

A DEFENCE OF VAN FRAASSEN'S CRITIQUE OF ABDUCTIVE INFERENCE: REPLY TO PSILLOS

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In a recent contribution to this journal,¹ Stathis Psillos criticizes van Fraassen's arguments against abduction or inference to the best explanation (hereafter 'IBE'), a mode of reasoning underlying almost all current defences of scientific realism. According to Psillos, not only do van Fraassen's arguments fail to undermine IBE, if they were successful they would equally undermine his own empiricist position by reducing it to a bald scepticism. In this paper we argue that those arguments against IBE stand unrefuted and that Psillos fails to show that van Fraassen's renunciation of IBE reduces his position to bald scepticism.

IBE is, very roughly, the type of inference in which one derives the conclusion that explains the available evidence best. It is ampliative, in that it takes us beyond what can logically be inferred from the data. Psillos distinguishes between what he calls *horizontal* and *vertical* IBE: if one is inferring to the (probable and/or approximate) truth of an explanation which involves unobserved but in principle observable things, the IBE is said to be horizontal, whereas if one infers to an explanation involving unobservables, it is vertical. According to Psillos, it is solely vertical IBE which is disputed by van Fraassen. We shall argue below in §III that this is a mistake, and that Psillos makes several other important errors of interpretation. We shall then go on in §IV to point out the importance for the issues at stake of van Fraassen's broader epistemology, which Psillos ignores. Then we shall conclude with a brief look at the suspected relationship between constructive empiricism and scepticism (§V). But first we consider the two arguments against IBE discussed and criticized in Psillos' paper.

¹ S. Psillos, 'On van Fraassen's Critique of Abductive Reasoning', *The Philosophical Quarterly*, 46 (1996), pp. 31-47.

I. THE ARGUMENT OF THE BAD LOT

The argument of the bad lot purports to show that, even if it were in general the case that the best explanation of the evidence is true (or highly probable), that would not suffice by itself to make IBE acceptable as a rule of inference. For, evidently, the potential explanations between which we can choose are the ones we have actually come up with. So to conclude that the best of these is true an additional premise is required, *viz.*, that none of the possible explanations we have failed to come up with is as good as the best of the ones we have.

Psillos' presentation of the argument (p. 37) is contentious. He takes its main premise to be that 'it is more likely that the truth lies in the space of hitherto unborn hypotheses'. Then he argues that van Fraassen places too great a demand on the proponent of IBE, namely, to show that there is no possibility of error. Such a demand would imply too strong a notion of warrant required for the conclusions reached. Indeed it would; but Psillos both misrepresents the argument from the bad lot and concludes too much even from his own formulation.

First, if van Fraassen were saying that it is *more likely* that the truth will be outside the hypotheses available, then to rebut this the proponent of IBE would only need to argue that it is unlikely that this is so, rather than needing to argue that it is impossible that this is so, i.e., that there is no possibility of error. So if Psillos' summary of van Fraassen's argument is correct, then his claim about what van Fraassen is demanding cannot be.

If on the other hand we pay attention to the passage that Psillos quotes we see that what van Fraassen actually argues is that 'our best theory *may* well be "the best of a bad lot"',² not that it is more likely to be than not. This suffices for the argument, since the connection between the best available explanation and truth is only assured (and then only probabilistically, of course) if it is more likely that the truth lies inside the range of hypotheses being considered. Hence IBE cannot be rationally compelling unless we assume *privilege*, that is, that for some reason or other we are predisposed to hit upon the right hypothesis and include it in the range under consideration. So whereas Psillos challenges van Fraassen to show that it is more likely that the truth is outside the range, van Fraassen need only ask the proponent of IBE for reasons for believing that the truth is inside it.

² B. van Fraassen, *Laws and Symmetry* (Oxford UP, 1989, hereafter 'LS'), pp. 142–3 (our italics).

In fact Psillos seems to concede this, for he bites the bullet and contends that we *can* appeal to some kind of privilege at this point. Explicitly following Boyd in this, he argues that scientists do not have to think up hypotheses in a knowledge vacuum; they can draw on the available background knowledge, incorporated in already accepted theories. This information may drastically cull the number of theories among which the truth is to be found. As Psillos concedes, this appeal seems to beg the question. But he thinks that in a discussion with van Fraassen it is legitimate. For, he argues (p. 41), the empiricist will also have to invoke some kind of background-knowledge privilege. Without such a privilege, van Fraassen's argument backfires:

Let us suppose, for the sake of the argument, that scientists are not interested in choosing the theory which is more likely to be true, but, as van Fraassen would have it, that which is more likely to be empirically adequate. How can they know that the best theory that they have ended up with is not the most seemingly empirically adequate theory in a bad lot? In other words, how do they know that the real empirically adequate theory does not lie in the spectrum of hitherto unborn hypotheses?

These are rhetorical questions. Psillos wants us to answer that in constructive empiricism the scientist is seen as engaged in something like IBE, namely, inferences to the empirical adequacy of the best available hypothesis, and must therefore likewise rely on some assumption of epistemic privilege. He concludes (*ibid.*) that since 'even van Fraassen needs background beliefs in order to support his claims about empirical adequacy', the disagreement between the realist and the empiricist can only be over the extent of scientists' privilege.

We shall postpone to a later section the question whether this correctly represents van Fraassen's (or anyone's) view of what scientists are engaged in. Suppose *some* empiricist is willing to defend this. Then, because of an apparent misunderstanding of the term 'empirically adequate', Psillos' formulation conceals the extent to which this empiricist's appeal to background knowledge would differ from the appeal the scientific realist has to make. If it is correct, as van Fraassen thinks (see below), and as is also believed by some scientific realists,³ that there are to any scientific theory indefinitely many empirically equivalent rivals, then it is evidently wrong to speak of '*the* real empirically adequate theory' (our italics), as Psillos does (cf. also 'it is logically possible that *the* really empirically adequate theory lies outside the spectrum of theories that scientists have come up with', p. 37, our italics). There are, in that case, obviously indefinitely many empirically adequate

³ Cf. for instance R. Boyd, 'On the Current Status of Scientific Realism', in R. Boyd *et al.* (eds), *The Philosophy of Science* (MIT Press, 1991), pp. 195–222.

theories (every theory empirically equivalent to the true theory is empirically adequate). But then whatever privilege the scientist, as depicted by *that* empiricist, would have to appeal to in order to sustain his claim that at least one empirically adequate theory is among the ones we actually have, the realist would, as a matter of logic, have to appeal to an indefinitely much stronger privilege.

More importantly, it is not at all evident that the difference between the realist and the empiricist is, as Psillos thinks, just a matter of less or more of the same thing, and not a qualitative, principled difference. For even if the scientist (so depicted) could not get by without invoking some sort of privilege, why would that have to be an appeal to the *truth* of background theories rather than an appeal to their *empirical adequacy*?

Psillos briefly considers an empiricist retrenchment along these lines, but takes such a move to be completely wrongheaded. Certainly the realist takes an extra epistemic risk by believing the background theories to be (approximately) true rather than only empirically adequate. But although it must be granted to the empiricist that belief in the approximate truth of our background theories cannot be more secure than belief that these theories are empirically adequate, the former belief 'can be secure enough to warrant the extra risk that one takes in asserting that background theories are approximately true' (p. 42). Besides (*ibid.*),

taking an extra risk is the necessary consequence of aspiring to push back the frontiers of ignorance and to get to know more things, in particular about unobservable causes of the phenomena. In taking this extra risk, the realist wants to know more about scientific theories than the constructive empiricist.

The extra risk in question is taken in order to have a chance at something realists consider a great boon – knowledge, or at least true opinion, about 'unobservable causes of the phenomena'. Since empiricists notoriously see no value in this, and consider the character of this supposed boon to be enmeshed in philosophical confusion, Psillos is here at most preaching to the converted. Second, scientific realists do indeed have arguments for their contention that scientists draw on a belief in the truth of their accepted background theories, and that to have such a belief is the sole reasonable option. But all their better known arguments for this claim depend on IBE, the legitimacy of which is at stake.

Psillos' confidence that belief in the approximate truth of accepted theories 'can be secure enough' might seem to suggest that he has something new to say in defence of IBE. But he has not, at least here, and in fact he is quite explicit that it is not the aim of his paper to do so (pp. 32, 47). His *tu quoque* arguments against a view of science as driven by some putative

empiricist analogue to IBE are therefore inconclusive. They are also beside the point if the argument of the bad lot is considered simply by itself, as a critique of IBE, rather than in the context of some hypothetical empiricist epistemology which might be accompanying it.

II. THE ARGUMENT FROM INDIFFERENCE

The argument from indifference adds to the first that since, for every choice of a particular theory T as best explaining the evidence e , there will be (probably infinitely) many unborn hypotheses, inconsistent with T and with one another, which explain e at least as well, and since only one of these can be true, it is very improbable that the theory considered to be the best explanation is true (see *LS* p. 146).

Psillos responds (p. 43) that

in order to assert [that T ... is as probable as all other unborn potential explanations of e] one must first show that *there always are* other potentially explanatory hypotheses to be discovered, let alone that they explain the evidence at least as well.

Indeed, it seems that van Fraassen overplays his hand in claiming that T is just a random member of a (probably infinite) class of hypotheses all of which explain the evidence at hand just as well as T ; in any case he does not argue for it. (Psillos ignores the role actually played by this move in van Fraassen's critique. He quotes Armstrong's reaction 'van Fraassen is having a bit of fun here', but omits van Fraassen's response at *LS* p. 147.)

However, the argument from indifference can be reformulated in such a way that no supposition about the existence of T 's rivals is made while its essential point is left untouched.

First, let us assume for a moment that we are indeed privileged in the sense discussed earlier – none of the unborn hypotheses offers a better explanation of the evidence than the best of those which scientists have come up with. Even this would not suffice for the conclusion that IBE is acceptable. For that conclusion would require (at least) one further premise, *viz.*, that there is (almost) always a *unique* best explanation, i.e., that the ordering of explanations for e according to some standard of 'goodness' almost always has a greatest element. But what justification is there for this premise?

Second, and more importantly, for the argument from indifference to go through, it is irrelevant whether T *possibly* is a random member of a class of equally good explanations or whether T *actually* is a random member of such a class; the possibility that there may be equally good rivals to T already suffices to make an ampliative step from the evidence to T unwarranted.

It may be objected that, although the mere possibility that every theory has equally good rivals among the unborn hypotheses is sufficient for the empiricist's argument to hold good, mere possibility is not enough to make constructive empiricism an interesting rival to scientific realism (any more than the mere possibility that we are all brains in a vat can make scepticism an interesting epistemological position).

Third, however, we know that we are not dealing with a mere possibility here. Fundamental physics provides us with some well known examples of empirically equivalent theories. (Recently much in the limelight: Bohm's mechanics, which is demonstrably empirically equivalent to elementary quantum mechanics.) Of course, empiricism purports to be a general philosophy of science, not just an alternative philosophy of physics. Realists have recently argued that the occurrence of empirically equivalent rivals in physics may well be quite exceptional, because of some highly peculiar features of physics itself.⁴ Hence we cannot simply generalize from the situation in physics to the other sciences. Admittedly there is more to be said about the extent to which the argument from indifference challenges IBE.

In fact Psillos does have more to say about the argument from indifference. He claims (p. 45) that, if correct, the argument would undermine constructive empiricism no less than it would scientific realism. For, calling our best current theory T_{ea} , 'which we now project as empirically adequate', since constructive empiricists

aim to avoid bald scepticism and retain grounded judgements of empirical adequacy.... They ... need to resist the claim that ... T_{ea} ... is just a random member of the class of theories (most of which are hitherto unborn) that also save the phenomena. In order, however, to place T_{ea} in a privileged position *vis à vis* its unborn rivals, they must show that T_{ea} is much more likely to be empirically adequate than its unborn rivals.

But, Psillos goes on, such a judgement must be based on something in addition to the data, for *ex hypothesi* the data alone do not tell between T_{ea} and its rivals. But then why should scientific realists be denied an additional criterion for theory choice?

To start with, it can readily be seen that the cited passage is based on the same misunderstanding of the term 'empirical adequacy' as we have encountered earlier. How could van Fraassen, who apparently believes there to be indefinitely many equally good rivals to any scientific theory, ever want to argue that T_{ea} is privileged *vis à vis* its unborn rivals? If T_{ea} is really empirically adequate, then all unborn hypotheses which do equally well on

⁴ Cf. for instance S. Leeds, 'Constructive Empiricism', *Synthese*, 101 (1994), pp. 187–221; E. McMullin, 'Selective Anti-realism', *Philosophical Studies*, 61 (1991), pp. 97–108.

the data are *ipso facto* empirically adequate, and hence on a par with T_{ca} . However, some more serious misunderstandings underlie this passage; in fact, they underlie virtually all of Psillos' arguments. To these misunderstandings we now turn.

III. INTERPRETATION

Apart from the above, a serious flaw in Psillos' discussion is his particular interpretation of van Fraassen's aim in his critique of IBE. According to him van Fraassen attempts to show that IBE cannot provide epistemic warrant for hypotheses about unobservables, whereas it can for hypotheses concerning only observables. Psillos refers (p. 34) to the former case as vertical IBE and the latter case as horizontal IBE, and asks

Given that van Fraassen does not doubt horizontal IBE, ... what really is his objection to vertical IBE and the formation of warranted beliefs about the unobservable world?

The reading he gives of van Fraassen suggests the following answer: IBE only ever warrants belief in the empirical adequacy of a hypothesis – it is just that empirical adequacy coincides with truth in the case of horizontal IBE. But the rule that Psillos calls horizontal IBE was introduced in *The Scientific Image*⁵ as a foil, as part of a critique of IBE, and not as part of an empiricist epistemology. We shall come back to this below; more importantly, van Fraassen's assault on IBE in his recent work makes no distinction between horizontal and vertical forms. (Of course it may well be that constructive empiricism is ultimately untenable because of its reliance on a distinction between the observable and the unobservable, but this is not relevant to the particular issue that concerns us here.)

Neither of the arguments discussed in §§I–II above makes specific reference to vertical IBE as opposed to horizontal IBE. Rather the objective is to show that IBE in general is not the ideal of an ampliative *rule* of induction (that was 'baptised but never born', *LS* p. 132). For example, the section in *Laws and Symmetry* on IBE (p. 131) advances the view that 'both induction and IBE fail as rational bases for opinion and expectation of the future'. Reiterating what he had said earlier,⁶ van Fraassen argues (*LS* p. 132) that IBE cannot fulfil the ideal of a rule of induction that is *rationaly compelling, objective and ampliative*. In the section 'Why I do not believe in inference to the best explanation' we find the following (*LS* p. 142):

⁵ B. van Fraassen, *The Scientific Image* (Oxford UP, 1980), hereafter '*SI*'.

⁶ In 'Empiricism in the Philosophy of Science' ('EPS'), in P. Churchland and C. Hooker (eds), *Images of Science* (Univ. of Chicago Press, 1985), pp. 245–308.

Someone who comes to hold a belief because he found it explanatory is not *thereby* irrational. He becomes irrational, however, if he adopts it as a rule to do so, and even more if he regards us as rationally compelled by it.

Laws and Symmetry contains other arguments against IBE besides the two criticized by Psillos. In fact, probably the best known argument against IBE is van Fraassen's Dutch Book argument (pp. 160ff.), not discussed by Psillos, an argument to the effect that adopting IBE as a rule for belief revision must eventually make one's belief system incoherent. In this argument no reference is made to the distinction between what is and what is not observable, nor, correspondingly, to the distinction between truth and empirical adequacy. Indeed, the argument's conclusion is that the rule of IBE is unacceptable in general.

Therefore we claim (a) that there is no discrimination to be made between horizontal and vertical IBE – thus Psillos is wrong to think that van Fraassen's attack on IBE is selectively directed against inferences about unobservables; and (b) that van Fraassen's arguments are directed against IBE understood as a *rule* of inference, not as an inferential practice. IBE might be indispensable – to a certain kind of thinker, under certain conditions, perhaps – in acquiring reasonable expectations, and might thus be pragmatically indispensable, but that would not make it a rule of reasoning that issues in rationally compelled belief.

(It is interesting to note that the term 'abduction' was introduced by Peirce to refer to the process by which we decide which hypotheses are worthy of empirical attention, while 'induction' referred to the process by which hypotheses are tested. Thus the *Cambridge Dictionary of Philosophy* defines *abduction* as 'canons of reasoning for the discovery, as opposed to the justification, of scientific hypotheses or theories'. On such a view abduction belongs within pragmatics. In his comments on an earlier version of this paper Psillos protests that he cannot see the difference between IBE as 'inferential practice' and IBE as 'rule of inference'. The above suggests one way of articulating such a distinction, namely by distinguishing pragmatics from epistemology.)

How then do we explain the passage in *The Scientific Image* (pp. 19–20) where van Fraassen appears to endorse the use of IBE in reasoning about the observable?

It is argued that we follow this rule in all ordinary cases.... And surely there are many telling 'ordinary' cases: I hear scratching in the wall, the patter of little feet at midnight, my cheese disappears – and I infer that a mouse has come to live with me.

He goes on (p. 21):

For the mouse *is* an observable thing; therefore 'There is a mouse in the wainscoting' and 'All observable phenomena are as if there is a mouse in the wainscoting' are totally equivalent; each implies the other.

This section is read by Psillos as arguing that we can legitimately infer the existence of observable entities using IBE because in such cases empirical adequacy will coincide with truth. Thus he understands van Fraassen as advocating a rule of *inference to the empirical adequacy of the best explanation*, which happens to coincide with IBE where the hypotheses are restricted to those which do not quantify over unobservables. Were this the case, we would indeed need to ask why vertical IBE must be considered unreliable. However, it is not the case that van Fraassen is endorsing horizontal IBE here.

We need to understand the type of argument that this passage is intended to rebut. The section on IBE in *The Scientific Image* begins (p. 19) with the argument of Sellars, Smart and Harman that 'If we are to follow the same patterns of inference with respect to this issue as we do in science itself, we shall find ourselves irrational unless we assert the truth of the scientific theories we accept'. Many defences of realism similarly begin with the claim that IBE is fundamental to our normal inferential practice and to the inferential practice of scientists: theory-choice in science is often based on the relative ability of theories to explain the data in some domain. Thus if we accept the rationality of scientific practice, the argument goes, then we have to accept the rationality of IBE. If the theory in question refers to unobservable entities, then accepting its truth entails accepting the existence of these entities, hence the practice of IBE in science commits us to realism.

This discussion in *The Scientific Image* provides us with a way of reconstructing that practice in empiricist terms and blocking the defence of realism based on claims about how people ordinarily reason. It may indeed appear to be the case that we all use IBE routinely and that it is of particular importance in scientific reasoning. This appearance can be explained by the (psychological) hypothesis that we do use IBE. However, if those appearances admit of some alternative explanation as well, the realist cannot take that hypothesis for granted. It only takes one example to establish that the hypothesis has such a rival – for this purpose what Psillos calls horizontal IBE does very well. There may be still other explanations of those appearances; who knows? But even this one alternative presents a problem. Trying to decide between even these two alternatives on the basis of their explanatory power would of course court circularity at this point. If the 'obvious' hypothesis cannot be taken for granted, the realist argument loses its main premise.

The non-realist need not dispute that scientists routinely use IBE, in some way or other, but may say something like the following. Where scientists adopt theory *T* on the grounds of its explanatory power, the realist construes this to mean that *T* is true, but the non-realist can assert that *T* is merely empirically adequate or instrumentally successful. As *The Scientific Image* (pp. 20–1) puts it:

I can certainly account for the many instances in which a scientist appears to argue for the acceptance of a theory or hypothesis, on the basis of its explanatory success... We have therefore two rival hypotheses concerning these instances of scientific inference, and the one is apt in a realist account, the other in an anti-realist account.

The point of the mouse in the wainscoting example is that it ‘cannot provide telling evidence between the rival hypotheses’ (p. 21). Therefore merely displaying the *prima facie* nature of scientific inference does not tell us how to interpret and evaluate the results of such inference.

It is also worth noting the denial, in *The Scientific Image* and later, that there must always be some explanation for all the ‘persistent similarities’ in the phenomena, which equates the universal applicability of IBE with the universality of causal explanation. This is the point of van Fraassen’s various discussions of the EPR experiment, where he argues that the demand for an explanation for every regularity, made specific in the form of Reichenbach’s *principle of the common cause*, forces one to adopt a hidden variable interpretation of quantum mechanics. Again there is no discrimination here between vertical and horizontal forms of IBE.

To summarize, realists claim that the use of IBE in scientific practice, and acceptance of the rationality of that practice, forces us into realism. Van Fraassen attempts to show that, on the contrary, in the domain in which the use of IBE is commonplace, it can always be recast as a decision to believe in the empirical adequacy of a hypothesis and that this can be given a pragmatic justification. Psillos is wrong to think that this amounts to an endorsement of horizontal IBE. Therefore his main claim, that van Fraassen offers no reason to discriminate between vertical and horizontal IBE, is no criticism of van Fraassen’s position.

We admit that some passages in *The Scientific Image* concerning this point are at best ambiguous and perhaps even outright misleading. (One example is the use of ‘apt’ in the passage cited above, for this adjective admits of both stronger and weaker readings.) However, *Laws and Symmetry*, from which the arguments against IBE discussed in Psillos’ paper are taken, sets out a new epistemology in which the possibility of having grounded judgements or warrants, or of being ultimately justified in one’s beliefs, is given up explicitly and unconditionally. It is to this epistemology that we now turn.

IV. NEW EPISTEMOLOGY

The question to be addressed is this: if even horizontal IBE is rejected, then how can we retain 'grounded judgements of empirical adequacy', as van Fraassen allegedly wants? In that case, how can van Fraassen suggest, as Psillos claims (p. 34), that 'belief in the empirical adequacy of theories can be, and often is, warranted by the evidence'? Well, is there any reason to suppose that he does suggest this? Psillos refers (*ibid.*) to the section 'Sketch for an epistemology' in EPS, saying that here van Fraassen 'suggests that only belief in the empirical adequacy of theories can be ... warranted by evidence'. What van Fraassen actually says is that according to his theory of belief/opinion, the empirical adequacy of a hypothesis is always more credible than its truth. Realists often seem to think that given that a particular explanation is agreed to be the best explanation of the phenomena in question, and supposing its adequacy as an explanation, it is irrational not therefore to adopt it. This does not follow on the constructive empiricist view of science. But neither does it follow, on that view, that one should believe the theory to be empirically adequate while remaining agnostic about its truth. That epistemic attitude is presented, not as a doctrine that must be adopted on pain of irrationality, but as a position that may be adopted while accounting for all that we need to about science.

In explanation of this van Fraassen cites the distinction between so-called Prussian and English law: the former forbids that which is not specifically allowed, while the latter allows anything that is not specifically forbidden. (Is this still true?) There are analogously two conceptions of rationality. On the Prussian model 'what it is rational to believe is exactly what one is rationally compelled to believe'. On the English model 'rationality is only bridled irrationality ... what it is rational to believe includes anything that one is not rationally compelled to disbelieve' (*LS* pp. 171–2). Van Fraassen opts for the latter view, so according to him rationality is a *permission* term and not an *obligation* term. He is therefore not interested in warrant (i.e., the rationality of beliefs), but in the rationality of changes of belief.

But of course, as realists are fond of pointing out, even to believe that a theory is empirically adequate is to stick one's neck out to some extent: we never in fact know that *all* phenomena are as if something is the case, for we can never have access to all the possible observational contexts at once. In other words we can in fact only directly know that all *observed* phenomena are as if such and such ('experience can give us information only about what is both observable and actual', EPS p. 253). Thus there is a gap between the

evidence that we have and the conclusion that we draw from it. Bridging it requires a leap from the observed to the unobserved, and there is always the possibility of error. This is Hume's problem – what is the extra problem with unobservables?

We think it is clear that there can be an extra problem with IBE over and above Hume's problem. Even supposing that in everyday life we routinely use IBE to go beyond the observed phenomena, we do not routinely introduce new ontological commitments. In the case of the earlier example, *we already believe that mice exist*, that is, we use IBE to conclude new facts about tokens of types that are already included within our ontological commitments. (Several people have objected at this point that the particular mouse in question is not part of our ontological commitments and thus that we do use IBE to expand these. However, to admit the existence of a new type of entity is what is at stake in the realism debate, and this goes beyond what is at stake in everyday IBE.)

The realist of course thinks that it is arbitrary to accept the risk involved in inductive inference, but not in abduction to the existence of unobservables. Van Fraassen's response is that if we *need* go no further than belief in the empirical adequacy of theories to account for the nature and practice of science, then we take an unnecessary epistemic risk if we do go further, for no extra empirical gain. This gives rise to the infamous slogan 'It is not an epistemological principle that one might as well hang for a sheep as for a lamb' (*SI* p. 72).

In a recent article Kukla says of all this:

If van Fraassen's disdain is elevated to the status of an epistemological principle, it looks something like this: if two hypotheses are *empirically equivalent* and one is logically weaker than the other, then we should repudiate the stronger one.⁷

As Kukla correctly points out, such a principle could not be advanced in arguments against scientific realism. For one could have reason to adopt such a principle only if already committed to the view that empirical factors alone are epistemically significant, and this is what is denied by many realists. Moreover, the realist insists that realism issues benefits that constructive empiricism does not. After all, realists have explanations to offer for the phenomena we see around us which constructive empiricists have not, and they may claim, as Psillos does, that science has 'push[ed] back the frontiers of ignorance'.

Van Fraassen, however, is content to argue that *empiricists* should not be realists but should adopt constructive empiricism, because realism has no

⁷ André Kukla, 'Scientific Realism and Scientific Practice', *The British Journal for the Philosophy of Science*, 45 (1996), pp. 955–75, at p. 967.

more *empirical* goods to offer than his position has. Thus from an empirical point of view the extra strength of the realist position is illusory. If we are dealing with the unobservable, belief in empirical adequacy entails no less of an empirical nature than belief in truth does. Even if it is necessary to make inductive inferences, abduction gains us nothing further, for there is no further confrontation with experience that may tell in its favour beyond what supports the induction. What a theoretical explanation explains is not past observations but some regularity itself. To take an example from physics, the standard explanation of the Stern–Gerlach experiment is by a hypothesis about the spin of particles emitted by the source. This hypothesis implies (in the context of quantum mechanics and various auxiliary hypotheses) statistics for the ‘spin-up’ and ‘spin-down’ outcomes. But the empirical adequacy of that hypothesis (together with the assumed background theory and auxiliary hypotheses) implies the same statistics.

So van Fraassen rejects realism, not because he thinks it irrational, but because he rejects the ‘inflationary metaphysics’ that must accompany it, i.e., an account of laws, causes, kinds, and so on:

A person may believe that a certain theory is true and explain that he does so, for instance, because it is the best explanation he has of the facts or because it gives him the most satisfying world picture. That does not make him irrational, but I take it to be part of empiricism to disdain such reasons (EPS p. 252).

The misunderstanding we have just pointed to is a quite common one among realists. So let us briefly try to diagnose the source of the confusion. Van Fraassen articulates part of his controversy with the scientific realist in terms of the aim of science, saying (*SI* p. 12) that it is ‘to give us theories which are empirically adequate’, whereas for the scientific realist it is ‘to give us ... a literally true story of what the world is like’. On a first glance this may seem to suggest that van Fraassen thinks empirical adequacy to be a reachable aim for science. But of course that is not implied at all. In fact, he nowhere says that empirical adequacy is within the reach of science – nor that it is not. It is simply an issue van Fraassen does not address and *need not* address in order to make his point against the realist. Perhaps the most unambiguous way to state this point is thus: even if empirical adequacy should be an attainable goal for science, this does not mean that truth is attainable as well.

V. CONSTRUCTIVE EMPIRICISM AND SCEPTICISM

Scepticism is an ugly threat; a philosophical position which leads to scepticism reduces itself to absurdity. That is correct, though only, of course,

for a truly debilitating scepticism and not for just anything that anyone might regard as such. Psillos does not always distinguish his main targets, constructive empiricism, as a position opposed to scientific realism, and the epistemology which accompanies it. Let us begin with this distinction.

Constructive empiricism is not an epistemology but a view of what science is. That view characterizes science as an activity with an aim or point, a criterion of success; and it construes (unqualified) acceptance of science as involving the belief that science meets that criterion. The aim is not truth but empirical adequacy, according to this view. (Scientific realism is in this context understood as the contrary view, of the same form, which characterizes the activity as pursuit of truth, and unqualified acceptance of science as therefore involving the belief that its theories are true.) This view of science could accompany many different attitudes towards it, its value, its worthiness of acceptance, its chances of success. Traditionally, empiricists have both held up science as a paradigm for rational enquiry and been critical of its reach. Peter Forrest introduced a useful terminological distinction here:

scientific agnostic: someone who believes the science he accepts to be empirically adequate but does not believe it to be true.⁸

We can introduce its cognate contrary as

scientific gnostic: someone who believes the science he accepts to be true.

Constructive empiricists and scientific realists are two types of philosopher who have differing views of what science is, while scientific gnostics and agnostics need not be philosophers at all. The scientific gnostics' beliefs are always changing, as science changes, but the scientific realist's view of science stays the same throughout these changes. On the other hand, a scientific realist may have a very poor opinion of the science of his day, and a constructive empiricist might wish to believe a good deal more than is required by acceptance of current scientific theories. The tendency to confuse constructive empiricism with scientific agnosticism (and scientific realism with scientific gnosticism) has tended to exacerbate the scepticism issue considerably.

There is one obvious connection between the two cross-classifications. Scientific realists think that scientific gnostics truly understand the character of the scientific enterprise, and that scientific agnostics do not. Constructive empiricists think that scientific gnostics may or may not understand the scientific enterprise, but that they adopt beliefs going beyond what science

⁸ P. Forrest, 'Why Most of Us should be Scientific Realists: a Reply to van Fraassen', *The Monist*, 77 (1994), pp. 47–70; see further B. van Fraassen, 'Gideon Rosen on Constructive Empiricism', *Philosophical Studies*, 74 (1994), pp. 179–92.

itself involves or requires for its pursuit. As Forrest also pointed out in this connection, there is no disagreement about rationality involved here; it is not part of constructive empiricism to say that the adoption of such additional beliefs is irrational, just that it is more than what is involved in scientific theory-acceptance.

So, logically, constructive empiricists could be scientific gnostics. Logically speaking, also, they could be philosophers with no distinctively epistemological position – no views about what we ought to believe, or how we ought to adjust or manage our opinions. In both respects, such philosophers would be in a somewhat uncomfortable position. For scientific realists have argued mightily that we ought to be scientific gnostics, or that belief in empirical adequacy is unreasonable or even irrational outside belief in the truth of some explanation thereof, and they have done so on traditional epistemological grounds. Empiricists need to refute those arguments if they want any hope of aligning science with rationality or reasonable expectations. Thus empiricists face the challenge of formulating an epistemological position – but can presumably reject the challenge of justifying their position in traditional epistemological terms.

Let us finally turn to the charge that, if even the possibility of having warranted judgements of empirical adequacy is given up, we are left with a blanket scepticism. To start with, whatever 'blanket' (or 'bald') scepticism may mean in the context of this discussion, van Fraassen's scepticism is certainly not of the Cartesian variety:

we can and do see the truth about many things: ourselves, others, trees and animals, clouds and rivers, in the immediacy of experience (*LS* p. 178).

But yes, van Fraassen's disagreement with the scientific realist does run much deeper than is so often thought; it is not just about the possibility of justifying our beliefs about the unobservable part of the world. What this means, however, is that the scepticism which is entailed by a rejection of IBE in general is simply accepted by van Fraassen.⁹ Hence any attempt to reduce that rejection to absurdity along the lines of Psillos' attempt must fail.

In the face of this the realist may fall back on the following view expressed by John Worrall:

Nothing in science is going to *compel* the adoption of a realist attitude towards theories.... But this leaves open the possibility that some form of scientific realism, while strictly speaking unnecessary, is none the less the *most reasonable* position to adopt.¹⁰

⁹ See *Laws and Symmetry* ch. 7 §6, 'Between Realism and Sceptical Despair'.

¹⁰ J. Worrall, 'An Unreal Image', review of *SI*, *The British Journal for the Philosophy of Science*, 35 (1984), pp. 64–8, at p. 67.

Why then is realism the most reasonable position to adopt, according to the realist? Because, Worrall says (p. 67),

to take an analogy with *physical* realism, I know that in order to make sense of my sense perceptions I am not *compelled* to assume the existence of a real, external world; none the less, physical realism seems not only a reasonable position to take, but the only reasonable position to take.

This is contentious. Both Kant and Sellars, to take two widely spaced examples from history, gave well known arguments to the effect that phenomenalism is not a tenable or even coherent position. Experience is, phenomenologically, experience of myself among and confronted by things and events – perhaps it cannot be otherwise, perhaps this form is a precondition for the very possibility of coherent experience. It is at least curious to see the coherence of naïve phenomenalism so blithely assumed.

Devitt expresses a view similar in this respect to Worrall's: 'an argument that undermines Scientific Realism will also undermine Common-Sense Realism'.¹¹ And (*ibid.*),

It is common to think that abduction is the primary issue in the defence of Scientific Realism.... This is a mistake: abduction is not the primary issue, unless, perhaps, Common-Sense Realism is also in question.

Devitt claims that IBE is a red herring in the scientific realism debate and is not at stake unless common-sense realism is also. Since it is clear that IBE as a rule of inference is at stake, then so too must be the metaphysics which some philosophers, such as Devitt, claim is involved in common-sense realism. After all, if van Fraassen's argument works, how could we ever be sure that the objects of perception, such as jets, actually exist, given only the phenomena? Is not the existence of the jet supposed to explain the persistent similarities in the phenomena? Therefore do not all the good arguments for the existence of jets carry over to the existence of electrons?

Though the assumption involved in Worrall's and Devitt's discussion is contentious, it may be right. 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. As pointed out above, van Fraassen of course does not argue against common-sense realism. However, whether or not he intends to argue against it, denying the existence of sense-data (or the coherence of naïve phenomenalism) is not sufficient to establish the metaphysics of the world of common sense that philosophers like Devitt and most scientific realists want. If his position in epistemology makes the common-sense realism of philosophers, though not necessarily of common

¹¹ M. Devitt, *Realism and Truth*, 2nd edn (Oxford: Blackwell, 1991), p. 147.

sense, lack any justification, this is surely important. This is an issue that has not had the attention it deserves.

That van Fraassen allows his scepticism to stretch to hypotheses about the observable world will undoubtedly make his position even more unattractive in realist eyes than a general scepticism *vis à vis* the unobservable already is. However, to infer from this that constructive empiricism must be false and scientific realism true would presuppose an epistemological principle far more dubious than any discussed here, *viz.*, that of inference to the most appealing conclusion.¹²

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¹² See also S. Psillos, 'How Not to Defend Constructive Empiricism: a Rejoinder', this journal pp. 369–72 below.