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Review Article

AN UNREAL IMAGE*

- 1 *Between Realism and Empiricism?*
- 2 *Syntax or Semantics?*
- 3 *What Does it Take to 'Save the Phenomena'?*
- 4 *Is Causal Explanation 'Context Dependent'?*

Bas van Fraassen's *The Scientific Image* has rightly been the focus of a good deal of recent philosophical attention. It contains challenging theses and clear arguments. One of its virtues, therefore, is that it provides a great deal to disagree with.

The central purpose of the book is to build and defend a half-way house between realism and empiricism. I shall first describe the main features of this compromise and then explain why I see no reason to buy into it (section 1). Van Fraassen's whole approach to scientific theories is premised on the 'semantic' characterisation of them as sets of models rather than sets of sentences. He insists that this characterisation opens up many insights which are not to be had from the usual 'syntactic' viewpoint. In section 2 I shall explain why I remain unconvinced of the advantages of the semantic approach. In section 3 I take up one point of detail—van Fraassen's inadequate characterisation of the notion of 'empirical adequacy'. In the course of pursuing his central purpose van Fraassen advances several interesting theses concerning more particular issues. In section 4 I take up one of these: his thesis that the notions of cause and of causal explanation are importantly and essentially 'context-dependent'. My comments will be restricted to these four topics even though this means leaving out of account some interesting features of the book—notably the treatment of probability in chapter 6.

I BETWEEN REALISM AND EMPIRICISM ?

According to van Fraassen's characterisation, *scientific realism* asserts (i) that the languages in which scientific theories are expressed are to be interpreted literally—talk of elementary particles, curved spacetime and the rest is not to be watered-down to mere 'as if' talk; (ii) that scientific theories

* Review of BAS C. VAN FRAASSEN [1980]: *The Scientific Image*. Clarendon Press. Pp. xi + 235.

I completed this review while holding a research fellowship at the Center for Philosophy of Science at the University of Pittsburgh. I should like to thank all those who made my stay there such an enjoyable and stimulating one. I received valuable comments on an earlier draft from my fellow visiting fellows Ron Giere and Ron Laymon and also from my colleagues Colin Howson and Peter Urbach.

are true-or-false attempted descriptions of the world—and true-or-false again not in some watered-down sense but in the fully-fledged correspondence sense; and (iii) that acceptance of a scientific theory involves the belief that the theory is true (although this belief may be tentative and hedged). *Scientific empiricism* on the other hand, asserts that in so far as theories transcend all possible observational data they are not to be interpreted literally—theoretical entities irreducible to observational data are to be interpreted as fictions, codification schemes or whatever. Indeed some empiricists, of course, still insist that no real sense can be made of observation-transcendent truth.

Van Fraassen rejects both realism and this sort of empiricism and constructs instead an unusual middle position, which he calls ‘constructive empiricism’.¹ This says first that the languages in which scientific theories are expressed should be interpreted literally:

If the theory’s statements include ‘There are electrons’, then the theory says that there are electrons (p. 11).

In other words, no positivistic reduction of science is countenanced: semantic scruples about whether sense can really be made of statements which transcend in principle all possible data are to be disregarded; theories are indeed true-or-false (attempted) descriptions of reality.² *On the other hand*, there is, according to constructive empiricism, no reason to assume that what even our best accepted theories tell us is true—nor even that the *aim* of science is to produce true theories. Acceptance of a theory involves, *not* the belief that it is true, but only the belief that it is ‘empirically adequate’³—that it ‘saves the phenomena’. It is important to note that this is taken to involve *all* the phenomena—past, present, and future, observed and unobserved. Hence we can never know for sure that a theory is empirically adequate. Nevertheless, acceptance is taken to involve an, at any rate tentative, belief in empirical adequacy *and* to involve further a ‘certain commitment to confront any future phenomena by means of the conceptual resources of this theory’ (p. 12). It has often been argued that if empiricism were widely adopted by scientists then science would stagnate: the heuristic impetus supplied by a realist outlook would be lacking. This ‘commitment’ concerning future phenomena is intended to supply the potentially missing heuristic force.

What arguments might incline us to adopt van Fraassen’s compromise ‘constructive empiricism’? His main tactic is to argue that the view makes sense of science and is, moreover, the *minimal* view which does so; therefore

¹ There seems to be no particularly good reason for the choice of this name (for van Fraassen’s remarks on the name see pp. 4–5): in particular it should not be taken as suggesting that van Fraassen’s position is at all constructivist in the sense made familiar by recent debates in the philosophy of mathematics.

² This would not, however, be van Fraassen’s own way of expressing this thesis—he prefers a ‘semantic’ formulation (see section 2).

³ What exactly this term is, and what exactly it should be, taken to mean is discussed in section 3 of this review.

to adopt any stronger, realist position would involve assumptions which are otiose. So he spends a good deal of time countering various arguments to the effect that some undoubted feature of science *entails* a realist view of theories. Several such arguments have, for example, been based on the success of science: how can present scientific theories account for such a wide range of phenomena if they are not, at any rate ‘approximately’, true? Van Fraassen counters by pointing out, first, that we can’t ask for explanations of everything on pain of infinite regress; and, secondly, that anyway alternative explanations of science’s success are possible. The explanation he himself favours is ‘Darwinian’:

... any scientific theory is born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive—the ones which *in fact* latched on to actual regularities in nature (p. 40).

Independently of the merits of van Fraassen’s particular alternative, his general counter-argumentative strategy is clearly bound to work. Nothing in science is going to *compel* the adoption of a realist attitude towards theories (especially given that we are allowed to regard scientists’ apparent determination to hold onto certain theories despite empirical and conceptual difficulties as only being *as if* they believed their theories to be true). But this leaves open the possibility that some form of scientific realism, while strictly speaking unnecessary, is nonetheless the *most reasonable* position to adopt.

In order to explore this possibility properly, an ambiguity in van Fraassen’s development of constructive empiricism must first be resolved. There is a weaker and a stronger position which he might be holding. The weaker one states that acceptance of a theory involves *at least* belief in its empirical adequacy—it is simply agnostic on the question of whether some further belief that the theory is true or approximately true is also involved. The stronger position holds that theory-acceptance involves *exactly* the belief that the theory is empirically adequate *and nothing more*.

Van Fraassen’s principal argument for constructive empiricism is the minimality argument. But clearly only the weaker of the two positions just outlined is logically weaker than realism, the stronger of the two being, on the contrary, inconsistent with it. In order for the argument to entail the stronger position a further premise is required. This is that anything more than is absolutely necessary not only need not be, but *is not to be*, assumed. This extra premise is at least debatable and will generally be denied by scientific realists. In sum, the stronger position and realism are inconsistent rivals—each makes assumptions not made by the other—and surely some argument is needed in terms of the superior plausibility of one over the other, if we are to decide between them.

No such argument is supplied by van Fraassen, who, as already noted, rests content with demonstrating that particular arguments do not *establish* realism. Various traditional arguments—notably that from under-determination—*are* mentioned, but none is systematically developed and no

attempt is made to consider and rebut realist counters. Moreover, he ignores the particular kind of scientific success which has traditionally carried the most pro-realist persuasive power. This is *not* the fact that present scientific theories have many correct observational consequences. These might after all have been consciously built into the theories. The most striking sort of success has been when one theory, introduced to deal with one set of phenomena, has turned out to predict in a straightforward and uncontrived way, a completely different (and perhaps hitherto unsuspected) phenomenon. Even the most committed instrumentalists, when faced with this sort of success, have found it difficult to fight off ‘creeping realism’.¹ (Note that van Fraassen’s own favoured evolutionary explanation of scientific success can obtain no foothold here: presently accepted theories have clearly been moulded, directly or indirectly, by the ‘environment’ of known empirical results, and would not be ‘presently accepted’ if they did not have many correct observational consequences. But this supplies no account whatsoever of why they should have successfully predicted results which were not even *in* their environment when they were first proposed.) I do not, of course, claim that this sort of predictive success *entails* scientific realism, but only that it has traditionally supplied an argument in favour of realism, an argument which stands in need of a counter from the anti-realist.

As for positive arguments in favour of definitely restricting our claims about presently accepted theories, the traditional positivistic ones are, of course, unavailable to van Fraassen. If assertions of the literal truth of theories failed to make genuine sense, then realism would have to be rejected as a matter of principle, no matter how great its superficial attraction. But van Fraassen explicitly allows that coherent sense can be made of observation-transcendent truth. If assertions were legitimate only if they could be fully justified, then straightforward claims about the (approximate) truth of our theories would have to be rejected, again as a matter of principle. But van Fraassen’s account of what is involved in the acceptance of a theory requires the highly unjustifiable belief that the theory ‘saves’ all past, present, *and future*, all observed *and observable though actually unobserved* phenomena.

This is all on the (probably correct) assumption that van Fraassen’s is the stronger of the two positions outlined above. But what if only the weaker is advocated? This weaker position says, remember, that in accepting a theory we commit ourselves to *at least* a belief in its empirical adequacy. Hence it is not inconsistent with realism. The minimality argument would, if accepted, establish this weaker position but would *not* thence supply a further

¹ A notable example of a committed instrumentalist who succumbed to a degree of ‘creeping realism’ is Pierre Duhem. He explicitly denied that high-level theories are true-or-false, holding instead that they are classification schemes. Yet faced with some theory’s prophetic success he felt forced to concede that this indicates that the theory supplies something approaching a ‘natural classification’. It is not easy to see too many significant differences between a ‘natural classification’ and a true theory. (See Duhem [1906] and, for analysis, my [1982].)

argument against taking the step into some form of realism. So long as the realist acknowledged that he makes assumptions which are not strictly speaking necessitated by any aspect of science then he could accept the argument without its affecting his position.

Is there nonetheless an argument in favour of making as few assumptions as possible (and maintaining an uncommitted attitude towards the rest)? Van Fraassen seems to believe that this is almost self-evident; that, at any rate in philosophy, fewer assumptions are automatically better. But this cannot be right. After all, 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; nonetheless, physical realism seems not only a reasonable position to take, but the only reasonable position to take.

Once the positivist line has been broken, as van Fraassen allows, what real argument is there for only going a little beyond it? There is, so far as I can tell, just one remark in the book addressed to this question. This is that no one is actually compelled to hang for a sheep rather than a lamb (p. 72). But the point of the old maxim is surely that if a little and a lot are both available, and if the penalty is in either case the same, then one may as well go the whole hog and take the whole sheep. It is not that anyone is compelled to take the sheep, but that it seems rather perverse not to. If scientists when they accept a theory are to be considered as holding beliefs (that the theory saves *all* the phenomena), which in the nature of the case cannot be justified, then why debar them from a little extra belief (that the theory is at any rate our present best guess as to the truth),¹ which they anyway seem generally to hold, and which now fails to clash with any general principle?

There is a well-known anti-realist answer to this question, but it is not clear that van Fraassen can avail himself of it. This answer is, roughly speaking, that the belief that our present theories are true is strongly at odds with the history of science. If theories are interpreted literally, then this history can only be seen as punctuated by radical revolutions: theories which were once widely regarded as true are now regarded as unambiguously false (though still of course containing many correct observational consequences). If we are not to adopt the absurdly presumptuous view that history consists of a series of false starts and that we have *now* definitely hit on the truth, then we must certainly modify and weaken the typical realist claim about the status of our theories. They can, at best, be regarded as only

¹ As this remark indicates, it seems to me that van Fraassen requires the realist to overstrain his belief faculties when he accepts a theory. In the sort of 'conjectural realism' which I advocate in my [1982] there is no commitment either to the truth, 'approximate truth' or even probable truth to degree x of presently accepted scientific theories. Indeed the 'conjectural realist' may well believe that they are all very probably false. This does not prevent him believing that they are true-or-false attempted descriptions of reality, both of observable reality and of the reality 'hidden behind the phenomena,' (I take this to be the *core* of scientific realism) and it does not prevent him believing that these theories constitute our presently best guesses as to the nature of reality.

'approximately' true, or as 'reflecting to some extent the real structure of the universe'. These notions have notoriously proved resistant to precise analysis. More fundamentally, it is not clear that, so far as high-level theories are concerned, this approach holds out any real hope of success. Is any account of the notion likely to make 'heat is a fluid' 'approximately true' (assuming that heat really consists of molecular motion)? Is it now regarded as 'approximately true' that the ether exists? On the other hand, later developments are not at odds with the idea that, *intuitively speaking*, the caloric theory and the classical ether theory were to some extent 'empirically adequate'. Even here there are well-known difficulties: superseded theories are generally inconsistent with their superseding rival even at the empirical level. Nonetheless, there remains a strong intuition that scientific development has been essentially continuous at the empirical level. If the anti-realist could produce a precise account which captured this intuition, then the resulting picture of high-level theoretical discontinuity and low-level observational continuity would seem to supply him with a strong argument against taking a realist attitude towards theories.¹ Can van Fraassen take advantage of this opportunity? This is not entirely clear because of certain difficulties with his precise notion of 'empirical adequacy'. As we shall see (in section 3), this is a very weak notion. Nonetheless if we take fully-fledged versions of the caloric theory, classical physics or whatever, *complete with all the necessary auxiliaries*, then these are inconsistent with accepted observation statements and this in turn certainly makes them empirically inadequate in van Fraassen's sense. *These* theories have been rejected as empirically inadequate no less definitely than they have been rejected as false on realist accounts. If, on the other hand, we take just the 'core' of the caloric theory or of classical physics then these do remain 'empirically adequate' on van Fraassen's characterisation—but only because, as Duhem pointed out, they make, in isolation, no claim which is directly testable at the empirical level. In this sense *any* theory is 'empirically adequate'. But what is really needed in order to solve this problem of giving a more precise characterisation of the claim that science has been 'essentially' continuous at the empirical level despite high-level discontinuities is some precise account of how once accepted but now superseded theories could, in their fully-fledged versions, have had observational consequences which, although strictly false, were nonetheless 'nearly' right. Van Fraassen provides no machinery with which to approach this problem.

To sum up this section: my claim is that if van Fraassen is advocating a genuine *alternative or rival* to realism, then he should argue more persuasively for his alternative's greater plausibility; if he is advocating merely a *weakening* of realism, then he should tell us why, rather than merely assume that, weakening a philosophical position makes it better.

¹ I do not believe that this argument would in fact destroy realism though it would strongly favour a move to 'conjectural realism' (see preceding footnote).

2 SYNTAX OR SEMANTICS ?

Van Fraassen characterises all his more precise methodological notions in terms of the 'semantic approach' to theories. According to this approach, theories are best seen, at any rate 'in the first instance', as consisting of sets of models rather than sets of sentences:

The syntactic picture of a theory identifies it with a body of theorems, stated in one particular language chosen for the expression of that theory. This should be contrasted with the alternative [which van Fraassen advocates] of presenting a theory in the first instance by identifying a class of structures as its models. In this second semantic, approach the language used to express the theory is neither basic nor unique; the same class of structures could well be described in different ways. . . . The models occupy centre stage (p. 44).

Newtonian particle mechanics, for instance, should be seen as first and foremost a set of Newtonian particle *systems*, and *not* as a set of axioms.

It is not clear to me why it should matter very much how a theory is regarded 'in the first instance'. Certainly, and despite the impression which van Fraassen seems to try to convey, the primacy of the semantic approach cannot rest on logical considerations. So far as logic is concerned, syntax and semantics go hand-in-hand—to every consistent set of first-order sentences there corresponds a non-empty set of models, and to every normal ('elementary') set of models there corresponds a consistent set of first-order sentences.¹ As van Fraassen himself made clear in an earlier (and perhaps clearer) account of the matter:

There are natural interrelations between the two [syntactic and semantic] approaches: an axiomatic theory may be characterised by the class of interpretations which satisfy it, and an interpretation may be characterised by the set of sentences which it satisfies; though in neither case is the characterisation unique. These interrelations make implausible any claim of philosophical superiority for either approach.²

Whether intentionally or not, van Fraassen seems to go back on this in the present work by suggesting that the semantic approach can indeed be shown to be superior on grounds of philosophical principle. For example, he argues that there are certain basic defects in the syntactic approach which are avoided by the semantic. These arguments, however, go through only against strawmen (or, perhaps, against really held but long superseded views). For instance, he produces one particular syntactic characterisation of the empirical content of a theory (set of the theory's consequences

¹ The interrelationships admittedly become much more complicated if we go beyond first-order theories. But if van Fraassen wants to found his case on the insufficiency of first-order logic, he should say so explicitly and should argue explicitly why anything *essential* is left out of a first-order representation. He should finally spell out why switching to the semantic approach would help in this less clear-cut area.

² Van Fraassen [1970], p. 326. (I am not sure what he is getting at when he says that 'in neither case is the characterisation unique'.)

expressible in the observational sub-vocabulary); shows that this characterisation is unacceptable and encourages the inference that this means that the syntactic approach cannot at all adequately characterise the notion. More generally he criticises various particular assumptions which particular alleged proponents of the syntactic approach have made, and concludes from the success of these criticisms that the whole approach is in trouble.¹

Finally, van Fraassen often asserts, as in the passage quoted above (p. 71), that the syntactic view makes the notion of a theory unacceptably language-dependent. But there is no real force in this point: the sensible axiomatiser is not the prisoner of any particular language. The choice of a language in which to express a theory will be made on the grounds of suitability or convenience without any claim being made that this is the one “true” language of the theory. Take, for example, Newton’s theory as formulated using Hamiltonians and alternatively using Lagrange’s equations. Even if these were originally formulated in two different languages with different sets of primitives, it is easy to express the idea that they *are* simply two different formulations of the same theory—they can either be proved logically equivalent in a suitable metalanguage or we can switch to a new wider language in which both sets of original primitives become defined terms. It could be objected that if the proponent of the syntactic view has a notion of a theory *prior* to its expression in some particular precisely defined language, then he must have some more basic language in mind in which the theory is representable. Ultimately he is committed to the notion of a truly basic language—the only serious contender for this role seems to be set theory—in which all his theories are expressible given suitable definitions. This is no doubt correct but reveals no unacceptable language-dependence, and is anyway an assumption to which van Fraassen too is ultimately committed: for he too must presuppose some system in which the isomorphisms between structures that his approach requires can be stated and established.

If then we give up the idea of establishing the superiority of the semantic approach on the grounds of logical or philosophical principle, and we surely must give up that idea, then we are left only with arguments from something like greater ‘naturalness’, or, perhaps, greater fruitfulness. (Just as, while admitting the logical equivalence of the two formulations, we *might* argue, say, for the superiority of the Hamiltonian formulation of classical mechanics over the Lagrangian on the grounds of the former’s greater naturalness or the fact that it is more fruitful since extendable in a certain

¹ For instance he makes a good deal of the undoubted fact that Carnap’s distinction between ‘meaning postulates’ and ‘empirical postulates’ often seems arbitrary. But many philosophers whom van Fraassen would presumably classify as followers of the syntactic approach (Lakatos and Glymour immediately spring to mind) have already argued that the right reaction to Duhem’s point about the logical structure of tests is *not* to try to restore direct testability of ‘central’ theories by lumping all the auxiliaries together as specifying the ‘meaning’ of the central theories’ theoretical terms, but instead to allow the auxiliaries as extra empirical assumptions which may themselves be amended because of empirical difficulties. Hence Carnap’s distinction can obviously be dropped without dropping the ‘syntactic’ view.

way to quantum mechanics.) Those who favour the model-approach do indeed generally rest most of their case on the claim that it presents a much more faithful picture of how scientists themselves regard theories. I doubt that this is correct, but assume that it is. Does it follow that the semantic view is a superior view of theories? Consider an analogy. First-order logic can be developed using *Principia Mathematica*-style formalisations with logical axioms and a small set of rules of proof (generally just two) or, alternatively, it can be developed using Gentzen-style so-called ‘natural deduction’—which involves no axioms but a larger set of rules. As the name indicates, some people at least would argue that the Gentzen system allows a ‘more faithful’ picture of how mathematicians, for example, actually reason. While certainly not indisputable, there is surely a stronger *prima facie* case for this claim than for the analogous claim about the semantic view of theories. But anyway say that we accept it. There is no reason why this in itself should be taken as establishing the superiority of the natural deduction approach—for logic is out to capture correct reasoning only in the sense of providing the ‘right’ demarcation between valid and invalid inferences. Except for pedagogic or other pragmatic purposes, the extent to which the ways it supplies for deciding validity reflect the ways people go about making this decision does not matter at all. Moreover, there are other merits that a system of logic can have—principally, of course, ease of performing meta-proofs. Many important meta-theorems are more easily achieved for *Principia Mathematica* systems than for natural deduction systems (because of the former’s relative paucity of primitive machinery). The only reasonable position seems to be to exploit both approaches and not to regard them as competitors that we must decide between. Similarly in the case of the syntactic and semantic views of scientific theories: despite van Fraassen’s propaganda in favour of the former, these are complementary not rivals; there is simply no point in arguing in favour of one and against the other.

3 WHAT DOES IT TAKE TO ‘SAVE THE PHENOMENA’?

One point of detail in the book should be commented on since it is likely to lead to confusion. One would expect someone who claimed that scientific theories aim at empirical adequacy to characterise a theory as empirically adequate when it *entailed* some given range of phenomena—or, rather more precisely, when it entailed the phenomena, *given* accepted statements of initial conditions and perhaps accepted auxiliaries. Only then could a theory be said really ‘to save the phenomena’—mere *consistency* with the phenomena is not enough to ‘save’ them.

Most of van Fraassen’s general remarks, I believe, presuppose this characterisation. But surprisingly, when he gives precise characterisations of adequacy he requires *only* consistency with the data: an empirically adequate theory is one that has ‘*at least one* model that all the actual phenomena fit inside’ (p. 12; italics added). This seems so clearly

unacceptable that one might suspect a simple slip has been made. But not so: the characterisation is repeated and some of its seemingly unacceptable consequences explicitly drawn and embraced. The most obvious such consequence is that ‘empirical adequacy, like truth, is “preserved under watering down”’ (p. 67). In other words, any theory logically weaker than an empirically adequate theory must itself be empirically adequate, and so, in particular, logical truths are empirically adequate.

Van Fraassen is, of course, right that truth is also ‘preserved under watering down’. But this only shows that a realist must have science aim at something more than truth—namely truth plus high informative content, or, if you like the ‘whole truth’.¹ Similarly the anti-realist must surely make some greater demand of his theories than mere consistency with the data. I suspect that van Fraassen has been overinfluenced and misled by the old Duhem point that what we standardly call scientific theories do not have observational consequences of their own. His favourite examples—classical and quantum mechanics—are clear cases in point. Auxiliary assumptions about particular systems must be added if either theory is to ‘yield the data’. This does not, however, vindicate van Fraassen’s characterisation. Even if we take these sorts of theories without auxiliaries we surely must demand more of them than mere consistency with the phenomena. It is not enough that *some* further assumptions consistent with the theory together with it yield the data. Rather the theory must yield the data when added to *particular* auxiliaries—namely those which are themselves ‘correct’.

As it stands, van Fraassen’s notion of empirical adequacy is itself inadequate.

4 IS CAUSAL EXPLANATION ‘CONTEXT-DEPENDENT’ ?

Van Fraassen arrives at the topic of explanation *via* the following route. A possible argument for realism is from the ‘explanatory power’ of theories. Theories explain certain phenomena. In order to have such explanatory power, theories need to possess something more than mere empirical adequacy—maybe ‘truth *plus*’, but at any rate truth. His counterthesis is that there is indeed something more to explanatory power than empirical adequacy, but this extra factor has nothing to do with truth and is instead ‘pragmatic’ in character.

His argument in favour of the ‘pragmatic’ or ‘context-dependent’ nature of explanation is independently interesting and has certainly been well received. (Indeed I have heard it described as ‘definitive’.) The crucial part of this argument is the attempt to demonstrate that the ‘pragmatic’ account solves certain difficulties which are generally regarded as intractable on the ‘standard’ Hempel-Oppenheim view.

I shall try to outline van Fraassen’s account of explanation, then indicate why it seems to me both unnecessary and unacceptable and, finally argue

¹ On this point see, for example, Popper [1972].

that, contrary to his own explicit claims, it does not in fact solve the difficulties which afflict the Hempelian account.

Requests for an explanation of an event can be regarded in one of two different ways: either as requests for the *cause* of an event, or as requests for an answer to a 'why-question'. Either notion, when properly analysed, is, claims van Fraassen, context-dependent. I shall concentrate here just on the notion of cause.

The standard analysis is that if *A* caused *B*, then *A* was a sufficient condition for *B*.¹ According to van Fraassen this cannot be correct since, for example, we would certainly allow that a particular plant which died when sprayed with defoliant died *because* it was sprayed, even though we knew that the defoliant is only 90 per cent effective.

Now this is certainly no knockdown refutation of the standard analysis, for a defender of that analysis could react to the alleged counterexample as follows. 'I readily admit that my account does not directly mirror ordinary usage, but would argue that it is none the worse for that. We do indeed often speak of 'the cause' of an event when the factor concerned was less than a fully sufficient condition for the event. But this is loose (though generally perfectly harmless) talk: we could always in such situations, find extra conditions which operated and which together with the cited factor, do indeed amount to a sufficient condition for the event. The spraying with defoliant was not, strictly speaking, the cause of the plant's death, though it was an important *causal factor*. The real cause was the spraying plus certain features of the particular plant's constitution. (Of course, the fact that *we* may have no interest in finding out what these features were—because for practical purposes we are only interested in the effects of the spraying on large populations of plants and not on any particular plant—is quite irrelevant to the question of whether they were in fact part of the cause.)'

Van Fraassen must regard any such response as insufficient, since he regards the plant example as refuting the sufficient condition analysis, and sets out to produce an alternative which captures ordinary usage more directly. He first tries the idea that the cause of the event is its *conditio sine qua non*: had the plant not been sprayed it would not have died. The straightforward logical translation of this suggestion is as the thesis that a cause is a necessary condition. This, however, makes the account clearly unacceptable. For one thing, the plant obviously *might* have died in some other way. Moreover, the plant's having been put in the ground in the first place was clearly necessary for its death, but no one would say that the planting caused the death. Sticking to the idea of cause as *some sort* of necessary condition, van Fraassen is led into the slough of counterfactuals and *ceteris paribus* clauses ('had the plant not been sprayed then it would not, other things being equal, have died when it did'.) But counterfactuals and

¹ Notice that this analysis is not, of course, committed to the thesis that if *A* is a sufficient condition for *B* then *A* caused *B*. (This thesis is indeed clearly wrong—see *below*.) No doubt a full analysis of the notion of cause requires several extra, and rather subtle conditions.

ceteris paribus clauses require a ‘pragmatic’ or ‘context-dependent’ analysis. In fact, claims van Fraassen, the right conclusion to draw is that there is no scientific objective notion of the cause of an event. Science, which is itself *not* context-dependent, aims to describe the whole ‘causal net’ of necessary conditions in which an event is enmeshed. But ‘the cause’ of the event is characterisable only in terms of (varying) human interests: what *we* regard as *the* cause of an event, and hence what *we* regard as explanatory, are certain ‘salient features’ of the ‘lines’ within the ‘net’ leading up to the event. Since what appears ‘salient’ in one situation, and to one person, may not appear salient to another in another, the notions of cause and hence of causal explanation are context-dependent.

In the case of causal explanation van Fraassen sees the context-dependency as characterisable by two factors: the ‘contrast class’ (we are always in context explaining why x occurred *rather than* y_1, y_2, \dots, y_n) and the ‘relevance relation’ (the explanandum must not only pick out x above y_1, \dots, y_n , but must also bear the right relevance relation to x). Detailed criticisms could be made of this account, but more importantly I remain unconvinced that this sort of approach is, quite generally, the right way to go. The ‘sufficient condition’ view provides a simple analysis which admittedly is somewhat distant from ordinary usage—though explanations (in terms of hidden extra factors) for the divergences between the ideal analysis and ordinary usage are readily forthcoming. If instead we insist on reflecting ordinary usage more fully, the whole analysis becomes terribly complicated and, so far as I can see, nothing is gained. The situation is analogous to one in logic. Some philosophers argue that classical logic must be modified and elaborated because it pronounces formally invalid certain inferences which would—in ‘normal’ argumentative practice—be immediately accepted as valid. The counter-argument would be that we should stick with classical logic and explain this situation by showing that there are certain implicit premises which are being tacitly accepted, and which, if articulated and added as explicit premises, would render the initially classically invalid inference classically valid. The alternative, of trying to capture the validity of *some* of these inferences directly, would in fact write some of these tacit premises into the rules of reasoning themselves. This would in turn make logic context-dependent, since what is accepted as ‘obvious’ (and hence what can function successfully as a tacit assumption) will in general differ from context to context. While this is, of course, a possible way to go, it does seem to lead to unnecessary complexity, without freeing logic from all idealisation.

Moreover, just as in the logic case there are inferences which are *absolutely* valid (because no tacit premises are needed), so in the case of causality there are surely *absolute causes*—where any context-dependence collapses, and sufficiency is restored. This is precisely what happens in the case of deterministic theories in science. Assuming, for the sake of an example, that the 19th Century account is true, the cause of Jupiter’s having position \mathbf{r} at

time t is it's having had position \mathbf{r}' at time $t-\Delta t$ and it's being a body of a certain mass in a certain force-field. The cause is, given accepted laws, a fully sufficient condition for the event. *Ceteris paribus* assumptions need not be separately made—they are, if you like, written into the cause, in particular into the claim about the *total* force acting on Jupiter. Similarly, again assuming 19th Century science, the cause of the light and dark fringes in (some particular instantiation of) the two-slit experiment is the coherence of the unobstructed light waves emerging from the two slits and affecting the screen. These conditions are, given the classical wave theory of light, *sufficient* for the production of the fringes. Taking these as the clear-cut cases—as has traditionally been done—we obtain a clear-cut analysis and lose nothing since the admitted fact that ordinary people often talk of causes which do not immediately fit this model can be otherwise explained than by giving up the model.

Finally, there is a further reason for rejecting van Fraassen's account of cause and of causal explanation. As it stands, no restrictions are imposed on 'relevance relations' and so any group seems to be free to set itself up complete with its own relevance relation and simply deny that presently accepted causes or presently accepted explanations are causes or explanations at all. Creationists, for example, can merely insist that the factors cited by neo-Darwinians to explain certain evolutionary developments fail to bear the right relevance relation to the events concerned.

I would claim, therefore, that van Fraassen has certainly not demonstrated that we *need* to incorporate explicit reference to the context in our account of cause and of causal explanation. I would also claim that there are certain dangers in doing so, which he has done nothing to lessen. But he also argues that incorporation of the context *helps* in certain ways—in particular that it leads to a solution of certain basic problems which have proved stumbling blocks to the orthodox approach, especially in the case of explanation. The sharpest of these problems come in the form of apparent counterexamples to the Hempel account: cases of intuitive explanations which fail to fit Hempel's conditions, or cases which fit Hempel's conditions but which fail intuitively to be explanations.

Perhaps the most famous counterexample is due to Bromberger.¹ The laws of geometrical optics, together with statements about the elevation of the sun and the lie of the land at the relevant point of the earth's surface, entail that a flagpole situated there is of a certain height *if, and only if*, its shadow is of a certain length. It seems to follow from Hempel's account that we can *either* explain the length of the shadow on the strength of the laws plus initial conditions, including the height of the flagpole, *or* explain the

¹ See his 'Why-Questions' in Colodny (*ed.*): [1966]. Other counterexamples, concerned with 'relevance' and with low-probability 'explanations' have been highlighted by Wesley Salmon (see Salmon *et al.* [1972]). Van Fraassen does not systematically apply his account to these more challenging, counterexamples. The *impression* he creates that the problems they pose fall to his approach (which certainly includes consideration of relevance and of probabilities) is, I think, merely an impression, but I shall not attempt to argue this thesis here.

height of the flagpole on the strength of laws plus initial conditions, including the length of the shadow. Yet intuition rebels at the latter 'explanation'. Since this is the only type of counterexample for which van Fraassen develops any sort of detailed demonstration that his approach solves the problem, I shall concentrate on it here.

The 'relevance relation' is what is meant to supply the solution. It may be that laws plus initial conditions imply that *A* will be the case if, and only if, *B* is, and yet *A* describes one sort of factor and *B* another; when someone asks for an explanation he is, in context, asking for a factor of a particular type; hence he may be satisfied with 'Because *B*' as an answer to 'Why *A*?' (*B* bears the right 'relevance relation' to *A*), and yet not accept 'Because *A*' as an answer of 'Why *B*?' (the relevance relation need not be symmetric). In the flagpole example, we are usually, in context, asking for a 'mode of production' (p. 131). Hence since the flagpole produces the shadow and not *vice versa*, the flagpole's height, in such contexts, may explain the shadow's length, but not *vice versa*.

One point of detail is that a 'mode of production' is surely a cause by any other name. Hence it ought, on van Fraassen's account, itself to be 'context-dependent' and therefore not employable in a specification of context. But still it is of course true that we think flagpoles cause shadows to have a certain length, but do not think that shadows cause flagpoles to have a certain height. I would claim that this so-called contextual factor is part and parcel of the notion of a causal explanation: in deterministic science we are *always* looking for causes or 'modes of production'. If this is right then the problem posed by the flagpole example is just that it shows that a condition may be sufficient, in certain circumstances, for the occurrence of an event and yet not be a cause of this event: there is an extra intuitive, or metaphysical component to our conception of cause which has eluded the formal analysis; but there is certainly no case in which the length of a shadow explains the height of a flagpole. If, on the contrary, van Fraassen is right then we would at any rate *expect* there to be such cases—cases in which the relevance relation is different and where shadow lengths do indeed bear the right such relation to flagpole heights. Van Fraassen claims to have produced precisely such a context.

He tells an X-certificate story with himself as a hero. On a visit to the estate of an old French chevalier, he is taking a pleasant tea with his host on the terrace when the shadow of a tower built in the grounds falls on the terrace (dramatic licence having turned the flagpole into a tower). The shadow makes it too cold for tea to continue comfortably. Exclaiming 'Why does the tower have to have such a long shadow?' our hero is taken literally and is told that the tower commemorates a visit of Queen Marie Antoinette to the estate, was built on the spot where the chevalier's ancestor first greeted the queen, and is the same height in feet as the age the queen would have achieved in the year that the tower was built. Later that evening, however, he is told a quite different story by a mysterious chambermaid: that the

chevalier had fallen in love with a maid whom he subsequently murdered; that the tower marks the spot where he murdered her; that he had vowed that the shadow of the tower would with every setting sun cover the terrace where he had first declared his love; and that he had chosen the height of the tower accordingly.

This version has been bowdlerised for publication in this family magazine. The full story can be found in van Fraassen's book. What *cannot* be found there, however, is a case of an explanation of the height of a tower by the length of its shadow. Unless, that is, we are allowed to cheat quite blatantly by changing the event to be explained to suit our purposes. The problem arose remember within physics—geometrical optics yields a biconditional in a circumstance in which the causal arrow definitely flies only one way. If we switch to the realm of human decisions, then, of course, the problem may well disappear. Someone may decide to bring about x directly, or because physics, say, tells him that x will have effect y and y is what he really wants to achieve. But to say in the latter context that y caused x is simply a confusion. If you like, it was the agents' *desire to achieve* y which caused *him to bring about* x . Van Fraassen's story only illustrates this obvious lesson—it provides two competing (*not* complementary) explanations of *the chevalier's decision* to build the tower to a certain height. It is therefore completely independent of the problem at hand. This is shown by the fact that van Fraassen's trick could just as well be pulled in cases where the causal situation and the logical formalism of our theory go hand in hand—both being asymmetric. Suppose some scientific theory yields the conditional $A \rightarrow B$, does *not* yield the conditional $B \rightarrow A$, and that we are quite happy to say that A explains the occurrence of B (the necessary theory being assumed), and finally that there is no question of B similarly explaining A . Except, that is, on van Fraassen's account. For suppose that a teacher has chosen to realise circumstances A because he wanted to show his students effect B . Then if we are allowed to say that we can explain the height of the tower by the length of its shadow (the chevalier wanted something with a shadow that long), then we should equally well be allowed to say that we can explain the cause A in terms of its effect B (the teacher wanted some cause that would have effect B). This is absurd.

Van Fraassen's treatment does nothing to solve the problem posed by Bromberger's example. He has given us no reason to import the 'context' into our formal account of scientific explanation.

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REFERENCES

- COLODNY, R. G. (ed.) [1966]: *Mind and Cosmos*. University of Pittsburgh Press.
 DUHEM, P. [1906]: *The Aim and Structure of Physical Theory*. Reprinted 1962 by Atheneum.
 POPPER, K. R. [1972]: *Objective Knowledge*. Oxford University Press.

- SALMON, W. *et al.* [1971]: *Statistical Explanation and Statistical Relevance*. University of Pittsburgh Press.
- VAN FRAASSEN, B. C. [1970]: 'On the Extension of Beth's Semantics of Physical Theories', *Philosophy of Science*, **37**, pp. 325–39.
- WORRALL, J. [1982]: 'Scientific Realism and Scientific Change', *The Philosophical Quarterly*, **32**, pp. 201–31.