MARK COLYVAN

THE MIRACLE OF APPLIED MATHEMATICS

ABSTRACT. Mathematics has a great variety of applications in the physical sciences. This simple, undeniable fact, however, gives rise to an interesting philosophical problem: why should physical scientists find that they are unable to even state their theories without the resources of abstract mathematical theories? Moreover, the formulation of physical theories in the language of mathematics often leads to new physical predictions which were quite unexpected on purely physical grounds. It is thought by some that the puzzles the applications of mathematics present are artefacts of out-dated philosophical theories about the nature of mathematics. In this paper I argue that this is not so. I outline two contemporary philosophical accounts of mathematics that pay a great deal of attention to the applicability of mathematics and show that even these leave a large part of the puzzles in question unexplained.

1. THE UNREASONABLE EFFECTIVENESS OF MATHEMATICS

The physicist Eugene Wigner once remarked that

> [t]he miracle of the appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift which we neither understand nor deserve. (Wigner 1960, 14)

Steven Weinberg is another physicist who finds the applicability of mathematics puzzling:

> It is very strange that mathematicians are led by their sense of mathematical beauty to develop formal structures that physicists only later find useful, even where the mathematician had no such goal in mind. [. . .] Physicists generally find the ability of mathematicians to anticipate the mathematics needed in the theories of physics quite uncanny. It is as if Neil Armstrong in 1969 when he first set foot on the surface of the moon had found in the lunar dust the footsteps of Jules Verne. (Weinberg 1993, 125)

Mark Steiner also believes that there is a problem here worthy of attention:

> [H]ow does the mathematician – closer to the artist than the explorer – by turning away from nature, arrive at its most appropriate descriptions? (Steiner 1995, 154)

Indeed, this puzzle,[1] which Wigner calls "the unreasonable effectiveness of mathematics", is often remarked upon by physicists and applied

Synthese **127:** 265–277, 2001.

mathematicians[2] but receives surprisingly little attention in the philosophical literature. It is hard to say why this puzzle has not caught the imagination of the philosophical community. It is not because it's unknown in philosophical circles. On the contrary, it is very well known; it just does not get discussed. This lack of philosophical attention, I believe, is due (in part) to the fact that the way the problem is typically articulated seems to presuppose a formalist philosophy of mathematics.[3]

Given the decline of formalism as a credible philosophy of mathematics in the latter half of the twentieth century, and given the rise of anti-realist philosophies of mathematics that pay great respect to the applicability of mathematics in the physical sciences (such as Hartry Field's fictionalism (Field 1980)), it is worth reconsidering Wigner's puzzle to see to what extent, if any, it relies on a particular philosophy of mathematics. The central task of this paper is to argue that although Wigner set the puzzle up in language that suggested an anti-realist philosophy of mathematics, it appears that the puzzle is independent of any particular philosophy of mathematics. At least, a version of the puzzle can be posed for two of the most influential, contemporary philosophies of mathematics: one realist, the other anti-realist.

In the next section I outline the puzzle in more detail and give an example of the unreasonably effective role mathematics plays in physical science. In Section 3 I discuss the suggestion that the puzzle is an artefact of a particular philosophy of mathematics. In the following two sections, Sections 4 and 5, I present two influential, contemporary positions in the philosophy of mathematics that, to my mind, pay the greatest attention to the applicability of mathematics and show that neither of these provide a solution to the puzzle.

2. WHAT IS THE PUZZLE?

Mark Steiner is one of the few philosophers to take interest in Wigner's puzzle (Steiner 1989, 1995, 1998). Steiner has quite rightly suggested that Wigner's "puzzle" is in fact a whole family of puzzles that are not distinguished by Wigner; it depends on what you mean by 'applicability' when talking of the applications of mathematics. Steiner claims that it is important to distinguish the different senses of 'applicability' because some of the associated puzzles are easily solved while others are not. For example, Steiner argues that the problem of the (semantic) applicability of mathematical theorems[4] was explained adequately by Frege (Steiner 1995). There is, according to Steiner, however, a problem which Frege did not address. This is the problem of explaining the appropriateness of mathematical concepts for the description of the physical world. Of particular interest here are cases where the mathematics seems to be playing a crucial role in making predictions. Moreover, Steiner has argued for his own version of Wigner's thesis. According to Steiner, the puzzle is not simply the extraordinary appropriateness of mathematics for the formulation of physical theories, but concerns the role mathematics plays in the very discovery of those theories. In particular, this requires an explanation that is in keeping with the methodology of mathematics – a methodology that does not seem to be guided at every turn by the needs of physics.

The problem is epistemic: why is mathematics, which is developed primarily with aesthetic considerations in mind, so crucial in both the discovery and the statement of our best physical theories? Put this way the problem may seem like one aspect of a more general problem in the philosophy of science – the problem of justifying the appeal to aesthetic considerations such as simplicity, elegance, and so on. This is not the case though. Scientists and philosophers of science invoke aesthetic considerations to help decide between two theories that are empirically equivalent. Aesthetics play a much more puzzling role in the Wigner/Steiner problem. Here aesthetic considerations are largely responsible for the development of mathematical theories. These, in turn, (as I will illustrate shortly) play a crucial role in the discovery of our best scientific theories. In particular, novel *empirical* phenomena are discovered via mathematical analogy. In short, aesthetic considerations are not just being invoked to decide between empirically equivalent theories; they seem to be an integral part of the process of scientific discovery.

I find Steiner's statement of the puzzle clearer and more compelling so when I speak of Wigner's puzzle I will have Steiner's version in mind. I will thus concentrate on cases where the mathematics seems to be playing an active role in the discovery of the correct theory – not just in providing the framework for the statement of the theory. I'll illustrate this puzzle by presenting one rather classic case and refer the interested reader to Steiner's article (Steiner 1989) and book (Steiner 1998) for further examples.[5] In the case I'll consider here, we see how Maxwell's equations predicted electromagnetic radiation.

Maxwell found that the accepted laws for electromagnetic phenomena prior to about 1864, namely Gauss's law for electricity, Gauss's law for magnetism, Faraday's law, and Ampère's law, jointly contravened the conservation of electric charge. Maxwell thus modified Ampère's law to include a *displacement current*, which was not an electric current in the usual sense (a so-called *conduction current*), but a rate of change (with respect to time) of an electric field. This modification was made on the basis of formal mathematical analogy, *not* on the basis of empirical evidence.[6] The analogy was, of course, with Newtonian gravitational theory's conservation of mass principle. The modified Ampère law states that the curl of a magnetic field is proportional to the sum of the conduction current and the displacement current:

$$(1) \qquad \nabla \times \mathbf{B} = \frac{4\pi}{c}\mathbf{J} + \frac{1}{c}\frac{\partial}{\partial t}\mathbf{E}.$$

Here **E** and **B** are the electric and magnetic field vectors respectively, **J** is the current density, and c is the speed of light in a vacuum.[7] When this law (known as the Maxwell-Ampère law) replaces the original Ampère law in the above set of equations, they are known as Maxwell's equations and they provide a wonderful unity to the subject of electromagnetism.

The interesting part of this story for the purposes of the present discussion, though, is that Maxwell's equations were formulated on the assumption that the charges in question moved with a constant velocity, and yet such was Maxwell's faith in the equations, he assumed that they would hold for *any* arbitrary system of electric fields, currents, and magnetic fields. In particular, he assumed they would hold for charges with accelerated motion and for systems with zero conduction current. An unexpected consequence of Maxwell's equations followed in this more general setting: a changing magnetic field would produce a changing electric field and *vice versa*. Again from the equations, Maxwell found that the result of the interactions between these changing fields on one another is a wave of electric and magnetic fields that can propagate through a vacuum. He thus predicted the phenomenon of electromagnetic radiation. Furthermore, he showed that the speed of propagation of this radiation is the speed of light. This was the first evidence that light was an electromagnetic phenomenon.[8]

It seems that these predictions (which were confirmed experimentally by Heinrich Hertz in 1888 – nine years after Maxwell's death) can be largely attributed to the mathematics, since the predictions were being made for circumstances beyond the assumptions of the equations' formulation. Moreover, the formulation of the crucial equation (the Maxwell-Ampère law) for these predictions was based on formal mathematical analogy. Cases such as this *do* seem puzzling, at least when presented a certain way. The question on which I wish to focus is whether the puzzlement is an artefact of the presentation (because some particular philosophy of mathematics is explicitly or implicitly invoked), or whether these cases are puzzling *simpliciter*. I will argue that it is the latter.

3. IS THE PUZZLE DUE TO A PARTICULAR PHILOSOPHY OF MATHEMATICS?

Applicability has long been the Achilles' heel of anti-realist accounts of mathematics. For example, if you believe that mathematics is some kind of formal game – as Hilbert did – then you need to explain why mathematical theories are needed to such an extent in our descriptions of the world. After all, other games, like chess, do not find themselves in such demand. Or if you think that mathematics is a series of conditionals – '$2 + 2 = 4$' is short for 'If the Peano-Dedekind axioms hold then $2 + 2 = 4$' – the same challenge stands.

In Wigner's article he seems to be taking a distinctly anti-realist point of view:

> [M]athematics is the science of skillful operations with concepts and rules *invented* just for that purpose. (Wigner 1960, 2)

(My italics.) Others, such as Reuben Hersh, also adopt anti-realist language when stating the problem:[9]

> There is no way to deny the obvious fact that arithmetic was *invented* without any special regard for science, including physics; and that it turned out (unexpectedly) to be needed by every physicist. (Hersh 1990, 67)

(Again, my italics.) Some, such as Paul Davies (1992, 140–60) and Roger Penrose (1990, 556–7), have suggested that the unreasonable effectiveness of mathematics in the physical sciences is evidence for realism about mathematics. That is, there is only a puzzle here if you think we invent mathematics and then find that this invention is needed to describe the physical world. Things aren't that simple though. There are contemporary anti-realist philosophies of mathematics that pay a great deal of attention to applications, and it is not clear that these suffer the same difficulties that formalism faces. Furthermore, it is not clear that realist philosophies of mathematics escape the difficulties. In what follows I will argue that there are puzzles for both realist and anti-realist philosophies of mathematics with regard to accounting for the unreasonable effectiveness of mathematics.

I will consider one influential realist philosophy of mathematics due to Quine (1980) and Putnam (1979) and one equally influential anti-realist position due to Hartry Field (1980). Both of these philosophical positions are motivated by, and pay careful attention to, the role mathematics plays in physical theories. It is rather telling, then, that each suffers similar problems accounting for Wigner's puzzle.

4. QUINEAN REALISM

Quine (1980) and Putnam (1979) have argued that we are committed to the existence of all the entities quantified over in our best scientific theories (once these theories are suitably formalised). That is, if talk of some entity ξ is indispensable to a theory T, and T is our best scientific theory of some phenomena, then we are committed to the existence of ξ. According to Quine and Putnam, quantification over mathematical entities is indispensable to our best scientific theories in precisely this way. Their conclusion is that we are committed to the existence of mathematical entities. Moreover, according to this line of thought, the existence of mathematical entities is empirically confirmed by exactly the same evidence that confirms the theory as a whole. So, for instance, Quine believes in the existence of sets for exactly the same reasons he believes in the existence of electrons. Crucial to this argument are the doctrines of *naturalism* – which delivers the commitment to entities required by our best scientific theories – and *holism* – which prohibits interpreting parts of these theories non-realistically.[10] Although both these doctrines have been called into question in various ways,[11] the argument remains influential, and Quinean realism is one of the more important contemporary positions in the philosophy of mathematics.

It is also worth mentioning that a great deal hangs on how we understand 'indispensable' in this context. I have argued elsewhere that an entity is dispensable to a theory T if there exists a second theory T' with the following properties: (i) T' has exactly the same observational consequences as T, but in T' the entity in question is neither mentioned nor predicted and (ii) T' is preferable to T (Colyvan 1999, 5). Thus, whether an entity is indispensable or not is really a question of theory choice and so is guided by the usual canons of theory choice. These may include: simplicity, unificatory power, boldness, formal elegance and so on. It seems, then, that an entity can be indispensable even though empirically equivalent theories exist that do not quantify over the entity in question. The entity might turn out to be indispensable because, for example, it's part of a theory that is more fruitful than its competitors.[12]

Now, granted all this, it might be thought that the Quinean realist has a response to Wigner. The Quinean could follow the lead of scientific realists such as J. J. C. Smart who put pressure on anti-realists by exposing their inability to explain the applications of electron theory, say. It's no miracle, claim scientific realists, that electron theory is remarkably effective in describing all sorts of physical phenomena such as lightning, electromagnetism, the generation of X-rays in Roentgen tubes and so on. Why is it no miracle? Because electrons exist and are at least partially causally responsible for the phenomena in question. Furthermore, it's no surprise that electron theory is able to play an active role in novel discoveries such as superconductors. Again this is explained by the existence of electrons and their causal powers. There is, however, a puzzle here for the anti-realist. As Smart points out:

> Is it not odd that the phenomena of the world should be such as to make a purely instrumental theory true? On the other hand, if we interpret a theory in a realist way, then we have no need for such a cosmic coincidence: it is not surprising that galvanometers and cloud chambers behave in the sort of way they do, for if there really are electrons, etc., this is just what we should expect. A lot of surprising facts no longer seem surprising. (Smart 1963, 39)

There is an important disanalogy, however, between the case of electrons and the case of sets. Electrons have causal powers – they can bring about changes in the world. Mathematical entities such as sets are usually taken to be causally idle – they are platonic in the sense that they do not exist in space-time nor do they have causal powers. So how is it that the positing of such platonic entities reduces mystery?[13] Cheyne and Pigden (1996) have recently suggested that in light of this, the Quinean is committed to causally active mathematical entities. While I dispute the cogency of Cheyne's and Pigden's argument (see Colyvan 1998), I agree that there is a puzzle here. The puzzle is this: on Quine's view, mathematics is seen to be part of a true description of the world because of the indispensable role mathematics plays in physical theories, but the Quinean account gives us no indication as to *why* mathematics is indispensable to physical science. That is, Quine does not explain why mathematics is required in the formulation of our best physical theories and, even more importantly, he does not explain why mathematics is so often required for the discovery of these theories. Indispensability is simply taken as brute fact.

It might be tempting to reply, on behalf of Quine, that mathematics is indispensable because it's true. This, however, will not do. After all, there are presumably many truths that are not indispensable to our best scientific theories. What is required is an account of why mathematical truths, in particular, are indispensable to science. Moreover, we require an account of why mathematical methods which, as Steiner points out (Steiner 1995, 154), are closer to those of the artist's than those of the explorer's, are reliable means of finding the mathematics that science requires. It is these issues, lying at the heart of the Wigner/Steiner puzzle, that Quine does not address.

The above statement of the problem for Quine can easily be extended to any realist philosophy of mathematics that takes mathematical entities to be causally inert. This suggests that one way to solve the puzzle in question is to follow Cheyne's and Pigden's suggestion and posit causally *active* mathematical entities (*a la* early Maddy (1990) or Bigelow (1988)). Now such physicalist strategies may or may not solve Wigner's puzzle.[14] But it is not my concern in this paper to decide which realist philosophies fall foul of Wigner's puzzle and which do not. My concern is to demonstrate that realist philosophies of mathematics do not, *in general*, escape the problem. In particular, I have shown that Quine's influential realist philosophy of mathematics, at least if taken to be about abstract objects, succumbs to Wigner's puzzle.

5. FIELD'S FICTIONALISM

Hartry Field's (1980) response to Quine's argument is to claim that mathematics is, in fact, dispensable to our best physical theories. He adopts a fictional account of mathematics in which all the usually accepted sentences of mathematics are literally false, but *true-in-the-story* of accepted mathematics. There are two parts to Field's project. The first is to justify the use of mathematics in its various applications in empirical science (since, according to Field, mathematics is a body of false propositions). To do this Field argues that mathematical theories don't have to be true to be useful; they merely need to be *conservative*. This is, roughly, that if a mathematical theory is added to a nominalist scientific theory, no nominalist consequences follow that wouldn't follow from the nominalist scientific theory alone. The second part of Field's program is to show that our best scientific theories can, in fact, be nominalised. To this end he is content to nominalise a large fragment of Newtonian gravitational theory. Although this is a long way short of showing that *all* our current best scientific theories can be nominalised, it is certainly not trivial. The hope is that once one sees how the elimination of reference to mathematical entities can be achieved for a typical physical theory, it will not seem unreasonable to expect that the project could be completed for other scientific theories.

There are many complaints against Field's program.[15] While such debates are certainly not without interest, I will not discuss them here. Instead, I wish to point out that despite Field's careful attention to the applications of mathematics, he leaves himself open to Wigner's puzzle. Field explains why we can use mathematics in physical theories – because mathematics is conservative. He also explains why mathematics often finds its way into physical theories – because mathematics simplifies calculations and the statement of these theories. What he fails to provide is an account of why mathematics leads to simpler theories and simpler calculations. Moreover, Field gives us no reason to expect that mathematics will play an active role in the prediction of novel phenomena.[16]

If I'm correct that facilitating novel scientific predictions (via mathematical analogy) is at least partly why we consider mathematics indispensable to science, then Field has not fully accounted for the indispensability of mathematics until he has provided an account of the active role mathematics plays in scientific discovery. So although Field did not set out to provide a solution to *this* particular problem of applicability (i.e. the Steiner/Wigner problem), it seems that, nevertheless, he is obliged to. (Indeed, this was the basis of my criticism of Field in Colyvan (1999).) On the other hand, if this shortcoming of his project is seen (as I'm now suggesting) as part of the more general problem of applicability – a problem that Quine too faces – Field's obligation in this regard is not so pressing. In short, it's a problem for everyone.

Now the fact that Field *does not* provide a solution to Wigner's puzzle does not mean that he *cannot* do so. But whether he can provide a solution or not, *the puzzle needs to be discussed* and that is all I am arguing for here. Still, let me put to rest one obvious response Field may be tempted with.[17] He might appeal to the structural similarities between the empirical domain under consideration and the mathematical domain used to model it, to explain the applicability of the latter. So, for example, the applicability of real analysis to flat space-time is explained by the structural similarities between $\mathbb{R}^4$ (with the Minkowski metric) and flat space-time. There is no denying that this is right, but this response does not give an account of why mathematics leads to novel predictions and facilitates simpler theories and calculations. Appealing to structural similarities between the two domains does not explain, for example, why mathematics played such a crucial role in the prediction of electromagnetic radiation. Presumably certain mathematical structures in Maxwell's theory (which predict electromagnetic radiation) are similar to the various physical systems in which electromagnetic radiation is produced (and it would seem that there are no such structural similarities with the pre-Maxwell theory). But then Wigner's puzzle is to explain the role mathematical analogy played in the development of Maxwell's theory. The fact that Maxwell's theory is structurally similar to the physical system in question is simply irrelevant to this problem.

6. CONCLUSION

To sum up then. I agree with Steiner that the applicability of mathematics presents a general problem. What I hope to have shown, and what

Steiner does not address, is that the problem exists for at least two major contemporary positions in the philosophy of mathematics. Moreover, the two positions I discuss – Field's and Quine's – I take to be the two that are the most sensitive to the applications of mathematics in the physical sciences. The fact that these two influential positions do not seem to be able to explain Wigner's puzzle, clearly does not mean that *every* philosophy of mathematics suffers the same fate. It does show, however, that Wigner's puzzle is not merely a difficulty for unfashionable formalist theories of mathematics.

While the problems I've discussed in this paper for both Quine and Field are not new, they can now be seen in a new light. Previously each problem was seen as a difficulty for the particular account in question (in the context of the realism/anti-realism debate). That is, whenever these problems were discussed (and I include myself here (Colyvan 1999), they were presented as reasons to reject the account in question, in favour of another. If what I'm suggesting now is correct, that is the wrong way of looking at it. There are striking similarities between the problem that Burgess and I have pointed out for Field and the problem that Balaguer and others have pointed out for Quine. I claim that these problems are best seen as manifestations of the unreasonable effectiveness of mathematics. Moreover, these difficulties seem to cut across the realism/anti-realism debate and thus deserve careful attention from contemporary philosophers of all stripes – realists and anti-realists alike.[18]

NOTES

[1] By calling it 'a puzzle' I do not mean to trivialise it. On the contrary, as I will argue in this paper, I think it is a deep and important problem in the philosophy of mathematics.

[2] For example: Paul Davies (1992, 140–60); Freeman Dyson (1964); Richard Feynman (1965, 171); R. W. Hamming (1980); Steven Weinberg (1986) and many others in Mickens (1990).

[3] Saunders Mac Lane (1990), for example, explicitly takes the puzzle to be a puzzle for formalist philosophies of mathematics. Others have taken the problem to be a problem for anti-realist philosophies of mathematics generally. See, for example, Paul Davies (1992, 140–60) and Roger Penrose (1990, 556–7). One exception here is Philip Kitcher (1984, 104–5) who presents it as a problem for platonism. I will discuss, what is in essence, Kitcher's problem in Section 4.

[4] This is the problem of explaining the validity of mathematical reasoning in both pure and applied contexts – to explain, for instance, why the truth of (i) there are 10 Victorian-based AFL football teams, (ii) there are 6 non-Victorian AFL football teams and, (iii) $6+10 = 16$, implies that there are 16 AFL football teams. (The problem is that in (i) and (ii) '10' and '6' seem to act as names of predicates and yet in (iii) '10' and '6' apparently act as names

of objects. What we require is a constant interpretation of the mathematical vocabulary across such contexts.

5 Steiner distinguishes between two quite different, but equally puzzling, ways in which mathematics has facilitated the discovery of physical theories: *Pythagorean analogy* and *formalist analogy*. Although this distinction is of considerable interest, it has no bearing on the main thesis of this paper, so I will ignore it. See Colyvan (2000) and Steiner (1998, 2–11) for details.

6 Indeed, there was very little (if any) empirical evidence at the time for the displacement current.

7 The first term on the right of Equation (1) is the conduction current and the second on the right is the displacement current.

8 Actually the story is a little more complicated than this. Maxwell originally had a mechanical model of electromagnetism in which the displacement current was a physical effect. (For the details of the relevant history, see Chalmers (1973), Hunt (1991) and Siegel (1991).) This, however, does not change the fact that there was little (if any) empirical evidence for the displacement current and the reasoning that led to the prediction of electromagnetic radiation went beyond the assumptions on which either the equations or the mechanical model were based (Steiner 1998, 77–8).

9 Also recall Weinberg's reference to Jules Verne in the passage I quoted earlier and Steiner's remark (again quoted earlier) about the mathematician being more like an artist than an explorer.

10 So, for example, holism counsels us to believe all of quantum theory (since it's our best theory of the sub-atomic), and this involves realism about all the entities quantified over in this theory. In particular, it rules against believing in some of the entities (electrons, say) but not others (complex numbers, say).

11 For example, see Maddy (1992) for a discussion of the tensions between naturalism and holism in Quine's philosophy of mathematics.

12 Note that a theory may be preferred because of the richness of the mathematical concepts it has to draw upon (for both purposes of analogy and description). For example, Geoffrey Hellman has argued against scientific theories that are limited to constructive mathematics on these grounds (Hellman 1992) and Alan Baker has argued that mathematics itself is often required for scientific progress (Baker, forthcoming). It is clear, I think, that in such cases we are not concerned with merely the semantic applicability of mathematics; we are also concerned with the applicability of mathematical concepts (and even the applicability of the concept of mathematics itself).

13 A few people have pointed to this problem in Quine's position (see Balaguer (1998, 110–1), Field (1998, 400), Kitcher (1984, 104–5) and Shapiro (1997, 46)).

14 It's not clear to me that they do.

15 See, for example, Malament (1982) and Resnik (1985) for some of these.

16 I discuss this matter in more detail in Colyvan (1999) and in Chapter 4 of Colyvan (2001). John Burgess (1983) raises similar issues.

17 Mark Balaguer seems to have something like this response in mind when he says that "I do not think it would be very difficult to solve this general problem of applicability [of mathematics]" (Balaguer 1998, 144). It should also be mentioned that if this response were successful, it would also be available to realist philosophies of mathematics

18 Earlier versions of this paper were presented at the University of Tasmania School of Mathematics and Physics Colloquia, at the 1999 Australasian Association of Philosophy Conference, to the Department of Philosophy at Smith College, to the Department of Philo-

sophy at the University of California, Santa Cruz, and to the Department of Philosophy at SUNY, Stony Brook. I thank the participants in the subsequent discussions for their valuable contributions. I also gratefully acknowledge the help of Adam Elga, Mark Steiner, Neil Thomason, and the anonymous readers for *Synthese*.

REFERENCES

Baker, A. R.: (forthcoming) 'Mathematics Indispensability and Scientific Progress', *Erkenntnis*.

Balaguer, M.: 1998, *Platonism and Anti-Platonism in Mathematics*, New York, Oxford University Press.

Bigelow, J.: 1988, *The Reality of Numbers: A Physicalist's Philosophy of Mathematics*, Oxford, Clarendon.

Burgess, J. P.: 1983, 'Why I Am Not a Nominalist', *Notre Dame Journal of Formal Logic* **24**, 93–105.

Chalmers, A. F.: 1973, 'Maxwell's Methodology and His Application of it to Electromagnetism', *Studies in History and Philosophy of Science* **4**, 107–164.

Cheyne, C. and C. R. Pigden: 1996, 'Pythagorean Powers or a Challenge to Platonism', *Australasian Journal of Philosophy* **74**, 639–645.

Colyvan, M.: 1998, 'Is Platonism a Bad Bet?', *Australasian Journal of Philosophy* **76**, 115–119.

Colyvan, M.: 1999, 'Confirmation Theory and Indispensability', *Philosophical Studies* **96**, 1–19.

Colyvan, M.: 2000, 'Review of Steiner's, *The Applicability of Mathematics as a Philosophical Problem*,' *Mind* **109**, 390–394.

Colyvan, M.: 2001, *The Indispensability of Mathematics*, New York, Oxford University Press.

Davies, P.: 1992, *The Mind of God*, London, Penguin.

Dyson, F. J.: 1964, 'Mathematics in the Physical Sciences', *Scientific American* **211**, 128–146.

Feynman, R. P.: 1965, *The Character of Physical Law*, London, BBC.

Field, H. H.: 1980, *Science Without Numbers: A Defence of Nominalism*, Oxford, Blackwell.

Field, H. H.: 1998, 'Mathematical Objectivity and Mathematical Objects', in S. Laurence and C. Macdonald (eds), *Contemporary Readings in the Foundations of Metaphysics*, Oxford, Blackwell, pp. 387–403.

Hamming, R. W.: 1980, 'The Unreasonable Effectiveness of Mathematics', *American Mathematics Monthly* **87**, 81–90.

Hellman, G.: 1992, 'The Boxer and His Fists: The Constructivist in the Arena of Quantum Physics', *Proceedings of the Aristotelian Society, Supplement* **LXVI**, 61–77.

Hersh, R.: 1990, 'Inner Vision Outer Truth', in R. E. Mickens (ed.), *Mathematics and Science*, Singapore, World Scientific Press, pp. 64–72.

Hunt, B. J.: 1991, *The Maxwellians*, Ithaca, NY, Cornell University Press.

Kitcher, P.: 1984, *The Nature of Mathematical Knowledge*, New York, Oxford University Press.

Mac Lane, S.: 1990, 'The Reasonable Effectiveness of Mathematics', in R. E. Mickens (ed.), *Mathematics and Science*, Singapore, World Scientific Press, pp. 115–135.

Maddy, P.: 1990, *Realism in Mathematics*, Oxford, Clarendon.

Maddy, P.: 1992, 'Indispensability and Practice', *Journal of Philosophy* **89**, 275–289.
Malament, D.: 1982, 'Review of Field's *Science Without Numbers*', *Journal of Philosophy* **79**, 523–534.
Mickens, R. E. (ed.): 1990, *Mathematics and Science*, Singapore, World Scientific Press.
Penrose, R.: 1990, *The Emperor's New Mind: Concerning Computers, Minds and the Laws of Physics*, London, Vintage.
Putnam, H.: 1979, 'Philosophy of Logic', reprinted in *Mathematics Matter and Method: Philosophical Papers Vol. I*, 2nd edition, Cambridge, Cambridge University Press, pp. 323–357.
Quine, W. V.: 1980, 'On What There Is', reprinted in *From a Logical Point of View*, 2nd edition, Cambridge, MA, Harvard University Press, pp. 1–19.
Resnik, M. D.: 1985, 'How Nominalist is Hartry Field's Nominalism?', *Philosophical Studies* **47**, 163–181.
Shapiro, S.: 1997, *Philosophy of Mathematics: Structure and Ontology*, New York, Oxford University Press.
Siegel, D. M.: 1991, *Innovation in Maxwell's Electromagnetic Theory*, Cambridge, Cambridge University Press.
Smart, J. J. C.: 1963, *Philosophy and Scientific Realism*, New York, Routledge and Kegan Paul.
Steiner, M.: 1989, 'The Application of Mathematics to Natural Science', *Journal of Philosophy* **86**, 449–480.
Steiner, M.: 1995, 'The Applicabilities of Mathematics', *Philosophia Mathematica (3)* **3**, 129–156.
Steiner, M.: 1998, *The Applicability of Mathematics as a Philosophical Problem*, Cambridge, MA, Harvard University Press.
Weinberg, S.: 1986, 'Lecture on the Applicability of Mathematics', *Notices of the American Mathematical Society* **33**, 725–728.
Weinberg, S.: 1993, *Dreams of a Final Theory*, London, Vintage.
Wigner, E. P.: 1960, 'The Unreasonable Effectiveness of Mathematics in the Natural Sciences', *Communications on Pure and Applied Mathematics* **13**, 1–14.

School of Philosophy
University of Tasmania
GPO Box 252-41
Hobart
Tasmania 7001
Australia
Email: mark.colyvan@utas.edu.au