

HOW TO BE A SCIENTIFIC REALIST

For Dick Blackwell, with gratitude and admiration

I begin with a thought experiment that Richard Rorty once proposed. “Imagine,” he said, “that a few years from now you open your copy of the *New York Times* and read that philosophers, in convention assembled, have unanimously agreed that values are objective, science rational, truth a matter of correspondence to reality, and so on.”¹ Rorty notes that the public reaction to such an event “would not be ‘Saved!’ but rather ‘Who do these philosophers think they are?’” And rightly so, he says, since what we should expect of philosophers (and all intellectuals) is not “getting it right” but “making it new,” not truth but originality.

Perhaps the most interesting thing about Rorty’s thought experiment is that, if we substitute “natural scientists” for “philosophers,” it is no longer a fantasy but an everyday fact. Nuclear physicists, industrial chemists, molecular biologists, et al. do routinely announce agreement on previously controversial issues. The public reaction, however, is the opposite of what Rorty imagines it being to a similar philosophical development. And, I would add, “rightly so.”

Why? The obvious answer, which Rorty rejects, is that science has access to the truth about the natural world in a way that philosophy — along with all other humanistic and most social scientific disciplines — does not. He acknowledges that there is something strikingly distinctive about the natural sciences: their remarkable success in predicting and therefore in controlling events in the world. But, he asks, “What is so special about prediction and control?”² By which he means, Why should we think that success at prediction and control is an indication of some special access to the truth about reality? “Why,” for example, “should we think that the tools which make possible the attainment of these particular human purposes are less ‘merely’ human than those which make possible the attainment of beauty or justice?”³

By holding that science has no ontological privilege over other types of knowing, Rorty is rejecting what I will call “methodological realism” regarding science. Among those who accept methodological realism, there is the further

question of what I will call “theoretical realism.” This is the question, Is there good reason to think that the entities postulated by scientific theories exist? For example, Is there sufficient evidence for the claim that the objects of ordinary experience are in fact made up of atoms too small to be seen? The theoretical realist says yes; the theoretical antirealist — Bas van Fraassen will be my main example — says no. My aim in this paper is to sketch defenses of both methodological and theoretical realism about science.

I. RORTY AND METHODOLOGICAL REALISM

On Rorty’s view, the only epistemological difference between the natural sciences and their less vaunted counterparts in the *Geisteswissenschaften* is that the former are able to achieve a high degree of consensus. “If we say that sociology or literary criticism ‘is not a science,’ we shall mean merely that the amount of agreement among sociologists or literary critics on what counts as significant work . . . is less than among, say, microbiologists.”⁴ But doesn’t it seem likely that the agreement among microbiologists is connected with their ability to make successful predictions? If you think antibiotics are an effective treatment for ulcers and I don’t, the best way for you to convince me will be to show a high rate of recovery for ulcer patients who took antibiotics. Predictive success is one of the best sorts of evidence for the truth of a scientific hypothesis, and since scientists want the truth about the world, they tend to accept hypotheses with a high degree of predictive success.

Rorty sometimes gives the impression that any scientific interest in truth is an unfortunate misdirection, that we should “replace the desire for objectivity — the desire to be in touch with a reality which is more than some community with which we identify ourselves — with the desire for solidarity with that community.”⁵ But such remarks misleadingly suggest that scientific knowledge is entirely constituted by our agreement to think and talk in a certain way. This obscures the fact that the “human community” is, to adapt the Heideggerian phrase, a “community-in-the-world,” from the start interacting with things that exist in causal independence of it. Rorty is right to reject a metaphysical realism that views knowing as a matter of forming mental representations that can be somehow shown to accurately picture a world beyond those representations with which we have no direct contact. But rejecting metaphysical realism in this sense should not lead us to deny (or, like Berkeley, philosophically reinterpret) the existence of anything ordinary people believe in. In the same vein, there is no reason to agree

¹Richard Rorty, “Science as Solidarity,” in *Objectivity, Relativism, and Truth* (Cambridge: Cambridge University Press, 1991), 23.

²Richard Rorty, “Is Natural Science a Natural Kind?,” in *Objectivity, Relativism, and Truth*, 58.

³Ibid.

⁴Richard Rorty, “Science as Solidarity,” 40.

⁵Ibid., 30.

with fictionalists and instrumentalists, who, on the basis of philosophical theories, deny the existence of the theoretical entities postulated by science.

We can, however, share Rorty's concern that we not interpret the predictive success of science as a sign of its ability to reach some special sort of truth — a truth beyond that of ordinary experience and thought. He rejects the idea that "some of our true beliefs are related to the world in a way in which others are not."⁶ Any true belief is, humdrumly, true of the world with which we find ourselves engaged from the beginning. But no belief — not even those of the most successful science — is true in some deeper sense.

Those who think there is some deeper truth in science appeal to the idea that science has a special method (e.g., abductive inference) that reveals to us special objects of knowledge (e.g., the "really real"). Such ideas are typically formulated in terms of the classical modern picture of a mind that knows directly only representations and needs the special scientific method to ground an inference to the existence of, for example, the microentities that are the realities to which true representations correspond. This, for example, is often the thinking behind efforts to argue that the truth of scientific theories (and hence the existence of the entities they postulate) is the best explanation of their predictive success. Here, an argument for scientific realism (the existence of theoretical entities) is equated with an argument for metaphysical realism (the existence of a noumenal world beyond that of the world that appears to us).

Rorty is right to reject such arguments. There is no basis for asserting the existence of a noumenal world or of a special scientific method for knowing about it. If the cognitive privilege of natural science can be based only on the privileged representations of classical modern epistemology, then this privilege is an illusion. But this is not the only way. There is no need to take the predictive success of science as evidence (via an argument to the best explanation) for a special sort of scientific truth. Predictive success (of the right sort and sustained over time) itself makes natural science cognitively privileged. This privilege does not consist in knowing a world beyond that in which we all live, by methods no one else uses. It consists rather in knowing the world in which we all live much better by making particularly effective use of one of the common ways of knowing this world.

More fully: from time immemorial, one of the ways of finding out more about the world has been to perform tests, to guess on the basis of past experience what might happen in the future, see if our guess is correct, revise it if not, etc. Over the centuries, and with increasing momentum since the 1600s, this "empirical" approach — this effort to know about things by trying to predict their behavior — has been made more refined, precise, and rigorous. And, particularly in the

How To Be a Scientific Realist
Gary Gutting

last 300 years, the resulting knowledge about the world has been spectacular. However, far and away the largest and most precise body of such knowledge has been about the non-human domain of the world, from stars and planets to the micro-structures of animals and chemicals. The natural sciences, in short, are cognitively privileged simply because they have yielded so much more and so much more precise knowledge of the world than any other inquiries.

This privileged status has nothing to do with knowing a metaphysical world-beyond-the-world nor with having gained access to some privileged set of representations. It does have something to do with the now evident superiority of studying the natural world through systematic empirical methods, as opposed to relying on traditional authorities, common-sense intuitions, or a priori philosophical analyses. But this is just to say that these methods have, as a matter of fact, been the most successful in telling us about the way the natural world behaves. Rorty is right to insist that the success of these methods in this context does not prove that they should be made the model for inquiring about other things we are interested in (art, ethics, politics), or that inquiries not using these methods are senseless or unimportant. But he is wrong to say that, apart from a contingent consensus of opinion, natural science has no cognitively privileged status.

I acknowledge the entirely mundane character of the points I have been making about natural science. I have insisted on them only because Rorty has, in opposing a misunderstanding, gone past the truth in the other direction. In rightly arguing that science does not have the sort of privilege representationalism accords it, he ignores the substantial sort of privilege that it does have.

This obscuring of the true status of science is particularly evident in Rorty's discussion of the "ontological significance" of science's predictive success. He begins by rightly noting the vacuity of a general argument for the truth of scientific theories from their predictive success (e.g., in technological applications). A general argument of this sort is no more convincing than one from the success of Moslem armies to the truth of Islam. He also rightly notes that the problem with such an argument is precisely the abstract, overly general level at which it operates. We would need to have detailed explanations of just how specific scientific procedures have led to specific successes. (This, for example, is the problem with the proof of Islam from military success. No one can explain how and why the will of Allah has brought about just these victories in just this sort of way.) Rorty sets out what is required as follows: "They [proponents of a special cognitive status for science] are going to have to isolate some reliability-inducing methods which are not shared with all the rest of culture and then isolate some features of the world which gear in with these methods. They need, so to speak, two independently describable sets of cogwheels, exhibited in sufficiently fine detail so that we can see just how they mesh."⁷

This requirement is acceptable, with two qualifications. First, as we saw above, what matters is that the results, not the methods, of science are distinctive.

Science is superior to other cognitive efforts because of its distinctively successful developments of the standard methods we use to learn about the world. Second, it is misleading to portray the move from predictive success to cognitive significance as made by philosophers (or others) who reflect on scientific practice and somehow correlate it with “an independently describable” account of the world. Scientists themselves are after knowledge, and their practices involve constructing descriptions of the world, the truth of which they infer from their predictive success. Given these two qualifications, however, there is no problem pointing out in detail the case, from predictive success, for the truth of scientific results (and hence for the cognitive significance of the enterprise). We need only review the complex series of theoretical developments, mathematical calculations, and empirical tests whereby the scientific community has convinced itself of the correctness of a given hypothesis. We could, for example, trace the career of the atomic hypothesis from its initial modern employment by Dalton, through the decisive nineteenth-century triumphs that convinced even such skeptics as Mach, to our current total ease with speaking of manipulating and even “taking pictures” of atoms. Such an account would show how specific scientific developments (e.g., certain of Lavoisier’s experiments, specific solutions of the Schrödinger equation) convinced scientists that atoms had specific characteristics.⁸

I have said that the above sort of account provides what Rorty asks for as an explanation of just how predictive success leads to ontological significance. Rorty apparently would not agree, since his own discussion looks for (and fails to find) the explanation in a quite different direction. This is the direction of causal production rather than rational justification. His idea is that the realist would need to show just how the independently existing world brings it about that we have beliefs that accurately represent it. The sort of thing he is looking for is, for example, an explanation of “how gravity caused Newton to acquire the concept of itself.”⁹ He says further that “the closest I can come to imagining what such an explanation would be like would be to describe what happens in the brain of a genius who suddenly uses new vocables”¹⁰ We are hardly surprised that such a bizarre explanatory search comes up empty. But we should be surprised that Rorty thinks this is the sort of account required to explain the success of science in describing the world. It is, perhaps, the kind of account required to ground a representationalist move from the world as it appears to us to the world as it really is in itself. But, as I have been insisting, a realistic view of natural science need not endorse this sort of metaphysical realism.

On the view I’m supporting, realism is just the claim that natural science has been able to achieve a remarkable amount of knowledge about the world (the one

How To Be a Scientific Realist
Gary Gutting

we all live in and know about); such a claim is justified by natural science's high level of success in predicting events in that world.¹¹ The explanatory accounts that show this connection between truth and predictive success are not given in the space of causes but, as fits Rorty's own account of justification, in the space of reason-giving. Such accounts — which merely rehearse the rational path natural science itself has followed in justifying its claims — show just how it is that natural science has achieved more cognitively than other modes of inquiry. They thus provide, in a way that entirely accords with his view of justification, the answer to Rorty's question, "What is so special about prediction and control?" This answer shows why, with no compromise in our critique of modern epistemology, we can and should accept a privileged role for natural science as a description of reality.¹²

Critics may suggest that my talk of science's ontological significance and cognitive superiority is merely fancy language for its obvious advantage in prediction and control. To make anything more of it will, they may urge, return us to a discredited representationalism. For such critics, I go wrong by arguing from predictive success to ontological significance in the sense of "conveying the truth about nature as it is in itself, independent of our beliefs and desires." But this sort of objection confuses the problematic sense of "ontological significance" that underlies representationalist theories of knowledge with the entirely unproblematic sense required by the humdrum realism of our everyday presence in the world. Quite apart from any philosophical theories, we have a good deal of knowledge about things that exist independent of the web of our beliefs and desires. Much of this is simply the result of ordinary sense experience (what we have seen, heard, or run up against), but much of it is also an extension of such knowledge by the focused and controlled efforts of experimental science. Some of the latter is just a matter of closer direct observation, perhaps aided by simple instruments such as calipers or magnifying glasses. But just as ordinary knowledge involves inferences from observed facts to unobserved explanations of these facts (e.g., from noises in the wall to a mouse making the noises), so scientific knowledge involves inferences to unobserved (though in principle observable) entities such as the earth's core, dinosaurs, and gases in the interior of stars. To say that natural science has achieved a body of objective knowledge beyond that of common-sense is to say that such entities exist in the same sense that the ordi-

⁶Richard Rorty, "Natural Science a Natural Kind?," 51.

⁷*Ibid.*, 55.

⁸For an example of such an account, see Ernan McMullin, "What Do Physical Models Tell Us?" in *Logic, Methodology, and Philosophy of Science*, ed. B. von Rootselaar (Amsterdam: North Holland, 1968).

⁹"Natural Science a Natural Kind?," 57.

¹⁰*Ibid.*

¹¹This is the sort of realism Rorty defines as

"the idea that inquiry is a matter of finding out the nature of something which lies outside the web of beliefs and desires" (Rorty, "Inquiry as Recontextualization," *Objectivity, Relativism, and Truth*, 96). I remain puzzled as to why Rorty thinks he needs to reject this view.

¹²I discuss Rorty's views on science more thoroughly in my "Paradigms and Hermeneutics: A Dialogue on Kuhn, Rorty, and the Social Sciences," *American Philosophical Quarterly* 21 (1984), 1–5.

nary objects of direct sense experience do and are not merely convenient fictions facilitating prediction and control. To say that the natural sciences are cognitively superior to the social sciences is to say that, on the whole, sociology, anthropology, and even psychology and economics have not developed comparably rich and detailed extensions of common-sense knowledge.

My defense of methodological realism is independent of most of the issues currently debated by philosophers of science under the heading of scientific realism. The position is, for example, consistent with Ian Hacking's combination of an acceptance of the existence of theoretical entities with a rejection of the detailed truth of specific theories about them.¹³ It is even consistent with Bas van Fraassen's thorough-going antirealism about theoretical entities.¹⁴ Methodological realism can be maintained simply in terms of the new *observable* entities and truths about them discovered by natural science; it does not require any particular view about the ontological status of unobservable entities.

Methodological realism is even consistent with many contemporary claims about the fundamentally social nature of the scientific enterprise — including claims about the socially negotiated nature of scientific concepts and justifications. It contradicts social constructivist accounts only when these are pushed to a skeptical limit; that is, when, in challenging claims of scientific truth, they also, incoherently, challenge the humdrum realism about the world of everyday experience that, as Hume urged, none of us can reject in a sustained way. This point can be generalized to express the core of methodological realism: a realistic view of science is necessary precisely to the extent that denying it will, in virtue of science's status as an extension of everyday knowledge, also require a denial of humdrum realism. But such realism is, as we have seen, consistent with a rejection of theoretical realism. This means that I need to make a separate case for this view.

II. VAN FRAASSEN AND THEORETICAL REALISM

I have long thought that a decisive affirmative answer is provided by what I will call "McMullin's Master Argument." I will begin with an exposition of this argument and some comments on its advantages over other defenses of theoretical realism, and I will then move to an examination of the antirealist response to the argument available from the work of Bas van Fraassen.

A. The Argument

The argument I have in mind derives from the work of Ernan McMullin at

How To Be a Scientific Realist
Gary Gutting

least in the sense that it continually occurs to me when I read his writings on realism. Beyond this, I think it is implicit in his case for realism, even if he does not explicitly formulate it in the way I will.¹⁵ The Master Argument is based on a comparison of the disputed arguments for the reality of theoretical entities with unproblematic arguments for other postulated entities. We all agree that the earth has a molten core, that there are immensely hot gases in the interior of the sun, that dinosaurs once roamed parts of North America. In each case, we believe this because of abductive arguments (or inferences to the best explanation — I will not be concerned with details of the precise inference form or the best way to describe it) from a set of data. What the Master Argument points out is that arguments for theoretical entities are of exactly the same form. Just as we postulate a molten core, solar gases, and dinosaurs as the only sensible explanations of certain observed facts, so we postulate atoms, electrons, and neutrinos as the only sensible explanations of other sets of data.

Apart from anything else, the Master Argument has the advantage of traveling light. It does not, for example, require an account of the precise structure of abductive inference in general. Its point is simply that, regardless of what such an account would be like, the arguments in the two cases are the same, so that if one is valid so is the other. Further, the Master Argument does not require the dubious assumption (à la Boyd and Putnam) that philosophers should or could explain the success of scientific practice. It is not a meta-explanation of the power of scientific explanations but merely an effort to be consistent in drawing conclusions from scientific evidence. Finally, the Master Argument does not require a commitment to metaphysical realism. It makes no assumptions at all about whether scientific entities are mind-independent. It merely maintains that the unobservable entities postulated by scientific theories should be given the same metaphysical status (whatever that may be) as observable entities. These last two features, in particular, make it immune to some of the most powerful critiques of realism, such as those of Bas van Fraassen and Arthur Fine.

This immunity is not bought at the price of triviality. The argument's conclusion does, if sound, refute the skepticism about theoretical entities expressed by empiricists from Mach to van Fraassen. But the immunity is due to the argument's separation of the precise question of *scientific realism* from other issues about the methodology and metaphysics of science.

¹³Ian Hacking, *Representing and Intervening* (Cambridge: Cambridge University Press, 1983).

¹⁴Bas van Fraassen, *The Scientific Image* (Oxford: Oxford University Press, 1980).

¹⁵See especially, Erman McMullin, "The Case for Scientific Realism," in ed J. Leplin, *Scientific Realism* ed. J. Leplin (Berkeley: University of California Press, 19).

B. An empiricist critique of the argument

Van Fraassen's response to this argument is straightforward: even if, in some broad sense, the logic of these two arguments (whatever it may be) is the same, there is a crucial epistemic difference between them. The arguments for dinosaurs, etc. conclude to entities that are observable (even if never in fact observed), whereas the entities postulated by the other argument are unobservable.

Why should this difference be relevant? One line of explanation is based on a fundamental emphasis of empiricist epistemology: the primacy of what falls within the domain of our experience and the cognitive risks of going beyond it. Empiricists have traditionally insisted on grounding all knowledge claims in what our senses can know, and they have been justifiably reluctant to make leaps beyond it.

But this explanation is not satisfying for two reasons. First, it approaches the issue from the wrong end, pointing out the need for epistemic *justifiers* to be in the observable domain, when the issue is rather about the status of what is epistemically *justified*. The Master Argument, after all, does find its justification in the observed facts. Second, with respect to what is justified, the response bestows an apparently arbitrary privilege on the observable. What is observable is contingent on physiological facts about the human organism. As van Fraassen puts it:

The human organism is, from the point of view of physics, a certain kind of measuring apparatus. As such it has certain inherent limitations — which will be described in detail in the final physics and biology. It is these limitations to which the 'able' in 'observable' refers — to our limitations, *qua* human beings.¹⁶

How can the existence of such limitations affect the justifiability of an inference? We might understand a (very radical) empiricism that refused to accept as fact anything that had not itself been directly observed. But such an empiricism would have to reject both the arguments for dinosaurs, the earth's core, etc. and the arguments for theoretical entities, since none of these things are in fact observed. But, once we allow inferences to what is *not observed*, why should there be any scruples about inferences to what also happens to be *unobservable* to human beings? If, as van Fraassen says, the human organism is, in this context, just "a certain kind of measuring apparatus," why privilege its limitations over those of other kinds of measuring apparatus?¹⁷

Such privilege might make empiricist sense if there were some fundamental difference in kind between what is observable and what is not. A Kantian, for example, might characterize unobservability in terms of the inapplicability of the

categories of the understanding. But the empiricist argument we are now discussing makes no such move. What is unobservable may be so simply because it is just a tad too small for us to see — e.g., a grain of sand just below the threshold of perceivability.

When challenged, van Fraassen has usually defended the epistemic relevance of observability in two ways. The first is to maintain that the relevance follows from the fundamental empiricist principle that “experience is the sole legitimate source of information about the world.”¹⁸ More fully, he says:

If we choose an epistemic policy to govern under what conditions, and how far, we will go beyond the evidence in our beliefs, we will be setting down certain boundaries. I could not envisage a nonextreme rational policy that would make these boundaries independent of our opinions about the range of possible additional evidence.¹⁹

But such general declarations of empiricist principle hardly support the conclusion that there can never be empirical evidence sufficient to support an inference to the existence of nonobservable entities. The realist is entirely faithful to the principle van Fraassen has stated as long as his argument for realism does not appeal to *evidence* that goes beyond what is empirically accessible. Van Fraassen thinks that, in addition, the entities inferred on the basis of the evidence must be empirically accessible. But this is a very special reading of empiricism that requires support beyond any given by the general principle itself.

Van Fraassen’s second way of defending the epistemic relevance of observability is based on what he calls his “disdain for opinions inflated beyond what can run the gauntlet of experience.”²⁰ Here his point is that “if I believe the theory [that postulates unobservable entities] to be true and not just empirically adequate, my risk of being shown wrong is exactly the risk that the weaker, entailed belief will conflict with actual experience.”²¹ Put it this way: we have the choice of saying merely that the observable world goes just as it would if there were electrons (i.e., that electron-theory is empirically adequate) or more robustly, of saying that there actually are electrons. But making the metaphysically more robust assertion requires no more epistemic courage than does making the weaker claim. Both assertions have exactly the same class of potential defeaters: (refuting empirical evidence).

Van Fraassen’s point is correct, but why should it deter us from realism? Once again, the issue is the epistemic relevance of mere observability as opposed to what is actually observed. There are many cases of inferences to entities that

¹⁶Van Fraassen, *Scientific Image*, 17.

¹⁷I owe this thought to Michael Bergmann.

¹⁸“Replies to Critics,” in *Images of Science*, ed. Paul M. Churchland and Clifford A. Hooker, (Chicago: University of Chicago Press, 1985), 258.

¹⁹van Fraassen, “Replies to Critics,” 254.

²⁰*Ibid.*

²¹*Ibid.*, 255.

are observable in principle (dinosaurs, the earth's core), but that we are quite certain we will never observe. Asserting the existence of such entities involves no more real risk of refutation than does the assertion of the existence of unobservable entities. The two sorts of cases are equivalent as far as any evidence we will ever have is concerned. Surely, then, it is arbitrary to say that assent in the one case violates empiricist principles, but in the other does not. Mere non-observability, therefore, is too weak a reed to support van Fraassen's antirealism.²²

C. The Critique Renewed

But perhaps there is a more effective way to revive the empiricist critique of the Master Argument. The theoretical entities postulated by science often differ from common-sense objects in more than just their non-observability. In particular, the entities of theories formulated in terms of contemporary physics are notable for their "weirdness," where "weird" can be taken as a technical term for "violating one or more of the categories (of, for example, causality, identity, spatio-temporal location) that govern ordinary perceptual objects." I want to suggest that weirdness can constitute an epistemically relevant difference between the two sorts of abductive inferences considered by McMullin's Master Argument. Further, as we shall see, weirdness is often closely linked to observability.

Consider the hackneyed case of an inference, from sounds inside a wall, the disappearance bits of cheese over night, etc., to the existence of an unobserved mouse. Such an inference is frequently justified, but only in circumstances when we have reason to think that there are likely to be such things as mice around. If, for example, our wall is in Hamelin just after the first visit of the Pied Piper, then we might well not be justified in concluding from the given evidence to the existence of a mouse in the wall. The inference would be similarly unjustified any time we are in an environment (for example, a planet of another galaxy) that we have no reason to expect to resemble ours in its fauna. The general point is this: an abductive inference to a certain sort of entity will often be justified only if we have independent reason to think that there are (in the relevant domain) entities of that sort. Accordingly, inferences to the earth's core, etc. may well not be relevantly similar to inferences to weird theoretical entities, since we don't have independent reason to think that entities of this sort exist.

It is worth noting that van Fraassen himself has in effect cited weirdness as relevant to his reluctance to accept the existence of unobservable entities. Reflecting on various roots of his antirealism, he mentions his "appreciation of just how unimaginably different is the world we may faintly discern in the models science gives us from the world we experientially live in." He goes on to drive the point home with some striking examples:

How To Be a Scientific Realist
Gary Gutting

Do the concepts of the Trinity, the soul, haecceity, universals, prime matter, and potentiality baffle you? They pale beside the unimaginable otherness of closed space-times, event-horizons, EPR correlations, and bootstrap models.²³

My discussion of weirdness can be read as a development of the case against realism implicit in these remarks.

The appeal to weirdness is readily connected to van Fraassen's insistence on the epistemic relevance of observability. In typical cases, we know that an entity is observable precisely because it is of the same sort as entities that we have observed. Thus, its observability is an indication that it is not weird and hence that we are entitled to an abductive inference to its existence. Or, to make the point in a slightly different way, we know that entities of the kind that we postulate exist because we have observed them. Thus weirdness supplies the missing link in the empiricist critique of the Master Argument for realism. Abductive arguments to observable entities are relevantly different from abductive arguments to nonobservable entities when nonobservability is an indication of weirdness.

The above line of thought can also be formulated in terms of another source of challenges to realism: the underdetermination of theories by data. Although such underdetermination is always a logical possibility in inferences to what is not actually observed, it loses its epistemic force when we have good reason to think that the postulated entity is of the same sort as ones we have observed. A mouse, for example, is just the sort of thing we expect, on the basis of past experience, to be making noises inside our walls. In ordinary circumstances, this gives us good reason to exclude alternative hypotheses, such as murine simulators planted by space aliens. But when we postulate weird unobservable entities, we lose this basis for excluding hypotheses compatible with our data and the problem of underdetermination must be faced.

D. The limits of theoretical realism

Supposing this rehabilitation of van Fraassen's critique is successful, just how far does it go toward supporting antirealism? It surely shows that we are not obligated to accept an argument for theoretical entities just because it is based on evidence as strong as a corresponding argument for observable entities. The weirdness of the theoretical entities may require an argument with a much higher degree of evidence. However, the critique does not achieve the antirealist's goal of offering an obstacle in principle to any possible realist inferences. First, there is still room for inferences to theoretical entities that are not weird; e.g., ones that are just smaller versions of ordinary perceptual objects. One might, for example, maintain that the Master Argument still supports inferences to the existence of

²²I have also developed this line of argument in "Scientific Realism vs. Constructive Empiricism," *The Monist* (1982):336-349

(reprinted in *Images of Science*, 118-131).

²³Van Fraassen, "Replies to Critics," 258.

unobservable organic molecules. Second, the critique does not show that there could not be sufficient evidence for the existence of weird theoretical entities, only that such evidence would have to be far more than what is required in cases of inferences to what is observable. Here, of course, we would have to judge each case for the existence of a theoretical entity on its own merits. It seems to me, however, that the case for entities such as the electrons postulated by successive versions of quantum electrodynamics are very strong indeed.

The critique of the Master Argument based on weirdness is, then, limited in its impact and does not simply refute realism. On the other hand, the critique achieves as much as I think an empiricist ought to want. It gives us serious reason to question any but the most rigorous experimental demonstrations of the existence of theoretical entities, and it will serve to short-circuit many casual claims about what science has discovered. (Certainly, for example, there is no reason to accept every new form of elementary particle announced as “discovered” by experimental physicists.) There is, however, no exclusion in principle of any and all inferences to the unobservable. But why should an empiricist want such a thing? Empiricists, after all, are dedicated to following the path of the sensory evidence wherever it leads and are famously leery of general principles that are not unquestionably supported by specific facts. The effort to show that no argument for theoretical entities can possibly succeed amounts to a negative transcendental argument that is just the sort of thing that should arouse empiricists’ suspicions. The spirit of their position is much better represented by the view we have arrived at in our discussion of the Master Argument: that the acceptance or rejection of theoretical entities cannot be settled by general philosophical considerations, but needs to be determined one case at a time by assessing the evidentiary force of each specific set of empirical data. Empiricists like van Fraassen should remain faithful to their roots by abandoning their transcendental rejection of theoretical realism.

How To Be a Scientific Realist
Gary Gutting