In Defence of Indispensability

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Indispensability arguments for mathematical realism have been around a long time in various forms, but the modern formulation due to Quine [1980] and Putnam [1979] is an argument that implores us to give mathematical entities (numbers, functions, sets and such) the same ontological standing as other entities in our best scientific theories (electrons, neutron stars, quarks, and such). This argument, when properly appreciated, is a very powerful and persuasive device for warding off nominalism. While, strictly speaking, it does not establish mathematical realism as its conclusion, it does create a serious problem for scientific realists who refuse to admit mathematical entities into their ontologies. This argument, however, has suffered attacks from seemingly all directions in recent times. First Charles Chihara [1973], then Hartry Field [1980], raised doubts about the indispensability of mathematics to science, and more recently Elliott Sober [1993] and Penelope Maddy [1992], [1995a], [1995b] have raised separate doubts as to whether we really ought to be committed to entities which are indispensable to our best scientific theories.

In the face of these worries, indispensability arguments do not enjoy the widespread support they used to. Indeed, for mathematical realists such as myself, who think that indispensability arguments offer the *only* good reason for that realism, these are worrying times! In this paper I will address what I take to be the most serious of the attacks on indispensability—the Maddy objections—and I show that indispensability arguments can survive this attack. In particular, I will be defending the Quine/Putnam version of the argument against Maddy's claim that there are internal tensions between the doctrines of naturalism and confirmational holism. As we shall see, both these doctrines are crucial to the indispensability argument, so it is important that they be mutually consistent. I want to make it clear at the outset, that in this paper I will not be engaging in the project of providing independent support for naturalism or holism; I will only be concerned with showing that the internal tensions Maddy points to are

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apparent rather than real.

In the next section I outline the indispensability argument and discuss its reliance on the doctrines of naturalism and holism. In Section 2, I outline the three Maddy objections to this argument, followed in Section 3 by a discussion of Maddy's conception of the doctrine of naturalism and, in particular, how this conception differs from the Quinean conception. Then in Section 4, I defend the indispensability argument against each of Maddy's objections, taking particular notice of the role naturalism plays in these objections. This is followed by a brief conclusion.

1. Indispensability, Naturalism and Holism

Firstly let's review the Quine/Putnam indispensability argument. The argument has two premises:

1. We (ought to) have ontological commitment to all and only those entities that are indispensable to our best scientific theories (Quinean Ontic Thesis).[1]
2. Mathematical entities are indispensable to our best scientific theories.

From these we conclude that we (ought to) have ontological commitment to mathematical entities.

Since Field's challenge to the second premise in *Science Without Numbers* (Field [1980]) there has been much discussion on the issue of the indispensability of mathematics to science. I won't enter into that debate here. I take it (as does Maddy) that it is a fact about our scientific theories that mathematics *is* an indispensable part of those theories.[2]

Instead, I wish to concentrate on what is, in some ways, a more fundamental attack on the indispensability argument—Penelope Maddy's claim that the first premise of the argument is not supported by scientific/mathematical practice. In order to understand her attack, though, we first need to look at the motivation for the Quinean Ontic Thesis, which lies in the doctrines of naturalism and holism.

Naturalism is the denial of a first philosophy. As Quine rather famously puts it:

> ... naturalism: abandonment of the goal of a first philosophy. It sees natural science as an inquiry into reality, fallible and corrigible but not answerable to

[1] Quine actually speaks of those entities that are existentially quantified over in the canonical form of our best scientific theories, rather than indispensability. The difference (if any) need not concern us here.

[2] Although the meaning of the terms 'indispensable' and 'dispensable' are extremely unclear in this context, for the most part this need not concern us, since, as I've already mentioned, Maddy agrees that mathematics is indispensable, at least on any intuitive reading of 'indispensable'. For the record though, I take 'dispensability' to be defined as follows: An entity is dispensable to a theory if there exists a modification of that theory resulting in a second theory, functionally equivalent to the first, in which the entity in question is neither mentioned nor predicted. Furthermore, the second theory must be preferable to the first. See Colyvan [forthcoming] for further details.

> any supra-scientific tribunal, and not in need of any justification beyond observation and the hypothetico-deductive method. ... The naturalistic philosopher begins his reasoning within the inherited world theory as a going concern. He tentatively believes all of it, but believes also that some unidentified portions are wrong. He tries to improve, clarify, and understand the system from within. He is the busy sailor adrift on Neurath's boat. (Quine [1981], p. 72)

For instance, when faced with the task of legitimising (or not) current scientific practice the philosopher of science does not occupy some privileged, pre-scientific position, rather, s/he works from *within* the scientific program, using the methods prescribed by that program.

This high regard for scientific methodology has an important consequence for ontology. When asking questions about what exists, naturalism counsels us to look to science for the answers—we are to find what exists in our best scientific theories. Naturalism rules out other means of being granted real status. For instance, if there are no scientific grounds for believing in ghosts, then there are *no* grounds for believing in ghosts. Thus naturalism ensures that *only* entities in our best scientific theories ought to be granted real status. The Quinean Ontic Thesis, however, states that we ought to believe in *all and only* those entities indispensable to our best scientific theories. I take it that naturalism alone doesn't ensure that *all* the entities indispensable to our best scientific theories ought to be granted real status. This, however, is ensured by holism.

There are at least two distinct doctrines in Quine's philosophy that go by the name of 'holism'. These are usually referred to as *semantic holism* and *confirmational holism*. The first is the thesis that the unit of meaning is not the single sentence, but the whole language (or at least a large portion of it). This is a result of Quine's famous thesis of indeterminacy of translation.[3] The second, confirmational holism, is the thesis that scientific theories are confirmed (or falsified) as wholes. As Quine puts it:

> [T]he falsity of the observation categorical[4] does not conclusively refute the hypothesis. What it refutes is the conjunction of sentences that was needed to imply the observation categorical. In order to retract that conjunction we do not have to retract the hypothesis in question; we could retract some other sentence of the conjunction instead. This is the important insight called *holism*. (Quine [1992], pp. 13–14)

It is only confirmational holism that is required for the indispensability argument.[5] We simply note that if some theory is confirmed by empirical evidence, then the *whole* theory is confirmed, so naturalism implores us to believe in the existence of *all* the entities of the confirmed theory.

[3] See Quine [1960], pp. 26-79 for details.

[4] By 'observation categorical' Quine simply means a statement of the form 'whenever P, then Q'. For example, 'where there's smoke, there's fire'.

[5] Cf. Resnik [1995] and Hellman [forthcoming]

Maddy accepts the naturalistic backdrop, but, as we shall see, she has some serious reservations about how well confirmational holism accords with that backdrop. So, having outlined the indispensability argument and the motivation for the first premise, which is the one that Maddy finds objectionable, I will move on to discuss Maddy's objections.

2. Maddy's Objections

Although Maddy's three objections to indispensability arguments are largely independent of one another, there is a common thread that runs through each of them. Each draws attention to problems of reconciling naturalism and confirmational holism. In particular, she points out how a holistic view of scientific theories has problems explaining the legitimacy of certain aspects of scientific and mathematical practices, which presumably *ought to be* legitimate given the high regard for scientific practice that naturalism endorses. That said, I will now outline each of Maddy's objections separately.

2.1. The Scientific Fictions Objection

The first objection to the indispensability argument, and in particular to confirmational holism, is that the actual attitudes of working scientists towards the components of well confirmed theories vary 'from belief to grudging tolerance to outright rejection' (Maddy [1992], p. 280). In 'Taking naturalism seriously' (Maddy [1994]) Maddy presents a detailed and concrete example which illustrates these various attitudes. The example is the history of atomic theory from early last century, when the (modern) theory was first introduced, until early this century, when atoms were finally universally accepted as real. The puzzle for the Quinean 'is to distinguish between the situation in 1860, when the atom became "the fundamental unit of chemistry", and that in 1913, when it was accepted as real' (Maddy [1994], p. 394). After all, if the Quinean Ontic Thesis is correct then scientists ought to have accepted atoms as real once they became indispensable to their theories (presumably around 1860) and yet renowned scientists such as Poincaré and Ostwald remained sceptical of the reality of atoms until as late as 1904.

For Maddy the moral to be drawn from this episode in the history of science is that 'the scientist's attitude toward contemporary scientific practice is rarely so simple as uniform belief in some overall theory' (Maddy [1994], p. 395). Furthermore, she claims that '[s]ome philosophers might be tempted to discount this behavior of actual scientists on the grounds that experimental confirmation is enough, but such a move is not open to the naturalist' (Maddy [1992], p. 281), presumably because 'naturalism counsels us to second the ontological conclusions of natural science' (Maddy [1995a], p. 251). She concludes:

> If we remain true to our naturalistic principles, we must allow a distinction to

> be drawn between parts of a theory that are true and parts that are merely useful. We must even allow that the merely useful parts might in fact be indispensable, in the sense that no equally good theory of the same phenomena does without them. Granting all this, the indispensability of mathematics in well-confirmed scientific theories no longer serves to establish its truth.(Maddy [1992], p. 281)

I will not discuss my reply to this or any other of her objections until I present them all, since at least some of my remarks will apply to more than one of them.

2.2. The Role of Mathematics in Science

The next problem for indispensability that Maddy suggests follows on from the last. Once one rejects the picture of a scientific theory as a homogeneous unit, the next question concerns whether the mathematical portions of theories fall within the true elements of the confirmed theories. To answer this question Maddy firstly points out that much mathematics is used in theories that make use of hypotheses that are explicitly false, such as the assumption that water is infinitely deep in the analysis of water waves or that matter is continuous in fluid dynamics. Furthermore, she argues that the hypotheses are indispensable to the relevant theory, since it would be unworkable without them, but it would be foolish to argue for the reality of the infinite simply because it appears in our best theory of water waves (Maddy [1995a]).

Next she looks at instances of mathematics appearing in theories not known to contain explicitly false simplifying assumptions and claims that '[s]cientists seem willing to use strong mathematics whenever it is useful or convenient to do so, without regard to the addition of new *abstracta* to their ontologies, and indeed, even more surprisingly, without regard to the additional physical structure presupposed by that mathematics' (Maddy [1995a], p. 255). In support of this claim she looks at the use of continuum mathematics in physics. It seems the real numbers are used purely for convenience. No regard is given to the addition of uncountably many extra entities (from the rationals, say) or to the seemingly important question of whether space and time (which the reals are frequently used to model) are in fact continuous or even dense. Nor is anyone interested in devising experiments to test the density or continuity of space and time. She concludes that '[t]his strongly suggests that *abstracta* and mathematically-induced structural assumptions are not, after all, on an epistemic par with physical hypotheses' (Maddy [1995a], p. 256).

2.3. The Mathematical Practice Objection

Maddy begins this objection by noting what she takes to be an anomaly in Quinean naturalism, namely that it seems to respect the methodology of empirical science but not of mathematics. It seems that, by the indispensability argument, mathematical ontology is legitimised only insofar as it is useful to empirical science. This, claims Maddy, is at odds with actual mathematical practice, where theorems of mathematics are believed because they are proved from the relevant axioms, *not* because such theorems are useful in applications (Maddy [1992], p. 279). Furthermore, she claims that such a 'simple' indispensability argument leaves too much mathematics unaccounted for. Any mathematics which does not find applications in empirical science is apparently without ontological commitment. Quine himself suggests that we need some unapplied mathematics in order to provide a simplificatory rounding out of the mathematics that is applied but '[m]agnitudes in excess of such demands, *e.g.*, $\beth_\omega$ or inaccessible numbers' should be looked upon as 'mathematical recreation and without ontological rights' (Quine [1986], p. 400).[6]

Maddy claims that this is a mistake, as it is at odds with Quine's own naturalism. Quine is suggesting we reject some portions of accepted mathematical theory on non-mathematical grounds. Instead she suggests the following modified indispensability argument:[7]

> [T]he successful application of mathematics gives us good reason to believe that there are mathematical things. Then, given that mathematical things exist, we ask: By what methods can we best determine precisely what mathematical things there are and what properties these things enjoy? To this, our experience to date resoundingly answers: by mathematical methods, the very methods mathematicians use; these methods have effectively produced all of mathematics, including the part so far applied in physical science. (Maddy [1992], p. 280)

This modified indispensability argument and, in particular, the respect it

[6] More recently Quine has refined his position on the higher reaches of set theory and other parts of mathematics which are not, nor ever likely to be, applicable to natural science. For instance, in his most recent work, *From Stimulus to Science*, he suggests:

> They are couched in the same vocabulary and grammar as applicable mathematics, so we cannot simply dismiss them as gibberish, unless by imposing an absurdly awkward gerrymandering of our grammar. Tolerating them, then, we are faced with the question of their truth or falsehood. Many of these sentences can be dealt with by the laws that hold for applicable mathematics. Cases arise, however (notably the axiom of choice and the continuum hypothesis), that are demonstrably independent of prior theory. It seems natural at this point to follow the same maxim that natural scientists habitually follow in framing new hypotheses, namely, simplicity: economy of structure and ontology. (Quine [1995], p. 56)

A little later, after considering the possibility of declaring such sentences meaningful but neither true nor false, he suggests:

> I see nothing for it but to make our peace with this situation. We may simply concede that every statement in our language is true or false, but recognize that in these cases the choice between truth and falsity is indifferent both to our working conceptual apparatus and to nature as reflected in observation categoricals. (Quine [1995], p. 57)

[7] This suggestion was in fact made earlier by Hartry Field (Field [1980], pp. 4–5) but of course he denies that any portion of mathematics is indispensable to science; so he had no reason to develop the idea.

pays to mathematical practice, she finds more in keeping with the spirit, if not the letter, of Quinean naturalism.

She then goes on to consider how this modified indispensability argument squares with mathematical practice. She is particularly interested in some of the independent questions of set theory such as Cantor's famous continuum hypothesis: Does $2^{\aleph_0} = \aleph_1$? and the question of the Lebesgue measurability of Σ^1_2 sets. The problem though, for indispensability-motivated mathematical realism, is that it is hard to make sense of what working mathematicians are doing when they try to settle such questions, or so Maddy claims.

For example, in order to settle the question of the Lebesgue measurability of the Σ^1_2 sets, two new axioms have been proposed as supplements to the standard ZFC axioms. The two competing axiom candidates are Gödel's axiom of constructibility, $V = L$, and some large cardinal axiom, such as MC (there exists a measurable cardinal). These two candidates both settle the question at hand, but with different answers. MC implies that all Σ^1_2 sets are Lebesgue measurable whereas $V = L$ implies that there exists a non-Lebesgue-measurable Σ^1_2 set. The informed consensus is that $V \neq L$ and that some large cardinal axiom or other is true,[8] but the reasons for this verdict seem to have nothing to do with applications in physical science. Indeed, much of the appeal of large cardinal axioms is that they are less restrictive than $V = L$, so to oppose such axioms would be 'mathematically counterproductive' (Maddy [1995a], p. 265). These are clearly intra-mathematical arguments that make no appeal to applications.

Furthermore, if the indispensability argument is correct, it is quite possible that physical theories would have some bearing on developments in set theory, since they are both part of the same overall theory. For example, Maddy claims that if space-time is not continuous, as some physicists are suggesting,[9] this would undermine much of the need for set theory (at least in contexts where it is interpreted literally) beyond cardinalities such as $\aleph_0$. Questions about the existence of large cardinals would be harder to answer in the positive if it seemed that indispensability considerations failed to deliver cardinalities as low as $\beth_1$. Maddy thus suggests that indispensability-motivated mathematical realism advocates set theorists' looking at developments in physics (theories of quantum gravity in particular) in order to tailor set theory to best accord with such developments.[10] Given that set theorists in general do not do this, a serious revision of mathematical practice is being advocated, and this, claims Maddy, is a violation of naturalism (Maddy [1992], p. 289). She concludes:

[8] There are, of course, some notable supporters of $V = L$. In particular, Quine [1992], [1995] and Devlin [1977].

[9] For example, Richard Feynman ([1965], pp. 166-167) suggests this.

[10] Cf. Chihara ([1990], p. 15) for similar sentiments.

> In short, legitimate choice of method in the foundations of set theory does not seem to depend on physical facts in the way indispensability theory requires. (Maddy [1992], p. 289)

I now wish to defend the indispensability argument against the three objections I outlined in this section. Before I look at each objection in detail though, it will be useful to examine Maddy's conception of naturalism a little more closely, as my reply to her objections depends on a clear understanding of her naturalism. Indeed, I believe that there has been a certain amount of confusion over how naturalism is to be understood in the context of the indispensability argument, and this confusion has allowed her objections to seem more damaging than perhaps they ought.

3. Maddy's Naturalism

It might be argued that there are two ways in which Maddy's conception of naturalism differs from Quine's. The first she points out herself:

> On this view [*i.e.*, Quinean naturalism], the philosopher occupies no privileged position from which to critique the practice of natural science; if philosophy conflicts with that practice, it is the philosophy that must give. As a philosopher of mathematics, I extend this compliment to the practice of classical mathematics as well. (Maddy [forthcoming])

She then remarks in a footnote that '[i]t isn't clear that Quine would approve this extension' (Maddy [forthcoming]). The result of this extension is seen in her modification of the Quine/Putnam indispensability argument which I discussed in Section 2.3. Recall that she finds Quine's rejection of quantities such as $\beth_\omega$ against the spirit of naturalism, since accepted mathematical practice is rejected on non-mathematical grounds. I shall discuss this departure from Quinean naturalism in more detail when I come to defend the indispensability argument against the mathematical-practice objection. At this stage I merely wish to point out that there *is* a departure and that Maddy recognises this.

The other way in which Maddy's naturalism might be thought to differ from Quinean naturalism is also illustrated in the above quote. It is seen in the move from 'the philosopher occupies no privileged position' to 'if philosophy conflicts with [scientific] practice, it is the philosophy that must give'. Surely the former does not imply the latter. Quinean naturalism tells us that there is no supra-scientific tribunal, whereas Maddy seems to be suggesting that this implies science itself is in a privileged position. That is, the philosopher of science must merely rubber-stamp *any* scientific practice. Elsewhere she echoes this view of naturalism. For example, in 'Set theoretic naturalism' she writes 'the [set theory] methodologist's job is to account for set theory as it is practiced, not as some philosophy would have it be' (Maddy [1996], p. 490).

There is much ground between a first philosophy, which Quine rejects, and the rubber-stamp role which Maddy seems to advocate. For instance, there is the position that science and philosophy are continuous with one another and as such there is *no* high court of appeal. On this view, the philosopher of science has much to contribute to discussions of both scientific methodology and ontological conclusions, as does the scientific community. It may be that one is inclined to give more credence to the views of the scientific community in the eventuality of disagreement between scientists and philosophers, but even this does not imply that it is philosophy that must always give. I take it that this view of science and philosophy as continuous, without either having the role of 'high court', is in fact the view that Quine intends. It seems that Maddy's interpretation of naturalism represents a significant departure from this view.

Unfortunately things aren't that simple, for in 'Naturalizing mathematical methodology' Maddy points out that on her view of naturalism '[c]urrent scientific practice need not be taken as gospel, but as a starting point, as *prima facie* gospel only, subject to ordinary scientific critique' (Maddy [forthcoming]). She then goes on to consider the role of the philosopher:

> How... does the philosophical methodologist differ from any other scientist? If she uses the same methods to speak to the same issues, what need is there for philosophers at all? The answer, I think, is that philosophical methodologists differ from ordinary scientists in training and perspective, not in the evidential standards at their disposal. (Maddy [forthcoming])

The view expressed in the above quotes seems at odds with the previous picture of Maddy's naturalism. In particular, the role of the philosopher suggested in the last quote is decidedly different to the powerless bureaucrat rubber-stamping any scientific practice. Indeed, I have no quarrel with Maddy on the account of naturalism suggested in the above passages.

Which view does she take then? Is it always the philosophy that must give or can philosophers participate as equals in debates on scientific methodology? Before answering these questions I think it's important to emphasize that Maddy's claim is that naturalism implies that in the event of a dispute between philosophy and scientific practice it is the *philosophy* that must give, not that *philosophers* must give. She, like Quine, is against first philosophy no matter who the practitioners are, scientists, philosophers or anyone else. She is careful to point out that the naturalistic enterprise must separate the good philosophy (*i.e.*, the philosophy which is continuous with science) from the bad philosophy (*i.e.*, the first philosophy) and that this is a very difficult enterprise (Maddy [1995a], p. 261). So perhaps rather than 'philosophy must give' she really just means *first* philosophy must give. No doubt Quine would agree with the latter but, as I've already suggested, not the former.

Although in Maddy's writings it is not always clear which of the two formulations of naturalism I've been discussing she endorses, I take it that she does in fact endorse the standard Quinean position of rejecting first philosophy (not *all* philosophy). My evidence for this is, in part, passages such as the ones above from 'Naturalising mathematical methodology', where she is clearly more careful about stating her position and, in part, private communication with Maddy on the matter. So Maddy's naturalism departs from Quine's in only the first way (*i.e.*, she extends naturalism to endorse the practice of classical mathematics), but we must be careful, for she sometimes writes as though she departs in the second way as well (*i.e.*, to endorse the 'philosophy must give' formulation of naturalism). As we shall see in the next section, both these points are important when considering her objections to the indispensability argument.

4. Defending the Indispensability Argument

In this section I will consider the three objections to the indispensability argument raised in Section 2.

4.1. The Scientific Fictions Objection Revisited

Recall that this objection draws attention to the fact that scientists themselves distinguish between the real and the fictional entities in scientific theories. There are two cases to be considered here. The first is the case of scientific fictions that are clearly intended as fictions. I have in mind here such entities as frictionless planes, inertial reference frames, and incompressible fluids. There are a number of reasons for such entities to be taken to be fictional. One reason is that typically the presumed existence of such entities renders inconsistent either the theory in which they occur or another related theory.[11] Given that consistency is one of the more important virtues of scientific theories, any entity that renders the best available theory inconsistent is unlikely to be indispensable to that theory (no matter how useful it is) because there exists a better theory (*i.e.*, a consistent theory) that does not quantify over the entity in question.[12]

The second case is more problematic. Here we have some entity, such as the mid-nineteenth-century atom, which was indispensable to the best available theory, and yet many working scientists of the time treated it instrumentally. Maddy takes this to be a problem for Quinean naturalism, since the naturalistic philosopher of science must 'second the ontological

[11] For another treatment of such cases see Quine [1960], pp. 248–251.

[12] Strictly speaking the assertion of the existence of a single entity doesn't render the relevant theory inconsistent. It is the conjunction of that sentence and the rest of the theory that is inconsistent; however, we can quite rightly place the blame on a single sentence (or existence statement) in certain circumstances. Consider the example of the frictionless plane. Appeal to frictionless planes simply makes the statement of certain laws of mechanics easier, so omitting such appeals makes little difference to the overall theory. On the other hand, to assert the existence of frictionless planes would require a great deal of modification to existing theory to explain how such an entity as a frictionless plane would be possible, given our current understanding of frictional forces. So, to be more precise, I should say that the frictionless planes are *dispensable* to the theory of mechanics.

conclusions of natural science' (Maddy [1995a], p. 251). Here she writes as though naturalism prohibits *any* philosophical critique of scientific methodology, but, as we saw in the previous section, this is a mistake; this is not the way Maddy understands naturalism. Once this misconception is cleared up we see that the door is open for a critique of the sceptical scientists from a philosophical perspective located *within* the scientific enterprise. The naturalistic philosopher can point to what Putnam ([1979], p. 347) calls the 'intellectual dishonesty' of using atoms, say, in our best chemical theories, then denying the existence of these very same atoms. This is not to say that the philosopher occupies any privileged position in this debate, but neither is s/he without power.

It may be that scientists such as Poincaré, who were reluctant to believe in the existence of atoms, were being unduly influenced by some non-naturalistic philosophy (such as verificationism). Here the role of the (naturalistic) philosopher of science is clear: try to convince the scientists in question of the benefits of naturalism and of the consequences for the matter at hand. Again I stress that there is no first philosophy in this strategy, just fair interplay of ideas as one would expect in the holistic, naturalistic, Quinean vision of science.[13]

From what I have said so far, it seems that the Quinean must think that those scientists refusing to believe in atoms prior to 1904 were doing something wrong. Maddy obviously disagrees; she thinks that these scientists were right and that something is wrong with the Quinean position. The crux of this objection, then, seems to rest on which way your intuitions go on this and other such episodes in the history of science. I'm inclined to think that the scientists in question *were* wrong in this case, but I appreciate that many would not share my intuitions here, so let me investigate briefly other possible responses the Quinean might make.

One alternative is to deny that atoms were indispensable to science prior to 1904; however, this seems unpromising. Another is to consider the possibility of the Quinean Ontic Thesis applying only to cases where the theory in question is well accepted amongst the scientific community (*i.e.*, during periods of what Kuhn calls 'normal science'). The suggestion is simply that in cases where the best theory is controversial, for whatever reasons, one may suspend judgment on the ontological commitments of the theory. Similarly, one might think that ontological commitment is not an all or nothing affair—we could have degrees of belief in theories and, in particular, to the ontological commitments of those theories.[14] If this is correct

[13] In fact, I think that those scientists who treated the atomic hypothesis instrumentally were adhering to verificationist philosophical principles. One wonders whether a similar incident could occur now in less verificationist times—I suspect not.

[14] One might think that a confirmational holist is committed to belief or disbelief in whole theories, so that a differential degree of belief in parts of theories is not an option.

then we have two alternatives: (i) the controversy over atomic theory at the time gives us good reason to think that prior to 1913 chemistry/atomic theory was in a crisis period and thus the Quinean could suspend judgment on the ontological commitments of the theory (indeed, this may be all that Ostwald and Poincaré were doing); (ii) one could argue that given the evidence at the time it would be unwise to give total commitment to either the existence or the non-existence of atoms—some degree of belief strictly between zero and one would be appropriate (again Ostwald and Poincaré's insistence on more evidence could be taken to be nothing more than this). While I shall not pursue these two alternatives any further here, they do seem to be promising replies to the Maddy objection which avoid charging Ostwald and Poincaré with 'intellectual dishonesty'.

One other point worth noting, before moving on, is that the Quinean picture of science is not necessarily intended to be in accordance with every episode in the history of science. Presumably science can go wrong, and when it does, it will not accord with the Quinean picture. Quinean naturalism is, in part, a normative doctrine about how we ought to decide our ontological commitments; it is not purely descriptive. This is not to say that Maddy's example of nineteenth-century atomic theory is a case where science went wrong. On the contrary, I think some scepticism towards novel entities such as atoms is a healthy part of the scientific method.

4.2. The Role of Mathematics in Science Revisited

Recall that this is the objection that scientists seem willing to use whatever mathematics is required, without regard to ontic commitment.

Given my remarks on Maddy's naturalism and my consequent reply to the scientific fictions objections, my reply to this objection is predictable, I think. Firstly, I claim that in cases where mathematics is used in blatantly false hypotheses, such as infinitely deep water in physical theories of waves, we need draw no ontological conclusions from the mathematics used, since the theory as a whole is not taken to be literally true. Maddy and I agree thus far![15] Furthermore, I suggest that there is no essential difference between these cases and the case of a physicist using a strong mathematical theory which carries with it certain physical assumptions (such as that

This is not the case though. Even a confirmational holist such as Quine must decide which parts of a disconfirmed theory are to be rejected and which are to be retained. Such decisions are made by appeal to pragmatic considerations such as simplicity. It seems plausible, at least in the case of a disconfirmed theory, that when it is not clear which part of the theory is to be rejected, one may have different degrees of belief in the various parts.

[15] Michael Resnik [1995] recently presented a very interesting 'pragmatic indispensability argument' in which he argued that the truth of mathematics is presupposed when doing science, even when the scientific theory in which it is being used is false (and even if it is *known* to be false). Although I have some sympathy with this view, for the sake of the present discussion at least, I will take the less controversial line of drawing ontological conclusions only from theories believed to be true.

space-time is continuous). We no more accept that space-time is continuous because of our use of the reals to model it than we believe that our oceans are infinitely deep because this assumption is sometimes necessary when describing waves. The only difference here is that the latter is clearly false while the former is an open question.

What of the mathematics that appears in theories believed to be true? Here Maddy suggests that the naturalistic philosopher must endorse the views of working scientists, which is simply to use whatever mathematics is convenient, without regard for its apparent ontological commitment and, in particular, without affirming the existence of the entities they are using. As in the previous section, I simply deny that naturalistic philosophers must endorse such apparently dishonest behaviour. I am not suggesting that the naturalistic philosopher need be so heavy handed as to attract the charge of 'practising first philosophy', but nonetheless such a philosopher is not without the power to enter into debate with such scientists about their alleged metaphysical dishonesty.[16]

Furthermore, it is not clear that this *is* the attitude working scientists have towards the mathematics they use. In Maddy [1995a] she cites the example of Richard Feynman's use of real analysis to describe motion, despite his misgivings about the continuity of space and time.[17] While it is clear that Feynman is using real analysis because it is convenient, it is not clear that he is doing so without regard for the ontological commitment. After all, real analysis is ubiquitous in modern physics, so perhaps Feynman is thinking that whatever ontological load comes with the use of real analysis is already being carried. Then, given that real analysis *would* be convenient to use in describing motion, there seems no reason not to use it.[18] Contrast this case with the controversy surrounding the *first* usage of calculus in the seventeenth century.

My claim, then, is that scientists do not worry too much about the ontological commitments of some mathematical theory, if that theory is already widely used (such as in Maddy's Feynman example). On the other hand, when some *novel* mathematical theory or entities are introduced,

[16] Certainly the portrayal of the difference between first philosophy and continuous philosophy as a matter of being 'heavy handed' or not is a bit of a caricature, but like all caricatures there is some truth in it. First philosophy is unwilling to compromise; continuous philosophy is not. In any case, I suspect that we can do no better than such vague characterisations, although I won't be pursuing the matter in this paper. Whether first philosophy is afoot is determined on a case-by-case basis and by careful attention to the details of the cases. On this Maddy clearly agrees.

[17] The Feynman work she refers to is Feynman *et al.* [1963].

[18] This might seem implausible, since surely the description of motion was one of the first uses of real analysis; so to represent it as I have here is anachronistic. I agree, but Feynman is presenting the material in an undergraduate physics textbook (Feynman *et al.* [1963]) *as though* this were the first time that real analysis had been put to such a use.

it seems that scientists do worry about the mathematics in question. As I've already suggested, the earliest usage of calculus and, in particular, infinitesimals, seems a clear example of this. Another, more recent, example is the introduction of the *Dirac delta function* to quantum physics.

In order to get around certain problems (such as differentiating a step function) it was necessary to appeal to a 'function', $\delta : \mathbb{R} \to \mathbb{R}$, with the following properties:

$$\delta(x) = 0, \quad \forall x \neq 0,$$

$$\int_{-\infty}^{+\infty} \delta(x)\, dx = 1.$$

The delta function, although very useful, is a rather strange entity and its usage naturally attracted much criticism. Even Dirac, who first introduced the function, was not without some concern:

> [A]lthough an improper function [*i.e.*, a Dirac delta function] does not itself have a well-defined value, when it comes as a factor in an integrand the integral has a well-defined value. In quantum theory, whenever an improper function appears, it will be something which is to be used ultimately in an integrand. Therefore it should be possible to rewrite the theory in a form in which the improper functions appear all through only in integrands. One could then eliminate the improper functions altogether. The use of improper functions thus does not involve any lack of rigour in the theory, but is merely a convenient notation, enabling us to express in a concise form certain relations which we could, if necessary, rewrite in a form not involving improper functions, but only in a cumbersome way which would tend to obscure the argument. (Dirac [1958], pp. 58, 59)

If, as Maddy claims, physicists are inclined simply to use whatever mathematics is required to get the job done, without regard for ontological commitments, why was Dirac so intent on dispelling doubts about the use of his new 'function'?

You might be inclined to think that Dirac's (and other's) concerns[19] were entirely concerns about rigour and/or consistency. Indeed, you might think that whenever physicists are concerned about the introduction of new mathematics their only concerns are concerns about rigour and/or consistency. In that case, Maddy could argue that, while her claim that physicists will use whatever mathematics is required is not quite correct, nonetheless concerns about ontology never constitute a reason for concern over the legitimacy of a piece of mathematics. Certainly concerns about rigour and consistency played important roles in the initial controversy

[19] Dirac's informal argument justifying the use of the delta function went some way to dispelling those concerns, and certainly the 'function' continued to be used, albeit with reservations. The reservations, however, continued until the mathematical theory of distributions was developed to justify the delta function's usage rigorously. As it turns out, the Dirac delta function is not a function at all but a distribution.

surrounding both infinitesimals and the delta function. That much is clear. It is less clear that these were the *only* concerns. It would be an interesting exercise to try to disentangle the issues of rigour and ontology in such cases. Fortunately this is not required for the task at hand, as there are other cases where concerns over the introduction of novel mathematical entities are extremely difficult to interpret as being purely about rigour and/or consistency. The first use of the complex numbers to solve quadratic equations by Cardan, around 1545, springs to mind as a case of a consistent theory over which there was considerable debate.

Unlike the cases of infinitesimals and the Dirac delta function, it appears that it was primarily the unusual nature of the entities concerned that worried those making the earliest use of complex number theory. The controversy was over whether the strange new entity $i = \sqrt{-1}$ was a number. Descartes for one thought not, and introduced the term 'imaginary' for complex roots of quadratics (see Kline [1972]). Others who were suspicious of complex numbers included Newton (*ibid*, p. 254) and even Euler, who, in 1768–69, claimed that complex numbers 'exist only in imagination' (*ibid*, p. 594). In particular, Newton's suspicions were seemingly due to the lack of physical significance of complex roots (*ibid*, p. 254); nothing to do with rigour. Let me make it clear, though, that I'm not claiming there were *no* concerns about rigour in the debate over the use of complex numbers, it's just that such concerns, if they existed, were secondary to what appear to be ontological concerns.

It is also interesting to note in relation to this case that although complex numbers were used in other areas of mathematics and that work on the algebra of complex numbers continued, despite concerns about their use, often proofs appealing to complex numbers were supplemented with proofs that made no such appeals. It wasn't until Gauss's proof of the fundamental theorem of algebra (in 1799), which made essential reference to complex numbers, and until physical applications for complex function theory were developed (also in the latter part of the eighteenth century) that controversy over the usage of complex numbers gradually began to subside (see Kline [1972], p. 595). In both cases applications were important: the former an intra-mathematical application, the latter a physical application.

Whether controversy surrounding the use of novel mathematical entities in physical theories is widespread or not I am in no position to say, but at least it seems that there are *some* cases where physicists are genuinely suspicious of new mathematical entities. Furthermore, in some of these cases it seems extremely plausible that the concerns were, at least in part, concerns about ontology. In any case, even if physicists *did* use whatever mathematics was required, without regard for ontological considerations, this would not imply that the naturalistic philosopher need simply endorse such behaviour, for reasons I have already made clear.

4.3 The Mathematical Practice Objection Revisited

This objection to indispensability arguments I take to be the most serious. Recall that this objection suggests that a mathematical realism motivated by indispensability is inconsistent with current accepted mathematical practice. Before addressing the main point of the objection though, I wish to say a few words about Maddy's modified indispensability argument (see Section 2.3).

I think Maddy is quite right in claiming that (pure) mathematicians are, by and large, not concerned about the applicability of their mathematics, and that they believe a particular theorem because it has been proved from the axioms, not because it has useful applications. There is still an important question about what this belief amounts to: Does believing a theorem to be true in this context simply mean that if the relevant axioms were true, then the theorem would be true, or does it mean the much stronger claim that there is ontological commitment to all the entities of the theorem? Let me illustrate with a fairly simple example. If I tell you that Sherlock Holmes is a detective and that all detectives have keen eyes for detail, then you can reasonably infer that Sherlock Holmes has a keen eye for detail. That is, you may conclude that Sherlock Holmes has a keen eye for detail in the first sense (*i.e.*, it's true if the relevant axioms are true), but you may not conclude that Sherlock Holmes has a keen eye for detail in the second sense (*i.e.*, that Sherlock Holmes *exists* and has a keen eye for detail). I suggest that when mathematicians believe a particular theorem to be true, independent of whether it has applications, they are speaking in the first sense. Mathematicians believe that the theorem follows from the relevant axioms but remain agnostic about the ontological commitments of the theorem (or the axioms).[20] The ontological questions are answered if and when this particular fragment of mathematical theory finds its way into empirical science.

In fact, it seems quite right that these two questions ought to be separated in such a way and, moreover, that mathematicians should be largely unconcerned with the question of ontological commitment (in their working lives at least). This is no different from other areas of science. Theoretical physicists may investigate various implications of some given theory without any regard for the ontological commitments of that theory—the ontological commitments will come later, if the theory is found to be useful in explaining empirical findings.

[20] Michael Resnik has pointed out to me that mathematicians *are* concerned with ontological commitment to the extent that they want the mathematical theory in question to have a model. But this is just to say that they want their theories to be consistent. Presumably the set of *true* mathematical theories is properly contained by the set of *consistent* mathematical theories, so the job of ontology is to decide which of the consistent mathematical theories are true. My claim that mathematicians are agnostic about ontology is simply the claim that they are largely unconcerned with this task.

Maddy's concerns run a little deeper though. She is concerned that the *methodology* of set theory also depends on how much set theory is required by physics.

> Set theorists appeal to various sorts of nondemonstrative arguments in support of their customary axioms, and these logically imply the existence of $\beth_\omega$. Inaccessibles are not guaranteed by the axioms, but evidence is cited on their behalf nevertheless. If mathematics is understood purely on the basis of the simple indispensability argument, these mathematical evidential methods no longer count as legitimate supports; what matters is applicability alone. (Maddy [1992, pp. 278–279)

She goes on to suggest that such a conclusion is unacceptable, given her endorsement of 'a brand of naturalism that includes mathematics' (*ibid.*, p. 279). Although I have some sympathy with her concern here, such a critique of the simple Quine/Putnam indispensability argument relies explicitly on Maddy's version of naturalism. In particular, it relies on the first departure from Quinean naturalism that I discussed in Section 3. Furthermore, this objection cannot be sustained given Quinean naturalism.

This is a rather hollow victory for the Quinean though, if Maddy's brand of naturalism is the more plausible. Fortunately this is not the case. Maddy's naturalism, and its respect for the methodology of mathematics, gains much of its appeal by contrasting it with Quinean naturalism, which allegedly pays little or no respect to purely mathematical methodology. But this portrayal of Quinean naturalism is a gross overstatement. The Quinean can agree with Maddy that naturalism demands respect for mathematical methodology, but that this respect is *earned* by the work mathematics does both within mathematics and, ultimately, in empirical science. Maddy, it seems, is willing to pay respect to the methodology of mathematics for its work in mathematics alone. I have no serious objection to Maddy on this score. Although I'm inclined to prefer the Quinean account, Maddy's is an interesting alternative that deserves attention. My point is simply that Quinean naturalism also legitimates respect for mathematical methodology, and so this cannot be a reason for preferring Maddy's naturalism over Quine's.

As for the charge that the simple indispensability argument leaves too much mathematics unaccounted for (*i.e.*, any mathematics which does not find its way into empirical science). This seems to misrepresent the amount of mathematics that has directly or indirectly found its way into empirical science. On a holistic view of science even the most abstract reaches of mathematics are applicable to empirical science so long as they have applications in some other branch of mathematics, which may in turn have applications in some further branch until eventually one of these find applications in empirical science. Indeed, once put this way it is hard to imagine what part of mathematics could possibly be unapplied.[21]

Still I concede that there may be such areas, perhaps as Quine suggests, inaccessible numbers (although even these may be a bad example). According to the indispensability argument then, these remote reaches of mathematics are without ontological commitment, so again this seems to me to be right. Maddy's alternative of endorsing ontic commitment to all mathematical entities just because they were arrived at by mathematical methods seems misguided. Mathematicians must be free to investigate possible axiom systems, for instance, without being committed to all the resulting entities. There must be room for what Quine calls 'mathematical recreation', for otherwise it starts to look as though the simple act of a mathematician thinking of some entity implies that such entities exist, and such a position, if not outright absurd, faces huge epistemological problems.[22] In short, I reject Maddy's modified indispensability argument. I think the original Quine/Putnam argument gives a perfectly adequate account of mathematics as practised.

Now to the mathematical-practice objection. Maddy begins this objection by claiming that a mathematical realist must agree that there is a fact of the matter about the truth values of such independent hypotheses as the continuum hypothesis and the measurability of Σ^1_2 sets. Clearly from what I've said already, I think that for some statements we may refrain from assigning truth values, in particular, to those in areas of mathematics we consider part of mathematical recreation.[23] This, however, clearly does not apply to the questions that Maddy is interested in. These are questions about sets of real numbers and, as I have mentioned previously, real analysis is ubiquitous in natural science and so, by the indispensability argument, has as great a claim to real status as any portion of scientific theory.

[21] Whether these more abstract reaches can be considered indispensable to empirical science is another matter. All I'm claiming here is that these higher reaches are not ignored by the indispensability argument, as Maddy seems to be suggesting.

[22] As Maddy has rightly pointed out to me, there is nothing in her account that explicitly rules out recreational mathematics. The difference between her and Quine on this point is that for Quine recreational mathematics is marked by its isolation from empirical science, whereas for Maddy it is marked by its differing methodology. For example, the investigation of finite models of arithmetic (see Priest [forthcoming]) will presumably be considered recreational by both Maddy and Quine. By the former because such models are too restrictive and by the latter because they lack the required relationship with empirical science. Thus we see that on Maddy's account, if a mathematician is using accepted methodology (*i.e.*, doing non-recreational mathematics) to investigate some area of abstract mathematics we must interpret the area of mathematics in question realistically. Again it looks as though the act of mathematical investigation implies the existence of some class of mathematical entities.

[23] Although Quine seems to prefer the assignment of truth values in such cases (see footnote 6), this is mainly to avoid the complications of non-bivalent logics (see Quine [1995], p. 57). The difference is not really important here.

So is it correct that a realist about some class of entity should agree that every statement about the entities in question is either true or false? It seems not. Many scientific realists would be inclined to dismiss statements about the simultaneous *exact* locations and momenta of fundamental particles as neither true nor false. Can the mathematical realist take a similar stand with regard to the truth of the continuum hypothesis, for instance? Although this is a line that some mathematical realists may be inclined to take, I don't find this approach at all appealing. The difference between the scientific realist refusing to assign truth values to statements about the positions and momenta of fundamental particles and the mathematical realist refusing to assign truth values to any independent statement of mathematics, is that in the former case there is a theorem that states exact limitations on the accuracy of measurements of position and momenta (the Heisenberg uncertainty principle). In the latter case it seems that the refusal to assign truth values is merely to avoid the problem at hand and as such is *ad hoc*.

Adrian Riskin [1994] argues that the mathematical realist need not accept that there is a fact of the matter about new set-theoretic axioms. He argues that it is quite consistent with Platonism to accept all consistent models of set theory as real, just as most mathematical realists are inclined to think of both Abelian and non-Abelian groups as real. Although I agree with him that such a position is viable and has, as Riskin points out, considerable support from mathematical practice, the claim that all consistent mathematical theories are true is unlikely to receive support from the indispensability argument. Recall that the indispensabilist is interested in which of the consistent mathematical theories are required by our best scientific theories, and hence deserve to be thought of as true (cf. footnote 20). While it is clear that we require both Abelian and non-Abelian groups in order to do science, it is not so clear that more than one set theory is required. In any case, it seems unlikely that we would require *every* consistent set theory.

So I am inclined to agree with Maddy that a mathematical realist ought to believe that there is a fact of the matter about answers to independent questions concerning the real numbers (or provide some cogent reason for *not* doing so). It then seems natural that such a realist should also concede that since ZFC is not strong enough to answer such questions, there must be some theory that *is* strong enough, presumably some extension of ZFC. Now Maddy points out:

> that this acceptance of the legitimacy of our independent question [Are Σ_2^1 sets Lebesgue measurable?] and... the legitimacy of its pursuit is not unconditional; it depends on the empirical facts of current science. The resulting mathematical beliefs are likewise *a posteriori* and fallible. (Maddy [1992], p. 285)

After considering the implications of the relevant physical theories, namely quantum gravity, in which the possibility that space-time is discrete arises, she concludes:

> [S]et theorists should be eagerly awaiting the outcomes of debates over quantum gravity, preparing to tailor the practice of set theory to the nature of the resulting applications of continuum mathematics. But this is not the case; set theorists do not regularly keep an eye on developments in fundamental physics. Furthermore, I doubt that the set-theoretic investigation of independent questions would be much affected even if quantum gravity did end up requiring a new and different account of space-time; set theorists would still want to settle open questions about the mathematical continuum. (*ibid.*, p. 289)

The first thing to say here is that on the version of the indispensability argument that I endorse (*i.e.*, the original Quine/Putnam argument, *without* Maddy's modification), if there were no use for continuum mathematics anywhere in science (not just as a model of space-time) then a mathematician involved in settling independent questions of real analysis would be pursuing mathematics that has no ontological commitment. That is, s/he would be participating in mathematical recreation. This is not, however, to denigrate such behaviour. Certainly continuum mathematics is an interesting area of mathematics, independent of its applications. In any case, real analysis would certainly still be required as a useful, though dispensable, approximation in many applications. The mathematician working in real analysis would be in the same boat as a modern physicist working on Newtonian mechanics or vacuum solutions to the Einstein equation.

The crux of this objection, then, is to give an account of why set theorists do not keep a close watch on developments in physics in order to help settle the independent questions of set theory. I suggest that this might simply be a case of division of labour. Set theorists do what they do best—set theory! If developments in other areas of science are to have impact on their discipline then most set theorists will not be in a position to assess that impact properly. This is no different from other areas of science, except for the matter of the *scope* of mathematics.

Michael Resnik [forthcoming] plausibly suggests that we may construct a rough ranking of the sciences in terms of their scope, in which mathematics is the most global theory, since it is presupposed by physics, which in turn is presupposed by chemistry, and so on. Furthermore, this hierarchy imposes certain natural methodological considerations. Anomalies in fairly specialised areas of science, such as molecular biology, are best not resolved by making alterations to more global theories, as alterations in the latter will have ramifications in many other areas of science that will not be foreseen by the molecular biologist. This is nothing more than Quine's Maxim of Minimum Mutilation (Quine [1992], pp. 14–15) in action.

So we find two reasons for experts not to resolve problems in their own field by proposing changes to another. The first is simply that typically such experts lack the required expertise in the field in which they are proposing the changes. Secondly, even if they were to possess the required expertise, if the other field were a more global theory than their own, they could not possibly know all the ramifications of such alterations in all the theories that depend on that global theory. Even experts in the more global theory are not in a position to assess such ramifications. This means that scientists working in the most global theories such as mathematics and physics are unlikely to need to revise their theories in light of developments in less global theories. The converse, however, is not true. Scientists working in a particular local theory may need to keep an eye on the relevant global theories to make sure that their work is consistent with those theories.[24]

The relevance of all this to the problem at hand is obvious. Set theorists are working in arguably the most global area of the most global theory. They do not expect to have to modify their theories in light of developments in other areas of science. They are not regularly keeping an eye on less global theories, simply because there is usually no need to do so. On the other hand, it seems that this is a place where the philosopher of mathematics can contribute something. The philosopher of mathematics can keep an eye on developments in other areas of science that may be relevant and assist in the assessment of the importance of those developments and of proposed modifications of current mathematical theory in light of those developments. Of course set theorists would still want to settle the open questions of set theory, regardless of developments in physics, but if such developments meant continuum mathematics had no applications, then set theorists working on the continuum hypothesis, say, would be pursuing mathematical recreation.

5. Conclusion

I have considered Maddy's three objections to indispensability arguments and found that in the cases of the objection from scientific fictions (Section 2.1) and the objection from the role of mathematics in scientific theories (Section 2.2), much of the force of the objections derived from taking the 'philosophy must give' reading of naturalism. This, I argued, was a mistake since neither Quine nor Maddy takes naturalism in this way. On the standard reading of Quinean naturalism, the force of these objections is much reduced, and in both cases more than one solution is possible.

[24] There are, of course, examples where workers in a less global theory continued research in a particular area known to be inconsistent with a relevant global theory. For example, early evolutionary theory required that the earth be much older than pre-atomic physics allowed. That is, pre-atomic physics could not provide a model of the sun emitting energy for the required length of time required for evolution to take place. In this particular case the anomaly was resolved in favour of the biologists, but this does not alter the point that the biologists were painfully aware that evolutionary theory was in tension with the more global theory of physics.

The third of Maddy's objections, the mathematical-practice objection (Section 2.3), was seen to turn on a misconception about Quinean holism. I argued that because a theory is confirmed or disconfirmed as a whole unit does not imply that each fragment of that theory has the same priority, as Maddy seems to suggest. When modification of a theory is required, Quine's Maxim of Minimum Mutilation implores us to modify those areas of the overall theory upon which the least depends. Michael Resnik's global and local distinction was seen to be particularly useful in bringing out this point. The upshot is that, given this understanding of holism, once again the Maddy objection cannot be sustained.

Finally, by allowing room for the possibility of mathematical recreation, as does the original Quine/Putnam indispensability argument, we see that mathematicians may pursue research programs that have no direct or indirect application to empirical science and that this accords very well with actual mathematical practice. Furthermore, this feature of indispensability theory is able to explain Maddy's puzzle of why set theorists would want to settle the open questions of set theory, regardless of the applications of such theory. They would be pursuing mathematical recreation.

In closing I should mention one other related issue in which Maddy is interested and which I have not addressed in this paper. This issue is the different methodological consequences of a Quinean naturalist's approach to the independent questions of set theory as opposed to Maddy's naturalistic approach. She claims that there are significant differences in the type of justification these two approaches would give for the rejection of $V = L$, for instance. Set-theoretic naturalism, as Maddy calls her position, argues against this axiom on the grounds that it is too restrictive and that it is desirable to have as rich a set theory as possible, whereas, the Quinean naturalist must give some reason to think that $V = L$ is false. As Maddy rightly points out 'desirability (notoriously!) is no guarantee of truth' (Maddy [1992], p. 288). The matter is made more complicated by Quine's own preference for $V = L$ over large cardinal axioms on the grounds of ontological parsimony (Quine [1992], p. 95). Maddy claims, in effect, that Quinean naturalism delivers up $V = L$ and that this is the wrong answer, or at least it's at odds with general consensus. Indeed Maddy takes this to be a reason to prefer her naturalism over the Quinean variety.

Whether the disagreement over $V = L$ is due to the differing conceptions of naturalism or whether it is due to different weighting of theoretical virtues such as ontological parsimony, unificatory power, elegance and so on, is not clear and is a topic I hope to address elsewhere. In this paper I have been content to disarm the Maddy objections to the indispensability argument from *within* the framework of Quinean naturalism; to defend that framework is another project altogether.[25]

[25] Earlier versions of this paper were presented to the Department of Philosophy at the

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ABSTRACT. Indispensability arguments for realism about mathematical entities have come under serious attack in recent years. To my mind the most profound attack has come from Penelope Maddy, who argues that scientific/mathematical practice doesn't support the key premise of the indispensability argument, that is, that we ought to have ontological commitment to those entities that are indispensable to our best scientific theories. In this paper I defend the Quine/Putnam indispensability argument against Maddy's objections.