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Book Review

Michael Resnik. *Mathematics as a Science of Patterns*. Oxford University Press, Oxford, 1998. xiii + 285 pages.

1 Introduction In this ambitious and engaging book Michael Resnik weaves together the various strands of his philosophy of mathematics. These strands have been developed in thirteen papers published between 1981 and the present. The book goes beyond the papers, however, in that it synthesizes the material and strengthens it. In addition, there is some previously unpublished material on realism—which is really the main focus of the book. The writing is very clear and accessible, while raising many important philosophical questions about the nature of mathematical reality, truth, reference, and epistemology. The book is thus appropriate as a textbook for a philosophy of mathematics course while also being very entertaining reading for professionals.

Though its title may give the impression that the book elaborates and develops Resnik's well-known structuralism, it really constitutes an extended argument for mathematical realism. The chapters on structuralism which come at the end complete the ontological picture, but they are largely irrelevant to the main arguments for realism. Here is a sample of the wide variety of views which make up the philosophical position espoused in the book. Resnik proposes a modified version of the indispensability arguments for mathematical realism. The objects about which he is a realist are positions in structures (structuralism). He bolsters his indispensability arguments with negative arguments against certain competing antirealist programs. He appeals to naturalism as a reason we should avoid "supernatural" epistemologies, such as those based on a priori intuition. He endorses an immanent, disquotational theory of truth, an immanent theory of reference, and he argues that these are compatible with realism. Acknowledging that epistemology is the biggest obstacle for the realist, Resnik argues for a postulational epistemology and an evidential holism about mathematics and science.[1] The idea here is that though there appear to be methodological differences between mathematics and the natural sciences, there is no genuine separation to be made, either ontologically or epistemologically. Mathematics is empirical, not a priori; and the abstract-concrete dichotomy—on which certain versions of apriorism depend—is argued to be indistinct. The ultimate evidence for mathematics comes from its role in science. Finally Resnik is an antirealist about logic. Though logic is the part of our system of beliefs which is least susceptible to revision, this is because of matters of convenience and not logical facts.

That Resnik manages to get so many different themes into one book is impressive. Each of his theses has something going for it. The questions on which I will focus in this essay are whether the central arguments are strong enough to establish his theses, and whether when put together the views comprise a consistent and coherent philosophical position.

At first glance, there are several jarring combinations of ideas in this book. Mathematical realism and logical antirealism is one. Immanent theories of truth and reference also seem at odds with realism. For the purposes of this paper, however, I will focus on Resnik's realism about mathematical objects and truth combined with his epistemological holism. Two questions need distinguishing. First, do Resnik's arguments result in a unified position? Second, is it even possible for mathematical realism to be supported by holism? The second question invokes the legacy of Quine, whom Resnik cites as the progenitor of the position he is defending. Though no Quine expert I will attempt to touch on the second, more general, question as well as the first.

2 Mathematical realism Realism involves views about existence, the (approximate) truth of current theory, and the (relative) independence of both of these within a certain domain. Depending on the domain in question, being a realist requires a commitment to at least one and at most three of these views (pp. 12–13). Resnik makes a good case for thinking that, in contrast with realism about certain other domains, mathematical realism requires all three. So mathematical realism is the view that mathematical objects exist; much of current mathematics is true; and both the existence of mathematical objects and the truth of our current mathematical theories are independent of "our beliefs, theories, and proofs" (p. 4). In addition, Resnik endorses the view that the mathematical objects are causally inert and outside space and time, (p. 82) or as he sometimes says, abstract (p. 4). Thus his version of realism is a kind of platonism.[2]

Though he mentions some other arguments for mathematical realism, Resnik believes that indispensability considerations provide the strongest case for realism. So this is the strategy on which he focuses to try to establish his realism. The type of argument Resnik has in mind descends from Quine and Putnam and concerns the necessity of mathematical realism to account for the success of applied mathematics in science. In one sense appeals to realism to explain applied mathematics are rather odd. On the face of it, and not presuming a Platonic account of "participation", traditional mathematical realism, or platonism, makes a mystery of applied mathematics. Why should a realm of independently existing abstract objects have anything to say about concrete, spatiotemporal objects? Unless one builds a theory of logical necessity into mathematical realism, a la Frege, the so-called explanation of mathematical realism to the question of applied mathematics makes little immediate sense.

Indispensability arguments, however, focus more on the bare question of what commitments science makes to truth, and thereby ontology, in mathematics. They appear to be intentionally agnostic about any metaphysical mechanism for explaining the relationship between the mathematical and the concrete realms. As represented here, the thought behind indispensability is that the truth of (much of classical) mathematics is required for science. If we add to this the naturalistic view that "science is our ultimate arbiter of truth and existence" (p. 45) plus confirmational holism, we appear to end up with the conclusion that much of classical mathematics is true. Science tells us what truths there are, and it presupposes mathematics. It does not merely presuppose the formalism of mathematics, it presupposes the truth of its principles—which in turn entails the existence of the mathematical objects involved. For example, as Resnik explains it, acceleration, velocity, and the welfare function are all defined in terms of real numbers. So, the indispensability inference goes, real numbers must exist in order for these functions to be well defined (p. 43).

There are several lines of thought that appear weak in these indispensability considerations. One obvious problem is that history teaches us that most scientific theories are not strictly true. By modus tollens at least some of the premises or presuppositions of science are not true either. Thus, if indispensability or holistic considerations are what ultimately support mathematics, then whatever lack of confidence we have in science should in principle trickle back to the mathematics presupposed. So if we should not have confidence that the scientific theories are true, neither should we have confidence that the mathematics presupposed by them is true.

Resnik's main worry about the typical indispensability arguments is precisely this standard one—that they seem to make mathematical truth and existence depend on scientific truth. So the inference to the truth of current mathematics is blocked by the historical, inductive observation. This version of indispensability also jars with the confidence we have in the background mathematics, and with the fact that we don't tend to revise mathematics when evidence does not agree with a scientific prediction. Mathematical truth is somewhat independent of scientific truth.

Resnik's new version of the indispensability argument allows mathematical truth to be independent of scientific truth without it being independent of science. His thesis is that "whatever attitude scientists take towards their own theories, they cannot consistently regard the mathematics they use as merely of instrumental value" (p. 46). So even for scientific antirealists, and even if scientists distinguish between the merely useful and the true parts of their natural science theories, the way they use mathematics commits them to its truth. Resnik's "pragmatic indispensability argument" thus focuses on the activity of science rather than its truth. His claim is that the *activity* of science requires the *truth* of mathematics.

More fully his argument has three parts which I quote:

1. In stating its laws and conducting its derivations science assumes the existence of many mathematical objects and the truth of much of mathematics.
2. 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.
3. 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. (pp. 46–7)

This is a clever move which avoids the standard worries about scientific truth. But it does not avoid all of the problems of indispensability arguments; plus it generates some problems of its own.

Clause 1 seems innocuous, so I will not address it. Clause 2 contains two claims which I will treat separately. That science could not be pursued without certain assumptions does not entail that the assumptions made are actually true or that science could not be pursued if those assumptions were false. It just means I have to make certain assumptions, which may be true or false, in order to do science. The second half of clause 2 appears slightly different but I don't think it is. It appears to argue that in order to draw many scientific conclusions the underlying mathematical assumptions must actually be true. But it is really the same, more innocuous, claim that the underlying mathematical assumptions must be *taken*, or treated, as true. And this just means assumed. So far we don't have an argument for the conclusion that the mathematics presupposed by science must in fact be true, which is what we need for the conclusion of mathematical realism.

Clause 3 makes an inference from (1) and (2) about justification. It says: if we are justified in using mathematics to draw scientific conclusions, then we are justified in taking this mathematics to be true. Resnik appears to take the antecedent of the conditional for granted, as he does not argue for it explicitly. It may be implied by his naturalism, and I will not question it. Whether the conditional is true, and thus whether the consequent is shown true by this argument, depends on what is meant by "justified" in this context. Again, a more innocuous reading allows me to be "justified in taking" a belief used in science to be true without it being (literally) true. Sometimes this is the case, for example, idealization assumptions and *ceterus paribus* clauses are not usually literally true. Yet I think we are justified in taking them to be true for the purpose of making certain scientific inferences. Why should the same not hold for mathematics? That its assumptions are a necessary tool for science, including making inferences, does not entail that those assumptions are true—even if we are justified in making these assumptions and even if the scientific conclusions drawn from them are true. As we all know, it is possible to draw true conclusions from false premises.[3]

Realism may be the best attitude for natural scientists to take toward the mathematics they use, and perhaps the indispensability arguments show this. But this does not mean that realism is *true*. What is needed for the conclusion of mathematical realism is that science could not be pursued, and scientific conclusions could not be drawn, if the mathematical assumptions were not in fact (literally) true. But this is not actually argued for. Furthermore, to complete such an argument would require additional arguments against alternative, antirealist construals of mathematics. This is a problem Resnik's "pragmatic" version shares with traditional indispensability arguments. Antirealists do not, in general, deny the existence of mathematical objects and the truth of much of mathematics. They just want to limit these claims and/or reinterpret them. So, for example, even if real numbers are needed for defining acceleration (p. 43), the question is, Does this justify the *independence* clause of realism, and that we need *all* of the real numbers of standard, classical analysis?

The thought that it does relies on an additional premise that alternative accounts are insufficient for science and Resnik devotes much space to undermining antirealist challenges to realism. While I wish to focus on his positive arguments and positions, I will say that I did not find his negative arguments against antirealism altogether convincing. First, he does not attempt to give a general argument against antirealism and restricts his attention to several recent writers (Chihara, Field, Hellman, and Kitcher) who have contested the indispensability thesis. This leaves a whole range of constructivist positions undiscussed.[4]

Second, a major thought which Resnik deploys against most of the philosophers he criticizes (Field, Kitcher, and Hellman, but not Chihara) is that science requires statistics and probabilities. But, he claims, these antirealists "cannot talk about probabilities because they [probabilities] are numbers" (p. 74). While not sure that it is even accurate to say that probabilities *are* numbers, rather than expressed in terms of numbers, I am more troubled by Resnik's claim that such antirealists cannot "talk about" numbers. Though Field and Hellman argue that it is possible to *do* science or mathematics "without numbers", I take this to mean without the metaphysical commitment to the independent existence of numbers. The whole point of such programs is not to reject talk in science and mathematics but to reinterpret it. So I don't see why they can't "talk about" numbers provided there is a way of accounting for such talk given by their reconstructions. Now Resnik denies that they have a way of getting around this problem without presupposing other sorts of abstract objects and I am very sympathetic to this criticism. It is unclear what is ontologically or epistemically gained by rejecting talk of numbers but accepting talk of space-time points or logical possibilities. But I find the charge that proponents of these programs simply cannot "talk about" numbers to be too fast and easy.

In any case, Resnik's main arguments against the antirealists in question support clause (i) of realism. Whereas in my view the interesting antirealist programs don't generally deny clause (i). Characteristic of realism is the independence of the mathematical objects not their bare existence. So what is really needed here is a defense of clause (iii), the independence clause.

The puzzling part of this and some other indispensability arguments is the following move. The fact that we can calculate something scientifically useful, for example, the welfare function, is taken to support the claim that what is used in our calculation must exist independently of the scientific calculation, the background scientific theory, and the mathematical theory. Such arguments are often given in terms of the range of certain bound variables. But how much can we really tell about the range of a mathematical variable from its use in science? That science uses these domains uncritically does not seem to require that they be understood classically.[5] Reference to a particular object, no less a domain, does not entail its *independent* existence. For example, at my institution the average salary at a particular rank is used to determine the basic cost of living raise for all at that rank. This is a useful way to calculate raises, and raises are a good, useful thing. But, of course, this does not mean that anyone actually receives the average salary in any rank. Resnik himself suggests another example. By using the fiction of a single planet and a fixed star, Newton was able to calculate a basic model for planetary orbits. So scientifically useful facts—Newton's explanation of the approximate orbits of the planets—can follow from a fiction—the fiction of a single planet and a fixed star (p. 44).

Both Resnik's positive indispensability argument, and his negative arguments against antirealist alternatives, are original and deserve further analysis. But in my view they harbor some weaknesses which significantly undermine his case for mathematical realism. I turn next to his arguments for evidential holism. Then I will examine whether a holistic approach to the epistemology of mathematics coheres with the mathematical realism to which Resnik is committed.

3 Epistemological holism Resnik's epistemology is postulational and holistic, where "[t]o postulate something is simply to make up a theory that asserts that it exists" (p. 99). In mathematics this can take the form of introducing a new predicate and claiming that it has instances (p. 175). Resnik acknowledges that there is a strong analogy between positing and writing fiction, but there is also one crucial difference: mathematical objects exist and fictional characters do not (pp. 184–5). Since positing is not creating on Resnik's realistic account, we still need a link between the posits and the independently existing mathematical objects. In other words, positing provides an account, one with some insight, of how we come to articulate mathematical theories and form mathematical *beliefs*. But it does not provide a *justification* for those beliefs. So in some ways it seems more like a psychological-historical account than a proper epistemology. For this, Resnik turns to holism.

Holism claims that we have reason to think that our posits have "hit" on the real mathematical objects and predicates when the mathematics involving them proves scientifically fruitful (p. 188). So what grounds are there for thinking that this is right—for thinking that mathematical truth is ultimately to be judged in terms of its usefulness to science? Resnik devotes Chapters 6, 7, and 8 to answering this question; and these form some of the most central arguments in the book.

There are many issues raised in these chapters. I will consider here what I take to be Resnik's two main arguments for holism. One is an argument against the abstract/concrete distinction; the other involves an application of Duhemian holism to mathematics.

3.1 First argument The first main argument for holism is that the abstract/concrete distinction is not sharp, and so it cannot be the basis for epistemological separatism between mathematics and science. That the abstract/concrete distinction is blunt seems correct. Though there are clear cases on either side—for example, the empty set is clearly abstract and the chair I am currently sitting on is definitely concrete—there are vague, intermediary cases, such as quantum fields, which seem to be neither clearly concrete nor clearly abstract. The existence of these latter, according to Resnik, undermines any epistemological distinction built on this blunt ontological difference.

It is important to note that Resnik's argument is targeted only against those antirealists who are both realist about theoretical physics and who invoke the abstract-concrete distinction in their "typical" arguments for mathematical antirealism. However, since Resnik believes that this holds for "[m]ost antirealists in recent philosophy of mathematics" (p. 102), his argument should apply to most of his competitors. So if it is true that most antirealists about mathematics are realist about theoretical physics, and if Resnik is correct that the abstract/concrete distinction is the typical reason antirealists believe there to be an epistemic difference between science and mathematics, and if it is true that this is a vague distinction, and if one cannot base an epistemic difference on a distinction with vague boundaries, then mathematical apriorism and separatism are undermined.

Let us consider these four antecedents. First, I am uncomfortable with Resnik's claim that most antirealists about mathematics are realist about quantum particles. For the very reasons he so effectively gives, this seems an unstable view. One might believe that the objects studied in the natural sciences are generally concrete, and that this does distinguish mathematics from natural science, without making such a commitment to quantum particles and other strongly unobservable objects of current theoretical physics.

Second, I think Resnik is correct that our common feeling that mathematics is different from natural science is at least partly derived from the view that the objects studied are different. Physical objects seem, well, physical. They are in space and time, subject to laws of causality, or in other words, concrete. I can bump into them. Even the ones I don't sense bumping into are investigated by means of machines that I can sense bumping into. In contrast, mathematical objects are isolated from causal interactions with physical objects and processes and they are certainly not located in space and time. I can't bump into any of them. However, is this the typical reason we regard mathematics as a different discipline from the natural sciences? Or is it because the methodology, the justification of its inferences, is so different? It is simply a brute fact that calculation and logical deduction have a very different epistemic (and phenomenological) character from observation and experiment. It is simply not clear to me that the typical reason separatists consider mathematics to be distinct from natural science is the disputed ontological distinction, since there are also important methodological and epistemic differences. Even if ontology is the main motivation, we have two more antecedents to examine.

Third, I think Resnik is right that the abstract-concrete distinction has a vague boundary. His arguments here are clear and effective in making his point. But, fourth, this does not automatically undermine mathematical apriorism, for it does not automatically undermine the existence of an epistemic difference between mathematics and natural science. What sorts of objects straddle the border according to Resnik? A few objects from contemporary quantum physics. It is important to note that none of the intermediary cases cited are mathematical. Furthermore, when Resnik talks about these intermediate cases he refers to them as "straddl[ing] the border between mathematics and physics" (p. 104), thus invoking the very distinction he is trying to undermine.

In my view, the intermediate cases from physics do not show that it is wrong to think of mathematical objects as abstract or at least as distinct from most physical objects. As mentioned above, Resnik himself admits that mathematical objects are a-causal and not in space and time (p. 82). What they show, if anything,[6] is that though most objects studied in the natural sciences are concrete (p. 101), some of the theoretical constructions of physics currently lie on the boundary between mathematics and natural science. What does this view of physics tell us about mathematics? Nothing. It may tell us something about the epistemology of (certain areas of) physics but it says nothing about mathematics.

I wish to stress that I agree with Resnik's main point in this argument.[7] Mathematics and science are not as disconnected as we philosophers might tend to assume. The epistemology of science, realistically construed, is not unproblematic as contrasted with that of mathematics; not all of the objects of physics are clearly concrete, and so on. But I don't think that the analogies and connections between the disciplines refute mathematical apriorism. In short, I think that there are real, and useful, distinctions which have vague, intermediary cases.[8] Since, for our border, none of the vague cases are mathematical, their existence does not undermine the classification of mathematical objects as abstract. In my view, the abstract/concrete distinction is useful to underpin the difference between the disciplines of mathematics and science despite the existence of cases from physics which do not clearly, or at present, lie on the purely concrete side.

3.2 Second argument Resnik's second main argument for holism is based on the famous point associated with Duhem, Poincaré, and Quine, that it is impossible to test a scientific hypothesis in isolation. Among the auxiliary hypotheses are often mathematical ones. So mathematics is part of the whole scientific network which gets supported by positive empirical testing. Resnik believes that this is a purely logical point for holism which means that the burden of proof is on the side of separatism (p. 120).

Some important counterarguments are considered in these chapters, but before looking at how these are handled I want to raise the question whether the two arguments for holism are really analogous. Duhem's point was that in order for a hypothesis to yield a testable consequence, other premises need to be added. These premises take the form of background scientific theory, auxiliary hypotheses, initial conditions, and so on. For physics, in particular, mathematics is an important component of the background scientific theory. Poincaré's famous arguments for the conventionality of geometry spring from this common logical insight. The possibility of testing the geometry of physical space depends on adding background physical assumptions such as that light always travels in straight lines, or along geodesics. The point in favor of conventionalism is that some features of the overall theory must be fixed in order for another part to be confirmed or disconfirmed by the experimental outcome. And, with Duhem, which part is taken as fixed and which part is regarded as tested is partly a matter of convenience since it is not determined by logic.

Does this mean that mathematics and science are on an evidential par? I don't think so and here is why. Physics may (sometimes) require mathematics for the deduction of particular predictions, or consequences. But mathematics does not require physics, or any other empirical hypotheses, for the deduction of its particular consequences. The deduction of mathematical consequences from mathematical premises just requires logic. These consequences are the theorems of a mathematical system. The mere inclusion of mathematics among auxiliary hypotheses in scientific testing does not entail evidential scientific holism for *mathematics* any more than the inclusion of observation sentences among initial conditions in deducing a testable scientific outcome makes the evidence for observation sentences holistic. Unless we require that the mathematical consequences be empirically testable, mathematics does not need science for the deduction of its consequences. But requiring that the mathematical consequences be empirically testable would be to presuppose mathematical empiricism and holism, rather than to argue for them. I conclude that Duhem's argument for scientific holism does not automatically show anything about mathematics. The two cases do not share the same "logical structure."

Resnik goes on, however, to give additional grounds for thinking that the analogy does apply. These occur in the context of some more direct arguments for mathematical empiricism. He first wants to detach the mathematical concept of computation from the idea of computation as a physical process (p. 149). Then he argues that mathematical inferences depend upon the reliability of physical computations—those we have done ourselves and/or those of others. This is especially the case with complicated proof-sketches where other proofs, computations, and so on, are cited (p. 154).

> [I]nstead of following the commonplace pattern of using mathematical and physical premisses to draw empirical conclusions, these examples use mathematical and empirical premisses to draw *mathematical* conclusions. (p. 150)

If this is so then Resnik has a case for the analogous Duhemian situation in mathematics.

Two questions, however, undermine this case. First, is computation an empirical justification just because it takes place in or on a physical medium—*in* a computer or a brain, or *on* paper? Second, when we cite lemmas are we invoking an empirical auxiliary hypothesis about the reliability of a particular physical instantiation of the proof of the lemma? I don't want to belabor these points, but I think the answer to both questions is "no". Frege already answered the first. The fact that our brains are physical, and require a certain blood flow, should not automatically entail that anything known via them is empirical. Resnik regards his argument as showing mathematics to be empirical, but his way of implicitly characterizing a priori knowledge essentially rules all of it out. It is an uninteresting way of arguing that mathematics is empirical. (One can find the same move in Kitcher [3] whom Resnik cites approvingly on this matter.)[9] The second question is answered similarly. When we cite a prior proof we are relying on its logical correctness and not on some empirical hypothesis about a physical process. This strategy for extending Duhem's argument to mathematics does not strike me as working unless one already accepts the empirical nature of mathematics and the nonexistence of significant a priori knowledge.

3.3 Objections to holism The main objection to holism considered by Resnik is that it does not ring true to the actual practice of mathematics. Mathematics has its own internal, or "local", methods. It does not treat theorems, or even axiom systems, as empirically testable. This is a serious problem for anyone who endorses both naturalism and holism.

Resnik has a multifaceted response to this objection, including the following. (i) Weak, as opposed to strict, evidential holism can accommodate the practice of local standards of mathematical evidence (p. 119). (ii) There is a hierarchy of sciences with mathematics as the most global (then physics, then chemistry, etc.); and "practical rationality" tells us that the least disruption in the whole is best (p. 125). This, and not some logical or ontological difference, is what underpins the practice that physicists don't generally question the mathematics they presuppose just as biologists don't generally question any background physics they use (p. 129). (iii) So empirical success or failure may not usually be taken as confirming or disconfirming individual mathematical claims—for reasons of "practical rationality"—but it does provide a justification for the general mathematical practice (p. 129). (iv) Though mathematicians appear to use deduction differently from scientists—who must test their consequences *in addition* to deriving them—this does not mean that the premises (axioms) have a different character from scientific hypotheses (p. 139). I will briefly consider each of these responses.

First, if there are local standards of evidence, and it is by these that mathematicians operate, then given Resnik's naturalism this should count against evidential holism in mathematics. Perhaps weak holism is so weakened—so as to accommodate all separatist intuitions (p. 131)—that it isn't really evidential holism. (What other sort of holism it might be is discussed below.) Second, the appeal to 'practical rationality', though intuitively appealing, is as unilluminating here as it is in Quine. Resnik invokes the virtue of preserving more truths in cases such as these but this appears to beg the very question that we want answered. What we want is a justification for the practice of minimizing mutilation, for example, by "Euclidean rescues".[10] Certainly the least disruption preserves more beliefs, or truth-contenders, but why should we believe that it preserves more truths?

The third and fourth responses are more serious and can be considered together. The ideas that though individual bits of mathematics are not confirmed empirically the general practice of mathematics is, and that though mathematics appears to use deduction differently from science this does not mean that the premises have a different status, involve analogous problematic maneuvers. They both move from a question about how mathematicians, in fact, justify, or provide evidence for, individual mathematical claims such as theorems, to a response about general mathematical practice and axiom systems as a whole. But, of course, this move does not answer the original question. It changes the topic.

There are really three topics here which need to be distinguished. (1) How are individual mathematical claims justified? (2) How are axioms and axiom systems justified? (3) How is the "general practice" of mathematics justified? I think it is fair to say that most of the research activity of professional mathematicians is activity of type (1): proving new theorems from accepted axioms. A mathematical "result" is a new theorem. To show that evidential holism applies here Resnik must show not only that confirmation from empirical evidence trickles back to these individual mathematical results—a fact which he believes is given automatically by the "logic" of the situation. He must also show that individual mathematical results can be *dis*confirmed by empirical evidence. The problem is, he does not find one actual case of this happening in the history of mathematics. Cases where the mathematics is relatively undeveloped, or where the physical evidence prompts a re-examination of the mathematical arguments, are not to the point (as he admits). An example to the point would be one where there is no internal, logical error suspected in the mathematical argument, there are no unclarities in the mathematical concepts involved, and yet the empirical evidence induces a rejection of the theorem as a theorem of mathematics. This is different from rejecting the theorem as merely not applying to the physical situation, that is, making a "Euclidean rescue", which is generally the more sensible thing to do. Since there are no such examples, Resnik makes one up with a thought experiment, admitting that even to imagine it happening is very difficult.

> Would it be rational for the mathematicians to see the experiment as refuting ZFC$+A$? According to our story, it implies a claim that when combined

> with physical hypotheses forms a package contrary to the experimental results. *Stretching our imaginations to near the breaking point*, we can think of the mathematicians as arguing as follows: some statement in the package must be false, and the physical ones are beyond question, so the mathematical statement must be false; but since this is implied by ZFC+A it must be false too. (p. 134, emphasis added)

One problem is that though this perhaps could be how mathematics is done, it isn't. So imposing this picture on mathematical evidence is not only counterintuitive, it seems in tension with Resnik's naturalism.[11]

Second, it is not so clear that this could be how mathematics is done. For to Resnik, rejecting a part of mathematics as false in this context (as opposed to merely not applying) is to reject it as *inconsistent*. The quote above continues:

> But notice how strong a claim this would be. Because one can always save a consistent branch of mathematics via a Euclidean rescue (and we have assumed our mathematicians have excluded this), for them to reject the axioms of ZFC+A would be to take them to be inconsistent. (p. 134)

Notice what an odd situation this would be. We would be taking events as they are in the physical world—contingent truths—and drawing necessary, or at least logical, conclusions from them. Perhaps my imagination is simply not elastic enough, but Resnik's example does not seem to plausibly show anything about the nature of evidence for mathematical theorems.[12]

Duhem's original argument and Poincaré's famous example of applied geometry make holism more than a mere logical possibility. In order for a practice to exhibit evidential holism, confirmation and disconfirmation must actually affect a larger body of knowledge than the hypothesis under focus. Poincaré's point about applied geometry was exactly this: given parallax, we have a real choice. We can fix the assumption that physical space is Euclidean and infer that light bends near large gravitational objects; or we can fix the assumption that light travels in "straight" lines and infer that physical space is non-Euclidean. This was a real choice in a real situation. There are no similar examples of pure mathematics being so affected by an empirical result. Resnik's thought experiment about ZFC+A is implausible for it appears to conflict with actual practice. The "choice" to reject mathematics because of an empirical disconfirmation has not been shown to be a "real" rational choice. Thus, it is not at all clear from the mere "logic" of the situation that scientific, empirical evidence bears on individual mathematical theorems (or via theorems on axiom systems).

The second question, of how axioms and axiom systems are accepted or rejected, is a completely different matter from that of theorems within particular, developed, axiom systems. This is why the two cases need to be kept separate. Here the method is not straightforward deduction and the evidence might well be, at least in part, holistic. But the holism, I would argue, rarely extends beyond mathematics and into empirical science. One mathematically holistic consideration would be to take into account how well the axiom, or axiom system, connects with and improves other areas of mathematics. This sort of holism does not undermine mathematical apriorism, or separatism. One nonholistic consideration would be to "test" the axiom(s) by deriving some results and seeing whether these accord with the intuitions and intentions of the mathematical community. Another consideration—a necessary one is, of course, logical consistency. The main point is that this is a very different sort of case from the issue of how mathematicians justify individual theorems. So one cannot answer a question about the justification of theorems by talking about that of axioms.

The third question, of how to justify the "general practice" of mathematics, is rather peculiar and even further removed from ordinary mathematical activity. Suppose we wanted to justify the general practice of science. We would not expect the answer to come from within science itself. Answers such as, "science is the search for knowledge and knowledge is a good thing," or "science leads to technological innovations which are useful," are not *scientific* answers. For they do not employ the methodology of science. In order to answer such a question one must step outside of the discipline in question and consider its worth or utility as a whole. The reason a question about the general practice of science cannot be answered from within science, then, is that it is not a proper scientific question.

Similarly, we should not expect to be able to justify the general practice of mathematics from within mathematics by its "local" standards of evidence. And this is because the question of the status of the general practice of mathematics is not a mathematical question. So that we must appeal to broader considerations that lie outside of mathematics in order to justify its general practice does not show that the justification of mathematical claims is holistic and involves natural science. I agree that there is a form of holism here which Resnik has perceived. However, what this holism justifies is not mathematical claims but philosophical claims about mathematics. So it does not support Resnik's view that scientific holism applies to evidence *in* mathematics.

To put it another way, the idea of its utility to science as a way of justifying the general practice of mathematics is something far removed from the justification of mathematical claims. It might well be that its utility to science is (part of) why we have mathematics around. But this *justification for mathematics* should not be associated with a *mathematical justification*. So while I will not dispute that mathematics is useful to science, and that this (in part) may justify its existence and role in our general body of knowledge, this does not confirm that scientific holism applies to—or is evidential for—mathematical claims. Scientific holism may contribute to justifying the general practice of mathematics, but justifying the general practice of mathematics is not on a par with justifying the ordinary claims of mathematics. So this reason for appealing to science does not imply evidential holism for mathematics, though it may imply a kind of pragmatic holism.[13]

In sum, I do not buy these arguments that scientific holism applies to the ordinary propositions of mathematics. I think that there are good *philosophical* reasons for a mathematician to consider the role of mathematics in science before espousing revisionary foundations for mathematics such as intuitionism or finitism. In this sense mathematicians can be holistic when they wear their philosophical "hats". When considering the logic of mathematics, for example, there may even be a blurry line between philosophical/pragmatic and mathematical/pragmatic reasons. Perhaps this shows that at their disciplinary edges mathematics and science invoke the tools of philosophy. There are philosophical issues, just as there are scientific issues, relevant to the *practice* of mathematics. But for the most part mathematical questions are answered within mathematics and not in science or philosophy. I will now turn to the final question of whether extending scientific holism to mathematics can plausibly and coherently be conjoined with Resnik's mathematical realism.

4 Holism and realism The main question is now the following. Supposing that we accept the initial arguments for holism and realism—do the two positions cohere? Let us begin by comparing our case with that of someone who is both a realist and a holist about science.

For the analogous scientific realist, the objects of science exist independently of us (our beliefs, theories, and proofs). But these objects exist in a causal network with the other objects of science, including ourselves. Even theoretical objects are at least causally connected to objects and processes which are causal and in space and time. So manipulation of the world by us, plus observation of what happens as a result, creates a connection between the scientific facts and ourselves. It is this connection which accounts for our supposed knowledge of the truths of science for the realist.

The main problem for the combination of holism and realism about mathematics concerns the apparent absence of the link outlined above. The question is how to explain why causal processes conducted, or observed, by causal beings (us) leads to knowledge about noncausal objects? *Pace* Resnik's disputation of the abstract-concrete distinction, he does accept that mathematical objects are acausal and outside of space and time. Now, I agree with Resnik that we should not presume a causal theory of knowledge.[14] But this does not help to answer the basic question of why certain forms of empirical evidence should bear on mathematical claims. Why should causal interaction between us and the physical world tell us anything about the acausal, mathematical world? This is especially acute owing to the independence clause of Resnik's realism. The mathematical objects and truths, remember, are independent of our beliefs, proofs and theories—including our scientific theories.

Resnik's holism goes along with his empiricism and one famous source of mathematical empiricism comes from J. S. Mill. For Mill, mathematical knowledge is empirical because mathematical claims are disguised claims about the empirical world. On this view '$2 + 2 = 4$' is really about combining apples, oranges, and the like. Whereas Resnik thinks mathematical knowledge is empirical, he does not think that mathematics is *about* the empirical world. *Qua* mathematical realist and platonist, Resnik thinks mathematics is about the independently existing *mathematical* objects. And while there may be a fuzzy ontological boundary between the mathematical and the physical, there is still a difference to which he is committed. Mathematical objects, for Resnik, are acausal, nonspatiotemporal (p. 82), featureless positions in structures (p. 201). Physical objects are not, in general, so featureless. Resnik's empiricism is thus not of the Millian variety. *Qua* realist, the subject matter of mathematics is the *mathematical* independently existing objects, not the *scientific* independently existing objects—whichever ones there are. (Recall that Resnik thinks that even most scientific antirealists must be mathematical realists.)

Now, for Resnik, mathematical objects are positions in structures and at least some structures can be instantiated in the physical world. In other words, physical objects can be positioned in structures, some of which are related to mathematical structures. For example, the positioning of spectators at a baseball game may not have any interesting mathematical properties. But their position prior to the game may: the sequence of people in line to get in should instantiate the natural number sequence up to, but not over, the number of seats available in the park. So information from the physical world—patterns—can tell us about the physically instantiated mathematical structures, and thus about mathematics. But there are many more mathematical structures than concrete ones and none of the infinite mathematical structures can be empirically investigated in any straightforward way. So the empirical information is relatively limited to be of much use in telling us about the mathematical realm. The empirical, finite world simply cannot disclose many features of the infinite world of mathematical structures.

I would certainly not deny that the physical world, and physical sciences, can be suggestive for mathematics. They can suggest new ideas, new ways of combining old ideas, new "patterns", and so on. This activity fits well with the postulational side of Resnik's epistemology. But recall that though positing is a historical-psychological account of the formation of our mathematical beliefs, it is not an account of the justification of those beliefs. For it does not deliver any link to the independently existing mathematical realm, and so no justificatory link between the posits and mathematical truth. This is where Resnik turns to holism for justification. The challenge for holism is to explain why information from the physical world should be regarded as justifying or falsifying mathematical claims, rather than merely suggesting them. Can the physical world really tell us what is true and false in mathematics for the mathematical realist/platonist?

I think these considerations suggest three basic problems with the combination of realism and holism espoused by Resnik. These should be thought of as challenges. I do not claim to show that Resnik's position is inconsistent on this matter, and I do not claim that all such positions share the same problems. My claim is that there are some apparent tensions in views such as Resnik's, and these tensions need to be resolved before the combined picture can be adequately defended.

First, Resnik's main argument for realism is a *pragmatic* indispensability argument, not the ordinary one. The strength of the pragmatic indispensability argument, recall, is that it does not require the truth of current scientific theory. It is the activity of science, whether it yields truth or falsity, which yields the truth of current mathematics. But this argument for realism seems to pull us away from holism. Detaching the truth of science from that of mathematics in this way implies that whether or not science yields true theories there is a core of mathematics which, if presupposed by science, is true. But this appears to be an argument that mathematics—at least in any core which is presupposed by science—is immune to scientific change, that it is isolated, or separated.[15] In other words, this seems to be a strategy for arguing for separatism, and *against* holism. Thus, Resnik's indispensability argument for realism, novel though it is, seems at odds with his holism about evidence in science and mathematics.

Second, if mathematics is about structures which need not be physically instantiated why should its empirical utility be our ultimate criterion for its truth? Since, for the realist, mathematical objects are independently existing, they do not need to be physically instantiated, or even possibly physically instantiated, in order to exist. Thus the truths of mathematics do not reduce to truths about actual, or even possible, *physical* structures. But evidential holism must commit to the idea that its role, and utility, in science is the ultimate indicator of truth in mathematics. Otherwise the holism in question is of a different stripe—pragmatic perhaps. The point is this simply makes no sense to me, if we presume (a platonistic form of) mathematical realism—that the objects and truths are independent of our beliefs, theories, and proofs, and in particular, independent of physical instantiability.[16]

The third challenge to the combination of realism and holism is as follows. I agree with Resnik that for mathematical realism to be a distinctive view it must commit to his three conditions—the existence of mathematical objects, the acceptance that much of current mathematical theory is true, and the independence of both of these from us and our activities. For the mathematical realist, mathematical objects cannot, therefore, be illusory, nor can mathematical claims reduce to claims about empirical objects. But I think this yields a general problem.

Either there is a difference between mathematical and empirical objects or there is not. If there is, as the mathematical platonist/realist claims, then I don't see how Resnik can maintain his argument against separatism. One of his main criticisms of separatism is that the abstract-concrete dichotomy which underlies it has vague boundaries. But if one is already committed to the existence of a distinctive ontology of mathematical objects, it seems not to matter whether or not this ontology can be explained in terms of some further difference between the abstract and the concrete. Whether or not the abstract-concrete distinction has a sharp boundary, and whether or not we should appeal to this distinction in order to explain the difference between mathematics and physics, if there is an ontological difference between mathematical objects and physical objects then one can use this basic difference on which to rest an epistemological distinction.[17] I interpret Resnik as arguing that the mathematical realist is committed to the distinctive ontology of mathematics, and I believe he is correct to do so. But then, whence the antiseparatism which supports his holism?

If there is no genuine ontological difference between mathematical and physical objects, then I can see why mathematics is both evidentially holistic and empirical. Because there are no distinctively mathematical objects, mathematics is just about what there is—and the information for this (perhaps invoking naturalism) comes from science. But then, whence the realism, or platonism, about *mathematical* objects?

In short, one position on mathematical objects leads to platonism/realism but away from holism; and the other position on mathematical objects leads to holism but away from realism. As I see it, the essence of the problem is the combination of ontic separatism with epistemic holism. This may not be a genuine dilemma, but I do think that there is, at least, a tension here which needs to be explicitly and openly addressed. In the meantime I conclude that the positions espoused here appear to add up to an unstable whole.

5 Conclusion I hope to have raised some interesting questions about the philosophy of mathematics argued for in this book. This should not be mistaken as an argument that the book is not a good book. Part of the worth of a philosophical project should be measured by the opposition it raises. Coming from opposing philosophical prejudices (constructivism, partial separatism, etc.), I am predisposed to look for problems in Resnik's view where others might not be. Resnik's clear, accessible writing style perhaps further encourages this, though I applaud it as a virtue to be emulated.

In short, there are a wide variety of views espoused in the book. They are discussed clearly and honestly. Some of the arguments show genuine progress over former work on the same or similar theses. I do not happen to think that the main arguments are decisive; and I believe that the combined theses create internal tensions which need to be resolved. A step toward such a resolution might be to give up the commitment to *evidential*, or confirmational, holism, and embrace only *pragmatic* holism instead.[18] This would preserve the intuition that scientific methodologies and forms of evidence are different from mathematical methodologies and forms of evidence (which Resnik accounts for via "local forms of evidence"). And this would marry better with naturalism as well as with mathematical realism/platonism. Yet it would acknowledge that the needs and results of science are relevant to mathematics. Whatever resolution one might find appealing, and whether the tensions I have raised are real or merely apparent, the book presents a nice research program for philosophers who are sympathetic to some such combination of views as is advocated here. The book is a valuable addition to the literature for this reason alone.[19]

NOTES

1. Though he does not use exactly the term "evidential holism", he does use the term "confirmational holism", and it is clear that what he is arguing for is a holism about mathematical and scientific evidence. (For example, the title of Chapter 7 is "Holism: Evidence in Science and Mathematics".) He later uses the term "pragmatic holism" but this use should not be taken as the view that it is for pragmatic, or practical, reasons that holism applies. It is still evidential holism: the view that logically, epistemologically, methodologically, and evidentially mathematics is not separate from the rest of science. But we should be pragmatic and nevertheless allow mathematicians their local standards of evidence so long as no harm is done to science as a whole. I will use the term "pragmatic holism" to espouse the opposite point of view: separatism about evidence *in* mathematics, with the acknowledgement that for practical reasons mathematicians may need to consider the larger, scientific picture.

2. I should note that in correspondence Resnik has clarified this as a two component view. All platonists are realists but not all realists are platonists. I will try to be clear about this in the following way. When speaking of realism I will try not to presuppose platonism. But when speaking of Resnik's realism I will usually mean the entire package, including platonism. Resnik is a realist about mathematical objects, and this, he acknowledges, is a metaphysical position (p. 32). His platonism thus fills in his metaphysical position as a realist.

3. Of course, Resnik does not even want his indispensability argument to include the premise that the scientific conclusions drawn from mathematics are, in fact, true. He believes that though science is not necessarily true, the mathematics it presupposes is (p. 49). In its general form, however, "what science presupposes is true" is, of course, a preposterous claim. For example, economics might presuppose a codification of human beings as self-interested pleasure maximizers. Though this is a useful, perhaps even necessary, presupposition for certain branches of economics, it is probably not literally true. The question is, Why should the fact that science presupposes mathematics entail that it is literally true, since the rule does not apply to the other presuppositions of science?

4. Though he does remark earlier that "[r]igorous reflection on the great, but unsuccessful, attempts by Frege, Hilbert, and Brouwer . . . have shown us exactly why it will not do

to take mathematics to be an a priori science of mental constructions, or . . . " (p. 3). Perhaps he is ruling out more competitors with realism than I would.

5. This is where Quine's view may diverge from other indispensability arguments. As I understand his position, he does not advocate taking the language of science at face value, and he is in favor of minimalist reconstructions so long as they are adequate.

6. I am really not convinced that they show anything. Though they are represented mathematically, quantum phenomena are still affected by causal processes.

7. And I should like to thank him for clarifying it in correspondence.

8. Perhaps most distinctions are of this sort.

9. See Chapter 3 of Folina [2] for an argument against this move in Kitcher.

10. "Euclidean rescue" is Resnik's term for a situation where the mathematics is taken as not applying to the physical situation rather than as shown to be (mathematically) false by the empirical outcome.

11. Though, to be fair, we do not really know to what extent Resnik is a naturalist about mathematics. In correspondence he has stressed, for example, that he would not embrace a naturalism which de-legitimized philosophical criticism of scientific or mathematical practice.

12. Interestingly, if this remark about inconsistency could be taken as illuminating Resnik's conception of mathematical truth, then 'truth' means two different things for mathematics and science. So even if the use of deduction in mathematics does not mean that the mathematical axioms have a different character from scientific hypotheses, Resnik's view on truth and falsity would differentiate the character of mathematical axioms from that of scientific hypotheses. In correspondence he has denied that his view on mathematical truth has anything to do with logical possibility. How to make sense of the above remark, then, remains a mystery to me. Given his desire to keep the concept of logical possibility out of his accounts of mathematical truth and existence, he might be better off simply asserting that rejecting the axioms means that they are *false*.

13. Again, our use of these terms is distinct. See endnote 1.

14. We want, as far as possible, to avoid getting stuck on the horns of the famous dilemma, as argued in Benacerraf [1].

15. If it didn't yield a kind of separatism, then the indispensability argument would be in tension with condition (iii) of Resnik's realism: that the mathematical objects and truths about them are independent of our beliefs, theories, and truths. I have been taking it that this includes our beliefs, theories, and truths of natural science.

16. In correspondence Resnik says he would begin a response here by distinguishing a criterion of truth from reasons one might have for believing something. But the separatist does not deny that the physical world can suggest new ideas and new reasons to mathematics. As a scientific holist about the ultimate evidence for mathematical truth, I believe he needs holism to apply to the truth criterion and not merely the belief criterion. In other words, a correct justification for any belief must be appropriately connected to that which makes it true. Thus, if scientific holism is ultimately the correct justification for mathematical claims, then scientific evidence must be appropriately connected to mathematical truth.

17. In fact, Resnik does invoke the abstract-concrete distinction itself: "[Structuralism] also figures in my explanation of how manipulations with concrete numerals and diagrams can shed light on the abstract realm of mathematical objects. Here the key idea consists in noting that these concrete devices represent the abstract structures under study" (p. 7).

18. In my sense of the term. See endnote 1.

19. I would like to thank Michael Resnik for detailed comments on an earlier draft, and for further email correspondence regarding residual questions. I would also like to thank Dave MacCallum for comments and conversation about some of the issues raised here.

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Janet Folina
Department of Philosophy
Macalester College
1600 Grand Avenue
St Paul MN 55105
email: folina@macalester.edu