Explanation and Indispensability

Russell Marcus, Ph.D.

Department of Philosophy, Hamilton College

198 College Hill Road

Clinton NY 13323

rmarcus1@hamilton.edu

(315) 859-4056 (office)

(315) 381-3125 (home)

July 15, 2010

~12,000 words

Abstract:

A new version of the indispensability argument in the philosophy of mathematics may be found in works of Mark Colyvan and Alan Baker. The new argument relies in part on recent work in mathematical explanation, and alleges that our mathematical beliefs are justified by their indispensable appearances in scientific explanations. This paper provides an analysis of the new argument, as well as a survey of some earlier versions of the indispensability argument which share some of the new argument's features. Like some earlier versions, the explanatory indispensability argument is unfortunately elliptical about a criterion for ontological commitment, and so liable to an instrumentalist, or fictionalist, response.

There are two overlapping arguments in this paper. The first is that explanations which increase our understanding may not be those in which we express our ontological commitments. The second is that what has become known as easy-road, or weasel, nominalism is an appropriate response only to some indispensability arguments, and not to Quine's original argument, which was designed to block such weaseling. I conclude that the new explanatory indispensability argument is no improvement on the Quinean one.

§1: Explanations and theories

Q: Should the following inference convince us that there are numbers?

IM I have two mangoes.
Andrés has three different mangoes.
So, together we have five mangoes.

A: No, because simple, adjectival uses of arithmetic are easily eliminated.

IN $(\exists x)(\exists y)(Mx \bullet My \bullet Bxm \bullet Bym \bullet x \neq y)$
$(\exists x)(\exists y)(\exists z)(Mx \bullet My \bullet Mz \bullet Bxa \bullet Bya \bullet Bza \bullet x \neq y \bullet x \neq z \bullet y \neq z)$
$(x)[(Mx \bullet Bxa) \supset {\sim}Bxm]$
$\therefore (\exists x)(\exists y)(\exists z)(\exists w)(\exists v)(Mx \bullet My \bullet Mz \bullet Mw \bullet Mv \bullet x \neq y \bullet x \neq z \bullet x \neq w \bullet x \neq v \bullet y \neq z \bullet y \neq w \bullet y \neq v \bullet z \neq w \bullet z \neq v \bullet w \neq v)$[1]

The contrast between IM, the mathematical inference, and IN, the parallel nominalistic inference, demonstrates that some statements which use numbers may be taken as convenient shorthand for more complicated statements that eschew mathematical objects. There may be statements more complex than those in IM from which mathematical objects are ineliminable. Indeed, the last thirty years of the philosophy of mathematics has been centrally concerned, in one way or another, with this question. But the eliminability of numbers is uncontroversial in some cases. If someone were to present IM as a reason for believing that there were numbers, anyone familiar with first-order logic would be justified in denying an inference to the existence of mathematical objects by proffering IN as a replacement. Whether we believe in mathematical objects or not, IM is not a good reason for believing in them.

The reason that IM can not justify beliefs in mathematical objects is that it is a casual inference, which does not reflect our serious commitments. If we want to display our ontological commitments, we must speak soberly. We must invoke parsimony, and rewrite our more casual sentences. We reflectively remove from our language references to sakes and behalves and centers of mass, and to adjectival uses of

[1] A more heavy-handed option to remove numbers from IM would be to replace the terms which refer to them with references to ordered regions of substantivalist space-time; see Field 1980 and Burgess and Rosen 1997: §IIA.

natural numbers.[2] IN makes it clear that the essential subjects of IM are mangoes, not numbers.

Now, consider the question, "Why are there five mangoes here?" A good answer to that why-question, an explanation of the fact that there are five mangoes on the table, is that I brought two and Andrés brought three. That fact is explained by IM, and only awkwardly demonstrated, if explained at all, by IN. IM is not a complete, best theory of these mangoes, of course. It requires some background assumptions about object constancy, and that mangoes do not annihilate each other when, say, there are more than three together. But, it is an explanation that will satisfy any ordinary person, and even a philosopher who is not thinking too hard about theories of explanation. In fact, the only way for IN to have any real explanatory force for someone is for that person to translate it back to something like IM.

Both IM and IN have their virtues. IM provides a perspicuous, easily-understood, and satisfying explanation. The conclusion of IN, while imperspicuous, follows from its premises in first-order logic by purely computational means. The philosopher who thinks hard about explanation may insist that the derivability of its conclusion makes IN more explanatory. Such a philosopher may note that IN is the kind of inference on which the traditional Deductive-Nomological (D-N) model of explanation relies.

On the D-N, or covering-law, account, the explanation of a state of affairs is a logical inference, involving the laws of a serious theory combined with appropriate initial conditions. The theories to which D-N explanations appeal are ones in which we speak most strictly. Modifications of the D-N model, like Railton's model of probabilistic explanation, or Kitcher's unificationist account, work similarly. Kitcher, for example, relies on unifying argument patterns which also answer why-questions by proffering inferences made within a serious theory. While IN is a simple logical inference which suppresses auxiliary presuppositions involving laws governing mangoes, we could easily tidy it up, appealing to the relevant laws of conservation of mangoes, and such. IN is thus a good candidate for playing a central role in a D-N, or related, explanation of why there are five mangoes here.

---

[2] On sakes and behalves, see Quine 1960: 244.

So, there are two distinct senses of 'explanation' on the table, here. The first sense is represented by the casual inference IM. In contrast, the D-N and related models of explanation capture the relation between laws and explanations, perhaps the most salient feature in common to a wide range of explanations. For reasons that will become clear, I need to focus in this paper on the first sense of 'explanation'. Briefly, there is a new version of the indispensability argument in the philosophy of mathematics promoted as an alternative to the traditional Quinean argument. The new explanatory indispensability argument puts aside the question of whether mathematical objects are eliminable from our best, most serious theories, and focuses on whether mathematics plays an indispensable role in scientific explanations. If the explanatory argument relied only on a D-N (or related) model of explanation, it would not differ from the Quinean argument. The central question about the argument would be whether mathematical objects could be eliminated from our best theories, as IN eliminates the references to numbers in IM. Thus, the proponents of the explanatory argument must appeal to a different model of explanation.

Let's call the kind of explanation that IM provides, but that IN does not provide, epistemic, in contrast to the D-N and related kinds of explanation, which we can call metaphysical. An epistemic explanation is essentially tied to increasing understanding.[3] When we invoke metaphors in science, or use models for explanation, we often rely on an epistemic notion of explanation. We do not think that the atom is literally constructed like the solar system; nevertheless, the image of electron orbits can be a useful heuristic, as long as it is not taken too seriously.

Metaphysical explanations essentially involve covering laws or unifying principles and initial

---

[3] Pragmatic accounts of explanation have epistemic elements, as do causal-relevance accounts. See Friedman 1974 on the failure of D-N accounts to emphasize understanding. Some more recent accounts of scientific explanation, like Kitcher's, which I have classified as metaphysical, do attempt to assimilate inference and understanding. Still, the influence of Hempel's original covering law model, and its attempt to provide an objective, or non-epistemic, account of explanation extends to more recent work. Railton, for example, promises, "An account of probabilistic explanation free from relativization to our present epistemic situation" (Railton 1978: 219).

conditions. Most importantly, they rely on arguments in which we speak most soberly, like IN, in contrast to inferences, like IM, in which we speak casually. To account for residual concerns that explanations should foster understanding, the proponent of a metaphysical account promises that when we understand the laws or causal structures or unifying principles underlying a phenomenon, we will understand why that phenomenon occurred.

Both senses of 'explanation' have legitimate uses. This paper takes no position on whether our ordinary notion of explanation is sufficiently captured by any metaphysical or epistemic account. The point I want to make requires only that we can distinguish between a sense of 'explanation' which depends exclusively on our most parsimonious theories, and another sense, which underlies the new indispensability argument, for which our best theories do not suffice. I have illustrated the two kinds of explanations in the inferences above: IM is not explanatory in the metaphysical sense, but is explanatory in the epistemic sense. IN is explanatory in the metaphysical sense, but not in the epistemic sense.

There are two morals to be learned from the differences between IM and IN.

Moral 1: We are committed to mathematical objects not by our casual uses of numbers, but only when we are speaking most seriously.

Moral 2: The theory we use to specify our ontological commitments may not be most useful when we want to explain facts about the world, in the epistemic sense of 'explain'.

The central claim of this paper is that the explanatory indispensability argument is no improvement on the Quinean argument because it depends on an equivocation between the two senses of 'explanation'. If it appeals to an epistemic notion of explanation, then our uses of mathematics should not be taken seriously. If it appeals to a metaphysical notion of explanation, then it is just a restatement of the original argument.

There are a wide variety of versions of the indispensability argument available, and a slew of different responses. The criticism I present in this paper applies to several of the different arguments, so it will be helpful to examine and distinguish some of the specific arguments. I start with a weak version.

§2: Uncritical acceptance of face-value interpretations of mathematical practice

Any indispensability argument which purports to justify mathematical beliefs must specify when we are speaking most seriously. It is clear that the chips were not down in IM, but that was an easy case. The more general problem of knowing precisely when we are speaking most seriously, when we reveal our true ontological commitments, is trickier. In this section, to illustrate the importance of knowing when the chips are down, I discuss the relevance to the indispensability argument of the observation that mathematical objects are essential to mathematical practice. A version of the criticism in this section is precisely the problem with the explanatory argument.

Indispensability arguments attempt to justify mathematical beliefs while eschewing all appeal to rationalist epistemology. That is, the indispensability argument is supposed to be consistent with naturalist, even empiricist, principles.[4] One way of naturalizing mathematical epistemology would be to take the claims of practicing mathematicians at face value. Mathematicians say things like, "There are infinitely many primes." If we take such claims at face value, there is no doubt that mathematical objects exist. Call this argument the mathematical practice argument.

Alan Baker, calling deference to the practice of mathematicians strong mathematical naturalism, observes that such deference renders the indispensability argument moot.[5] The proponent of the traditional indispensability argument wonders whether mathematical objects are required for science. But, mathematical objects are obviously required for face-value interpretations of mathematics. Thus, the defender of the mathematical-practice argument has no need to wonder whether our scientific theories or explanations or practices commit us to mathematical objects. We are already so committed by mathematical practice.

---

[4] See, for example, the mere title of Resnik's "A Naturalized Epistemology for a Platonist Mathematical Ontology" (Resnik 1983).

[5] See Baker 2003: 63-4.

The central problem with the mathematical-practice argument is the same as the problem with reading commitments from IM. We lack justification for taking those statements as our most serious. We do not know if mathematical practice itself is to be taken seriously, in the way that we do not take seriously the practice of purported psychics. Even if we accept mathematical practice generally, the references to mathematical objects by mathematicians in their work may be illusory. As with IM, the chips may not be down. The work of Geoffrey Hellman and Charles Chihara in reformulating mathematical claims as modal claims is meaningful precisely because we need not take mathematical claims at face value.[6] Michael Potter extends the point: "What mathematicians *say* is not always a reliable guide to what they are doing: what they mean and what they say they mean are not always the same" (Potter 2007: 18).

Baker contrasts strong naturalism with Penelope Maddy's weak mathematical naturalism. According to Maddy, ontological questions are external to the practice of mathematics; they float free of working mathematicians. On Maddy's view, the indispensability argument is back in play. The indispensabilist ignores face-value interpretations of mathematical sentences and focuses on serious claims in science. Of course, scientists may be no better than mathematicians in always saying precisely what they mean when it comes to ontological commitments. As we will see in the next section, Joseph Melia argues that we can interpret both mathematicians and scientists as taking back all prima facie commitments to abstracta. "It is quite common for both scientists and mathematicians to think that their everyday, working theories are only partially true" (Melia 2000: 457).

In this section, we saw a reductio on a particular interpretation of naturalism, a version evocative of Quine's claims that we have no external tribunal, that there is no first philosophy. If there really were no external tribunal to the claims of mathematicians, then we would be forced to take their claims at face value. In contrast, we have some position from which to evaluate the claims of mathematicians and

---

[6] See Chihara 1990 and Hellman 1989.

scientists, some place to reflect on their more casual claims, at least to insist that they speak seriously about their commitments. And of course, Quine recommended that we seek an austere, parsimonious theory when we wanted to discover our ontological commitments, so what Baker calls strong mathematical naturalism is clearly not Quine's naturalism.

§3: Letting in the weasel

In contrast to the weak mathematical practice argument of the previous section, the strongest indispensability argument, the original argument at which the nominalist strategies of Field 1980 and others are aimed, is Quine's indispensability argument. Quine's argument is strongest because he takes seriously the problem of knowing when the chips are down. Despite the fact that Quine's argument is well-known, I want to display it carefully. Key details of Quine's argument are sometimes overlooked, and this neglect has opened the argument to inappropriate criticism. In this section, I discuss how some misinterpretations weaken Quine's argument. In the next section, I will present Quine's argument with its full armor.

Quine himself is at least partly at fault for some of the confusion surrounding the indispensability argument, since he never presented a specific, detailed version of the argument. Some philosophers see its reliance on holism as its central characteristic.[7] But, any indispensability argument has to give holism some role. For, the indispensabilist's central claim is that evidence for our scientific theory extends in some way to the mathematical elements of that theory. Without some measure of holism, it is impossible to make the case that evidence extends from science to mathematics.

The really important feature of Quine's argument, the one which distinguishes it from other

---

[7] See, for example, Baker 2005: 224 and Melia 2000: 455-6. By 'holism' in this paper, I refer to what is ordinarily known as confirmation holism, and which is the only holism directly relevant to the indispensability argument. Quine famously argues for confirmation holism from the stronger and more controversial semantic holism, especially in "Two Dogmas of Empiricism" (Quine 1951). But there are simpler argument for confirmation holism. See Colyvan 2001: §2.5.

versions, is Quine's insistence on spelling out the details of how and when we are to be taken as speaking most seriously about our ontological commitments. These details arise out of a combination of his holism and his naturalism, as well as his criterion for determining our ontological commitments. The broad way in which the indispensability argument is generally represented masks these central claims. For example, Mark Colyvan's version of the argument suppresses Quine's criterion for ontological commitment:

> 1. We ought to have ontological commitment to all and only those entities that are indispensable to our best scientific theories.
> 2. Mathematical entities are indispensable to our best scientific theories.
> Therefore:
> 3. We ought to have ontological commitment to mathematical entities (Colyvan 2001: 11).

Colyvan's first premise does not answer the question of when an entity is indispensable to our best theory: How does a theory make its posits? How do we read those posits? Are all the posits made in the same way, with the same force?

Colyvan's presentation of Quine's argument leaves open the possibility for denying commitment to mathematical objects despite not being able to eliminate mathematics from our best theory. A nominalist can respond to Colyvan's version of Quine's argument by accepting, say, that vectors in Hilbert space are indispensable to the practice of quantum mechanics, but adding that we can, when speaking most seriously and parsimoniously, deny that our best theory really posits them. This response is found in Melia 2000, who claims that one can "weasel out" of the indispensability argument, accepting that mathematics is ineliminable from scientific theory but maintaining that we need not believe that mathematical objects exist.[8]

Melia defends the weaseling strategy by claiming that scientists use mathematics in order to

---

[8] For other recent versions of the weaseling strategy, see Balaguer 1998, Chapter 7; Yablo 2005; Leng 2002; Leng 2005; Pincock 2004a; Pincock 2004b; and Colyvan forthcoming.

represent or express facts that are not representable without mathematics. But, such representations are not supposed to be ontologically serious. “The mathematics is the necessary scaffolding upon which the bridge must be built. But once the bridge has been built, the scaffolding can be removed” (Melia 2000: 469).

Balaguer, Leng, and Pincock make similar remarks concerning the ontologically casual role of mathematics as a tool for expressing, representing, and modeling. For example, “We are not committed to belief in the existence of objects posited by our scientific theories *if their role in those theories is merely to represent configurations of physical objects*. Fictional objects can represent just as well as real objects can” (Leng 2005: 179).

Melia provides a helpful analogy for those resistant to the weasel response. Consider the two-dimensional surface of a sphere. From a three-dimensional perspective, it is easy to describe the surface as the locus of all points equidistant from the center of the sphere. In order to describe the non-Euclidean spherical surface, we appeal to the center point of the sphere and three-dimensional Euclidean properties. But, the center is not part of the two-dimensional surface. From the point of view of the surface of the sphere, we can both appeal to the center while not really taking it as part of the world. “We do successfully and unproblematically describe a particular non-Euclidean world by taking back some of the implications of what we earlier said” (Melia 2000: 468).

Note that there are two entirely different kinds of negative responses to the indispensability argument. The first, which we can call the dispensabilist strategy, argues that to eliminate our beliefs in mathematical objects, we must rewrite our claims which refer to mathematical objects as ones which do not refer to mathematical objects. Thus, the replacement of IM by IN illustrates a simple dispensabilist strategy. Field’s reformulation of NGT is a more sophisticated version. In contrast, the second kind of negative response to the indispensability argument, the weaseling strategy, just issues a flat denial of any commitments to mathematical objects, whether or not nominalist alternatives to our claims which refer to mathematical objects are available.[9]

The weaseling strategy is related to instrumentalist views about ideal elements in scientific theory. For example, Penelope Maddy alleges that scientists often view certain commitments, e.g. to infinitely-deep water waves, instrumentally.[10] If scientists view some of the posits of their theory instrumentally, they could avoid sincere commitments to mathematical objects, as well.

It is not my goal in this paper to defend or criticize the weaseling strategy. My argument is merely that certain versions of the indispensability argument are particularly liable to weaseling, while others resist the weasel. Though, I will note that a proponent of the weasel strategy who provides an alternative criterion of ontological commitment, like Azzouni, who defends a version of the eleatic principle, is in a better position than one who merely denies commitments to mathematical objects.

Mark Balaguer's version of the indispensability argument, while providing a weaker conclusion than Colyvan's, also suppresses the details of Quine's argument.

> (i) mathematical sentences form an indispensable part of our empirical theories of the physical world—i.e., our theories of physics, chemistry, and so on; (ii) we have good reasons for thinking that these empirical theories are true, i.e., that they give us accurate pictures of the world; therefore, (iii) we have good reasons to think that our mathematical sentences are true (Balaguer 2008, §2.1).

Balaguer's version does not conclude that we should believe that mathematical objects exist, which is, presumably, the job of the theory of truth.[11] Clearly, Balaguer suppresses Quine's premises about how to determine the ontological commitments of our best theory. Like Colyvan's version of

---

[9] Colyvan forthcoming calls the dispensabilist strategy hard-road nominalism and the weasel strategy easy-road nominalism.

[10] Maddy 1992: 281. See also Azzouni 2004.

[11] On standard theories of truth, if a sentence like '28 is a perfect number' is true, then a number exists. Of course, various non-traditional semantics are available for mathematical sentences, ones on which mathematical sentences can be true without entailing commitments to mathematical objects.

Quine's argument, Balaguer's argument is liable to weaseling.

§4: Quine's indispensability argument

In contrast to the versions of the indispensability argument we have seen so far, Quine's version of the argument resists weaseling. Quine's argument not only demands that we transfer evidence from science to mathematics, and that our scientific theories are the locus of our real commitments, but also that we find our commitments in a particular way.

QI
QI1. We should believe the theory which best accounts for our sense experience.
QI2. If we believe a theory, we must believe in its ontological commitments.
QI3. The ontological commitments of any theory are the objects over which that theory first-order quantifies.
QI4. The theory which best accounts for our sense experience first-order quantifies over mathematical objects.
QIC. We should believe that mathematical objects exist.[12]

QI tells us that the chips are down in our best scientific theory. It does not tell us how to determine which theory is our best, but criteria for ranking theories are, according to Quine's naturalism, matters for scientists to determine. More importantly, QI tells us precisely how to determine the commitments of our best theory. Premises QI2 and QI3 are vital because they block attempts to weasel out of the indispensability argument. When the weasel says that we can differentiate between the real and the merely instrumental posits of our theory, Quine's holism blocks the move: all posits are on par. When the weasel says that we can take back what we allege, Quine's naturalism denies that such double-talk is sensible or defensible.

The double-talk criticism of instrumentalists and weasels is essential to QI. For Quine, if our best theory requires electrons for its bound variables, then we are committed to electrons. If it requires sets, we are committed to sets. Quine criticizes double-talk throughout his work. His response to

[12] See Quines 1939, 1948, 1951, 1955, 1958, 1960, 1978, and 1986.

Carnap's internal/external distinction, for example, relies on the double-talk criticism.[13] Once one has accepted mathematical objects as an internal matter, one can not merely dismiss these commitments as the arbitrary, conventional adoption of mathematical language. Similarly, in "On What There Is," Quine's response to the Meinongian Wyman, who presents two species of existence, is a variation on the double-talk criticism. We must distinguish between the meaningfulness of 'Pegasus' and its reference in order to avoid admitting that Pegasus subsists while at the same time denying that it exists.

Worries about double-talk bother both Quine's friends and his critics. Putnam makes the double-talk criticism explicitly. "It is silly to agree that a reason for believing that p warrants accepting p in all scientific circumstances, and then to add 'but even so it is not *good enough*'" (Putnam 1971: 356).

Field applies the double-talk criticism directly to mathematics. "If one *just* advocates fictionalism about a portion of mathematics, without showing how that part of mathematics is dispensable in applications, then one is engaging in intellectual doublethink..." (Field 1980: 2).

Proponents of the weaseling strategy depend on the legitimacy of double-talk, of taking back what one initially alleges. "In order to communicate his picture of the world, [the weasel-nominalist] Joe must make clear that, as far as he is concerned, [the platonistic scientific theory] T* is wrong in certain respects. What Joe wants to do is *subtract* or *prune away* the platonistic entities whose existence is entailed by T*. Accordingly, in communicating his belief about the world, Joe might say, 'T* - but there are no such things as sets. In doing so, he is asserting some sentences, whilst denying one of their logical consequences" (Melia 2000: 466-7).

An indispensability argument which rejects the legitimacy of double-talk is resistant to such

[13] Here's Carnap, an early proponent of weaseling: "...[S]ome contemporary nominalists label the admission of variables of abstract types as 'Platonism'. This is, to say the least, an extremely misleading terminology. It leads to the *absurd consequence*, that the position of everybody who accepts the language of physics with its real number variables (as a language of communication, not merely as a calculus) would be called Platonistic, even if he is a strict empiricist who rejects Platonic metaphysics" (Carnap 1950: 215, emphasis added).

weaseling. QI claims that our commitments are to be found in the quantifications of our one best theory. Once we differentiate among the posits of that theory, we are rejecting the cornerstones of QI which blocks the weasel: the combination of Quine's holism, his naturalism, and his criterion for ontological commitment.

§5: Problems with Quine's holism

In claiming that QI is resistant to weaseling, I do not mean to imply that QI is resistant to all criticism. Among the lesser problems with QI is that the indispensabilist's mathematical commitments are limited to those mathematical objects required by science. Mathematicians, especially set theorists, ordinarily work with a vaster universe than science requires. The indispensabilist has two options. She can distinguish between legitimate (i.e. applied) mathematics and the rest on the basis of some criterion external to mathematical practice, as Quine did. Or, she can find an independent justification for the mathematical objects, like large cardinals, which have little hope of ever finding application. The former option seems to abandon the naturalism underlying the indispensability argument. The latter option, if successful, is likely to render the indispensability argument moot.

In addition to the problem of a restricted mathematical universe, the indispensabilist's posits of mathematical objects are justified in the same way as her posits of concrete objects so that an abstract/concrete distinction is difficult to defend. Claims that the indispensabilist's mathematical objects exist necessarily, or eternally, are strained. And, since the justifications of mathematical beliefs are the same as those of science, the special, a priori methods of mathematics are impugned. Many indispensabilists welcome these latter consequences. Quine, of course, was happy to give up the apriority of mathematics. Colyvan embraces both the contingency of mathematical objects and the empirical status of our beliefs about them.[14]

More seriously for present purposes, QI depends on, and derives its resistance to weaseling, from its holism. Unfortunately, since holism spreads evidence among the empirical and mathematical axioms of a theory, any reason to doubt the truth of the empirical axioms also transfers to the mathematical axioms. And, it is notoriously difficult to establish the truth of scientific theory. If scientific anti-realists like Bas van Fraassen and Nancy Cartwright are correct, our confidence in much of standard mathematics, which seems more well-founded than our beliefs in even the most well-confirmed scientific theory, is undermined.[15]

Elliot Sober questions the holism presumed by QU directly. Sober argues that we never cede mathematical beliefs on the basis of empirical experiments. We subject mathematical claims to completely different kinds of tests, and do not hold them open to refutation on the basis of empirical evidence.[16]

Sober calls the problems which confront science discrimination problems. We evaluate a scientific hypothesis against other hypotheses. We are only able to do this when other hypotheses are available. Experiments solve discrimination problems among competing hypotheses by providing evidence in favor of one or another. For example, Sober considers these three competing hypotheses:

Y_1. Space-time is curved.
Y_2. Space-time is flat.
Y_3. Space time is not curved, although all evidence will make it appear that it is.

Empirical evidence will discriminate between Y_1 and Y_2, but no evidence will discriminate

---

[14] See Colyvan 2001, Chapter 6. Field embraces the conceptual contingency of mathematical objects for distinct reasons. See MacBride 1999: §4 and Field 1993, and the references therein.

[15] See Cartwright 1983 and van Fraassen 1980.

[16] Sober 1993. See Leng 2002: §3 and §6, in support of Sober.

between Y_1 and Y_3. Similarly, no discrimination problem can help us to confirm the truth of mathematical statements, or the existence of mathematical objects. "If the mathematical statements *M* are part of every competing hypothesis, then, no matter which hypothesis comes out best in the light of the observations, *M* will be part of that best hypothesis. *M* is not tested by this exercise, but is simply a background assumption common to the hypotheses under test" (Sober 1993: 45).

Sober thus questions the holist's allegation that it is always in principle possible to cede any beliefs in light of recalcitrant experience by examining the ways we actually test our hypotheses. He provides examples of everyday failures of additivity: two gallons of salt and two gallons of water do not yield four gallons of salt water; two foxes and two chickens yield only two fat foxes and a pile of feathers. If holism were right, there should in principle be an option of giving up our mathematical beliefs in such cases. If all examples where we would cede mathematical beliefs are unavoidably abstruse, QI and its holism appear suspect.[17]

Michael Resnik, defending indispensability, denies that the differences in practice which Sober cites refute holism. "Sober is right that in practice we rarely, if ever, put mathematical laws to the sorts of specific tests that we apply to some scientific hypotheses. But this does not imply that purely logical considerations show that mathematics is immune to such testing" (Resnik 1997: 124).

Confirmation holism, as a logical matter, is immune from criticism. In any theory, there will be many ways to resolve a contradiction, or to hold a particular statement true in light of contravening evidence. Sober's point is that scientific methodology holds mathematical principles immune from revision in practice.[18] If the indispensabilist is committed to even a weak naturalism, one that defers to practice at all, the holism which supports QI also undermines it.

---

[17] Sober is agnostic about whether empirical evidence can ever influence our beliefs about number theory. See Sober 1993: 36-37, fn 5.

[18] Maddy agrees: "Logically speaking, this holistic doctrine is unassailable, but the actual practice of science presents a very different picture" (Maddy 1992: 280). See also Leng 2002: 397.

There were two reasons to examine some difficulties with Quine's holism. First, in contrast to the weaseling strategy, the criticisms of holism illustrate a direct attack on QI, one which would seriously undermine its strength, if successful. Second, the criticisms of holism led proponents of the indispensability argument to look for versions of the argument which do not rely on holism. This move eventually led to the development of the explanatory argument, which is the central topic of this paper.

While some indispensabilists, like Resnik, are unrepentant holists, others are not. Furthermore, the indispensability argument is supposed to support platonism, and certainly not all platonists are holists.[19] As Alan Baker points out, it would be useful for the platonist to have a version of the indispensability argument that does not depend on holism.

Here is another way to put the dialectic, so far. As I argued in §3, the weasel can attack versions of the indispensability argument which do not rely on holism to block any differentiation among mathematical and other posits and to block attempts to find our commitments elsewhere than our best scientific theories. The indispensabilist, seeing the difficulties with holism, wants a version of the argument which eschews what seems to be an expendable and controversial element of the argument. In the next section, I will examine two more version of the indispensability argument. These versions of the argument eschew holism, as the explanatory argument does, and fall right into the weasel's trap, like the mathematical practice argument of §2 and the misrepresentations of Quine's argument of §3.

§6: Antecedents of the explanatory argument

In this section, I examine two indispensability arguments, one from Hilary Putnam and the other from Michael Resnik. Putnam's argument may have been intended as an augmentation of Quine's argument, though it differs significantly. Resnik's argument was intended as an alternative. I argue that

[19] The most serious problems arising from the indispensability argument for non-indispensabilist platonists are the restrictions of mathematical commitments I mentioned at the beginning of this section.

both arguments fail to specify explicitly when the chips are down, and so are liable to weaseling.

Quine's indispensability argument is sometimes called the Quine-Putnam argument. To be sure, Putnam defended Quine's argument, especially early in his career.[20] But, Putnam later formulated his own version of the indispensability argument, the success argument. Putnam's success argument in mathematics is analogous to Putnam's success argument for scientific realism. The scientific success argument relies on the claim that any position other than realism makes the success of science miraculous. Putnam's success argument for mathematics, independent of the scientific success argument, defends what he calls mathematical realism.

PS
PS1. Mathematics succeeds as the language of science.
PS2. There must be a reason for the success of mathematics as the language of science.
PS3. No positions other than realism in mathematics provide a reason.
PSC. So, realism in mathematics must be correct.[21]

PS clearly leads to questions about the nature of mathematical realism, and how realism provides a reason for the success of mathematics in science. The existence of abstract objects, by itself, does not explain their applicability. Indeed, it seems prima facie puzzling how abstract objects could have any application to the concrete world.

Even putting aside (for a moment) worries about how realism explains the applicability of mathematics, PS is weak at its third premise. Any account of the applicability of mathematics to the

[20] On holism: "I should like to stress the monolithic character of our conceptual system, the idea of our conceptual system as a massive alliance of beliefs which face the tribunal of experience collectively and not independently, the idea that 'when trouble strikes' revisions can, with a very few exceptions, come anywhere" (Putnam 1962: 40).

On indispensability: "This type of argument stems, of course, from Quine, who has for years stressed both the indispensability of quantification over mathematical entities and the intellectual dishonesty of denying the existence of what one daily presupposes" (Putnam 1971: 347). Note the criticism of double-talk.

[21] See Putnam 1975: 72-3. I'll take 'realism' in this argument to entail belief in the existence of mathematical objects.

empirical world other than the indispensabilist's would refute PS3. For example, Field 1980 argued that mathematics is successful as the language of science because it is conservative over nominalist versions of scientific theories. Mathematics is just a convenient shorthand for a theory which includes no mathematical axioms when cast in its most austere form.[22]

The problem of accounting for the applications of mathematics in science has received a fair amount of attention since Putnam formulated PS, and there are several different accounts available, now. On Mark Balaguer's plenitudinous platonism, which takes any consistent mathematical theory to be true, mathematics provides a theoretical apparatus which applies to all possible states of the world. The problem of application is solved merely by noting that for all consistent situations there is a mathematical theory which applies to it.[23] Balaguer's account of application is also, broadly speaking, realist, but it leads to a very different kind of platonism from Putnam's. While Putnam, Field, and Balaguer all present their accounts as support for their ontological claims, Chris Pincock's structuralist, or mapping, account of the applications of mathematics approaches the problem head-on, from an ontologically neutral perspective. For Pincock, we can account for the applications of mathematics in science as long as there are appropriate structural relations, or mappings, between the physical and mathematical worlds.[24] All of these alternatives to Putnam's account undermine PS3.

One could amend PS3:

PS3*. Realism best explains the success of mathematics as the language of science.

---

[22] Put aside, here, questions about whether mathematics is really conservative over nominalist versions of science. Though, see Melia 1998 for some recent worries and see §7 below for observations on the current status of Field's claim. MacBride 1999 is a useful survey.

[23] See Balaguer 1998, Chapters 3 and 4. Balaguer's account is actually slightly more subtle. "[A]ll purely mathematical theories truly describe some part of the mathematical realm, but...it does not follow from this that all such theories are *true*" (Balaguer 1998: 60).

[24] See Pincock 2004a and Pincock 2004b.

Swapping PS3 for PS3* does not suffice, though, since realism does not best explain the application of mathematics. Realism is just the claim that some mathematical claims are true, and some mathematical objects exist. It says nothing about the applicability of mathematics to the physical world. Moreover, a dispensabilist construction like that of Field 1980, in conjunction with his conservativeness claim, erodes confidence in both PS3 and PS3* by presenting an alternate account of why mathematics is useful in science.

Putting aside worries about the weakness of PS3/PS3*, we can see similarities between PS and Resnik's pragmatic indispensability argument.

RP

RP1. In stating its laws and conducting its derivations, science assumes the existence of many mathematical objects and the truth of much mathematics.

RP2. These assumptions are 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.

RP3. So we are justified in drawing conclusions from and within science only if we are justified in taking the mathematics used in science to be true.

RP4. We are justified in using science to explain and predict.

RP5. The only way we know of using science thus involves drawing conclusions from and within it.

RPC. So, by RP3, we are justified in taking mathematics to be true (Resnik 1997: 46-8).

Both PS and RP differ from QI in two key ways. First, neither argument is explicitly holistic. They refer to two distinct theories, a scientific theory and mathematical theory, rather than a single theory with both mathematical and empirical premises. Also neither argument specifies how one determines ontological commitments either within science or mathematics, leaving that matter elliptical.[25] They are thus liable to both dispensabilist and weaseling responses.

Both PS and RP, if successful, would avoid the holist's problem of the falsity of science. They

[25] Resnik describes the difference between QI and RP differently: "Instead of claiming that the evidence for science (one body of evidence) is also evidence for its mathematical components (another body of statements) it claims that the justification for doing science (one act) also justifies our accepting as true such mathematics as science uses (another act)" (Resnik 1997: 48).

leave open the option for instrumental interpretations, and even the falsity, of portions of scientific theory. That is, they are both attempts to justify mathematical beliefs while avoiding worries about whether we must believe in the less secure portions of scientific theories. Mathematics is clearly useful, or successful, as the language of science, and even if our best scientific theories are false, their undeniable practical utility still justifies our using them. Even if scientific theory turns out to be largely false, that falsity may not affect our beliefs in the mathematics we used to express that theory. A tool may work fine, even on a broken machine.

This flexibility comes at a cost. The inference to the truth of mathematic in RP1 is unjustified in the absence of a clear explanation of how science assumes the existence of objects. The utility of mathematics is not by itself an argument for its truth. We need a procedure for determining existence, or truth, or reference to establish that these are to be found in science.

The same problem appears in RP2. The scientist may work without considering the question of mathematical truth at all: without employing a truth predicate applicable to mathematical statements, without taking mathematical theorems to be true. Again, we need a procedure for determining commitment, for knowing when the chips are down.

Moreover, a pragmatic argument for the indispensability of mathematics is no indispensability at all. All scientists need, whether we interpret their work as true or merely instrumentally useful, is the practical utility of mathematics. They need not presuppose mathematical truth.

Similar problems beset PS. Putnam's argument for the key third premise is essentially a version of Quine's double-talk argument. For the Quinean holist, the double-talk argument has significant force. The holist has no external perspective from which to evaluate the mathematics in scientific theory as instrumental. S/he can not say, "Well, I commit to mathematical objects within scientific theory, but I don't really mean that they exist." Instrumentalism entails a rejection of holism.

For the indispensability arguments which eschew explicit commitments to Quinean holism, instrumentalist interpretations of the mathematics used in scientific theory are even more compelling. Without holism, we are no longer constrained to limit existence claims to the quantifications of our best theory. We are free to adopt an eleatic principle, for example, as the fundamental criterion for existence. What makes QI immune to the weasel, and what makes PS and RP liable to the weasel, is Quine's claim that the ontological chips are down precisely in our single best theory, and that we find our commitments in the quantifications of that holistic theory.[26] Putnam and Resnik tried to save the indispensability argument from problems arising from holism, but they opened the argument up to weaseling criticisms to which Quine's original argument was immune.

§7: The explanatory argument

The explanatory indispensability argument, though reminiscent of PS and RP, mainly arose in response to dispensabilist criticisms of the original Quinean argument. Field 1980, which attempted to rewrite Newtonian Gravitational Theory (NGT) by quantifying over space-time regions rather than real numbers, and Burgess's later improvements, gave hope to nominalists. This hope was not unbridled, and the project received significant criticism. The current consensus about dispensabilist projects is that something pretty close to Field's project can work for NGT, but that other theories, including those based on curved space-time and those which rely on statistical frameworks, are resistant. Still, advances have been made, like Balaguer's steps toward nominalizing quantum mechanics, and, as Burgess and Rosen argue, the lack of dispensabilist strategies currently available is weak evidence for their eventual non-existence.[27] It is clear that no neat, first-order theory which eschews all mathematical axioms will suffice

[26] Pincock 2004a: 62 makes a similar point, in passing.

[27] "As a consequence of nominalism's being mainly a philosopher's concern, this open research problem is...one that has so far been investigated only by amateurs - philosophers and logicians - not professionals - geometers and physicists; and the failure of amateurs to surmount the obstacles is no strong grounds for pessimism about what could be achieved by professionals." (Burgess and Rosen 1997: 118)

for all of current and future science. But, the dispensabilist has reasonable hope of finding moderately attractive reformulations of large swaths of scientific theory.

Indispensabilists have thus been both emboldened by the lack of convincing success on the side of the nominalists, and eager to fortify the original argument against dispensabilist criticisms. According to the new explanatory indispensability argument, we should believe in mathematical objects because of their indispensable roles in our scientific explanations. Alan Baker defends the explanatory argument, and we can see versions of it in the work of Mark Colyvan. Baker does not state his version of the argument explicitly, but Paolo Mancosu does:

EI
EI1. There are genuinely mathematical explanations of empirical phenomena.
EI2. We ought to be committed to the theoretical posits postulated by such explanations.
EIC. We ought to be committed to the entities postulated by the mathematics in question (Mancosu 2008: §3.2).[28]

The provenance of EI is a matter for dispute. Baker and Mancosu both misleadingly credit Field. “Hartry Field, one of the more influential recent nominalists, writes that the key issue in the platonism-nominalism debate is ‘one special kind of indispensability argument: one involving indispensability for explanations’ (Field 1989, p. 14)” (Baker 2005: 225).

Sorin Bangu follows Baker and Mancosu on this misdirection: “Field noted that even if, contrary to what he argued in his (1980), mathematical posits turn out to be indispensable to scientific theorizing, they still can’t be granted ontological rights until they are shown to be indispensable in a stronger, more specific sense; in particular, the realists should be able to show that mathematical posits are indispensable for scientific explanations (Field, 1989, pp. 14-20)” (Bangu 2008: 13-4).

A careful reading of the selection cited by both Baker and Bangu shows no such argument by

[28] See Baker 2005: 224 for the claim that EI is not holistic. Mancosu supports Baker’s claim that EI is a non-holistic argument, though like all indispensability arguments, it must presume enough holism to support the transfer of evidence from science to mathematics.

Field. Specifically, Field makes no claim that there is a heavier burden on the dispensabilist than recasting standard scientific theories to remove quantification over mathematical entities. Field's interest in explanation depends exclusively on how explanatory merit factors into evaluations of our theories. His concern is with the traditional indispensability argument, i.e. QI, where one factor in determining whether a theory is our best theory is whether it has explanatory force. Other factors include breadth and simplicity. For Field, once we have settled on a best theory, the central question for the indispensabilist is whether that theory can be recast to avoid quantification over mathematical objects.

In the section cited, Field explicitly refers to his own work rewriting NGT, and he moves directly from talk about explanation to talk about theories.

> What we must do is make a bet on how best to achieve a satisfactory overall view of the place of mathematics in the world... My tentative bet is that we would do better to try to show that the explanatory role of mathematical entities is not what is superficially appears to be; and the most convincing way to do that would be to show that there are some fairly general strategies that can be employed to purge theories of all reference to mathematical entities (Field 1989: 18; see also fn 15 on p 20).

Field is clearly thinking of explanation on a metaphysical model, like a traditional covering-law account. In fact, Field says that an explanation is, "A relatively simple non-*ad hoc* body of principles from which [the phenomena] follow" (Field 1989: 15).[29]

In contrast, the key feature of the explanatory argument is that it puts aside the question of whether theories can be recast in order to eliminate mathematical entities. Instead, the proponent of the explanatory argument wonders whether non-mathematical explanations of physical phenomena are available. Indeed, recent work on EI grants the availability of nominalist reformulations of standard scientific theories and continues to urge that mathematical explanations of empirical phenomena support

[29] Mancosu does note that Field's discussion of the explanatory indispensability argument leads to questions about the success of his dispensabilist project, but misses the point that if such projects were relevant to the success of the indispensability argument, then the argument in question must be the original version, and not the new explanatory argument.

belief in mathematical objects. Exploring Malament's claim that phase-space theories resist dispensabilist constructions, Lyon and Colyvan write, "Even if nominalisation via [a dispensabilist construction] is possible, the resulting theory is likely to be less explanatory; there is explanatory power in phase-space formulations of theories, and this explanatory power does not seem recoverable in alternative formulations" (Lyon and Colyvan 2008: 242).

Given a metaphysical interpretation of 'explanation', Lyon and Colyvan's claim would be nearly nonsensical. Conserving explanatory power is a standard requirement on nominalist reformulations, and it works unlike other theoretical virtues. One might wonder whether sacrifices in simplicity of ideology are worth parsimony in ontology or not. But, scientists never give up (metaphysical) explanatory power in order to increase simplicity or parsimony. Indeed, Field constructs representation theorems for his reformulation precisely to support the claim that it does not lack any (metaphysical) explanatory power present in the standard version of the theory. One just could not successfully nominalize a scientific theory by producing an alternative with less explanatory power, unless one is using a non- metaphysical sense of the term.

If we interpret 'explanatory power', as Lyon and Colyvan use it, in the epistemic sense, though, their claim is plausible. Unlike standard scientific theories, dispensabilist reformulations will be imperspicuous, and not useful to working scientists. Dispensabilists generally do not suggest that scientists adopt the reformulations. Indeed, Field grants that standard theories are more explanatory in the epistemic sense by arguing for the conservativeness of mathematics over standard scientific theory, the claim that adding mathematical axioms to nominalist theories will not allow one to derive any further nominalist conclusions. The nominalist wants to be sure that mathematics is conservative precisely because we are inevitably going to take advantage of the greater epistemic explanatory force of standard theories.

Thus, the explanatory indispensability argument, in order to differentiate itself from QI, must rely on a notion of explanation that is not metaphysical. We can see that EI depends on an epistemic sense of explanation by looking at EI1, which claims that there are mathematical explanations of empirical phenomena. Standard (metaphysical) accounts of scientific explanation do not comfortably apply to mathematical explanation. It is quite easy to produce mathematical inferences which conform to standard criteria for scientific explanation, but which are not at all explanatory. For example, one can derive in complicated fashion from basic axioms that 2+2=4; such complex derivations are not taken as explanations of the simple claim. In response, some philosophers of mathematics distinguish between explanatory and non-explanatory proofs. Another option would be to abandon the notion of mathematical explanation altogether. But, one could not feasibly hold onto a metaphysical interpretation of 'explanation' in EI1.

The distinction between whether we take explanation to be a theoretical virtue, and thus are relying on QI, and whether we are putting aside theoretical virtues and looking at the indispensability of mathematics for epistemic explanations, is subtle but important. The availability of a dispensabilist reformulation of a standard scientific theory is *essential* to QI. The availability of a dispensabilist reformulation of a standard scientific theory is *irrelevant* to EI. A reformulation inevitably loses explanatory strength, in the epistemic sense.

Lyon and Colyvan's position seems confused between the two. One could pack all of the observations about explanatory virtues of standard science that defenders of EI offer into a defense of QI, but that would abandon the new, explanatory argument. The defenders of EI, remember, are looking for ways to avoid the question of whether dispensabilist reformulations of standard theories are available.

These options allow us to raise a question about presenting EI as an alternative to other indispensability arguments. We can see the argument as an additional demand on the platonist, and thus an additional option for the nominalist. Bangu and Melia say that even if dispensabilist constructions do not work, we should withhold commitments to mathematical objects since there are no genuinely mathematical explanations. On the Bangu/Melia view, the platonist has to show mathematics indispensable from both theories and explanations; the nominalist needs to show that mathematics is eliminable only from explanations or theories.[30]

In contrast, we can see the argument as an additional option for the platonist, and thus an additional demand on the dispensabilist. Baker and Lyon and Colyvan argue that even if the dispensabilist constructions do work, we should grant commitments to mathematical objects as long as there are genuinely mathematical explanations of physical phenomena. On the Baker/Lyon and Colyvan view, the platonist needs to show that mathematics is indispensable only from explanations, and the nominalist must show how we can eliminate mathematics from both theories and explanations.

It will not really matter here whether EI is taken as an additional burden on the nominalist or on the platonist. But, future proponents and critics of EI should agree on an interpretation.

§8. Mathematical explanations of physical phenomena

Debate over EI has focused on its first premise. Sorin Bangu, responding to Baker's version of EI, argues that any purported mathematical explanation of a physical phenomenon is really a mathematical explanation of a mathematical phenomenon, and so question-begging against the realist. In the remaining three sections of this paper, I first defend EI1 against Bangu's criticism, arguing that there are mathematical explanations of physical phenomena. Then, I argue that such explanations do not decide the soundness of EI, since the real problem is at EI2.

Examples of mathematical explanations of physical phenomena can be used to support both QI, taking explanatory merit as a theoretical virtue, or EI. Colyvan 2001 presents three examples intended mainly to support the claim that standard, mathematized theories have greater explanatory merit.

[30] See Bangu 2008, and Melia 1998: 70. Melia 2002 and Leng 2005: 179, though working with explanation as a theoretical virtue, can also be seen as taking this route.

ME1. The bending of light. The best explanation of light bending around large objects is geometric, rather than causal.
ME2. Antipodes. The Borsuk-Ulam topological theorem, along with appropriate bridge principles, explains the existence of two antipodes in the Earth's atmosphere with exactly the same pressure and temperature at the same time.
ME3. The Fitzgerald-Lorentz contraction. Minkowski's geometrical explanation of the contraction of a body in motion, relative to an inertial reference frame, relies on equations in four dimensions, representing the space-time manifold.[31]

Colyvan 2007 present three further illuminating examples.

ME4. Squaring the circle. That π is transcendent explains why we can not construct a square with the same area as a circle, using straight-edge and compass.
ME5. Mountaineering. A hiker, leaving base camp on one day and top camp the next, at the same time, will pass one point on the trail at the exact same time on both days.
ME6. Altruism. Simpson's paradox explains, in part, how a maladaptive trait like altruism can succeed, despite the fact that altruistic populations, taken individually, are less fit.[32]

Not all of these examples are equally compellingly described as cases of mathematical explanations of physical phenomena. Baker rightly worries about the status of the geometry on which ME1 and ME3 rely. If the relevant geometry is physical geometry, then the explanation may proceed without appeal to abstract mathematical objects. Baker also argues that ME2 is a prediction, rather than an explanation. It is implausible to claim that we could discover the two antipodes, given both limitations on the precision of our instruments and independent interest in the phenomenon. Baker's complaint, which is echoed by Mary Leng, is that if explanations are answers to why-questions, ME2 could not be an explanation, since there is no antecedent why-question. Note that this complaint only really holds on what I have called an epistemic account of explanation; one can easily provide a deduction, or unifying argument, which yields the given phenomenon. Leng also complains that ME2 requires contentious idealizations, and so the requisite bridge principles will not really apply. Still, these

[31] See Colyvan 2001: 81-6.

[32] See Colyvan 2007: 120-1.

worries do not impugn the claim that if such antipodes were found on the Earth, the Borsuk-Ulam theorem would help explain them.[33]

Leng also calls ME4 a prediction. But, it is not a prediction of a physical fact. It is possible to construct a square with an area arbitrarily close to that of a given circle, by choosing arbitrarily close rational approximations of pi. We can draw a square-ish region with the same area as a given circle, within any given margin of measuring error. Still, if we had arbitrarily good measuring tools, we could always find a difference in the areas of the square and circle. If we found that we could not square the circle, the transcendence of pi would help explain that fact.

ME5 is more plausibly an explanation, in Baker and Leng's sense. Though, ME6 may not be true. Colyvan presents the explanation as conclusive, citing Malinas and Bigelow, but they only conclude that the mathematical result is worth examining since it *could* explain the persistence of altruism: "It is of considerable theoretical significance to explore the applications of Simpson's Paradox, to see whether this might help to explain not only the altruism but also the irrationality, inefficiency, laziness and other vices that may prevail in populations, and that can cause a population to fall short of the economic rationalist's or Darwinian's ideal of the ruthlessly efficient pursuit by each individual of its own profits or long-term reproductive success" (Malinas and Bigelow 2008).

Baker, interested in defending the claim that there are mathematical explanations of physical phenomena but worried about Colyvan's ME1-ME3, constructed an additional and influential cicada example.

> ME7. Cicadas. That prime-numbered life-cycles minimize the intersection of cicada life-cycles with those of both predators and other species of cicadas explains why three species of cicadas of the genus Magicicada share a life cycle of either thirteen or seventeen years, depending on the environment.

---

[33] See Baker 2005: 226-7 and Leng 2005: 181-2.

Whether all of ME1-ME7 work exactly as proponents of EI require is too strong a demand for establishing EI1. What is important is the underlying claim that there are mathematical explanations of physical phenomena. If we take 'explanation' in the epistemic sense, as I have alleged that the defenders of EI must, that claim seems overwhelmingly plausible. Baker and Colyvan's examples describe either actual or possible physical phenomena. They invoke mathematics in attempts to explain, in an epistemic sense, those phenomena. It may be possible to re-describe some of the phenomena or explanations either to eliminate or to isolate the mathematical elements. But, as they stand, such examples provide decisive, if unsurprising, evidence for EI1. Indeed, even the simple IM supports EI1. For the purposes of the remainder of this section, which is to defend Colyvan and Baker's claim of EI1 from Bangu's criticisms, I will focus on ME7, which Bangu discusses and which is taken to be the strongest example.

Three species of cicadas of the genus Magicicada share a life cycle of either thirteen or seventeen years, depending on the environment. The phenomenon of having prime-numbered life-cycles intrigued biologists, who sought an explanation. Baker claims that the phenomenon is explained thus:

CP

CP1. Having a life-cycle period which minimizes intersection with other (nearby/lower) periods is evolutionarily advantageous.
CP2. Prime periods minimize intersection.
CP3. Hence organisms with periodic life-cycles are likely to evolve periods that are prime.
CP4. Cicadas in ecosystem-type, E, are limited by biological constraints to periods from 14 to 18 years.
CP5. Hence, cicadas in ecosystem-type, E, are likely to evolve 17-year periods (Baker 2005: 233).

Baker argues that the mathematical explanans, at CP2, supports the empirical explanandum, at CP3. In fact, as Baker notes, CP3 is a "'mixed' biological/mathematical law." He proceeds to use this law to explain the further empirical claim CP5. Note that to apply the mathematical theorem used in CP2 to the case at hand at CP3, we need bridge laws, assurances that number theory applies to the cicadas' cycles. The pure number-theoretic premise refers to numbers. It might be reinterpreted as referring to sets. It says nothing about life-cycles and their intersections.

Bangu argues that the explanandum in question at CP5 is, like CP3, a mixed statement, composed of both mathematical and physical facts: a physical phenomenon (the time interval between successive occurrences of cicadas); the concept of a life-cycle period, expressed in years; the number (17, here); and the mathematical property of primeness. The mathematical explanation only explains the mathematical portions of the explanandum. "So, if the explanandum is the relevance of the primeness of a certain number, since primeness is a mathematical property, it is not surprising that we have to advance a mathematical explanation of its relevance, in terms of specific theorems about prime numbers" (Bangu 2008: 180). Further, it is question-begging to profess ontological commitments to mathematical objects on the basis of their use in mathematics.[34]

Bangu is correct that assuming an explanandum with a mathematical element weakens the claim that there are mathematical explanations of physical phenomena. But, the charge of circularity is too strong. CP is, as it stands, an explanation of a biological fact which refers to mathematical objects. Bangu's allegation that the mathematical elements of CP1-4 only explain the mathematical portion of CP5 depends on whether he can analyze, or reformulate, CP5 to separate the mathematical portion from the empirical remainder of CP5. If the mathematical elements of CP5 were inseparable, then we could conclude, with the indispensabilist, that there are essentially mathematical elements of our descriptions of physical phenomena. The mathematical explanations of those elements will thus contribute essentially (ineliminably, or indispensably) to our explanations of the phenomenon. If the mathematical elements of the explanandum were truly ineliminable, then we would have reason to believe that the world is essentially as the indispensabilist alleges. In other words, Bangu's claim that the mathematical elements of CP1-4 explain only the mathematical portion of the explanandum begs the question against the indispensabilist of whether the mathematical portion is essential to a description of the phenomenon.

[34] See §2, above.

Bangu's denial of Baker's claim seems plausible since the elementary use of numbers in CP5 is easily excisable. It is only drudgery to remove the reference to seventeen in CP5, no more sophisticated machinery is required than was used to rewrite IM as IN. The concept of primeness requires a bit more machinery. But, as Leng observes, it does not even demand a completed ω-sequence.[35]

Conversely, if we can eliminate the mathematical elements of the explanandum and explananda, recasting CP5 without appeal to numbers, then we can deny that it supports EI1. Bangu's criticism thus replays the dialectic between the indispensabilist and the dispensabilist. If we can construct explanations of physical phenomena which eliminate references to mathematical elements, then there are no essentially mathematical explanations of physical phenomena, and EI1 fails. If we can not reformulate our explanations to eliminate references to mathematical objects, then we have to grant EI1. But the whole point of introducing EI was to avoid precisely this dispute.

Once we recast our explanations to remove references to mathematical objects, as the dispensabilist can try to do with all of ME1-7, we have traded a satisfying (epistemic) explanation for an appeal to an austere, parsimonious theory. Moral 2 of §1 thus reappears: The theory we use to specify our ontological commitments may not be most useful when we want to explain (epistemically) facts about the world. Recasting CP to avoid references to numbers will undermine its explanatory force. Nothing Bangu says weakens the claim that CP, as it stands, is a mathematical explanation of a physical (biological) phenomenon. If he wants to defend nominalism in light of ME1-ME7, his claim should be either that we can reformulate such explanations to eliminate references to mathematical objects, taking a dispensabilist approach to explanations, or that the references to mathematical objects in the explanandum are not to be taken seriously, taking a weaseling approach.

Bangu's criticism of EI1 does not undermine the claim that there are mathematical explanations of physical phenomena, in the sense required by the proponent of EI. If we take 'explanation' in the

---

35 See Leng 2005: 186.

metaphysical sense, Bangu's criticism seems promising, but only against the original QI. If we take 'explanation' in the epistemic sense, as the proponent of EI must, the claims ME1 - ME7 are overwhelmingly plausible.

§9. Why the explanatory argument is no improvement on QI

Proponents of EI presume that their central challenge is to establish that there are mathematical explanations of physical phenomena.[36] Even if not all of the examples work the way that Colyvan and Baker want them to work, taken together with the epistemic interpretation of 'explanation' they provide a compelling case for EI1. The real problem with CP and the other examples is not that they fail to support EI1. The real problem is that we have no reason to take such examples as expressing our ontological commitments. EI2, which claims that we ought to be committed to the objects postulated by mathematical explanations of physical phenomena, is problematic for reasons we have already seen in §1. We need not be ontologically serious when we provide an explanation. Once we realize that the sense of 'explanation' in question is epistemic, any force that EI2 is supposed to have is lost. There is little reason to believe that the explanations which facilitate our subjective understanding are ones in which we reveal our ontological commitments by speaking most soberly. EI seems plausible, if we have a metaphysical sense of 'explanation' in mind; but then it's no improvement on QI.

In essence, my claim is a version of the weaseling strategy promoted by Melia. In a similar vein, Leng argues that the weasel strategy applies just as well to explanations as it does to theories. First, she relies on Melia's claim that mathematics just provides a language for representing, or modeling, physical facts, and that such representations need not be ontologically committing. Then, she argues that Baker's

---

[36] "Baker and Colyvan focus on establishing only [that there are some good scientific explanations that posit mathematical objects], and simply assume that all good explanations must have true explanans, so that if we drop the assumption that the mathematical objects posited by our explanations exist, then these 'explanations' cease to explain at all" (Leng 2005: 180).

explanation is liable to weaseling, just as many other indispensability arguments are.

> To the extent that the sequence of natural numbers provides a good model of the sequence of years since some first, 'overlapping' year, that is to the extent that this sequence of years can be considered as structurally isomorphic to an ω-sequence, then we should expect that facts about what follows from the assumptions of number theory will be relevant to facts about relations between these years. Nothing here requires there actually to be a sequence consisting of 'the' natural numbers, or even that there is any completed ω-sequence. So nothing is lost in the explanation of cicada behavior if we drop the assumption that natural numbers exist (Leng 2005: 186).

Similarly, Leng criticizes ME2 and ME4 by arguing that Melia-type weasel responses are appropriate. "We model the earth as a sphere, and pressure and temperature as continuous functions on the surface of this sphere. Once we have done this, the Borsuk-Ulam theorem can be seen to apply, and, to the extent that are [sic] model is a good one, we can draw a conclusion about the existence of a pair of points on the earth's surface. Does the question of whether the sphere and the functions in our model *really exist* matter to the success of this piece of reasoning? It is hard to see how it should" (Leng 2005: 182).[37]

The difference between my claim and those of Melia and Leng is that I explain the aptness of a weaseling response to EI: we can not expect our explanations to be the locus of our ontological commitments. In contrast, it is reasonable to expect our ontological commitments to be represented by our best theories, in the sense required by QI, since that is their purpose. We constructed IN from IM precisely to be clear about our commitments, in a way that was not required in the original explanation.

Since we should not expect our serious commitments to appear in our explanations, the liability of EI to weaseling does not extend to the original QI. Thus, the nominalist can not use my argument as a general strategy for resisting the indispensability argument. In contrast, Leng derives her response to the

---

[37] Regarding ME4: "As with [ME2], the effectiveness of this explanation can be put down to the correctness of the model as a *representation* of the physical system, and not to the truth of the mathematics involved in this model" (Leng 2005: 184). See Leng's remarks quoted in §3, above, concerning ontological commitments and representations.

explanatory argument from her response to QI. "If the original indispensability argument can be rejected on the grounds that some theoretical components can be good representations without being true (so that 'fictional' assumptions would do the representative work just as well), then the same considerations can be applied in the case of theoretical explanations" (Leng 2005: 187).

Since EI is susceptible to weaseling responses, its defenders EI might re-cast the argument in a Quinean style, including explicit instructions for speaking seriously. Quine's argument, re-cast for explanations, would say that our ontological commitments are to be found in our best explanations.

QEI
- QEI1. We should believe explanations of our sense experience.
- QEI2. If we believe explanations of our sense experience, we must believe in their ontological commitments.
- QEI3. The ontological commitments of any explanation are the objects over which that theory first-order quantifies.
- QEI4. The explanations of our sense experience first-order quantify over mathematical objects.
- QEIC. We should believe that mathematical objects exist.

But QEI is no help at all to the defender of EI, since QEI1-2 are completely implausible. We need only believe our explanations in the sense in which we believe that we can re-cast them in ontologically serious ways. We need not believe the ontological commitments of every explanation, since the chips are just not down in our explanations.

If the indispensabilist is tempted to believe in mathematical objects because of an explanation which uses mathematics, the explanation is not doing the work. What is doing the work is the background claim that there is a good theory supporting that explanation which requires those mathematical objects. Defenders of EI may be relying on a metaphysical notion of explanation in order to motivate the seriousness of our speech, but then switching to an epistemic notion of explanation in order to defend the viability of the claim that there are mathematical explanations of physical phenomena. If there are mathematical explanations of physical phenomena, and we want them to be taken as ontologically committing, then we have to find a way to fit the explanation into a traditional model so that QI applies. The explanatory indispensability argument is no improvement on QI.

§10. Mathematical explanations of physical phenomena

The central argument in this paper is that we speak in ontologically serious tones only in the most austere version of our scientific theory. In a wide range of cases, scientific explanations are better if they include references to mathematical objects. But since our epistemic explanations need not appeal to our most parsimonious theories, we need not take the presence, even the indispensable presence, of mathematical objects in such explanations as ontologically serious.

Still, Colyvan and Baker present a serious case for EI1, the claim that there are mathematical explanations of physical phenomena. Even though that claim can not support the argument for which they use it, even though it does not entail that such explanations are ontologically serious, we need an account of the importance of such explanations. Consider again ME5, the mountaineering example. Colyvan argues that the explanation of the presence of a point on the mountain on which the hiker passes at the same time on consecutive days includes reference to a topological fixed point theorem. The weasel argues that the appeal to that theorem need not be taken as ontologically serious. In siding with the weasel, I argued that such mathematical explanations give us no direct reason to believe in the existence of mathematical objects. We are still left wondering, though, whether we should believe in their existence or not, and whether we can learn anything from the fact that there are satisfying mathematical explanations of physical phenomena.

There are two distinct attitudes that one can take toward such explanations. If one is a nominalist, like Melia and Leng, then since appeals to mathematical objects in such explanations are not ontologically serious, they give us no reason to believe in mathematical objects. They play a representational or modeling role, perhaps akin to idealizations in physics.

Conversely, we could observe that such explanations are more convincing if they do not refer to fictional objects. There is something uncomfortable about the attitude of the weasel, here, despite Melia's assurances that we can take back some portion of our serious assertions. If we had another route to the justification of mathematical objects, then we could have our mathematical explanations, without appealing to fictional objects. This is not the explanatory indispensability argument, that the explanations themselves must be taken as ontologically serious. This is a separate observation that an explanation which refers to fictional objects is less compelling than one which we can take fully literally.

Any platonist account of the application of mathematics in science will support a satisfying picture of mathematical explanations of physical phenomena. Additionally, a fictionalist who can truly establish that mathematics is conservative over nominalist physical theory can also provide a satisfying account of the power of mathematical explanations: they are just a convenient shorthand for more complex theories that do not refer to mathematical objects.

There were two overlapping arguments in this paper. The first is that explanations which increase our understanding may not be those in which we express our ontological commitments. The second is that easy-road, or weasel, nominalism is an appropriate response only to some indispensability arguments, and not to Quine's original argument, which was indeed designed to block such weaseling. Proponents of various alternate versions of the indispensability argument have weakened its ability to block the weasel.

In particular, I have argued that the explanatory indispensability argument provides the platonist with no further ammunition than the traditional Quinean argument, QI. I did not defend QI. In fact, I believe that the problems raised in §5, both with Quine's holism and with the limitations on the platonism that the argument supports, are serious. My claim is simply that QI is resistant to weaseling in ways that other arguments are not. My argument against EI assumes that it is based on an epistemic notion of explanation. I contrasted the epistemic sense of 'explanation' with a metaphysical sense of the term on which QI is based. Perhaps there is an independent sense of 'explanation' on which EI might be based, and on which it would be more successful.

References

Azzouni, Jody. 2004b. *Deflating Existential Consequence: A Case for Nominalism*. New York: Oxford University Press.

Baker, Alan. 2005. "Are there Genuine Mathematical Explanations of Physical Phenomena?" *Mind:* 114: 223-238.

Baker, Alan. 2003. "The Indispensability Argument and Multiple Foundations for Mathematics." *The Philosophical Quarterly* 53.210: 49-67.

Baker, Alan. 2001. "Mathematics, Indispensability and Scientific Practice." *Erkenntnis* 55: 85-116.

Balaguer, Mark. 2008. "Fictionalism in the Philosophy of Mathematics." *The Stanford Encyclopedia of Philosophy* (Fall 2008 Edition), Edward N. Zalta (ed.), URL = <http://plato.stanford.edu/archives/fall2008/entries/fictionalism-mathematics/>.

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

Bangu, Sorin Ioan. 2008. "Inference to the Best Explanation and Mathematical Realism." *Synthese* 160: 13-20.

Bueno, Otávio and Mark Colyvan. Forthcoming. "An Inferential Conception of the Application of Mathematics." *Noûs.*

Burgess, John. 1983. "Why I am Not a Nominalist." *Notre Dame Journal of Formal Logic* 24.1: 93-105.

Burgess, John, and Gideon Rosen. 1997. *A Subject with No Object*. New York: Oxford.

Carnap, Rudolph. 1950. "Empiricism, Semantics, and Ontology." In his *Meaning and Necessity: A Study in Semantics and Modal Logic*, second edition. The University of Chicago Press, 1956; Midway reprint, 1988.

Cartwright, Nancy. 1983. *How the Laws of Physics Lie*. Oxford: Clarendon Press.

Chihara, Charles. 1990. *Constructabiliy and Mathematical Existence.* Oxford: Oxford University Press.

Colyvan, Mark. Forthcoming. "There's No Easy Road to Nominalism." *Mind*.

Colyvan, Mark. 2007. "Mathematical Recreation versus Mathematical Knowledge." In Leng, Mary, Alexander Paseau and Michael Potter, eds, *Mathematical Knowledge*, Oxford University Press, 109-122.

Colyvan, Mark. 2002. "Mathematics and Aesthetic Considerations in Science." *Mind* 111: 69-78.

Colyvan, Mark. 2001. *The Indispensability of Mathematics*. Oxford University Press.

Field, Hartry. 1993. "The Conceptual Contingency of Mathematical Objects." *Mind* 102: 285-99.

Field, Hartry. 1989. *Realism, Mathematics, and Modality*. Oxford: Basil Blackwell.

Field, Hartry. 1980. *Science Without Numbers*. Princeton: Princeton University Press.

Friedman, Michael. 1974. "Explanation and Scientific Understanding." *The Journal of Philosophy*

71.1: 5-19.

Hellman, Geoffrey. 1989. *Mathematics Without Numbers*. New York: Oxford University Press.

Kitcher, Philip. 1981. "Explanatory Unification." *Philosophy of Science* 48.4: 507-531

Leng, Mary. 2005. "Mathematical Explanation." In Cellucci, Carlo and Donald Gillies eds, *Mathematical Reasoning and Heuristics*, King's College Publications, London, 167-189.

Leng, Mary. 2002. "What's Wrong With Indispensability? (Or, The Case for Recreational Mathematics)." *Synthese* 131: 395-417.

Lyon, Aidan and Mark Colyvan. 2007. "The Explanatory Power of Phase Spaces." *Philosophia Mathematica* 16.2: 227-243.

MacBride, Fraser. 1999. "Listening to Fictions: A Study of Fieldian Nominalism." *The British Journal for the Philosophy of Science* 50.3: 431-55.

Maddy, Penelope. 1997. *Naturalism in Mathematics*. Oxford: Clarendon Press.

Maddy, Penelope. 1992. "Indispensability and Practice." *The Journal of Philosophy* 89: 275-289.

Malinas, Gary, and John Bigelow. 2008. "Simpson's Paradox." *The Stanford Encyclopedia of Philosophy* (Winter 2008 Edition), Edward N. Zalta (ed.), URL = <http://plato.stanford.edu/archives/win2008/entries/paradox-simpson/>.

Mancosu, Paolo. 2008. "Explanation in Mathematics." *The Stanford Encyclopedia of Philosophy* (Fall 2008 Edition), Edward N. Zalta (ed.), URL = <http://plato.stanford.edu/archives/fall2008/entries/mathematics-explanation/>.

Mancosu, Paolo. 2001. "Mathematical Explanation: Problems and Prospects." *Topoi* 20: 97-117.

Melia, Joseph. 2002. "Response to Colyvan." *Mind* 111: 75-79.

Melia, Joseph. 2000. "Weaseling Away the Indispensability Argument." *Mind* 109: 455-479.

Melia, Joseph. 1998. "Field's Programme: Some Interference." *Analysis* 58.2: 63-71.

Pincock, Christopher. 2004b. "A New Perspective on the Problem of Applying Mathematics." *Philosophia Mathematica* 12.2: 135-161.

Pincock, Christopher. 2004a. "A Revealing Flaw in Colyvan's Indispensability Argument." *Philosophy of Science* 71: 61-79.

Potter, Michael. 2007. "What is the Problem of Mathematical Knowledge?" In *Mathematical Knowledge,* Mary Leng, Alexander Paseau, and Michael Potter, eds., Oxford University Press.

Putnam, Hilary. 1975. "What is Mathematical Truth?" In his *Mathematics, Matter, and Method: Philosophical Papers, Vol. I*. Cambridge: Cambridge University Press.

Putnam, Hilary. 1971. *Philosophy of Logic*. In his *Mathematics, Matter, and Method: Philosophical Papers, Vol. I*. Cambridge: Cambridge University Press.

Putnam, Hilary. 1962. "The Analytic and the Synthetic." In his *Mathematics, Matter, and Method: Philosophical Papers, Vol. II*. Cambridge: Cambridge University Press.

Quine, W.V. 1986. "Reply to Charles Parsons." In Hahn, Lewis Edwin and Paul Arthur Schilpp, eds. *The Philosophy of W.V. Quine*. La Salle: Open Court, 1986.

Quine, W.V. 1978. "Success and the Limits of Mathematization." In his *Theories and Things*. Cambridge: Harvard University Press, 1981.

Quine, W.V. 1960. *Word & Object*. Cambridge: The MIT Press.

Quine, W.V. 1958. "Speaking of Objects." In his *Ontological Relativity and Other Essays*. New York: Columbia, 1969.

Quine, W.V. 1955. "Posits and Reality." In his *The Ways of Paradox*. Cambridge: Harvard University Press, 1976.

Quine, W.V. 1951. "Two Dogmas of Empiricism." In his *From a Logical Point of View*. Cambridge: Harvard University Press, 1980.

Quine, W.V. 1948. "On What There Is." In his *From a Logical Point of View*. Cambridge: Harvard University Press, 1980.

Quine, W.V. 1939. "A Logistical Approach to the Ontological Problem." In his *The Ways of Paradox*. Cambridge: Harvard University Press, 1976.

Railton, Peter. 1978. "A Deductive-Nomological Model of Probabilistic Explanation." *Philosophy of Science* 45: 206-226.

Resnik, Michael. 1997. *Mathematics as a Science of Patterns*. Oxford: Oxford University Press.

Resnik, Michael. 1993. "A Naturalized Epistemology for a Platonist Mathematical Ontology." In, *Math Worlds: Philosophical and Social Studies of Mathematics and Mathematics Education*. Sal Restivo, et. al., eds. Albany: SUNY Press, 1993. Originally appeared in *Philosophica*.

Sober, Elliott. 1993. "Mathematics and Indispensability." *The Philosophical Review* 102: 35-57.

Van Fraassen, Bas. 1980. *The Scientific Image*. Oxford: Clarenden Press.

Van Fraassen, Bas. 1977. "The Pragmatics of Explanation." In Richard Boyd, Philip Gaspar and J.D. Trout, eds., *The Philosophy of Science*, Cambridge: The MIT Press, 1991.

Woodward, James. "Scientific Explanation." *The Stanford Encyclopedia of Philosophy* (Fall 2009 Edition), Edward N. Zalta (ed.), forthcoming URL = <http://plato.stanford.edu/archives/fall2009/entries/scientific-explanation/>.

Yablo, Stephen. 2005. "The Myth of the Seven." In *Fictionalism in Metaphysics*, M. Kalderon, ed., New York: Oxford University Press.