

would be necessary to make possession of a language possible. But the idea that self-understanding requires translation is certainly not Davidson's, as his insistence on the asymmetry between solitary and interacting people makes clear;¹⁴ nor is it one that I see any independent reason to take seriously.¹⁵

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¹⁴ For other difficulties with Glock's interpretation of Davidson, see M. Alvarez, 'Radical Interpretation and Semantic Nihilism: Reply to Glock', *The Philosophical Quarterly*, 44 (1994), pp. 354–60.

¹⁵ Thanks to Robert Myers for his comments on earlier drafts of this paper, and to anonymous referees of this journal for their suggestions.

HOW NOT TO DEFEND CONSTRUCTIVE EMPIRICISM: A REJOINER

BY STATHIS PSILLOS

No doubt my earlier paper has struck a sensitive nerve among existing and prospective constructive empiricists – hence their united reply.¹ I shall, for brevity, introduce an imaginary single author of their critique and call him CE. In this rejoinder, I try to show, first, that CE's counter-arguments do not refute my original arguments; and second, that a claim of CE's paper is very close to the conclusion of my original paper.

A central point of my original piece was that there is a symmetry between scientific realism and constructive empiricism *vis à vis* van Fraassen's arguments from the bad lot and from indifference. Scholastic charges of an 'apparent misunderstanding' of 'empirical adequacy' do not cast any light on the issues at stake. (However, the notion of empirical adequacy I employed, p. 41, is the standard one: 'a theory is empirically adequate if and only if it saves all phenomena, past, present and future, and squares with all actual and possible observations'.) The issue between CE and myself is more substantive: CE relies on the thesis that for any theory there are 'indefinitely many empirically equivalent rivals' (p. 307), in order to infer that there are infinitely many empirically adequate theories, and then to argue that if realists

¹ S. Psillos, 'On van Fraassen's Critique of Abductive Reasoning', *The Philosophical Quarterly*, 46 (1996), pp. 31–47; J. Ladyman *et al.*, 'A Defence of van Fraassen's Critique of Abductive Inference: Reply to Psillos', this journal pp. 305–21 above.

want to claim that one of them is true, they need to appeal to an ‘indefinitely much stronger privilege’ than empiricists. I think this argument is flawed.

First, there is a slide here from empirical equivalence to empirical adequacy. When it comes to claims about empirical equivalence, all we *might* have is an argument that in a certain family of theories, if T_i implies certain observational consequences, so does T_j ($i, j = 1, 2, \dots$). If T_i and T_j are empirically equivalent, then if T_i is empirically adequate, so is T_j . At *any* given time, however, there is only a finite amount of data from which each T_i can draw support. At *any* given time, at best all we know of all theories in the family is that (i) they are unrefuted; and (ii) if a piece of evidence is entailed by one of them, it is also entailed by any other. Van Fraassen suggests that a theory should at best be accepted as empirically adequate. But he has noted: ‘If you accept a theory, you must at least be saying that it reaches its aim, i.e., meets the criterion of success in science (whatever that is).’² It should be clear, then, that accepting each and every theory in the given family as empirically adequate (given the finite set of data already available) *does* require some privilege: this family of theories has hit upon universal regularities in virtue of which each of its members can be projected to be empirically adequate. This privilege is indefinitely strong too, given that (a) there is an infinity of ways in which each T_i in the family can be refuted, and (b) there is an infinity of unborn theories which agree with each T_i on all *actual* data but entail different predictions about unavailable data. Does the realist claim that one of the T_i s is approximately true require even more privilege? I am not comfortable with infinities, but whatever extra privilege it requires, it is of the same type. Because if one assumes that claims about unobservables require a different *type* of privilege, that begs the question: it presupposes that coming to assert the truth of claims about unobservables is inherently different from coming to assert the truth of claims about observables.

Second, (ii) above does *not* entail that each piece of evidence supports equally well all theories in the family of empirically equivalent theories. To assume this is at best question-begging, since realists deny that empirical congruence entails epistemic congruence, and at worst false, since recent work has shown how common entailed consequences can differentially support theories.³ CE’s arguments casually move from the actuality of ‘empirically equivalent theories’ to the possibility of ‘equally good rivals’ (p. 309). But this is precisely the issue at stake: pointing to the existence of the former would do nothing to establish that empirically equivalent alternatives are ‘equally good’ or equally well supported by the evidence. I think this, if anything, is what needs to be dealt with in the realist argument.

In my original paper, I implied that if ‘horizontal’ inference to the best explanation (‘IBE’) is abandoned, commitments to unobserved but observable entities (e.g., a mouse in the wainscoting) would be left unsupported. Surprisingly, CE agrees with this conclusion: ‘the scepticism which is entailed by a rejection of IBE in general is simply accepted by van Fraassen’ (p. 319). However, CE also endorses the view that

² In ‘Theory Confirmation: Tension and Conflict’, *Seventh International Wittgenstein Symposium* (Vienna: Hölder-Pichler-Tempsky, 1983), pp. 319–29, at p. 327.

³ See L. Laudan, *Beyond Positivism and Relativism* (Boulder: Westview Press, 1996); and my ‘Naturalism without Truth?’, *Studies in the History and Philosophy of Science* (forthcoming).

‘a philosophical position which leads to scepticism reduces itself to absurdity’ (p. 317). Van Fraassen is said to accept the scepticism entailed by the rejection of any kind of IBE – be it about observables or unobservables – and yet he is also said not to be a sceptic about things whose truth we can see ‘in the immediacy of experience’ – hence not a sceptic ‘of the Cartesian variety’ (p. 319).

This position, however, leaves him with very little that he is not a sceptic about. If we do ‘see’ the truth about observed things in our experience, do we also see truths about unobserved-but-observables in the immediacy of experience? If anything, immediate experience is about observed things, not about unobserved but observable ones. When we posit unobserved but observable entities (e.g., when we claim that the present copy of *The Philosophical Quarterly* still exists when we go out of the library and cease to read it) we need to perform some kind of inference (rudimentary and unconscious though it may be) from what we immediately experience to an unobserved but observable thing that causes or sustains our immediate experiences (past and future). Similarly, positing extinct animals is surely reasonable, although they are ‘observable’ only in a very loose sense of the term. Yet the truth of such claims is by no means seen in the immediacy of experience of, say, fossils. If IBE is generally abandoned, then we are left with a poor epistemology that admits only judgements about observed things. Cartesian scepticism might well be evaded, but Humean scepticism is in the offing.

At this point CE retrenches: he claims that even if IBE about observables might, after all, be acceptable, it is problematic when it comes to unobservables, because in the former case, but not in the latter, ‘we do not routinely introduce new ontological commitments’ (p. 316). This is contentious. IBE about observables does involve the introduction of new types of entity. For instance, positing an extinct type of animal both is an instance of IBE and does introduce new ‘ontological commitments’. And IBE about unobservables does involve introduction of new instances of known types, e.g., instances of the virus HIV. At any rate, there is no reason why our epistemic attitude towards a posit should relate to whether it introduces instances of a new type of entity or instances of a known type. What matters, in either case, is that the posit is introduced to cement causally our ‘immediate experiences’.

What if CE is right in suggesting that judgements about unobserved observables could well be based on an empirically indistinguishable inference, an as-if IBE? How, then, are we to find out whether it was IBE or an as-if IBE that is being employed? If, as CE claims, the conclusions of an as-if IBE and of IBE are equivalent when it comes to claims about observables, then there is no need to choose between them: if an as-if IBE is reliable in its conclusions (in the restricted set of claims about observables), so is IBE. So if one doubts the reliability of IBE when it comes to claims about observables, then one should also doubt the reliability of its rival that is ‘apt in an anti-realist account’ (p. 314), and conversely. Strictly speaking, however, ‘There is a mouse in the wainscoting’ and ‘All observable phenomena are as if there is a mouse in the wainscoting’ are *not* equivalent. The former entails the latter, but not conversely. The pet cat Tom may perhaps be determined to make us think that there is a mouse in the wainscoting, so that we shall keep him. So, even at this level, we cannot just stay indifferent between ‘All observable phenomena are as if there is

a mouse' and 'There is a mouse'. We need to stick our necks out and endorse, after we balance things, the best explanatory hypothesis on which we shall base our future actions: shall we punish Tom, or install a mousetrap instead?⁴

I conclude with a few remarks on van Fraassen's 'new epistemology' (though this is an issue that needs to be dealt with in a separate article). In the present debate, it seems that the aim of the new epistemology is to allow constructive empiricists to move between rejection of IBE in general and the ensuing scepticism about anything other than observed posits. IBE can go; grounded judgements of empirical adequacy can go too; one does not even have to believe in the empirical adequacy of the theory while one remains agnostic about its truth (see p. 315); and yet scepticism is not forthcoming, because under the new epistemology beliefs need not be justified to be rational. '[van Fraassen] is not interested in warrant (i.e., the rationality of beliefs), but in the rationality of changes of belief' (*ibid.*). Van Fraassen has certainly done a lot of interesting work on this issue recently; I do not pretend to dismiss it. But I suggest that a full explication of rationality cannot just deal with belief-change. It is perfectly reasonable to argue that not all beliefs are equally rational, even though their entertainers might update them, say, via conditionalization. A creationist scenario is not, at least for some of us, equally as rational (warranted) as evolutionary theory, and it should be part of epistemology to say what it is that makes belief in the latter more rational (or more warranted) than belief in the former.

One of the central lines of 'new epistemology' is 'what is rational to believe includes anything that one is not rationally compelled to disbelieve' (*ibid.*). We still need to know what kinds of things one is rationally compelled to disbelieve, i.e., what kinds of beliefs are *not* warranted. A full theory of rational belief should certainly be open-minded and avoid dogmatism. But it should also allow for *comparative judgements*: some beliefs are more rational than others. Belief in the existence of middle-sized material objects should certainly come out as more rational than belief in the existence of sense-data and constructs of them. Whatever else it does, the 'new epistemology' should make this comparative judgement available. But if explanatory considerations contribute to making the belief in material objects more rational, then so much the better for my molecules. And this is exactly the point on which my own overall conclusion meets the conclusion that three-quarters of CE is willing to draw: if explanatory considerations are jettisoned, how can we ever be sure that the objects of perceptions actually exist, given only the phenomena? We are told that 'Three of the four authors of this paper see the issue as possibly raising serious problems for constructive empiricism and for van Fraassen's steps towards a new epistemology' (p. 320). They can count me in, too.⁵

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⁴ G. Harman, 'Pragmatism and the Reasons for Belief', in C.B. Kulp (ed.), *Realism/Anti-realism and Epistemology* (Lanham: Rowman & Littlefield, forthcoming), and T. Day and H. Kincaid, 'Putting Inference to the Best Explanation in its Place', *Synthese*, 98 (1994), pp. 271–95, esp. pp. 285–7, have already argued against van Fraassen's claim that IBE, conceived as a rule, is incoherent.

⁵ Many thanks to David Papineau and John Worrall for useful comments.