MARY LENG

WHAT'S WRONG WITH INDISPENSABILITY?

(Or, The Case for Recreational Mathematics)⋆

ABSTRACT. For many philosophers not automatically inclined to Platonism, the indispensability argument for the existence of mathematical objects has provided the best (and perhaps only) evidence for mathematical realism. Recently, however, this argument has been subject to attack, most notably by Penelope Maddy (1992, 1997), on the grounds that its conclusions do not sit well with mathematical practice. I offer a diagnosis of what has gone wrong with the indispensability argument (I claim that mathematics is indispensable in the wrong way), and, taking my cue from Mark Colyvan's (1998) attempt to provide a Quinean account of unapplied mathematics as 'recreational', suggest that, if one approaches the problem from a Quinean naturalist starting point, one must conclude that all mathematics is recreational in this way.

1. INTRODUCTION

Ever since Hilary Putnam, following Quine, stressed "the intellectual dishonesty of denying the existence of what one daily presupposes" (Putnam 1971, 347), indispensability arguments for the existence of mathematical objects have been something of a bugbear for intellectually honest anti-realists about mathematics. Early attempts to defuse the argument, which holds that reference to mathematical objects is indispensable to science and that therefore mathematical objects must be presumed to exist, concentrated on the claim of indispensability. Most famously, Hartry Field (1980) went to great lengths to show that mathematics is not indispensable to science, but his view has won few followers. More recently, Penelope Maddy (1992, 1997) and Elliott Sober (1993) have argued against the indispensability arguments by pointing to the implausibility of such arguments in the light of actual mathematical practice. Both Maddy and Sober believe the problem to be with confirmational holism, the idea that our theories are confirmed wholesale by experience. However, while Maddy presents no clear alternative to confirmational holism, Sober's alternative picture of confirmation does not stand up to further mathematical examples. By

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focusing on the evidence provided in examples of the use of mathematics in science, I shall argue that, while scientists do make indispensable use of mathematics in their work, mathematics is not indispensable to science in any sense required for a realism about mathematical objects.

The standard indispensability argument for the existence of mathematical objects can, following Michael Resnik (1995), be taken as having three main Quinean strands:

Indispensability: a) Mathematical theories are indispensable components of our best scientific theories; b) referring to mathematical objects and invoking mathematical principles is indispensable to the practice of science.
Confirmational Holism: The evidence for a scientific theory bears directly upon its theoretical apparatus as a whole and not upon its individual hypotheses.
Naturalism: Science is our ultimate arbiter of truth and existence. (Resnik 1995, 166)

If the indispensability thesis is true, then by confirmational holism, any evidence for our scientific theory is evidence for the mathematical principles and objects used in science. Then, by naturalism, the mathematical principles used are true and the mathematical objects referred to exist. Resnik proposes a revised indispensability argument which he claims does not rely on confirmational holism, but for now we shall take for granted that these are the three ingredients that are at work in standard versions of the indispensability argument.

2. INDISPENSABILITY AND PRACTICE

Penelope Maddy, who used the indispensability argument to bolster her set-theoretic realism in her 1990 book *Realism in Mathematics*, has more recently found reason to drop this argument and the realism that goes along with it. She argues that the holism required for the indispensability argument (in Quine's words, the view that "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body" (Quine 1951, 41)) is at odds with actual mathematical and scientific practice:

Logically speaking, this holistic doctrine is unassailable, but the actual practice of science presents a very different picture. (Maddy 1992, 280)

Maddy's examples, from mathematics and science, seem to show that scientists and mathematicians do not themselves always take the results of science to confirm the existence of the mathematical and scientific objects referred to in their theories. For Maddy, whose brand of naturalism (as applied to mathematics) has been expressed by the maxim: "if our philosophical account of mathematics comes into conflict with successful mathematical practice, it is the philosophy that must give" (Maddy 1997, 161), this conflict between holistic doctrine and the actual behaviour of mathematicians and scientists provides an unacceptable tension. However logically unassailable arguments for Quinean holism seem to be, we cannot accept Quinean holism if it has as a consequence that successful mathematical or scientific practice is shown to be unfounded. In the light of the attitudes of mathematicians and scientists to confirmation, it is Quine's holism that must give.

A quick response for the Quinean might be just to say "so much for Maddy's naturalism". Maddy is not just a naturalist in the Quinean sense of the word: she argues, in her *Naturalism in Mathematics* and elsewhere, that anyone of a Quinean bent who has respect for mathematics should extend their naturalism to include mathematics. The Quinean might argue that it is Maddy's extension of Quinean naturalism to mathematics that causes the problem for the indispensability argument. For the Quinean naturalist, it is *science* that is the arbiter of truth and existence. The observation that *mathematicians* do not behave as if this is correct simply shows that these mathematicians are mistaken. It is Maddy's extension of naturalism to mathematics that forces her to abandon theories that conflict with mathematical practice, but Quine–Putnam indispensability arguments are based on *scientific* naturalism, and as such, the argument goes, they need not answer to problems caused by Maddy's extended version of naturalism.

Maddy has two possible responses to this. First of all, in Maddy's eyes, she has enough examples from scientific practice alone to cast doubt on the indispensability arguments. In her *Naturalism in Mathematics*, evidence against the indispensability arguments from mathematical practice is relegated to a postscript, included after Maddy has considered scientific examples that tell against indispensability, and after she has concluded that "science seems not to be done as it would have to be done if it were in the Quinean business of assessing mathematical ontology" (Maddy 1997, 157). These examples from science do *not* require a naturalism about mathematics for their efficacy against Quine's argument. Maddy's arguments against indispensability that stem from scientific practice alone claim to show that "there is a tension between the Quinean indispensability arguments and Quinean naturalism" (Maddy 1997, 182), so that it is not just Maddy's own mathematical naturalism that is at risk. We shall return to this claim when we consider Maddy's arguments and an attempt by Mark Colyvan (1998) to reconcile them with Quine's naturalism.

A second response available to Maddy, in defence of her use of examples from *mathematical* practice to support rejection of the indispensability argument, is to reiterate her original argument for extending naturalism to cover mathematics. Maddy's version of naturalism stems from a dissatisfaction with the place pure Quinean naturalism (which is restricted to science) gives to mathematics.

> Mathematics, after all, is an immensely successful enterprise in its own right, older, in fact, than experimental natural science. As such, it surely deserves a philosophical effort to understand it as practiced, as a going concern. (Maddy 1992, 276)

The very same naturalistic instinct that leads Quine to advocate evaluating science on its own terms should, according to Maddy, lead one also to evaluate our 'immensely successful' mathematics from within.

> To judge mathematical methods from any vantage-point outside mathematics, say from the vantage-point of physics, seems to me to run counter to the fundamental spirit that underlies all naturalism: the conviction that a successful enterprise, be it science or mathematics, should be understood and evaluated on its own terms, that such an enterprise should not be subject to criticism from, and does not stand in need of support from, some external, supposedly higher point of view. (Maddy 1997, 184)

Even if we do not wish to countenance Maddy's full blown naturalism about mathematics, we should at least have enough respect for the work of mathematicians to take their divergence from Quinean doctrine seriously. If mathematicians do not do mathematics according to Quinean doctrine, they may well be doing something wrong (this is a departure from strict Maddian naturalism). However, it is the job of the mathematically sensitive philosopher to try to understand why it is that mathematicians continue in the way that they do, with a great deal of success, in order to discover whether their practice is justified, instead of merely dismissing their methods in the light of Quinean worries.

In support of Maddy's first potential response to the Quinean, we should note that much of her evidence against Quinean indispensability arguments does come from scientific, rather than mathematical, practice. Maddy considers, first, scientific entities occurring indispensably in theories, and argues that scientists themselves will tend to suspend belief in these entities until they have some 'direct confirmation' of their existence, above and beyond the success of the theories in which they occur. Her prime example is the state of atomic theory in 1900. While reference to atoms was indispensable to this successful theory, many scientists reserved judgement regarding their actual existence until they had some direct experimental confirmation of atoms. Maddy argues that "the case of atoms makes it clear that the indispensable appearance of an entity in our best scientific theory is not generally enough to convince scientists that it is real" (Maddy 1997, 143). So, even before we consider the use of mathematics in science, actual scientific practice does not sit comfortably with Quinean confirmational holism. Hence the perceived tension between naturalism and holism, both of which seem to be essential premises for the indispensability argument.

More specifically for mathematical entities, Maddy considers the variety of uses of mathematics in science. First are the many uses of mathematics in idealized situations, for example in dealing with frictionless planes or in treating liquids as continuous substances in fluid dynamics. Just as the success of our science here does not justify a belief in the actual existence of frictionless planes or continuous fluids, neither, says Maddy, should we expect it to justify a belief in the mathematical entities used to deal with these useful fictions. Of course, Quine has a story to tell about the use of such idealizations, in order to explain why their occurrence in our scientific theory doesn't require our belief in their existence. According to Quine, they are linguistic conveniences that can be replaced by literally true paraphrases. This is done by, for example, utilizing the Weierstrassian theory of limits. Talk of frictionless planes, for example, can be paraphrased as talk about the behaviour of actual planes as friction is reduced to a minimum. "When we paraphrase our talk of ideal objects in the Weierstrassian spirit", Quine tells us, "we are merely switching from a theory that is conveniently simple in a short view and complex in a long view to a theory of opposite character. Since the latter, if either, is the one to count as true, the former gets the inferior rating of a convenient myth, purely symbolic of that ulterior truth" (Quine 1960, 250).

In response to this, Maddy notes that, in contrast with the convenient example of frictionless planes, we simply cannot paraphrase a claim about the continuity of fluids as a claim about discrete fluids as they approach some limit. For one thing, it is difficult to see how to make sense of such a claim; furthermore, Maddy points out, "fluid dynamics isn't more applicable to one fluid than another, depending on how closely that fluid approximates a continuum" (Maddy 1997, 145). The assumption of continuity is a suitable idealization for an account of any fluid. The problem for Quine here is that he has not been able to show that all explicit idealizations are really shorthand for literal explanations of scientific phenomena. "[I]t is clear", Maddy concludes, "that the method of Quinean paraphrase will not successfully eliminate idealizations from natural science" (Maddy 1997, 145). Without such translations of the ideal into the literal, Quine's indispensability argument must be artificially restricted so as not to apply to explicit idealizations, if it is not to force our belief in the existence of those idealized structures.

So Quinean indispensability arguments, if they work at all, must be applied to mathematics in its use outside of explicit idealizations. Again, according to Maddy, scientific practice speaks against the indispensability arguments here. Maddy considers Feynman's discussion of the question of the continuity of space-time, where, contrary to Quinean expectations, she discovers that physicists seem "happy to use any mathematics that is convenient and effective, without concern for the mathematical existence assumptions involved ... [and] without concern for the physical structural assumptions presupposed by that mathematics" (Maddy 1997, 155). If scientists see evidence for their theories as evidence for the existence of the objects referred to in the mathematics they use, then they should be just as concerned with finding evidence for the continuity of space-time as they are for the existence of atoms. However, Feynman's example shows that renowned scientists will readily help themselves to parts of mathematics with no apparent ontological concerns: they will use the mathematical continuum without worrying about whether such a mathematical object exists, and without concern about whether the phenomenon they are explaining with the help of the mathematical continuum literally is continuous.

Maddy's final set of worries about indispensability arguments do stem more directly from her own brand of naturalism which extends Quinean naturalism to mathematics. As such, these concerns are not so much of a problem for those who see Quine's scientific naturalism as sufficient to deal with mathematics, for these hard headed Quineans may respond that when mathematicians fail to act as the Quinean doctrine says they should, they are simply mistaken. However, as was mentioned earlier, most mathematically sensitive philosophers will be wary of ascribing too many mistakes to mathematicians, even if they are not full blown Maddian naturalists. With this in mind, we should also see these last worries as a concern even for those who do not subscribe to Maddy's mathematical naturalism.

These last of Maddy's concerns spring from the idea that, if science is the only arbiter of existence, then mathematicians should look to scientific developments to tell them what mathematical questions to pursue. Looking again at the problem of the existence of a continuum in the physical world, Maddy argues that,

> ... if our indispensability theorist is right, it seems proper methodology in set theory depends on developments in physics; in particular, on how the question of the literal application of continuum mathematics is resolved. (Maddy 1997, 159)

Take, for example, the continuum hypothesis, which says that any infinite set of real numbers has cardinality $\aleph_0$ or $2^{\aleph_0}$ (i.e., there are no infinite sets of real numbers with cardinality in between these two). The continuum hypothesis is independent of the usual set theoretic axioms, in that models of set theory which fit all of these axioms can be found in which the continuum hypothesis is true and in which it is false. Set theorists, depending on how realist or anti-realist they are in inclination, may see their job as either (a) to look for further axioms which give the continuum hypothesis its correct truth value (i.e., to decide which of the many alternative extensions of ZFC is *true*), or (b) to consider all possible extensions of ZFC as equally worth pursuing as potentially interesting formal games. However, if there *really is* a collection of $2^{\aleph_0}$ objects (i.e., the points on a continuous line), then the second option (which Maddy calls 'Glib Formalism'), seems inappropriate, because there would at least *seem* to be a definite answer as to how many different infinite sets can be made out of these points. As it is, though, no set theorist looks to physics to determine the stance to be taken on the continuum hypothesis, or other questions in continuum mathematics. Here we have a clear case of our philosophical account conflicting with mathematical practice, and hence, for the Maddian naturalist, it is the philosophy that must give.

3. A PROBLEM WITH CONFIRMATIONAL HOLISM?

In response to Maddy, Mark Colyvan (1998) has attempted to show how her examples can be dealt with from a Quinean perspective. Before considering Colyvan's defence of indispensability, though, it is worth noting that even if Maddy is correct in her diagnosis, to the effect that there is something wrong with taking the dual assumptions of holism and naturalism as a basis for indispensability arguments, this still leaves some things unsettled. If we grant that Maddy has shown that the two assumptions conflict, there is still some work to be done to show that it is confirmational holism and not naturalism that should be rejected here. Indeed, there are good arguments for both confirmational holism and naturalism. If we are going to reject the conjunction of these views on the basis of scientific or mathematical practice, we would thus do well to discover where our arguments have gone wrong.

Elliott Sober (1993) attempts to fill in the gaps for Maddy, with an argument against confirmational holism based on his position of 'contrastive empiricism'. Sober appeals to a *Likelihood Principle* which, he claims, is the standard basis for scientists' regarding an observation as evidence for one hypothesis over another:

> Observation O favours H_1 over H_2 if and only if $P(O/H_1) > P(O/H_2)$. (Sober 1993, 38)

This principle leads Sober to maintain that confirmation is relative, not absolute:

> The evidence we have for the theories we accept is evidence that favors those theories *over others*. (Sober 1993, 39)

But, if this is so, then the mathematical theories used in successful science are not confirmed by the success of that science, if they would also be used in any of the other possible competing theories that do not happen to succeed:

> If the mathematical statements M are part of each hypothesis under test, then the observational outcome does not favor M *over any of its competitors*. (Sober 1993, 45)

This is not to say that science can never confirm mathematical hypotheses, but rather that it can only do so in cases where observation would confirm one set of mathematical hypotheses over another.

> We would need to test M against M', where M' is a competing set of mathematical hypotheses. What is required is that M and M' confer different probabilities on some set O of statements that can be checked by observation. . . . In this testing procedure, M is *not in*dispensable; there is at least one other candidate hypothesis that we can consider. (Sober 1993, 45)

If Sober is right that scientists do follow the likelihood principle, scientific results are thus of no interest to the mathematician whose work provides background assumptions for all competing theories.

What of cases where science *does* seem to provide a test between two alternative mathematical theories, say Euclidean and non-Euclidean geometry? Sober evidently believes that the discovery that space is non-Euclidean provided confirmation for the mathematics of non-Euclidean geometry. This is one place where Sober's picture has difficulties. The discovery that space was non-Euclidean certainly had mathematical consequences: non-Euclidean geometry came to be considered more worthwhile as a subject of investigation, and it was no longer assumed that Euclidean geometry embodied *a priori* truths about space. However, it is nonetheless difficult to maintain that the empirical discoveries confirmed the truth of non-Euclidean geometry and showed the falsity of Euclidean geometry in any sense other than that one was a correct model of the physical world and the other was not. But this is not *mathematical* truth: the applicability of non-Euclidean geometry did not falsify any mathematical theorems in Euclidean geometry – the Pythagorean theorem still holds for Euclidean triangles – it merely confirmed the assumption of Gauss and others that the scope of the theorems of Euclidean geometry only covers systems that assume the parallel axiom.

A further difficulty with Sober's diagnosis of the problem with indispensability arguments is presented by Maddy, and considered in Sober's paper. There is at least one type of situation where observation seems to confirm mathematical results even on Sober's picture. Sober considers the set of hypotheses H_n:

(H_n) $\quad 2 + 2 = n,$

and the premise,

(A) There are $2 + 2$ apples on the table.

The objection is then that the observation that there are four apples on the table favours hypothesis H_4 over all others. So surely, even on Sober's picture of relative confirmation, we can say that such arguments confirm the truths of elementary arithmetic.

Sober's response to this example is revealing. While reiterating that there may be cases where mathematics is confirmed by observation, he stresses that this is not one of them. In this case, he argues,

> If there had failed to be 4 apples on the table, I do not think we would have concluded that 2 + 2 has a sum different from 4. Rather, we would have concluded that the auxiliary assumption (A) is mistaken. If this is how we comport ourselves, then the "experiment" just described need never have been run. If we hold our belief that 2 + 2 = 4 immune from revision in this experiment, then the outcome of the experiment does not offer genuine support of that proposition. (Sober 1993, 49)

This is a reasonable response to the claim that '2 + 2 = 4' is confirmed by the experiment, but one that puts Sober back on a level footing with Maddy. Maddy had no explanation of why indispensability arguments break down, or of why mathematics fails to be confirmed by scientific results, and while Sober attempts an explanation through his picture of relative confirmation, his response to this objection, that we *just don't* see our experiment as confirming our mathematics, shows that he is as much at a loss as Maddy is in explaining why we should not be swayed by Quinean indispensability arguments.

4. A QUINEAN RESPONSE

Back, then, to Maddy, and in particular to the objection that the Quinean naturalist has been given nothing to worry about in Maddy's observations. This objection is pressed by Mark Colyvan, who points out that Maddy's naturalism seems to depart from Quine's on two fronts. Obviously, Maddy has made a departure in her extension of naturalism to cover mathematics as well as science, but Colyvan finds another apparent departure in Maddy's view of naturalism in general. Colyvan considers the claim that naturalism is the doctrine that "in a conflict between philosophy and practice, practice wins" (Maddy 1997, 169), and argues that this is far from what Quine intends for *his* naturalism. The problem, Colyvan tells us, is with

> . . . 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. (Colyvan 1998, 46)

In fact, Colyvan argues, in her more careful moments even Maddy does not endorse the 'rubber stamp' view of naturalism (for which she has been roundly criticized), and Quine certainly does not. Philosophy may well criticize scientific (and mathematical) practice, but when it does so, it is not from some privileged position: science and philosophy stand alongside one another, and may criticize each other.

Maddy's arguments from scientific practice can be dealt with by the Quinean, then, who can answer, with Colyvan,

> 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. (Colyvan 1998, 50)

This is not to say that Colyvan wants to argue that in *each* of Maddy's examples of scientific practice the scientists are mistaken: sometimes they are, but he also gives Quinean justifications for some of their actions. Colyvan adds further justification for the Quinean picture by noting initial scientific suspicions about the use of complex numbers and the Dirac function: "at least it seems that there are *some* cases where physicists are genuinely suspicious of new mathematical entities" (Colyvan 1998, 53). There is less suspicion about the use of the continuum, he argues, because it is widely used with great success.

Moving to Maddy's mathematical practice example, we have already noted that the force of this objection depends largely on accepting Maddy's extension of naturalism to require respect for mathematical methodology. We also suggested that even those who do not accept Maddy's naturalism about mathematics must provide an explanation of the behaviour of mathematicians, and should be impressed enough by the success of mathematics not to dismiss the paradigmatic mathematical activity seen in Maddy's examples as irrational without some strong justification. Colyvan realizes this, and argues that set theorists are rational in ignoring for the most part the developments of physics. They are simply following Quine's Maxim of Minimum Mutilation.

> [J]ust 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. (Colyvan 1998, 60)

This maxim might be thought of as extending Quine's claim that all statements are equally revisable by adding a parenthetic *but some are more equally revisable than others*. However, it is an important addition to Quine's holism, making a distinction between practical and theoretical revisability which adds plausibility to his original more radical claim. Applying the Maxim, we see that because of set theory's high level of generality – that is, because many things depend on set theory while set theory itself depends on little else – set theorists are to be considered rational in not putting too much stock in developments in less general theories such as physics.

So, if we stick with naturalism as Quine meant it, then it looks like Maddy's objections can be overcome on Quine's own terms. "This", says Colyvan, "is a rather hollow victory for the Quinean though, if Maddy's brand of naturalism is the more plausible" (Colyvan 1998, 55). Maddy's extension of naturalism to cover respect for the methodologies of mathematicians is motivated by the worry that Quine's naturalism short changes mathematics. Quine's naturalism, with its emphasis on natural science, seems to ignore the methods specific to mathematics. If science is the arbiter of existence, then, as Maddy points out, it seems that for the Quinean,

> ... (most of) what mathematicians actually say in defence of their existence claims, their axioms, and their methodological decisions is beside the point. (Maddy 1997, 183–184)

Furthermore, if we accept the conclusion of the indispensability argument based on Quinean naturalism about science alone, then we are faced with the problem that too little mathematics is justified through its use in science. After all, it is possible that even the use of the continuum should be considered as a simplifying idealization, which is inessential, though extremely useful, for science. Finally, the standard mathematical practice of considering the implications of varieties of axiom systems, regardless of their applications, seems like utter folly from the Quinean perspective. Should we, then, accept Maddy's extension of naturalism to cover mathematics, along with the problems for indispensability that this brings?

Colyvan's answer, of course, is that we should not extend Quine's naturalism in this way. Quine, he argues, does have the resources for a sympathetic account of mathematical activity even in areas of mathematics that are unapplied. These resources are found in Quine's account of unapplied mathematics as ‘recreational’. Mathematicians involved in proving theorems in axiomatic systems which do not seem to apply to the physical world are, according to Colyvan, engaging in ‘mathematical recreation’. While they might assert their theorems to be true, they do so only in the sense that they believe them to follow from axioms which they believe to be consistent.

> I suggest that when mathematicians believe a particular theorem to be true, independent of whether it has applications, they ... believe that the theorem follows from the relevant axioms but remain agnostic about the ontological commitments of the theorem (or the axioms).[1] The ontological questions are answered if and when this particular fragment of mathematical theory finds its way into empirical science. ...
>
> 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. (Colyvan 1998, 54–56)

Mathematicians may, thus, investigate whatever formal systems they like, using whatever mathematical methods they see fit. Pure mathematicians may believe a theorem to be true on the grounds that they have proved it from axioms. However, *good Quinean* pure mathematicians should see this belief as best interpreted merely as a belief that the theorem is derivable from their axioms using their chosen rules of inference. It is *science* that confirms the existence of the objects described in these axiom systems, and when a (good Quinean) mathematician *really* believes a theorem to be true it is because it is indispensably referred to in our best science.

If Colyvan is right in his claim that the Quinean may appeal to a substantive notion of mathematical recreation in order to understand the practice of mathematicians (who proceed without concern for the place of their mathematics within science), then it seems he has removed the motivation for moving to a full blown naturalism about mathematics. It is less clear, though, that he has saved the indispensability argument from Maddy’s objections. Although Colyvan has given us reasons to drop the strong ‘philosophy must give’ version of naturalism, there is still the concern that it would be difficult in the light of the success of mathematics and science to hold that scientists (and mathematicians) are wrong in their practice most of the time. Appealing to such cases as concern over the use of complex numbers and the Dirac function would thus not be enough to save the indispensability argument if it was nevertheless the case that most scientists most of the time are not concerned about the mathematics they use. Further to this, even if it were to be found that scientists *are* much more hesitant in their introduction of new mathematics than Maddy has suggested, the Quinean would still need to show that their hesitance is the result of genuine worries about the ontological commitments they would be making if they did use these areas of mathematics. Scientists may hesitate in introducing new mathematics as a result of worries about the consistency of the mathematics to which they are appealing, or about the appropriateness of using that mathematics as a model of a particular scientific phenomenon – this hesitancy does not help the Quinean who wishes to show that scientists have *ontological* concerns when they introduce new mathematical entities. I will not, though, attempt here to argue that Maddy's examples are more typical than Colyvan's of the attitudes of most scientists to the mathematics they use, although (for the record) I do believe that this is true. Rather, I wish to point to a problem with Colyvan's appeal to recreational mathematics which suggests that the indispensability arguments carry no weight for those interested in the ontology and epistemology of mathematics.

One should first note that, on the face of it, Colyvan's appeal to the Maxim of Minimal Mutilation, while explaining the standard attitude of set theorists to science as rational, does not explain the apparent *total* insulation of mathematics from scientific results. While mathematics is often developed with particular areas of science in mind, it never seems to be the case that new scientific discoveries result in the rejection *as false* of those areas of mathematics developed for use in a particular scientific theory. While we would expect, in Colyvan's picture, to see set theorists trying to hang on to their highly general theory come what may, we should also see many cases of specialized areas of mathematics, developed to help with particular scientific problems, rejected as false in the light of difficulties with their application to those scientific problems. But this does not seem to happen. Consider the paradigm case of a mathematical theory which did not do what was expected of it: Catastrophe Theory. This area of mathematics was heralded as "The most important development since calculus" (*Newsweek*), but its initial promise proved to be a great deal of hot air. The result? Catastrophe Theory became a much less popular area of research, but no one would claim that the mathematics of Catastrophe Theory had been *falsified* by its magnificent scientific failures.[2]

Where Colyvan can explain this insulation between mathematics and science is in his account of recreational mathematics. In the case of Catastrophe Theory, while its failure in science stopped it from winning the status of a literally true theory, mathematicians could on Colyvan's account still believe its results in the sense that they believed the theorems were derivable from the assumptions involved. And this is the same with any area of mathematics, applied or unapplied. Once recreational mathematics is acknowledged as an important mathematical activity, mathematicians need *never* change their attitude to their theories in the light of their success or failure in science. Science may show (fallibly) which of the consistent theories mathematicians work with are really true, but this fallible confirmation or denial of the existence of the objects referred to in mathematical theories need make no difference whatsoever to the work of mathematicians. As Colyvan says, were it to be shown that there were no use of continuum mathematics in science, it would still be the case that "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" (Colyvan, 60). Mathematicians show the *mathematical* truth of theorems by giving proofs of those theorems from the assumptions of their theories. They are thus, according to Colyvan, not interested in *genuine* truth, which is the job of the scientist to uncover.[3]

Considered in this light, Colyvan's distinction between literally true mathematics and merely recreational mathematics begins to look like a distinction without a difference. The literal truth of a mathematical theory will make no difference to how a mathematician goes about working in that theory. A sympathetic modification of the indispensability argument might seem to explain this anomaly. Maddy considers the following as a more plausible version of the indispensability argument:

> We could argue, first, on the purely ontological front, that the 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. (Maddy 1992, 279)

On this version of the argument, it makes sense to say that science confirms the existence of mathematical objects which are nevertheless discovered by mathematical methods. It remains odd, however, that the very same methods by which mathematicians come to discover and learn about *really existing* mathematical objects are also the methods used in *genuinely recreational* mathematics, where there really are no underlying objects to which the mathematicians' beliefs refer. Presumably, our world could be significantly different from the way it actually is, and the mathematics *confirmed* by the science of that world would be very different from the mathematics confirmed by our science, yet the mathematics *done* in both worlds could be identical.[4] If there is a difference between recreational and non-recreational mathematics, it is a contingent one, and one that makes no difference to the work of mathematicians.

There is also something strange going on in Colyvan's picture when it comes to those parts of applied mathematics that we should take to be falsified in the light of some scientific result. Bringing recreational mathematics into the picture means that any previously applied area of mathematics may be kept, in its entirety, as recreational mathematics, after it is shown that it conflicts with scientific results. We need not, then, alter an area of mathematics in the light of scientific results (although it might become less interesting for mathematicians to work on, as was the case with Catastrophe Theory). The mathematician may keep working on that area of mathematics, but must drop the assumption that it is *their* area of mathematics which describes the physical phenomena they had originally thought it to describe. This seems a very good description of what actually does happen when it is discovered that a mathematical theory does not fit with experimental results (it fits well, for example, with the case of Euclidean geometry). However, it causes problems for the idea that successful mathematics is *confirmed* by its use in science. If a conflict with science merely leads us to drop the assumption that an area of mathematics describes a particular physical phenomenon, and never to drop that area of mathematics, then (and this harks back to Sober's discussion of confirmation) surely when we do use an area of mathematics successfully in science, what is being confirmed is not the mathematics itself, but the assumption that it correctly describes the scientific phenomenon we are considering. I shall return to this in my final section where I discuss the relation between mathematics and science.

5. THE STORY SO FAR

We should take a moment, at this point, to run through the argument so far. Penelope Maddy has provided some evidence, from scientific and mathematical practice, against accepting the conclusions of indispensability arguments. However, her account lacks an explanation of the relationship between mathematics and science which would explain the behaviour of mathematicians and scientists as rational. Such an explanation is needed for all but the most extreme naturalist, as most would allow that, even if philosophers are not allowed to criticize the practices of mathematicians and scientists, they may still ask *why* they behave as they do. Elliott Sober's explanation, in terms of his contrastive notion of confirmation, fails to provide a plausible explanation of the behaviour of scientists and mathematicians in cases where his conditions for the confirmation of one mathematical hypothesis relative to another are met, and yet the mathematics is still not confirmed. We saw the break down of Sober's account in two contrasting cases: the example of Euclidean/non-Euclidean geometry and Maddy's apple experiment.

Against the simplest Quinean defence of indispensability, which simply notes that Maddy's problems are presented within her extended version of naturalism and are thus irrelevant to Quine's own scientific naturalism, it was noted that, even if one does not accept Maddy's naturalism about mathematics, it is still up to the Quinean to defend the indispensability argument against Maddy's arguments from scientific practice, and to have enough respect for the work of mathematicians to avoid a categorical dismissal of their methods in the light of divergences from Quinean doctrine. Mark Colyvan attempted such a defence, arguing first that if one sticks with Quinean naturalism, properly understood, Maddy's objections can be deflected, and second, that it is open for the mathematically sensitive Quinean to stick to Quine's scientific naturalism, without following Maddy's extension of naturalism to mathematics, provided that respect is given to the practice of mathematical recreation. Aside from voicing some concerns that Colyvan might still have to dismiss too much scientific activity as irrational if he is to stick with the indispensability argument, it was noted that Colyvan's appeal to recreational mathematics, while presenting a plausible picture of mathematical practice, made it difficult to attach any importance to the notion of literally true (in Quine's sense) mathematics, at least as far as *mathematicians* are concerned. Furthermore, the fact that mathematicians may still work on an area of mathematics after it has been shown to 'conflict' with science suggests a more complex picture of confirmation than is assumed in Quine's indispensability arguments.

If the Quinean is right, then the mathematical entities not referred to in science are the products of a perfectly acceptable recreational mathematics. This mathematics consists in the drawing of conclusions from hypotheses, which are not themselves assumed to be true in the same way as the statements of science are said to be true, or even to be capable of truth value on this understanding of truth (there just might not be objects which fit the hypotheses, and whether there are or not is a question for science). I now wish to argue that Maddy's examples lend credence to the hypothesis that *all* mathematics is, in essence, recreational in this way. Maddy's examples, and my own, fit well with a picture of the relationship between mathematics and science which sees areas of mathematics as *modelling* scientific phenomena. On this picture, mathematical entities are never directly referred to in science, and the truth of mathematical principles is not invoked. So mathematics, though indispensable to science, is not indispensable in the sense required by the indispensability argument (as characterized on page 2). This picture of the relationship between mathematics and science would be something that a Gödelian Platonist[5] could be happy with. However, for one convinced by Quinean naturalism, who thinks that the only good arguments for realism in mathematics come from the indispensability considerations,[6] an argument for the claim that mathematics is not referred to in science is an argument for the claim that all mathematics is recreational. It is this claim that I shall support in this paper.[7]

6. THE CASE FOR RECREATIONAL MATHEMATICS

The examples of the relationship between mathematics and science considered in this paper suggest the following observations:

1. Mathematics is insulated from scientific discoveries, in the sense that the falsification of a scientific theory that uses some mathematics never counts as falsification of that mathematics (beyond simple cases of calculation error).
2. In particular, a scientific observation that conflicts with some scientific theory may suggest a move to a different background mathematics, but does not suggest that mathematicians should abandon that mathematics. In line with Sober's discussion of confirmation (in which a part of a theory is not confirmed by its success if it would not also be considered to be disconfirmed by the theory's failure), this indicates that the success of a scientific theory does not confirm the mathematics used in that theory.
3. What does seem to be disconfirmed by the failure of a scientific theory that relies strongly on a background mathematics is the claim that this mathematics is applicable to the scientific phenomena that it has been used to describe.

These observations lead naturally to an understanding of the relationship between mathematics and science in which areas of mathematics are used to model physical phenomena. When we use mathematics in science we do not invoke the existence of mathematical objects. Rather, we interpret our strictly meaningless[8] mathematical terms by tying them to scientific phenomena, and use their mathematical consequences to draw conclusions about the scientific phenomena. In some areas of science, biology for example, this relationship is explicit: we talk of mathematical models of population growth and take it for granted that if we do not get favourable results then this is a problem with the model (i.e., with the way in which we have tied our mathematics to the physical phenomenon).

When we use mathematics to model physical situations in this way, we never refer to mathematical objects or assume the (mathematical) truth of their relations. Rather, we interpret our mathematical stories physically and assume that our model is good enough in the relevant respects that the theorems derived in our mathematical recreations, when transcribed into physical language, will give us truths about the physical phenomena we are considering. It is this picture that explains the *insulation* of mathematics against physical developments, as well as the indifference of scientists to the literal truth of the mathematics they employ. If Colyvan is right (and I think he is) that mathematics that is not assumed by science to be true should be seen as recreational (and given some important status as such), then it follows from the modelling picture of the relationship between mathematics and science that *all* mathematics is recreational.

The success of this modelling should be no real surprise: many of the mathematical stories we create are created with scientific interpretations in mind. Euclidean geometry is best understood as a mathematical story whose axioms were meant to model truths about physical space. It was developed with our assumptions about real points and lines in mind, but it was always an empirical question whether it actually did provide the best model of points and lines in the physical world. When it turned out that the parallel axiom was not a part of the best model for physical space on a large scale, non-Euclidean geometries became a more profitable mathematical recreation. However, the breakdown of the Euclidean model made no difference to the acceptance of Euclidean theorems as consequences of Euclidean axioms, and Euclidean geometry could still be used to model physical space on a small scale. When we use the continuum to model space-time, while we might be interested in whether this model breaks down in certain situations, whether it does so or not has no implications for our mathematical talk of the continuum, and need not even stop our use of this model in science in cases where it still makes sense to do so. (Feynman was happy to use the continuum to deal with space-time even though he suspected that the model would break down at the micro-level.)

The real test case for this perhaps counterintuitive picture is, of course, simple counting. How does this picture deal with our experiment where we seem to use the counting of apples to confirm the truth of the mathematical statement '2 + 2 = 4'? Do we not really refer to the mathematical object '4' when we say that there are four apples on the table? And do we not invoke the truth of the mathematical statement '2 + 2 = 4' when we count two sets of two apples and conclude that there are four apples? I claim not. Our mathematical stories concerning arithmetic and numbers have clearly grown up alongside counting practices, but to understand the relationship between arithmetic and counting we should separate our mathematical language and our counting language. When we say "There are 2 + 2 apples", what we mean is (something like) that we can count 'one, two', and associate these words with distinct apples, and then count 'one, two', and associate these words with further distinct apples, and in doing so we have labelled all the apples. '2 + 2' here does not mean the same as the mathematical usage of '2 + 2' in '2 + 2 = 4', which, independent of its various interpretations in our counting language, is meaningless. When we conclude that there are four apples, on the basis of our counting two sets of two apples, we do not invoke the truth of the mathematical statement '2 + 2 = 4', but rather the belief that the 'game' of elementary arithmetic provides a good model of our counting practices.

Our basic arithmetic is developed so as to model our counting practice. In fact, it does this so successfully that we do not tend to see it as a model at all. Similarly, we can see our language of real numbers in mathematics as having been created to model our measuring practices. Of course, the power of the model is that it goes beyond what can be translated out back into physical language. Thus the familiar translation of 'there are two apples' to '$\exists x \exists y(Ax \;\&\; Ay \;\&\; x \neq y \;\&\; \forall z(Az \rightarrow z = x \vee z = y))$ does not show that all uses of numbers can be eliminated. From our 'recreational' perspective, in contrast with Field's programme, we do not need to show that all uses of the numbers in purely mathematical contexts are translatable out into physicalist language in such a way. The basic structure of the mathematical number system is an abstraction from the linguistic structures allowed in these counting contexts, but ultimately comes apart from them, so that the simple counting of physical objects uses a different (though related) language to the language of number theory. It is here where we get the indispensability of our mathematical models to science – our mathematical language goes beyond the scientific language that it models.

7. AN OBJECTION

I have not considered Michael Resnik's (1995) alternative 'pragmatic indispensability argument' in this paper, for reasons of space. Although it does not assume confirmational holism, I believe it is still rendered false if the picture I have given of the relationship between mathematics and science is correct.[9] However, Resnik's paper does contain an important, and seemingly damning, objection to this sort of anti-realist argument.

> The problem with this approach is that even if we replace truth with truth in a story, we want some constraints on our stories. They should be consistent, and what is true in them should follow logically from their premises. Stating and proving that various stories have these properties will require a background mathematics. Even if we take a purely syntactic approach to logical validity and consistency, we will be committed to the natural numbers,

elementary number theory and axioms stating the arithmetical consistency of various other mathematical theories. (Resnik 1995, 173, n. 6)

Resnik's point is that the anti-realist who says that all mathematics is just the telling of consistent stories is too quick with her reliance on consistency. Any mathematical consistency proof will itself have to assume, without proof, the consistency of at least some part of mathematics (i.e., whatever mathematics is used to express the proof). To avoid infinite regress, we must ultimately believe that some mathematical systems are consistent, without having a proof of consistency. In the absence of an internal consistency proof for arithmetic, our faith in its consistency must, it is suggested, ultimately boil down to the belief that the statements of arithmetic are actually true of genuinely existing objects. The problem, then, is that if the story-teller anti-realist is to put a consistency requirement on her stories, then she must admit at least some mathematics to be true.

This objection, though, misses the force of the anti-realist picture of the relation between mathematics and science. For the anti-realist is only concerned with consistency insofar as it ensures the creation of stories which are likely to be applicable to physical situations (and perhaps insofar as it keeps mathematical story-tellers on their toes). While the anti-realist does not see number theory as literally true, this is an extremely successful model of our counting talk (though it might be the case that not every statement derived in number theory can be translated into a truth about counting). We do not expect this model to lead us into contradiction, because it is so well grounded in our correct counting practices. If other theories have a model in the natural numbers, or in another well-used area of mathematics, then this is good enough for the anti-realist to ensure that the stories being told may be safely put to use in science.

8. CONCLUSION

There is much more work to be done to flesh out the picture of the relationship between mathematics and science that I have sketched here. In particular, to offer a rich picture of mathematical activity, it will need to be extended to a picture of the interrelations between different branches of mathematics. Furthermore, I have only hinted at how to defend this argument against another argument for realism in mathematics, based on the 'unreasonable effectiveness of mathematics in the natural sciences'. This realist argument does not require that science refers to mathematical objects in order to ensure their existence, but merely remarks that the usefulness of mathematics would be incredibly difficult to explain if that mathematics was not actually true. I believe that such an argument can be defused by a consideration of the genesis of mathematical stories, but I will not pursue that line of argument in this paper. For now I hope at least to have shown how an understanding of the interaction between mathematics and science as based on a modelling relationship can account for the breakdown of indispensability arguments as seen in Maddy's examples.

NOTES

[1] In order for this point to remain plausible, it seems clear that a 'should' should be inserted here – mathematicians may believe all sorts of things about ontology – the salient point as far as Colyvan's suggestion goes is what they *should* believe. I am grateful to Colin McLarty for pointing this out to me.

[2] The *Newsweek* quote is taken from Hector J. Sussmann and Raphael S. Zahler's damning (1978) criticism of Catastrophe Theory. It is notable that, while the authors viciously tear apart the claims of Catastrophe Theorists to have found any plausible applications for their subject, they also take pains to "emphasize that the validity or importance of CT from a mathematical standpoint is not at issue here". The authors add that they "are concerned only with evaluating the usefulness of CT for extra-mathematical applications" (Sussmann 1978, 118).

[3] Colyvan has pointed out to me that he has a very liberal conception of application, which might result in there being very little genuinely recreational mathematics. Nevertheless, it would seem that to account for the behaviour of mathematicians in ignoring scientific results he would have to allow that the default attitude of mathematicians should be to see their work as recreational, in his sense, as only this would explain their lack of concern for scientific developments.

[4] This fits nicely with a point against indispensability arguments raised by Alan Musgrave: "if natural numbers do exist, they exist of necessity in all possible worlds. If so, no empirical evidence concerning the nature of the actual world can tell against them. If so, no empirical evidence can tell in favour of them either" (Musgrave 1986, 90–91).

[5] That is, a mathematical realist who believes that our knowledge of mathematical objects is through some form of mathematical intuition, and does not depend on the usefulness of mathematical objects in science.

[6] Although I have been arguing against Quine's indispensability argument in this paper, it does seem to me to be correct to hold that ontological questions should be answered by science alone. At any rate, for reasons of space, I will not consider the Gödelian Platonist alternative in this paper.

[7] I should at this point make clear that the claim that all mathematics is recreational is not simply a standard version of 'if-thenism' (although it is close to at least one possible understanding of this view). The 'if-thenist' picture of mathematics sees the claims of pure mathematics as being of the form "This theorem follows from these axioms", so that a mathematical claim that P is true is actually a claim that the conditional statement, "If [axioms], then P", is true. To say, as the recreationalist account does, that the justification for a claim that a mathematical statement is true is based on its following from axioms and definitions, does make the view appear close to 'if-thenism'.

Two important differences should be noted, however. First of all, different species of 'if-thenism' can be defended depending on how one chooses to understand the claim that "If [axioms], then *P*" is true. To avoid unacceptable (to the anti-realist) metaphysical assumptions, this claim is perhaps best interpreted syntactically as saying that "If [axioms], then *P*' is a logical truth. However, our recreationalist account need not stick so closely to the idea that truth consists of derivability within a formal system. By invoking the wider notion of a mathematical game, the view that all mathematics is recreational can account for a variety of mathematical arguments which may not always be reinterpreted as logically valid derivations.

Secondly, unlike the 'if-thenist' account, which wishes to recast mathematical truth as the logical truth of conditional statements, the recreationalist account is compatible with a substantial notion of the truth of unconditional mathematical statements, provided that it is allowed that truth in mathematics comes apart from empirical truth. The species of recreationalist account I favour sees mathematical truth as assertibility within a mathematical game. Unlike 'if-thenist' alternatives, if one sees mathematics as consisting of informal inference games, and mathematical truth as assertibility within such a game, then unconditional assertions of mathematical statements as true are acceptable. There is, of course, some work to be done to support the idea that assertibility within a mathematical game can provide the recreationalist with a *truth* predicate for mathematics, but I shall not have the space to defend this view in this paper.

[8] Meaningless in the sense of having no reference. A Gödelian Platonist who took a 'modelling' view of the relationship between mathematics and science would clearly not wish to call mathematical terms meaningless. This picture comes from the assumption that Quine's indispensability arguments are the only good reason one might have for realism in mathematics. From a Quinean perspective, if mathematics is not referred to in science, its objects do not exist and mathematical statements which appear to refer to mathematical objects are meaningless.

[9] Briefly, although Resnik's paper does allow for mathematics to be used to model science, he argues that "[e]ven when they develop a purely speculative theory or a highly idealized model, scientists presuppose the truth of the mathematics they use. For the models will not have the properties they are supposed to have unless the background mathematics holds" (Resnik 1995, 169). If the recreational mathematics picture is correct, then all that is required for the usefulness of mathematical models of scientific phenomena is that they are consistent and reasonably interpreted in terms of the scientific phenomena under study. The consistency assumption is dealt with in what follows.

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Department of Philosophy
University of Toronto
215 Huron Street, 9th Floor
Toronto, Ontario M5S 1A1
Canada
E-mail: mcleng@chass.utoronto.ca