Van Fraassen's Unappreciated Realism*

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What is not often noted about Bas van Fraassen's distinctive approach to the scientific realism issue is that constructive empiricism, as he defines it, seems to involve a distinctively realist stance in regard to large parts of natural science. This apparent defection from the ranks of his more uncompromisingly anti-realist colleagues raises many questions. Is he *really* leaning to realism here? If he is, why is this not more widely noted? And, more important, if he is, is he entitled to this shyly realist concession? Does his many-pronged attack on what he sees as the main arguments in support of realism leave him with the wherewithal?

In the spirited debates around the topic of scientific realism (SR) that have engaged philosophers of science in recent decades, the name that has most often, perhaps, appeared at the head of the anti-realist column is that of Bas van Fraassen. In his 1980 work, *The Scientific Image,* he divides philosophers of science into two opposing ranks, realist and anti-realist, and presents his own distinctive position, constructive empiricism (CE), as an expressly anti-realist one. What is not often noted, however, is that constructive empiricism, as he defines it, seems to involve a distinctively realist stance in regard to large parts of natural science. This apparent defection from the ranks of his more uncompromisingly anti-realist colleagues raises many questions. Is he *really* leaning to realism here? If he is, why is this

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‡This paper recalls one of the issues debated between Ernan McMullin and Bas van Fraassen on two earlier occasions: first, during the course of the annual Cardinal Mercier Lectures at the Catholic University of Leuven (Belgium) given by Ernan McMullin, and then during the annual Cuming Lectures at University College Dublin which took the form of an extended discussion between McMullin and van Fraassen on the more general theme: "Realism and anti-realism in the philosophy of science." The author wishes to express his gratitude to his hosts on both those occasions: Professor Carlos Steel (Higher Institute of Philosophy, Leuven), and Professor Dermot Moran (Dublin).

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not more widely noted? And, more important, if he is, is he entitled to this shyly realist concession? Does his many-pronged attack on what he sees as the main arguments in support of realism leave him with the wherewithal?[1]

1. Preliminaries: Aim and Acceptance. It may be best to begin from van Fraassen's idiosyncratic, and much-discussed, definitions of the two positions he presents as, on the face of it at least, totally opposed. I will focus on some ambiguities in the key terms he uses to set up the contrast. The two positions differ, he says, in regard to their *aims* (truth versus empirical adequacy), and in what they take *acceptance* of a theory to involve (belief that it is true versus belief that it is empirically adequate) (van Fraassen 1980, 1985, 1989, 1994; Ladyman et al. 1997).

First, in regard to aim, commentators have fretted about what it might mean for "science" to "aim at" something (see, for example, Rosen 1996). Van Fraassen can hardly be attributing to scientific realists and constructive empiricists sociological generalizations in regard to what scientists consciously intend as they go about their work. Under this reading of "aim", the debate between the two sides would be settled, not by philosophical argument but by the use of questionnaires. Rather, what the "aim" or "goal" of a particular kind activity may more plausibly be taken to be in a context like this is what the activity should achieve, if properly carried out.[2] And this is investigated by means of a careful study of the activity, of the criteria its practitioners employ, and so forth. Realists and constructive empiricists, van Fraassen is telling us, will come up with different answers. Later we shall see that this is only partly true.

The second element in the contrast van Fraassen draws between SR and CE concerns what it is they "accept" and how this relates to what they "believe". According to him, acceptance of a theory "clearly involves more than belief," (van Fraassen 1980, 8) whether one is a realist or a

1. I thought of titling this essay, "Realism in the Pays Bas" but decided that this suggestion would hardly pass editorial scrutiny!

2. There is an echo here of a very similar ambiguity in regard to the notion of *telos* or end in Aristotle's natural philosophy. Commentators, particularly those who are critical of Aristotle's doctrine, often take *telos* to be an intentional concept, implicitly involving the action of mind. Yet this cannot be what Aristotle had in mind when, for example, the *telos* of the natural motion of a stone, when disturbed, is said to be to return to the stone's natural level. The sort of *telos* which Aristotle finds everywhere in evidence in the operations of nature can be discovered from those operations themselves, regarded as part of a larger order. This implicit reference to an *order* favors the use of mind-related terms like "*telos*" in the first place, but the risk of misunderstanding, as history demonstrates, is real.

constructive empiricist. But this seems at odds with how these terms are ordinarily understood. "Acceptance", as he himself underlines, is a pragmatic term. To "accept" a theory is to commit to exploring its potential. Scientists can be said to "accept", to one degree or another, the theories they are working with. But this in no way implies that they believe either in their truth or even in their empirical adequacy. Acceptance, then, involves much *less* than belief. Acceptance is tentative and routine in scientific work. The issue of *belief* ordinarily does not come up in the course of that work. Scientists are not accustomed to face the question: "Do you really believe that your theory is true (or empirically adequate)?" And most would be loathe to answer "yes" to a query about the unqualified truth of the major theories, say, of contemporary physics.

Van Fraassen's definition of SR, in his own words, "equates acceptance of a theory with belief in its truth." But then he goes on to say that this "does not imply that anyone is ever warranted in forming such a belief" (van Fraassen 1980, 9). But if this were to be the case, the same doubt would apply as to whether anyone is ever warranted in *accepting* a theory from the realist standpoint. And this is surely questionable. Scientists and realist philosophers of science alike find no difficulty in accepting the major theories that form the background of current scientific work. But both groups would hesitate to say that this commits them to belief in the unqualified *truth* of the theories. To draw the contrast between SR and CE that van Fraassen evidently wishes to draw, it would be better to avoid altogether terms that carry pragmatic/psychological overtones, like "accept". Neither the realist nor the empiricist is making a claim about what working scientists take the success of their theories to connote. What the two sides dispute is what people in general (and not just scientists) are *entitled* to believe in such a case.

Believe about what? Here comes a fundamental correction. Scientific realism bears primarily on the *reality* of the theoretical entities postulated by the theory, as the term "realism" conveys. Does the success of electron theory warrant a qualified belief in the existence of electrons? The emphasis here is on reality, not on truth, directly on ontology rather than on epistemology. Of course, the two are connected. But what realists are concerned about are the existence-judgments that theory-success licenses. Truth is a more complex affair; it is not necessary for the realist to become embroiled in the notorious controversies about the nature of truth. But isn't it the case that if the success of a theory gives reason to believe that the entities postulated by the theory exist, it would give equal reason to believe that theory to be true? This is not as straightforward as it seems; more of that below. In the meantime, what matters is to hold on firmly to the quite simple idea that realism has to do with the reality of certain postulated entities.

2. Empirical Adequacy: Aim or Criterion? Van Fraassen's frequent use of the classic phrase, "saving the phenomena", as an equivalent to the desideratum he views as distinctive of CE should set up a warning flag. Though the phrase does not occur in those of Plato's works that remain, *sozein ta phainomena* appears to have its origin in the Platonic tradition, where saving the appearances is what astronomers are said to bring about by means of the mathematical combinations of the circular motions that Plato supposedly laid down as canonical.[3] Which phenomena? The phenomena at hand, accessible to the person making the assessment. Of course, the hope would be that the formalism would also "save" (now meaning "correctly predict") later phenomena. As time went on, the evident contrast between the physical astronomy of Aristotelian inspiration that set out to explain the motions of the heavenly bodies by means of carrier spheres, and the complex mathematical structures employed by the mathematical astronomers in the tradition of Ptolemy, whose first goal was simply to save the phenomena at hand, set a puzzle for natural philosophers in the Arabic and Latin Middle Ages. Saving the phenomena was first and foremost a practically applicable criterion, the criterion proper to the assessment of the formalisms of mathematical astronomers. And it was routinely contrasted with the criterion of explanatory success on which the physical astronomers relied.[4]

One can see why van Fraassen, like Duhem before him (Duhem 1969) would find in this ancient confrontation between the quasi-instrumentalism of mathematical astronomy and the realism of physical astronomy an early evidence of the division that contemporary philosophers of science know so well. And his sympathies would, of course, lie with those who were content to have their constructions judged solely by the criterion of saving the phenomena already at hand, setting aside the realism-explanation link. Note, however, that the context in this earlier discussion was ordinarily that of practical assessment; saving the phenomena was thus taken to be the criterion that an astronomical formalism should satisfy. And in that case it was a decidable matter whether the criterion was satisfied or not. To the extent that saving the phenomena might have been thought of as the aim that defined mathematical astronomy in epistemic terms, the phrase was clearly ambiguous: it could apply only to the data actually at hand, or it could apply to the class of relevant phenomena—past, present,

3. Whether Plato *would,* in fact, have allowed that the phenomena could be "saved" by a mathematical formalism seems doubtful. The phrase is not found in the remaining (fragmentary) Greek record until the second century A.D., in a passage by Sosigenes. The neo-Platonist, Simplicius, writing in the sixth century A.D., uses the phrase frequently.

4. The story is a very complex one to which it is difficult to do justice in short space (see McMullin 1984b).

and future. In context, the former might more plausibly qualify as the aim, the latter at best as a hope.

Turn now to empirical adequacy. The context is no longer the specific one of theory-assessment but the broader one of defining aim and acceptance of theory. True, empirical adequacy could (it would seem) be thought of as the criterion that directs theory-assessment in much the same way as saving the phenomena did in early astronomy. But this would not be van Fraassen's empirical adequacy. It is true that if a theory were known to be empirically adequate, it would follow that it saves the phenomena currently at hand. But the reverse is not the case: a theory that saves the phenomena at hand might very well not be empirically adequate. In the practical assessment of theory, one can tell whether or not a theory saves the phenomena, that is, satisfies the data already on hand. But one cannot in the same circumstances determine that a theory is empirically adequate. The latter does not, therefore, function properly as a criterion in actual theory-assessment because, as van Fraassen emphasizes, it "refers to *all* the phenomena; these are not exhausted by those actually observed [those to which the term 'saving the phenomena' ordinarily applies], nor even by those observed at some time, whether past, present, or future" (van Fraassen 1980, 12). The scope of the claim to empirical adequacy of T is *all* actual situations to which T could properly be said to apply.

This is what distances van Fraassen's brand of empiricism from other versions in the classical Humean tradition, as well as from both the positivism and instrumentalism that he sets aside on other (to my mind less persuasive) grounds (van Fraassen 1980, 10). Adherents to any one of those positions would be most unlikely to say, as he does in defining CE, that acceptance of T involves the belief that T is empirically adequate, in the expansive sense van Fraassen attributes to that latter term. They would be more likely, rather, to settle for the claim that acceptance of T involves only the belief that T saves the phenomena.

To avoid ambiguity, then, van Fraassen needs a different term in the context of actual theory-choice. Empirical adequacy sounds too much like the innocent criterion of saving the phenomena, an equivalence that his text promotes by using the two phrases as though they were interchangeable. If "saving the phenomena" sounds too cumbersome, something like "data-fit" might do. It has to convey as criterion that T fits, satisfies the data actually on hand, and no more than that. ("Accounts for," with its overtone of explaining, is a riskier choice.) There is no really satisfactory compact common term in English for this criterion; the oft-used "predictive accuracy" is also open to challenge, both because "predictive" suggests a reference to the future and because accuracy is not quite the emphasis desired in this context. It is the ambiguity of this phrase, "empirical adequacy", its ambivalent status as (ambitious) aim or (harmless) criterion that prevents many readers from realizing how far-reaching the consequences are of van Fraassen's employment of this phrase in defining CE.

3. Two Sorts of Theory: A Historical Note. According to constructive empiricist tenets, there are, effectively, two different sorts of theory, according as to whether the theoretical entities postulated by the theory themselves are observable or not. A brief return to the developing natural sciences of the seventeenth and eighteenth centuries may help to situate this distinction in a concrete way.

Natural philosophy in the Greek tradition was centered on the discovery of *natures,* of characteristic modes of activity that served as clues to essence. From observed regularity, then, one could infer to nature directly, and a case could be made for a demonstrative science of nature that would link these regularities in a hierarchy of essence and property. Less often, observed regularity, notably in the case of the heavenly bodies, became the source of a further round of inquiry, this time into what underlying causes, themselves unobserved, could account for the behavior in question. In his own account of how demonstration is to be understood, Aristotle gives as example inference to the nearness of the planets as explanation for the fact that they, and they alone, among the heavenly bodies do not twinkle, and he does his best to construe this inference as a demonstrative one. In this instance, the nearness of the planets is the theoretical element itself not observed, and the warrant for postulating it is the quality of the explanation it affords. A more striking illustration, of course, would be the (unobservable) carrier spheres, postulated in order to explain planetary motions in quasi-mechanical terms. Implicitly supposing that a combination of such spheres affords the only possible explanation allowed the inference in the *Physics* to the agency of spheres (though not to their actual number) to be regarded as demonstrative, at least in a broad sense (McMullin 2000).

With the broadening of scope that was perhaps the most striking characteristic of seventeenth-century inquiry into nature, this kind of reasoning to unobserved cause took on a more explicitly hypothetical character. There was, first of all, the enlargement of reach afforded by the telescope which gave evidence of entities, that though they were, in principle, observable were not close enough to be identified. Postulating the newly-sharp images of the lunar surface as evidence that the moon possesses mountains and seas much like those of earth afforded a striking example of a powerful mode of inference which was neither a simple inductive registering of regularity nor a straightforward deduction.

In the first day of his great *Dialogue on Two Chief World Systems,* Galileo recognizes the hypothetical character of his claim regarding the affinity between the terrestrial and lunar surfaces, but rests his case for it on the elegant explanation it affords of a variety of observed features of the lunar surface, particularly the changes over the course of the lunar day (McMullin 1978, 240–247). The lunar mountains and seas appear here as theoretical entities. The evidence for their existence under this description is given by the theory. And the term 'theory' itself is applied to something that affords an understanding (*theoria*) of something observed. The theory does not simply fit the data ("save the phenomena"), it explains in causal fashion how these appearances are brought about.

A more radical extension of the human reach encountered more obstacles and was slower in coming. The expansion here was into past time. Until then, history had been a matter of stories, genealogies, chronicles, that relied on human memory and record for its evidence. Its reach was limited and often uncertain. But in the investigations of the earth's surface that were gradually beginning at this time and that came to fruition in the work of Werner and Hutton in the later eighteenth century, a new kind of theory and a new kind of theoretical entity made their appearance. From the traces observed in the present, the new "geologists" were led to postulate whole sequences of events and processes in past time (Laudan 1987). Here the theoretical entities were unobserved and in one sense unobservable, that is, unobservable in practice by us. Yet they could be called observable in principle. They were of a kind broadly familiar to us, a point that Hutton, in particular, would stress. Were we to have been present in those distant ages, we might have observed the mighty changes in the earth's surface that now for the first time were coming within the reach of the human imagination.

Astrophysics (as Galileo's accounts of the lunar surface, of the natures of sunspots and comets would later be called) and geology, were the first developed examples of a new kind of science that reached where the limited inductive registration of observed regularity could not of itself go (McMullin 1996a). The distant cosmos of the present and the even more distant historical processes of the earth long past gradually took shape, populated by theoretical entities of all sorts. Galaxies, dinosaurs, mass extinctions of life, are just as much theoretical entities as are electrons; our confidence in their reality is supported by just the same sort of evidence in one case as in the other. Retroduction, the causal inference involved in this sort of theory-assessment, became the key to a whole suite of "natural sciences of the distant," that would eventually include such historical sciences as archaeology, physical anthropology, palaeontology, evolutionary biology.[5]

5. Peirce proposed two labels, "retroduction" and "abduction." The former seems preferable; it suggests a backwards movement: effect to plausible cause. In Peirce's own account, there is an ambiguity between two different aspects: the invention of a causal hypothesis and the epistemic *assessment* of that hypothesis. I use the term here primarily to denote the latter (see McMullin 1992).

Back now to van Fraassen: the theoretical entities in which these sciences terminate are, in large part, observable in his sense, i.e. observable, in principle, by human beings "were they to be there." We might call such theories *O-theories.* To describe such a theory as "empirically adequate", would be, in effect, to claim the *existence*—past, present, or future—of the distant "observable" entities postulated by the theory: the neutron stars or the dinosaurs or the asteroid impacts. O-theories characterize a very considerable segment of the natural sciences of today. When one recalls van Fraassen's claim that assenting to a theory involves belief in its empirical adequacy, one can now begin to sense the dimensions of the task that the constructive empiricist has set himself.

Just to complete the historical note, at the same time that Galileo was probing the lunar surface by a new and oblique form of reasoning, the imaginations of the natural philosophers were also stretching in another direction: scale. The new "corpuscular philosophy", with its echoes of ancient atomism, postulated a world of tiny particles far below the level of observation, in an effort to explain many of the properties of the observed realm. It is striking to see how confident virtually all the natural philosophers of the time were about the existence of these "corpuscles", though Locke for one was pessimistic about the prospects of constructing a science that would reach them. The theories were, indeed, much slower in coming than the sanguine rhetoric of Bacon and Boyle and the rest had led people to expect. But come they did in the nineteenth century: in chemistry, in the physics of gases, in biology. The theoretical entities at that point were unobservable (in van Fraassen's terms) in principle. And the theories that relied on them might be called U-theories, if one is to maintain the strong distinction van Fraassen sees as essential to his brand of empiricism.[6]

One science where the new sort of theoretical entity proved particularly problematic was mechanics. What was one to make of the attractive forces that Newton described as responsible for planetary motion? What (his critics urged) *could* one make of such entities? The challenge here was primarily to their ontological status, and thus in consequence to the implicit claim that they explain, on which the retroduction might be thought,

6. Stathis Psillos distinguishes between "horizontal" IBE (inference to best explanation) which "involves only hypotheses about unobserved but observable entities" and "vertical" IBE which "involves hypotheses about unobservables" (Psillos 1996, 34). It seems preferable to draw a corresponding distinction instead between two types of *theory* (O and U). Making it a distinction between two types of inference tends to obscure the point that Psillos himself wants to emphasize, namely, that because there is no difference, in terms of the inference criteria employed, between the two sorts of inference, horizontal and vertical, it is illogical for van Fraassen to allow one sort of inference to terminate in affirming the existence of the postulated entities and in the other case, not to do so.

in part at least, to rest. U-theories seemed to have a problem here that the more sedate O-theories bypassed. When skeptical doubts were raised about the credentials of the theoretical entities that by the nineteenth century seemed to be emerging everywhere in the natural sciences, it was nearly always the U-theories that became the target, most especially those in mechanics. Retroduction to the distant universe or to the rapidly populating past did not seem to meet with the same sort of critical suspicion.

4. Two Sorts of Anti-Realism. Although there were other sources, the resurgence of anti-realism in the philosophy of science in the 1960's was in considerable part due to the challenge offered by Kuhn's *The Structure of Scientific Revolutions* (1962). Drawing on examples from the history of science, Kuhn argued for the occurrence of discontinuities ("revolutions") in the history of science such that the theoretical entities of one paradigm would be substantially altered or even eliminated in its replacement. Though he strove to retain a broadened notion of scientific rationality, Kuhn was quite emphatic, in consequence of this sort of discontinuity, in his rejection of the realist import of scientific theory generally.[7] In the Postscript he added to *Structure* in 1971, speaking from the perspectives of both the philosopher and the historian, he decried the "implausibility" of this common, but to his mind, mistaken assumption: "The notion of a match between the ontology of a theory and its 'real' counterpart in nature now seems to me illusive in principle" (Kuhn 1971, 206).

This line of attack was developed further by later writers, notably by Larry Laudan (1984). And it was broadened in ways that Kuhn had not anticipated (and did not much like) by a regular flood of writings exploring social-constructionist, feminist, postmodern, and other allied perspectives. Their emphasis in this context was not so much on the implications of theory-change as on the underdetermination of theory by observational evidence and the consequences for theory-choice of the opening this afforded for values other than the conventional epistemic ones to make a decisive difference. The implications here could, of course, be strongly anti-realist and many writers in these traditions were not slow to emphasize this point.[8]

What is striking about this variety of anti-realism is its global character. It is scientific theory in general whose ontological implications are being questioned. Thus it would apparently call into question widely-shared be-

7. For this contrast between Kuhn's effort to retain a measure of rationality and the "objectivity" it brought with it and his conviction that realism in any shape or form had to be abandoned, see McMullin 1993.

8. Writers as diverse—and as influential—as, for example, David Bloor, Helen Longino, and Richard Rorty.

liefs in the existence, present or past, of such theoretical entities as tectonic plates or dinosaurs. The arguments used by these critics make no exception for O-theories: all scientific theory is, apparently, suspect if interpreted in realistic terms. One cannot but wonder whether these critics find themselves at home in the shrunken world to which their polemic against realism would seem to confine them. Or might they be inveighing, merely, against varieties of realism that no one or almost no one, is actually defending? Or, again, might they be doing no more than criticizing a selection of the arguments customarily brought in favor of realism without themselves embracing an explicitly anti-realist position? Fine, with his capacious "natural ontological attitude" can be interpreted perhaps in this last way; Kuhn and Laudan to my mind cannot. However, that is not my topic here.

What *is* my topic is where van Fraassen stands in this debate. His anti-realism is *not* global; it is not directed against belief in the ontological significance of theoretical entities in the natural sciences generally. For him acceptance of a theory involves, as we have seen, "belief that it is empirically adequate." This goes leagues further than belief that it merely saves the phenomena already at hand. To call an O-theory empirically adequate is to commit oneself, as we have seen, to holding that the theoretical entities postulated by the theory, the dinosaurs and the tectonic plates, actually exist or existed, for it would, after all, be a consequence of such a theory that the entities it postulates could, in principle, be observed by us. And this existence-claim, as we also have seen, is sufficient to qualify this belief as a fully realist one.

Van Fraassen is thus, potentially at least, a realist in regard to O-theory; his realism is, then, of a selective kind. It follows that so is his anti-realism. This partitioning of theoretical entities into two categories, one of which may qualify for realistic import and the other which cannot, is not unique to him. One is reminded immediately, for example, of Ian Hacking who allies himself with those who express global doubts about theory-based ontological claims but is willing to make exception for the class of theoretical entities that lend themselves to manipulation and thus do not have to rely on the explanatory strengths of the relevant theory (Hacking 1983). He is thus a realist in regard to one large class of theoretical entities but he partitions the field along lines that are notably different from those implicit in van Fraassen's presentation. He is, indeed, among van Fraassen's sharpest critics in regard to the latter's reliance on the observability criterion to question the ontological status of the deliverances of the microscope, for instance. On the other hand, Hacking has to turn anti-realist in regard to the unmanipulable entities that populate astrophysics, about which van Fraassen in contrast can be quite sanguine.[9]

9. See, for example, Hacking's argument for, in his words, "anti-realism in astrophysics", in Hacking 1989. He does leave open the possibility that there might be "other

To the extent that he admits the empirical adequacy of a theory, therefore, van Fraassen implicitly embraces scientific realism in its regard. This would presumably apply to a wide swath of the natural sciences, in particular, to the historical natural sciences where the aim of the scientist is assuredly to discover what happened in the past and just when and why it happened. One wonders what the practitioners of such sciences as geology or palaeontology would make of an instrumentalist's claim that their theories are merely devices to save the phenomena, without ontological import. Van Fraassen's guarded admission of these sciences into the realist column is surely prudent, then, even though his reason for doing so might not be widely shared.

However, a further question now suggests itself. How strongly realist can the claim be that van Fraassen makes for theories whose theoretical entities are observable? How is one to know that an O-theory *is,* in fact, empirically adequate? A recent article, with van Fraassen as one of its four co-authors, allows that the definition of CE:

> may seem to suggest that van Fraassen thinks empirical adequacy to be a reachable aim for science. But, of course, that is not implied at all. In fact, he nowhere says that empirical adequacy is within the reach of science—or that it is not. It is simply an issue that van Fraassen does not address and *need not* address in order to make his point against the realist. Perhaps the most unambiguous way to state this point is thus: even if empirical adequacy should be an attainable goal for science, this does not mean that truth is attainable as well. (Ladyman et al. 1997, p. 317, emphasis theirs)

It is true that van Fraassen does not say that empirical adequacy is a reachable aim, any more than the careful scientific realist would say that the explanatory success of a theory warrants the unqualified claim that it is true.[10] In both cases, the important point is the aim. But much of the rhetoric of *The Scientific Image,* the assertion that acceptance of a theory involves belief in its empirical adequacy, for example, surely seems to lean towards reachability, otherwise acceptance (recall that this is a pragmatic notion involving a commitment to employ the theory in question to "confront future phenomena") would also, it seems, be out of reach. In van Fraassen's terminology, it is clearly the case that acceptance of an O-theory implies belief in the existence of the entities the theory introduces to account for the observed phenomena.

grounds for scientific realism in that domain" than the arguments in its favor that are customarily brought forward and that he rejects.

10. The qualification "careful" is important here. The definitions of scientific realism given by its critics all too often imply that a realist is committed to holding that the inference from explanatory success to truth is direct and unqualified. For four needed qualifications, see McMullin 1984a.

How about the conclusion of the passage quoted above? Should proven empirical adequacy commit one to the truth of the theory in question? The authors' negative answer clearly is intended to suggest that proven empirical adequacy is not enough to warrant a realist claim, and hence that CE can be protected from the charge of being soft on realism. The authors' assertion appears to hold for U-theory; think of the Bohr and Bohm interpretations of quantum theory, for example. If one of these were to be empirically adequate, the other would necessarily also be. But it is not possible for both of them to be true together. But the matter is not so simple for O-theory. Is it conceivable that the theoretical entities postulated by such a theory might exist and yet the theory be false?

I am going to lay this question aside since it does not have to be resolved in order to make the point of the overall argument here. Attributing realist import to a theory involves, as we have already seen, a claim about existence, not primarily one about truth. Where the issue of *realism* is what is at stake, the only relevant question is: Can one under certain circumstances claim existence for the entities postulated by a particular theory? The question of the *truth* of the theory does not come up explicitly.

Van Fraassen's understanding of CE commits him to holding that in the case of O-theory (but not U-theory), proven empirical adequacy is sufficient to establish the realist credentials of the theory. That this makes him a realist of some sort in the locality of O-theory seems a fair conclusion. What kind of realist? That will depend on what kind of argument he can put forward for going beyond the safe haven of merely claiming to save the phenomena at hand to make the more hazardous ampliative claim of empirical adequacy. If he is entirely agnostic about the extent in any given case to which the aim can be said to be warranted, then he could perhaps avoid the realist label but his definition of CE would be borderline misleading and CE itself would seem to reduce to a traditional type of empiricism. It is crucial to inquire, therefore, into the sort of warrant van Fraassen may have for transcending, in so unapologetic a way, the aims of his empiricist predecessors. But first it will be instructive to review his grounds for rejecting SR.

5. Retroduction Versus Inference to Best Explanation. Like other critics of realism, van Fraassen takes for granted that the argument for realism depends on what has come to be called "inference to the best explanation". The phrase derives originally from an essay by Gilbert Harman who claims that it "corresponds approximately to what others have called 'abduction,'" but adds that it "avoids most of the misleading suggestions" of this and other alternative terminologies (Harman 1965). I would argue, on the contrary that Harman's choice of terms is more, rather than less, misleading than Peirce's "abduction" or "retroduction".

It suggests first of all that one somehow infers *to* the explanation, making it sound as though the explanation has been arrived at directly by means of some rule of inference. This impression is heightened by other comments Harman makes: "An hypothesis is a potential explanation if it is the sort of thing that can be directly inferred" (Harman 1968, 169). But an hypothesis is not something that can be directly inferred. And one certainly cannot infer *to* the best explanation in scientific contexts. One may be able to infer *that* an hypothesis is the best explanation currently available. But this is a very different way to put the matter. It leaves open the possibility that the hypothesis may have been arrived at in any one of innumerable ways, none of them deductive or by rule, some of them entirely fortuitous. It is the assessment of the hypothesis as an explanation, even more specifically as the best available explanation, that involves inference but inference of a significantly indirect non-rule-governed sort. Harman at one point in the original essay shows himself to be aware of this,[11] but by allowing "inference that" to shade into "inference to", he has allowed a dangerous ambiguity to creep in.

The importance of this for our theme is the emphasis van Fraassen gives to this notion of rule, of the choice of best explanation as rule-governed, in the critique of IBE that constitutes his main criticism of scientific realism. He interprets it as an "epistemic categorical imperative" (van Fraassen 1989, 150), as a form of "rational compulsion", and as such quite rightly rejects it:

> Someone who comes to hold a belief because he found it explanatory is not *thereby* irrational. He becomes irrational, however, if he adopts it as a rule to do so, and even more if he regards us as rationally compelled by it. (van Fraassen 1989, 142)

He is surely right about this. But this is *not* what advocates of scientific realism (not this one at least!) have in mind.

Retroduction has two aspects, as Peirce pointed out. It involves the *invention* of hypotheses, guided by context, background theories, and much else. In this respect, it need not be, and ordinarily is not, a matter of inference. It also involves the assessment of the hypothesis as an "explanation" for a particular phenomenon, a complex matter involving multiple criteria. What is meant by "explanation" in this context is something very broad: it is a matter of satisfying *all* the relevant criteria at once to a

11. "There is, of course, a problem about how one is to judge that one hypothesis is sufficiently better than another hypothesis" (89). He goes on to list such "considerations" as simplicity and greater explanatory power. But without developing these considerations further, he concludes: "I do not wish to deny that there is a problem about explaining the exact nature of such considerations. I will not, however, say anything more about this problem" (89). Surely this is to lay aside the very issue that makes the use of the term "inference to" so problematic in this context?

degree that determines how good the explanation is, how successful the theory is, how secure the claim to have hit on the cause(s) of the phenomenon to be explained.

Theory-assessment may, then, be treated as a complex form of inference, much more complex than (rule-governed) deduction, or (statistically treatable) induction. It is more complex because the criteria (as Kuhn effectively pointed out) function as *values* to be maximized, not as rules (Kuhn 1977, McMullin 1983). Retroduction is not a matter of rule; it is a matter of value-judgment. Assessing the merits of a proposed causal explanation will, of course, rarely be "rationally compelling" but can be rational nonetheless, conveying a greater or lesser degree of likelihood.

Now comes an even more important issue. The case for scientific realism is liable to be sent off in the wrong direction if emphasis is placed upon the search among specified alternatives for the "best explanation". This would make it vulnerable to the altogether reasonable objection put forward by van Fraassen: deciding that E gives the "best explanation" relative to a group of specified competing explanations may mean no more than that E is "the best of a bad lot", worthy of little or no realistic claim (van Fraassen 1989, 143). What matters to the realist case in the first place, on the contrary, are the intrinsic merits of the theory under consideration, quite apart from the altogether contingent availability of alternatives. Does the discovery of an even better explanation make the first one any the less good as an explanation? The answer, of course, is yes and no, revealing a familiar ambiguity in the notion of explanation.

Recall Descartes' famous illustration of the clock, the movement of whose hands we wish to explain without checking the inside of the clock. The mechanism may involve either weights or springs. Either would explain the movements fully. We are then told that in this case, the mechanism relies on a spring. This is then the true (T) explanation. Does this mean that the alternative is no longer explanatory, or even that it is less explanatory? The answer is no, if by the question we mean: would the appeal to weights causally (C) explain the movements in the absence of other information? The power of gravity affords a C-explanation, but (in this case) not a T-explanation. When scientists assess a theory, they have both sorts of explanation in mind. How plausible is it as a C-explanation of the data at hand? How does it fare in competition with its rivals, if any? Though the answer to the second depends on the answer to the first, the answer to the first is not affected by the answer to the second. The point is a simple one, but the resultant ambiguity in the concept of explanation can be troublesome. IBE refers to T-explanation, which includes C-explanation, if you wish, but is not identical with it.

Why should this matter? The arguments in support of a realist-construal of a given theory derive from an analysis of C-explanation, not T-explanation, and from the form of retroduction on which it relies, one that invokes a multiplicity of criteria, as already noted: fit with the data already at hand, logical consistency, coherence with accepted theories, absence of ad hoc features, fertility, unifying power.[12] Among these there are some that carry more realist weight than others, a point that was clearly grasped by Kepler, for example, as long as four centuries ago. He appealed in particular to the property of the true theory to give rise over time to novel predictions that proved to be correct. One might perhaps attribute success in the original saving of the phenomena to the ingenuity of the mathematician. But the fertility of the theory in directing research over time could not be attributed to the same source: only some purchase on real structure could explain it (McMullin 1996b).

The argument is a familiar one in most defenses of SR today. Given the underdetermination of hypothetical causes by the empirical data brought in their support, the opponent of SR can charge that other causal theories might account equally well or better for the empirical data. That the theory at hand does so may be no more than a tribute to the cleverness of the theorist or sheer accident, the critic may urge; the theory may not reflect real structure at all. This charge bears most directly against such virtues as data-fit, consistency, and coherence, that lie within the scope of the original evidence that the theorist had in hand to work with. But satisfying the diachronic virtues, ones that manifest themselves only over time, cannot be explained in this way. As Kepler put it (though he may have been unduly optimistic), the truth will show itself over time. There is much more to be said in this regard but the summary above may suffice for the moment.

The realist appeals in the first place to the virtues of C-explanation and to each of these virtues to a different degree. The assessment of alternative explanations (IBE) is not part of this, not in the first place at least. It is true that if one could show that the proffered explanation is the only possible one, this would directly affect the realist claim that could be made for it. But if it is only a matter of judging that the proffered one is the best of those that happen to be available, this of itself, for the very reasons advanced by van Fraassen, Fine, and other critics of SR, carries much less (some would say no) realist weight. Consistently with this, if an alternative causal explanation comes to be preferred to the one originally under scrutiny, the realist case for this latter based on diachronic performance, if it is impressive, still has to be explained in the light of the preferred alternative. The realist case for phlogiston, for example, was moderately strong in diachronic terms. But it was explained by noting that the causal struc-

12. Simplicity is deliberately omitted from this list for several reasons. It is difficult to define, problematic in application, and lends itself all too easily to dismissal as merely "pragmatic" on the part of critics of SR.

ture on which it depended could be attributed to the presence of oxygen just as easily (and in other respects more successfully) than to the absence of phlogiston. In short, then, IBE's emphasis on comparison of theories and its often vague account of the virtues involved in the notion of explanation employed, offers a road that defenders of SR should not be tempted to try, not least because of the critics of SR who (understandably) line it every inch of the way!

One further ambiguity in the notion of explanation ought to be noted before we leave this topic. When van Fraassen talks about explanation in the context of IBE, he often means explanation in the global sense intended by exponents of IBE, one to which a multiplicity of epistemic factors contribute. In this sense, data-fit would count as an explanatory factor, as would logical consistency: they would contribute to the estimate of a particular theory as furnishing the "best explanation". But at other times, he clearly has a more limited sense in mind: explanatory power is just *one* of the factors that are involved in theory-assessment and, unlike data-fit which is respectably empirical, it is a merely pragmatic consideration. Of explanatory power in this more limited sense, he has this to say:

> These are specifically human concerns, a function of our interests and pleasures, which make some theories more appealing and valuable to us than others. Values of this sort . . . cannot rationally guide our epistemic attitudes and decisions. (van Fraassen 1980, 87)

Van Fraassen's conviction that explanatory power in the narrower sense has no epistemic weight is part of his reason for distrusting IBE and the realism he thinks is based on it, even though this is not the sense of "explanation" on which IBE is actually based. It is not clear just what explanatory power in the narrower sense amounts to. Van Fraassen links it to causal understanding and sees this in turn in pragmatic terms, depending as it does on the prior knowledge and expectations of the person making the evaluation.

One instance of explanatory power that is independent of data-fit can make the point that explanatory power is not necessarily merely pragmatic in nature. When Kepler pointed to various features of the planetary phenomena, for example the fact that the outer planets are brightest and therefore (probably) nearest when "in opposition" (appearing in the opposite side of the sky to the sun), and noted that these fall out "naturally" in his system and are entirely ad hoc in that of Ptolemy, he intended this as a realist argument quite different from an appeal to merely saving the phenomena (McMullin 1993). The claim here is epistemic: one theory explains, the other simply doesn't and thus, by comparison, appears ad hoc. What helped to make these features so persuasive for Copernicus and Kepler was that they were not part of what had prompted the choice of the heliocentric alternative in the first place. They fell out of that choice, as it were, in that way providing realist testimony (recalling that the choice here is between two alternatives only: the explanation is reality-based or it is purely accidental). They did not appeal to, and therefore could not be reduced to, superior data-fit.

Denying any kind of realist weight to such explanation leaves van Fraassen in a particularly delicate situation with reference to the historical practice of science. Someone following CE precepts in the days of Copernicus and Kepler would have had little incentive to accept the novel heliocentric doctrine, which in terms of the criterion of saving the phenomena had relatively little advantage over the Ptolemaic alternative. Van Fraassen appears to acknowledge this: The most that can be said of the Copernican theory, he remarks, is that it saves the phenomena (assuming, he notes, that, in fact, it does) (van Fraassen 1980, 24). That it saves such odd coincidences as the exact yearly period of one of the two circular orbits involved in Ptolemy's epicycle-model for every one of the planets can, then, he implies, be treated as no more than "brute fact". And then a striking claim: "It does not matter to the goodness of the theory nor to our understanding of the world" whether this brute fact can or cannot be explained by some feature that lies "behind the phenomena", such as the reality of the earth's motion. This would seem to reduce to futility the debate that so convulsed astronomy in the seventeenth century. Indeed, it is not at all clear for that matter whether a constructive empiricist could, even today, consistently assert the reality of the earth's motion or even admit that it affects "our understanding of the world"![13]

Linking the realist claim, then, to the merits of "explanation" has in certain limited respects something to be said for it. But the emphasis on "explanation" as the clue to theory-assessment is better avoided.[14] Van Fraassen links it to the claim that "the demand for explanation is supreme" and that, thus, explanatory power (this time, evidently, in the narrower sense) trumps every other evaluative consideration (van Frassen 1980, 23), thus for example validating, implausibly, Bohm's hidden-variable interpretation of quantum mechanics over the orthodox Copenhagen interpretation (94–96). But the realist is in no way tied to such

13. There is one further reason for distinguishing between the broad and the narrow senses of explanation: it helps to neutralize the objection, popular among many critics of realism, including van Fraassen, that the appeal to explanatory success they see as an essential part of the case for realism involves a disabling *petitio principii.*

14. Aristotle's spheres displayed far more strictly *explanatory* plausibility than did the epicycles of Ptolemy and for that reason medieval philosophers, overestimating the realist import of explanatory power, leant to giving the spheres preference from the realist standpoint. As as we know, explanatory persuasiveness in this case proved a far less reliable guide ultimately than did the epicycles.

claims (nor, I suspect, are most defenders of IBE). Nor are realists committed to holding that *all* apparent coincidences in nature can, in principle, be eliminated (25). All they claim is that when they can be eliminated, this offers epistemic advantage. The moral for the realist in all this long tale, then, is once more: stay with retroduction and treat "explanation", when necessary, in an appropriately guarded manner.

6. What van Fraassen's Realism Requires. I want to carry the argument now in a different direction. I have tried to show that van Fraassen's polemic against realism amounts in considerable part to a critique of an admittedly defective way of defending realism. It is time to recall the earlier finding that van Fraassen himself is implicitly committed to a measure of realism in regard to O-theories. The aim of empirical adequacy he postulates for these theories entails for its satisfaction the ability to make existence-claims for a very wide set of theoretical entities. For an empiricist, this is (as we have already emphasized) an extraordinarily ampliative aim which is, perhaps, one reason why he attaches the label "constructive" to his version of empiricism.

But now the question arises: how is this aim to be accomplished? Saving the phenomena, in the sense of fitting the data already to hand, will not suffice. This criterion by itself leaves open all of the problems of underdetermination and adhocness that classical instrumentalism had to face. What is needed is a positive realist argument, reasons, that is, to suppose that a given O-theory is, in fact, empirically adequate. What would lead one to be reasonably confident about this? It is not a matter of determining once for all that a given O-theory is empirically adequate. Van Fraassen, as already noted, is not committed to holding that this is in practice achievable, any more than the more conventional realist is committed to holding that the realist case for a given theory can be secured beyond any question. What van Fraassen needs is something less than this but considerably more than merely a saving of the phenomena already in hand.

What I am getting to, then, is that he needs realist considerations in support of (the aim of) empirical adequacy just as much as do realists less constrained by restriction to the observable. In *The Scientific Image,* he allows that there are supplementary theory-virtues, other than data-fit,[15] that, in practice, play a part in theory-choice. But he insists that these virtues must be regarded as merely pragmatic ("person- and context-related") in character; they do not bear on the truth of theory. Yet he then

15. He says other than "empirical adequacy" (van Fraassen 1980, 88) but, as we have seen, empirical adequacy is *not* a virtue in the sense of a criterion practically applicable to theory-choice as the other virtues he lists are.

has to explain why in fact scientists appear to set such epistemic store on such virtues in evaluating the merits of particular theories. His answer is that pursuing "explanation" (broad sense) necessarily includes empirical adequacy and thus it is rational for scientists to pursue it, even though explanation also includes the non-empirical, more specifically "explanatory", (narrow sense) virtues also. One might even allow that "the pursuit of explanatory power is the best means to serve the central aims of science", he concludes, provided it be remembered that the features other than empirical adequacy and consistency that contribute to explanatory power are not to be supposed to make empirical adequacy itself more likely (van Fraassen 1980; 88–89, 93–94).[16] But this still leaves the importance attributed to these virtues in apparently epistemic contexts unaccounted for. And more to the point here, it leaves the realistic component of empirical adequacy unsupported except by current data-fit, a weak reed surely in the light especially of van Fraassen's own objections to realism elsewhere.

In his later "Empicism in the philosophy of science", he opens up an alternative line of argument when discussing the historical contributions of Herschel and Whewell to the philosophy of science. This is the notion that evidential support for a theory must be independent support (van Fraassen 1985, 266). Thus when a theory predicts a novel phenomenon, one that was not part of what the theory was originally put forward to explain, this constitutes independent support for the theory. The same can be said for bringing together two hitherto disparate classes of phenomena in unexpected "consilience", Whewell's by now familiar term. Linking this to some special sort of explanatory power is wrong, van Fraassen adds. What is crucial here is the independence of the support this offers to the original theory. As far as I can see, he is here alluding to an epistemic, and not merely a pragmatic, claim.

If "explanation" be taken in the narrow sense, this line of argument seems right. Van Fraassen evidently feels that this enlargement of horizon beyond the sole factor of empirical adequacy is justifiable, even for the empiricist; after all, the verified novel predictions and the unification of theories involve matters of fact and do not depend for their force on a claim to provide a new level of understanding, the sort of claim whose epistemic credentials he above all distrusts. He goes on to note the difficulties in attaining greater precision in defining what constitutes a fact as "novel" or just what sort of claim on truth it warrants one in making.[17]

16. This argument brings out in a particularly effective way how unfortunate it is that "explanation" has, as already noted, two significantly different meanings.

17. These difficulties have been exhaustively discussed in several recent writings, notably in Leplin 1997 and Psillos 1999.

And in the end, it is not clear just what importance he is allowing to this line of argument as part of his own account of theory-assessment.

But here I would like to return to my earlier discussion of retroduction. Among the virtues guiding it, the most important for making a realist case for the theory involved were, we saw, the diachronic virtues, those that show themselves over the course of time. They are, so far as I can see, just the virtues to which van Fraassen here attaches the note of independent support. Their importance from the realist standpoint is that they come down on the realist side in the decision between the two alternatives: that the original theory owes its success mainly to the ingenuity of the theorist in fitting the data at hand or that it can be attributed to a match of sorts between the entities it posits and real underlying structure. The notion of independent support is relevant here to the realist case: the support is for the theory as a whole and not just for some sort of detached version of the theory as a formalism without an ontology.

It is relevant to van Fraassen precisely because of his version of empiricism, precisely because CE goes beyond mere data-fit to make a claim on all relevant phenomena whether past, present, or future, whether observed or not. If CE's aim of empirical adequacy were not ampliative in this way, the notion of independent support would make no sense here. Independent support for what? For the theory in its ampliative aspect precisely. So this is where van Fraassen's realism of O-theory can find a respectable measure of justification. To the extent that he can commit himself to asserting that a particular O-theory is empirically adequate and that thus its theoretical entities are real, it is to the diachronic virtues, in his version to those virtues that afford independent support, that he would need to turn. (It should be remembered, however, that satisfying the remaining virtues, including data-fit, may carry with them some measure, at least, of realist weight.)

But if this be so, why should not this form of argument be open in the case of U-theory also? It is his refusal to allow even the smallest degree of realist implication in this case that sets him off from those with whom he shares in one way or another a realism of O-theory. I want to present in closing a plausible motive for his hesitation, though not for his outright refusal, when it comes to assigning realist weight to U-theories. As will be immediately evident, it is not van Fraassen's primary motive for this hesitation, but it might just possibly help to explain the vigor with which he defends the partitioning of O- from U-theory on which his epistemology so centrally depends.

7. A Happy Ending? The feature of CE that drew most criticism, in the first years after the publication of *The Scientific Image* especially, was the chasm it set, epistemologically speaking, between what could be observed by the human senses and what could not be. This seemed, even to many who were otherwise sympathetic to the anti-realist bent of the book, to be open to serious question. In the laboratories of the contemporary natural sciences, very few of the properties that are being "observed" would qualify as observed under the rules of CE. To allow that one can "observe" through a telescope but not a microscope brought Ian Hacking's notable critical faculties to energetic life (Hacking 1985). It is one thing to stress the epistemic primacy of sense-testimony in time-honored empiricist manner. But it is something else entirely to allow, for example, that creatures of the distant past can count as "observable" even though they can never, in fact, be observed by humans.

My suggestion is this. What may lie, in part at least, behind this skittishness about entities that lie in principle beyond the range of the human senses is that by that very fact they might be regarded as ontologically problematic from our perspective. It is not accidental, I somewhat hesitantly propose, that van Fraassen's early concern in the philosophy of science lay in significant part with quantum mechanics. And as everyone knows, the ontological status of the quantum world is indeed problematic. Things that we can see and touch belong to intuitively comfortable categories, like "substance", "individual", "corpuscle", in earlier ontologies. Quarks, photons, electrons, even atoms, clearly do not. When Galileo recognized the lunar shadows as the shadows of mountains, he was appealing to a familiar ontological category. When Bohr proposed that hydrogen consisted of atoms containing a single electron orbiting a single proton, his model, despite its clear analogy with our solar system, made use of entities we cannot even imagine, in the usual sense of "imagine".

Faced with this, even the staunchest realist has to hesitate. It is not that the realist argument entirely fails here, as van Fraassen would, effectively, maintain. The long-term success of a theory in this domain betokens some form of ontological support here as elsewhere. What marks a theory off in this context is that one is not quite sure what a realist ontological claim amounts to, an uncertainty that can be traced all the way back to the challenge to their credentials as explanation of the notions of force and attraction that were the backbone of Newton's mechanics, and yet that in retroductive terms were so enormously successful.[18] When Newton insisted on the reality of force or Faraday on that of field, the problem was to create, and then make an intelligible home for, a new ontological category

18. This familiar episode illustrates, as well as any other, why the exclusive emphasis on explanatory success of the IBE label can be so misleading. Newtonian mechanics was *not* regarded as a good explanation, in the sense of providing a causal explanation of why gravitational motion occurs. Yet it was rapidly and almost universally accepted as the best available theory.

(McMullin, 2002). What is it, even the realist is forced to ask, for a quark to "exist", no matter how impressive its realist credentials?

Realists therefore have difficulty with just the same sort of theoretical entities as do constructive empiricists. They are both wary of the products of U-theory. Not for the same reason nor to the same extent, of course. But here comes a friendly suggestion from the realist to the constructive empiricist. The reason why the realist is willing to mark off the U-category as ontologically problematic is, as it stands, a more plausible reason than that advanced by the constructive empiricist who places the emphasis on the wrong factor. Even van Fraassen himself seems to lean occasionally in the direction I am suggesting, when, for example, he contrasts the "different appreciation" realists and empiricists have:

> of just how unimaginably different is the world we may faintly discern in the models science gives us from the world that we experientially live in (the scientific image from the manifest image, the intentional correlate of the scientific orientation from the phenomenological life-world). (van Fraassen 1985, 258)

The third Rule of Reasoning that Newton saw as basic to his reliance on induction postulated that "the qualities we have found to belong to all bodies within the reach of our experiments are to be esteemed the universal qualities of all bodies whatsoever."[19] There could, in principle, be a world where this would hold good. But we now know (and it is to this realist appreciation that van Fraassen is forced to resort in the passage cited above . . . note his phrase "faintly discern") that in our world the third Rule. It is because we now know this, and not primarily because of the empiricist's dichotomy between the observable and the unobservable, that the theoretical entities of U-theory are regarded with epistemologically-inspired caution. Might not constructive empiricists be persuaded, then, to set aside the troublesome criterion of what should count as "observable" in order to make their case for wariness in regard to the U-category? The case for wariness can be made much more persuasively by referring to what scientists have already discovered about the nature of the U-world.

Taking this tack, of course, demands at least a whiff of realism, to recall a metaphor made famous by Imre Lakatos in another context. And it does not support the complete cut-off of epistemic contact with the U-world favored by CE in its original guise. Would this entail a further loss of empiricist credentials? Perhaps so, but they are already strained in consequence of CE's incorporation of the optimistic aim of empirical adequacy and further strained again, as we have seen, by the necessity of

19. *Principia,* introduction to Book III.

allowing epistemic merit to virtues other than data-fit in order to defend that aim as a plausible one.

Might this whiff of realism prove fatal to the cause of empiricism, to which van Fraassen is so devoted? Or might it not lead to a larger and more defensible form of empiricism? After all, CE already authorizes a departure from strict empiricism. Why not take the chosen label "constructive" more seriously, retain empirical adequacy as aim, provide an argument in support of its viability as aim, and adopt the more plausible way of separating U-theory from O-theory? Wouldn't that be constructive?

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