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Realism versus Constructive Empiricism

The demise of logical positivism has been followed by a rising tide of scientific realism. Bas van Fraassen is to be congratulated for swimming against that tide. But we must also ask whether he manages to make much headway. I shall argue that he does not. My first section explores van Fraassen's rather attenuated antirealism and the distinction between truth and empirical adequacy on which it depends. My second section argues that van Fraassen succeeds no better than his predecessors in answering a major objection to antirealism. My third section examines the link between realism and explanation and van Fraassen's attempt to sever that link.

I | Truth, Empirical Adequacy, Empirical Equivalence

Scientific realism is an old issue, and over the years both realism and antirealism have taken various forms. Van Fraassen defines realism thus: "*Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true.*" He says that this is a minimal formulation which "can be agreed to by anyone who considers himself a scientific realist."[1] [1066]* Later, however, van Fraassen extends this minimal formulation by adding to it a realist 'demand for explanation.' As we will see, his antirealism stems in large part from criticisms of this demand. As we will also see, his version of the demand is an absurdly strong one.

What is the nature of van Fraassen's antirealism? The most radical opponents of realism (the instrumentalists) deny that scientific theories

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* The numbers in brackets refer to pages in this volume.

have truth-values at all. Van Fraassen's antirealism is not of this radical kind. He accepts a "literal construal of the language of science" whereby "the apparent statements of science really are statements, *capable of* being true or false" (p. 10) [1068]. In the same vein, he rejects positivist interpretations of scientific language, whereby the 'real meaning' of theoretical assertions is somehow cashed out in terms of the observable:

> Most specifically, if a theory says that something exists, then a literal construal may elaborate on what that something is, but will not remove the implication of existence. . . . If the theory's statements include "There are electrons," then the theory says that there are electrons. If in addition they include "Electrons are not planets," then the theory says, in part, that there are entities other than planets. (p. 11) [1068–69]

Thus, contrary to the positivists, two theories may say exactly the same thing about the observable yet remain distinct and perhaps incompatible theories.

All this puts van Fraassen firmly in the realist camp as far as the *interpretation* of scientific theories is concerned.[2] His antirealism proceeds entirely on the *epistemological or methodological* level. (The same can be said of the antirealism espoused by Larry Laudan in *Progress and Its Problems*.) He thinks that, although scientific theories are capable of literal truth, they "need not be true to be good" (p. 10) [1067–68]. Accordingly, it is not the aim of science to provide true theories, and to accept a theory is not to believe it to be true. What matters in science is that theories are correct so far as the observations and experiments go. Hence, constructive empiricism: "*Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate*" (p. 12) [1069]. A theory is empirically adequate "exactly if what it says about the observable things and events in this world, is true—exactly if it 'saves the phenomena' " (p. 12) [1069].

The distinction between truth and empirical adequacy, and hence between realism and constructive empiricism, is a subtle one. For theories about the observable, truth and empirical adequacy coincide (p. 21) [1077]. For theories about the unobservable, truth entails empirical adequacy but not vice versa: such a theory may be empirically adequate yet false. Accordingly, to believe that a theory about the unobservable is true is more risky than to believe that it is empirically adequate. Not that the latter is without risk: empirical adequacy "goes far beyond what we can know at any given time" since it requires that the theory save all the phenomena in its field, past, present, and future, not merely all actually observed phenomena (p. 69). Now, the chief difficulty for realism has always been skeptical arguments to the effect that we can never know a scientific theory to be true nor ever be rationally warranted in accepting, however tentatively, a theory as true. This is as much a difficulty for con-

structive empiricism. The same skeptical arguments might be used to show that we can never know a scientific theory to be empirically adequate nor ever be rationally warranted in accepting, however tentatively, a theory as empirically adequate. Van Fraassen insists, however, that the positions are different:

> There does remain the fact that . . . in accepting any theory as empirically adequate, I am sticking my neck out. There is no argument there for belief in the truth of the accepted theories, since it is not an epistemological principle that one might as well hang for a sheep as for a lamb. (p. 72)

Epistemological or not, the principle that one might as well hang for a sheep as for a lamb is a pretty sensible one. Given two criminal acts *A* and *B* whose risks of detection and subsequent penalties are the same, but where *A* yields a greater gain than *B*, the sensible criminal will do *A*. But are the risks and penalties of realism and constructive empiricism the same? And does realism bring with it gains that constructive empiricism does not? Van Fraassen addresses these questions; to evaluate the cogency of his position we must address them too.

Suppose the realist tentatively accepts a theory as true, while the constructive empiricist tentatively accepts it as empirically adequate. The realist does take a greater risk. But he takes no greater risk of being detected in error *on empirical grounds*. So, given strict empiricism (the principle that only evidence should determine theory choice), it seems that we might as well be hung for the realist sheep as for the constructive empiricist lamb.

The trouble is, van Fraassen argues, that realism and strict empiricism do not mix and that realism must pay the penalty of rejecting strict empiricism. He makes the point by considering the case of empirically equivalent yet incompatible theories. This is not, of course, the humdrum case where the *available* evidence fails to discriminate between two incompatible theories. This case need not trouble the realist, who may always hope to show that the two theories are not empirically equivalent and then press for an experimental decision between them. Rather, it is the esoteric case where such hopes are unfounded, where two incompatible theories say exactly the same things about *all* matters observational. The constructive empiricist could accept both theories (believe both to be empirically adequate); the realist cannot on pain of contradiction believe both to be true. But how is the realist to choose between them? In the nature of the case, empirical evidence cannot guide his choice, which must therefore be made on nonevidential grounds. Realism runs counter to strict empiricism and allows nonevidential or 'metaphysical' considerations to intrude into matters of theory choice.

How might the realist respond to this? Presumably, as a realist, he will have no truck with the positivist idea that empirically equivalent theories are really the same theory and not incompatible after all. Nor, as a realist, will he have any truck with the related idea (perhaps it is the same idea in new dress) that there are no 'verification-transcendent truth-conditions' and therefore no truth of the matter for the two theories to disagree about. These ideas, after all, seem to entail that Berkeley's immaterialism is really the same theory as the commonsense belief in independently existing material objects or that there is no truth of the matter for Berkeley and commonsense to disagree about. And these conclusions are anathema to the commonsense realist, let alone the scientific one.[3]

Taking a cue from this example, one might wonder whether the problem is philosophical or metaphysical rather than scientific, in which case metaphysical considerations would not be an intrusion after all. How often have empirically equivalent but incompatible theories occurred in real science? Van Fraassen gives one example, and it is a notorious one. Newton hypothesized that the center of gravity of the solar system is at rest in absolute space. He also pointed out that the appearances would be no different if that center were moving through absolute space at any constant velocity *v*. So all of the theories *TN(v)*—Newton's theories of mechanics and gravitation plus the postulate that the center of gravity of the solar system has constant absolute velocity *v* for any *v*—were claimed by Newton to be empirically equivalent (p. 46).

Van Fraassen's account can be disputed: Newton only claimed the empirical equivalence of the theories *TN(v) so far as appearances within the solar system are concerned*. Hypothesize that some other star is at rest in absolute space, for example, and the empirical equivalence vanishes: if the solar system has any nonzero velocity, then it will approach or recede from that star and, given sufficient time, the effects of this will become apparent.

Here I resort to a realist ploy whose efficacy van Frassen considers. This is to say that, where equivalent theories occur, by *extending* these theories (that is, embedding them in wider theories) their equivalence will disappear (that is, the wider theories will not be empirically equivalent). In the trivial example just cited, the wider theories are formed simply by adding the statement that some star is at absolute rest to each of the existing theories. The example is trivial because we can, by the Newtonian principle of relativity, construct empirically equivalent theories to each of these wider theories (including the only empirically adequate one): simply consider theories attributing an absolute velocity *v* to the center of mass of the extended system consisting of the solar system and the star. The process can be continued (assuming the number of masses is finite) until they are all taken into account. And then, again by the Newtonian principle of relativity, we will have an infinite family of empirically equivalent theories each of which consists of Newton's laws plus the hypothesis that the center of mass *of the universe* has velocity *v* for any value of *v*.

Van Fraassen considers a more interesting extension or embedding of

his Newtonian example, the attempt to combine it with Maxwell's electromagnetism where forces depend upon velocities and not upon accelerations as in Newton. This feature made it possible to devise experiments to detect absolute velocities. The null results of such experiments were an important factor in leading scientists to abandon the Newtonian doctrines of absolute space and time in favor of relativistic ones. And this was to abandon *all* of the empirically equivalent Newtonian theories. Van Fraassen asks us to imagine, however, that null results had not been obtained, that, on the contrary, an absolute velocity for the center of mass of the solar system had been measured. Here it might seem that one of the empirically equivalent Newtonian theories had been confirmed and the rest refuted, and hence that they were not empirically equivalent after all. Van Fraassen finds this reasoning spurious (p. 49). But I find his reasoning, if not spurious, at least hard to follow. Operating within this piece of science fiction (or, rather, history-of-science fiction), he says that we could make compensating adjustments in electromagnetic theory so as to retain whichever of the empirically equivalent Newtonian theories we like. In other words, had the history of science been different, we could construct a new family of empirically equivalent combinations of mechanics and electromagnetism.

But, first, van Fraassen has done nothing to impugn the fact that his empirically equivalent Newtonian theories, when combined with Maxwell's electromagnetism, ceased to be empirically equivalent. Second, could the Newtonian readily have accepted that electromagnetic forces depend upon absolute velocities rather than absolute accelerations? Van Fraassen concedes that, had his piece of history-of-science fiction occurred, it would have "upset even Newton's deepest convictions about the relativity of motion" (p. 48). But did not these convictions follow from Newton's laws of mechanics and the doctrine of absolute space? Last, and perhaps most important, all of this is a piece of history-of-science fiction: the historical facts are that in this notorious real example of empirical equivalence, the only good example known to me, the actual development of science removed the problem.

Van Fraassen has a further retort to the idea that empirical equivalence can be removed by extension or embedding. He can say that it is the empirical adequacy of the extended theories which counts and that one should accept the victor only as empirically adequate, never as true. And, to offset the scarcity of empirically equivalent theories in real science, he can point to the fact that we can artificially concoct empirically equivalent alternatives to any theory by resorting to notorious logical tricks. The simplest such trick is to conjoin any theory with "The Absolute is lazy" to form a new theory empirically equivalent with the original.

The standard response to such tricks is to eliminate the concocted theory on the ground of its reduced simplicity or unity. Van Fraassen does not object to the appeal to simplicity, but he insists that simplicity is a

pragmatic virtue of a theory which has nothing to do with that theory's truth or likelihood of being true. The realist, for whom accepting a theory is believing it true, must forge a link between simplicity and truth if he is to appeal to the former. And the link can be forged only by a metaphysical principle:

> Simplicity . . . is obviously a criterion in theory choice, or at least a term in theory appraisal. For that reason, some . . . suggest that simple theories are more likely to be true. But it is surely absurd to think that the world is more likely to be simple than complicated (unless one has certain metaphysical or theological views not usually accepted as legitimate factors in scientific inference). The point is that the virtue, or patchwork of virtues, indicated by the term is a factor in theory appraisal, but does not . . . make a theory more likely to be true (or empirically adequate). (p. 90)

So the argument seems to be this: the realist can solve the problem of empirical equivalence only by appealing to simplicity; but he can appeal to simplicity only if he assumes a metaphysical principle ("Nature is simple" or some such); realism therefore involves an illegitimate intrusion of metaphysics into science and the abandonment of strict empiricism.

Is the constructive empiricist in any better position? Presumably he, too, will prefer a respectable theory to an artificially concocted empirically equivalent alternative. He, too, will appeal to simplicity and abandon strict empiricism. But he, apparently, can do this in good conscience, cheerfully admitting that pragmatic virtues such as simplicity have nothing to do with the real aim of science, empirical adequacy. Indeed, how could simplicity have anything to do with that aim? To say that the simpler of two empirically equivalent theories is more likely to be empirically adequate is to contradict oneself.

Returning to the realist, there are several ways he might respond to van Fraassen's argument. The first is simply to admit that there is nothing to choose between empirically equivalent theories. This is hardly satisfactory in view of the ubiquity of the logical tricks. The second is to spice scientific realism with a dash of pragmatism, admitting that there is nothing to choose on realist grounds between empirically equivalent theories but preferring some on the pragmatic ground of simplicity. Despite van Fraassen's argument, I see no reason why the realist cannot appeal to pragmatic virtues just as the constructive empiricist does. The third response is to say that simplicity is not, after all, a merely pragmatic virtue. Realists and constructive empiricists alike value empirical strength; they value it for different reasons, but both connect it with the central aim of science. Is it not a sufficient reason to eliminate concocted alternatives to existing theories that they are not empirically stronger than the theories from which they are concocted?

The realist has a problem here, however. Insofar as simplicity and

strength go together (and they do not always), simplicity is not merely a pragmatic virtue. But insofar as simplicity and strength go together, simplicity and truth cannot: the stronger theory is, in some intuitive sense at least, less likely to be true. And here lies the problem for any realist seeking to forge a link between simplicity and truth. Yet the problem may not be completely intractable. "Nature is simple" is a metaphysical principle and a hopelessly vague one to boot. But scientists have made various attempts to say more precisely what it means and to construct theories which conform to it.[4] This transforms it into a metaphysical principle which can, at first remove so to speak, be empirically assessed: roughly speaking, it is acceptable metaphysics if theories constructed under its aegis are empirically successful, while theories which violate it are not.[5] In our postpositivistic age, we should not regard the intrusion of this kind of metaphysical principle into science as illegitimate. If vague appeals to simplicity can be transformed into precise principles of theory construction and if such principles are acceptable (in the sense roughly defined), then the virtue they indicate is not merely pragmatic. It may not be absurd to think that Nature is simple (in some carefully specified sense or senses), if we can point to the empirical success of science in vindication of our belief.

I do not know whether this third response, which I have merely sketched, will work in the end. Perhaps it could be shown (though it would be a far from trivial task) that, for any precise and acceptable sense of the term *simple*, one could concoct empirically equivalent and equally simple theories. I would not see this as the demise of scientific realism, for (and this is the second response again) I cannot see why the realist is barred from invoking a pragmatic virtue to deal with the problem of empirical equivalence just as the constructive empiricist does.

II | Theory and Observation

Antirealists need to draw a dichotomy between theory and observation. Van Fraassen is no exception: after all, he could not even distinguish truth from empirical adequacy without it. An old and powerful objection to antirealist views is that no such dichotomy exists. How does van Fraassen deal with this objection?

He first agrees that no such dichotomy can be drawn in scientific language, agreeing with the realist that "All our language is thoroughly theory-infected" (p. 14) [1071] and pointing out against the positivist that highly theoretical assertions can be made using only so-called 'observational vocabulary' (pp. 54–55). (Here I was reminded of how Popper formulated "There exists an omnipotent, omnipresent, and omniscient personal spirit" in a physicalistic observation language.)[6]

Van Fraassen does insist, however, that some objects and/or events

are observable and some not. He concedes the familiar realist point that there is a continuous spectrum between 'directly observing' an object and 'indirectly detecting' it using instruments. This only shows that *observable* is a vague predicate. But a vague predicate is perfectly usable provided it has clear cases and clear noncases—and this one has:

> A look through a telescope at the moons of Jupiter seems to me a clear case of observation, since astronauts will no doubt be able to see them as well from close up. But the purported observation of micro-particles in a cloud chamber seems to me a clearly different case—if our theory about what happens there is right . . . while the particle is detected by means of the cloud chamber, and the detection is based on observation, it is clearly not a case of the particle's being observed. (pp. 16–17) [1073]

What if we had microscopic or electron-microscopic eyes? (Actually, we do, only they are not built into our heads!) Could we not then observe things which at present we can only detect, showing that they were not unobservable in principle? Lockean speculations like this merely change the subject:

> The human organism is, from the point of view of physics, a certain kind of measuring apparatus. As such it has certain inherent limitations—which will be described in detail in the final physics and biology. It is these limitations to which the "able" in "observable" refers—our limitations, *qua* human beings. (p. 17) [1074]

But current physics and biology tell us that what is observable by humans varies (some of us are color-blind) and depends on our particular evolutionary history (other organisms can observe things we cannot). So, even if we can draw a rough and species-specific distinction between what is observable by humans and what is not, should any philosophical significance be attached to it?

Van Fraassen agrees with the realists against the idealists that it has no *ontological* significance: things that humans do not happen to be able to observe may nonetheless exist (p. 18) [1074]. (Actually, I will argue later, there are problems about van Fraassen's making this concession.) But he wants to give the distinction an *epistemological* significance: humans should never believe to be true a theory about what they cannot observe; instead, they should believe such theories only to be empirically adequate, to tell the truth about what they can observe (p. 18) [1074].

Can a distinction which is admitted to be rough-and-ready, species-specific, and of no ontological significance really bear such an epistemological burden? Van Fraassen gives an example of a so-called inference to the best explanation:

> I hear scratching in the wall, the patter of little feet at midnight, my cheese disappears—and I infer that a mouse has come to live with me. Not merely that these apparent signs of mousely presence will continue, not merely that all the observable phenomena will be as if there is a mouse; but that there really is a mouse. (pp. 19–20) [1076][7]

Will not the same style of argument lead us to the conclusion that there really are electrons (or whatever)? Van Fraassen thinks not. He accepts "inference to the best explanation" but puts his own gloss upon it: such inferences should (and do) only lead us to accept the best explanation as empirically adequate (p. 20) [1076]. If the best explanation is a theory about the observable, then empirical adequacy and truth coincide and we can (and do) conclude that there really is a mouse (or whatever). But if the best explanation is a theory about the unobservable, empirical adequacy and truth do not coincide and we cannot (and do not) conclude that there really are electrons (or whatever).

There is an empirical claim here (about what scientists actually do infer) and also a methodological claim (about what they ought to infer). I find the methodological claim quite unreasonable. On any plausible theory of evidential support, one would have to admit that there could be far better evidence for an explanation couched in terms of unobservables than for an explanation couched in terms of observables. Is the evidence for the existence of electrons better or worse than the evidence for the existence of the yeti or of the mouse in van Fraassen's wainscoting? It is a curious sort of empiricism which sets aside the weight of *available* evidence on the ground that a casual observer might one day see his mouse or yeti, while the scientist can never see (but can only detect) his electrons.

Van Fraassen's factual claim (that scientists do infer only the empirical adequacy of theories about the unobservable but never their truth) is even harder to swallow. Admittedly, I have not done a sociological survey to settle the matter. And, even if such a survey were to reveal, as I believe it would, that realism is the instinctive philosophy of working scientists, this would not of course settle the methodological question. But to indicate how difficult it is to avoid realist ways of thinking and talking, let us see how van Fraassen thinks and talks. He talks of *detecting* an electron in a cloud chamber. Can one say truly that one has detected an object without also believing it to be true that the object really exists? Later he describes how Millikan *measured* the charge of the electron (pp. 75–77). Did not Millikan think it true, and does not anyone who accepts Millikan's results think it true, that electrons exist and carry a certain charge? Can one say truly that one has measured some feature of an object without also believing that the object really exists?

I shall quote at some length what I *think* is van Fraassen's answer to very obvious questions like these:

> The working scientist is totally immersed in the scientific world-picture. And not only he—to varying degrees, so are we all. . . . But immersion in the theoretical world-picture does not preclude "bracketing" its ontological implications. . . . To someone immersed in that world-picture, the distinction between *electron* and *flying horse* is as clear as between *racehorse* and *flying horse*: the first corresponds to something in the actual world, and the other does not. While immersed in the theory, *and* addressing oneself solely to problems in the domain of the theory, this objectivity of *electron* is not and cannot be qualified. *But this is so whether or not one is committed to the truth of the theory.* It is so not only for someone who believes, full stop, that the theory is true, but also for . . . someone who . . . holds commitment to the truth of the theory in abeyance. For to say that someone is immersed in theory . . . is not to describe his epistemic commitment. . . . it is possible even after total immersion in the world of science . . . to limit one's epistemic commitment while remaining a functioning member of the scientific community. (pp. 80–83)

This is, I fear, nothing but a sleight-of-hand and an endorsement of philosophical schizophrenia. The sleight-of-hand converts belief in the reality of electrons (belief in the objectivity of *electron*, belief that the term *electron* corresponds to something in the actual world) into belief in, belief full stop in, and finally commitment to something called "the theory of electrons." But there have been several theories about electrons, and no scientist believes them all to be true. As for the most up-to-date theory about electrons, sensible scientists would do well not to believe it to be wholly true either, for details of it are quite likely to be further refined. All this is quite consistent with a pretty firm belief in the reality of electrons, with a refusal to "bracket" this particular ontological implication of science. The philosophical schizophrenia stems from talk of immersion (even total immersion) in the "scientific world-picture" or the "world of science." These metaphors are meant to suggest, if I understand them rightly, that scientists should believe in electrons or whatever while immersed in their scientific work, but should become agnostic about everything they cannot observe once they leave their laboratories. I suppose that split-minded scientists like this are possible, but I wonder whether they are desirable.

Finally in this section, I want to argue that van Fraassen's treatment of the observable/unobservable distinction verges on the incoherent. He insists that what is observable by humans is a "function of facts about us *qua* organisms in the world," so that it is for *science* to tell us what is observable and what is not (pp. 57–58).[8] Now, suppose some theory T does distinguish "the observable which it postulates from the whole it postulates" (p. 59). T might even be van Fraassen's "final physics and biology," if such a theory is possible. T will say, among other things, that

A is observable by humans, while *B* is not. Of course, if we are to use *T* to delineate the observable, we must *accept* it. But van Fraassen cannot have us accept it as true, since it concerns in part the unobservable *B*. The constructive empiricist can accept *T* only as empirically adequate, that is, believe to be true only what *T* says about the observable. But "*B* is not observable by humans" cannot, on pain of contradiction, be a statement about something observable by humans. And, in general, the consistent constructive empiricist cannot believe it to be true that *anything* is unobservable by humans. And, if this is so, the consistent constructive empiricist cannot draw a workable observable/unobservable dichotomy at all.

It might be objected that

1 *B* is not observable by humans

is logically equivalent with

2 Everything observable by humans is distinct from *B*

[and] since (2) is a statement about the observable, so is the logically equivalent (1). But even accepting that there is a sense in which (2) is "about" the observable, it is *also* about the unobservable *B* and therefore cannot be accepted as true by the constructive empiricist.

Nor does it help if we say that "observable by humans" is an "observational predicate," that we humans can tell from observation that a thing is observable by us. For one thing, this marks a retreat from van Fraassen's insistence that there is no observable/unobservable dichotomy in scientific *language*. For another thing, "observable by humans" will be a nonstandard observational predicate whose negation is not also observational, a predicate akin, for example, to the predicate "is an inscription of finite length." For we cannot observe that anything has a property without also observing that thing. Anyone who claims to have *observed* that something is unobservable contradicts himself. But if "unobservable by humans" is *not* an observational predicate, our conclusion stands. We can grant that "observable by humans" is an observational predicate so that the constructive empiricist can accept as true, on the basis of observation, statements of the form "A is observable by humans." But the consistent constructive empiricist cannot accept as true, on the basis of observation or anything else, a statement of the form "*B* is not observable by humans." Constructive empiricism requires a dichotomy which it cannot consistently draw.

III | Realism and Explanation

Realism and explanation are doubly linked. Realists think science explains facts about the world, and they think realist philosophy of science explains facts about science. I will consider the latter claim first.

The claim is that only a realist philosophy of science can explain the fact that science has had a great deal of predictive success. If the unobservables postulated by (successful) science really exist and if what (successful) science says about them is true or nearly so, then we can explain predictive success. Otherwise, such success is just a lucky accident. As Putnam famously remarks, realism is "the only philosophy that doesn't make the success of science a miracle" (cited on p. 39) [1083].

Van Fraassen gives short shrift to this Ultimate Argument for realism:

> The explanation provided is a very traditional one—*adequatio ad rem*, the "adequacy" of the theory to its objects, a kind of mirroring of the structure of things by the structure of ideas—Aquinas would have felt quite at home with it.
>
> . . . Will this realist explanation with the Scholastic look be a scientifically acceptable answer? I would like to point out that science is a biological phenomenon, an activity by one kind of organism which facilitates its interaction with the environment. And this makes me think that a very different kind of scientific explanation is required.
>
> I can best make the point by contrasting two accounts of the mouse who runs from its enemy, the cat. St. Augustine . . . provided an intentional explanation: the mouse *perceives that* the cat is an enemy, hence the mouse runs. What is postulated here is the "adequacy" of the mouse's thought to the order of nature: the relation of enmity is correctly reflected in his mind. But the Darwinist says: Do not ask why the *mouse* runs from its enemy. Species which did not cope with their natural enemies no longer exist. That is why there are only ones who do.
>
> In just the same way, I claim that the success of current scientific theories is no miracle. It is not even surprising to the scientific (Darwinist) mind. For 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. (pp. 39–40) [1084]

Amusing though this is, it does no more than play cat-and-mouse with the argument. The scientist does ask why the mouse runs from the cat and answers in roughly the terms made fun of here: the mouse perceives the cat, perceives the cat as an enemy, and runs. This does not commit the

scientist to ascribing thoughts, adequate or otherwise, to the mouse: his response might be quite instinctive. But with this proviso, there is nothing unscientific or un-Darwinian about this kind of explanation. Of course, the Darwinian question is not "Why does the mouse run away from the cat?" but, rather, "How did this piece of mouse behavior evolve?" The Darwinian answers this question roughly in the terms suggested by van Fraassen: given an environment full of mouse-hunting cats, cat-fleeing mice are more likely to survive, reproduce, and pass their cat-fleeing behavior on to future generations. But the Darwinian explanation is not a substitute for the "intensional" one, for they are addressed to quite different questions. The Darwinian explains what the "intensionalist" postulates: that the mouse's perceiving the cat as an enemy (or, better, the mouse's genetically programmed behavioral response to cats) is adequate to the order of nature.

Just as with cats and mice, so also with scientific success. It is one thing to explain why some theory is successful and quite another to explain why only successful theories survive. Van Fraassen's Darwinian explanation of the latter can be accepted by realist and antirealist alike. But to say that only successful theories are allowed to survive is not to explain why any particular theory is successful.

Not that a realist explanation of this in terms of the theory's *adequatio ad rem* will do as it stands. The Ultimate Argument is actually very old, and a brief look at an old example of it should give us pause. Christopher Clavius (in his *Commentary on the Sphere of Sacrobosco* of 1581) said it was incredible to suppose that Ptolemaic astronomy could correctly predict eclipses even though its eccentrics and epicycles were mere figments. But the eccentrics and epicycles were figments, and it was no miracle at all that a geometrical model expressly devised to yield some phenomenal regularity (periodic eclipses) should be successful in doing so. It is different, however, if a theory devised to accommodate some phenomenal regularities should turn out to predict *new* regularities. The realist has a ready explanation: the entities postulated by the theory really exist, and what the theory says about them is true (or nearly so). The antirealist seems forced to say that figments dreamed up for one purpose have turned out, miraculously, to be well adapted for a quite different purpose. So it was that thoughtful realists such as William Whewell distinguished two kinds of predictive success (predicting known effects and predicting new ones) and argued that the antirealist cannot explain the latter. So it was that a thoughtful antirealist such as Duhem, seeing the force of the argument, came to spice his instrumentalism with a whiff of realism: a theory is able to make successful *novel* predictions because it is not "a purely artificial system" but, rather, "a natural classification [whose] principles express profound and real relations among things."[9]

As this brief historical excursion shows, the only form of the Ultimate Argument which *might* work is that which focuses on *novel* predictive

success. Yet this focus is lacking in recent discussions, both from prominent defenders of the argument (such as Putnam) and from prominent critics of it (such as Laudan).[10] Difficulties remain, of course, not least that of making precise the intuitive distinction between known effects and novel predictions. These difficulties notwithstanding, van Fraassen has done nothing to impugn the Ultimate Argument in its refined form.

The Ultimate Argument proceeds on the metalevel: epistemology is to be naturalized, and philosophy of science is to explain facts about science. But there is a more direct argument, which proceeds from the assumption that science should explain facts about the world. The connection between the demand for explanation and the realist demand for true theories is apparently very obvious. An explanation is not adequate unless what does the explaining is true. So, given that theories figure in explanations, adequate explanations require true theories. It is worth noting that Duhem found the argument cogent and confessed that since in his view science did not aim at true theories, it could not explain anything either. Others adopt the curious view that science does aim truly to describe the world but cannot really explain any features of it. Van Fraassen's response to our simple argument is twofold. He attacks the realist demand for explanation; and he argues that explanation, where it can be had, does not require true theories, that explanatory power is a pragmatic virtue for which an empirically adequate theory will do just as well as a true one.

He softens us up with a linguistic point. We still speak of explanations even when we think the explanatory principles false:

> I say that Newton could explain the tides, that he had an explanation of the tides, that he did explain the tides. In the same breath I can add that this theory is, after all, not correct. Hence I would be inconsistent if by the former I meant that Newton had a true theory which explained the tides. (p. 99)

Quite so. We can speak without contradiction of a false explanation because truth is not a *defining* condition of explanation but an *adequacy* condition upon it. (In a similar way, we can say that Bode's Law is false without contradicting ourselves.)* Van Fraassen says that we may agree

* Despite its name, Bode's law is not a law. It is a formula that says that the distances of the planets from the sun are in the following ratio: (0 + 4), (3 + 4), (6 + 4), (12 + 4), (24 + 4), and so on. The formula fits the planets from Mercury to Uranus quite well, if we include the asteroids as a "missing planet" between Mars and Jupiter. But Neptune violates the series, since its distance calculated from Bode's law is 388, and its actual distance is about 300. So the formula is false and hence not a law. The interesting question, of course, is whether we would count Bode's law as a genuine law if there were no exceptions to it. Our reluctance to do so would stem largely from the fact that, as yet, no one has given a convincing explanation of why it holds (in the cases that it does). It stands isolated from the rest of science as a cosmic coincidence. See chapter 7 for further discussion of the distinction between laws and accidental generalizations.

that a theory is false without at all undermining our previous assertion that it explained many phenomena (p. 98). But could we say that a theory *adequately* explained many phenomena even though it is false? Realists think not. Scientists appear to agree: no modern astronomical text cites the vortex theory of planetary motion as the explanation of why the planets all go around the sun in the same direction, though some may have no other explanation of this fact. If this is wrong, and truth is not required for adequate explanation, then it will take more than a linguistic point to show it—van Fraassen gives us more.

First, he attacks the realist demand for explanation. He claims that in science this demand is severely limited, that explanation is not a "pre-eminent" or "rock-bottom" scientific virtue:

> If explanation of the facts were required in the way consistency with the facts is, then every theory would have to explain every fact in its domain. Newton would have had to add an explanation of gravity to his celestial mechanics before presenting it at all. (p. 94)

But Newton did present his theory. He "declined to explain," admitting famously that he had "not been able to discover the cause of . . . gravity" (p. 94). Thus:

> Newton's theory of gravitation . . . did not (in the opinion of Newton or his contemporaries) contain an explanation of gravitational phenomena, but only a description. (p. 112)

It is the same in modern physics, where the "unlimited demand for explanation leads to a demand for hidden variables, which runs counter to at least one major school of thought" (p. 23) [1077]. And, quite generally, to demand that regularities be explained and shown to be more than cosmic coincidences is self-defeating. For what of the regularities postulated to do the explaining?

Something is obviously wrong somewhere in all this. On the one hand, Newton explained the tides (p. 99) and, on the other hand, Newton's theory did not explain gravitational phenomena at all (p. 112). What has gone wrong is a tacit conflation of realism with essentialism, of the demand for explanation with the demand for ultimate explanation. This will take a bit of explaining.[11]

Suppose we explain the phenomenal regularity that sticks look bent when half-immersed in water by postulating, among other things, that unobservable light rays refract when passing through media of different densities. This is not, of course, to explain the refraction of light, though we might then try to do so. But at any point in our explanatory endeavors there will be things for which we have no explanations, namely the deepest explanatory principles we have reached at that point. One realist response

to this situation is to demand that these principles require explanation in their turn. Another realist response, quite the antithesis of the first, is to demand that our deepest explanatory principles should require no explanation, that they should somehow be ultimate or self-explanatory. This second response is central to the tradition of Aristotelian essentialism, the tradition which holds that the only genuine explanations are ultimate or self-explanatory explanations.

The essentialist tradition is buttressed by rhetorical questions like these. Do we *really* explain why sticks look bent in water by postulating some mysterious and unexplained law of refraction? Did Newton *really* explain the tides by postulating his mysterious and unexplained force of gravity? The intuition is that a *real* explanation does not remove one mystery by postulating another one. And behind this intuition lies another one: the intuition that a real explanation should serve the *pragmatic* function of removing puzzlement, setting the curiosity of the inquirer at rest. It is because nonultimate scientific explanations do not serve this pragmatic function, do not remove puzzlement so much as relocate and enhance it, that they are said not to be real explanations at all.

I use the term *pragmatic* here advisedly and roughly as van Fraassen uses it (sometimes). Whether an explanation removes puzzlement depends very much on the person we are considering. What relieves one man's puzzlement may enhance the next woman's. I dare say that some of the most efficacious puzzlement relievers in the history of thought have been explanations which are not scientific at all and which are, from a scientific point of view, quite inadequate (what about "God moves in mysterious ways," said in explanation of anything whatever which is puzzling?). I dare say that on occasions the incurious have had their puzzlement removed by a scientific explanation—but they should not have. For if it is feelings of puzzlement we want to get rid of, we should turn not to science but to the whiskey bottle!

I think that essentialism and the intuitions which lie behind it are to be rejected. And I think it one of Newton's chief claims to methodological fame that he was among the first to see this. Newton admitted that he could not explain gravity *and* that gravity was a perfectly proper thing to try to explain (since it was not an "essential property" of matter). Yet in the same breath he insisted that his theory of gravity did explain celestial motions and the tides:

> Hitherto we have *explained* the phenomena of the heavens and of our sea by the power of gravity, but we have not yet assigned the cause of this power . . . hitherto I have not been able to discover the cause of . . . gravity from phenomena, and I frame no hypotheses . . . to us it is enough that gravity does really exist, and act according to the laws we have explained, and abundantly serves to *account for* all the motions of the celestial bodies, and of our sea.[12]

Elsewhere Newton contrasted his procedure with that of his opponents, the Cartesian essentialists: he gives precise deductive explanations; they mutter about essences and can explain nothing:

> To tell us that every Species of Things is endow'd with an occult specifick Quality by which it acts and produces manifest Effects, is to tell us nothing: But to derive two or three general Principles of Motion from Phaenomena, and afterwards to tell us how the Properties and Actions of all corporeal Things follow from these manifest Principles, would be a very great step in Philosophy, though the Causes of those Principles were not yet discover'd: And therefore I scruple not to propose the Principles of Motion above mention'd, they being of very general Extent, and leave their Causes to be found out.[13]

Van Fraassen quotes from the first of these famous passages (p. 94), but he misunderstands Newton (as Duhem and others have misunderstood him) in a way that can only stem from a tacit conflation of realism with essentialism. He says that Newton "decline[d] to explain" (p. 94) and that Newton's opinion was that his theory "did not contain an explanation of gravitational phenomena, but only a description" (p. 112). But Newton explicitly claimed to have explained or accounted for gravitational phenomena such as the tides *by* describing precisely how gravity works. The antithesis between explanation and description is quite illusory: we explain one thing *by* describing another. Newton did decline to explain gravity. But to take this as a confession that nothing can be explained by the law of gravity is to father upon Newton a view that was not his.

It is, however, the view which van Fraassen calls the "realist demand for explanation" and attacks. He formulates the demand innocently enough as "every theory should explain every fact in its domain" and then takes "every fact" to include *the theory itself* (p. 94, quoted earlier). Only a theory which was somehow self-explanatory could meet this demand. But one can demand explanation without also demanding ultimate or self-explanatory explanation, as Newton tried to teach us. Van Fraassen's rejection of the latter demand leaves the former quite intact.

The tacit conflation of explanation with ultimate explanation also emerges from the delightful joke which forms the last chapter of van Fraassen's book. There, taking his cue from the remark that "everyone believed in the existence of God until the Boyle lectures proved it" (p. 229), he modifies Aquinas's Five Ways into proofs of scientific realism. What gets proved is actually Aristotelian essentialism, which is perhaps not surprising considering the provenance of the arguments. The First Way gives the flavor of the whole:

> *So I argue*: Everything that is to be explained, is to be explained by something else. That some things are to be explained is evident, for the regularities in natural phenomena are obvious to the senses and surprising to the intellect. So we must either proceed to infinity, or arrive at something which explains, but is not itself, a regularity in the natural phenomena. However, in this we cannot proceed to infinity. (pp. 205–6)

Now, what gets "proved" here is that science can achieve something which explains but is not itself a regularity in the natural phenomena, something, in other words, which is not itself to be explained. What gets proved is the essentialist view that science can achieve ultimate explanation. But the "proof" has a missing premise (required to obtain the statement beginning with "So"): the essentialist principle that *A* does not really explain *B* if *A* also requires explanation and has not received it. Van Fraassen hopes that readers skeptical of ultimate explanations will reject the conclusion, miss the missing premise, and infer by *modus tollens* that science cannot explain anything at all. But I am spoiling a clever joke.

I cannot leave this topic without saying a word about van Fraassen's favorite example of how science has transcended realist demands for explanation, quantum mechanics. Hidden variable explanations of quantum mechanics are said to "run counter to at least one modern school of thought" (p. 23) [1077]. A philosophy of science is not refuted by pointing out that it runs counter to a scientific school of thought, not even a dominant school of thought. But this school can point to *proofs* that hidden variable theories are impossible, and this should give the realist pause. Van Fraassen's comments on these proofs (which he seems to approve of) reveal some interesting things. One proof apparently assumes that "if we cannot point to some possible differences in empirical predictions, then there is no real difference at all" between two theories (p. 34) [1081]. In endorsing this proof, van Fraassen's earlier resolute antipositivism has wavered, for this assumption requires a positivist reinterpretation of scientific language to show that empirically equivalent theories are really the same theory. Not that realists would be too happy with an explanation of quantum mechanics which was demonstrably empirically equivalent with it: such an explanation could have no *independent* evidence in its favor. Now, if we assume the truth and completeness of quantum mechanics (or even its empirical adequacy and completeness), we will be able to prove that no explanation of it could have independent evidence in its favor. And if we assume that quantum mechanics is not only true and complete but also ultimately so, we will be able to prove that no explanation of it (independently confirmable or not) will be adequate. These various assumptions are no part of quantum mechanics; rather, they are philosophical assertions about quantum mechanics. Insofar as the various proofs rest upon assumptions like these (I do not know whether they do), they are not so much proofs as philosophical arguments, and pretty questionable ones to boot. Finally, the issue of determinism, important though it is in other contexts, is something of a red herring in this context. It is true that

some hidden-variable theorists wanted a deterministic explanation of quantum mechanics. But there is no *a priori* reason why a deeper explanation of quantum mechanics has to be deterministic.

Van Fraassen says nothing to impugn a modest realist demand for nonultimate explanation. And realists can defend such a demand by pointing to cases where the attempt to explain, even to explain theories regarded as empirically adequate, has paid off handsomely. Van Fraassen is not impressed with the argument:

> Paid off handsomely, how? Paid off in new theories we have more reason to believe empirically adequate. But in that case even the anti-realist, when asked questions about *methodology* will *ex cathedra* counsel the search for explanation! We might even suggest a loyalty oath for scientists, if realism is so efficacious. (p. 93)

Realists might retort that explanation has a payoff in terms of *understanding* the world—but that is unlikely to impress van Fraassen. And realists who are also empiricists are impaled on the horns of a dilemma here (as the case of the hidden-variable theories suggested). Realists who are also empiricists will want any proposed explanation to yield empirical regularities other than those it was devised to explain; otherwise there could be no independent evidence for the truth of the explanation. If an explanation does yield them, then the constructive empiricist can value it, too, but for its predictive rather than its explanatory power. If an explanation does not yield them, then it should be rejected as mere "metaphysical baggage." Heads constructive empiricism wins, tails realism loses:

> I think we must conclude that science, in contrast to scientific realism, does not place an overriding value on explanation in the absence of any gain for empirical results. . . . the point is that the true demand on science is not for explanation *as such*, but for imaginative pictures which have a hope of suggesting new statements of observable regularities and of correcting old ones. (p. 34) [1081–82]

This true demand is not, it seems, to be vouchsafed to scientists themselves. They are to take an oath of loyalty to realism, the desire to understand the world, and the search for explanatory truths. Realism is the constructive empiricist's Noble Lie, propounded *ex cathedra* in case scientists should find the true aim (enhancing the empirical adequacy of "imaginative pictures") uninspiring! More seriously, is van Fraassen right to say that "the interpretation of science, and the correct view of its methodology, are two separate topics" (p. 93)? I think it preferable to have an interpretation of science which harmonizes with methodological pronouncements.

At any rate, this is part of what van Fraassen means when he calls

explanation a *pragmatic* virtue. The search for explanation *works* because theories which are good explainers will *ipso facto* be good savers of phenomena and the real "name of the game is saving the phenomena" (p. 93). But this is not all that he means. The rest is meant to undercut the simple realist idea that adequate explanations must contain theories that are true (or nearly so). Van Fraassen defends a "pragmatic" analysis of explanation according to which theories do not figure in explanations at all but somehow lie behind or underpin them. And he says that good explanations can be underpinned by empirically adequate theories just as well as true ones. At least, I think that is what is going on.

Charles Morris divided the study of language into syntax, semantics, and pragmatics. The last was meant to deal, among other things, with *context dependence*, as when the truth or falsity of "I'm hungry" depends upon the context of utterance, upon who says it and when. In philosophical circles, "pragmatic" (or better "merely pragmatic") has also come to mean "useful but not true." There is no obvious connection between these two philosophical usages. Utterances can express both truths and falsehoods in virtue of contextual factors. And an utterance can be useful but not true whether or not contextual factors enter into it. ("He went thataway" may be useful for diverting the pursuer though it is false; it is also heavily context-dependent. "John Brown took the road to California" may be similarly useful but false, and it is less context-dependent.)

Van Fraassen thinks that explanation is also heavily context-dependent. He begins from Bromberger's puzzle about explanatory asymmetries: the height of the flagpole explains the length of its shadow but not vice versa, though the two deductions may be structurally identical. (Actually, van Fraassen also tells a blue story which is meant to show—I do not think it does show it—that there are "contexts" in which the length of the shadow does explain the height of the flagpole.) The obvious solution to this puzzle is an appeal to causality: explanations exhibit causes, while nonexplanatory deductions do not. But this is to jump out of the contextual frying pan into the contextual fire, for which factor is picked out as "the cause" also varies enormously with the context:

> . . . [T]he salient feature picked out as "the cause" . . . is salient to a given person because of his orientation, his interests, and various other peculiarities in the way he . . . comes to know the problem—contextual factors. (p. 125)

Now, John Stuart Mill, who first drew attention to this kind of thing, insisted that only the entire constellation of factors, amounting to a sufficient condition for the event to be explained, is really entitled to be called the cause of it. And John Mackie, with Mill in spirit though more amenable to ordinary ways of talking, said that a cause is an insufficient but necessary part of an unnecessary but sufficient condition. Both Mill and Mackie can admit that contextual factors may influence *which event a*

person wants explained. And this is enough to dispose of many of the usual examples adduced to demonstrate context dependence of the explanations given (car crashes, fires, and such like).[14]

Van Fraassen takes a different course: he accepts the context dependence of explanations and tries to make it more precise. An explanation "is an *answer* . . . to a why-question" (p. 134). Every why-question has a *topic* (if we ask, "Why *P*?" the topic is *P*), an implied *contrast-class* (what we actually ask is, "Why *P* rather than *Q*, *R*, etc.?"), and an implied relation of *explanatory-relevance* which determines what shall count as a possible answer to the question (pp. 142–43). The topic, contrast class, and relation of explanatory relevance all depend upon the context, in particular upon "a certain body *K* of accepted background theory and factual information," which in turn depends "on who the questioner and his audience are" (p. 145). This gets complicated. The upshot of it is that

> the discussion of explanation went wrong at the very beginning when explanation was conceived of as a relationship like description: a relation between theory and fact. Really it is a three-term relation, between theory, fact, and context. . . . So to say that a given theory can be used to explain a certain fact, is always elliptic for: there is a proposition which is a telling answer, relative to this theory, to the request for information about certain facts (those counted as relevant for *this* question) that bear on a comparison between this fact which is the case, and certain (contextually specified) alternatives which are not the case. (p. 156)

Is it *really* elliptic for all this? At the outset of his discussion, in order to combat "the increasing sense of unreality" the usual examples bring, van Fraassen sets forth three "workaday examples of scientific explanation" (pp. 101–3). And he manages to set these examples forth *without mentioning contextual factors at all.* There are why-questions all right, but there are no implied contrast classes, relevance relations, or anything else which depends "on who the questioner and his audience are." Contextual complications have little to do with explanation in science, if van Fraassen's own "workaday examples" are anything to go by.

Is it true to say that explanation was ever "conceived of as a relationship like description: a relation between theory and fact"? The orthodox account says that explanations are arguments in which three things figure: theories or general laws, initial conditions specifying the cause of the event being explained, and in the conclusion a statement of the event being explained. Explanations *contain* descriptions, but they are not *like* them.

Van Fraassen (his earlier examples notwithstanding) wants to drop the theories out of explanations and relegate them to the "context," to the "background information" relative to which why-questions are asked and answered. (Here I was reminded of the Wittgensteinians, who assimilate theories to rules of inference and insist that these rules do not figure as

premises in the inferences constructed in accordance with them.)[15] Still, one might think, the theories must be true if the explanations proffered in the light of them (or constructed in accordance with them) are to be correct. Van Fraassen thinks not. Empirically adequate and empirically strong theories will do just as well as true ones. His argument seems to be as follows. The fact to be explained is always an observable fact. The facts cited in explanation of it are also always observable facts. So what the theory has to get right, to be a good explainer, are just the observable facts. And empirically adequate theories, by definition, do just that:

> So scientific explanation is not (pure) science but an application of science. It is a use of science to satisfy certain of our desires; and these desires are quite specific in a specific context, but they are always desires for *descriptive information.* . . . in each case, a success of explanation is a success of *adequate and informative description* [*of the phenomena*]. And while it is true that we seek for explanation, the value of this search for science is that the search for explanation is *ipso facto* a search for empirically adequate, empirically strong theories. (pp. 156–57)[16]

I am not sure that I have got the argument right here—but, if I have, there is lots wrong with it. Sometimes we explain observable facts by citing other observable facts (and laws). But this is not always the case, though it tends to be the case in the usual philosopher's examples, which bring an "increasing sense of unreality" to the subject. The flagpole is a good example again: both its height and the length of its shadow are presumably observable facts. (I can scarcely bring myself to mention the "explanation" of why some bird is black, which consists in pointing out that it's a raven and they are all black!) But, in van Fraassen's own examples of scientific explanations, there are initial conditions such as: the specific heats of water and copper are 1 and 0.1, respectively; the earth's magnetic field at a certain point has a vertical component of approximately $5/10^5$ Tesla; the energy levels associated with stable electron orbits in hydrogen atoms take the form $E_n = -E_o/n^2$ where E_o is called the ground state energy. These initial conditions are *generalized* ones because the facts to be explained are *general* ones (with the possible exception of the second). But, setting aside the problem of their generality, they do not look much like observable facts, and their provision does not look much like providing descriptions of the observable phenomena. Nor, in science, is it always observable phenomena that we try to explain: we sometimes try to explain theories.

Van Fraassen not only thinks that explanation is a pragmatic affair in Morris's sense (context dependence), he also thinks that explanatory power is one of the *pragmatic virtues*, concerning which he says in general:

> In so far as they go beyond consistency, empirical adequacy, and empirical strength, they do not concern the relation between the theory and the world,

> but rather the use and usefulness of the theory; they provide reasons to prefer the theory independently of questions of truth. (p. 88)

If what I have said about scientific explanation is right, then the explanatory power of a scientific theory does depend on whether it tells the truth about the unobservable and therefore does go beyond empirical adequacy and empirical strength. But van Fraassen is obviously right when he says:

> Nor can there be any question of explanatory success as providing evidence for the truth of a theory that goes beyond any evidence we have for its providing an adequate description of the phenomena. (pp. 156–57)

This is obviously right, because empirical adequacy is *defined* as correctness so far as the observable evidence is concerned. Explaining things cannot provide a special sort of *evidence* that theories are true rather than just empirically adequate. Realists, made of sterner stuff than constructive empiricists, still demand that a theory be true for the explanations in which it figures to be adequate. And realism carries this much metaphysical baggage: realists can point to no evidence over and above evidence of empirical adequacy that their sterner requirement has been met.

But there is excess baggage of a different kind in the constructive empiricist position. There is, above all, the philosophical excess baggage of defending an observable/unobservable distinction and giving it crucial epistemological significance. There is the excess baggage of providing an alternative to the obvious realist explanation of science's novel predictive success. And there is the excess baggage of a complex account of the pragmatics of explanation.

I suggested earlier that, in comparing constructive empiricism with scientific realism, we should assess the risks, penalties, and gains associated with each. The risks have been discussed, as have the penalties in the form of philosophical "excess baggage" of various kinds. As to the respective gains (or losses), I can only repeat a hackneyed point. The realist values theoretical science as an attempt to *understand* the world and sees continuity between commonsense and scientific knowledge. The constructive empiricist, browbeaten as much by the positivist emphasis on prediction as by esoteric problems in interpreting quantum theory realistically, jettisons understanding and seeks to drive a wedge between theoretical science and commonsense (taking his excess baggage aboard to do so).

Let me conclude by agreeing with van Fraassen that it would be a pity if scientific realism were to become a philosophical dogma. Bas van Fraassen's book certainly roused me from any dogmatic slumber to which I might have been prone. It has, in fact, given me sleepless nights! His antirealism is more viable than earlier antirealist positions. But, in philosophy of science as well as in science, viability directly depends on weakness. Constructive empiricism is weaker than earlier antirealist views in all kinds of ways, and correspondingly closer to realism. This is why I conclude, undogmatically I hope, that realism emerges a little bloodied but unbowed from its encounter with constructive empiricism.[17]

■ | Notes

1. *The Scientific Image* (Oxford: Clarendon Press, 1980), 8 [1066]. Henceforth, all page numbers in the text refer to this book. [The numbers in brackets refer to the excerpt from this book included as the second reading in this chapter.]

2. Here and in what follows I ignore, through lack of space, a central feature of van Fraassen's position, his preference for a semantic approach to scientific theories whereby they emerge as sets of models rather than as sets of (true or false) sentences. I have two excuses for this. First, in much of his own discussion van Fraassen ignores it too, and talks as though theories consisted of true or false sentences. Second, and more important, I think that there is little to choose between the two approaches from a logical point of view. As van Fraassen himself once wrote, "There are natural interrelations between the two approaches: an axiomatic theory may be characterized by the class of interpretations which satisfy it, and an interpretation may be characterized by the set of sentences which it satisfies. . . . These interrelations make implausible any claim of superiority for either approach" ("On the Extension of Beth's Semantics of Physical Theories," *Philosophy of Science* 37 [Sept. 1970]: 325–39; cf. p. 326). I am indebted, both for the general point and for the reference to van Fraassen's endorsement of it, to John Worrall's review article of *The Scientific Image*, ["An Unreal Image,"] *British Journal for the Philosophy of Science* [35 (1984): 65–80].

3. Here I assume that Berkeley's immaterialism is empirically equivalent to commonsense realism. I am not sure that this is so. Immaterialism can be formulated so as to be consistent with all possible experience and so irrefutable by it. But empirical adequacy should require more than mere consistency with the evidence; it should require (at least) that the theory in question entail the evidence. It might be argued that commonsense realism entails consequences about the stability and reobservability of physical objects which Berkeley's immaterialism does not. Berkeley does invoke God's benevolence to "explain" *post factum* the stability of tables and trees. But Berkeley cannot predict it because of his admission that God might always make an exception to his "laws of nature" and work a miracle instead.

4. Einstein was always appealing to simplicity or unity. For an analysis of how these vague appeals were articulated into quite powerful principles of theory construction, see E. Zahar, "Why Did Einstein's Programme Supersede Lorentz's (II)?" *British Journal for the Philosophy of Science* 24 (1973): 223–62.

Philosophers of science still, of course, lack a precise and general account of what simplicity is; perhaps there is none to be had and simplicity is, as van Fraassen says, a patchwork of virtues, some pragmatic and some not. Popper's identification of simplicity and strength works nicely sometimes ("All swans are white" is simpler and stronger than "All non-Australasian swans are white") and badly at other times ("All swans are white and ferocious" is less simple and stronger than "All swans are white").

5. For more detail of how certain metaphysical principles can be rationally assessed in this way, see Watkins, "Confirmable and Influential Metaphysics," *Mind* 67 (July 1958): 344–65, especially pp. 363–65. Watkins does not apply these ideas to principles of simplicity.

6. See K. R. Popper, *Conjectures and Refutations* (London: Routledge and Kegan Paul, 1963), 274–76.

7. Here, incidentally, there is a curious prejudice in favor of vision. True, I have not *seen* the mouse, but have I not *heard* it, and is this not a way of observing it? There is a curious tension in the view that, though we can see (and touch) things, we never hear (or taste or smell) things but only the *noises* they make (or the tastes and smells they emit). Notoriously, one way to resolve the tension is to say that we never really see things, either, but only visions (visual sense-data) caused by them (and the same will have to go, even more implausibly, for touch). That way leads to idealism. Realists resolve the tension by saying that we can hear, taste, and smell things as well as see and touch them. Van Fraassen, for all his talk about hearing "an apparent sign of mousely presence" rather than a mouse, is once again with the realists. He says that "sense-data, I am sure, do not exist" (p. 72). And he has to be with the realists if truth and empirical adequacy are to be the same so far as observable things are concerned. If all observable phenomena were only apparent signs of mousely presence (mousely visions, mousely noises, mousely smells, etc.), then "All observable phenomena are as if there is a mouse in the wainscoting" would not entail "There is a mouse in the wainscoting," contrary to what van Fraassen says on p. 21 [1077].

8. In case anyone is reminded here of the talk of "observables" in quantum mechanics, we should remind ourselves that the so-called "observables" of quantum mechanics are in the present context remotely calculable theoretical quantities. If electrons are not observable, neither is their charge, momentum, or spin.

9. P. Duhem, *The Aim and Structure of Physical Theory* (Princeton, N.J.: Princeton University Press, 1954), 28 (see also pp. 297ff).

10. For example, Laudan presents as historical counterexamples successful theories which did not genuinely refer and were not true or nearly true ("A Confutation of Convergent Realism," *Philosophy of Science* 48, no. 1 [March 1981], 19–49). But few, arguably none, of the theories cited had any *novel* predictive success. Laudan also saddles the realist with the principle that successful reference alone breeds success. I do not know if any realist has thought this, but no realist should think it. For, as Laudan shows, we can construct referring theories which will be quite unsuccessful: take a successful theory containing the term *t* and negate it. Successful reference is a necessary condition for truth (or near truth) but not a sufficient one. And when it comes to success, it is the truth (or near truth) of what a theory says about its theoretical entities which counts, not whether those entities exist. [Laudan's paper follows next in this chapter.]

11. Further detail can be found in my "Explanation, Description and Scientific Realism," *Scientia* 112 (1977), 727–55.

12. *Principia*, Book 3, General Scholium; Motte's translation, revised by Cajori, vol. 2 (Berkeley and Los Angeles: University of California Press, 1962), 546–47. Italics mine. Also important is the famous passage where Newton says he will treat

forces "not physically but mathematically" (*Principia*, Book 1, Definition 8; Cajori, vol. 1, 5–6). Newton is saying that he will describe how gravity works in precise mathematical terms, rather than try to explain it physically. He is often misinterpreted as saying that gravity does not really exist.

13. *Opticks*, Book 3, Query 31; Dover edition (New York, 1952), 401–2. The most extended defense of Newton and attack on Cartesian essentialism is, of course, Roger Cotes's preface to the second edition of the *Principia*. Yet Cotes is sometimes interpreted as defending the essentialist view that gravity is, after all that Newton had said to the contrary, essential to matter (for example, by Popper in *Conjectures and Refutations*, 106). It is unlikely that Newton would have allowed Cotes to defend a view he himself had specifically denied. Cotes himself specifically denied that this was the view he was defending, in a letter to Samuel Clarke, who had questioned him on the point (see Cajori's appendix, n. 6; vol. 2, 634–35).

14. This is shown in John Worrall's review article, referred to in note 2 above. Worrall also shows that we can dispose in the same way of van Fraassen's story about the length of a flagpole's shadow explaining its height.

15. For a discussion of this view, see my "Wittgensteinian Instrumentalism," *Theoria* 46 (1980), pts. 2–3, 65–105.

16. The italics in this quotation are mine. And I felt justified in adding "of the phenomena" to "description" because in the preceding sentence (not quoted here) the phrase "adequate description of the phenomena" occurs.

17. Previously published in a shorter form as "Constructive Empiricism versus Scientific Realism," *Philosophical Quarterly* 32 (July 1982): 262–71. Reproduced by permission. I am grateful to Cliff Hooker for his helpful suggestions about what I might focus attention upon in the paper and to Greg Currie, Bob Durrant, and Martin Fricke for comments on earlier versions.