engage in, namely anti-philosophy', where the 'description' is not a description in the scientific sense but is more of a speculation [p164].

Wittgenstein compared this approach to Freud's view that anxiety is always a repetition of the anxiety we felt at birth and says this view is not established 'by reference to evidence', but is 'an idea which has a marked attraction' [p164], a 'mythological explanation' or 'speculation', the acceptance of which makes things 'clearer and easier' for people. Similarly, Wittgenstein sees his speculations, which can be characterised as generalisations from 'particular cases he has treated' as a 'therapy', one of several different philosophical methods. Maddy observes that one such generalisation is 'the notion that philosophical problems arise out of linguistic confusion': words that work perfectly well in their ordinary context lose their 'contextual backing' when we try to apply them out of their context in a philosophical way. Wittgenstein notably describes how philosophical problems arise when language 'goes on holiday' and this is compared with Carnap's notion of asking questions outside a linguistic framework [p165]. When we apply Wittgenstein's diagnosis, philosophical problems should completely disappear but philosophy does not therefore leave everything 'as it is', for only the original, problem-free context is unchanged, whereas the metaphysical 'problem areas' are eliminated. In particular, the aim would be to leave mathematical practice as it is, without the confusion that philosophy has brought to it, leaving only genuine mathematical problems to be addressed.

Maddy cautions against thinking that the solution is simply for philosophers not to become involved in mathematics, because even mathematicians themselves 'are not immune to the temptations of linguistic error' [p166], introducing what Wittgenstein calls 'prose' ('names and allusions') into calculus. For Maddy, the 'prose' or 'fog' that mathematicians try to introduce into mathematics is platonism itself - 'the view that mathematics is the study of a non-spatio-temporal realm of abstract entities'. For Wittgenstein, such talk is a misunderstanding, that we can transfer talk of 'reality', 'objects' and so on from science to mathematics without robbing it of its sense. Wittgenstein holds that this arises from the mathematician's preoccupation with the interests that mathematics serves. For applied mathematics, the importance is clearly its use to great effect in science, but in the case of pure mathematics, Wittgenstein thinks mathematicians are wrongly tempted to ascribe its importance to a 'mathematical realm' which it describes. A perfectly good answer to the question 'what use does that theory serve?', is to draw attention to its role in mathematics, without resorting to talk of how it 'introduces us to the mysteries of the mathematical world' [p167][1].

However, notoriously, once Wittgenstein has stripped pure mathematics of its 'metaphysical' connotations and denied it any justificatory resort to Platonism, he is left with the conclusion that pure mathematics is merely a 'meaningless sign-game' and that fields such as set theory, while not making "wrong" statements, do not succeed in making any important ones [p168]. Thus, to Hilbert's 'No-one shall drive us out of the paradise Cantor has created for us', Wittgenstein replies that he would not dream of doing so, but *would* try to show Hilbert that he was not in a paradise at all, so that he would 'leave of his own accord'. Wittgenstein thought set theory would thus be naturally 'pruned' from mathematics, leaving mathematics to grow in healthier directions [p169]. But surely,

[1] On that point, would it not be a good answer for say, Hardy - whom Wittgenstein criticises for equating mathematical studies with physics - to say that his talk of such 'mysterious realms' is itself a Wittgensteinian 'mythological explanation' which makes mathematics 'clearer and easier' and more attractive for young mathematicians in particular?

Maddy argues, there are other goals driving mathematicians on? These goals include an underlying 'temptation to Platonism' or at least a 'sense of constraint': one cannot just say *anything* in mathematics, because there is an underlying reality there to be explored. Maddy concludes that while Wittgenstein leaves this question of internal motivation open, his work does accord with the naturalism she is espousing, in that it urges us to leave mathematics 'as it is', not allowing philosophy to alter mathematics for philosophical reasons.

1.7 A second line of thinking from Gödel

Maddy here considers a line of thinking Gödel pursued which she sees as more consistent with mathematical naturalism than with Gödelian realism. Gödel says 'the objective existence of the objects of mathematical intuition...is not decisive for [the meaningfulness of the continuum problem]. The mere psychological fact of the existence of an intuition which is sufficiently clear to produce the axioms of set theory and an open series of extensions of them suffices to give meaning to the question of the truth or falsity of propositions like Cantor's continuum hypothesis' [p175]. Hence Maddy's reference to Gödel's 'second' line of thinking: an objection based not on an analogy between science and set theory (with sets as something 'given to us' and axioms 'forcing themselves upon us' - as discussed earlier) but on the actual practice of mathematicians. She gives as an example of this approach Gödel's response to Russell's 'no-class' theory. This considered class terms as 'incomplete symbols' which could be translated away from discourse, so that a statement about 'the class of all x's with the property φ' became a definitional abbreviation of a statement about the propositional function φ(x). Gödel argued that this idea did not adequately serve the needs of mathematics and he saw this as verification of the view that logic and mathematics (like physics) are built up on axioms with real content which cannot be 'explained away' [p172-3]. Maddy sees Gödel here as concerned not with philosophical arguments but with 'particular issues in the actual practice of mathematics' and indeed, she argues that nothing would be lost by subjecting Gödel's work to Wittgensteinian pruning of its philosophical elements, leaving 'an inventory of ordinary mathematical considerations' which she sees as a starting point for the naturalism she is advocating.

1.8 Quinean naturalism

Maddy recalls Quine's conversion of philosophical questions into scientific ones: ontological questions are decided on pragmatic grounds and these standards are no different from the evidential rules of science itself [p177]. However, not all philosophical questions are legitimate on this reading: those requiring a 'properly philosophical solution' using a 'properly philosophical method' are ruled out and Maddy uses an argument of van Fraassen to illustrate this. He claims we have good reason to believe in observable things, but that we also have good reason to refrain from believing in unobservable things such as electrons. He claims that he is not thereby advocating the elimination of unobservables from science but that this (a question concerning the *methodology* of science) can be distinguished from questions concerning the 'interpretation' of science. So while there are good grounds *within* science for believing in atoms, van Fraassen claims that there is a 'higher arena' where questions of ontology are decided. Quine denies this claim - that 'extra-theoretical' discourse is possible - either for ontological questions (a position Maddy refers to as Quine's 'ontological relativity': that ontological enquiry has to take place within science) or for questions about reference (Quine's 'inscrutability of reference') [p178n]. Quine likewise considers questions of epistemology as belonging to natural science: he argued that the drive to find a secure foundation for sensory experience was not assisted by Cartesian scepticism as the 'very notion of sensory illusion depends on a rudimentary science of commonsense physical objects' [p179]. For Quine, this foundation is provided by psychology, which provides a scientific account of how the world gives rise to our sensations and enables him to describe naturalism as the view that 'it is within science itself, and not in some prior philosophy, that reality is to be identified and described' and to say that science is therefore 'not answerable to any supra-scientific tribunal, and not in need of any justification beyond observation and the hypothetico-deductive method' [p180].

The link Maddy seeks to draw between Gödel's 'second' position and Quine's naturalism is, arguably, weakened in this sense: Gödel's argument for realism was based not, as Maddy said, on questions of mathematical *practice*, but on questions of psychological *beliefs*, which are not part of the linguistic framework of mathematics but of psychology. Of course, one could claim that Gödel's argument takes place in the framework of an overarching mathematico-scientific theory (in Quine's sense) but Maddy has argued against seeking to justify mathematics by appeal to science.

Maddy deals with a number of possible objections to Quinean naturalism. First, in answer to the claim that naturalism is circular, in that it relies on science to justify itself, she appeals to Neurath's image of science as a boat which, if it is to be rebuilt, must be rebuilt while we are standing in it: in other words, science must be evaluated from within itself, there is nothing firmer to stand on outside. Also prayed in aid is Putnam's view: it is the very things that make it rational to accept a theory for scientific purposes which make it rational to believe it. To say our reason for believing S is good enough to warrant accepting S for all scientific purposes, but that this reason is not good *enough* entails that we have some superior - extra-scientific - standard to apply, but there does not seem to be any such standard.

Second, in answer to the objection that philosophy is, on this view, reduced to nothing more than a sociological examination of science, Maddy says that science is self-critical and develops and debates its own methodological norms and that the naturalistic philosopher can join in this part of the scientific enterprise, as can anyone else. What naturalism does not allow the philosopher, however, is the use of purely philosophical methods while doing this: he is confined to *scientific* methods, so that the evaluation of scientific methods takes place 'within science, using those very methods themselves' [p181].

A third objection is that if this self-referential method of assessment is used, science will always ratify its own methods and Maddy counters this with reference to her discussion of examples like mechanism, which she claims demonstrate that science can and does reject its own methods.

Lastly, there is the objection that naturalism does not demonstrate that our science is the only possible science and Maddy refers to Quine, who concedes this point but says that the possibility that there are other sciences practised by some other species gives us no reason to doubt our own, as it is 'the only point of view [we] can offer' [p182].

Maddy's answer to the third objection is arguably a weak one: her account of Mechanism shows that scientists rejected one of their theories, not the methods by which the theory was supported. Part of this difficulty is caused by the ambiguity of the word 'method': if it refers to scientific reasoning itself, then the view of Putnam she quotes on naturalism's 'circularity' , noted above, would be a better response. This reflects Strawson's argument that to ask 'is induction a justifiable procedure?' is as senseless as asking 'is the law legal?' because the criteria for determining whether something is justified are inductive ones [Strawson pp 256ff]. If on the other hand 'method' refers to specific scientific *procedures* then Maddy would be perfectly justified in giving specific examples of when these have been rejected in favour of others (most recently perhaps, paediatricians have rejected certain widely held methods of diagnosing abuse in children as unreliable). It is for this reason that Maddy rejects van Fraassen's 'higher arena' questions: they are senseless until given a sense within science [201n].

1.9 Mathematical naturalism

Having detailed her view of scientific naturalism, Maddy sets out to apply naturalism to mathematics. Starting from Quine's position, she needs to resolve the inconsistency between his indispensability argument (which allegedly runs counter to actual scientific practice) and his naturalism (which takes scientific practice as the bedrock and arbiter of our scientific worldview). In addition, she draws on the insights from Wittgenstein and Gödel discussed earlier. So where Quine justifies mathematical existence claims by claims to the indispensability of mathematics to *science*, Maddy observes that naturalism's thesis is that a successful enterprise should be judged on its own terms so that in particular, mathematics should be judged on its terms.

Her naturalism entails distinctive views about four issues: (i) the question of mathematical ontology; (ii) the 'boundary problem' - namely the issue of the demarcation between mathematics, science and philosophy; (iii) the methodology of mathematics and (iv) the role of philosophy.

As regards (i), she observes that *commonsense* tells us that scientific objects exist, but that whereas it also tells us that '2 + 2 = 4' and that 'triangles have three sides', it does not tell us that '2' exists. She remarks on a similar dichotomy between scientific statements and mathematical statements: whereas claims about the actual existence of mathematical objects are a central part of the former, the latter do not necessarily say anything about the ontology of numbers, in the sense of telling us whether they are spatio-temporal or exist objectively[2]. Quine also claims, however, that epistemologically physical objects and Homeric gods 'differ only in degree and not in kind' as both 'enter our conception only as cultural posits' [op cit p44]. Since both authors hold that (naturalised) epistemology is part of science, their difference on this issue could be taken as partly one about the psychology of how we learn about numbers and objects. Maddy later [2003b p30] identifies a prior difference between the two versions of naturalism: Quine seeks to explain how we arrive at these 'cultural posits' from the givens of experience, but Maddy's naturalist starts with the scientific question of 'how we come to be able to detect external objects by sensory means'. As Maddy puts it: 'though ZFC and its consequences present a wonderfully rich picture of the universe of sets, it is mum on the nature of their existence' [p186]. But mathematical questions *are* dealt with at a metamathematical level as part of mathematicians' everyday discourse and Maddy gives examples of Gödel and Bernays' espousal of Platonism and Heyting's advocacy of subjectivism [p187]. The key question is then not whether mathematicians talk about the ontological issues, but where the boundary between such talk and talk at a 'lower level' falls. Maddy turns again to science as a starting point for the solution of this 'boundary problem' and says that mechanism is a good example of naturalised philosophy, with its insistence that all phenomena should receive a mechanistic explanation - it is 'naturalistically acceptable' philosophy which is 'continuous with science' [p188] and originated in actual scientific methods [p190].

[2] This remark recalls Quine's claim that 'Science is a continuation of common sense, and it continues the common-sense expedient of swelling ontology to simplify theory' [Quine p45].

A passage from Einstein's essay *Reply to criticisms* is included in support of this argument [p189], where he describes the scientist as an 'unscrupulous opportunist' because he picks and chooses between different philosophies as his scientific practice demands. The reason Einstein gives for the scientist's 'opportunism' is not methodological preference but 'external conditions which are set for him by the facts of experience', which do not allow him to adhere too rigidly to any particular philosophical scheme - in other words, the 'given' of the independent and external, physical world trumps any invented philosophical scheme. But as Maddy observed, this is just the opposite of the approach adopted by the mathematician, who is reluctant to jettison a mathematical theory because of external data from the physical sciences. On Maddy's reading, however, we clearly have science, and the naturalistic philosophy which is continuous with it, on one side of the boundary and 'first philosophy'[3] on the other. And for Maddy, the boundary between philosophy and mathematics is equally clear: on the one side, mathematical existence questions are answered by appeal to methodological considerations - mathematicians opt for whichever answer is more useful and fruitful for their practice. On the other side, philosophical questions (such as the ontological staus of numbers) have not been answered, demonstrating that the mathematical questions have been settled without recourse to philosophy at all.

In summary, the 'boundary problem' is solved in similar ways for science and mathematics, with methodological considerations solving questions of the practice of the discipline on one side, and pure philosophical questions on the other. But the difference between mathematics and philosophy is that mathematics does not have within itself a naturalised epistemology at all and only a very rudimenatry ontology [p192]. An important corollary of this dichotomy between pure philosophy and mathematics is noted by Maddy: if philosophical considerations are not allowed to restrict or criticise mathematical ideas (such as the idea of impredicative sets) which mathematical practice demands, then such considerations cannot *support* such ideas, so the appeals of mathematicians such as Gödel

[3] Maddy later [Maddy 2003 pp12-13] explains the use of this phrase: in Descartes' *Meditations on First Philosophy* the sceptical meditator begins by rejecting science and common sense and looks for a foundation for knowledge in philosophy. The naturalist however, does not put philosophy *first*, but *second* after science. Thus Maddy refers to the naturalist as the Second Philosopher.

to philosophical realism as a *justification* for mathematical ideas are impermissible: either philosophy is a determinant of mathematical practice or it is not. Indeed, Maddy sees this as another failing of Quine's indispensability argument: it attempts to use philosophical considerations (the criterion of ontological commitment) to prove the existence of mathematical objects while at the same time ruling out philosophical criticism of scientific methods [p191n].

This touches upon the third issue Maddy deals with, namely 'naturalised methodology' which she sees as the search for what is left once the purely philosophical is stripped from mathematics, because it is there that she believes lies the 'justificatory structure' for mathematics which is her central goal. She has given many examples of how mathematical ideas have been adopted because they lead more readily to the goal mathematicians desire, but she now questions 'what justifies these goals?' [p194] . Maddy says the naturalist answers this by appeal to consideration of historical practice: he has to examine how mathematical questions were answered in the past, stripping out 'methodologically irrelevant' issues, identify the actual goals sought and critically observe the means-end reasoning employed, in order to arrive at a naturalistic model accurately reflecting the 'underlying justificatory structure' of mathematical practice. The 'practice', insofar as it approximates the model, is rational provided the issues that the naturalist has identified are truly irrelevant, the goals he has identified are the actual ones and the means-end reasoning used is sound [p197].

How is this claim to be justified? Maddy describes four tests for its truth: (i) the procedures used in constructing the model can be compared with other historical cases in which methodological questions were debated and resolved; (ii) the plausibility of the means-end reasoning can be tested by presenting it to mathematicians for their evaluation; (iii) the predictive power of the model can be tested by seeing if present controversies are eventually settled on the grounds identified in the model and (iv) the rationality of the arguments presented in the model is based on 'a simple fundamental of practical reason' namely the 'soundness of means-end reasoning'. Maddy makes two points about this last claim - firstly she says such instrumental reasoning is used in all human undertakings, including mathematics, and gives as an example someone who says 'I want to prove that P is false, so I'll assume P and derive a contradiction'. But secondly she says that naturalism does not thereby hold mathematics accountable to an *external* standard (e.g. saying it should be revised if it does not meet this means-end reasoning standard) but notes as a 'descriptive claim' that the underlying structures of mathematics do in fact meet this standard. This last point seems to undermine the aim of mathematical naturalism: if it is only *contingently* true that mathematical practice is rational and if being rational is a *condition* for being justified, then mathematics fails to be justified as Maddy claims, because mathematics is not required by naturalism to meet this condition (i.e. it is not held 'accountable' to this standard). On the other hand, if being rational is *not* a condition for being justified, then why appeal to a structure dependent on the identification of goals and the means-end reasoning used to attain them?

This appears to be a dichotomy inherent in Maddy's naturalism: on the one hand, the desire to base mathematical justification on mathematical practice alone, and on the other, the need to appeal to external sources to justify the model itself. A similar dichotomy appears in the claim that the naturalistic model can be tested empirically [p200]. The testing envisaged here for the identification of the goals that mathematicians strive for belongs to the science of sociology - a matter of questioning mathematicians and eliciting their goals [p198]. She even allows that sociology could show mathematicians to be mistaken as to their goals: they might say they were striving for A and B when in fact they were directing their work towards C. But what Maddy's naturalism does *not* allow is any question of testing the goals themselves against non-mathematical standards and she gives as an example mathematicians deciding to abandon the principle of consistency and accepting both '2 + 2 = 4' and '2 + 2 = 5' on the grounds that this would meet the goal of benefiting schoolchildren's self esteem [p198n]. The mathematical naturalist would find nothing in this to protest about as mathematicians would be entitled to insist that they were thereby 'pursuing a legitimate mathematical goal' and the natural scientist has 'no independent grounds on which to rule against a conclusion of the entire community'. Albeit Maddy describes this as a 'wild example', it does illustrate a difficulty with the notion of justification in mathematics as entirely limited to mathematics itself. If 'mathematical goal' is taken literally, then Maddy's claim is false, because 'increasing children's self-esteem' is self-evidently not part of mathematics. If it is taken rather as 'a goal *of* mathematics' then while the non-mathematician may be unable to criticise this choice of goal, whether the goal is *realised* will be a matter for the *non*-mathematician (or rather, will be a matter for the scientist, who on Maddy's reading practises a separate discipline - the qualification is necessary as the scientist may use mathematical methods in reaching a decision). But then the scientist will be assessing the justification of '2 + 2 = 5' from a *scientific* viewpoint - using psychological testing of children's self-esteem perhaps - and so mathematical naturalism will become no different from scientific naturalism.

The fourth element of Maddy's naturalism concerns the role of naturalised philosophy in relation to mathematics and she emphasises an important difference between the naturalistic study of science and that of mathematics: whereas the former uses the same methods as its subject matter (i.e. scientific methods), the latter does not. Mathematics uses *mathematical* methods, so while the naturalist can study it using *scientific* methods, the resultant study cannot seek to influence or question the mathematical model. The practical consequence of this is that some philosophical conclusions may be overidden on practical grounds: if set theorists concluded that the truth of the Continuum Hypothesis was undecidable, for example, then realism, which maintains that CH is either true or not, is unacceptable.

One obvious objection to Maddy's naturalism is that if mathematics is to be the arbiter of its own justification, with science not permitted to pass comment on its rationality, why would disciplines such as astrology not be similarly justified? Maddy recognises this problem [p203] and answers it by attempting to show that mathematics should be treated differently from other 'non-scientific' disciplines [p204]. Her argument is that firstly the scientist has good reason from a *scientific viewpoint* not to criticise mathematics and secondly that he nevertheless also has good reason - from the same viewpoint - for trying to account for mathematics, the result being mathematical naturalism. Maddy claims that these reasons do not, however, apply to astrology. She justifies the first part of her argument by contrasting mathematics and astrology: whereas the former makes no claim about science's 'realm' (i.e.

the whole of spacetime, 'the entire causal order'), the latter is a 'pseudo-science' which purports to tell the future with reference to the allegedly causal powers of physical heavenly bodies. Astrology is thus open to correction and criticism by science, whereas mathematics is not. That argument, if accepted, is enough to dispose of the 'astrology' objection: on this reading, there is no need to question the reasons for justifying mathematics (as these are irrelevant to astrology, which is *ex hypothesi* different from mathematics) or astrology (as this is subject to scientific testing and thus stands or falls by it). However, Maddy considers a variant of astrology which would still demand that we address the issue of showing its worth: what if astrologers said they dealt with "supernatural vibrations" which, as they do not interact with physical bodies, do not encroach on science's realm? Maddy answers that astrology would still not be worth studying because it lacks a key quality that mathematics possesses, namely that mathematics is indispensable to the practice of science. Because of this quality, we see that understanding what mathematics is and why it is so useful to science is part of understanding science itself and this motivation is not present in the case of astrology.

One objection to this argument is that, as Maddy herself argued when discussing the role of continuum mathematics in science, not all of mathematics is indispensable to science. So while *applied* mathematics might trump astrology in that it possesses a motivating appeal that astrology lacks, namely indispensability for science, other fields of mathematics do not. But it is precisely these fields which naturalism purports to justify: plainly, applied mathematics does not need any justification as it is indispensable not only to 'science' in the philosophical sense, but to everyday life. Maddy has no answer to this seeming contradiction, but merely states that on her reading 'the mathematical naturalist sees mathematics as a unified undertaking which we have reason to study as it is' [p205n]. In a sense, Maddy's naturalism works against her here: surely it would be for the scientist to say what study was worth undertaking and what parts of mathematics were to be regarded as indispensable to science? This is on the basis that, on Maddy's reading, science and mathematics are separate and that each is to be accorded the privilege of determining its own justification. On the other hand, in Maddy's favour is the argument mentioned above: if astrology makes claims about science's realm then it stands or falls by scientific standards. If it does not, then *none* of it is of scientific value and there is no comparison with mathematics, of which at least part is indispensable to scientific practice. Maddy also meets objections stemming from the possibility of a half-way house in interpreting astrology: what if it were taken to be not about astral bodies but about some sort of Jungian archetypes, leading to a deep understanding of human psychology [p204n]? Her answer is that it would then still be subject to scientific accountability, but that of psychology rather than of physics and astronomy.

Having considered Maddy's exposition of mathematical naturalism, and her differentiation between science, mathematics and first philosophy, one might well ask what is the status of her naturalism? Maddy herself describes it as 'more a method than a thesis' and notes that 'one might say its conclusions are shown rather than said' [p200]. She later [Maddy 2001 p37] describes it as 'not a doctrine, but an approach; not a set of answers, but a way of addressing questions'. The 'method' she adopts follows Wittgenstein in discarding areas of questioning - usually philosophical - which are shown by the consideration of past and present mathematical practice to be irrelevant. What we are left with forms the basis of her naturalistic model, which she says can then be empirically tested, throwing light on the nature of mathematics (and set theory in particular) and its methodology.

1.10 V = L revisited

The final part of Maddy's book is a reassessment of the question whether 'V = L' should be accepted or rejected, an enterprise now undertaken from the viewpoint of the mathematical naturalist. Maddy recalls her analogy between definabilism and mechanism, two theories once commonly accepted but now, she claims, supplanted by combinatorialism and field theory respectively. The realist's approach would be to compare 'V = L' with definabilism and argue that, because it limits the mathematician's goals, it should be similarly rejected. On this reading, in mathematics as in science, the more 'fruitful' theory prevails. But Maddy has already argued that the fact that an axiom is *useful* to mathematicians would not be enough to justify the realist's claim that the axiom in question was true of an objective world of mathematical objects: mere convenience to human thought cannot dictate arrangements in the realist's thought-independent mathematical world. This problem (or 'worry' as she puts it on p131) does not arise for the mathematical naturalist because he is looking not to a notionally objective mathematical world but to the real life practices of set theorists. Thus, she explains the acceptance of pathological functions, the axiom of choice and impredicative definitions purely in terms of their utility in meeting internal mathematical goals such as solving problems of number theory and providing a foundation for calculus [p207]. According to Maddy, this overarching notion of 'utility' explains the general acceptance by mathematicians of two maxims, 'unify' and 'maximise', as both contribute towards the more general goal of 'providing a foundation for mathematics' [p208]. The first maxim arises from the desire to arrive at a 'single, fundamental theory of sets', a goal which counters Mostowski's objection that 'if there are a multitude of set-theories then none of them can claim the central place in mathematics'. The second maxim arises from a desire by mathematicians to introduce any and all new mathematical objects and structures which take their interest - a desire Maddy contrasted earlier with that of scientists, who seek parsimony in their work. Maddy argues that the set theorist therefore has two reasons to oppose any limit on the set theoretic universe - (i) to keep in step with the mathematicians, in order to maintain the goal of using set theory to provide a solid foundation for mathematics and (ii) to meet the goal shared by set theorists and mathematicians, of pursuing whatever new objects interest them [p211]. Maddy recognises that the two aims may clash in some circumstances and gives as an example the possible extension of ZFC in alternative, inconsistent ways. She thinks that in those circumstances, 'maximise' would prevail over 'unify' so as to permit exploration of the different alternatives.

How convincing is Maddy's choice of these maxims as defining characteristics of mathematical practice, given that on Maddy's reading, it is precisely the differences between scientific and mathematical practice which provide a basis for a distinctive *mathematical* naturalism? 'Unify' as a principle is of course one shared by scientists of all fields: geologists, biologists, physicists and so on all seek their own 'grand universal theory', be it plate tectonics, Darwinism or string theory. Equally, Maddy supports her case that scientists decry 'maximise' as a goal with her quotations from several leading scientists describing the undesirability of increasing numbers of scientific objects or theories. But it is also part of her case that 'convenience' does not provide a philosophical justification for either the scientist or the mathematical realist: so the scientist may make use of continuous functions without thereby being committed to a belief in a continuous universe. On this reading, the truth of a scientific theory is determined not by its usefulness but by the 'given' of the independent objective world. So while a plurality of scientific objects or theories is not desirable, if the data demand them, the scientist must comply. But the difficulty is that the realist would argue that he is in the same position: a plurality of mathematical objects are similarly 'forced' upon him simply because that is the way the set theoretic universe is. The difference between the mathematician and the scientist is then, contrary to Maddy's claim, not methodological but merely that the scientist bemoans a plurality of objects while the mathematician rejoices at them: a purely psychological difference. Putting it another way, one could say that both the scientist and the realist mathematician share the goal of discovering everything there is - of 'maximising' their knowledge - while at the same time seeking to 'unify' the theoretical basis for that knowledge. But if scientists and mathematicians *share* their goals, where is the need for a distinctively mathematical naturalism and are we not brought back to the realist's analogy between mathematics and physics? In fact the case can be put even more forcefully, as while some scientists may wish to find the *least* number of objects to explain the universe (for example, to discover just one single string or quark type-object which constitutes all other particles) many scientists spend their time hoping to discover as many objects as possible (zoologists, botanists or astronomers for example), just as mathematicians may hope to discover new types of function or set. Thus, both mathematician and scientist may share a desire to (i) 'unify' the basic *theory* of their discipline, while (ii) 'maximising' the *objects* with which it deals so demonstrating that there is no simple principled difference between science and mathematics at all.

Maddy might counter that she has already shown that realism is not viable and thus that the realist cannot, from his holistic standpoint, aim an attack on her appeal to a distinctive *mathematical* practice utilising the two maxims. She could say that the realist is here using the very methods of mathematical naturalism - sociological study of mathematicians - to attack one of her grounds for advancing it. The realist could say he is here appealing to actual scientific and mathematical *practice* so while it *seems* that he is getting a second bite at the realist cherry, he is actually attacking Maddy on her own ground: her appeal to practice as justification. On this reading, Maddy's naturalism has not succeeded *by its own lights*. Another option is for the realist to accept part of Maddy's argument - her case against the indispensability argument - but to attack her case for a distinctive mathematical naturalism from *within* the scientific framework, on the grounds that science (sociology/psychology) shows that any differences between the goals of mathematicians and scientists are not sufficient to prove the case for a separate mathematical naturalism.

The same might be said of Maddy's discussion of V = L at the end of her book, where she explores the case against 'V = L' from the viewpoint of the naturalist seeking to observe the 'maximise' goal. The question of whether to reject or accept 'V = L' is therefore settled by consideration of how restrictive (in an internal sense) it is of the other notions which the set theorist wishes to explore. Having considered this question, Maddy concludes that 'V = L' should be rejected because its acceptance would preclude exploration of the full range of mathematical objects that the set theorist in practice wishes to include. Suppose the realist were to attack mathematical naturalism on the grounds that scientific study has shown either (i) that enough set theorists have changed their minds to give a majority in favour of V = L or (ii) that set theorists in general do not believe that the goals Maddy discusses are relevant to their work and in particular that notions of restrictiveness are irrelevant to whether V = L. Could the realist use this research to undermine naturalism? On the one hand, he would be appealing for justification to the views of that very community whose role as arbitrators of a distinctive linguistic framework he denied: he would be saying 'your views *are* relevant to deciding whether it is justified to hold that V = L'. He could not therefore consistently use those views to undermine mathematical naturalism. On the other hand, Maddy cannot argue that her naturalism is justified by a finding that the majority of set theorists agree with her and reject V = L as such a finding would not amount to *external* grounds for choosing between her framework and that of the realist but would be only *internal* grounds for rejecting V = L.

Accompanying Maddy's naturalistic approach to mathematics is her view of the discourse in which mathematics takes place [p212]. She distinguishes 'metamathematics' - which includes theorems about formulas and theorems about models, linked by theorems such as the Completeness Theorems and the Compactness Theorem - from the 'penumbra around the object-talk of the core' in which mathematicians discuss working practices, arguments for and against accepting axioms and so on. It is to this 'penumbra' Maddy assigns the two maxims 'unify' and 'maximise'. The naturalistic approach to the consideration of new axiom candidates is thus to confine the notions utilised in arguments for or against the candidates to the penumbra or the core, so that for example an appeal to a notion of 'truth' *indispensable* of mathematical theorising is ruled out of contention as irrelevant.

Part B Maddy's critics

2.1 Some general criticisms

While generally endorsing Maddy's attack on what he sees as Quine's indispensability argument on naturalistic grounds, Decock does question - on those very grounds - some specific assertions that Maddy makes. For example, he observes that not all mathematicians think set theory is relevant for their work in other branches of mathematics and he questions whether set theory can be transposed to other branches of mathematics, in the way that geometry and number theory, for example, were transposed to give Wiles' proof of Fermat's Last Theorem. Maddy herself recognises the force of this position but counters that she is seeking to promote the *unity* rather than the *uniformity* of mathematics. To say that all mathematical objects have set-theoretical surrogates is not to say that only set theoretical methods can be used to study mathematical objects: the analogy is that the claim that all scientific objects are physical does not entail that botanists, geologists and biologists should all become physicists [p34][4].

Shapiro [2004] broadly endorses Maddy's mathematical naturalism and sets it in the context of an exploration of what a 'foundation' of mathematics amounts to. He observes

[4] The possibility of attacking mathematical naturalism on its own ground is discussed further below.

that there are several frameworks for which the status of mathematical foundation is claimed: the various set theories, higher-order logic, structuralism, proof theory, ramified type theory and so on. He notes also that there are different *uses* for which foundations are intended and distinguishes between three: the philosophical, the epistemic and the mathematical. The first purports to provide an ultimate ontology for mathematics, so that a set-theoretical foundationalist might argue that all mathematical objects are sets and a Fregean logicist might say that mathematical objects are logical objects [p17]. Shapiro poses two difficulties for this approach: first, he says that most mathematicians are not interested in such ontological questions, being interested only in the *mathematical* properties of mathematical objects (Maddy also notes [p192] that mathematical practice gives little guidance on ontology). Second, he refers to the Benacerraf problem: even if we settled on say V as the basis for a set theoretical foundation for mathematics, how could we know which particular set the number 2 was as 'there can presumably be only one "being" or "intrinsic nature" of each individual mathematical object like the number 2' [p19]. Shapiro claims that Quinean realism avoids this problem: instead of insisting on discovering the intrinsic nature of mathematical objects, it advocates a parsimonious ontology restricted to sets. However, given the possibility of several alternative set theoretical foundations that meet the parsimony principle equally well, Shapiro says we are 'not exactly committed to *sets*' but only to a structure which is as rich as some set theory or other[5].

His second use of foundation is the epistemic, to provide a justification for mathematics but he makes the point that 'on any reasonable scale, ZFC is less secure than Peano arithmetic' [p22], a point also taken by Maddy in her discussion of Russell's notion of empirical and logical premise [p30]. Shapiro thus argues that this 'strong' epistemological role claimed

[5] Baker [2003] takes this to be a further weakness of the indispensability argument: he argues that if Peano arithmetic can be reduced to ZF set theory, then *numbers* are not indispensable to science, though *sets* may be. But there are alternative set theories to ZF and alternatives to set theory (such as category theory) with entirely different ontologies which can act as foundations for mathematics. Therefore no one such theory is indispensable to science and the indispensability argument thus does not demonstrate that we are rationally committed to any particular ontology of mathematical objects. This might suggest that *structures* are what our scientific discourse commits us to, as it seems not to matter which specific *object* serves as a foundation for mathematics. However, Baker argues that the weakness of the indispensability argument remains as neither the structure of ZF set theory nor that of category theory are indispensable for science.

for set theory can be set aside but that this leaves a weaker sense of 'foundation', one which seeks to provide the *ultimate* foundation for the axioms that we already know in the sense of shedding light on their epistemic basis. One result of this quest might then be to show that the axioms are analytic and/or knowable *a priori.*

The third type of foundation is the *mathematical* and it is this which Shapiro sees Maddy as providing in her book: the aim is to find in set theory faithful representations or, as Maddy says, 'surrogates', of all mathematical objects. Shapiro agrees that the motivation for doing this is to more clearly display the relationships between mathematical objects and to provide a 'court of final appeal' for mathematical existence questions. So to find out if a mathematical object exists, one asks if there is a set theoretic surrogate of it and to say a statement is provable or disprovable is to say that it is so from the set theoretic axioms. Shapiro finds the notion of providing surrogates accords well with his conception of mathematics as the science of structures but he does make the point that the process of proving that a set theoretic surrogate is a faithful representation of a mathematical idea cannot be carried out *within* ZFC because the mathematical idea is not in the language of set theory.

These general points aside, the two aspects of Maddy's naturalism that seem most liable to criticism are her approach to the indispensability argument and her response to the 'astrology' objection.

2.2 The indispensability objection

Rieger suggests that Maddy's naturalism is not a metaphysical account of mathematics but rather a view of the relationship between mathematics and philosophy. Maddy does not rule out metaphysical enquiries, but insists they must be constrained by methodological considerations. Maddy's case against the indispensability arguments is not decisive in Rieger's view and instead faces two objections. First, although Quine rejects $V = L$, one would need further argument to show that it is impossible to argue for $V \neq L$ in a Quinean way, for example 'by appealing to minimal disturbance of our naive view of a set as an arbitrary collection'. In defence of Maddy, one could argue that while she quotes from several authors who express the view that V = L appears to restrict unduly the notion of (an arbitrary) 'set' [p84], her argument against Quine's indispensability argument by no means rests on this one contradiction between Quine's views and those of the set theoretic community but is supported by a wider consideration of scientific and mathematical methodology. Furthermore, as noted above, Maddy challenges both the premise and the validity of the indispensability argument, prior to any objection to any specific conclusions that it entails. The second point Rieger makes is about Maddy's reference to scientists' use of idealisations as evidence that they are not committed to the existence of the mathematical objects which are used in their theories. He adopts Resnik's [1999] claim that although scientists use idealisations, they still rely on mathematics when doing so, and claims that 'thus for an indispensability argument to work, we do not need to assume that the science itself is true, but only that science cannot be done without assuming that the mathematics is true'.

Resnik [1999] actually distinguishes *two* indispensability arguments: he calls the one Maddy attacks the 'Holism-naturalism' indispensability argument ('H-N'). Resnik characterises this version of the argument as resting on three theses: *Indispensability* (referring to mathematical objects is indispensable to the practice of natural science), *Confirmational Holism* (the observational evidence for a scientific theory bears upon the whole theory rather than individual component hypotheses) and *naturalism* (natural science is our ultimate arbiter of truth and existence). The argument then runs as follows: mathematics is an indispensable part of science so by holism, whatever evidence we have for science is just as much evidence for the mathematical objects and principles it presupposes; thus, by naturalism, this mathematics is true and the existence of mathematical objects is as well grounded as that of other scientific entities [p45]. Resnik dismisses Maddy's argument that scientists use mathematics even though they do not consider it *true*, but merely because it is useful, on the grounds that this is 'inconsistent' and that they are really committed to the truth of the mathematics they are using, even when the theories in which it figures are known by them to be false [p46]. He claims scientists in using mathematics are 'presupposing the existence of the mathematical objects and the truth of the mathematical principles' even when using idealised models or 'false' theories [p43] and uses Newton's account of planetary orbits based on an idealised planet as an example. Newton, he says, 'presumably took for granted the mathematical principles he used' and when using a mathematical model based on an idealised planet, 'he presumably took its mathematics to be true' because for his model to work , it 'must be true' that it had the mathematical properties Newton attributed to it [p44].

Resnik puts another objection based on his version of the indispensability argument - the 'Pragmatic' indispensability argument - which he says is not open to attack by Maddy. This runs:

(R1) in stating its laws, science 'assumes the existence of many mathematical objects and the truth of much mathematics';
(R2) these assumptions are indispensable to the pursuit of science; moreover, many of the important conclusions drawn within science could not be drawn without taking mathematical claims to be true;
(R3) therefore we are justified in drawing conclusions in science only if we are justified in taking the mathematics used in science to be true.

He gives an example: psychologists who are computing statistical measures on finite data taken from observable subjects and who claim that the data are normally distributed, 'presuppose that real numbers defining the data curve exist' and that the mathematical equations they use are true. Resnik claims that this argument, unlike H-N, does not presuppose that our best scientific theories are true or even well-supported and applies 'whenever science presupposes the truth of some mathematics' [p47]. Thus, Resnik claims, this indispensability argument applies also to cases of theories known to be false, for example Newtonian physics, and in particular to idealisations such as Maddy's case of water waves in an infinitely deep ocean.

Resnik himself raises an objection to this in turn: (R3) seems to imply that we would not be justified in using mathematics in science unless we had prior evidence of its truth. He claims that indeed we do have 'quite a bit of independent evidence for mathematics' and mentions how 'practice with counting, measuring, surveying and carpentry suggested and confirmed the elementary rules of practical arithmetic and geometry' long before they were elevated to laws [p48]. He then later says that it this indispensability argument which commits scientists to the truth of the mathematics they use [p49]. Resnik's version of the indispensability argument still rests on the notion of ontological commitment then, but he claims that all that is necessary for the argument to work is that there is commitment to the truth of the *mathematics* in a scientific theory: commitment to the truth of the *science* is not necessary. But this claim rests on the 'presumptions' mentioned above that scientists must believe the truth of the mathematics they use in a theory or they would never be able to conclude that the result of their theory was correct.

One of the difficulties with Resnik's argument is that if 'science assumes' in (R1) means 'scientists assume', then (R1) is undermined by Maddy's argument that scientists do not in practice make such assumptions. If on the other hand it means that stating a scientific law entails the assumption that the mathematics in the law is true, then we do not require an indispensability argument at all, as the purpose of such an argument is just to show that the mathematics is true. Equally, if we already have independent evidence for the truth of mathematics, from measuring and carpentry and so on, then we do not need an indispensability argument to demonstrate its truth. Furthermore, since this indispensability argument is constructed on the assumption that (our best) science need not be true, we must also agree that, since (our best) science is useful and successful (*ex hypothesi*), it follows that something need not be true to be useful and successful. But then the fact that mathematics is indispensable for science would not entail that mathematics must be true.

But in any event, (R1) does not appear to be true at all: as Maddy shows [p155], Feynman, for example, uses continuum mathematics for convenience in describing the motion of a car even though he leaves open the question whether time is truly continuous. Hence it is not true that scientists are *committed* in any descriptive, psychological sense to the truth of the mathematics they use. Another instance is found in the use of the symbol '≈' in mathematics - those studying for the University of London Board A-level in Pure mathematics, for example, are told that:

'When x is small, it is evident that x^2, x^3... get successively smaller. ... So if you take terms in x^3 and higher powers of x to be negligible then $\mathbf{\sin x \approx x}$ and $\mathbf{\tan x \approx x}$ ... [Such] approximations have widespread applications in science, technology and engineering...' [Mannall p27]

It is precisely this apparent dichotomy between the actual practice of both mathematicians and scientists that Maddy expressed as giving her doubts about the viability of realism as based on indispensability arguments. Maddy is justified in posing this question of practice as a difficulty to any such argument which appeals to notions of 'commitment' in order to demonstrate the independent existence of mathematical objects.

Another objection along similar lines can be raised against (R2). Firstly, even if (R2) were true, it would not follow from the fact that '*many* scientific conclusions cannot be drawn without taking mathematical claims to be true', that we are justified in 'drawing conclusions *only* if mathematical claims are taken to be true'. Presumably what Resnik means by the first statement is that 'scientific conclusions *which rest on mathematical premises* cannot be drawn without taking mathematical claims to be true'. As argued above, if this is a *descriptive* statement and says that you cannot add 2 + 2 to get 4 unless you believe in the literal truth of '2 + 2 = 4', it is false for the reasons Maddy gives: scientists (let alone ordinary people) do not work like that. If on the other hand it is a *normative* statement, it says that you are not *justified* in concluding that there are four trees on your property (2 at the front plus 2 at the rear) unless you take '2 + 2 = 4' to be literally true. But if that is the case, then Resnik does not need either (R1) or (R3) as he has already assumed the conclusion of his argument in his second premise. I argue below that in this respect, his H-N indispensability argument is no different from Quine's as it contains a premise - that of ontological commitment - which assumes its conclusion.

If this is right, it does not entirely answer Resnik's point as if his (R2) *is* true then it poses a difficulty for Maddy who has argued that (R2) as the conclusion of an indispensability argument is false. But the problem is that Resnik does not advance any grounds for the truth of (R2). He quite correctly says that his H-N argument has only a 'limited aim': to defend mathematical realism by pointing out that any philosophy of mathematics which does not recognise the truth of classical mathematics must then face the problem of explaining how it can be used in science [Resnik p47]. But all this amounts to is a rhetorical question: how on earth could you be *justified* in saying you have 4 trees unless '2 + 2 = 4' were literally true? Resnik himself suggests where a Maddyesque naturalist could begin to look for a positive answer to this, in his claim that 'counting, measuring...and carpentry' provide 'quite a bit of evidence for mathematics'. But it would be sufficient for the naturalist to give a negative answer, namely 'how does assuming the existence of abstract entities explain one's conclusion that one has 4 trees any better than *not* assuming such entities?'. Along this line, Maddy makes the point [p93] that Gödel's realism cannot even account for how we have mathematical beliefs, let alone how mathematics actually works in science. And as against Quine's explanation that mathematics is confirmed just as the rest of our theories are, she appeals to scientific and mathematical practice to demonstrate that this is not how science works. So while Maddy does not provide in this book a theory of why mathematics is so useful for science, she does aim to show that she is in good company since neither Gödel nor Quine does any better.

This is a point developed by Mary Leng [Leng 1999], who says that having abandoned the Quinean view of the relationship between mathematics and science, any naturalistic study of mathematics must provide an alternative, which is missing from Maddy's account. Leng supports Maddy's claim that Quinean naturalism does not accord with scientific and mathematical practice and she characterises Maddy's naturalism as a search for a reason to adopt one consistent extension of ZFC as against another, once one has given up a realist metaphysics which argues that the answer is to find the 'true' version. For a solution to this problem, naturalism then looks to the goals set theorists strive for, particularly that of providing a foundation for mathematics and asks which version of ZFC best meets those goals. Leng sees a useful role for the naturalist philosopher in this scheme of things, that of analysing the extent to which a proposed axiom candidate would fulfil the mathematicians' goals, though not criticising the choice of the goal itself.

In a later article [2002], Leng begins to set out just such a picture. It is based on a passage from Quine which Maddy quotes [p106n] as an example of the restrictiveness of Quine's indispensability argument. In this Quine says that mathematical notions (e.g. inaccessible numbers) beyond a certain point - he says V = L is a 'convenient cut-off' - are merely 'mathematical recreation and without ontological rights'. Leng's view is that using Maddy's naturalism as a starting point, one can see that *all* mathematics is 'recreational', because science does not confirm any of it; but that it is indispensable for science because it successfully models scientific theories. Thus the statement '2 + 2 = 4' is indispensable to science not because it is a *true* statement, but because the mathematical game it is a part of provides a good model of our counting practices in the real world.

Leng considers [Leng 2002 p397] a possible objection that the Quinean might raise to Maddy: that her argument merely comes down to a choice between naturalism and holism. In applying the naturalistic view to science, Maddy has wrongly treated mathematics as a separate discipline and tried to apply naturalism there too, with the result that, based on the practices of mathematicians, she has concluded that holism is wrong. But it is the mathematicians that are mistaken, says the Quinean, in failing to see that science is the arbiter of mathematical existence and therefore I need not answer the problem of divergent practice on the mathematicians' part. Maddy might propose two answers to this: firstly that she has sufficient examples from scientific practice alone to throw doubt on the indispensability argument, without needing to appeal to any extension of naturalism to mathematics. Second, that since mathematics is, like science, a highly successsful enterprise, Quine should extend the same respect to mathematics as he does to science, and allow it to be the arbiter of its own justification. Leng agrees with Maddy that Quine's view of idealisations as replaceable by literally true paraphrases does not work - for example, she says we cannot paraphrase talk of continuous fluids as talk of discrete fluids as they attain a limit [p399] - and concludes that Quine fails to show that the indispensability argument applies to cases where scientists are using idealisations. She includes in this the scientific use of the continuum, for the reasons given by Maddy when she quotes Feynman and Davies, and also points to the fact that set theorists themselves do not look to physics when considering what stance to take on questions such as the truth of the Continuum Hypothesis [p401].

Leng then considers an argument proposed by Colyvan: that Maddy is wrong to believe that mathematicians are acting in a non-Quinean way insofar as they do not look to physics to determine set theoretical questions. Colyvan says they are in fact acting in accordance with Quine's 'Maxim of Minimum Mutilation': mathematical statements are just more resistant to revision as they lie more towards the centre of the holistic web of our best theory, so mathematicians are rational not to revise them just because of particular experimental outcomes. Even in cases of non-applied mathematics, where mathematicians do not seem to be committed to the truth of their statements, Quinean naturalism applies, as here mathematicians are to be seen as engaged in 'recreational' mathematics, where 'true' means no more than 'derived from axioms'. Colyvan argues that Maddy wrongly takes Quineanism as giving science itself a position privileged from philosophical criticism. He says that in fact, Quine merely tells us that there is no supra-scientific tribunal, but the philosopher can use science to criticise scientific practice: on this reading, Colyvan says that Quinean naturalism is 'in part a normative doctrine about how we ought to decide our ontological commitments: it is not merely descriptive' so that Maddy's examples of how scientists appear *not* to be committed to the truth of their statements might be examples of where they are just *wrong* [p404]. Leng observes however, that in fact mathematicians *never* seem to reject mathematical theories on the grounds of new scientific discoveries and gives as examples Euclidean geometry (where the discovery that spacetime is non-Euclidean did not lead mathematicians to deny Pythagoras' Theorem) and catastrophe theory (where its rejection as an explanation of the real world did not lead to rejection of the mathematics used in it). She concludes that therefore the distinction between recreational and non-recreational mathematics appears to be one without a difference: whether the world is or is not a certain way does not seem to determine the mathematics that mathematicians undertake or alter the content of the mathematical statements. Therefore, what scientific discoveries show is not that the mathematics is true or false, but that it does or does not successfuly model the world [p408]. Furthermore, this modelling need not even be applied on a global scale: so Feynman can use continuum mathematics on a large scale while acknowledging that it may not apply at the micro-level, and conversely, Euclidean geometry can be used to model space on a small scale, while not being accurate of spacetime as a whole [p412]. Leng claims this also answers Resnik's objection to Maddy: a scientist need not be committed to the truth of his mathematical statements on this reading (beyond the notion of a mathematical statement being true 'within the game' which is the recreational context of the use of the statement), but only to their consistent and reasonable interpretation in terms of the scientific phenomena they model [p410].

Colyvan, in addition to his 'recreational mathematics' objection, echoes Rieger's reservation about the use Maddy makes of Quine's support of $V = L$ as an argument against Quine's realism. He argues that Quine's support of $V = L$ stems from his 'zeal for simplicity' rather than with the indispensability argument and refers in support of this to Quine's explicit reason for endorsing $V = L$: from 'considerations of simplicity, economy and naturalness'. Therefore, he argues, a defender of the indispensability argument could still support an alternative to $V = L$ on the grounds of its unifying and expressive power for science, even if that alternative bore the cost of an inflated ontology. There is a reflection of that approach in a passage from Quine which Maddy quotes [p106n] where he says that he recognises nondenumerable infinites 'only because they are forced on me by the simplest known systematizations of more welcome matters', though as Colyvan concedes, Quine does not carry this as far as endorsing $V \neq L$. The premise of Colyvan's objection to Maddy is that Maddy believes Quine's support of $V = L$ is sufficient evidence of the implausibility of his indispensability argument and Colyvan's counter objection is that you can have Quine's indispensability argument without his ontological parsimony. But as far as the latter is concerned, Quine himself says 'Our acceptance of an ontology is, I think, similar in principle to our acceptance of a scientific theory...we adopt, at least insofar as we are reasonable, the simplest conceptual scheme into which the disordered fragments of raw experience can be fitted and arranged. Our ontology is determined once we have fixed upon the over-all conceptual scheme which is to accommodate science in the broadest sense..." [Quine p16]. But this is to say no more than that we accept into our ontology just that which is indispensable for our best theory and the principle of ontological commitment follows from this: that we are committed to endorsing the existence of what our best theory says there is. Colyvan does not therefore demonstrate that there were two separate motivations behind Quine's endorsement of V = L.

As far as his premise is concerned, that Maddy's objection to the indispensability argument hinges on the V = L issue, Maddy herself specifically says that even if actual mathematical practice and Quinean realism arrived at the same conclusions about V = L, Quine's reliance on the role of mathematics in science, rather than on proof or other intra-mathematical considerations, to justify mathematical claims is an 'unsettling peculiarity' of his realism [p106]. And in general Maddy makes it clear that it is Quine's appeal to science as the arbiter of mathematical ontology and his inclusion of mathematics as a part of a holistic best theory of science, which she sees as the prime reasons for rejecting his version of realism as far as mathematics is concerned.

Colyvan does make another point: he claims that Maddy is misleading to portray Quine as opposing set theorists over V = L on *philosophical* grounds as Quine is not the only author who supports V = L and he cites Devlin's endorsement of the axiom as an example. Colyvan argues that Quine's view could thus be seen as part of an *intra-mathematical* debate about the goals of set theory, of the sort which Maddy herself admits to be the only legitimate source for criticism of mathematical claims. On this reading, Quine would be arguing that set theory itself should limit its goals to those necessary for carrying out science and that since Quine was part of the set theoretic community, he was entitled to set such a goal.

Colyvan presents other objections to Maddy's naturalism (in its earlier form) in his book *The indispensability of mathematics* [Colyvan 2001]: the argument concerning mathematical recreation was discussed above in relation to Leng's critique and another argument also mentioned above (the question of whether the criterion of ontological comitement is normative or descriptive) is discussed below in section 2.3. Another objection Colyvan makes is to Maddy's argument that the fact scientists use fictional concepts like frictionless planes in their theories without being ontologically committed to them refutes the indispensability argument. Colyvan argues that it may not be the case that such scientists actually believe the concepts in question are fictional and gives two alternatives: (1) scientists could just be suspending judgement on whether the objects they refer to are fictional or real or (2) they could be committed only to a degree of belief (somewhere between 0 and 1) in the objects in question, until more evidence turns up. The difficulty with (1) is that the indispensability argument is supposed to demonstrate the existence of mathematical objects, not that we can 'suspend judgement' on them. And even if suspension of judgement is adequate to describe the thought-processes of Ostwald and Dalton, as Colyvan claims, Maddy's point was that any such suspension lasted only until the 'extra' proof she says is required for physical objects was obtained by Perrin. But there is no such proof available for the existence of numbers so the indispensability argument breaks down. There is a similar difficulty with (2): if this premise is accepted, the indispensability argument shows only that we ought to have a degree of belief between 0 and 1 for the existence of numbers but if the degree of belief was only say 0.05, it would imply a belief of 0.95 that numbers do *not* exist which is a strange conclusion for an indispensability argument. Equally, Maddy could argue that the only evidence that would change our degree of belief would be the 'extra' proof not available for numbers. Finally, Colyvan's alternatives seem to depart from Quine's insistence that the criterion of ontological commitment 'applies in the first instance to discourse and not to men' [Quine p103].

Decock [2002], like Resnik, argues against Maddy by formulating a second version of the indispensability argument which is not vulnerable to her attack on Quine's version. Decock concedes that Maddy has a good case against Quine's indispensability argument but details a second indispensability argument, which he claims is 'almost unassailable'. He distinguishes a 'strong' indispensability argument (that is, Quine's, which he characterises as 'all and only those mathematical objects exist which are indispensable to science') from a 'weak' one. His weak argument has five premises: (i) An object exists if it is the value of a general variable of the accepted scientific theory expressed in first order logic; (ii) there is a large consensus over what is accepted as science within the scientific community; (iii) science is a global, connected theory; (iv) science can be regimented into first order logic and (v) quantification over mathematical entities is indispensable in some domains of science. The conclusion is that 'mathematical entities exist'. This weak argument is, he claims, based on the theses of holism, naturalism and indispensability and is the 'genuine' Putnam-Quine argument (as it accords with Putnam's version of Quine's argument) and the one used by Resnik ('H-N') and Colyvan. He argues that Quine's case for the strong argument rests on his own principle of parsimony but agrees with Maddy that this is not supported by the set theoretic community, whereas her maxims of 'maximise' and 'unify' have stronger support than Quine's among that community. He claims that Quine's version of the indispensability argument is not an argument at all but a thesis or maxim and as he sees its premise (the parsimony principle) as unargued and lacking in support from the set theoretic community, he sees it as unsustainable in the face of Maddy's naturalism which is supported by argument and by methodological considerations. Decock says this version of the argument does not rule out metaphysical realism but does not provide a ground for it, as it shows only that mathematical objects exist within the scientific theory or framework, that is it proves the case for pragmatic or immanent realism only.

There are two difficulties with Decock's argument: firstly, his claim that if one objects to his first premise, one has to abandon first order logic is not convincing. Indeed, he concedes that use of modal or second order logic are 'respectable alternatives' to realism in mathematics and his argument is really that there is no convincing rebuttal of the first premise [p234]. I discuss this claim further below in section 2.3. Secondly, it is not entirely clear what the distinction between the two versions of the argument is, after all, Quine's version of the argument at p103 of [Quine] rests on a premise almost identically worded to Decock's first. The difference appears to be lie in an equivocation: in Decock's conclusion - that 'mathematical entities exist' - is the implication that '*all* mathematical entities exist' or '*some* mathematical entities exist'? This in turn rests on whether 'all' or 'some' entities is implied in premise (v). If 'all' is intended in premise (v), then Quine's indispensability argument - which on Decock's reading states that all the mathematical entities indispensable to science exist - proves that all mathematical entities exist, so Quine cannot be criticised for undue parsimony. But if only 'some' is intended (the more likely interpretation, as no one suggests science presently quantifies over *all* mathematical entities) then the question is 'which entities?' and the answer can only be 'just those which are indispensable to science' and Decock's indispensable argument would thus prove no more than Quine's. The only difference between the two arguments would then be that the 'weak' one leaves it open that there might be some other way of proving the existence of all the other mathematical entities - but this does not constitute a difference between the two arguments, so much as a difference between the conclusion to be drawn from the scope of any indispensability argument. In any event, Putnam - whose version of the argument Decock claims is the same as his 'weak' one - draws exactly the same conclusion as Quine [Putnam p347], that sets of cardinalities higher than are needed for science are not proved to exist by the indispensability argument: Putnam calls them 'speculative and daring extensions' of the basic mathematical apparatus of science, Quine calls them 'recreational mathematics' and the difference does not really affect Maddy's arguments.

2.3 Conclusions (1): the indispensability objection

Maddy's main thesis on this issue was that scientists are not in practice ontologically committed to the existence of mathematical objects. This seems well supported by her detailed consideration of Feynman's views and by the following remark of the practising physicist Chris Isham [p151]:

'It must be admitted that, at both the epistemological and ontological levels, our current understanding of space and time leaves much to be desired. In a gross extrapolation from daily experience, both special and general relativity use a model for spacetime that is based on the idea of a continuum...But the construction of a 'real' number from integers and fractions is a very abstract mathematical procedure, and there is no a priori reason why it should be reflected in the empirical world.'.

Furthermore, even among mathematicians, there are grounds for questioning the 'commitment' to mathematical objects that exists on a practical level. Davis and Hersh, for example, quote an estimate that 65% of mathematicians are Platonists, 30% formalists and the rest constructivists but say that in their view, mathematicians are both Platonists (when it comes to the motivation for their work) and formalists (when it comes to justifying their work) [Davis and Hersh p322]. These authors also report their interview of a scientist specialising in engineering who uses mathematics frequently and whose views, they say, are representative of the scientific community. He says that mathematics is for him a useful tool, a 'model', and the question is not how 'true' it is but how 'good': situations in physics are often too messy and uncertain for one to arrive at the 'real thing' so the best that can be done is to construct a 'tentative' model which expresses a 'partial truth', the important thing being that it should accurately predict phenomena which are observed [pp 44ff].

It would appear that in practice, all the scientist is committed to is the belief that the mathematics will give him the true (empirical) *answer* to the scientific problem at hand (e.g. that the mathematics should give the correct position of a car at time T even if this involves using an idealised continuous graph) but this only requires a commitment to its utility, not to its truth.

However, there is another aspect to Maddy's treatment of the indispensability argument which requires more attention. Both Resnik and Maddy appear to consider the notion of ontological commitment (as Quine calls it or 'presupposition of the truth of mathematics' as Resnik calls it) as a *descriptive* one: Maddy because she rebuts it with empirical examples, Resnik because he 'presumes' it applies to scientists. On this reading, Maddy's argument is that Quine's argument depends on a description of the practices of scientists (that they are committed to the existence of mathematical objects) which is not borne out by the facts and therefore Quine's conclusion, that science confirms the existence of mathematical objects, is false. Here, Maddy does not need to rely on mathematical naturalism to make her point: *from his own viewpoint* Quine's holism is false (this is a point Leng makes, as discussed above). Thus the Quinean cannot simply say that it is Maddy's naturalism which is wrong. Resnik at times, however, veers towards ontological commitment as being a *normative* concept: he says, for example, that scientists cannot 'consistently' regard the mathematics they use simply as a tool [Resnik p46] and also that if the mathematics scientists used 'were not true we would have no reason to believe in the...correctness of their calculations'. This is not correct as it stands, for the correctness of the scientists's calculations can be demonstrated by the observation of the phenomena which his theory predicts: what is more difficult to demonstrate is what Wigner calls the 'unreasonable effectiveness' of mathematics and Resnik himself makes this point [p47].

Quine claims that the criterion of ontological commitment (the test for what a theory says there is) 'applies in the first instance to discourse and not to men' [*Logic and the reification of universals* in Quine 1961 p103] and gives examples of how a speaker can 'free himself' from his ontological commitments. This comes about by 'taking an attitude of frivolity' - as in telling a story - or by finding an expanded paraphrase of the discourse which eliminates the objects quantified over. Another exception arises in what Quine refers to as 'polemical' discourse, where the speaker simply refuses to accept the 'unwelcome' ontological commitment of his discourse which 'we impute to him' [p105 *op cit*], in which case Quine says that we then cannot say what 'there is' in his discourse as we can have no idea what he is referring to in using the terms in dispute.

Colyvan, as mentioned above, also refers to Quinean naturalism as 'in part a normative doctrine about how we ought to decide our ontological commitments: it is not merely descriptive'. Although she does not discuss the explicit issue of whether Quine's criterion of ontological commitment is descriptive or normative, Maddy certainly considers what a normative view would entail: on this view, the Quinean could say that the scientists and mathematicians, by not honouring their ontological commitments, were simply *wrong* and should correct their methods [pp159-160]. It is at that point that Maddy relies on naturalism and she says that it 'offends against [the] spirit' of Quine's position for a naturalist to hold a view contrary to widespread scientific practice. While she concedes that it would not be *inconsistent* for a Quinean to hold the view that scientists were wrong, she claims that the clash between the criterion of ontological commitment and actual scientific practice reveals a 'tension' between the two that she seeks to resolve with her mathematical naturalism. To that extent therefore, Maddy advances a normative argument against Quine: that her version of naturalism ought to be preferred because it more consistently achieves the goals which mathematicians wish to pursue. Colyvan [2001] argues, to the contrary, that it is perfectly consistent with Quinean naturalism for the philosopher to attempt to persuade scientists that they are simply *wrong* not to accept their commitment to mathematical objects. Colyvan cites as one of the grounds for such persuasion Putnam's accusation of 'intellectual dishonesty', a charge Colyvan believes the philosopher can put to the scientist from within science, as an equal and on the scientist's own terms, rather than claiming a privileged position namely that of extra-scientific 'first philosophy' [p99].

Leng observes that Maddy has no explanation of *why* the indispensability argument does not, in her terms, work. Maddy speculates that it may be Quine's holism that is wrong, that maybe scientific theories are not confirmed as a unit, or it may be the notion of ontological commitment which is wrong, that we are not committed to everything our theory says there is. It would clearly strengthen Maddy's position if a way could be found to put her case against the indispensability argument without relying on mathematical naturalism, as it could otherwise be said that her stance simply pits holism against naturalism, and that Leng's defence (that Maddy has enough examples from scientific practice to rebut the indispensability argument, without needing to rely on naturalism) does not succeed if the indispensability argument is taken as normative. One way for Maddy to do this would be to question the first premise of the indispensability argument: that the scientific theories quantify over numbers in the same way that they quantify over physical objects. Both Quine [p103] and Putnam [p349] represent sentences such as 'there are even numbers greater than 10' as:

NS: $(\exists x)$ (x is a number $\cdot$ x is even $\cdot$ x is > 10)

and Putnam [op cit] observes that it follows from this that $(\exists x)$ (x is a number) which is the desired result of the indispensability argument.

The question now is, what does the variable 'x' in NS range over? Quine uses the neutral term 'entity' but if this is to include abstract entities as well as physical ones, we have already assumed the existence of the former before coming to the conclusion that they exist. If we attempt to leave it open as to what exactly 'x' ranges over, we come up against Maddy's point that scientists as a matter of fact require an *extra* test (say, the 'M-test') for the 'existence' of atoms over and above such considerations as their usefulness in theory or the fact that atomic theory appeared (to put it neutrally) to quantify over them. The 'M-test' was satisfied only when scientists were satisfied that, in quantificational terms, either (i) the 'x' in '(∃x) (x is an atom)' ranged over physical objects, rather than 'ideas' or 'words' or (ii) there was some y such that 'x = y' and 'y is a physical object' were both true. The same applies of course to statements such as 'are there unicorns': this will be true in scientific theory just when it is true that there is some physical object such that that object is a unicorn. The question is then, is the notion of 'physical object' part of the notion of '∃', of 'x' or of a further property 'being a physical object'?

In a sense, Maddy does not have to answer this (just as she leaves the question of the ontological status of mathematical objects open): it is enough for her to show that the proponent of the indispensability argument is himself assuming the existence of abstract objects before stating his first premise and that he is therefore relying on some other argument for their existence. It would then be open to Maddy to reject the claim that the criterion of ontological commitment is normative unless and until this was demonstrated by some other argument for the existence of mathematical objects and revert to her argument from scientific practice without the need to rely on mathematical naturalism. Of course, it might be objected that Maddy is equally assuming the *non*-existence of abstract entities by insisting (via the 'M-test') on 'x' being able to range over physical objects only, but that objection merely prevents her setting up a criterion of ontological commitment as a premise to an indispensability argument, when her argument is that such an argument is flawed and not to be relied on. The issue then becomes whether the proponent of the indispensability argument can construct it in such a way as not to have to rely in his premise on the existence of what the argument is supposed to prove (Quine considered this question [Quine p3] but his answer to the 'inflationist' who wished to allow variables to range over unactualised entities - so as to say that 'Pegasus exists' - is true was to 'give' him the word 'exist' and keep 'is' for his own use).

2.4 The astrology objection

Rosen agrees with Maddy's point about the 'restrictiveness' of V = L and describes her approach as primarily a contribution to the 'anthropology of mathematics', in which Maddy studies the 'exotic tribe' of mathematicians, describes their practices and shows how certain notions are or are not justified by the standards held by the tribespeople. But Rosen distinguishes what is justified 'by native lights' and what is justified '*simpliciter*' and points out that it is one thing to say S is the case by the standards of the tribe's belief system, but quite another to say that S is the case in fact. He says Maddy's most striking claim is that in the case of mathematics, this distinction does not exist: what is accepted by the mathematical community just is what is acceptable and there is no appeal to any extra-mathematical norm. However, he observes that the significance of this claim depends crucially on how Maddy intends the notion 'accept' to be understood. On the one hand, he says the word could mean 'believe', i.e. 'believe to be *true*' in which case Maddy's naturalism would be the radical claim that we are justified in believing the axioms just because they are accepted by the set theoretic community: no other reason would be needed or possible. On the other hand he says the word could mean simply 'choice-worthy' (in terms of the goals of the practice which are endorsed). In this case, mathematical naturalism would be the 'relatively unremarkable' view that intra-mathematical judgements as to which axioms to choose are immune to extra-mathematical criticism.

Rosen points out that it is difficult to say from reading Maddy's book which of these meanings she intends but he believes there is reason to think Maddy intended the first, 'strong', meaning. If this is so, he says there arises a problem: if a statement S is to be believed simply because it is accepted by the practitioners of a field of study (on the grounds that accepting it best serves to bring about Goal G of the field of study), then any statement of say astrology should be believed just because it is adopted by astrologers as best serving their goals. He calls this the 'Authority Problem for Naturalized Epistemology': why is it that we should believe mathematical or scientific statements just because mathematicians and scientists adopt them, but not astrological or theological ones? He rules out the answer that 'set theory is different because it has arrived at a series of striking truths about sets', as Maddy herself never claims this. He also rules out as absurd the notion that mathematics is different because its theories have successfully achieved the goals set by its own practitioners, because a theology which produced similarly internal successful theories would then be deemed to be credible without more. As we saw earlier, Maddy distinguishes between astrology and mathematics on the grounds that mathematics is indispensable to science while astrology is not. Thus there is a motivation for understanding mathematics, as it is important for understanding science but Rosen observes that she cannot simply be making a case for *studying* mathematics here, as the indispensability of mathematics would be irrelevant to whether to study it or not. He makes this point on the grounds that cultural anthropology studies widely held practices just because they are widely held, with no appeal to their indispensability to anthropology itself. In addition, he draws attention to the contradiction between Maddy's claims for the autonomy of mathematics from science and her claim that its indispensability to science is a reason for its authority. This defence would be available only to applied mathematics but Maddy claims mathematics is a 'unified undertaking' so that set theory has to be taken all of a piece with applied mathematics: he calls this claim the 'doctrine of authority by contagion'. From an epistemological view, therefore, Rosen argues that Maddy does not answer the 'authority problem'.

Rosen also considers Maddy's naturalism as a theory of ontology: Maddy describes mathematics as telling us nothing about the ontological status of mathematical objects but does not deny that questions about the ontology of such objects are intelligible ones and ones that might be answered in due course. Maddy considers naturalism to be compatible with a number of different ontologies, including fictionalism. Rosen distinguishes two types of fictionalism: one - *minimal realism* - which he argues 'amounts to a species of minimal realism about sets', as it regards statements about sets as true just when they are true in the 'story' of set theory and the other - *anti-realism* - which denies statements about sets are true at all. It is the second sort of fictionalism that Maddy seems to have in mind in Rosen's view, but in that case she cannot be saying that intra-mathematical acceptance of S is grounds for *belief* in S as the fictionalist will not believe in any set theoretical statement at all. Therefore, Maddy's naturalism must just amount to the claim that mathematics is immune from criticism as far as its judgement of what makes for good mathematics is concerned. Thus, naturalism is not open to the authority problem as it does not claim authority for mathematical statements but merely says they are what makes for good mathematics, a courtesy which can be extended safely to astrologers as there is nothing surprising about leaving the last word on what is acceptable to astrology to the practitioners of that field. Regardless of which claim Maddy is making for naturalism, the strong one which is open to the authority problem or the weak one which says nothing about the credibility of mathematical statements, Rosen says Maddy leaves open the question why we should believe what set theory says and he wonders whether such a form of naturalism is satisfactory.

Rosen's argument seems fairly convincing but one aspect that might be objected to is this. He says that if 'accept' means 'believe', then Maddy is claiming that we should *believe* statements about sets just because mathematicians *accept* them so we should therefore believe astrological statements just because astrologers make them. But if 'accept' means 'believe' then what naturalism asks us to do is to believe set theoretical statements because set theorists believe them and they are the experts in the field. This still leaves open what the grounds those experts have for believing the statements in the first place are. As far as ontology is concerned it is worth recalling that Maddy not only thinks the premise of the indispensability argument is false (she thinks 'few if any' mathematical entities are indispensable to science [p153]) but in addition does not believe it is a valid argument because she does not believe the *evidence* for mathematical entities is the same as the evidence for physical objects, which are indispensable for science. As we saw, she dismisses Quine's claim that the evidence for the two sorts of object is of a piece and thinks science is not the arbiter of mathematical ontology at all [pp154, 156]).
Another objection to Rosen's argument concerns his claim that Maddy leaves the ontological mathematical questions open: arguably, she considers these to be part of first philosophy and hence 'pseudo'. She claims that mathematical debates (e.g. about whether to accept impredicative definitions) have historically been settled without recourse to philosophical considerations [p191], implying that these are irrelevant, and she rules out the possibility of arriving at an ontological position which conflicts with mathematical

practice [p201]. It would be fairer to say that when Maddy claims that a certain philosophy does not conflict with naturalism, she does not mean it may eventually found to be the correct view. Instead, she is claiming that while the philosophy does not say anything contrary to mathematical practice and so is not ruled out, it is 'true' only insofar as it reflects that practice: there is no appeal to any higher extra-mathematical philosophy to adjudicate on the matter. Rosen's most cogent objection remains his point that Maddy eventually has to fall back upon indispensability to science as a justification for mathematics, whether in the sense of a reason for studying it or a reason for believing its statements to be true. Where she does not do this, she is caught by the astrology problem and ends up with what Rosen calls the 'relatively unremarkable' view that mathematicians are the final arbiters of what is good for mathematics.

Jill Dieterle [1999] also concentrates on the 'astrology' objection and argues that Maddy's response to it is inadequate because it catches her on the horns of a dilemma. Maddy's response either undermines her mathematical naturalism, leaving her only with scientific naturalism, or it leaves the door open to 'astrological naturalism'. Dieterle firstly characterises Maddy's naturalism in terms of its rejection of extra-mathematical criticism or evaluation of mathematics, then puts the question which Maddy herself poses: why should mathematics be so special, why cannot astrology, for example, claim the same sort of privilege from external criticism? She distinguishes in Maddy's response two disanalogies (which we can call D1 and D2) said to exist between mathematics and astrology: (1) that astrology, unlike mathematics, makes claims about the physical world, so is subject to the judgement of science, which deals with the whole spatio-temporal realm and (2) that mathematics, unlike astrology, is 'staggeringly useful, seemingly indispensable to science'. Dieterle's tactic is to argue that if either disanalogy is made out, then mathematical naturalism falls away, but that if either is *not* made out, then mathematics is on a par with astrology or theology or other 'unpalatable naturalisms' as Dieterle calls them [p131].

She makes two points to start with: firstly, Maddy herself posits a possible non-causal variety of astrology which deals with some sort of 'supernatural vibrations' and is thus not caught by D1. Maddy thus ultimately relies on D2 as her defence to the astrology objection. Secondly, Dieterle reminds us that we should not be tempted to simply dismiss Maddy as inconsistent to rely on D2's indispensable argument, given that she has already rejected this argument when considering Quine's holism. This is because there, Maddy was attacking the use of the indispensability argument to prove the *objective existence* of mathematical objects, whereas here it is being used to justify the role of mathematics itself (this ambiguity is also noted by Rosen). However, notwithstanding these points, Dieterle argues that D1 and D2 whether taken alone or together, still lead to other difficulties for Maddy.

In relation to D1 Maddy claims that mathematics has *nothing* to say about the domain of natural science. But if this is so, asks Dieterle, how can mathematics be *indispensable* to science, as claimed by D2? If mathematics is just a tool for scientists to use (and Dieterle draws attention to Maddy's discussion of Feynman's use of continuum mathematics which, she says, suggests Maddy is of this view), even if it is an *indispensable* tool, it has that role only in relation to applied mathematics, so its justification depends on science and mathematical naturalism therefore falls away to leave scientific naturalism. But if it is not just a tool (if, for example, it works by being a model of the physical world), then it must have something to say about the physical world, so D1 is not made out and again we are left with scientific naturalism because mathematics will be as subject to scientific evaluation as causal astrology is. Thus, Dieterle argues that to maintain both D1 and D2 is inconsistent.

Dieterle goes on to consider D2 on its own, in the context of an imagined variety of theology which was non-causal so not caught by D1 but which was still indispensable to science because in it 'God' could provide an explanation for anything which science itself could not explain. It might be objected here that this is a contradiction because 'God' is conceived of as both non-causal *and* as figuring in physical explanations but Dieterle argues that in that case, mathematics is caught by the same contradiction as *ex hypothesi* it is supposed to be non-causal yet still indispensable to science. Thus, if we accept mathematical naturalism and hold mathematics as immune from scientific evaluation yet still justified, then we are committed to the same view concerning non-causal theology and any other similar theory.

Rieger, like Rosen, raises the objection that Maddy seems to need to fall back on the indispensability arguments she has rejected when it comes to defending mathematical naturalism from the astrology problem and he refers to Dieterle's article discussed above. He also takes up Maddy's claim that mathematics is completely immune from extra-mathematical criticism so that even if mathematicians were to decide that '2 + 2 = 5', provided they said their reasons for doing were mathematical, nothing could be done, and he remarks that 'something is surely amiss here'.

2.5 Conclusions (2): the astrology objection and success

The astrology objection, in the various forms discussed above, says in essence that either (a) Maddy's naturalism entails absurdities such as the claim that astrology is true or that mathematicians can decide that 2 + 2 = 5 or (b) that her naturalism relies on the very indispensability of mathematics to science that she purports to reject. This question turns on how we take the notion of 'justification'. As Rosen remarks, if we take it as implying 'choice-worthy', Maddy is not caused any difficulty by the astrology objection. On this reading, astrological statements are justified by sociological examination of their acceptance by the astrological community, whose word on their inclusion in the 'theory' is final. The disadvantage of this is one Rosen remarks on, namely that the 'justification' arrived at by this route is fairly unremarkable, but at least this reading has the advantage of enabling Maddy to meet the astrology objection. This is because it no longer amounts to a *reductio ad absurdum* to say that if mathematical naturalism is accepted, then astrology becomes the arbiter of its own success, as success is defined on its own terms.

On the other hand, if we take 'justification' to mean 'reason for believing', then Maddy does face the difficulties Rosen and Dieterle focus on: how to avoid classing astrology as credible and how to do so without appealing to the extra-mathematical notion of indispensability to *science*. It is this, rather than Maddy's attack on the indispensability argument, which arguably poses the major obstacle for mathematical naturalism and Maddy does not provide a convincing rebuttal to the problems raised. In addition, her 'wild' example of the consequences of her view (the '2 + 2 = 5 for social reasons' scenario) does not assist her defence: the obvious response is that this is just that, a 'wild' example and not one which in practice could arise. However, Davis and Hersh observe that during the rule of Chairman Mao, Chinese mathematicians were ordered to adopt only that mathematics which was socially useful. A delegation of American mathematicians visiting a Chinese university asked if the beauty of mathematics was not a good reason for studying it and the response was that 'Before the Cultural Revolution some of us believed in the beauty of mathematics but failed to solve practical problems; now we deal with water and gas pipes, cables and rolling mills. We do it for the country and the workers appreciate it. It is a beautiful feeling.' [Davis and Hersh p88]. On Maddy's reading, that is a deviant view and not accepted by the whole community of mathematicians, but if that situation had continued and perhaps spread to other countries, it might have some stage have become the majority view of mathematicians in the world. It is a consequence of mathematical naturalism that at that point, 'maximise' and 'unify' would no longer be the goals of mathematics and set theory would have no justification, and the question is whether mathematical naturalism is a tenable doctrine in that light.

Can Maddy take a position somewhere between Rosen's two alternatives and thus defend her positive thesis (mathematical naturalism) in a meaningful sense, or must she be content with arguing her negative thesis (against Quine's indispensability argument and holism)? Some considerations on the nature of 'justification' might lead to a position from which mathematical naturalism can be defended. The first point is that in the weak sense, even (the study of) causal astrology can be justified, either on the grounds that it makes a huge amount of money for its practitioners or that it comforts its customers, both of which are perfectly rational goals for those practitioners to follow. The subject can also be considered successful on internal grounds (consistency, agreement of the community etc) or on other external grounds: as Rosen notes, anthropologists could study it for purely anthropological reasons, even though it is useless for science. It is not entirely 'unremarkable', as Rosen claims, that one should look to the astrological community for the last word on whether some new astrological "axiom" (example: should the discovery of a planet beyond Pluto change planetary astrology?) should be accepted. One can imagine a non-expert (from the astrologer's viewpoint) giving an answer which was totally at odds with the view of the astrological community so that even the customers would feel short changed by it. Second, while causal astrology is not indispensable for science and is not part of it (indeed, is contradicted by it), its success can be measured by scientific means: whether astrologers earn a lot of money and whether they comfort their customers (or even have genuine beneficial effects on them) are scientific questions, as Maddy herself notes [p204n]. Third, some astrological theories might turn out to have scientific applications and thus become part of science (example: much work has been done by psychologists on whether there is a statistical correlation between birth-sign and character). Fourth, none of these considerations is a reason to believe any non-applied astrological statement.

Of course, none of the above considerations will appeal to one who considers the notion of astrology as a self-justifying subject too absurd to accept (as this assumption of absurdity underwrites use of the astrology objection as a *reductio ad absurdum* against Maddy). Thus Rosen [Rosen p471] considers it 'obviously absurd' that a theology which achieved success by its own lights should be considered authoritative. Putnam expresses the same thought, pointing out that theology is either inconsistent or, if made consistent, would be trivial [Putnam p73]. He implies a definition of a 'successful' theory: the construction of a 'highly articulated body of theoretical knowledge with a long tradition of successful problem solving is a truly remarkable *social* achievement'. He later remarks that 'in the long run, true ideas are the ones that succeed...' [p269]. The difficulty with this is that we can see from everyday life that astrology (to take just one example of a non-scientific discipline) is as popular as ever and astrologers as rich as ever. Sutherland says, based on opinion polls and other empirical sources, that '...in the Western world three-quarters of adults accept at least some psychic phenomena as genuine. For example, the majority of people both in Britain and the US think there is something in astrology.' [Sutherland p309]. Thus, to turn Putnam's reasoning against him, if you manage to arrive at a definition of success which does not include astrology, it is likely to be one which is so *ad hoc* (as it was designed to keep astrology out and include mathematics) as to be trivial.

The most tempting *ad hoc* definition - that a successful theory is one supported by scientific evidence - is of course one that is ruled out to the mathematical naturalist, who does not look to science for justification of mathematics. But this is not to say that Maddy adopts an entirely relativistic position: she denies that the naturalistic philosopher advocates acceptance of any and every conceptual scheme [Maddy 2001 p56]. Maddy does after all accept Quine's argument that Carnap's 'pragmatic' grounds for choosing a theory are no different from ordinary scientific grounds for accepting theories. In evaluating astrology she therefore starts from within science and if by means of anthropology, sociology and so on she finds that astrology makes false claims about how the world is, then she holds the norms of astrology to be 'outright incorrect' [p57]. On the other hand, if she finds that astrology is pursued for other reasons (e.g. as a sort of psychoanalytic process) she will not reach such a conclusion. Maddy could argue therefore that the role of the physicist is to disprove any astronomical claim made by the astrologer and the role of the psychologist is to disprove any therapeutic claim he makes. But the role of the philosopher is to point out to the astrologer (or more urgently, to the astrologer's customer) that *if* you claim astrology makes true astronomical statements, or *if* you claim it has therapeutic effects *then* your claim is subject to scientific evaluation. Furthermore, even if you claim that astrology works on principles entirely unknown to science and thus constitutes a new 'framework', the only rational method of choosing whether to *adopt* that framework is that of science. Lastly, if my scientific enquiry finds that astrology does not make true physical statements but *does* have a therapeutic effect, then your only justification for accepting it is the therapeutic one.

There remains the dilemma that while Maddy wanted mathematics to be accorded the privilege, like science, of supporting itself by its own bootstraps, it seems on this approach that mathematics enjoys that privilege only on the same terms as astrology does: namely, insofar as science deems it acceptable. So while the justification for a particular theory *within* mathematics/astrology is entirely a matter for mathematicians, (a) our reason for accepting this justification in the first place is that science indicated that we were justified in choosing to accept the mathematical/astrological framework and (b) it is science that ultimately adjudicates on whether a proposed new mathematical/astrological statement is a properly intra-theoretical one and not a scientific statement in disguise. This suggests, however, that scientific naturalism takes on an imperialistic role once adopted: if the acceptance of any other framework is to be judged in the first place by scientific standards, can the scientific naturalist ever 'go native' and begin judging theories from *within* another framework?

Maddy later [2003 p12] seems to accept these implications of scientific naturalism and speculates that if Descartes' Method of Doubt (which she rejects as 'first philosophy') turned out to generate beliefs judged *by scientific means* to be reliable, then she would be inclined to describe his Method as itself 'scientific'. This echoes Strawson's argument [Strawson p259]: if someone said he arrived at answers to questions by shutting his eyes, asking the question and accepting the first answer that came into his head, his method would be inductively supported if it usually turned out to be right. The question then remains: if like Maddy we arrive at mathematical naturalism via scientific naturalism, in what sense is mathematics immune from external criticism? Granted, it is consistent with scientific naturalism to hold mathematics (like science) to be immune from first philosophy. But Maddy argues (on the basis that mathematics is like science a highly successful theory and therefore to be accorded the same privilege as science) that it is inconsistent with scientific naturalism to hold mathematics to account to science [p184]. However, the logic of accepting scientific naturalism, if followed to its conclusion, seems to entail that the only way of *reliably* evaluating any theory is from within science, where 'evaluating' includes both the initial Carnapian 'pragmatic' choice of theory and, once inside the theory, an ongoing 'reality check' against scientific principles.

The conclusion that might therefore be argued is that you can have an autonomous practice which is successful and whose success is underwritten by science, while the practice itself is not part of science and not indispensable to science. The practice can have its own goals and norms and any new statement is to be justified only by the practising community, while not thereby becoming *believable* unless and until it becomes applied to science and thus taken over by science. This privilege of self-assessment need not be accorded to every discipline, but only to 'successful' ones, the definition of success being restrained by general rational principles. Thus a successful discipline which adopted theories suited to the efficient achievement of its goals would earn the status of its own naturalism, others would not. The criteria for 'success' (of the theory as a whole, rather than individual "axioms" inside it) could be scientific ones or could be internal ones, or both. The criteria for the rationality of the pursuit of the goals (though not of the choice of goals) could be based on generally accepted notions of logic and reason, common to all disciplines.

Lastly, the question of the ultimate truth of the statements of the discipline would be one for first philosophy (and could thus be ruled out of contention), while the naturalistic philosopher would be content with a sociological role in examining the discipline in question. On this basis, mathematical naturalism could be defended, but the defence (a) *could* also include that of other disciplines such as astrology and (b) would not allow the *justification* of the statements of the discipline to be adjudicated upon outside of that discipline unless and until those statements became applied to science. The question is whether this would be too high a price to pay for acceptance of mathematical naturalism, leading one to perhaps accept Maddy's argument against the indispensablility argument and holism, but to seek an alternative approach thereafter to the question of the justification of mathematics.

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