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SCIENTIFIC REALISM: OLD AND NEW PROBLEMS

ABSTRACT. Scientific realism is a doctrine that was both in and out of fashion several times during the twentieth century. I begin by noting three presuppositions of a succinct characterization of scientific realism offered initially by the foremost critic in the latter part of the century, Bas van Fraassen. The first presupposition is that there is a fundamental distinction to be made between what is "empirical" and what is "theoretical". The second presupposition is that a genuine scientific realism is committed to their being "a literally true story of what the world is like". The third presupposition is that there are methods for justifying a belief in the empirical adequacy of a theory which do not also suffice to justify beliefs in its literal truth. Each of these presuppositions raises a number of problems, some of which are quite old and others rather newer. In each case, I briefly review some of the old problems and then elaborate the newer problems.

1. SCIENTIFIC REALISM: WHAT IS IT?

Scientific realism is a doctrine that was both in and out of fashion several times during the twentieth century. At the end of the century it seemed once more definitely back in fashion with the publication of several book length defenses of some form of scientific realism (Leplin 1997; Niiniluoto 1999; Psillos 1999). I will not attempt to review these latest offerings here. Rather, I will begin with a pair of characterizations offered initially by the foremost critic of scientific realism in the latter part of the century, Bas van Fraassen. "The correct statement of scientific realism," he writes, is:

> 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. (1980, 8)

His own alternative to scientific realism, constructive empiricism, is similarly characterized as follows:

> Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate. (1980, 12)

These beautifully brief statements contain a number of presuppositions, three of which I will examine in this essay.

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The first presupposition I will consider is that there is a fundamental distinction to be made between what is "empirical" and what is "theoretical." The second presupposition is that a genuine scientific realism is committed to their being "a literally true story of what the world is like," rather than, say, only idealizations and approximations. Finally, the third presupposition is that there are methods for justifying a belief in the empirical adequacy of a theory which do not also suffice to justify beliefs in its truth (or maybe just approximate truth). Each of these presuppositions raises a number of problems, some of which are quite old and others rather newer. In each case I will briefly review the old problems and then spend rather more time elaborating the new problems.

2. THE EMPIRICAL AND THE THEORETICAL

A prototype for debates about scientific realism in the twentieth century occurred at the end of the nineteenth century with questions about the reality of atoms and molecules (Nye 1972). Imagine a glass tube open at both ends with one end stuck down into a small beaker containing mercury. Neglecting minor surface effects, the level of mercury in the tube will be observed to be about the same as that in the beaker. That is a bit of *empirical* data. The disputed *theoretical* explanation is that the density of air molecules is the same inside and outside the tube, so the pressure on the surface of the mercury due to impacts from air molecules is the same inside and out. There is, therefore, no force difference to make the mercury rise or fall in the glass tube. Now suppose a vacuum pump is hooked up to the open end of the glass tube. As the pump works, the mercury is observed to rise in the glass tube. Again empirical data. The theoretical explanation is that removing molecules of air from inside the tube leaves fewer molecules to impact the surface of the mercury inside the tube, so the greater density of air molecules hitting the surface outside the tube forces mercury up the tube. Of course, this experiment is hardly decisively in favor of an atomic theory of gasses. The same observations would be expected if air were a continuous fluid.

Part of the background to nineteenth century debates about the reality of atoms was the classical empiricism of Locke, Berkeley and Hume, who assumed that experience of one's own sensations is the foundation of all empirical knowledge. The most famous of the skeptics about atoms, Ernest Mach, held this phenomenalistic form of empiricism. From this perspective, it is not quite correct to say that the mercury rising in the glass tub is empirical data. Rather, it is the *experience* of seeing what one takes to be mercury rising in the tube that is the real empirical data. Concluding that the mercury is rising is as much of an inference as inferring the motions of molecules, though maybe more reliable.

In retrospect, positing a realm of sensations, later “sense data,” provided an unambiguous distinction between what is “observed” and everything else. There is a clear qualitative difference between sensations and things out in the world. The existence and quality of sense data were taken to be of a uniform and indubitable nature. For sense data it was indeed true that to be is to be perceived. Not so for atoms, or even for glass tubes. The existence of sense data was one of the (very) old problems connected with debates over scientific realism.

When, in the 1930s, the European scientific philosophers in Vienna and Berlin gave up phenomenalism in favor of physicalism, they attempted to retain the uniformity in the observational base of science that phenomenalism had provided. This attempt was eventually codified by Carnap (1956) in linguistic terms as a distinction between “observation terms” and “Theoretical terms.” But this distinction proved difficult to maintain except in the form of conventional stipulations of what gets put onto a list of “observation terms.” In practice, as Grover Maxwell (1962), among many others, pointed out in the 1960s, the desired uniformity simply does not exist. The predicate “is silver” is observational if any term is, but yet there are, for example, drops of mercury so small as to be invisible to the human eye, but which obviously look silver when viewed under moderate optical magnification. Again, one can weigh a beaker full of mercury, so here “weight” is an observational term. But molecules also have weight, which is why, on the atomic theory, the pressure of air is greater at sea level than in the mountains. Here, then, is another (more recent) old problem of scientific realism.

Van Fraassen takes a different approach for distinguishing the empirical from the theoretical. For him, the empirical is whatever can be reliably distinguished by an unaided human observer. Thus, determining what is empirical is itself an empirical matter. And this criterion for what is observable (thus, empirical) is, like the phenomenalistic and linguistic approaches, is intended to be uniform across time and scientific fields.

This approach, however, raises a number of new problems. One set of problems arises because humans are quite variable in their ability to observe the world around them. Experienced experts, such as sports umpires and judges of show dogs, can reliably spot things that pass by an untutored observer. Worse yet, a person's beliefs, including theoretical beliefs, have been shown to influence what people report to have observed. To some extent, as often been demonstrated in controlled experiments, humans report observing what they had expected to observe even when the reported object or situation simply did not exist (Bruner et. al. 1956).

A more serious problem, I think, is that this radically anthropomorphic approach does not accord well with widespread scientific practice. In our simple pneumatic experiment, the scientific report of what was observed would typically be that the air pressure in the tube above the mercury was decreased by pumping out air. As noted above, this report is neutral between air being corpuscular and its being a continuous fluid. But nobody can literally observe the air pressure in the tube. All that anybody can see is that the mercury rises when the pump is turned on. That this is due to decreased air pressure is already a theoretical inference. In this relatively unproblematic sense, most scientific observation is theory laden.

Van Fraassen insists that, since science is a human enterprise, a good empiricist, who thinks empirical adequacy is the only criterion for theoretical belief, must insist that that the empirical consists of human experience and only human experience.[1] The purpose of instruments is, therefore, merely to produce results that humans can observe without instruments. So empirical adequacy means adequacy to unaided human experience. Here we have a new problem for realism. How can one reconcile the obvious fact that doing science is a human enterprise with the widespread scientific practice of taking as evidence results that go far beyond the observational capacities of unaided human observers? Van Fraassen has an empiricist answer to this question based on a strong distinction between "acceptance" and "belief." The *acceptance* of theory laden observation reports, and, indeed, of broader theories, he claims, may be justified by many pragmatic considerations, among which he includes virtues such as simplicity and explanatory power. But *belief* in the truth of a theory could only be based on empirical adequacy narrowly conceived in terms of unaided human experience. In fact, as noted above, for van Fraassen, legitimate belief only extends to overall empirical adequacy.

I will return the distinction between belief and acceptance when considering the third of the three presuppositions noted at the beginning of this paper. Here I want to suggest a more realistically inspired solution to the problem at hand based on the notion of *distributed cognition* being developed within the cognitive sciences. As I wish to understand it, the basic notion is that of a cognitive system. A human is a cognitive system, but so is a dog. But, more importantly, a human together with an experimental setup, such as the mercury and air-pump arrangement, is also a cognitive system. The cognition is distributed throughout the whole system. It is the system, not just the human, that produces the cognitive output. Thus, we do not have to think that all the cognition is going on in the person and that the instruments are producing only input to the human system. Maybe a person alone cannot *observe* air pressure, but a cognitive system consisting of a person and the relevant equipment can *detect a* decrease in air pressure.

The distinction between "observe" and "detect" is fundamental. In scientific practice, it is what can be *detected* that counts as the empirical evidence for theoretical claims. What humans by themselves can *observe* is not fundamental. But the humans are nevertheless fundamental. By itself the mercury, glass tube, and pump set-up cannot be said to *detect* anything. Even less does it *know* anything. It just does what it does. It is the human component of the cognitive system that provides content to the interpretation that a decrease in air pressure in the glass tube has been detected.

An immediate implication of the move from what unaided humans can observe to what cognitive systems incorporating humans can detect is that the uniformity of the empirical basis of science disappears. Different experimental cognitive systems detect different sorts of things. Which cognitive systems are invoked depends on what one wants to detect, which in turn may depend on what theories one is trying empirically to test. This does not erase all distinctions between evidence and theory. It only eliminates a uniform distinction applying across different times and sciences. In any specific case in which a theory is being tested empirically, there may be a clear distinction between what is being detected (the evidence) and the theoretical claims being tested. But the distinction is always local. And, indeed, the presence of entities once postulated by disputed theories may later be taken as evidence for other theories, as the presence or absence of various gas molecules in the air now serves as evidence for or against various theories of global warming.

To conclude this section, I am not maintaining that introducing notions of distributed cognition is the only way to raise new questions about the distinction between observation and theory, only that it is one way of introducing new problems into old debates over scientific realism.[2]

3. LITERAL TRUTH

For van Fraassen's scientific realist, the aim of science is to construct theories that provide "a literally true story of what the world is like." He is surely justified in characterizing scientific realism it terms of truth. Virtually every characterization of scientific realism I have ever seen has been framed in terms of truth. The old problems of scientific realism typically arose from trying to show why it is justifiable (or not) to take the aim of science to produce theoretical claims that are true. In talking about "literal truth," van Fraassen adds a new twist, but it is not at all clear what "literal truth" encompasses. Does it mean that theoretical claims are exactly true, leaving no room for error? Does it mean that theoretical claims are *complete*, that they encompass the "whole truth and nothing but the truth"? Here we have some newer problems of scientific realism.

Hilary Putnam combined these two ideas in his characterization of "metaphysical realism" as the view that there is one exact and complete true theory of the world. He argued that it is logically impossible that there should be such a theory, so that metaphysical realism is necessarily false. I will not attempt to summarize the ensuing literature on what has come to be known as "Putnam's set-theoretical argument".[3] Here I want to present an argument that may be new to the literature on scientific realism (though, given its obvious Hegelian overtones, it cannot be new in general).

The argument is that the question of whether any theoretical claim could be exactly true is connected to the question whether any theory could be complete in the sense that it encompasses the whole truth about everything. My conclusion is that the only way any particular claim could be exactly true is if it part of a complete theory that is exactly true in every respect. The assumption that connects exactness with completeness is that everything is causally connected with everything else by some more or less remote chain of causation. In more scientific terms, this is the assumption that there are no totally closed systems. So, suppose we have a theory that is not complete. Whatever the subject matter of this theory, there will be some (maybe remote) connections between this subject matter and other things in the universe not part of the subject matter of this incomplete theory. So, since there will be some influences on the subject matter not accounted for by our incomplete theory, it cannot be exactly correct. Thus, only a complete theory could generate claims that are exactly true.

This simple statement of the argument invokes what seems like a metaphysical assumption of connectedness in the universe. The argument can be made less metaphysical by assuming only that we do not *know* the extent of connectedness in the universe. Thus we do not know whether or not any of our theoretical claims are exactly true. I am happy with this more modest conclusion.

Turning to inexactness itself, a number of philosophers of science have for several decades been arguing that theoretical claims are typically *inexact* (Scriven 1961; Cartwright 1983, 1989, 1999; Giere 1988, 1999a). One way of expressing this idea is to say that theoretical claims are not strictly true but only *approximately true*. This stance raises the problem of saying just what it means for a claim to be approximately true.[4]

There have been several valiant efforts to solve this newer problem of scientific realism within a framework of standard logic, set theory, or model theory.[5] I won't attempt to go into details here because I think there are several general reasons for thinking that no such general attempt can really solve the problem of accounting for theoretical claims that are both realistic and inexact.

One general reason is that a solution at the level of formal semantics, of whatever variety, is just too abstract to account for the varieties of inexactness found in the sciences. Probably the easiest case of inexactness is that concerning the value of a so-called physical constant, such as the velocity of light or Plank's constant. Yet it is a long way from formal semantics to a quantitative theory utilizing real numbered variables. Moreover, since the "true" value of these constants is unknown, it is impossible to measure the "distance" from the theoretical value, or any measured value, to the true value. Worse yet, there is no uniquely correct measure of distance. Linear difference seems the most natural measure, but it is difficult to justify over, say, a logarithmic measure. Things are even more difficult if we move to sciences where differences are qualitative or structural. What sort of general measure could one use to determine even the difference between two rival proposed structures of a biologically active molecule, let alone the difference between a proposed structure and the unknown true structure?

My own solution to this newer problem of scientific realism involves a view which I share with Nancy Cartwright (1999) and Paul Teller (2001) as well as van Fraassen. This is that the primary representational media for theoretical claims are models. Models range from actual physical objects, such as Watson and Crick's original metal and cardboard models of DNA, through diagrams, such as Feynman Diagrams, to abstract entities, such as an ideal gas.[6] Returning to our earlier example, I suppose that any scientists performing such an experiment would have constructed for themselves an abstract version of the experiment. The abstract version, for example, might not specify the length of the tube, the amount of mercury in the beaker, or the temperature of the whole apparatus, except that it be roughly the same in all parts.

I continue to think that the most one can say it general about the representational relationship is that we desire that the model be similar to the real world situation in those respects corresponding to specified features of the model and, for each respect, to a degree desired by the relevant scientists given their scientific/historical position relative to the subject matter. Commentators have criticized this characterization as "hopelessly vague" and some have insisted that that without an "objective" measure of similarity, the view is excessively relativistic (Chakravartty 2001). My view is that there is no such thing as an objective measure of similarity that is completely general, but this does not matter because no such measure is needed. Once the experimental set-up and the model have been specified, the context provides whatever measures of similarity are required. In our simple example, the experiment has a positive result so long as the mercury rises in the tube by an amount clearly distinguishable from surface effects.

Here simple human perceptual abilities are all that are required to judge the required similarity. How much this result supports claims about theoretical similarities, such as there being invisible particles of air bouncing around inside the tube, is another question.

Van Fraassen invokes the notion of *isomorphism* when characterizing empirical adequacy for a theory understood as a set of models. The theory is empirically adequate just in case the empirical substructure of one of its models is isomorphic with all actual observations (past and future). Isomorphism is a notion that has its home in the realm of abstractions; sets, numbers, and the like. Just what it means outside this realm is not at all clear. Are the four legs of my chair isomorphic with the four legs of my dog? One can easily establish a one to one correspondence. But then there are insignificant isomorphisms all over the place. And what about qualitative results such as the rising of the mercury in the partially evacuated tube? What could it mean to say that the rising of the mercury in the actual tube is isomorphic with the rising of the corresponding element in the abstract model of the experiment? This is not clear even if we imagine the comparison not to be between a model and the world itself, but between a theoretical model and a model of the experiment that corresponds to part of the empirical substructure of the theoretical model. In fact, neither model specifies just how high the mercury rises in the tube, so a numerical comparison is not even possible. In sum, invoking the notion of isomorphism when specifying relationships between models and the world seems to be a case of misplaced precision. The notion of similarity has just the flexibility required.

The really important feature of similarity, however, is that it provides an alternative statement of the aim of scientific theorizing for a scientific realist. The aim of science is not "a literally true story of what the world is like," but merely the production of models similar to limited aspects of the world in ways determined by the scientific context. Some of these aspects may be of the type traditionally designated as "theoretical," such as the atomic structure of the air. But there is no special problem of inexactness in science. Inexactness is built into the enterprise from the start in the nature of its representational practices.

4. CAUSES AND CAPACITIES

In addition to an array of models, some sciences can boast of general theoretical principles. These have historically been most prominent in physics: Newton's Principles of Mechanics (three laws of motion plus the gravitational law), Maxwell's Principles of Electrodynamics, Einstein's Principle of Relativity, the Principles of Quantum Mechanics. But biology also has its Principle of Natural Selection, and Economics has various equilibrium principles. A potentially new problem for scientific realism is whether, or in what way, such principles can be understood realistically.

This problem has been brought to the fore primarily by the writings of Nancy Cartwright. Cartwright first argues that these principles cannot be understood as straightforward universal generalizations over existing entities. If they are understood as generalizations over existing entities, she argues, they cannot be both universal and true. Instead, she concludes, these principles describe the real *capacities* of things. Thus, for example, Newton's Principle of Gravitation is to be understood as stating that any body has the capacity to attract another body with a force proportional to the inverse square of the distance between them. Whether this capacity is realized in any particular instance depends on the specific circumstances of the two bodies in question. If both bodies are positively charged, for example, the net result may be that they repel rather than attract one another. Cartwright argues that positing the existence of such capacities is the best way to understand the actual practice of classical physics (and science in general). A new problem of scientific realism is whether this so.

I agree with Cartwright that Principles do not directly state empirical claims. For empirical claims one needs appropriate models. But I do not think it is necessary to introduce capacities as real powers operating in nature. It is enough to posit a robust sense of singular causation, which she also does. One can then understand Principles as defining very general abstract models of aspects of the causal structure of the world. By themselves they do not specify the behavior of anything in particular. They cannot be used to make predictions about anything that might be detected. Only when, as Cartwright says, they are fitted out to apply to specific kinds of situations can they be used to make empirical claims. My new problem for realism, then, is whether positing singular causal agency is enough.

The idea that the world itself has a definite causal structure that might be partially captured by physical principles may still strike many as excessively metaphysical. In fact, so strong a metaphysical claim is not necessary for scientific practice. All that is needed is a *methodological* commitment to proceed as if this were so. This commitment is amply justified by the continued success of science in devising principles that yield models that are judged through empirical tests to fit very closely the actual behavior of real systems. Science, unlike, for example, religion, can eschew metaphysics and get by quite nicely with sound methodology and empirically justified theory.[7]

5. ACCEPTING THEORETICAL MODELS

The above discussion of the first two of van Fraassen's presuppositions has transformed the third. The transformed question might be stated as whether it can be justifiable to believe or to accept similarities between a model and the world that involve features of the model that go beyond those connected more or less directly to the output of our measuring instruments. Before discussing this question, however, I would like to introduce a new example, one from contemporary astrophysics, an undeniable case of successful scientific practice.

During the 1990s, the astrophysical community received much new data from the second (after the Hubble Telescope) of NASA's major orbital laboratories, the Compton Gamma Ray Observatory (CGRO). Among four different instruments aboard, CGRO was the imaging Compton Telescope (COMPTEL) which was efficient in detecting gamma-rays in the 1 to 30 MeV range.[8] In one series of observations over a period of 5 years, data from COMPTEL indicated a high flux of gamma-rays measured at 1.8 MeV (±5–10%) coming from the center of the Milky Way Galaxy where it is assumed new stars are being formed. This data was taken as confirming long-held theories of star formation. In standard models of star formation, heavy isotopes like Aluminum 26 are produced in the cores of very massive stars such as those existing in the center of galaxies. Al 26 is known to decay into Magnesium 26 producing gamma-rays with the energy of 1.8 MeV.

Perhaps the major old problem of scientific realism has been how to *justify* inferences like those from the output of COMPTEL to conclusions such as their being sources of 1.8 MeV gamma-rays at the center of the Milky Way and, further, that these gamma-rays are due to the decay of Al 26, which, moreover, is being produced in a process of star formation. However, the idea that such conclusions are in need of *philosophical* justification seems to me mistaken. There is no genuine need of a philosophical justification. These conclusions are prime examples of justified scientific claims. The new problem for scientific realists is simply to understand how these conclusions have come to be justified. This is less *a normative* problem than a *descriptive* problem.

Within the philosophy of science, a major research program has been to understand such inferences in terms of a probabilistic logic of belief.[9] This program, however, creates problems for anyone wanting to understand how scientists could come to believe that current models of star formation are even approximately correct.

We can suppose that there is a quasi-deductive path from models of star formation, through models of radioactive decay, through models of the COMPTEL telescope, and, finally, to a model of the output produced by COMPTEL. I say "quasi-deductive" because these calculations involve many instances of idealization and approximation. Still, this is good enough to be able to say, roughly, that the models of the output of the COMPTEL system correspond to van Fraassen's empirical substructures of the whole hierarchy of models including the overarching model of star formation. So there is a quasi inclusion relationship from models of the COMPTEL output to models of star formation. Now it is part of the probabilistic logic of belief that probability decreases with increasing content. So, given the observed output, that output has probability one. But given the observed output, that there are sources of 1.8 MeV gamma-rays at the center of the Milky Way has probability less than one, and still less is the probability that these gamma-rays are being produced by decaying Al 26, which, however, is greater than the probability that the Al 26 is being produced in star formation. Thus, assuming that the more probable belief is the more justifiable, the fully realistic belief in the correctness of models of star formation will be the least justifiable belief in the hierarchy of beliefs one might have in this context.[10]

In terms of van Fraassen's distinction between belief and acceptance, I would suggest that the search for a logic of scientific belief is mistaken. What is needed is an account of acceptance. One of many reasons for making this switch is that the concept of belief is strongly connected to individual psychology. And, psychologically, beliefs often seem not under voluntary control. One just acquires them. Of course, individual scientists have beliefs like everyone else, but scientific claims are supposed to be publicly warranted. For this, acceptability seems a more appropriate notion. Moreover, if there is anything like a logic of acceptance, it may well look more like decision theory than like probability theory itself. In the framework of this paper, then, accepting a scientific claim would be deciding to regard a model as being similar to a given real system in various respects and degrees. The decision could be made by an individual, but it makes equally good sense to regard it as the collective action of a scientific community.

For a schematic example of such a scientific decision, imagine an experiment designed to test which of two different models better fits the given experimental situation. Suppose the outcome of the experiment is a reading along a linear scale. What is needed is a clear difference in the predicted outcome depending on which of the two models best fits the situation. More formally, we want there to be two regions of the outcome space, $\mathbf{R_1}$ and $\mathbf{R_2}$, such that $\mathbf{P(R_1/M_1) = High}$ while $\mathbf{P(R_1/M_2) = Low}$ and, conversely, that $\mathbf{P(R_2/M_1) = Low}$ while $\mathbf{P(R_2/M_2) = High}$, Here $\mathbf{R}$ means that the outcome is in region $\mathbf{R}$ of the outcome space, and $\mathbf{M}$ means that model $\mathbf{M}$ provides a good fit to the real situation. The obvious decision rule is to accept $\mathbf{M_1}$ as being the better fitting model if the outcome of the experiment is in the region $\mathbf{R_1}$ and accept $\mathbf{M_2}$ as the better fitting model if the outcome is in the region $\mathbf{R_2}$. Assuming this rule, then, whichever model is in fact the better fitting, there is a high probably of accepting that model as indeed better fitting and a low probability of accepting the less well fitting model as being better fitting. In standard epistemological terms, this is a description of a reliable procedure.

Note, first, that this account of acceptance does not presume that scientists assign probabilities to claims about how well models fit the world. All the probabilities involved are *physical* probabilities of the kind scientists are used to employing. They are the probabilities of specified physical results assuming physical characteristics of the subject matter, the experimental set-up, and the instrumentation. Of course scientists can be more or less confident in their judgments as to which model to accept as better fitting. On this account, that confidence is not measured as a probability. It would no doubt depend in some way on how high the probability of conforming outcomes and how low the probability of misleading outcomes. It might also depend partly on a second order judgment as to how reliable are the calculations of the probabilities of various outcomes, and presumably on many other things as well.

Note, second, that the acceptance of a model as similar to the real system is acceptance of the whole model. No distinction is made between observable and non-observable aspects of any models. So, on this account, acceptance can be fully realistic as it typically seems to be in actual scientific practice. This full acceptance, however, raises a further problem for scientific realism which, while not altogether new, has not been nearly as much discussed as other problems. The problem is that often not all aspects of models are regarded as even candidates for representing aspects of the real world. A classical example occurs in electrodynamics, where the equations defining the models yield solutions in which a wave comes in from infinity rather than radiating out from a local source toward infinity. Nobody thinks these waves coming in from infinity are real. They are regarded as an artifact of the mathematics. One way of stating the problem is this: Is there a principled way of distinguishing those aspects of models that are potentially representational from those that are not?

My solution to this problem takes the same form as van Fraassen's empiricism, but is much more liberal, pushing the realm of what is acceptable in a model far beyond what van Fraassen would deem believable. My criterion is this: For an aspect of a model to be regarded as potentially representational, it must be possible empirically to detect *differences* in that aspect. Here detection can be by means of a quite remote causal chain involving intricate instrumentation, just so long as there is some reliable causal connection. Though liberal, this criterion is not vacuous.

To take a very contemporary example, some cosmologists now argue that our universe is in fact a ‘‘multiverse.’’ That is, ours is but one of perhaps infinitely many whole universes.[11] Some see this conclusion as a direct implication of current cosmological models while others argue only that it is compatible with, or ‘‘suggested by’’ current models. Either way, if there is even just one more universe, it is, according to these same models, currently outside our light cone, so inaccessible by any causal interaction. There is no way to detect the existence of such an additional universe. By my criterion, there can be no empirical basis for accepting any claims about the actual existence of such logically possible universes. So the problem for scientific realism is whether the acceptance of such models can be legitimated by appeal to what are sometimes called ‘‘superempirical virtues’’ such as simplicity or explanatory coherence (Churchland 1985). My personal inclination is to reject such appeals as scientifically illegitimate, but this raises my final newer problem for scientific realism.

This last problem is a meta-problem, or perhaps a ‘‘meta-methodological’’ problem. On what grounds could one insist on anything like my criterion of empirical testability and thus question the acceptability of claims like those about multiverses made by respectable members of the scientific community? If one adopts a naturalistic stance, as I do, there can be no appeal to *a priori* principles or analyses of the meaning of evidence. One can only look to scientific practice, including the historical development of that practice. Historically, empirical testability has been a prominent feature of scientific methodology. On most accounts, careful observation of the motions of the planets played a major role in the Copernican Revolution, leading to the acceptance of Copernican models over all others. In the nineteenth century, careful optical experiments led to the acceptance of wave over particle models of the propagation of light. In the twentieth century, experiments played a major role in the acceptance of quantum theory and have produced severe constraints on any possible deterministic models of micro-phenomena.

But are empirical constraints enough? Advocates of multiverses respect the constraints provided by accepted experimental and observational results. They simply want to extend their models in ways that no further experiments or observations can touch. Which is not to say that they recognize no constraints whatsoever on their further theorizing, just not empirical constraints. Might this not be a case in which advances in theory inspire advances in methodology? So, rather than unjustified claims we have methodological progress. I am still inclined to insist on empirical testability for accepting realistic claims, but I must admit that vindicating this insistence is a large remaining problem for scientific realism.[12]

NOTES

[1] Actually, van Fraassen does allow the possibility that we could admit aliens from outer space with a different sensibility into our epistemic community (Churchland and Hooker 1985, 256–258). But, as he insists, this possibility does not now turn anything not observable by existing humans into observable things.

[2] For a general discussion of distributed cognition in science, see Giere (2002). More specific questions about the role of human agency in distributed cognitive systems are taken up in Giere (2003).

[3] Putnam (1978). A good summary of the argument and attempts to meet it appears in a book by the cognitive linguist, George Lakoff (1987).

[4] The most infamous attempt along these lines was Popper's notion of verisimilitude. I take it that Popper's approach has now been thoroughly discredited, so will say no more about it.

[5] Among those attempting to explicate a notion of approximate truth within a standard logical framework are Ninniluotto (1987) and Kuipers (2000). Da Costa and French (2003) present a more recent model theoretic attempt.

[6] For an elaboration on the varieties of models, see (Giere 1999b).

[7] This point is argued in Giere (Forthcoming).

[8] I say "was" because CCRO was deliberately deorbited in 2000 at the end of its useful life.

[9] Howson and Urbach (1989). In fact, many of those pursuing the Bayesian program think of it as providing a justification for scientific claims rather than merely a description of how such claims come to be regarded as scientifically justified. Here I am taking it to be descriptive only.

[10] This creates a special problem for van Fraassen since belief in the empirical adequacy of the hierarchy of models includes beliefs about what might be recorded by other instruments aimed at the center of the Milky Way. There is nothing in the logic of the situation that would make the conjunction of all such beliefs more probable than the single theoretical claim that there are sources of 1.8 MeV gamma rays in the center of the Milky Way. So, within this probabilistic methodology, there seems no possible in principle argument that belief in the overall empirical adequacy of the models is legitimate while belief in a single theoretical claim is not?

[11] For a popular exposition of this theme see Tegmark (2003).

[12] For a sensitive discussion of these sorts of issues, see the final chapter of Leplin (1997).

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REFERENCES

Bruner, J. S., J. J. Goodnow and G. A. Austin: 1956, *A Study of Thinking*, Wiley, New York.

Carnap, R.: 1956, 'The Methodological Character of Theoretical Concepts', in H. Feigl and M. Scriven (eds.), *The Foundations of Science and the Concepts of Psychology and Psychoanalysis*, pp. 38–76. Minnesota Studies in the Philosophy of Science, Vol. 1, University of Minnesota Press, Minneapolis.

Cartwright, N. D.: 1983, *How the Laws of Physics Lie*, Oxford University Press, Oxford.

Cartwright, N. D.: 1989, *Nature's Capacities and Their Measurement*, Oxford University Press, Oxford.

Cartwright, N.: 1999, *The Dappled World*, Cambridge University Press, Cambridge.

Chakravartty, A.: 2001, 'The Semantic or Model-Theoretic View of Theories and Scientific Realism', in *Synthese* **127**: 325–345.

Churchland, P. M.: 1985, 'The Ontological Status of Observablesn Praise of the Superempirical Virtues' in P. M. Churchland and C. A. Hooker (eds.), *Images of Science*, University of Chicago Press, Chicago, pp. 35–47.

Churchland P. M., C. A. Hooker (eds.): 1985, *Images of Science*, University of Chicago Press, Chicago.

Da Costa, N. C. A and S. French: 2003, *Science and Partial Truth: A Unitary Approach to Models and Scientific Reasoning*, Oxford University Press, Oxford.

Giere, N. R.: 1988, *Explaining Science: A Cognitive Approach*, University of Chicago Press, Chicago

Giere, R. N.: 1999a, *Science without Laws*, University of Chicago Press, Chicago.

Giere, R. N.: 1999b, 'Using Models to Represent Reality' in L. Magnani, N. J. Nersessian and P. Thagard (eds.), *Model-Based Reasoning in Scientific Discovery*, Kluwer, Dordrecht, pp. 41–58.

Giere, R. N.: 2002, 'Scientific Cognition as Distributed Cognition' in P. Carruthers, S. Stitch and M. Siegal (eds.), *The Cognitive Basis of Science*, Cambridge University Press, Cambridge, pp. 285–299.

Giere, R. N.: 2003, 'The Role of Computation in Scientific Cognition', in *Journal of Experimental & Theoretical Artificial Intelligence* **15**: 195–202.

Giere, R. N. Forthcoming. *Models, Metaphysics and Methodology*.

Howson, C. and P. Urbach: 1989, *Scientific Reasoning: The Bayesian Approach*, Open Court, La Salle, IL.

Kuipers, T. A. F.: 2000, *From Instrumentalism to Constructive Realism. On some relations between confirmation, empirical progress, and truth approximation*. Dordrecht, Kluwer.

Lakoff, G.: 1987, *Women, Fire, and Dangerous Things: What Categories Reveal About the Mind*, University of Chicago Press, Chicago.

Leplin, J.: 1997, *A Novel Defense of Scientific Realism*, Oxford University Press, New York.

Maxwell, G.: 1962, 'The Ontological Character of Theoretical Entities', in H. Feigl and G. Maxwell (eds.), *Scientific Explanation, Space, and Time*, pp. 3–27. Minnesota Studies in the Philosophy of Science, Vol. 3, University of Minnesota Press, Minneapolis.

Niiniluoto, I.: 1999, *Critical Scientific Realism*, Oxford University Press, Oxford.

Niiniluoto, I.: 1987, *Truthlikeness*, Reidel, Dordrecht.
Nye, M. J.: 1972, *Molecular Reality*, Macdonald, London.
Psillos, S.: 1999, *Scientific Realism: How Science Tracks the Truth*, Routledge, London
Putnam, H.: 1978, *Meaning and the Moral Sciences*, Routledge, London.
Scriven, M.: 1961, 'The Key Property of Physical Laws–Inaccuracy' in H. Feigl and G. Maxwell (eds.), *Current Issues in the Philosophy of Science*, Holt, Rinehart, and Winston, New York, pp. 91–101.
Tegmark, M.: 2003, 'Parallel Universes', *Scientific American*, 2003.
Teller, P.: 2001, 'Twilight of the Perfect Model Model' , in *Erkenntnis* **55**: 393–415.
Van Fraassen, B. C.: 1980, *The Scientific Image*, Oxford University Press, Oxford.
Van Fraassen, C. B.: 1985, 'Replies to his Critics' in P. M. Churchland and C. A. Hooker (eds.), *Images of Science*, University of Chicago Press, Chicago, pp. 245–308.

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