On Believing in Neutrons but not Numbers

Abstract: Scientific realists who do not want to be mathematical realists face a challenge. It seems that our justification for believing in unobservable entities like quarks and neutrons primarily stems from the fact that their existence is implied by our best, most well-confirmed scientific theories. But our best scientific theories are shot through with mathematics and thereby also imply the existence of mathematical entities. Doesn't epistemological consistency demand, if one believes in concrete unobservables, that one believe in mathematical entities as well? Elliott Sober has suggested a line of response to this challenge of which scientific realists who are not mathematical realists might want to avail themselves. But (as I argue) it is not obvious that the suggested line of response succeeds. I then show how to extend this line of response so that it does meet the above challenge.

I. Scientific Realism but not Mathematical Realism?: A Challenge

Scientific realists believe that our best scientific theories are to be interpreted at face value (rather than instrumentally or in some other non-literal or non-factive way). They also believe that the empirical support we have for those theories gives us good reason to accept their implications regarding concrete unobservables.[1,2] Similarly, mathematical realists believe that our established mathematical theories (or at least a significant subset of them) are to be taken at face value and that we ought to accept their implications (including those involving existential quantification over mathematical entities).[3]

Scientific realists who do not wish to be mathematical realists face a challenge. It seems that our justification for believing in unobservable entities like quarks and neutrons primarily stems from the fact that their existence is implied by our best, most well-confirmed scientific theories. But our best scientific theories are shot through with mathematics and thereby also

[1] Scientific realism is thus opposed to views like that of van Fraassen's (1980) constructive empiricism.

[2] Here I am following Leng's (2005, 82) advice by characterizing scientific realism in such a way as not to render it incompatible with mathematical anti-realism (see also (Leng 2010, 11-12)). I also do not claim to have given a precise characterization of scientific realism. As I see it, 'scientific realism' denotes a family of views, rather than a single, precise thesis.

[3] I hereby stipulate that part of what it is for a mathematical theory to be taken "at face value" is for its quantifiers to be given an objectual interpretation.

imply the existence of mathematical entities. Doesn't epistemological consistency demand, then, that those who accept the existence of the concrete unobservables of science accept the existence of mathematical entities as well? Many presentations of the so-called "Quine-Putnam indispensability argument" boil down to versions of this challenge.[4]

The scientific realist who rejects mathematical realism might try to respond to the above challenge by attempting to carry out something like Hartry Field's program. That is, she might attempt to show that our best scientific theories can be *nominalized* (i.e. reformulated in such a way that the reformulations dispense with the mathematical portions of the original theories while retaining all of their non-mathematical implications). Field himself tries to exhibit the feasibility of this program by offering a nominalistically acceptable reformulation of Newton's theory of gravitation.[5] It is controversial, however, whether the nominalization strategy that Field applies to Newton's theory of gravitation can be successfully extended to contemporary scientific theories such as the general theory of relativity and quantum mechanics.[6] And since the jury is still out when it comes to the success of Field's program, the scientific realist who rejects mathematical realism would do well to cultivate additional strategies.

An alternative strategy would involve, not eliminating the mathematical portions of our best scientific theories, as Field attempts to do, but reinterpreting those portions so that they no

[4] (Quine 1969) and (Putnam 1975) are often taken to be classical sources for this line of argument. But see (Liggins 2008) for a challenge to that interpretation. For one of many contemporary presentations of this line of argument, see (Colyvan 2001).

[5] See (Field 1980) and (Field 1989). Another part of Field's program (though one that is not as directly relevant here) is to attempt to show that the mathematical theories used in our best scientific theories are (what he calls) "conservative," where (to a first approximation) a mathematical theory is conservative if and only if it is such that, when it is conjoined with any nominalistically acceptable theory, the resulting conjunction has all and only the same nominalistically acceptable consequences as does the original nominalistically acceptable theory. This part of Field's program is aimed at helping to explain why the mathematics employed by our best scientific theories is instrumentally useful, even though ultimately dispensable.

[6] See (Urquhart 1990) for reasons to doubt that Field's strategy can be successfully applied to the theory of general relativity. See (Malament 1982) for reasons to doubt that it can be successfully applied to quantum mechanics, (Balaguer 1998, chapter 6) for a response, and (Bueno 2002) for a counter response.

longer imply the existence of mathematical entities.[7] A scientific realist who takes this route might attempt, as philosophers such as Charles Chihara and Geoffrey Hellman have done, to show that our established mathematical theories (or at least the ones that are used in science) can be reformulated so as to eliminate quantification over mathematical entities.[8] Such a strategy, as adopted by the scientific realist, however, threatens to be double-edged sword, one that might also be wielded against her by the scientific anti-realist. Once such "deviant" interpretations of the apparent quantification over mathematical entities found in our best scientific theories are permitted, why not similarly deviant interpretations of the apparent quantification over concrete unobservables found within those theories?[9] On what principled grounds is the scientific realist able to insist that the apparent implications of our best scientific theories pertaining to the existence of concrete unobservables are to be accepted at face value, while denying that the same is true of the apparent implications of those theories that pertain to the existence of mathematical entities?[10] In short, we are back to the challenge with which we began.

Elliott Sober, though not a scientific realist himself, suggests a line of response to this challenge of which scientific realists who reject mathematical realism might want to avail themselves. Sober has noted that there appears to be an asymmetry between the way in which claims about concrete unobservables are related to empirical observation and the way in which the claims of pure mathematics are so related. More specifically, he has noted that while we are often willing to take claims about concrete unobservables as capable of being disconfirmed by

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[7] For an extremely helpful survey of many of the different kinds of strategies that are available here, one to which I am indebted for the present discussion, see (Burgess and Rosen 1997).

[8] See (Hellman 1993), (Chihara 1990), and (Chihara 2007).

[9] Worries along these lines are suggested by some remarks of Burgess and Rosen (1997, 61-63), as well as by the discussion found in (Colyvan 1999, 4-5) and (Colyvan 2001, 76-78).

[10] A similar worry could be raised regarding Field's program. If we are prepared to accept reformulations of our best scientific theories that eliminate the mathematical portions thereof, why shouldn't we also be prepared to accept reformulations of those theories that eliminate quantification over concrete unobservables? See (Hawthorne 1996), however, for a powerful way in which a proponent of Field's program can respond to this worry.

various empirical observations, we are often not willing to take pure mathematical claims as being disconfirmed by those same sorts of observations. This fact, Sober argues, indicates that pure mathematical claims are also not confirmed (at least not typically) by empirical observation.[11] Sober's point, if correct, undercuts the sort of confirmational holism that many commentators have taken to serve as a key premise of Quine's original version of the indispensability argument, one according to which the empirical observations that serve to confirm a scientific theory do so by confirming that theory as a whole, rather than by confirming some parts of that theory but not others.[12]

Unfortunately, however, even if this line of response is entirely correct as far as it goes, from the perspective of the scientific realist, it doesn't go far enough. It is still the case, the scientific and mathematical realist might argue, that our primary reason for accepting claims about concrete unobservables is that they are implied by our best scientific theories, these being well confirmed by empirical observation. The mathematical realist can believe, furthermore, that our empirical evidence gives us good reason to believe that those theories are *true* (or, at least, approximately so), thereby making it rational for us to believe their known implications. The mathematical anti-realist, however, has no such luxury. Indeed, insofar as she believes that it is not likely that mathematical realism is true, she is committed to the claim that it is not likely that our best scientific theories are true (since a theory can be no more probable than its known implications). Once again, it is true that she could try to remedy this situation by offering a deviant interpretation of the mathematical portions of our best theories, one that is more congenial to her mathematical anti-realism. Also once again, however, this strategy on her part

[11] See (Sober 1993, especially 49-50, 52-53). For similar lines of argument, see (Vineberg 1996), (Maddy 1997), (Bigaj 2003), (Peressini 2008), and (Leng 2002).

[12] See (Morrison 2010) and (Morrison 2012) for further discussion.

threatens to be a double-edged sword, one that might be wielded against her by the scientific anti-realist.

Granted, it is true that if Sober's response is correct, it may well be that some of the implications of our best scientific theories pertaining to concrete unobservables, unlike the mathematical implications thereof, are confirmed by the same empirical observations that confirm those theories. But to say this is the case is not to say which implications are so confirmed; nor is it to say by how much. It may be, for all Sober's response, *taken by itself*, can tell us, that *none* of those implications are confirmed to a degree sufficient to warrant our belief in them. Perhaps this need not bother Sober, since he explicitly distances himself from scientific realism,[13] but for the scientific realist, the challenge posed at the outset goes unmet.

Fortunately, as I argue in the next two sections, there is a way to develop Sober's line of response on behalf of the scientific realist who rejects mathematical realism so as to fill the above gap. I begin, in the next section, by developing a more detailed version of the Sober-inspired line of response sketched above. Though I will not argue that the claims required for that version of the response are true, I will defend their plausibility. In the final section, I show how to fill out that line of response even further so as to meet the objection to it posed above. The upshot will be that the scientific realist who rejects mathematical realism is left with a plausible line of response to the challenge with which we began.

II. The Empirical Resilience and Isolation of Pure Mathematics

Above I noted Sober's observation that we are typically unwilling to regard the claims of pure mathematics as capable of being disconfirmed by the sorts of empirical observations that we

[13] (Sober 1993, 37, 41-42, 43-45, 48)

often bring to bear when testing scientific theories. Suppose, to take Sober's own example, we are considering the "hypothesis" that 2+2=4. We note that this hypothesis, in conjunction with the auxiliary assumption that 2+2 is the number of apples on the table, implies that there are four apples on the table. But suppose that we then observe that it is not the case that there are four apples on the table. Would that observation cast doubt on the hypothesis that 2+2=4? As Sober points out, it seems that the answer is "No":

> If there had failed to be 4 apples on the table, I do not think we would have concluded that 2+2 has a sum different from 4. Rather, we would have concluded that the auxiliary … assumption is mistaken. If this is how we comport ourselves, then the "experiment" just described need never have been run. If we hold our belief that 2+2=4 immune from revision in this experiment, then the outcome of the experiment does not offer genuine support of that proposition.[14]

We might summarize Sober's observation by saying that the hypothesis that 2+2=4 is "resilient" in this empirical situation; it is not capable of being disconfirmed by any of the possible empirical observations that might be brought to bear upon it (in the context of the present experiment).

It is also tempting to draw a more general lesson from this example. It seems that similar considerations would apply to *any* claim of pure mathematics and to *any* empirical situation. That is, it is tempting to embrace something in the neighborhood of the following principle:

> (Resilience) For any claim of pure mathematics, M, there are no possible empirical observations that count against M.

[14] (Sober 1993, 50)

Sober himself stops short of endorsing (what I'm calling) Resilience (for reasons I will soon consider), contenting himself with the weaker assertion that something like that principle typically holds now, with respect to the sort of pure mathematical claims implied by our best scientific theories, and with respect to the kinds of empirical observations brought to bear upon those theories.[15]

Before we consider objections to Resilience, however, it will be helpful to examine another conclusion that Sober draws from the above example. Note Sober's assertion that "if we hold our belief that 2+2=4 immune from revision in this experiment, then the outcome of the experiment does not offer genuine support of that proposition." Here Sober seems to be relying on something like the following principle of confirmation:

(Symmetry) O favors H if and only if ~O counts against H.

Sober's reliance on this principle (or something like it, at any rate) flows out of his endorsement of another familiar principle of confirmation, the Likelihood Principle:

(LP) O favors H over H' if and only if P(O/H) > P(O/H').[16]

It can be shown (via the probability calculus) that LP entails

(LP') O favors H over H' if and only if ~O favors H' over H.[17,18]

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[15] (Sober 1993, 50-51, 56)

[16] I am assuming that in order for O to favor H over H', it must be the case that neither P(H) nor P(H') is equal to an extreme probability (i.e. that neither is equal to 0 or to 1). This assumption ensures that in all cases in which the left hand side of LP is satisfied, the conditional probabilities that occur on the right hand side are well defined. I also stipulate that in those cases in which either of these conditional probabilities are not well defined, the right hand side of the relevant instantiation of LP is to be regarded as false.

[17] For the "only if" direction, assume that O favors H over H'. By LP, P(O/H) > P(O/H'). By the axioms of the probability calculus, it follows that 1 – P(~O/H) > 1 – P(~O/H') and therefore that P(~O/H') > P(~O/H). By another application of LP, it follows that ~O favors H' over H. The "if" direction is proved by a parallel argument.

Let's say that an observation *favors* a hypothesis if and only if it favors that hypothesis over its negation.[19] Let's also say that an observation *counts against* a hypothesis if and only if that observation favors its negation. By employing these definitions and substituting ~H for H' in LP', we may derive Symmetry.

Symmetry, in conjunction with Resilience, entails another strong principle regarding the bearing of empirical observation on mathematical theories:

> (Isolation) For any claim of pure mathematics, M, there are no possible empirical observations that favor M.

Isolation, furthermore, is just the sort of principle that would allow the mathematical anti-realist to resist empirical arguments for mathematical realism.

Unfortunately, however, both Resilience and Isolation can appear too strong. As they stand, both initially appear subject to counterexamples. Sober indicates, for example, that he does not wish to deny that Plateau obtained empirical confirmation for propositions of pure mathematics when he dipped pieces of wire into soap suds in order to discover (for a number of cases) the surface of least area that is bounded by a given closed contour.[20] Another kind of often cited example against principles like Resilience and Isolation is the alleged overturning of Euclidian geometry in the twentieth century on account of empirical considerations. Yet another

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[18] Sober (1993, 44) explicitly endorses a version of LP', noting that it follows from the Likelihood Principle.

[19] It might be thought by readers familiar with Sober's views that my employing the notion of a theory's being favored *simpliciter* contradicts Sober's (1993, 39) assertion that "the Likelihood Principle entails that the degree of support a theory enjoys should be understood relatively, not absolutely." But this is not so. Sober makes it clear that what he means by this claim is that "the evidence we have for the theories we believe does not favor those theories *over all possible alternatives*" (39, emphasis original). This does not entail that our evidence cannot favor a hypothesis over its own negation. In fact, Sober explicitly says otherwise: "The fact of the matter is that when scientists lack a developed substantive alternative to a theory, they contrast the theory with its own negation. This is a contrastive alternative that is always available" (52-53).

[20] (Sober 1993, 51)

kind of counterexample might involve discovering the solutions to various mathematical problems by employing calculators or computer programs.

One consideration that is relevant to the alleged counterexamples involving experiments like those of Plateau and the use of calculating devices is that the alleged confirmation for the claims of pure mathematics that occurs in these cases is mediated by impure auxiliary assumptions, these being assertions concerning how a certain physical system models various pure mathematical theories. Furthermore, in those situations in which we are confident that our observations diverge from what we know to be implied by the relevant pure mathematical theory, in conjunction with the relevant auxiliary assumptions, our inclination is to lower our confidence in the auxiliary assumptions rather than in the pure mathematical theory. This suggests that the underlying pure mathematical theory informing our auxiliary assumptions is not itself being empirically tested (and therefore is not itself being empirically confirmed).[21]

Similar things may be said about the alleged overturning of Euclidian geometry. Either we take Euclidian geometry as a pure mathematical theory or as a theory concerning the nature of physical space. If the former, then the thing to say in response to the relevant empirical observations is not that they have disconfirmed the pure mathematical theory, but that they have disconfirmed the hypothesis that the theory is appropriately modeled by physical space. If the latter, we have no genuine counterexample to Resilience.[22]

I have not given these alleged counterexamples the attention they deserve. Nevertheless, I believe that enough has been said to support the *plausibility* of the assertion that Isolation and

[21] For similar kinds of responses to cases of these sorts, see (Vineberg 1996), (Leng 2002) and (Bigaj 2003) and (Peressini 2008).

[22] For a similar line of response to the claim that the alleged fall of Euclidean geometry affords an example of a mathematical theory being disconfirmed by empirical considerations, see (Leng 2002, 402, 412) as well as (Leng 2010, 80-81).

Resilience are true, or at least that appropriately qualified versions of those claims are true.[23] My present concern is to investigate how much mileage the mathematical anti-realist might get out of a successful defense of these claims (or appropriately qualified versions thereof). In the next section, I will argue that if these claims can be successfully defended, mathematical anti-realists with inclinations toward scientific realism can develop a fully satisfying response to the challenge posed at the outset of this paper.

III. Scientific Realism without Mathematical Realism

Given the likelihood principle, the conjunction of Resilience and Isolation is equivalent to

> (R&I) For any claim of pure mathematics, M, and any possible conjunction of empirical observations, E, $P(E/M) = P(E/{\sim}M)$ (provided that $0 < P(M) < 1$).[24]

By means of the probability calculus, furthermore, it is easy to show that R&I entails

> (Independence) For any claim of pure mathematics, M, and any possible conjunction of empirical observations, E, $P(E/M) = P(E)$ (provided that $0 < P(M) < 1$).[25]

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[23] The mathematical anti-realist may be able to get by with claims considerably weaker than Resilience and Isolation. What is disconcerting to the mathematical anti-realist is the position that various of the general existential claims of pure mathematics (e.g. the claim that there are numbers) are subject to empirical confirmation. She might, accordingly, qualify Isolation and Resilience in such a way as they only apply to these sorts of general existential claims. The challenge would be to do this in a way that both captures the appropriate class of existential claims and which is not *ad hoc*.

[24] Concerning the proviso that $0 < P(M) < 1$, see note 16. If one is inclined to complain that the claims of pure mathematics are either necessarily true or necessarily false and therefore that, for any given pure mathematical claim, M, P(M) is either 0 or 1 (thereby leaving, for any claim E, either P(E/M) or P(E/~M) undefined), it should be borne in mind that the probabilities at issue here are to be regarded as epistemic probabilities rather than logical or objective probabilities. I will assume that claims that are necessarily true if true and necessarily false if false may nonetheless have non-extreme epistemic probabilities. I do not have the space here to address any skepticism that one might have concerning the applicability of the probability calculus to epistemic probabilities that allow necessarily true (or necessarily false) claims to have non-extreme probabilities. For one defense of the applicability of the probability calculus to epistemic probabilities of that sort, however, see (Garber 1983).

[25] Proof: Suppose that $P(E/M) = P(E/{\sim}M)$. By this and the total probability principle, it follows that $P(E) = P(M)*P(E/M) + P({\sim}M)*P(E/{\sim}M) = P(M)*P(E/M) + P({\sim}M)*P(E/M) = P(E/M)*[P(M) + P({\sim}M)] = P(E/M)*1 = P(E/M)$.

But surely Independence, if true, is an instance of a more general principle that is also true. It is implausible, for example, that while no possible empirical observations could count for or against a claim of pure mathematics, our somehow learning (though non-empirical means) that there are neutrons could do so.

Let's stipulate that a claim is to be regarded as "solely about non-mathematical entities" if and only if each of the quantifiers that occur in a sentence that expresses that claim are restricted to non-mathematical entities, and (given how all the non-mathematical entities in the world are intrinsically and in relation to one another) there being or not being mathematical entities makes no difference as to the truth value of that claim. A mixed mathematical claim such as *The ratio of an object's gravitational mass to its inertial mass is equal to 1*, therefore, is not a claim that is solely about non-mathematical entities. By contrast, the claim *Neutrons are more massive than electrons* is a claim that is solely about non-mathematical entities. Let it also be stipulated that any claim that is solely about non-mathematical entities is not to be regarded as a claim of pure mathematics. Given these stipulations, we can now see that if Independence is true, then surely it is an instance of the following, more general, true principle:[26]

> (Strong Independence) For any claim of pure mathematics, M, and any conjunction of claims that are solely about non-mathematical entities, C, P(C/M) = P(C) (provided that 0 < P(M) < 1).

[26] It is a more general principle, at any rate, if (as I am assuming) any claim that genuinely reports an empirical observation is a claim that is solely about non-mathematical entities. I do grant that scientists often state their observations by using mixed mathematical claims. A scientist might report an observation by saying, for instance, "The mass of the sample was measured as being equal to 3.24 grams." I assume, however, that, in a case such as this, the proposition that genuinely reports the empirical observation at issue would be something like *The digital readout screen displayed "3.24 g" when the sample was on the scale*.

And Strong Independence, as I shall now show, provides the mathematical anti-realist with a powerful resource for responding to the challenge that we have been considering.

Let 'T' stand for one of our best, most well-confirmed scientific theories, one that demonstrably implies some claim of pure mathematics, M.[27] Let 'E' stand for the conjunction of a body of observation reports (reports solely about non-mathematical entities) that together constitute our empirical basis for accepting T. Suppose that P(T/M&E) is high enough to warrant belief in T given hypothetical certainty about M and E. Finally, let 'U' stand for some claim solely about non-mathematical entities that pertains to concrete unobservables (e.g. the claim that neutrons exist) and suppose that U is also demonstrably implied by T.

Strong Independence gives us the following claims:

(1) $P(E/M) = P(E)$

(2) $P(U\&E/M) = P(U\&E)$

By means of the probability calculus, it can be shown that these claims jointly entail[28]

(3) $P(U/E) = P(U/M\&E)$

Since T demonstrably implies U, it is also the case that

(4) $P(U/M\&E) \geq P(T/M\&E)$

Finally, (3) and (4) together entail

[27] I am, *pace* van Fraassen (1980), taking scientific theories to be propositional entities rather than model-theoretic ones, largely for the sake of convenience more than anything else.

[28] Recall that, according to the standard definition of conditional probability, $P(A/B) = P(A\&B)/P(B)$. So, $P(U/E) = P(U\&E)/P(E)$. Thus, by 2, $P(U/E) = P(U\&E/M)/P(E) = P(U\&E\&M)/[P(M)*P(E)]$. Also, by 1 and the standard definition of conditional probability, $P(M\&E) = P(M)*P(E/M) = P(M)*P(E)$. It follows from the above that $P(U/E) = P(U\&E\&M)/P(M\&E)$. Since it is also the case that, by the standard definition of conditional probability, $P(U/M\&E) = P(U\&E\&M)/P(M\&E)$, it follows that $P(U/E) = P(U/M\&E)$.

(5) $P(U/E) \geq P(T/M\&E)$

If we take these conditional probabilities to reflect what our rational credences ought to be, (5) tells us that our confidence in U given certainty about E ought to be at least as great as our confidence in T given hypothetical certainty about M&E. But we stipulated that our confidence in T given hypothetical certainty about M&E ought to be high enough to warrant belief. It follows that our confidence in U given certainty about E ought to be high enough to warrant belief as well. This is all consistent, furthermore, with P(M/E) being quite low.[29] The upshot is that the mathematical anti-realist who accepts Strong Independence may take herself to have principled grounds for accepting U (by seeing that T implies it) while rejecting M (even though she sees that T implies M as well). More generally, she may, in a principled way, accept those implications of our best, most well confirmed scientific theories that are solely about non-mathematical entities (including those that pertain to the existence of concrete unobservables) while rejecting the mathematical implications of those theories.

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[29] We may demonstrate the *formal* consistency of these claims by exhibiting a model in which they are all true. And we may do that by reassigning whatever propositions to ‘T’, ‘U’, ‘M’, and ‘E’ that we like (regardless of what these terms originally denoted), provided there is a possible state of affairs in which the resulting claims concerning the relevant conditional probabilities are all true. To that end, suppose we let ‘M’ denote the proposition that a fair, one thousand sided di (each side of which is assigned a unique whole number in the inclusive range of 1 through 1000) comes up 3 on a fair roll. Let ‘E’ denote the proposition that a certain coin (recently fairly tossed) came up heads. Let ‘U’ denote the same proposition as ‘E’. Finally, let ‘T’ denote the conjunction of M and E. Clearly, given these assignments, P(T/M&E) is high enough to warrant belief in T given hypothetical certainty about M and E (its value equals 1, in fact) and T also demonstrably implies U and M. Furthermore, the background knowledge could easily be such that claims (1) and (2) above are both true (in fact, it will be such that those claims are both true unless it is rather atypical). Finally, P(M/E) is quite low (1/1000 to be exact).

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