MARK COLYVAN

CONTRASTIVE EMPIRICISM AND INDISPENSABILITY

ABSTRACT. The Quine–Putnam indispensability argument urges us to place mathematical entities on the same ontological footing as (other) theoretical entities of empirical science. Recently this argument has attracted much criticism, and in this paper I address one criticism due to Elliott Sober. Sober argues that mathematical theories cannot share the empirical support accrued by our best scientific theories, since mathematical propositions are not being tested in the same way as the clearly empirical propositions of science. In this paper I defend the Quine–Putnam argument against Sober's objections.

It is generally believed that empirical science provides us with propositions that are *a posteriori*, contingent and revisable in the light of empirical evidence. Mathematical propositions, on the other hand, are generally believed to be a priori, necessary and unrevisable in the light of empirical evidence. But the Quine–Putnam indispensability argument[1] tells us that mathematical knowledge is in the same epistemic boat as empirical knowledge. The tension is clear, and many authors[2] have exploited this tension, in various ways, to undermine the indispensability argument. In this paper I will address one very influential objection of this kind due to Elliott Sober (1993). Sober's objection is very specific: he takes issue with the confirmational holism appealed to by the Quine–Putnam argument. Sober grants that mathematics is indispensable to our best scientific theories but disagrees that mathematics is confirmed by the evidence that confirms these theories. Sober argues that mathematics cannot share the empirical support of our best scientific theories, because mathematics is a common part of *all* such theories. He believes that mathematics is not being tested in the same way as the clearly empirical claims of science, and so cannot be confirmed by the usual empirical methods.

Sober's objection is framed from the viewpoint of *contrastive empiricism*, so it will be necessary to first consider some of the details of this theory in order to evaluate the force of Sober's objection. As will become apparent, though, contrastive empiricism has some difficulties which I'm inclined to think cannot be overcome. This, in turn, robs Sober's objection of much – but not all – of its force. In the final section I will recast the

objection without the contrastive empiricism framework and show that this version of the objection also faces significant difficulties.

1. CONTRASTIVE EMPIRICISM

As we shall see, contrastive empiricism is best understood as a position between scientific realism and constructive empiricism. The central idea of contrastive empiricism is the appeal to the *Likelihood Principle* as a means of choosing between theories.

PRINCIPLE 1 (The Likelihood Principle). Observation O favours hypothesis H_1 over hypothesis H_2 iff $P(O|H_1) > P(O|H_2)$.

It's clear from Principle 1 that the support an hypothesis receives is a relative matter. As Sober puts it:

The Likelihood Principle entails that the degree of support a theory enjoys should be understood relatively, not absolutely. A theory competes with other theories; observations reduce our uncertainty about this competition by discriminating among alternatives. The evidence we have for the theories we accept is evidence that favours those theories *over others*. (Sober 1993, p. 39)

According to Sober, though, evidence can never favour one theory over all possible competitors since "[o]ur evidence is far less powerful, the range of alternatives that we consider far more modest" (Sober 1993, p. 39).

Another consequence of Principle 1 is that observational data may fail to discriminate between two theories. For instance, contrastive empiricism cannot discriminate between standard geological and evolutionary theory, and Gosse's theory that the earth was created about 4,000 years ago with all the fossil records and so on in place. Indeed, Sober's account cannot rule out any cleverly formulated sceptical hypothesis. Furthermore, Sober is reluctant to appeal to simplicity or parsimoniousness as non-observational signs of truth, and so such sceptical problems are taken to be scientifically insoluble. This is one important way in which contrastive empiricism departs from standard scientific realism (and, arguably, standard scientific methodology).

Although according to contrastive empiricism "science attempts to solve discrimination problems" (Sober 1993, p. 39) and the burden of solving these problems is placed firmly on the observational data, there is no restriction to hypotheses about observables, as in van Fraassen's constructive empiricism (Fraassen 1980).

Contrastive empiricism differs from constructive empiricism in that the former does not limit science to the task of assigning truth values to hypotheses that are strictly about

observables. What the hypotheses are *about* is irrelevant; what matters is that the competing hypotheses make different claims about what we can observe. Put elliptically, the difference between the two empiricisms is that constructive empiricism focuses on *propositions*, whereas contrastive empiricism focuses on *problems*. The former position says that science can assign truth values only to *propositions* of a particular sort; the latter says that science can solve *problems* only when they have a particular character. (Sober 1993, p. 41)

Much more could be said about contrastive empiricism, but we have seen enough to motivate Sober's objection to indispensability theory.

2. THE OBJECTION

Sober's main objection is that if mathematics is confirmed along with our best empirical hypotheses, there must be mathematics-free competitors (or at least alternative mathematical theories as competitors).

Formulating the indispensability argument in the format specified by the Likelihood Principle shows how unrealistic that argument is. For example, do we really have alternative hypotheses to the hypotheses of arithmetic? If we could make sense of such alternatives, could they be said to confer probabilities on observations that differ from the probabilities entailed by the propositions of arithmetic themselves? I suggest that both these questions deserve negative answers. (Sober 1993, pp. 45–46)

It is important to be clear about what Sober is claiming. He is *not* claiming that indispensability arguments are fatally flawed. He is not unfriendly to the general idea of ontological commitment to the indispensable entities of our best scientific theories. He simply denies that "a mathematical statement inherits the observational support that accrues to the empirically successful scientific theories in which it occurs" (Sober 1993, p. 53). This is enough, though, to place him at odds with the Quine–Putnam version of the indispensability argument.

In reply to this objection, then, I firstly wish to point out that there *are* alternatives to number theory. Frege showed us how to express most numerical statements required by empirical science without recourse to quantifying over numbers.[3] Furthermore, depending on how much analysis you think Field[4] has successfully nominalised, there are alternatives to that also. (At the very least he has shown that there are alternatives to differential calculus.)[5]

I take the crux of Sober's objection then to be the second of his two questions and I agree with him here that this deserves a negative answer. I don't think that Field's nominalist version of Newtonian mechanics and standard Newtonian mechanics would confer different probabilities on any observational data. But so much the worse for contrastive empiricism! The question of which is the better theory will be decided on the grounds of

simplicity, elegance and so on–grounds explicitly ruled out by contrastive empiricism.[6] Indispensability theory does not propose to settle all discrimination problems by purely empirical means, so of course it flounders when forced into the straight-jacket of contrastive empiricism.[7]

Another objection to the whole contrastive empiricism approach to theory choice is raised by Geoffrey Hellman and considered by Sober (1993). The objection is that often a theory is preferred over alternatives, not because it makes certain (correct) predictions that the other theories assign very low probabilities to, but rather, because it is the only theory to address such phenomena at all.[8] Sober points out that the relevance of this to the question of the indispensability of mathematics is that presumably "stronger mathematical assumptions facilitate empirical predictions that cannot be obtained from weaker mathematics" (Sober 1993, p. 52).[9] If this objection stands, then the central thesis of contrastive empiricism is thrown into conflict with actual scientific practice. Indeed, Sober admits that "[i]f this point were correct, it would provide a quite general refutation of contrastive empiricism" (Sober 1993, p. 52). I believe that Hellman's point is correct, but first let's consider Sober's reply.

Sober's first point is that when scientists are faced with a theory with no relevant competitors, they can contrast the theory in question with its own negation. He needs to be careful here though, for the negation of a theory surely does not deserve the status of "a rival theory".[10] We might, however, think of the negation of a theory as a class of theories that contains all the relevant alternatives to the theory in question. But this won't do either. Sober explicitly denies that evidence may favour one theory over all alternatives. What then does Sober have in mind? Despite his talk about the negation of a theory being the relevant rival in the cases that Hellman points to, it seems Sober actually has in mind what we might call "parasitic" rival theories. These are rival theories constructed from the theory in question.

Sober considers the example of Newtonian physics correctly predicting the return of Halley's comet, something which other theories were completely silent on. He claims that "alternatives to Newtonian theory can be constructed from Newtonian laws themselves" (Sober 1993, p. 52). For example, Newton's law of universal gravitation:[11]

$$F = \frac{Gm_1m_2}{r^2}$$

competes with:

$$F = \frac{Gm_1m_2}{r^3}$$

and

$$F = \frac{Gm_1m_2}{r^4}$$

and many others. There is no doubt that such parasitic alternatives *can be* constructed and contrasted with Newtonian theory, but surely we are not interested in what *we could* do, we are interested in actual scientific practice. Sober takes this a little further though when he claims that this *is* standard scientific practice for such cases (Sober 1993, pp. 52–53). He offers no evidence in support of this last claim, and without a thorough investigation of the history of relevant episodes in the history of science it seems quite implausible. It seems unlikely that scientists were interested in debating over whether it should be r^2, r^3 or r^4 in the law of universal grav- igation, as Sober suggests.[12] The relevant debate would have surely been over retaining the existing theory or adopting Newtonian theory. At the very least, Sober needs to present some evidence to suggest that scientists are inclined to contrast a theory with its parasitic variants when nothing better is on offer. Until such time, I'm inclined to think he is wrong about this.

In his second point in response to Hellman's objection he considers the possibility of "strong" mathematics allowing empirical predictions that cannot be replicated using weaker mathematics. He points out that strong mathematics also allows the formulation of theories that make false pre- dictions, and that this is ignored by the indispensability argument.

It is a striking fact that mathematics allows us to construct theories that make *true* pre- dictions and that we could not construct such predictively *successful* theories without mathematics. It is less often noticed that mathematics allows us to construct theories that make *false* predictions and that we could not construct such predictively *unsuccessful* theories without mathematics. If the authority of mathematics depended on its empirical track record, *both* these patterns should matter to us. The fact that we do not doubt the mathematical parts of empirically *un*successful theories is something we should not forget. Empirical testing does not allow one to ignore the bad news and listen only to the good. (Sober 1993, 53)

The first question is: How is this supposed to disarm the Hellman objec- tion? It may be useful at this point to spell out how I take the Hellman strategy to proceed. Hellman's point is that contrastive empiricism does not account for cases where a theory is preferred because it makes predictions that no other theory is able to address one way or another. If this is accep- ted, then contrastive empiricism as a representation of how theory choice is achieved seems at best only part of the story, and at worst completely misguided. Furthermore, if it is reasonable to prefer some theory because it correctly predicts new phenomena that other theories are silent on, then

it is reasonable to accept strong mathematical hypotheses, since theories employing strong mathematics are able to predict just such phenomena.

I take it that Sober's reply runs like this: contrastive empiricism can accommodate the Hellman examples of scientific theories that address new phenomena. This is done by contrasting such theories with their parasitic rivals. Thus, a general undermining of contrastive empiricism is avoided. This reply, however, seems to allow that strong mathematics is confirmed, because such theories correctly predict empirical phenomena that theories employing weaker mathematics cannot address. Here is where the second part of Sober's reply is called upon. The point here is simply that the case of strong mathematics is different from that of bold new physical theories, in that strong mathematics can also facilitate false predictions that competing theories are silent on. Thus, the mathematics cannot share the credit for the successful empirical predictions, since it won't share the blame for unsuccessful empirical predictions. (One admires Sober's sense of justice here, but as we shall see, it is misplaced.)

There are a couple of interesting issues raised by this rejoinder. Firstly, his rejoinder is in the context of a defence of contrastive empiricism and yet it is not an argument for that thesis. Neither is it an argument depending on contrastive empiricism. It seems like a new objection to the use of indispensability arguments to gain conclusions about mathematical entities. What is more, this objection appears to be independent of his contrastive empiricism. Given that many people (including myself) find contrastive empiricism implausible, I take it that this last point is, in many ways, the more substantial part of his objection to indispensability theory and I will discuss it further in the next section.

3. A RESIDUAL WORRY

So far I've pointed out that I think Sober is quite wrong about scientists contrasting bold new theories with their negations. At the very least he needs to give some evidence to support his claim that they do.[13] Indeed, it would be interesting to investigate some candidate cases in detail to shed some light on this issue, but fortunately this is not necessary for the present purposes since even if I grant Sober his first point (that contrastive empiricism can accommodate Hellman's examples of bold new theories) the second part of his reply also runs into trouble.

Sober claims, in effect, that mathematical theories cannot enjoy the confirmation received by theories that make bold new true predictions because the mathematics is not disconfirmed when it is employed by a theory that makes bold new false predictions. I've already noted that this point is

stated independently of contrastive empiricism. Indeed, I take this to be a separate worry about the indispensability argument as applied to mathematical entities. Also bear in mind that it is important to his case that there be a difference between mathematical hypotheses and non-mathematical hypotheses in this respect.

Unfortunately for Sober, though, there is no such difference. Many nonmathematical hypotheses are employed by false theories and yet are not held responsible for disconfirmations. One can always combine a correct theory with some false assumptions to obtain false predictions, so the fact that a theory is part of a larger theory that yields false predictions should not, in general, count against the sub-theory.[14] For example, take the (correct) theory of celestial mechanics and add to it some hypotheses about the significance of the positions of the planets for human behaviour (as astrology does). It is well known that such theories yield grossly false predictions and yet this (quite rightly) does *not* count against the theory of celestial mechanics.

The partial asymmetry between confirmation and disconfirmation that Sober points to is a direct consequence of confirmational holism (Hellman, forthcoming). When a theory is confirmed, the *whole* theory is confirmed. When it is disconfirmed, it is rarely the fault of every part of the theory, and so the guilty part is to be found and dispensed with. It's analogous to a sensitive computer program. If the program delivers the correct results then every part of the program is believed to be correct. However, if it is not working it is often because of only one small error. The job of the computer programmer (in part) is to seek out the faulty part of the program and correct it. Furthermore, the programmer will resort to wholesale changes to the program only if no other solution presents itself. This is especially evident when one part of the program *is* working. In such a case the programmer seeks to make a small *local* change in the defective part of the program. Changing the programming language, for instance, is *not* such a change.

Now if we return to Sober's charge that mathematics cannot enjoy the credit for confirmation of a theory if it cannot share the blame for disconfirmation, we see that blaming mathematics for the failure of some theory is never going to be a small local change, due to the simple fact that mathematics is used almost everywhere in science. What is more, much of that science is working perfectly well. Blaming the mathematics is like a programmer blaming the programming language. And similarly, claiming that mathematics cannot share the credit is like claiming that the programming language cannot share the credit for the successful program.

In some cases it may well be the fault of the mathematics or the language, but it is not a good strategy to start with changes to these.[15]

Furthermore, we see that mathematics is not alone in this respect. Many clearly empirical hypotheses share this feature of apparent immunity from blame for disconfirmation. Michael Resnik points out that conservation principles seem immune from liability for much the same reasons that mathematics is. He goes even further to express doubts about whether such principles could be tested at all in the contrastive empiricist framework and "yet we do not want to be forced to deny them empirical content or to hold that the general theories containing them have not been tested experimentally" (Resnik 1997, p. 120). Another such case is the hypothesis that space-time is continuous, rather than discrete and dense.

Before closing I should mention Sober's claim that the main point of his objection can be separated to some extent from the contrastive empiricist epistemology. He does not, however, seem to have the residual worry that I discussed above in mind. He is concerned that you might think that contrastive empiricism can't be right because it ignores nonempirical criteria such as simplicity. He then suggests that "even proponents of such nonempirical criteria should be able to agree that *empirical* considerations must be mediated by likelihoods" (Sober 1993, p. 55). Sober is suggesting that at the very least we discriminate between empirical hypotheses by appeal to likelihoods and that his objection goes through granting only this.[16] But why should we accept that all discriminations between empirical hypotheses must be mediated by likelihoods? After all, we have already seen that we cannot discriminate between the hypothesis that space-time is continuous and the hypothesis that space-time is discrete and dense on empirical grounds and yet these are surely both empirical hypotheses. So Sober's objections to indispensability theory fail because they depend crucially on accepting the Likelihood Principle as the only arbiter on empirical matters. The independent residual worry that I identified in Section 3, fails because it doesn't take account of the asymmetric character of confirmational holism.[17]

NOTES

[1] The Quine–Putnam indispensability argument is the argument that we ought to have ontological commitment to the indispensable entities of our best scientific theories, and since mathematical entities are amongst these, we ought to be realists about mathematical entities. Furthermore, according to Quine and Putnam, mathematical statements receive empirical support from the confirmation of the theories in which those statements appear. The argument is generally taken to depend upon naturalism and confirmational holism. See Quine (1980) and Putnam (1979) for further details.

^{2} These include Alan Musgrave (1986) and Charles Parsons (1996). Penelope Maddy has some related worries about indispensability arguments. See (Maddy 1992, 1995) for her concerns.

^{3} 'There are two Fs' or 'the number of Fs is two' is translated to:

$$(\exists x)(\exists y)(((Fx \wedge Fy) \wedge x \neq y) \wedge (\forall z)(Fz \supset (z = x \vee z = y))).$$

^{4} Cf. Field (1980).

^{5} This is only considering sensible alternatives. There are, presumably, many rather bad theories which do without mathematics. Most pseudosciences do without all but the most rudimentary mathematics.

^{6} This, of course, is no different from any other case of empirically equivalent theories. Generally we prefer a theory over another empirically equivalent theory because, for example, one is simpler. This does not mean that the simpler theory is without empirical support.

^{7} The role theoretic virtues other than empirical adequacy play in the indispensability debate is discussed in Colyvan (forthcoming).

^{8} Hellman gives the example of relativistic physics correctly predicting the relationship between total energy and relativistic mass. In pre-relativistic physics no such relationship is even postulated, indeed, questions about such a relationship cannot even be posed (Hellman 199?).

^{9} For example, Geoffrey Hellman argues for this in Hellman (1992).

^{10} For example, when statisticians contrast some hypothesis with the null-hypothesis, they are not comparing rival theories. Why do I say this? Well, if they find no support for the hypothesis in question, they do not conclude that they have confirmed the null-hypothesis *as the relevant theory*.

^{11} Here F is the gravitational force exerted on two particles of mass m_1 and m_2 separated by a distance r. G is the gravitational constant.

^{12} Not to mention $r^{2.0000000000001}$ or $r^{1.9999999999999}$. (Although it seems that cases such as these were considered when the problems with Mercury's perihelion came to light (Roseveare 1983), they were considered only in order to save the essentials of Newtonian theory which, by that stage, was already a highly confirmed theory.)

^{13} It is worth pointing out that he must provide evidence that contrasting theories with their negations is a general phenomenon. Even if there are only one or two counterexamples, contrastive empiricism is in trouble.

^{14} I thank an anonymous reviewer for this way of putting the point.

^{15} This is nothing more than Quine's Maxim of Minimum Mutilation (Quine 1992, pp. 14–15). See also (Resnik 1997, pp. 124–130) for a nice discussion on this point.

^{16} Since, according to indispensability theory, mathematics *is* empirical and yet we cannot discriminate between mathematical and non-mathematical theories by appeal to likelihoods.

^{17} Material from this paper was presented to the Philosophy Society of the Australian National University, the School of Philosophy at the University of Tasmania and at the 1997 Australasian Association of Philosophy Conference at the University of Auckland. I would like to thank the participants in the subsequent discussions for their contributions. I also gratefully acknowledge the help of Geoffrey Hellman, Frank Jackson, Bernard Linsky, Jack Smart and the reviewers for *Erkenntnis*.

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University of Tasmania
GPO Box 252-41
Tasmania 7001
Australia