Scientific Platonism

[published in M. Leng, M.Potter & A.Paseau (eds), *Mathematical* Knowledge, Oxford: Oxford University Press, 2007, pp. 123-149]

Alexander Paseau

1. Introduction

Ask a typical mathematician whether the truth of mathematical statements and the metaphysics of mathematics are settled by natural science, and the answer is likely to be that they are not. He or she might add that many questions of mathematical interest are connected to or originally arose from applications in science, but that the applications of mathematics to science are derivative and that they do not settle its truth or metaphysics. Ask a typical post-Quinean analytic philosopher the same question, and the answer might well be that the truth and metaphysics of mathematics can *only* be settled by considering its scientific applications. I stereotype of course; there are plenty of mathematicians and analytic philosophers who think otherwise, and plenty more who have no opinion to speak of. But such are the broad tendencies. Who is right?

The two questions must be distinguished. First, does natural science give us reason to believe that mathematical statements are true? Second, does natural science give us reason to believe in some particular metaphysics of mathematics? My argument here will be that a negative answer to the second question is compatible with an affirmative answer to the first. Loosely put, even if science settles the truth of mathematics, it does not settle its metaphysics. An epistemological implication is that a scientific defence of our knowledge of mathematical truth need not amount to a defence of knowledge of mathematical objects.

2. Preliminaries

Talk of science settling the truth or metaphysics of mathematics is loose and needs to be cashed out more precisely. We begin by stating the thesis to be defended and then spend most of this chapter's first half clarifying, motivating, and situating it. The second half defends the thesis against some objections.

Let scientific platonism provisionally be the doctrine that scientific standards endorse platonistically interpreted mathematics. The objection to scientific platonism to be articulated is that there is a gap between scientific endorsement of truth-value realism about mathematics and scientific endorsement of platonism. Even if scientific standards endorse the truth of mathematics under some interpretation, I argue, they might not endorse its platonist interpretation. If I am right, a stronger claim is in fact true: scientific standards do not endorse *any* specific interpretation of mathematics. Thus I shall defend the second of the typical mathematician's views, that the nature of mathematics is in this sense independent of natural science, assuming for the sake of argument that science endorses the truth of mathematics.

Platonic or abstract entities are understood in broad brush terms, as entities neither in space nor time. By 'science' we mean throughout natural science (physics, chemistry, biology, etc.). The expressions 'scientific standards', 'scientific grounds', 'scientific reasons', 'scientific norms', etc., are taken as synonymous, as are 'endorsement', 'recommendation', 'justification', etc. To say that scientific standards endorse *p* or that astrological standards endorse *q* is not thereby to endorse *p* or *q* oneself, but merely to point out that these propositions are supported by these respective standards. Thus to claim that *q* is endorsed on astrological grounds—because, say, astrologers infer *q* from the planets' alignment—is not to claim or imply that anyone should accept that as a reason for believing *q*. What I call scientific standards' endorsement of proposition *p* could in a different idiom be expressed by saying that *p* is scientifically confirmed. Endorsement by scientific standards is thus simply scientific confirmation.

Scientific standards are the standards underlying theory evaluation in the natural sciences. Empirical adequacy—agreement with empirical data[1]—for example, is a scientific standard, indeed the paradigm scientific standard, whereas compatibility with the sayings of some sacred person or text is not. Some form of the principle of simplicity,

[1] In practice of course, most actual theories fail to mesh with all the empirical data—they contain 'anomalies'—even from their inception. 'All theories, in this sense, are born refuted and die refuted' famously wrote Lakatos, because they contain 'unsolved problems' and 'undigested anomalies' (1978, p. 5). The better confirmed the theory, the fewer these anomalies and the easier they are to explain away.

which in its most general version states that the simpler of two theories enjoys a theoretical advantage over the less simple one (in this respect), is also a scientific principle. This follows from generally appreciated facts about scientific practice, for example from the fact that a complicated epicycle theory is generally thought scientifically inferior to one positing a more uniform trajectory, even if the former has been tweaked so as to agree with all existing data. To take another example, scientific standards also recommend unifying theories that account for different groups of phenomena and theories in terms of the same mechanisms, a classic instance being the unification of terrestrial and celestial mechanics in Newtonian mechanics.

Not all standards are scientific. Religious standards are often non-scientific: many religious claims, e.g. that there is an afterlife, or that God directly intervened to cause a tsunami, are endorsed by the standards of various religions but not by scientific standards.[2] Philosophy also serves up some self-conscious appeals to non-scientific standards. Goodman and Quine for example famously began their 1947 nominalist manifesto by declaring that the basis for their nominalism is a fundamental 'philosophical intuition' irreducible to scientific grounds.[3]

It is not always clear whether something is a scientific standard. Many philosophers say that ontological economy is a scientific principle. But the scientific status of the (absolutely) general principle of ontological economy, understood as the claim that any theory with an ontology smaller than that of another theory enjoys a theoretical advantage over it (in this respect), is controversial.[4] In particular, the highly

[2] Some of the claims could potentially be justified by scientific standards (e.g. these standards might recommend, following scrupulous verification of her other predictions, the infallibility of a prophet who claims there is an afterlife); but, as a matter of fact, such claims typically aren't so justified.

[3] 'We do not believe in abstract entities…We renounce them altogether…Fundamentally this refusal is based on a philosophical intuition that cannot be justified by appeal to anything more ultimate.' (1947, p. 105). Goodman and Quine go on to add that this fundamental rejection is fortified by certain a posteriori considerations. Note that Quine in his later, more famous, incarnation (which serves as the inspiration for contemporary scientific platonism) repudiated this intuition.

[4] Different understandings of what it is to have a small ontology result in different versions of this principle. (For example, one might distinguish between quantitative and qualitative economy.) Burgess (1998) argues that scientific standards do not endorse theories with smaller *abstract* ontologies. If he is right, it follows that the fully general

general form that ontological economy takes in the hands of philosophers who invoke it (along with other reasons) to defend, say, resemblance nominalism over a trope theory of properties, or a trope theory over a universals theory, or some particular 'ersatz' modal realism over Lewisian realism in the philosophy of modality, or nominalism over platonism in the philosophy of mathematics, or some regularity theory of natural laws over full-blooded nomological realism, or a semantics based on sentences rather than propositions, etc., *looks* very different from the more local and apparently empirically-grounded form it takes in the hands of scientists. Observe in particular that even the slightest difference between the weighting ontological economy is given in cases of philosophical theory choice and its weighting in typical scientific contexts constitutes a divergence from scientific standards. Indeed, it is the customary complaint of platonists such as John Burgess and Gideon Rosen that many philosophers give greater weight to ontological economy than is scientifically proper.[5]

The claim that there are scientific standards should not be confused with the claim that there is a scientific method, in the sense of a procedure which can or should in practice be used to develop new or better scientific theories. A scientific method is a kind of recipe, whereas scientific standards are evaluative.

Whether or not scientific standards are the same as the evaluative standards of other areas of inquiry, such as the social sciences, the humanities, mathematics, etc., is a deep and important question, but tangential here. A more relevant question concerns intrascientific differences. The scientific platonist assumes what Russell meant when he said that 'men of science, broadly speaking, all accept the same intellectual standards' (1945, p. 599). Yet it is not entirely obvious that the natural sciences all employ the same standards. It is not entirely obvious, for example, that biologists and physicists apply the

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principle of ontological economy is not scientific. Note that many philosophers call a principle along these lines Ockham's Razor. I find this usage unhelpful, because Ockham's Razor is the standard label for the principle that entities should not be multiplied beyond necessity. Without further elaboration, that is a platitude.

[5] 'the reconstructive nominalist [the philosopher who seeks to reconstruct science on nominalist lines] seems to be giving far more weight to factor (iv), economy, or more precisely, to a specific variety thereof, economy of abstract ontology, than do working scientists. And the reconstructive nominalist seems to be giving far less weight to factors (v) and (vi), familiarity and perspicuity' (1997, p. 210).

same set of standards, that is, that they use the same criteria to evaluate their theories. Still less obvious is it that scientific standards have remained fixed throughout history, even following the scientific revolution of the seventeenth century.[6] It is unclear how troubling, if at all, it would be for the scientific platonist to peg her thesis to the scientific standards of some specific era—perhaps she should simply peg it to the standards of 'our era'. More troubling is the prospect that, even at a given time, there might be no such thing as global scientific standards, but just the standards of this or that part of science, or even just the standards of this or that group of scientists. Substantial though they be, let us go along with the scientific platonist's assumptions in order to give scientific platonism a run for its money. In keeping with the assumptions, I shall use the monolithic label 'scientists' for the best (though not infallible) deployers of these standards, slurring over distinctions between them.[7]

Scientific standards can of course endorse propositions to a certain degree or with certain qualifications rather than outright. I ignore such qualifications as they do not affect the discussion.

To say that scientific grounds endorse some theory is roughly to say that scientists correctly endorse it *qua* scientists. Thus scientific grounds could endorse *p* even if all scientists disbelieve *p* for scientific reasons (they might all be mistaken in their scientific evaluations), or even if each scientist disbelieves *p* all things considered (e.g. 'scientific grounds unequivocally support *p* but my overriding religious convictions tell me not-*p*'). It would be nice if there were a reductive analysis of correct endorsement of a proposition *qua* scientist; but to my knowledge none exists. The thesis of scientific platonism is hardly the worse for it.

Endorsement is understood epistemically. If scientific grounds endorse platonism merely as a useful but ultimately false assumption, like, say, the useful but false assumption of the uniform density of some fluid, the conclusion, if we accept scientific

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[6] Larry Laudan is a philosopher known for arguing the opposite: 'The specific and "local" principles of scientific rationality which scientists utilize in evaluating theories are not permanently fixed, but have altered significantly through the course of science.' (1981, p. 144).

[7] The intended sense of 'scientist' is thus not a narrow institutional one: to qualify as a scientist one need not be employed in a university or a scientific research institutition (even if these days most scientists are).

standards, would be not that we should believe platonism, but that it is a useful but false hypothesis. Scientific platonism will accordingly turn out to be false if scientific grounds should recommend acceptance of platonist mathematics only in some non-epistemic sense. Following standard usage, I shall call any such non-epistemic sense of support for a proposition *p* ‘pragmatic’ support for *p*.

We should be clear about the difference between endorsement of a particular interpretation of some sentence and endorsement of the proposition expressed by that sentence under some interpretation. Suppose that history is right in relating that Heraclitus once said ‘παντα χορει και ουδεν μενει’. [8]Proper standards of translation endorse translating Heraclitus as saying (roughly) ‘all is in flux and nothing stays’; but it does not follow that these standards endorse the claim that all is in flux and that nothing stays. Likewise, when we say that scientific standards endorse the platonist interpretation of some sentence *s*, we mean that scientific standards endorse the proposition expressed by *s* interpreted platonistically, not that scientific standards endorse the claim that the platonist interpretation is the right one to put on the claim’s standard utterances or inscriptions. Endorsing *p* is quite distinct from endorsing the claim that an utterance of sentence *s* expresses proposition *p*.

The question of what follows if scientific standards endorse platonism is a fundamental epistemological one. Some scientific platonists have gone as far as to say that if scientific standards endorse *p* then we should believe *p*. This is a fairly extreme form of epistemic naturalism or scientism, which takes scientific standards to trump all others (the most extreme kind of naturalism would strengthen the conditional into a biconditional.) Less extreme views give scientific standards some weight without making them the ultimate authority. The lesser the weight, of course, the less significant the scientific platonist’s thesis. What motivates this paper is sympathy with the view that scientific standards have some say in the philosophy of mathematics. Exploration of logical space for its own sake has its place; but I for one would lose interest in the debate if it became clear that scientific standards should be accorded no weight whatsoever, as the stereotypical mathematician would have it. Be that as it may, our discussion can float

[8] Plato, *Cratylus*, 402a8-9.

free of the difficult epistemological issue of how much weight to accord scientific standards in the evaluation of a philosophy of mathematics.[9]

Scientific platonism is obviously inspired by the Quine-Putnam indispensability argument. The argument, roughly, is that platonism is true because platonist mathematics is indispensable to our best scientific theories. Implicit within this line of thought is the naturalist premise that we had better believe the deliverances of our best scientific theories, whatever the non-scientific reasons to the contrary. The second premise of the indispensability argument is that our best science indispensably contains platonist mathematics. The literature of the past few decades, however, has suggested that a non-platonist mathematics may be developed for the purposes of science (see below). If this is right then platonist mathematics is not in principle indispensable to science. The reply by scientific platonists has been that even if a non-platonist mathematics could be developed, it would still be inferior to platonist mathematics by scientific standards (e.g. this is the main thesis of Burgess and Rosen (1997)). The question thus becomes one of scientific superiority rather than indispensability in the strict sense, and the indispensability question accordingly turns into the question of whether scientific platonism is true. (Arguably, 'indispensability' was understood in this way from the start, despite the choice of word.)[10]

Quine's influence being what it is, particularly in the United States, scientific platonism has plenty of adherents. Contemporary Quineans include Alan Baker, John Burgess, Mark Colyvan, Michael Resnik and Gideon Rosen, among others.[11] It might be

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[9] For discussion of some of the issues here, see my (2005).

[10] For a succinct version of the indispensability argument by Putnam, see Putnam (1971). For Quine, see his famous (1953), and many of his later writings (e.g. the articles in his (1981b)). Resnik (2005) offers an accessible overview of Quine's philosophy of mathematics. Note that Quine often takes science in a broader sense than the one here (see e.g. his (1995, p. 49)). Some philosophers have recently claimed that a version of the indispensability argument can be found in §91 of Frege's *Grundgesetze*. Garavaso (2005) argues that this attribution is mistaken.

[11] See Colyvan (2001) and (2007), Resnik (1997), Burgess and Rosen (1997), Baker (2001). For exactly how to interpret Burgess and Rosen (1997) on this point, see footnotes 12 and 26 below. Resnik's version of the indispensability argument consists of the following two premises (by 'science' he understands natural science): '(1) In stating its laws and conducting its derivations science assumes the existence of many mathematical objects and the truth of much mathematics. (2) These assumptions are

thought somewhat misleading to call these and other Quineans scientific platonists. After all, several of them follow Quine in claiming that philosophy is continuous with science, and they might in principle baulk at the claim that scientific standards endorse Platonism if this is taken to imply a sharp demarcation between natural-scientific standards and philosophical ones. In practice, however, they are happy to speak of scientific standards as relatively well-demarcated. For instance, John Burgess and Gideon Rosen in a section of their 1997 book list the standards generally accepted by descriptive methodologists of science as scientific and use this as a club with which to beat nominalists, accusing them of producing reconstructions that are scientifically inferior when judged by these very standards (1997, p. 209 ff.; see also 2005 pp. 519-20). Baker (2001, *passim*) is another example of someone who speaks of 'scientific grounds' without hesitation. In general, theoretical caution about the perceived or potential continuity of scientific and philosophical grounds does not prevent Quineans from making the case for scientific platonism. The typical scientific platonist concedes that there is no sharp distinction between scientific and non-scientific grounds for belief, and no sharp demarcation of science, but nevertheless insists that this does not undermine scientific platonism.

A potentially stronger reason for not attributing blanket scientific platonism to the mentioned writers is that some of them understand 'science' and 'scientific' in a broad sense that goes beyond the natural sciences. For instance, if one counts mathematics as part of science, one could say that 'scientific' standards in this sense endorse platonism simply because mathematical standards endorse platonism and that natural-scientific standards do not speak against it. This seems to be John Burgess' view, and it might be the official view of Burgess and Rosen (1997).[12] This kind of 'scientific' platonism might

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indispensable to the pursuit of science; moreover many of the important conclusions drawn from and within science could not be drawn without taking mathematical claims to be true.' (1997, pp. 46-7). Colyvan's platonism is discussed below. Baker's platonism stems from his belief that scientific grounds support platonism together with the credo: 'that—given the naturalistic basis of the Indispensability Argument, which rejects the idea of philosophy as a higher court of appeal for scientific judgments,—the only sensible way of judging alternatives to current science is on scientific grounds.' (2001, p. 87).

[12] It is unclear whether Burgess and Rosen (1997) wish to defend scientific platonism or mathematical-cum-scientific platonism or both. Pages 32-5, for instance, seem to point to mathematical-cum-scientific platonism, perhaps augmented by common sense (e.g. 'Another form of objection questions whether there is any viable notion of "justification"

be more accurately termed *mathematical-cum-scientific* platonism. Mathematical-cum-scientific platonism is less controversial than scientific platonism proper, since it is often thought that mathematical standards endorse platonism and that the philosophical project begins when we start questioning whether there is a better all-things-considered account

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other than that constituted by ordinary commonsense and scientific and mathematical standards of justification.' (p. 32; see also p. 211). Elsewhere, however, Burgess and Rosen simply speak of scientific standards (e.g. 'The naturalists' commitment is at most to the comparatively modest proposition that when science speaks with a firm and unified voice, the philosopher is either obliged to accept its conclusions or to offer what are recognizably scientific reasons for restricting them.' (p. 65; see also p. 205)). And the fact that Burgess and Rosen do not define science non-standardly as comprising the natural sciences *as well as* mathematics, and moreover that they use the words 'scientific' and 'science' *in contrast* to 'mathematical' and 'mathematics'—as illustrated by the first quotation from p. 32 and in many other passages—supports interpreting them as scientific platonists and not mathematical-cum-scientific platonists. (Of course there is a tradition of calling mathematics and logic the 'formal sciences' , but the default contemporary understanding of 'science is to denote the natural sciences.) Likewise, saying that correctness and accuracy of observable predictions are among the standards that descriptive methodologists agree are operative in science (p. 209) is suggestive of the natural sciences, as these two standards are not relevant to mathematics, at least not in any literal sense. This unclarity runs throughout their book and is unfortunately never resolved. (It resurfaces for instance in their discussion of the publication-and-reception test, for more on which see footnote 23 below.) Perhaps the most reasonable interpretation is to take Burgess and Rosen (1997) as committed to both theses. Another terminological minefield is John Burgess' self-labelling. Burgess has in recent years taken to calling himself an 'anti-anti-realist' rather than a platonist (see e.g. his (2004)). An anti-anti-realist is said to be someone who does not take back in the philosophy seminar what he says in the mathematics classroom. But since Burgess also thinks that what is said in the mathematics classroom is true, and is intended literally, and that this literal truth entails platonism, it seems to follow that his anti-anti-realism is a version of platonism. In the time-honoured tradition of burdening one's opponents with an inflated version of their actual position, Burgess seems to think that to be a platonist is to be a 'capital-R-realist'. The principal non-metaphorical definition of this character Burgess offers is that a capital-R-realist thinks that 'what one says to oneself in scientific moments when one tries to understand the universe corresponds to Ultimate Metaphysical Reality' (2004, p. 19), a claim which Burgess disowns. Burgess' distinction between capital-R-realism and anti-anti-realism (for more on which, see also 2004, pp. 34-35) is difficult to understand. For one thing, it is difficult to see how metaphysical reality itself could come in various forms, ultimate, penultimate or preliminary: what is the case is simply the case, *c'est tout*. For another, Burgess's distinction between thinking that a statement is true and thinking that it corresponds to reality (in a sense that allows of course that other intelligent beings could correctly conceptualise the world differently from us) is unclear. All in all, even if he dislikes the label, there are good reasons for eliminating his coy double negations and calling Burgess a platonist.

of mathematics. From the perspective of the mathematical-cum-scientific platonist, who thinks that mathematical standards endorse platonism, a discussion that focuses only on scientific standards will accordingly be seen as conceding too much ground to the anti-platonist. Conversely, from the perspective of a philosopher who takes only scientific standards as justificatory, anyone who believes in platonism because it is sanctioned by mathematical standards but not necessarily scientific ones will be seen as begging the question on behalf of platonism. However that may be, we should be clear that mathematical-cum-scientific platonism is quite distinct from our present quarry, *viz.* scientific platonism. The least we can say is that scientific platonism is typically accepted by most contemporary Quineans—indeed, it is almost a definition of being Quinean in this domain—and that for them it usually forms the central plank of their case for platonism, just as it did for Quine. In short, even if you think there are justifications for platonism *other* than scientific ones, if you think that scientific standards justify platonism, that makes you a scientific platonist.

The unavoidably lengthy preliminaries finally over, I now develop an objection to scientific platonism in sections 3-5 based on the idea that scientific grounds are indifferent between platonism and interpretations equivalent to it (in a sense to be clarified). Sections 6-8 buttress that objection by replying to some counterarguments.

3. The pragmatic and indifference objections

A two-step line of thought leads to scientific platonism. The first step is that to accept a mathematical truth such as ‘2 is prime’ looks like it commits one to the existence of a mathematical entity (the number 2) and to a mathematical property (being prime). This literal reading of mathematics—call it *realism*—is the scientifically assumed one, and is therefore scientifically warranted. (NB realism as here understood makes no claims about the nature of these entities.) The second step takes us from realism to platonism, the claim that the objects of which mathematics speaks when correctly construed at literal face value are abstract. This second step is supposed to follow from the fact that scientists (along with everyone else) implicitly understand that mathematical entities are not concrete. Scientific standards may not condone any definite positive conception of the entities posited by scientifically applied mathematics. But they support at least a negative characterisation: these entities, whatever they are, are not concrete.

The assumption of scientific endorsement contained in this line of reasoning has recently been questioned. Penelope Maddy has argued that close attention to scientific practice suggests that 'the success of a theory involving certain mathematical existence assumptions and corresponding physical structural assumptions is not regarded as confirming evidence for those assumptions.' (1997, p. 156). For instance, she maintains that even though scientists standardly employ the hypothesis that spacetime is continuous, they do not think there is compelling evidence for it. She argues more generally that 'in some cases, a central hypothesis of an empirically successful theory will continue to be viewed as a 'useful' fiction until it has passed a further, more focused, and more demanding test' (1997, p. 142). Mathematical existence claims, according to her, often fail or are not subjected to this latter kind of test. Hence her conclusion, that scientific grounds strictly speaking do not endorse platonist (or even realist) mathematics.[13]

The claim that scientific grounds do not endorse the truth of platonistically interpreted mathematical statements may be broken down into disjunctive components. One is that scientific grounds do not endorse the truth of mathematical statements. The other is that the statements whose truth is thereby endorsed are not platonist. The rest of this section expands on the difference between these two claims and each of the objections associated with them. (We return to the first paragraph's reasoning in section 8.)

According to the first objection, which I call the *pragmatic* objection, scientists (*qua* scientists) are not epistemically committed to the mathematics they deploy. Science does not endorse mathematics in an epistemic sense, but at best in a pragmatic sense. Modern science stripped of mathematics would of course be highly impoverished, indeed more or less unrecognisable, but according to the pragmatic objection that does not mean that the mathematics that is part of science is thereby confirmed. In particular, confirmatory holism is not true, since the mathematical portion of a well-confirmed scientific theory is not necessarily confirmed. Indeed, according to the pragmatic

[13] For more detail on Maddy's views, see Maddy (1992), (1995) and (1997, ch. II.6).

objection, it is *not* confirmed. If sound, this objection would be sufficient to defeat the scientific platonist, since the latter aims to establish belief in, rather than pragmatic acceptance of, platonist mathematics.

By contrast, the *indifference* objection maintains that scientific standards endorse mathematics in the proper epistemic sense but that they do not endorse *platonist* mathematics. Let S^P be science together with platonically-interpreted mathematics and let S^{NP} be science together with some non-platonist interpretation of mathematics that makes the same claims about the physical world as S^P, assuming for now some such S^{NP} exists. The indifference objection to the argument's second premise is that scientific grounds do not endorse S^P (or S^R) over any such S^{NP}.[14]

The difference between the two objections is subtle but important, so it is worth highlighting. The pragmatic objection argues that scientific standards do not endorse the mathematics applied in science in the proper epistemic sense—the endorsement is, at best, merely pragmatic. The indifference objection concedes that scientific standards endorse the mathematics applied in science in the proper epistemic sense, but it urges that the mathematics thereby endorsed is not platonist (nor realist). More strongly, it contends that there are no scientific grounds for distinguishing between the apparent scientific equivalents S^P and S^{NP}. According to the indifference objection, science's epistemic endorsement is of the truth of mathematics under some acceptable interpretation, but not any specific one.

Unscrambling the two objections is important for the sake of clarity. But it is particularly important in light of the fact that the recent debate has focused on the pragmatic objection and ignored the indifference objection. Maddy's arguments are most naturally interpreted as versions of the pragmatic objection. The claim, for instance, that

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[14] S^R is (natural) science together with realistically-interpreted mathematics. The notation suggests that there is only one platonist interpretation, but of course there are many, as briefly mentioned at the end of this section. Note that I am implicitly restricting attention to non-platonist interpretations that make the same claims about the physical world as the platonist interpretation: a non-platonist interpretation that, say, identifies numbers with physical objects is therefore excluded. Note further that scientific standards rule out certain unacceptable interpretations such as 'mathematics is all false but if the platonist were right then it would be the case that_'. S^{NP} is therefore restricted to acceptable interpretations throughout.

the use of mathematics in science is analogous to the use of not-believed-to-be-true (or even known-to-be-false) idealisations and assumptions in science implies that scientific standards do not endorse the truth of mathematics. Another noted critic of scientific platonism, Elliott Sober (1993a), has also argued that mathematics does not receive empirical confirmation, his main objection essentially being that mathematics is apparently never disconfirmed by empirical *failure* (in a case of failure it is the science, not the mathematics, that gets blamed) and hence that mathematics should not be seen as confirmed by empirical *success* either. As he is also sceptical about non-empirical sources of confirmation (1994a, 1994c), Sober is therefore a proponent of the pragmatic objection. Despite its popularity in some quarters, however, this objection remains controversial. Developing the indifference objection therefore provides anti-platonists with a second, perhaps stronger, line of attack against scientific platonism, which can succeed even if the first fails. Anti-platonists may therefore base their case disjunctively on both objections and avoid putting all their eggs in one basket.

The indifference objection is to be distinguished from the claim that science is indifferent between standard mathematics and the segment thereof that finds scientific application. Even a relatively weak mathematical theory (much weaker than, say, standard set theory, ZFC) will probably do, at least in principle, for all scientific purposes.[15] The claim that scientific grounds only endorse some of current mathematics (e.g. anything below ‘higher’ set theory, on some way of demarcating it), however, is very different from that mooted here. The indifference objection is not committed to making a controversial distinction *within* mathematics between its scientifically and non-scientifically confirmed parts, the latter being regarded as mathematical recreation. It is compatible with the objection that science endorses mathematics wholesale, even if it does not endorse any of its particular interpretation. The indifference objection does not in itself draw a line across mathematics separating the part that is scientifically confirmed from the rest.

The indifference objection is also distinct from the claim that scientific standards endorse platonist mathematics but not any of its specific versions (e.g. set-theoretic, category-theoretic, property-theoretic, etc., platonism). This is thought by some to be a

[15] See for instance Feferman (1998, part V).

limitation of the scientific argument for platonism (Wagner 1996, Baker 2003). Steven Wagner expresses the charge succinctly:

> [A] limitation of the argument from science is that it leaves the abstract ontology indeterminate. Any abstract ontology that works will admit countless alternatives. Our numbers can be properties, properties sets, and sets numbers; our pure sets can be impure; ordered objects can be construed as unordered ones or vice versa; and so on. Science seems to have the curious feature of requiring a substantial abstract ontology but none in particular. (Wagner 1996, p. 80)

The complaint that scientific standards do not endorse a specific abstract ontology for mathematics clearly arises at a later stage than the indifference objection. One has to first accept that scientific standards endorse platonist mathematics even to consider whether they endorse a specific kind of platonism. The indifference objection urges that we cannot even go that far.

4. Weak and Strong Scientific Platonism

What I have called the indifference objection is really composed of two theses corresponding to its first and second words. The 'indifference' part is that scientific standards do not endorse S^P over (any acceptable) S^{NP} and vice-versa. The 'objection' part is that some such S^{NP} exists that is not committed to abstract objects.

Non-platonist interpretations abound, two recent examples being Geoffrey Hellman's modal-structuralism and David Lewis' structuralism. Modal-structuralism is an interpretation of mathematics mathematically equivalent to platonism.[16] Roughly, a claim *p* is interpreted as the claim that its structuralist analogue p^S necessarily holds in any structure that instantiates the axioms of the branch of mathematics in which *p* features.[17] Thus the interpretation of *p* is the necessitation of the universal Ramsification of *p* conditional on the axioms of the relevant branch of mathematics. A simpler version of structuralism, offered by David Lewis (1993, 1991), does away with the modal

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[16] See Hellman (1989) for the book-length treatment. A more recent exposition may be found in Hellman (2005, pp. 551-560).

[17] A background assumption must also be added that such a structure is possible. The case of set theory (in particular, unbounded set-theoretic sentences) requires special handling (Hellman 1989, pp. 73-79).

operators and posits the existence of enough entities to provide a model for standard set theory (and thus for all branches of mathematics). S^{NP} is more generally science together with some such non-platonist interpretation of mathematics.

To give readers a little more to sink their teeth into, let me expand briefly on Hellman's modal-structuralism as applied to arithmetic. Very roughly, Hellman interprets an arithmetical claim *p* as the claim that its structuralist analogue p^S necessarily holds in any structure that instantiates the axioms of Peano arithmetic. A semi-formal statement of the modal-structuralist interpretation of, say, '$0 \neq 1$', would be: 'Necessarily, for any collection of elements *X*, for any function S_X on *X* and element 0_X of *X*, if the Peano axioms hold of $(X, S_X, 0_X)$ then $0_X \neq S_X(0_X)$'. Hellman (1989, pp. 47-52) then argues that the logical apparatus required for his theory (e.g. second-order quantifiers) does not commit him to abstract objects. This is of great importance to him, since he would like 'to leave open the possibility of a "nominalist" reading of the mathematical theories in question' (1989, p. 20 fn. 11; see also pp. 47-52, pp. 105-117). It is equally important that a translation scheme (whose outline is straightforward but whose details need not detain us) should exist between realistically construed arithmetic and his modal-structuralist interpretation. As he explains,

> Recovery of proofs is, however, only the first step in justifying the translation scheme. As already emphasized, the modal-structuralist aims at much more: in some suitable sense, the translates must be mathematically equivalent to their originals. (1989 p. 26)

Most of the lengthy first chapter of Hellman (1989) is taken up with explaining the sense in which his proffered translation scheme provides a mathematical equivalence. The equivalence is of course not a logical one; otherwise Hellman would have to concede that a realist statement p^R is true iff its modal-structural counterpart p^{MS} is true, yet the whole point of his approach is that non-realist statements might be true even if realist ones are not. So the rough idea is that p^R and p^{MS}, although not logical equivalents, and not even truth-conditional equivalents (since one could be true and the other false), are associated by paraphrase and share the same inferential properties within their associated networks.[18]

I should emphasise that our sense of 'interpetation' is different from the standard model-theoretic one in which an interpretation is specified by fixing a domain of entities and a compositional function from a formal (uninterpreted) language to elements of the domain and set-theoretic constructions thereof. Hellman's modal-structuralism, Lewis' nonmodal-structuralism, realism and platonism all count as interpretations of mathematics in our sense but not the model-theoretic one. What we might call the Reinterpretation Function takes sentences of the body of accepted mathematics interpreted in one way (e.g. realism) to sentences interpreted in another (e.g. modal-structuralism). This function satisfies a compositionality constraint, is recursive on the language (of each branch of mathematics) and of course respects intended truth-values. Though it would be worth detailing the Reinterpretation Function function's further properties (in particular, its exact domain and range), this programmatic paper is not the place for it.[19]

As I am not sympathetic to formalism, I take mathematics as actually written or spoken to be a collection of meaningful sentences rather than uninterpreted ones. My use of locutions such as 'platonistically interpreted mathematics' is therefore not meant to suggest that mathematics has to be interpreted to be meaningful; rather, the phrase designates the collection of mathematical sentences understood as the platonist understands them. The term 'interpretation', I believe, nicely focuses what is at stake between, say, platonist and structuralist accounts of standard mathematics; but it should not mislead us into thinking that standard mathematics is uninterpreted. In particular, the natural (though perhaps naive) view that standardly understood mathematics is realist is compatible with everything in this paper (see section 8).

Earlier, we provisionally defined scientific platonism as the claim that scientific standards endorse the platonist construal of mathematics. Taking our cue from the two

[18] Some writers, such as Putnam (1967), see realist and structuralist interpretations as truth-conditionally equivalent, but this is the exception rather than the rule.
[19] Note that, as pointed out in footnote 14, not every interpretation that meets these minimal conditions is acceptable by scientific standards.

parts of the indifference objection, we now may refine the label for this thesis as *strong scientific platonism*. According to it, scientific grounds endorse the platonist interpretation of mathematical statements such as, say, '$\exists x \exists y(x \neq y \wedge x \in y)$'—they endorse the claim that there are two platonic objects, the first of which has the platonic property of being a member of the second. According to *weak scientific platonism*, by contrast, scientific standards endorse the claim that mathematics is committed to the existence of some platonic entities. Obviously a commitment to a platonist reading of '$\exists x \exists y(x \neq y \wedge x \in y)$' would amount to a commitment to a platonic entity. But another way such a commitment might be incurred would be if, for example, scientific standards endorsed the structuralist interpretation of set theory *and* the claim that there are insufficiently many concrete entities for the structuralist reading to deliver the right truth-values of set-theoretic statements. Strong scientific platonistm therefore implies weak scientific platonism but not the other way round. As a matter of fact, most past scientific platonists have been strong scientific platonists.

Now the indifference objection, if sound, immediately defeats strong scientific platonism. Whether or not it defeats weak scientific platonism depends on whether (it can be scientifically shown that) there is at least one acceptable S^{NP} not committed to abstract objects. The indifference objection to weak scientific platonism is therefore based on the important assumption that there is at least one such S^{NP}.

To appreciate the assumption, consider whether any form of structuralism must ultimately be committed to abstract objects (perhaps not distinctively mathematical ones). Modal structuralism's ultimate ontological commitment is currently moot, the case depending on the acceptability of its primitive modal ideology.[20] As for non-modal structuralism, a continuous spacetime and its regions offer a model of second-order real analysis.[21] Second-order analysis, however, is apparently sufficient for all current scientific applications of mathematics; and spacetime regions are arguably concrete.[22]

[20] For some points on both sides of the debate, see Hellman (1989) and Shapiro (1993).
[21] Spacetime points constituting the domain of first-order quantifiers, and regions (construed as mereological sums) constituting the domain of the second-order quantifiers. Some fiddling is required to account for the empty set.
[22] They arguably have spatiotemporal locations, although it might be unnatural to say they are *in* spacetime.

Hellman (1999) argues that (full classical) fourth-order number theory (or third-order real analysis) can be captured within his modal-structuralist framework (supplemented with mereology and plural quantification) without any overall commitment to abstract entities. When it comes to set theory and other branches of mathematics that go beyond this framework (e.g. category theory, some parts of functional analysis, algebraic topology, etc.), an ontology of Lewisian concrete possibilia would suffice for a model of its structuralist version. To determine whether an appropriate infinity of concrete possibilia exists would evidently lead into deep metaphysical waters. But to acknowledge as much is simply to acknowledge the current status of these claims. Like most philosophers, I am not particularly sympathetic to Lewisian realism about possibilia. Nevertheless, I recognise that it is hardly straightforward to show that a structuralist construal of mathematics is committed to abstract objects, if this involves refuting Lewisian realism in the bargain. Indicting structuralism of commitment to abstract objects is not that easy. And indicting structuralist second-order analysis of ultimate commitment to abstract objects is even harder. Moreover, it is dubious whether these charges could be upheld on the basis of a scientific argument. Finally, the claim that no metaphysics of concreta can supply a structuralist set theory's ontology is overweeningly general. Establishing it would require some capacity to enumerate or capture the relevant general features of any such metaphysics; but of course it is doubtful that we can currently do so. So it is fair to say that the jury is still out. Since I do not want to tackle the question further here, I shall continue to proceed conditionally, having indicated that it is not obvious that such an S^{NP} does not exist.

To summarise: the indifference objection, if sound, sinks strong scientific platonism. Whether it also sinks weak scientific platonism depends on whether *every* (acceptable) S^{NP} scientifically equivalent to S^{P} is ultimately committed to abstract objects. On the one hand, there is no acceptable S^{NP} universally agreed to be free of commitment to abstract objects (some think there is, others not). On the other hand, no half-decent argument is currently available for the sweeping conclusion that there could be no such S^{NP}. A cautious proponent of the indifference objection should currently withhold judgment on whether it defeats weak as well as strong scientific platonism.

5. For the indifference objection

One motivation for the indifference objection is that scientists (at least *qua* scientists), if asked to choose between mathematically equivalent interpretations such as platonism and modal-structuralism with which to do science, would see the choice between them as scientifically inconsequential. Underlying this attitude is scientists' understanding of mathematics as an auxiliary to scientific endeavour rather than its subject matter. Scientists' judgment that S^P (or S^R) and S^{NP} are scientifically equivalent (i.e. that scientific grounds do not favour one over the other) constitutes strong evidence on behalf of the indifference thesis. Scientists take the truth, though not the metaphysics, of mathematics to be a scientific concern.[23]

Proponents of the indifference objection may also deploy standard scientific realist arguments, taking in the mathematical parts of science, to support the claim that the best explanation of scientific success invokes the truth of mathematics. We shall not enter this debate here, since we are assuming for present purposes that the pragmatic objection fails. But note that the indifference objection is considerably stronger than well-known objections to scientific realism that proceed by claiming that scientific grounds do not distinguish between observationally equivalent but spatiotemporally inequivalent theories, e.g. between some standard theory T and the sceptical hypothesis that our observations are *as if* T were true though T is in fact false. Since S^P and S^{NP} differ only in their interpretation of mathematics, they are spatiotemporally equivalent: they make the same claims about the spatiotemporal world. Now it is one thing to say that the realm of the scientific goes beyond observational adequacy; it is quite another to say that it goes beyond spatiotemporal adequacy.

The indifference objection also captures some of the pragmatic objection's attraction by allowing that scientific grounds recommend S^P over many, perhaps all, instances of S^{NP} in a pragmatic sense. The platonist (or realist) interpretation, it might be agreed, is the more convenient one, practically speaking. This is consistent with the thought that scientific grounds do not recommend S^P over any S^{NP} in an epistemic sense, which is what the scientific platonist requires. The indifference objection can

[23] Notice that this point in favour of the indifference objection is also a point against the pragmatic objection.

acknowledge this important pragmatic point while simultaneously respecting the thought that scientific standards (epistemically) endorse mathematics.

Finally, the scientific platonist arguments in the literature appear to be mostly targeted at the pragmatic objection rather than the indifference objection. A representative example is Mark Colyvan's claim that 'there is good reason to believe that the mathematised version of a theory is "more virtuous" than the unmathematised theory, and so there is good reason to believe mathematics is indispensable to our best physical theories' (2001, p. 80). Colyvan's argument for this in his book on indispensability arguments, proceeds by 'appealing to a number of examples in which mathematics contributes to the unification and boldness of the physical theory in question, and therefore *is* supported by well-recognised principles of scientific theory choice' (2001, p. 81). For example, he cites the important role that complex analysis plays in differential equations, and the importance of the Dirac equation and Lorentz transformation in modern physics. The thing to notice, however, is that Colyvan's counterarguments apply only to the pragmatic objection.[24] If sound, they show that the truth of mathematics receives scientific backing; but they fall short of showing that platonist mathematics does. This is true more generally of most defences of scientific platonism—including standard 'indispensability' ones—with a few exceptions to be considered below.[25]

The charge that scientific grounds do not endorse platonist mathematics is therefore a conflation of two different objections, a fissile compound whose constituents are best treated individually. One is the pragmatic objection, which states that scientific grounds do not *endorse* platonist mathematics, because they do not endorse mathematics. The other is the indifference objection, which states that scientific grounds do not endorse *platonist* mathematics, even if they do endorse mathematics. Although the former has attracted attention, the latter, arguably the stronger of the two, seems to have gone unnoticed. To demonstrate that it cannot be straightforwardly dismissed, I tackle three objections to it in the rest of the paper. Section 6 discusses the idea that there is an easy

[24] Colyvan (2001) contains extended responses to Maddy's and Sober's respective versions of the pragmatic objection.

[25] Colyvan's assertion that general scientific principles favour platonism over non-platonism (2001, pp. 128-9) does speak to the indifference objection and is considered in section 7.

operational test that will confirm that scientific standards endorse platonism, namely: Let the scientists decide in the usual way! Section 7 considers the idea that general principles of scientific method favour S^P (or S^R) over any S^{NP}. Section 8 examines the argument that realism is the standard interpretation of what scientists mean by their mathematical utterances and that it is therefore condoned by scientific standards, since these standards condone mathematics' standard interpretation.

6. The publication test

Is there an operational test to determine what scientific standards endorse? One might argue for example that whether scientific grounds vindicate nominalism can simply be determined by submitting a nominalist construal of a scientific theory (e.g. Hartry Field's nominalisation of Newtonian mechanics) to a scientific journal. If the journal publishes it and it is well-received by the scholarly community, the answer is yes, and otherwise no. This startlingly simple proposal has in fact been advanced by John Burgess and Gideon Rosen:

> …ultimately the judgment on the scientific merits of a theory must be made by the scientific community: the *true test* would be to send in the nominalistic reconstruction to a mathematics or physics journal, and see whether it is published, and if so how it is received. (1997, p. 206; my emphasis)

These claims are repeated elsewhere in their book. They maintain that the scientific acceptability of various construals of mathematics and reformulations of science can be gauged by their success in passing this test, and that the test will vindicate platonism.

Despite what they say, however, the test is not a 'true' one. The publication test won't do as some kind of operational equivalent of scientific platonism (or mathematical-cum-scientific platonism).[26] The reason is that the question of an adequate construal of

[26] Burgess and Rosen's discussion of the publication-and-reception test once again illustrates the difficulty of determining whether in their (1997) they uphold a purely scientific form of platonism, or mathematical-cum-scientific platonism, or both. If their platonism were mathematical-cum-scientific rather than just scientific, they would have explicitly cited not just *Physical Review* but also some mathematical journals as potential venues for the nominalist reconstrual. And they would not without expansion or qualification have written that 'The [nominalist] innovation, we suspect, simply would

mathematics is not considered by science journals. The one science journal Burgess and Rosen do cite (1997, p. 210), *Physical Review*, is edited and read by physicists. It is a reasonable bet that few, if any, of these physicists have seriously contemplated any of the issues that preoccupy nominalists. And even if some of the editors have, they would not expect their readers to have done so. And even if, contrary to fact, many or most—even if you like, all—the editors, referees, and readers of *Physical Review* have pondered these issues, they might still collectively judge it to be an inappropriate forum for such discussion. So the editorial board is likely to reject offhand any nominalistic submission. But that does not in itself show that the nominalists haven't been addressing a scientific question, albeit a high-level one. Nor does it show that they have erred in the application of scientific standards. What is correct by scientific standards is not co-extensive with what is currently publishable in scientific journals.[27] This is not to say that scientific standards vindicate nominalism, but rather that the presumed unwillingness of *Physical Review* to publish a particular construal of mathematics does not establish its scientific untenability. Failure of the publication test (or a fortiori of the publication-and-reception test) is no touchstone of scientific inferiority.

Another reply would be that the question of nominalism versus platonism is not even a scientific question. If so, rejection of a nominalist submission would be a consequence of the unscientific nature of the question, and not, as Burgess and Rosen assume, of the answer's scientific inferiority. Failure of the publication (or publication-and-reception) test can thus in principle be attributed to two factors other than scientific inferiority. One is that scientists take their trade journals to be inappropriate fora for such discussions, even if they ultimately deem the question scientific. Another is that the

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not be recognized as progress by practising scientists. And this is so not just for physics, we suspect, but for every natural or social science.' (1997, p. 210). And yet the passage quoted above is more suggestive of mathematical-cum-scientific platonism than scientific platonism, since they allow for mathematical as well as scientific journals. So perhaps the most reasonable interpretation is to take them as espousing both. Although I discuss the publication-and-reception test only in relation to scientific platonism, the same points carry over to mathematical-cum-scientific platonism.

[27] We are only considering original, interesting, etc., research—otherwise the publication fails for more banal reasons. And we are of course also assuming, as Burgess and Rosen intended, that the test concerns actual rather than ideal journals.

question addressed is not scientific, and that scientists recognise this fact. Failing the test is compatible with both alternative explanations.

Evidence for which of the three explanations—the inappropriateness of the forum, the unscientific nature of the question, or the construal's scientific inferiority—is correct could be gleaned from the comments offered by the journals' editorial boards and referees in rejecting a submission. But this evidence would by its nature be limited, since it would consist solely of the judgements of scientists acting as journal editors and referees. A better and more comprehensive investigation would not restrict itself to scientists' judgments when donning a particular professional hat. The publication test, in sum, does not vindicate scientific platonism.[28]

7. General principles of scientific method

Scientific standards, according to the indifference objection, are indifferent between platonism and a non-platonist interpretation of mathematics equivalent to it. The platonist could respond that, despite their mathematical equivalence, standard scientific principles recommend the former. Take Hellman's modal-structuralism as an example of the latter. Modal-structuralism is a sophisticated, one might perhaps say abstruse, interpretation of mathematics. Platonism is simpler and more familiar. Given that simplicity and familiarity are scientific principles, surely scientific standards recommend platonism over modal-structuralism? A second response to the indifference objection is thus that general scientific principles recommend platonism over any non-platonist interpretation of mathematics.[29]

Let us agree, as noted earlier, that some version of simplicity is a scientific virtue. It would be naïve, however, to think that this means that scientific standards recommend any theory T_1 over any theory T_2 less simple than it. Suppose in particular that T_1 and T_2 are spatiotemporally equivalent. For example, take T_1 be the theory that there exists an

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[28] A slightly more promising version of the publication (or publication-and-reception) test would invoke general scientific journals such as *Nature* or *Science* rather than *Physical Review*, since the former represent what scientists regard as important contributions to science in general rather than physics in particular. The same moral applies to these too, however.

[29] See e.g. Colyvan (2001, pp. 128-9).

omni-benevolent deity who does not act on the world, and T_2 the theory that a thousand such impersonal deities exist, all with different, highly complex moral characters. Both theories are consistent and by hypothesis have the same spatiotemporal import, namely: none. Clearly, however, T_1 is simpler than T_2. Does that mean that scientific standards recommend T_1 over T_2? It seems not—the apparent verdict is rather that science does not speak to the issue of which of T_1 and T_2 is true. Another example is the venerable one of theories with different units of measurement. A scientific theory based on the metric system is simpler than one based on British imperial units (feet, inches, etc.); a theory based on the Kelvin scale is similarly simpler than one based on the Celsius scale; and so on. But superiority of this kind evidently does not epistemically privilege a theory over its more cumbersome counterpart.

These and countless other examples illustrate the point that most general form of the principle of simplicity oustrips its scientifically accepted form. The precept 'Prefer *any* simpler theory to any more complex theory (in this respect)' is not a principle of scientific theory choice. As the stress on 'any' shows, this formulation is too general: the real scientific principle has a more restricted range of application. The scientific platonist must therefore show that the appropriate restriction does not disable the principle from adjudicating between S^P and any acceptable S^{NP}.[30]

So far our defence rests on the implausibility of thinking that a scientific principle such as simplicity holds with unrestricted generality, and the lesser, but still considerable, implausibility of thinking that a properly restricted version of the principle privileges S^P over all (acceptable) S^{NP}. The specialist literature on the subject also backs these verdicts. In a series of articles,[31] Elliott Sober has examined whether scientific uses of the principle of simplicity underpin its philosophical uses. More precisely, Sober has investigated the following question: Does the rationale for scientific uses of simplicity to adjudicate between predictively non-equivalent theories carry over to the case of predictively

[30] One might respond that the theories in my examples do not genuinely differ in their simplicity, and that *genuinely* simpler theories would be scientifically preferable to more complex ones. This response only differs terminologically from the position I am considering here, since the challenge would be to demonstrate that a criterion of *genuine* scientific simplicity privileges S^P over any acceptable S^{NP}.
[31] In particular Sober (1996) and Forster & Sober (1994), as well as Sober (1990a, 1990b).

equivalent theories? Sober's considered answer is no. For example, he argues that a leading model of how to measure the simplicity of solutions to the curve fitting problem (which curve should be drawn through some data points) does not apply to the decision between predictively equivalent philosophical theories (Sober 1996). On this model, due to the statistician Hirotugu Akaike, differences in the predictive accuracy of two families of curves arise from differences in how well they fit the data points together with a difference in the number of adjustable parameters they contain. For example, given some collinear data points, the predictive accuracy of the family of straight lines (parametrised by $y = ax+b$) is greater than that of the family of parabolic curves (parametrised by $y = ax^2+bx+c$) because the goodness of fit of the closest-fitting member of each family is the same—namely, perfect—but the first family contains one fewer adjustable parameter than the second (*a*, *b* as opposed to *a*, *b*, *c*). As with some of the other examples Sober mentions, however, there is no difference in adjustable parameters between the spatiotemporally equivalent S^P and S^{NP}. Thus the simplicity criterion cannot take root here. Sober's measured conclusion about theories that are predictively, not even spatiotemporally, equivalent, expresses the general point nicely:

> …this treatment of the role of simplicity considerations in the curve-fitting problem provides no rationale whatever for choosing between theories that are predictively equivalent. This doesn't decisively *prove* that simplicity differences count for nothing in the case of predictively equivalent theories. However, it does lend support to that epistemological conclusion. To use the principle of simplicity in one context because it makes good sense in the other is to commit an epistemological equivocation. (p. 170)

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Other writers have stressed that what grounds the use of simplicity in its scientific applications are particular features of the situation in question: remove this grounding and you thereby remove the scientific rationale for deploying the principle. Wesley Salmon for instance writes:

> The most reasonable way to look at simplicity, I think, is to regard it as a highly relevant characteristic, but one whose applicability varies from one scientific context to another. Specialists in any given branch of science make judgments about the degree of simplicity or complexity that is appropriate to the context at hand, and they do so on the basis of extensive experience in that particular area of scientific investigation. Since there is no precise measure of simplicity as applied

> to scientific hypotheses and theories, scientists must use their judgement concerning the degree of simplicity that is desirable in the given context. The kind of judgement to which I refer is not spooky; it is the kind of judgement that arises on the basis of training and experience. This experience is far too rich to be the sort of thing that can be spelled out explicitly. (1990, p. 279)

This passage makes it clear just how context-sensitive a truly scientific principle of simplicity must be. It also indirectly supports the contention that any such application of simplicity to the case of S^P versus S^{NP} would be ungrounded, an inappropriate extrapolation beyond the contexts in which the principle finds its home.

What one sees in the specialist literature more generally is a suspicion of the idea that there is a universal, context-independent, scientific principle of simplicity. There is correspondingly little support for the idea that such a principle could provide scientific grounds for adjudicating between two spatiotemporally equivalent theories. In sum, methodological studies of the use of simplicity in science bid fair to rule out the use of simplicity as a tie-breaker in the contest between S^P versus S^{NP}. At the very least, uses of simplicity in standard scientific contexts are so different from its uses as a tiebreaker in philosophical controversies that the platonist's contentious extrapolation requires far more extensive support than it has hitherto received. As we might put it: it is simplistic to identify scientific simplicity with simplicity *simpliciter*.

The moral applies to all scientific principles, not just simplicity. For example, it is often argued that even if a theory T_1 is in some sense (logically, spatiotemporally, etc.) equivalent to another theory T_2, T_1 may nevertheless be preferable to T_2 on account of its greater scientific fertility. A theory's fertility consists in how many new theories, new extensions of old theories, or new connections, results, methods, etc., it leads to. In short, T_1 may be 'statically' equivalent to T_2 but 'dynamically' superior to it, hence scientifically preferable. So if platonism were scientifically more fertile than anti-platonism, that would be a reason for preferring it.[32]

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[32] This argument has been put forward by several philosophers, perhaps most fully in Baker (2001). See also Colyvan (2001, ch. 4), Steiner (2005, pp. 644-5), and Quine's discussion of the importance of mathematical theories as 'engines of discovery' (1960, p. 270).

An immediate response to this argument is that there is no difference in fertility between a platonist scientific theory T^P and its (acceptable) non-platonist counterparts T^{NP}. On a typical structuralist view, for example, actual mathematical usage can happily proceed with dummy variables, rendering it notationally indistinguishable from mathematical practice based on a platonist interpretation. If one has the structuralist interpretation in mind, apparent singular terms are understood as placeholders for positions in structures; if one has the platonist interpretation in mind, they are understood as bona fide singular terms. Everything else—every definition, manipulation, proof, etc.—goes through just the same. In the heat of mathematical practice, when background interpretation effectively drops out, there is no difference between the two. Alan Baker, who is a proponent of the argument, suggests that 'the application of group theory to particle physics…allowed the prediction of whole families of hitherto unobserved subatomic particles' (2001, p. 92). But a structuralist interpretation of mathematics, with the everyday syntactic notation unchanged, is of course just as capable of allowing this prediction as a platonist one. Likewise for the development of quaternions, and any other example one might care to mention. Differences in background mathematical interpretation do not seem to affect a theory's scientific fertility.

Another response parallels the response in the case of simplicity and is less dependent on the contingencies of humans' adeptness with various mathematically equivalent theories. Even if T^P were easier to handle and therefore ultimately more generative of scientific developments than T^{NP}, the two theories would not necessarily differ in their scientific fertility, properly so-called. Taking modal-structuralism as an example once more, we may confidently predict that all the mathematics ever needed by science is contained within ZFC, which modal-structuralism can accommodate (as we are assuming). Any proofs, results, techniques, etc., to be found in platonist mathematics that could conceivably find scientific application have analogues in modal-structural mathematics. And the spatiotemporal equivalence of platonism and modal-structuralism is true not just for the static snapshot of current scientific practice, but for any conceivable development thereof. Perhaps platonist mathematics is slightly easier for the human mind to grasp, and therefore takes up fewer intellectual resources, freeing scientists to spend those extra resources on scientific inquiry. But that is not in itself a scientifically relevant difference in fertility. The case of theories with different units of measurement again serves as an example here.

In short, mere psychological suggestiveness does not a scientifically fertile theory make; at least not in the sense relevant to scientific confirmation or endorsement. The boundary between the context of justification and the context of discovery may be more blurred than has often been assumed, but a pertinent distinction of this kind must obtain, on pain of making even the most arbitrary features of scientific theories and the most idiosyncratic aptitudes of individuals who deploy them relevant to scientific confirmation. Anything can suggest anything to anyone. What counts is what avenues the theory opens up in some appropriately logical sense, not the long-term production rate of the scientific community that adopts it. By assumption, every scientific application or implication of platonist theory T^P is shared by any of its acceptable non-platonist equivalents T^{NP}. Of course, one difference between the imperial-metric units case and that of a platonist versus a non-platonist version of the same scientific theory is that the latter contrast is between two theories with different contents, whereas by assumption the imperial-unit and metric-unit versions of a scientific theory share the same content. But my point is precisely that this difference must be shown to be of scientific relevance. So the question is whether the potential advantage in simplicity possessed by platonism, as a result of which it may—possibly— in the long run give rise to more developments than its non-platonist counterpart is relevant to its scientific superiority and confirmation.

These difficulties for scientific platonism have been obscured, I believe, by an over-emphasis on scientific principles at the expense of their context of application. Wrenching principles such as simplicity or fertility out of their proper domain is a snare against which every philosopher must guard. It is not enough to show that the application of general scientific principles to the choice of S^P versus some S^{NP} settles the issue one way or the other; one must also show that the question is scientific. The opposed view is that the issue is beyond the scientific pale, that to apply these principles to this particular question is to transcend the scientific realm. The debate cannot be decided by table-thumping declarations from either side that the question is or is not scientific, but must be settled by examining the exact boundary science itself posits between questions within the scientific realm and those outside it. As explained, the prospects for platonism on this point are not promising.

One reaction by the scientific platonist might be to try to reformulate her claim in terms of generalised scientific principles rather than scientific grounds. The revised claim would be that generalised scientific principles (be they applied to a scientific question or not) endorse platonist mathematics. A generalised scientific principle is one with the restrictions demanded by proper scientific method removed, as in the most general version of simplicity (roughly, the non-scientific 'prefer the simpler of *any* two theories' as opposed to the scientific 'for any two theories of such-and-such kind prefer the simpler of the two'). This revised claim, however, would be unfaithful to the inspiration behind the general scientific platonist position, which is to take scientific grounds—and not general scientific principles extrapolated beyond the domain of science—as authoritative. What makes the scientific platonist thesis interesting is the importance attached to *scientific* standards rather than ultra-scientific generalisations of these standards. Given any metaphysics of mathematics, it is a trivial exercise to concoct principles that support it over its rivals; the question is why anybody should care what those particular principles support. Anything gained by this reformulation seems divested of the force it was intended to have. It is, in effect, to give up on scientific platonism.

The scientific platonist might respond in a different way, by expanding the standard conception of the sciences to include the non-natural sciences. For example, one might classify semantics as a science and advocate semantic uniformity as a reason for preferring platonism to non-platonism. After all, a realist semantics for mathematics is in line with the proper semantics for (most of) the rest of language, as famously stressed in Benacerraf (1973). Mathematical and non-mathematical language *appear* very similar syntactically and semantically (including inferentially): predications, connectives, quantifiers, etc., all seem to function in the same way. The presumption of uniformity applies with particular force to so-called mixed statements containing a mixture of mathematical and empirical language, which are usually read in a semantically seamless way—a stock example is, 'If the first ball bearing has mass m_1 and the second ball bearing has mass m_2 and the gravitational force between the two masses when they are at distance d is Gm_1m_2/d^2'. So perhaps it will be said that our default semantics should take this apparent similarity to result from a real similarity, and hence that scientific standards favour realism over other interpretations of mathematics.

This semantic argument is undoubtedly to be taken seriously. One missing step is of course the transition from realism to platonism. But a more pertinent problem with the argument in the present context is that to resort to it is simply to abandon scientific platonism. Anyone who concedes that natural-scientific grounds do not support platonism but who holds out hope that semantic grounds support platonism has given up on scientific platonism and adopted semantic platonism instead (as by analogy we might label the view). Lest the reader should think that I have deliberately attacked the weaker thesis and dodged the really powerful argument, however, I should briefly sketch the reasons for thinking that the argument for scientific platonism is more likely to succeed than the argument for semantic platonism.

First, semantics is a relatively young discipline with many competing research programmes. In particular, there is nothing like agreement across the board that the realist view of mathematical language is correct. Second, many philosophers would incline to the view that the correct semantic theory for mathematics must flow from a metaphysics for mathematics rather than the other way round. On this view, metaphysics is the horse and semantics the cart,[33] and tackling the semantics of mathematics without having its ontology on the table is then a methodological solecism. Of course we can agree that trying to settle the metaphysics in a state of semantic naivety is a recipe for disaster; but this does not imply that the latter should take precedence over the former. I stress that my aim is not to peddle this methodological view; the point is merely that it is a respectable

[33] As Michael Jubien who vividly formulates the view (about properties) puts it: '…it is easy to think we must look hard at natural language semantics in order to evolve a theory of properties. Indeed, it is easy to think even that property theory is nothing *but* a certain region of natural language semantics. But once we reject conceptualism in favor of genuine realism, matters seem quite different. It then looks like a theory of properties should flow directly from general *metaphysical* considerations, and that the *semantical* projects should be founded upon the results of the metaphysical one. Of course none of this is to say that our metaphysical intuitions occur in some vacuum, independently of language, or that linguistic *data* are not influential in the formation of our metaphysical views. It is only to say which theoretical enterprise is the cart and which the horse. In my view, metaphysics is the horse. Which is not to disparage the cart at all, but only to suggest that it can't go very far on its own, especially if it thinks the horse is it cargo.' (1989, p. 164).

one. Finally, and perhaps most importantly, semantic platonism's significance, philosophically speaking, is also thought to be less than that of scientific platonism, the reason being that what semantic standards support is generally thought to have less of a claim on our belief than what scientific standards support (this is in turn related to the first claim). Steven Wagner, who is more sympathetic to semantic platonism than most, once again puts the point crisply.

> The problem [with semantic platonism compared to scientific platonism] is that science has clearer credentials than formal semantics. Physics is acceptable beyond doubt. If it admits no nominalistic construal, then Platonism is true. Semantics, however, is not clearly science and not clearly anything else that compels belief in its ontology. (1996, p 77).

So I leave detailed examination of semantic platonism for another occasion, and trust that the reader will recognise that this is not tantamount to discussing *Hamlet* without mentioning the prince.[34]

Observe finally that the scientific platonist must also contend with an intermediate position, according to which it is indeterminate whether scientific standards favour S^P over (any acceptable) S^{NP}, and perhaps more generally whether they ever favour one of two spatiotemporally equivalent theories. On this view, there is no fact of the matter. Perhaps the grounds for, say, positing an entirely general principle of simplicity as governing scientific practice may be very strong. The grounds for resisting its extrapolation to the non-spatiotemporal, however, may be just as strong. In other words, the practice may be indeterminate about whether S^P is scientifically preferable to S^{NP}. The platonist must also defeat this indeterminacy view if she is to uphold her position, since it represents a determinate stand.

8 Scientific grounds and the actual content of mathematics

According to the indifference objection, scientific grounds endorse mathematics but not its platonist interpretation. For example, scientific grounds endorse the truth of '2 + 3 =

[34] For further discussion of the 'semantic indispensability argument', see Colyvan (2001, pp. 15-17) and especially Wagner (1996), where, having noted the quoted difficulty for semantic platonism, the author ends up advocating it anyway (or at least 'bets on it').

5’ under some acceptable interpretation but they do not endorse its platonic interpretation ‘the abstract numbers 2, 3 and 5 stand in the abstract relation of addition’. A statement such as ‘2 + 3 = 5’ must, however, have *some* interpretation in its standard mathematico-scientific contexts. (Formalism, which takes mathematics as actually practised to be meaningless symbol-manipulation, is mistaken.) This observation gives rise to an objection and a challenge. The objection is that the standard content of mathematical sentences is realist (or perhaps even platonist), and therefore, since scientific grounds endorse this standard content, they endorse realism (or platonism). (This is essentially the argument we met in the first paragraph of section 3.) But even if the standard content is not realist (nor platonist), a challenge remains: What exactly is it that scientific grounds endorse?

Let me explain why the objection is misconceived before ending with a few words about the challenge. The objection assumes that scientific grounds endorse the standard content of mathematics. But there is no good reason to think that scientific grounds and our best semantic theory are aligned. Indeed, if the assumption that the content of standard mathematical utterances (and inscriptions) is realist is correct, then as a matter of fact the two are *not* aligned. The claim that the realist interpretation captures the content of standard mathematics is thus compatible with the claim that scientific standards do not endorse that interpretation over, say, a structuralist one.

Why would anyone think otherwise? Return to ‘2 + 3 = 5’ to fix ideas. The argument applied to this example runs as follows: (i) our best semantic theory informs us that the content of ‘2 + 3 = 5’ in standard scientific contexts is realist; (ii) scientists (*qua* scientists) endorse the proposition expressed by standard utterances of ‘2 + 3 = 5’; (iii) scientists’ endorsement (*qua* scientists) is our best guide to what scientific standards endorse; hence, scientific standards endorse the realist interpretation of ‘2 + 3 = 5’. Note that the argument does not assume that scientists have privileged access to the content of their utterances. The argument is not deductively valid of course; but if its premises were all true it would take special pleading to attribute widespread error to the scientific community concerning what scientific standards endorse. Premise (iii) seems unimpeachable, and we are also granting (i) for the sake of argument. That leaves (ii) as the potential site of dispute.

There seems to be independent reason, however, for rejecting premise (ii). The proponent of the indifference objection will urge that scientists (*qua* scientists) endorse the claim that some (acceptable) interpretation of '2 + 3 = 5' is true but not that it is true *tout court*. Of course, scientists casually say that '2 + 3 = 5' is true, but this seems to be unreflective usage, abbreviating a more complex truth-endorsement. In more reflective moments, scientists recognise that what they endorse (*qua* scientists) is rather, as we have put it, that the sentence '2 + 3 = 5' is true on some acceptable interpretation. This point is independently verifiable—though of course scientists might not couch in exactly these terms. Casual acquiescence in sentences that a semantic theory construes realistically should not be mistaken for reflective endorsement of the statement on the interpretation that semantic theory gives it (or any other interpretation). What is certainly not casual is the truth-endorsement.

The point is perhaps best put in the following more general way. To the extent that scientists recognise that some specific '…ism' (if any) is the correct interpretation of their mathematical discourse, to that extent they do not endorse their utterances under that interpretation (*qua* scientists). Thus scientists' casual willingness to accept whatever is the content of their mathematical utterances is trumped by their disposition to recognise that scientific standards do not endorse realism any more than they endorse a non-realist interpretation equivalent to it.[35] In sum, even if realism (or platonism) turns out to be the proper interpretation to put on standard utterances, it does not follow that this is the scientifically endorsed interpretation. In fact, if successful, what the arguments in the previous sections establish is precisely that it is *not* scientifically endorsed.

A belt and braces response to the objection could go on to argue that structuralism might be a better candidate, all things considered, for the standard content of mathematical utterances, at least when it comes to the non-set-theoretic parts of mathematics contained in scientific applications (this would be to challenge premise (i)). A further question would be how the transition from realism to platonism is effected. Alas, there is no room to examine either of these further responses here, so let me say a

[35] Of course any particular scientist might have her own individual non-scientific reasons in favour of some particular interpretation.

quick word about the challenge. I shall point to the first step along a path that the anti-platonist will wish to explore further.

The proponent of the indifference objection owes us a model of how scientific standards can endorse the truth of '2 + 3 = 5' without endorsing any particular content. A simple model might be that scientific standards endorse a disjunction of acceptable interpretations of the sentence '2 + 3 = 5' without endorsing any particular one.[36] Another model, which we have been presupposing throughout, is that scientific standards endorse the proposition that the uninterpreted sentence '2 + 3 = 5' is true under some acceptable interpretation (but not any particular one). Note that this latter commitment is existential and metalinguistic. The two proposals are not contradictory, of course, and in fact form a natural package: should scientific standards endorse both the disjunction as well as the existential claim, both proposals would be correct. Observe that on the disjunctive model, the propositional operator 'scientific standards endorse__' applies to a disjunction but none of its disjuncts, and that on the existential model the same operator applies to an existentially quantified statement but none of its instantiations. This logical behaviour is familiar from the properties of other epistemic operators. Many such operators apply to disjunctions without applying to any disjunct: for example it may be known, reasonably believed, thought, etc., that one of *p* or *q* is true, without it being known, reasonably believed, though, etc., that *p* is true or that *q* is true. Similarly, such operators can apply to existentially quantified statements without applying to any of their specific instances: for example, I know that there is some truth or other that I do not know, but there is no specific truth *p* such that I know that: *p* is true but I do not know *p*; or I know that some combination opens the lock but not which one; and so on. The epistemic operator 'scientific standards endorse__' as applied to a mathematical sentence's interpretation seems to be another instance of this phenomenon.

Clearly, these models—better, templates for a model—are in need of further elaboration, which I cannot give them here. But my defence to this section's objection is independent of this elaboration, as it rests on the earlier point about the relation between scientific grounds and semantic theory. It remains for the anti-platonist to explore possible models of how scientific grounds might endorse the claim that a mathematical

[36] The disjunction consists of two or more disjuncts.

statement is true under some interpretation, or some disjunction of interpretations, etc., without endorsing any specific interpretations. If my earlier arguments are sound, some such model must be correct.

9 Conclusion

Our discussion has thrown up a challenge for scientific platonism. Even if the scientific platonist can overcome the pragmatic objection—that scientific standards do not endorse the truth of mathematics but only acceptance of it in some non-epistemic sense—she must still confront the indifference objection, that scientific standards endorse that mathematics is true under some interpretation but not under any particular one such as platonism or realism or structuralism or modal-structuralism or... The indifference objection defeats strong scientific platonism, according to which scientific standards endorse the platonist interpretation of (at least some parts) of mathematics. Whether it defeats weak scientific platonism, according to which scientific standards endorse the thesis that mathematics is committed somewhere down the line to abstract objects, depends on whether any acceptable S^{NP} must ultimately be committed to abstract objects. That question, as explained, remains open.

Sections 6-8 considered three responses to the indifference objection on behalf of the scientific platonist. The first is that letting scientific referees decide the issue, by the usual means and in the usual fora, vindicates platonism. The second is that general scientific principles recommend platonism over non-platonism. The third is that scientific practice itself sanctions whatever is the standard—namely, realist (or perhaps platonist)—interpretation of mathematical statements. I take my responses to these objections to have shown that the scientific platonist cannot gain a quick victory by any of these means. On the question of science's relevance to the metaphysics of mathematics, the typical mathematician may be right after all.[37]

[37] I have benefited from presenting this material at the universities of York, Oxford and Wisconsin-Madison as well as at the Cambridge conference. My thanks also to two anonymous referees for this volume and to the following for discussion or comments at various stages: Alex Oliver, David Liggins, Elliott Sober, Gideon Rosen, Hallvard Lillehammer, Mary Leng, Michael Potter, Paul Benacerraf, Peter Smith, Sarah Teichmann, Tim Lewens and Tim Williamson.

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