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ON VAN FRAASSEN'S CRITIQUE OF ABDUCTIVE REASONING

BY STATHIS PSILLOS

It is also true that there are many things we can never find out without sticking our necks out. I've found this out in rock-climbing: often you cannot really discover whether something is really a foothold without committing yourself to it. Then if it isn't, you fall.¹

I. INTRODUCTION

Van Fraassen's innovative defence of empiricism in the philosophy of science (*SI*; *EPS*; *LS*) has made a *prima facie* case for the view that an epistemically optimistic attitude towards theoretical claims, typically associated with scientific realism, is not and can never be warranted. Belief in theoretical truth, he proclaims, is 'supererogatory' (*EPS* p. 255).

The novelty of van Fraassen's strategy consists in his straightforward attack on one of the most seemingly powerful ways to go beyond the phenomena and form theoretical beliefs, that is, inference to the best explanation [henceforth *IBE*]. Scientific realists have always suggested that *IBE* is the mode of reasoning that scientists follow in order to form their theoretical beliefs, and have argued that it can reliably produce and sustain (approximately) true beliefs about the world. In fact, various lines of defence of scientific realism against traditional instrumentalism were nothing but variants of *IBE*.² Van Fraassen flew bravely in the face of this tradition and made us all wonder whether we can trust our abductive practices.

¹ B. van Fraassen, 'Discussion', in J. Hilgevoord (ed.), *Physics and our World View* (Cambridge UP, 1994), p. 270. I refer to other works by van Fraassen as follows: *The Scientific Image* (Oxford: Clarendon Press, 1980) [*SI*]; 'Glymour on Evidence and Explanation', *Minnesota Studies in the Philosophy of Science*, 10 (Univ. of Minnesota Press, 1983) [*GEE*]; 'Empiricism in the Philosophy of Science', in P. Churchland and C. Hooker (eds), *Images of Science* (Univ. of Chicago Press, 1985), pp. 245-308 [*EPS*]; *Laws and Symmetry* (Oxford: Clarendon Press, 1989) [*LS*].

² See J.J.C. Smart, *Philosophy and Scientific Realism* (London: Routledge & Kegan Paul, 1963); G. Maxwell, 'The Ontological Status of Theoretical Entities', *Minnesota Studies in the Philosophy of Science*, 3 (Univ. of Minnesota Press, 1962); H. Putnam, *Mathematics, Matter and Method* (Cambridge UP, 1975).

I shall use the general term ‘abduction’ for the mode of uncertain reasoning in which one infers the (approximate) truth of the best explanation of the evidence, in so far as the best explanation is likely enough to allow an inference to be made. The aim of this paper is to evaluate some of van Fraassen’s recent arguments against abduction and to suggest that he has not offered good and compelling reasons to distrust it. In §II, I outline van Fraassen’s selective sceptical attitude towards forms of IBE that involve claims about unobservables. In line with some recent arguments by Menúge,³ I note that this partial scepticism cannot be justified on the basis of van Fraassen’s identification of the unobservable with the epistemically inaccessible.

Then I move on to discuss two of van Fraassen’s recent arguments against IBE. In §III, I examine his *argument from the bad lot*, and show that both scientific realists and constructive empiricists need to appeal to some sort of privilege in order to have grounded judgements of the epistemic goods they demand from scientific theories. The issue at stake between them, I argue, is the extent of this privilege. Finally, in §IV, I take on van Fraassen’s *argument from indifference*, and argue that judgements of approximate truth are no more affected by this argument than judgements of empirical adequacy. By the end of this paper, I hope, the reader will find it reasonable to believe that van Fraassen has failed to undermine abductive reasoning.

There is a general remark which I wish to make at the outset. The intent of this paper is critical rather than constructive. It does not offer a positive theory of the structure and substance of abductive reasoning.⁴ Nor does it offer a general defence of abduction against all possible sceptical arguments. Rather, the sole purpose of this paper is to establish that van Fraassen has not succeeded in undermining abduction in certain contexts. The friends of abductive reasoning have surely much more constructive work to do. However, if sound, the arguments of this paper will free them from an extra burden in their uphill battle.

II. CONSTRUCTIVE EMPIRICISM AND ABDUCTION

Van Fraassen admits that IBE can operate as a mode of inference in science, although he has insisted that the conclusion of such an inference, i.e., the hypothesis endorsed on the grounds that it is the best explanation of the evidence, is only accepted as empirically adequate (all observable pheno-

³ A. Menúge, ‘The Scope of Observation’, *The Philosophical Quarterly*, 45 (1995), pp. 60–69.

⁴ See S. Psillos, ‘Science and Realism: a Naturalistic Investigation into Scientific Enquiry’ (Ph.D. Dissertation: University of London, 1994).

mena are as the hypothesis says they are), as opposed to approximately true. As he put it (*SI* pp. 71–2):

explanatory power is certainly *one* criterion of theory choice. When we decide to choose among a range of hypotheses, or between proffered theories, we evaluate each for how well it explains the available evidence. I am not sure that this evaluation will always decide the matter, but it may be decisive, in which case we choose to accept that theory which is the best explanation. But, I add, the decision to accept is a decision to accept as empirically adequate. The new belief is not that the theory is true (nor that it gives a true picture of what there is and of what is going on plus approximately true numerical information) but that the theory is empirically adequate.

On van Fraassen's view, empirical adequacy replaces truth as the aim of science. Yet when the theory is solely about the observable world, empirical adequacy and truth coincide. Then to say that the theory is empirically adequate is to say that it is true (*ibid.*). It follows that when the explanatory hypothesis one arrives at by IBE is about observables, then any claim that this hypothesis is empirically adequate amounts to a claim that it is true.

Van Fraassen does not doubt that IBE operates reliably in many 'ordinary cases' which involve unobserved entities, like the well known case of the mouse in the wainscoting (see *SI* pp. 19–21). But then, he says, ordinary cases are about observable things like mice; and 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 (given what we know about mice)'.

The problem arises when the potential explanation involves reference to unobservable entities. Then empirical adequacy and truth no longer coincide. A given explanatory hypothesis and its *as-if* version do not imply each other. So it makes a difference to infer that *H* is true (e.g., "There is an electron in the cloud-chamber") rather than that *H* is empirically adequate ("All observable phenomena are *as if* there is an electron in the cloud-chamber"). If one infers that *H* is true, one is committed to the presence of electrons in the cloud-chamber; but not so if one infers that *H* is just empirically adequate.

So van Fraassen starts disputing IBE when the case at hand concerns explanatory hypotheses that stretch beyond the realm of observables. His doubt stems, as he stated recently, from his scepticism about 'general theories and explanations' that intend to give an account of the observable world in terms of unobservable entities and processes (*LS* p. 178). On his view, in an abductive problem where some unobservables are involved, the best explanatory hypothesis should be the one that should be chosen, but it must be at best entertained as empirically adequate. No pretension about its likely truth is warranted nor should be made.

Clearly, van Fraassen sustains a selective attitude towards IBE. The latter *is* a means of going beyond the realm of what has been actually observed and forming warranted beliefs about *unobserved* things or processes. Yet IBE *is not* a means of forming warranted beliefs about the realm of *unobservable* things or processes. In other words, van Fraassen claims that IBE does not warrant belief when the potential explanation of the evidence stretches to the unobservable world.

One should note here that van Fraassen is no Popperian. He does not want to exclude belief from science. Nor does he want to say that no beliefs are warranted by evidence. Rather he suggests that only belief in the empirical adequacy of theories can be, and often is, warranted by evidence (see EPS pp. 246–7, 276–81). Then, when empirical adequacy coincides with truth, belief in truth can also be warranted. What cannot be warranted, however, is belief in theoretical truth, i.e., in claims about unobservables.

For convenience, I distinguish two cases of abductive reasoning, as follows. I shall call *horizontal IBE* the species of abductive reasoning that involves only hypotheses about unobserved but observable entities, and *vertical IBE* that which involves hypotheses about unobservables. Given that van Fraassen does not doubt horizontal IBE, one can ask the following question: what really is his objection to vertical IBE and the formation of warranted beliefs about the unobservable world?

A widely discussed source of van Fraassen's worry against vertical abduction is his imposed dichotomy between observables and unobservables (see *SI* p. 16). He does not just want to point out that some things are visible to the naked eye while others are not. His distinction between observables and unobservables is intended to play an epistemic role. Despite his having dissociated observability from the old empiricist demand for a description in an observational vocabulary, his insistence on observability has been motivated by the empiricist view that 'our opinion about the limits of perception should play a role in arriving at our epistemic attitudes towards science' (EPS p. 258). He then suggests that the observable/unobservable distinction draws the borders between what is epistemically accessible and what is not, and that *all* statements about the unobservable world are undecidable, in that no evidence can warrant belief in theoretical claims about the unobservable world.

This aspect of van Fraassen's scepticism has been extensively discussed and heavily criticized by many philosophers of science. So for the rest of this section I shall content myself with some general observations. The key objection to the alleged epistemic relevance of the observable/unobservable distinction has rightly been this: it is wrong to suppose that the epistemic status of our beliefs about observables is in some way superior to that of our

beliefs about unobservables. Unaided senses can decide nothing but a tiny fraction of things, even observable things, about which we seem to have warranted beliefs (e.g., unaided senses cannot decide the rather elementary claim that the outer planets of our solar system are extremely cold).⁵

Besides, as Menuge has forcefully argued, it is wrong to suppose that observational beliefs are somehow immediately justified (or, worse, in no need of justification) in a way that theoretical beliefs cannot be. The point that Menuge brought into focus is that any plausible reason to think that a different *kind* of justification is always required for non-observational beliefs (e.g., beliefs based on instruments) would end up requiring this very kind of justification for observational beliefs as well. Suppose, for instance, that one were to argue that in order for an instrument-based belief to be justified one must first be justified in believing that the given instrument operates reliably. But exactly the same requirement can be imposed on the putative justification of eye-based beliefs, given that the human eye itself is a complex instrument known to be fallible. So how can we argue that eye-based beliefs are immediately justifiable while also holding the view that instrument-based beliefs require some extra justification? We should either consider both types of belief to be mediately justifiable, or else we should deny that observations with instruments always require an extra justification (see Menuge p. 68). What Menuge rightly concludes (pp. 66–7) is that there is no difference in quality between the evidence of unaided senses and that of instruments. Both can warrant belief, and sometimes beliefs based on unaided senses are less warranted than instrument-based beliefs.⁶

I think van Fraassen is right to claim that 'if we choose an epistemic policy to govern under what conditions, and how far, we will go beyond the evidence in our beliefs, we will be setting down certain boundaries' (EPS p. 254). What he has failed to establish, though, is that these boundaries justifiably include only claims about unobserved-yet-observables, and that they ought to exclude all claims about unobservables. As I have argued elsewhere,⁷ it is wrong to claim that all beliefs about unobservables are

⁵ See P. Churchland, 'The Ontological Status of Observables: in Praise of Super-empirical Virtues', in P. Churchland and C. Hooker (eds), *Images of Science*, pp. 35–47; A. Musgrave, 'Realism vs Constructive Empiricism', also in Churchland and Hooker (eds), pp. 197–221; W. Salmon, 'Empiricism: the Key Question', in N. Rescher (ed.), *The Heritage of Logical Positivism* (Lanham: Univ. Press of America, 1985); W. Newton-Smith, 'Realism and Inference to the Best Explanation', *Fundamenta Scientiae*, 7 (1987), pp. 305–16; A. Grobler, 'Van Fraassen's Metaphysical Move', *International Studies in the Philosophy of Science*, 5 (1991), pp. 21–34.

⁶ Apposite examples are given in C. and C. Chihara, 'A Biological Objection to Constructive Empiricism', *British Journal for the Philosophy of Science*, 44 (1993), pp. 653–8.

⁷ S. Psillos, 'A Philosophical Study of the Transition from the Caloric Theory of Heat to Thermodynamics: Resisting the Pessimistic Meta-Induction', *Studies in History and Philosophy of Science*, 25 (1994), pp. 178–83.

unsupportable by evidence. Some theoretical claims are better supported by evidence because, for instance, they are used centrally in the derivation of predictions and explanations of phenomena. Whereas some other claims are not well supported by the evidence, because, for instance, they are 'idle' components of a scientific theory, or serve merely as visualizations of putative causes, unable to generate any predictions. I think then that the boundaries of beliefs supportable by evidence are not to be circumscribed in such a way that they involve only observable entities.

The general moral I want to draw so far is this. It is one thing to demand some caution in knowledge-claims about the unobservable world, especially in the light of the fact that scientists have been in error in some of their beliefs about it, but it is quite another thing to adopt an implausible position which excludes from knowledge any claim that goes beyond what can be observed by the naked eye, felt, etc. Sceptical philosophers, van Fraassen in particular, are right to point out the need for caution, but wrong in so far as their demand for caution leads them to ban any knowledge whatsoever of the unobservable world. To say that *no* evidence can warrant belief in a claim that involves unobservable entities, to say that all claims about unobservables are inherently insupportable, is not to adhere to empiricism; it is dogmatism.⁸ It amounts to a desk-thumping thesis that because something is too little, or too attenuated, to be visible to the naked eye, it lies for ever beyond our epistemic reach.

Yet van Fraassen has recently produced two interesting arguments to the effect that truth cannot be had in any sort of vertical abductive problem, even if scientists are able to specify and choose the best explanation. I shall consider them in turn.

III. VAN FRAASSEN'S ARGUMENT FROM THE BAD LOT

The first argument, which I shall call *the argument from the bad lot*, is this:

Let us grant that scientists have effected an ordering of a set of theories T_1, \dots, T_n , all of which offer potential explanations of the evidence e and that they have sorted out which is the best explanation of e , say T_1 . In order for them to say that T_1 is the approximately true account of e , they must make 'a step beyond the comparative judgement that [T_1] is better than its actual rivals'. They must make 'an ampliative step'. This step involves belief that the truth is already more likely to be found within the lot of theories available to them, than not. But our best theory may well be 'the

⁸ Salmon, *op. cit.*, and also in 'Carnap, Hempel, and Reichenbach on Scientific Realism', in W. Salmon and G. Wolters (eds), *Logic, Language and the Structure of Scientific Theories* (Univ. of Pittsburgh Press, 1994), has convincingly argued that Reichenbach's empiricism was consistent with his belief that unobservable entities exist and can be known.

best of a bad lot'. So, in order for the advocate of IBE to argue that IBE leads to truth, he must *assume* a Principle of Privilege. That is, he must assume that 'nature pre-disposes us to hit on the right range of hypotheses' (LS pp. 142–3).

In brief, van Fraassen's point is that unless an unwarranted privilege is appealed to, it is more likely that the truth lies in the space of hitherto unborn hypotheses.

Any sensible model of abduction must not exclude the *possibility* that the truth might lie outside the set of theories that scientists have come up with. For surely there is no *a priori* warrant that scientists will hit on the truth. But one of the issues at stake here is this: should one first eliminate the possibility that the truth might lie outside the theories that scientists have come up with, before one argues that there are good reasons to believe that the truth lies within this range of theories? If this is what van Fraassen demands, then I must say that he operates with a very strong notion of warrant, so strong as to render unwarranted even beliefs about empirical adequacy. For it is logically possible that the really empirically adequate theory lies outside the spectrum of theories that scientists have come up with. So would van Fraassen say that unless this possibility is excluded, no belief in the empirical adequacy of a given theory is warranted? There is nothing wrong with such an answer apart from the fact that it leads to bald scepticism: very few beliefs, if any, can be warranted if warrant involves elimination of the possibility that the belief may be false. I do not think he can afford to have such a strong notion of belief-warrant without being an outright sceptic.

However, there is a point that the friends of abduction must concede. The history of science suggests that the *whole* truth (whatever that means) regularly lies outside the range of theories scientists consider at a given period. Even our best-supported theories can only be held to be approximately true. Yet, this admission, sound though it is, does not undermine abduction. All that it concedes is that, at any given stage of the scientific enquiry, scientists have come up with only *part* of the truth, and further truths are to be discovered. Then what the friends of abduction, normally scientific realists, need to show is that, contrary to van Fraassen's suggestion, the best explanatory hypothesis can be warrantably believed to be approximately true. In the next few paragraphs I shall argue for precisely this point.

As we have seen, the argument from the bad lot suggested that unless an unwarranted privilege is appealed to, it is more plausible to believe that the truth lies outside the spectrum of theories scientists have come up with. I think the best defence of vertical IBE is to go on the offensive. In response to van Fraassen, the realist can state that there is a sense in which we are privileged, and warrantably so. This is what I shall call the *background knowledge privilege*.

One should observe that the argument from the bad lot works only on the following assumption: scientists have somehow come up with a set of hypotheses that entail the evidence – their only relevant information being that these hypotheses just entail the evidence – and then they want to know which if any of the hypotheses is true. If this situation were representative of what goes on in an abductive problem, then, admittedly, scientists would not have the slightest clue as to whether any of these theories is likely to be approximately true. Even if they could specify which theory is the best explanation of the evidence, according to some criteria of ‘bestness’, they could not associate the best explanation with the likeliest one. However, as both Boyd and Lipton have persistently argued,⁹ it is at least doubtful and at most absurd to hold that theory-choice operates in such a knowledge-vacuum. Rather, theory-choice operates within and is guided by a network of background knowledge. An actual scientific example can illustrate this claim.

After the discovery and successful explanation of the phenomena of interference and diffraction, the wave theory of light began to supersede the emission theory in explanatory power. Light was believed to consist of waves, but the wave theory left it open whether the waves were longitudinal or transversal or both. In particular, given the successful wave theory of sound, it was taken, for instance by Young and Poisson, that light-waves were longitudinal, like sound-waves. Before the discovery of the phenomenon of polarization of light, the hypothesis that light-waves are longitudinal accounted for some phenomena of light propagation. But the phenomenon of polarization forced upon scientists the belief that light-waves exhibit sidedness, which could not be explained unless one accepted the hypothesis that light-waves have at least a transversal component.

In 1816, Fresnel and Arago discovered¹⁰ that two light-rays polarized at right angles to each other do not interfere, whereas two light-rays polarized parallel to each other do produce interference fringes. According to Fresnel, given the background wave theory of light, this phenomenon could be explained on the assumption that light-waves are purely transversal. However, there was an alternative hypothesis that entailed the evidence, namely that light consists of both transversal and longitudinal waves. This

⁹ R. Boyd, ‘The Current Status of the Realism Debate’, in J. Leplin (ed.), *Scientific Realism* (Univ. of California Press, 1984); and ‘*Lex Orandi est Lex Credendi*’, in P. Churchland and C. Hooker (eds), *Images of Science* (Univ. of Chicago Press, 1985), pp. 3–34; P. Lipton, *Inference to the Best Explanation* (London: Routledge, 1991); and ‘Is the Best Good Enough?’, *Proceedings of the Aristotelian Society*, 93 (1992–3), pp. 89–104.

¹⁰ F. Arago and A. Fresnel, ‘On the Action of Rays of Polarized Light upon Each Other’, *Annales de chimie et de physique*, 10 (1819), p. 288, trans. in F. Crew (ed.), *The Wave Theory of Light* (New York: American Books Co., 1902).

hypothesis provided a potential explanation of the phenomena, but this explanation was poorer than the one offered by the hypothesis that light-waves are exclusively transversal. And it was poorer because, although it entailed the observed phenomena of interference, by positing longitudinal waves it also created new intractable explanatory difficulties:

We both felt that these facts would be explained very simply, if the vibrations (oscillatory movements) of the polarized waves took place in the plane itself of these waves [i.e., if they are transversal]. But what became of the longitudinal oscillations along the light beams? How were these oscillations destroyed by the polarization phenomenon and why did not they reappear when the polarized light was reflected or refracted obliquely on a glass plate?¹¹

What Fresnel in effect stressed was that the hypothesis that light-waves have both a transversal and a longitudinal component would also have to account for the disappearance of the longitudinal wave after the light-wave had gone past the polarizer. The contrary hypothesis that light propagation is a purely transversal process would not have this extra burden: it explained the phenomenon of polarization more simply, more completely and without needing any *ad hoc* manoeuvre. Hence Fresnel accepted what he called (p. 786) 'the fundamental hypothesis', namely that the propagation of light is a uniquely and exclusively transversal process. This hypothesis was singled out as the best explanation of the phenomenon of polarization and was accepted as the correct account of the phenomenon of polarization. As I have shown elsewhere,¹² Fresnel's 'fundamental hypothesis' became itself part of the new background knowledge that constrained explanations of other light phenomena.

This case drives home two important aspects of what I earlier called 'the background knowledge privilege'. The first is that background knowledge can drastically narrow down the space for hypotheses that provide a potential explanation of the evidence at hand. (In the foregoing case, Fresnel ended up with two potential explanations of the Arago–Fresnel effect.) The second aspect is that when the background knowledge does not suggest just one theoretical hypothesis, then explanatory considerations, which are part and parcel of scientific practice, are called forth to select the best among the hypotheses which entail the evidence. (Here Fresnel's explanatory considerations dramatically favoured the hypothesis that light-waves are uniquely transversal.) I think both aspects of the 'background knowledge privilege' make it plausible that, contrary to van Fraassen's claim, scientists can have

¹¹ A. Fresnel, 'Considérations mécaniques sur la polarisation de la lumière', in *Oeuvres complètes*, Vol. 1 (Paris: Imprimerie Impériale, 1866), p. 629.

¹² S. Psillos, 'Conceptions and Misconceptions of Ether', in M.C. Duffy (ed.), *Physical Interpretations of Relativity Theory* (Univ. of Sunderland, 1992), pp. 544–56; and in my 'Science and Realism'.

strong evidence for the belief that the best explanation is the correct account of the phenomena.

Van Fraassen could challenge my appeal to background knowledge in abductive problems on the ground that the background beliefs may not be approximately true after all: they might again have been the best of a bad lot. However, van Fraassen's challenge would rest on a dubious and, I think, incorrect assumption, *viz.*, that evidence can *never* guide scientists to form (approximately) true theoretical beliefs. Even though evidence does not entail theoretical beliefs, it can *support* some theoretical beliefs up to a high degree, so that it would be unlikely that the beliefs are outright false and the evidence what it is – as happened in the example discussed. It is true that the probability of (and, I think, the degree of confidence in) a theoretical belief will be *at most* as high as the probability of the evidence it entails. And it is also true that the probability associated with a theoretical belief can hardly ever be unity. But this does not mean that it can never be high. Hence the fact that the probability of a theoretical claim can be at most as high as the probability of the evidence it entails does *not* mean that scientists can never have a warranted high degree of confidence in a theoretical claim in the light of the supporting evidence. Those beliefs for which scientists acquire overwhelming supporting evidence augment the mass of warranted background beliefs and become the pivots for new warranted beliefs.¹³

At this point, the reader might object that perhaps the issue at stake is *whether* scientists operate within an environment of approximately true background beliefs; my arguments may seem to have begged this question.

By way of addressing this objection, it seems to me relevant to distinguish between (i) the *general* sceptical (Humean) worry of how one goes about vindicating an ampliative mode of inference such as induction or IBE without begging the question, given that a kind of circularity is involved in such a vindication; and (ii) van Fraassen's *particular* sceptical worry that in order to have the cake of vertical abductive reasoning and eat it (i.e., show that it tends to generate approximately true beliefs), the friends of abduction must grant themselves an unwarranted privilege.

Providing a good positive argument for the reliability of IBE which addresses the Humean worry is an uphill task (but, I think, no more so than defending the reliability of normal inductive inferences), which falls outside the scope of this paper.¹⁴ Yet, although I do think that the friends of abduction must ultimately address the general sceptical worry, I also think that

¹³ I have discussed these issues in detail in my paper in *SHPS* 1994. For some similar thoughts see J. Norton, 'The Determination of Theories by Evidence: the Case for Quantum Discontinuity 1900–15', *Synthese*, 97 (1993), pp. 1–31; P. Forrest, 'Why Most of Us Should Be Scientific Realists: a Reply to van Fraassen', *The Monist*, 77 (1994), pp. 47–70.

¹⁴ But see D. Papineau, *Philosophical Naturalism* (Oxford: Basil Blackwell, 1993), ch. 5.

they can defend themselves against van Fraassen's argument from the bad lot. For as I shall now show, the issue at stake between van Fraassen and the realists is *not* whether scientists operate at all within an environment of correct background beliefs; rather the issue is the *extent* of their correct background beliefs. Even van Fraassen needs background beliefs in order to support his claims about empirical adequacy. So I think my arguments so far do not beg the question in the debates with van Fraassen. All they suggest is that scientists are more privileged than van Fraassen thinks.

In order to see that the issue at stake is the extent of scientists' background knowledge, let me ask the following question: is the background knowledge privilege excess baggage that only a realist seems to need to take on board? Or do not van Fraassen's claims about empirical adequacy also require some similar sort of privilege?

The privilege that a realist was said to need was that part of the truth lies already in background beliefs relative to which scientists are to choose their best explanatory theory. 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 theories?

There is a *symmetry* between realism and the constructive empiricism with respect to the argument from the bad lot. The constructive empiricist notion of empirical adequacy is that 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. It is perfectly possible that the best theory available now, which squares nicely with a finite number of actual observations and phenomena, may cease to do so with future phenomena, or with possible observations in space-time regions where it has not been tried yet, or with possible data on which it has not been tested yet. In the light of this possibility, would constructive empiricists say that a theory which saves the actual data that has been tried is empirically adequate *simpliciter*? If so, that would violate their own understanding of empirical adequacy. So in order to claim that the best currently available theory is empirically adequate, an ampliative claim is needed, asserting that scientists have already hit upon an empirically adequate theory. In particular, it would have to be claimed that it is unlikely that a theory which squares with observations up to now will cease to do so in the future, or in not yet tried space-time regions. That would force an appeal to the existence of universal regularities between phenomena, and to some principle of privilege which asserts that the theory

has hit upon them: it must be in virtue of this fact that the theory that saves a given range of phenomena *is* empirically adequate. In all this, constructive empiricism would appeal to a background knowledge privilege, of the kind denied to realism. Hence constructive empiricism cannot afford to deny that there is a background knowledge privilege, and must therefore concede that, to some extent, scientists operate in an environment of correct background beliefs. What is in dispute is the *extent* of their privilege. It is for this reason that I think my arguments do not beg the question.

Constructive empiricism may retrench here: it may be argued that some sort of privilege is needed to ground the judgement that current theories are empirically adequate, but that this involves less epistemic risk than asserting the privilege required by realism. In my inferential practices, van Fraassen could say (see *SI* p. 72), if I am to be hanged, why should I be hanged for a sheep and not for a lamb? Obviously, it is less risky to assert that there are universal regularities between phenomena, and that if a theory has hit upon them, then it is going to be empirically adequate, rather than asserting that a theory is approximately true.

Problems of epistemic risk are interesting because they contrast with problems of security: the more one is willing to believe, the more are the ways in which one can be in error. It is quite important that one be secure in one's beliefs, in the sense of having good warrants for what one believes. But it does *not* follow from this that one's belief in the approximate truth of background scientific theories is not secure. It may be at most as secure as beliefs in mere regularities (since the approximate truth of background theories entails the existence of universal regularities). Yet the belief in the approximate truth of background theories can be secure enough to warrant the extra risk that one takes in asserting that background theories are approximately true.

Note that epistemic risk contrasts also with ignorance: the less one is willing to believe, so that one minimizes one's probability of error, the less one pushes back the frontiers of ignorance. Undeniably, realists take an extra epistemic risk when they say that background theories are approximately true; but 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. So the latter is unjustified in suggesting that this risk is not worth taking on safety grounds for two reasons: first, this is also to take an inductive risk which goes beyond current evidence; and second, if risk is the price for pushing back the frontiers of ignorance, then, as the motto of this paper suggests, it is a price well worth paying.

Generally, avoiding ignorance is as important as avoiding error. So the challenging task is not to avoid error at the price of remaining ignorant but to find a compromise between avoiding error – that is making one's beliefs as secure as possible, and avoiding ignorance – that is, acquiring warranted beliefs about more things.

IV. VAN FRAASSEN'S ARGUMENT FROM INDIFFERENCE

I now turn to van Fraassen's second argument against IBE (*LS* p. 146). I call this 'the argument from indifference':

Let us grant that we have chosen the theory T that best explains evidence e . A great many of unborn hypotheses inconsistent with T explain e at least as well as T . Only one theory, either T or one of the hitherto unborn theories, is true. All the rest are false. Since concerning T we know nothing with respect to its truth-value other than it belongs to the (probably infinite) class of theories that explain e , we must treat it as 'a random member of this class'. But then we may infer that T is very improbable.

Responding to an earlier version of the argument from indifference, Armstrong said, quite nicely, 'I take it that van Fraassen is having a bit of fun here'.¹⁵ I think Armstrong is quite right. Van Fraassen's argument rests on a very controversial assumption, *viz.*, that the only thing we know about the best explanatory theory T is that it belongs to the (probably infinite) class of theories that explain e equally well. But this is absurd. Note that van Fraassen grants that T has passed several tests and has been qualified as the best explanation of e . Then he claims that T (the best available explanation of evidence) is as probable as all other *unborn* potential explanations of e . Yet in order to assert this 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. But how do we know this in advance? Of course, it is no surprise to argue that there always are trivial alternatives to T that entail the evidence, e.g., notational variants of T , or theories that are formed by just tacking things on to T . But this can hardly support the claim that T is as probable as all these alternative hypotheses. And at any rate, T would be as probable as all those alternatives only if the sole thing that counted towards the probability of a theory is that it entails the evidence. Yet why should one accept this hypothetico-deductive theory of confirmation in the first place? Relatedly, even if we granted that there always are hitherto unborn potential explanations of e , what shows us that they are as

¹⁵ D. Armstrong, 'Discussion: Reply to van Fraassen', *Australasian Journal of Philosophy*, 66 (1988), p. 228.

good explanations of the evidence as the one offered by T ? And if they are not, they are not as probable as T .

It is only reasonable, I think, to demand that any alternatives to T should be scientifically interesting, in the sense that the scientific community has independent theoretical reasons to accept them as genuine empirically equivalent rivals to T . For only then is there a serious issue as to whether and why scientists should believe in one theory rather than the other. Imagine a case in which there are two rivals T and T' which no current evidence, nor explanatory consideration, can distinguish. Then temporary suspension of judgement should definitely be the right attitude, while searching for further discriminatory evidence. But the argument from indifference goes far beyond this sound attitude. It seeks to establish that a permanent suspension of judgement is the right attitude towards a theory that provides the best explanation of the evidence, on the grounds that there are unborn hypotheses that explain the evidence at least as well. This is, however, an assumption which cannot be just taken for granted. Van Fraassen needs to argue for it. In particular, he needs to show that for any theory there is a non-trivial alternative such that the two theories are indefinitely indiscriminable by any evidence and the application of any method.¹⁶

What about the claim that the history of science is full of cases where a once-explanatory theory T has been replaced by another unborn at the time when T was entertained? I think this claim would support the argument from indifference only if abandoned theories were characteristically false. But, as I have shown elsewhere (see fns 7, 12 above), there is a strong case to be made for the approximate truth of some past mature scientific theories.

Van Fraassen could always appeal to his own theory of explanation to support his argument from indifference. He could first remind us of a difference between informational and confirmational virtues of theories: the fact that a theory T is more informative than T' does not make T more likely than T' . He could then argue that although explanatory power is indeed a virtue which stretches beyond a theory's ability to square with the phenomena and offers reasons to accept a theory, it is an *informational* virtue of a theory. So since no informational virtue raises the belief-worthiness of a theory, neither does explanatory power (see GEE pp. 166–9; EPS pp. 247, 280; *LS* pp. 185, 192).

Sweeping explanation under the carpet of information like this is, however, contentious. Van Fraassen is surely right to note that a potential explanation offers information about the putative causes of the phenomena, and that this fact does not *ipso facto* make an explanation likely. Nevertheless,

¹⁶ For a fuller discussion of these issues see L. Laudan and J. Leplin, 'Empirical Equivalence and Underdetermination', *Journal of Philosophy*, 88 (1991), pp. 449–72.

acquiring this putative information is just the first step in scientists' quest for well confirmed theoretical beliefs. If the explanatory hypothesis is rigid enough, so that it cannot be the product of *ad hoc* adjustments, if it coheres with other background beliefs that are well supported by the evidence, if, moreover, it yields novel predictions or unites hitherto unrelated phenomena, then one can claim that this hypothesis is better supported than another which either remains silent or gives a poorer explanation.

Let us, for instance, consider a case where there are ten theories $T_1 \dots T_{10}$, each of which explains a single phenomenon e_i ($i = 1 \dots 10$). Let us also imagine that a scientist proposes a grand theory T^* that unites all these diverse theories and explains all phenomena that they explained. T^* may also entail a great deal more than the individual theories. T^* is surely more informative than every single individual theory, even more informative than their mere conjunction, and this is definitely a virtue of T^* . However, it is arguable¹⁷ that the fact that T^* unites hitherto unrelated phenomena (or domains), and yields novel predictions, has also significant *confirmational value*. The fact that, on purely probabilistic grounds, the probability of T^* is less than or equal to the probabilities of each individual theory $T_1 \dots T_{10}$ (since T^* entails each of those) does not show that the probability of T^* cannot be high enough to warrant belief.

Hence it is not the mere fact that a theory tells an informative story that makes it likely. Rather, it is some features of the potential explanation which, having confirmational value, increase the theory's probability. Van Fraassen is too quick to sweep the features that an explanation has under the carpet of informational virtues and dismiss, out of hand, the relevance of these features to confirmation.

It is also noteworthy that the argument from indifference, if interesting at all, is symmetrical with respect to both scientific realism and constructive empiricism. As I noted before, van Fraassen wants to have grounded judgements of empirical adequacy. He wants to claim that current theories are empirically adequate, yet to suspend his judgement as to their truth-value. However, judgements of empirical adequacy are no less susceptible to the argument from indifference than judgements of truth. For suppose that we take the best theory T_{ea} which we now project as empirically adequate. Of course, there is an infinity of other theories which are consistent with the finite data that T_{ea} saves. All these theories differ from T_{ea} only in some observable respects, e.g., T'_{ea} states that in the mouth of the first black hole to the west of our galaxy there is a white raven (or, indeed, T'_{ea} is a variant of T_{ea} that involves *gruesome* predicates). However, only one of these theories

¹⁷ See M. Friedman, *Foundations of Space-Time Theories* (Princeton UP, 1983).

is really empirically adequate. Since the only thing that we know with respect to the empirical adequacy of our best theory T_{ea} is that it belongs to the (probably infinite) class of theories that save the available data, we may treat T_{ea} as a random member of this class, and hence we may conclude that T_{ea} is unlikely to be empirically adequate.

Constructive empiricists are no more at ease with the argument from indifference than realists. They aim to avoid bald scepticism and retain grounded judgements of empirical adequacy. They therefore need to resist the claim that the best available theory T_{ea} that currently saves the phenomena 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. Yet such a judgement cannot be solely based on the available evidence since, by hypothesis, T_{ea} as well as all of its unborn rivals save exactly the same evidence. So the belief that T_{ea} is more likely to be empirically adequate than its unborn rivals should be based on the claim that T_{ea} possesses some potentially confirmatory theoretical virtue (e.g., simplicity or explanatory power) which its rivals do not possess. Constructive empiricists could claim that it is because of this fact that they are justified in believing that T_{ea} has latched on to universal regularities, and therefore could use this claim to ground the judgement that T_{ea} is empirically adequate. But then how can they avail themselves of such theoretical virtues while denying the same thing to realism?¹⁸

I think the constructive empiricist's position *vis à vis* the argument from indifference differs only in degree from the realist's. The latter finds absurd the claim that the best available theory is as likely to be (approximately) true as all hitherto unborn hypotheses, whereas the former finds absurd the claim that the best available theory is as likely to be empirically adequate as all hitherto unborn hypotheses. But in order to have grounded judgements of the epistemic goods they demand from scientific theories, both need to appeal to some non-empirical yet potentially confirmatory theoretical virtues. As for the difference in the risk involved in their respective claims, I think I have taken care of this objection in the previous section.

I conclude then that the argument from indifference fails to establish that one should treat the best available explanation as a random member of the class of (mostly unborn) potential explanations of the evidence. In fact, it turned out that if this argument were sound it would prove too much. For it can also be equally effective against van Fraassen's attempt to keep hold of grounded judgements of empirical adequacy.

¹⁸ A similar point is made by Musgrave, in Churchland and Hooker (eds), pp. 202–3.

V. CONCLUDING REMARKS

As I said in the Introduction, this paper has aimed to show that van Fraassen has not offered good reasons to distrust vertical IBE. I have pursued this aim by showing that some of his central arguments against vertical IBE fail. In particular, I have aligned myself with Menuge, and noted that van Fraassen's qualms about unobservability do not warrant the epistemic inaccessibility of the parts of the world that cannot be accessed through unaided senses. Moreover, both *the argument from the bad lot* and *the argument from indifference* fail to undermine abductive reasoning. The former fails to show that the friends of IBE must appeal to an *unwarranted* privilege in order to defend abductive reasoning. And the latter fails to show that it is more likely, on *a priori* grounds, that a theory arrived at by abductive reasoning is going to be false rather than true. Moreover, if these two arguments are interesting at all, they cut equally on both sides. They put in danger constructive empiricism's ability to sustain grounded judgements of empirical adequacy and push it towards bald scepticism.

Still, friends of abduction face the steep task of providing good positive arguments in defence of abduction. But, if my criticisms of van Fraassen's position are sound, they have one less thing to worry about.¹⁹

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