SCIENTIFIC REALISM *VS.* CONSTRUCTIVE EMPIRICISM: A DIALOGUE

Note: *The following is a discussion between a scientific realist* (SR) *who has been strongly influenced by the work of Wilfrid Sellars and a constructive empiricist* (CE) *who has been equally influenced by the work of Bas van Fraassen. Indeed, the influence is so great that the interlocuters occasionally lapse into direct quotation of their masters. I do not, however, want anyone to* identify *the views of my two characters with those of Sellars and van Fraassen. What they say merely represents the dialectic of my own mind as I think through the issues raised by the debate between Sellars and van Fraassen on scientific realism.*

SR.: Realism is encapsulated in the claim that "to have good reason for holding a theory is *ipso facto* to have good reason for holding that the entities postulated by the theory exist."[1] For an appropriate scientific theory (say atomic theory), this claim allows us to argue as follows:

(1) If we have good reason for holding atomic theory, we have good reasons for holding that atoms exist;

(2) We do have good reason for holding atomic theory (it's highly confirmed, fruitful, simple, etc.);

(3) Therefore, we have good reason for holding that atoms exist.

CE: Everything depends on what we mean by "holding a theory." If it means "believing that the theory is true," then premise (1) of your argument is obvious but premise (2) strikes me as false. There's a lot to be said for atomic theory but none of it constitutes a cogent case for its truth. At the most, the evidence shows that atomic theory is empirically adequate, by which I mean that there may be reason to think that all its *observable* consequences are true. But the evidence does not support the existence of the particular unobservable mechanisms and entities the theory postulates. On the other hand, if you take "holding a theory," as I do, to mean "believing it to be empirically adequate," then there's no problem with premise (2) but, for the reasons I've just been urging, there is no basis for premise (1).

SR: I'm willing to stand by the argument even if we take "holding a theory" to mean "believing it to be empirically adequate;" but we need to get clear on just what's involved in empirical adequacy. For example, it won't do to take empirical adequacy in the minimal sense of "accurately describing all the

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observable phenomena." With this sense of "empirically adequate" the argument will fall to the old problem of the underdetermination of theory by data. Specifically, with this meaning of "empirically adequate," premise (1) says: "If we have good reason for believing that atomic theory accurately describes all the observable phenomena, then we have good reason for believing that atoms exist." But this isn't so since, first, there are an infinity of other sorts of theoretical entities that would produce the same observable phenomena and, second, we could just as well believe only in the phenomena and forget about underlying entities.[2]

CE: Your second point is just my view. I'm not saying theoretical entities don't exist or that talk about them is meaningless. I don't even say there's anything wrong with believing in them if you want to. My point is simply that there's no evidence that makes it irrational to withhold judgment about their existence. I'm defending my right to be an agnostic on the issue. I suspect however that, just like theists who deny the rationality of religious agnosticism, you're going to invoke the explanatory power of your postulations to support their existence.

SR: Of course, though the case for scientific realism can avoid the pitfalls of "theological realism." The point is that the empirical adequacy of a scientific theory needs to be taken broadly enough to include the theory's explanatory power and, specifically, its explanatory superiority to the physical thing language used in the observation framework. Atomic theory and its associated ontology is needed precisely because of the explanatory failures of the observation framework. So, roughly, premise (1) needs to be understood as saying this: If we have good reasons to believe that atomic theory is needed to explain the observable data, then we have good reason to believe that atoms exist.

CE: What I want to question is the move you're trying to make from an explanatory need for a theory to the existence of its entities. Consider, for example, Sellars's fictional case of observationally identical samples of gold that dissolve at different rates in *aqua regia*. I agree that available microtheory might explain the empirical fact of the different dissolution rates. "The microtheory of chemical reactions might admit of a simple modification to the effect that there are two structures of microentities each of which 'corresponds' to gold as an observational construct, but such that pure samples of one dissolve, under given conditions of pressure, temperature, etc., at a different rate from samples of the other. Such a modification of the theory would explain the observationally unpredicted variation in the rate of dissolution of gold by saying that samples of observational gold are mixtures of these two theoretical structures in various proportions, and have a rate of dissolution which varies with the proportion."[3]

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Of course, I'd expect the realist to admit in his turn that it might also be possible to sue the correspondence rules of our microtheory to "derive observational criteria for distinguishing between observational golds of differing theoretical compositions."[4] If so, we could formulate two empirical laws (in the observation framework), one for each variety of gold, that would explain the differing dissolution rates.

SR: But remember that it might also happen that a good theory does not allow the formulation of any such empirical generalizations. It might simply itself directly explain the singular observable fact of differing dissolution rates. In such a case, the theory would be necessary to give any explanation of the singular observed facts and so would have to be accepted as true if we were to have any explanation at all of these facts. And, of course, this is precisely my claim regarding postulational scientific theories that have actually been developed. If, for example, we do not accept the existence of atoms there are numerous singular empirical facts for which we simply have no explanation. This discussion lets me formulate more precisely premise (1) of the argument for realism. We should take it to say: If we have good reason to believe that atomic theory provides the only way of explaining some singular observed facts, then we have good reason to believe in the existence of atoms.

CE: Your more accurate formulation only serves to pinpoint the weakness of your position. Just what is the "explanatory failure" of the observation language? You agree that it can sustain inductive generalizations but insist that it cannot sustain "enough to explain all the singular facts that require explanation. In the case of the gold, one might have achieved a very precise statistical generalization: for each number r, the probability that a random sample of gold dissolves at rate r equal p(r) . . . . But faced with the question why a given sample of gold dissolves at rate r, the physical thing language provides us with no property X such that we could say: this is an X-sample, and all X-samples dissolve at that rate."[5] You conclude from this that we need a theory to explain what the observation language cannot. But the claim is far too strong. On your principles, an exactly parallel argument could be made for the existence of hidden variables underlying quantum phenomena. As is well known, the laws of quantum mechanics are irreducibly statistical; that is, they can explain why certain events occur a certain percentage of times over a given period but they cannot explain why one particular event occurs rather than another. For example, quantum mechanics has no explanation of the fact that a particular radium atom decays at a particular time, though it can explain why, over a period of time, a given fraction of the atoms in a sample of radium will decay. Using your principles, a quantum physicist would have to accept the suggestion, made by some physicists, that

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there are "hidden" entities and processes, not taken account of by quantum theory, that are responsible for the occurrence of the singular facts that theory cannot explain. But this conclusion, generated by an a priori demand for explanation, conflicts with the fact that the irreducibly statistical character of its laws does not, in the mind of the scientific community, constitute a case for the explanatory inadequacy of quantum theory and the need for the acceptance of hidden variables. So, just like the theist with his cosmological argument, you make your case by insisting on the need to explain something that there is no reason to think has to be explained. If the singular facts not explained by quantum theory don't need explanation, neither do the singular facts not explained by the observation framework.

SR: I entirely agree with you about quantum mechanics, and in arguing for realism I do not mean to "demand that all singular matters of fact be capable of explanation."[6] Perhaps the fictional gold example is misleading. The inadequacy of the observation framework does not consist in its inability to explain some singular empirical facts but in its inability to explain without relying on theoretical concepts. "It is not that the 'physical thing framework' doesn't sustain *enough* inductive generalizations, but rather that what inductive generalizations it *does* sustain, it sustains by a covert introduction of the framework of theory into the physical thing framework itself."[7]

CE: I take it, then, that you're revising even your most recent statement of premise (1) in your argument for realism. You now seem to be taking it to mean something like this: If we have reason to believe that all explanations of singular empirical facts (in a given empirical domain) must rely on the concepts of atomic theory, then we have good reason to believe in the existence of atoms.

SR: That's about what I have in mind.

CE: But then we need some clarification of what you mean by an explanation's "relying on the concepts of a theory." You're obviously assuming that explanation is at least partly a matter of subsuming singular empirical facts under generalizations. So one possible meaning of the claim that all of a set of observation-framework explanations "rely on the concepts of a theory" is that all the generalizations that accurately subsume the singular empirical facts must be expressed in theoretical terms. But taken this way the claim is clearly false. Even when theoretical corrections are provided for empirical generalizations, the results are typically expressible by a new empirical generalization in the observation language. For example, the correction of Boyle's Law by kinetic theory results in van der Waal's Law, which is still entirely observational.

SR: But my claim about the explanatory reliance of the observation framework on theory is not about the *content* of the generalizations used to

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explain singular empirical facts but about the way in which these generalizations are *inductively justified*. To a theoretical correction of an empirical generalization there will, I agree, typically correspond a revised empirical generalization. Further, this revised generalization will be compatible with the observational evidence. But, I claim, it will not typically be the law that would be accepted "on purely inductive grounds—i.e., in the absence of theoretical considerations."[8] Rather, lacking theoretical guidance, purely inductive reasoning in the observation framework alone would lead us to accept an empirical generalization that is shown by theoretical considerations to be false. So explanations of singular empirical facts rely on theory in the following sense: The empirical generalizations that explain these facts must be justified, ultimately, by theoretical considerations. This can be expressed by putting premise (1) in the following way: If we have good reason to believe that the empirical generalizations that explain singular empirical facts require an appeal to atomic theory for their justification, then we have good reason for believing in the existence of atoms.

CE: I have a number of reservations about your factual claim that the empirical data alone wouldn't lead us to the same law that theoretical considerations yield. We'd have to look at some examples to probe that thesis. But suppose I agree that theory plays the role you say it does, that it's essential for the development of adequate empirical generalizations. I still don't think there's any reason to accept such a theory as a true (or approximately true) description of the world. In other words, I might be prepared to accept the antecedent of your last formulation of premise (1), but I'll still deny the consequent. Here my position is like Duhem's. He shared your conviction that "postulation of unobservable entities is indispensable to science." But since he also held that improved description of observable phenomena is the only basic role for theoretical postulation, he maintained that there is no need for us to accept the existence of postulated entities. "If that is how one sees it, then truth of the postulates becomes quite irrelevant. When a scientific theory plays [its] role well, we shall have reason to use the theory whether we do or do not believe it to be true; and we may do well to reserve judgment on the question of truth. The only thing we need to believe here is that the theory is empirically adequate, which means that in its round-about way it has latched on to actual regularities in the observable phenomena. Acceptance of the theory need involve no further beliefs."[9]

SR: Don't we at least need an explanation of why the theory is empirically adequate; that is, of why what we observe is just as it would be if the theory were true? And isn't the best explanation just that the theory is in fact true (or anyway near the truth)? Don't we ultimately need to invoke realism to explain the success of science?

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CE: It seems to me that you're slipping back into the theological demand for explanation for its own sake. Why are you so sure that we need an explanation for science's success? But let me agree, at least for the moment, that we do need such an explanation. Even so, I think the appropriate explanation is quite different from the one you've proposed. After all, "science is a biological phenomenon, an activity by one kind of organism which facilitates its interaction with its environment."[10] Just as, from a Darwinian viewpoint, the only species that survive are those that are successful in coping with their environment, so too only successful scientific theories have survived. "Any scientific theory is born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive—the ones which in fact latched on to actual regularities in nature."[11] But this process of selection on the basis of empirical success need have nothing to do with the *truth* of the theories selected. Your argument is no different from that of an antievolutionist who holds that the survival of a species can be explained only by some design that has preadapted the species to its environment. But we don't need the hypothesis that theories are successful because truth has preadapted them to the world. We need only the hypothesis that theories that are not empirically successful have not survived.

SR: It seems to me you're ignoring the amazing *rate* at which empirically successful theories have emerged. Perhaps an extended process of trial and error would eventually lead to an empirically adequate scientific description of what we observe. But the use of theoretical postulations has led to success far more quickly than we could reasonably expect from mere trial and error selection. So even from your Darwinian viewpoint I think we need the realist hypothesis. But this is taking us off the track. The defense of realism that I'm interested in proposing need not be based on putting it forward as an explanation of science's success. Rather I have been arguing from the indispensable role of theoretical postulation in the formulation of empirically adequate laws. Think about it this way: Imagine that we are doing science initially only in the observation framework. We are aware of various singular observational facts and are trying to explain them by subsuming them under empirical generalizations—that is, under inductive generalizations that employ only the concepts of the observation framework.

CE: Excuse me a moment. Just how are you understanding the notion of *observation* when you speak of the observation framework? Some realists, you know, have maintained that anything—even electrons and other postulated submicroscopic entities—are in principle observable.

SR: I sympathize with some of the epistemological motives behind such claims. It is important to reject the idea that the realm of entities and properties we in fact observe functions as an unchangable given. But in this context

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we must avoid trivializing the distinction between what is observable and what is not. When someone says that everything is observable, he is envisaging a situation in which concepts from the theoretical framework of science have ingressed into the observation framework. I'm speaking of the observation framework prior to any theoretical ingressions.

CE: All right. So what we observe are the ordinary objects of everyday life and their properties.

SR: Yes, but we need to distinguish two sorts of properties that we attribute to observable things. On the one hand, there are occurrent (nondispositional) properties that are, strictly speaking, *what we perceive of an object* that we perceive. There is, for example, "the occurrent sensuous redness of the facing side" of a brick.[12] On the other hand, there are dispositional or, more broadly, causal properties that correspond to *what we perceive an object as* (e.g., the brick seen as made of baked clay). Within the observation framework properties of the first sort are a constant factor. They correspond to the way that, for physiological reasons, we must perceive the world. The second sort of properties corresponds to our conceptual resources for classifying objects into kinds with distinctive causal features. These kinds are not constant but can change as "our classification of physical objects . . . becomes more complex and sophisticated."[13] An essential feature of scientific inquiry is its *revision* of the causal concepts of the observation framework in order to arrive at maximally accurate empirical generalizations. In many cases, these revisions take place entirely within the observation framework. In such cases, the causal properties are always built out of concepts expressing the occurrent properties of physical objects. We can imagine that science never required any conceptual revision other than this sort. In that case, the observation framework would be conceptually autonomous; that is, its conceptual resources would suffice for formulating and justifying entirely accurate empirical generalizations about singular observable facts. If this were the case, then the framework of postulational science would be "in principle otiose,"[14] and what Sellars calls the "manifest image" would provide a correct ontology for the world. But, as we have learned from the development of science, the observation framework is not autonomous. We cannot do the job of science using only its conceptual resources. Rather, we can arrive at justified empirical generalizations in the observation framework only be appealing to theories that employ concepts that cannot be built out of the conceptual resources of the observation framework.

CE: You seem to be missing the point of my antirealism. I'm willing to admit everything you've said about the *indispensability* of theories. But why should we also have to accept the *truth* of theories? As I see it, the highest virtue we need attribute to a theory needed for the successful practice of science

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is empirical adequacy. In other words, we need only agree that all of a successful theory's *observable consequences* are true. If we regard a theory as empirically adequate—and of course we are entitled to so regard highly successful theories—then from that alone we have sufficient justification for accepting the empirical generalizations that theory entails. The further assertion of the theory's truth is a gratuitious addition, entirely unnecessary for the fulfillment of science's fundamental aim; namely, an exact account of observable phenomena. The enterprise of science can be entirely successful without ever accepting the truth of its theories. Consider two scientists. One accepts atomic theory in the sense that he thinks it is empirically adequate: he knows that it fits all observations to date and expects that it will continue to fit all further observations. Accordingly, he thinks in terms of atomic theory and uses its conceptual resources to solve relevant scientific problems. However, he remains agnostic on the question of whether atoms really exist. A second scientist shares the first's views about the empirical adequacy of atomic theory, but he also holds that atoms really do exist. But what sort of work is done by this latter belief? It makes absolutely no difference for what the second scientist expects to observe or for how he proceeds in his scientific work. His expectations and procedures are exactly the same as those of his agnostic colleague. So, while I agree with you that theories are not "otiose in principle," I do maintain that a realistic interpretation of theories is. There is nothing in the aims of scientific inquiry that is in the least affected by the acceptance or the rejection of the existence of theoretical entities.

SR: It seems to me that it's you who are missing the point of my realism. First of all, you misrepresent my view by taking it as a thesis about the aims of science. I am entirely content with the view that science aims only at empirical adequacy. Indeed, I agree that this aim might have (in some other possible world) been attained without the postulation of theoretical entities. My thesis is rather that empirical adequacy in fact requires theories that postulate unobservable entities and that this fact provides good reason for thinking such entities exist. My realism is not a thesis about the aim of science but rather a thesis about the philosophical (specifically, metaphysical) significance of the means that scientists have had to use in fulfilling this aim.

CE: I'm happy to accept this clarification of your views, but it does not affect the central point: that you haven't offered an argument from the indispensability of theories to the existence of the entities they postulate. Even Sellars, who develops the indispensability thesis much more cogently than you do, seems to ignore the need for such an argument. He seems just to assume that, once theories have been shown to be indispensable, the reality of the entities they postulate has been established. But in fact there is a gap that needs bridging. Sellars himself has emphasized this very point in parallel

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contexts. For example, he agrees that semantic concepts such as *meaning* and moral concepts such as *person* are indispensable, but nonetheless insists this doesn't entail that meanings and persons exist. Similarly, I think you should admit that the indispensability of theoretical language does not entail the existence of theoretical entities. Further, I submit that the only way of bridging the gap between indispensability and existence is by an act of faith that may be permissible for those who want to believe in atoms and similar things but is in no way required by the evidence.

SR: I agree that the gap needs to be bridged and I even agree that it cannot be bridged by a deductive argument. I admit that indispensability does not entail existence; theories could be indispensable and yet theoretical entities not exist. But I insist that there is a good *inductive* case for realism; reasons that make it highly probable that theoretical entities exist.

CE: I take it then that you're about to follow Putnam in presenting realism as a quasi-scientific hypothesis that is the best explanation of the success of science. I've already suggested my criticism of this sort of approach.

SR: On the contrary. I'm not entirely happy myself with this sort of "empirical" approach. Besides the difficulties you raised earlier about possible alternative hypotheses, it seems to me that realism lacks the fruitfulness we require of a good scientific hypothesis. From a purely scientific viewpoint, it looks a lot like an *ad hoc* explanation of one fact with no other explanatory significance. I rather see realism as a *philosophical* thesis based on an analysis of the nature of theoretical explanation in science. I have in mind the following strategy of argument: First find a generally valid type of argumentation from the explanatory power of a hypothesis to the reality of the entities the hypothesis refers to; then show that, in some specific cases, the results of theoretical science enable us to construct an argument of just this type for the existence of theoretical entities. Such a case is inductive because the argument type employed is inductive. But it's philosophical rather than scientific because it is not postulating an explanatory hypothesis but rather pointing out the essential similarity of two ways of arguing.

CE: I need to hear the type of argumentation you have in mind.

SR: The point is really quite simple. There's a standard way of arguing—in both everyday and scientific contexts—for the existence of unobserved entities. The mode of argument is this: from the ability of a hypothesis to (a) subsume all known facts and to (b) predict new and even unexpected facts, we infer the reality of the entities the hypothesis postulates. There's no doubt that we all accept this mode of argument in many cases that involve unobserved though observable entities. For example, this is just the way we proceed in arguing for the past existence of dinosaurs or for the present existence of stars, conceived as huge, tremendously hot, gaseous masses far dis-

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tant from us. But the very same mode of argumentation used in these non-controversial cases can be employed to argue for the existence of the unobservable entities postulated by microphysics. Just as we accepted the existence of dinosaurs because the hypothesis of their existence subsumed the known facts and successfully predicted new ones, so too we ought to accept the existence of electrons, neutrinos, etc. for precisely the same sort of reasons. The case for the existence of electrons and neutrinos is logically identical to the case for the existence of dinosaurs and stars. Since you can hardly reject the latter, I submit that you cannot consistently reject the former.

CE: You yourself have mentioned but ignored the crucial point: the non-observability of the entities postulated by microphysics in contrast to the observability of stars and dinosaurs. This difference undermines your claim that the mode of argument for the two sorts of entities is the same. As I see it, the mode of inference at work in the examples you've given is not from a theory's explanatory power to its truth but from its explanatory power to its empirical adequacy. At any rate, the uncontroversial uses of the mode of argumentation—to the existence of stars and dinosaurs—cannot decide between the realist and the antirealist interpretations of it. For these are cases of inference to *observable* entities; and, for such cases, the claim that a postulation is empirically adequate is equivalent to the claim that it is true. For example, 'Stars (as described by modern astronomy) exist' is equivalent to 'All observable phenomena are as if stars exist', since the existence of stars is itself (in principle) observable. So I can accept the inference to stars and reject the inference to electrons, etc. on the grounds that, in both cases, we are only entitled to infer the empirical adequacy of a hypothesis from its explanatory and predictive success. In the case of stars, this is equivalent to inferring their existence; but in the case of electrons, which are not even in principle observable, it is not.

SR: This response keeps your position consistent but at the price of arbitrariness. By maintaining that explanatory and predictive success supports only the empirical adequacy of a theory, you are implicitly committing yourself to a sharp epistemic distinction between the observable and the unobservable. You say the explanatory success of a hypothesis is evidence of its truth only if the hypothesis is about observable entities. But why should observability matter in this context?

CE: The answer depends, of course, on what you mean by 'observable'. As we noted above, in one sense everything is observable: there might be some creature with sense organs appropriate for perceiving it. But, as we agreed when discussing this point, here a more restricted sense of 'observable' is appropriate. Specifically, observability must be taken as a function of certain empirical limitations of human beings. "The human organism is, from the

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point of view of physics, a certain kind of measuring apparatus. As such it has certain inherent limitations—which will be described in detail in the final physics and biology. It is these limitations to which the 'able' in 'observable' refers—our limitations, *qua* human beings."[15]

SR: I agree with this construal of 'observable'. But my question is, What does observability in this sense have to do with the existence or nonexistence of an entity? You're surely not so much of a positivist as to deny the possibility of the existence of what's unobservable?

CE: You're misconstruing my point. Of course observability in the sense we're taking it has nothing to do with the existence or the nonexistence of an entity. But it has a great deal to do with what we have reason to believe exists. "The question is . . . how much we shall believe when we accept a scientific theory. What is the proper form of acceptance: belief that the theory, as a whole, is true; or something else? To this question, what is observable by us seems eminently relevant." And my answer to the question is this: "to accept a theory is (for us) to believe that it is empirically adequate—that what the theory says *about what is observable* (by us) is true."[16]

SR: Since you refer to what is observable "by us," I take it you make observability relative to an epistemic community, not to individuals?

CE: Of course. The dimmer component of the double star in the Big Dipper's handle is observable because some sharp-eyed people can see it, even if most of us can't.

SR: But then I have a problem. You surely must admit the possibility that our epistemic community might be enlarged, say by the inclusion of animals or extraterrestrials capable of observing things that we now can't observe. For example, we might encounter space travellers who, when we tell them our theories about electrons, say, "Of course, we see them all the time." If this happened, your principles would require that we then, for the first time, accept the existence of electrons. But this seems absurd. Why should the testimony of these aliens be decisive when the overwhelming evidence of our science was not? More generally, isn't it absurd to say that, just because our epistemic community has been enlarged in this way, our beliefs about what there is should change?

CE: Not at all. Your objection has weight only if we believe that "our epistemic policies should give the same results independent of our beliefs about the range of evidence accessible to us."[17] But I see absolutely no reason to believe this. On the contrary, it seems to me that to deny that what evidence is accessible is relevant to what we should believe is to open the door to scepticism or irrationalism.

SR: It seems to me you're equivocating on the expression "evidence accessible to us." Of course such evidence is relevant to our beliefs if it means "the

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evidence that we are in fact aware of." But this isn't what you mean here. Rather, you're saying that our beliefs ought to depend on the range of evidence that we *might* have even if we don't. Specifically, you're saying that believing that an entity exists ought to depend on whether or not there *could be* direct observations of its existence. Of course, actual observations of an entity are relevant to belief in its existence. And it's also true that, since evidence of actual observations of unobservable entities is not available, it's often harder to produce an adequate case for the existence of such things. But what reason could you have for thinking that the mere question of whether or not an entity could in principle be observed by us is decisive for the question of whether we ought to believe that it exists?

CE: What reason do you have for thinking this isn't the case?

SR: Well, consider an example. Suppose astronomical theory postulates the existence of a far distant star that has not been observed but which we have every reason to think we could observe if we were close enough. It might be, for example, that the star has been postulated as the much smaller double of a known star to explain certain anomalies in its motion. I suppose that if the evidence supporting this postulation is strong enough, you will agree that we have good reason to believe in the existence of this star.

CE: Of course, since it is in principle observable.

SR: All right. But suppose further that, entirely independent of astronomical investigations, physiological studies subsequently show that there are previously unknown limits on human powers of observation that make the postulated star unobservable. We may, for example, have assumed that the star was observable because it emitted light in the visible spectrum and so could be seen if we got close enough to it. But physiologists might discover that the visible spectrum is not continuous, that there are small "holes" of invisibility corresponding to specific wave lengths, one of which is that emitted by the star. On your principles, such a physiological result would require us to abandon our conclusion that the star exists, even though all the empirical evidence that led us to postulate it remains the same. But surely such a move would be unreasonable; whether or not the star is observable does not alter the evidence in favor of its existence.

Notice that I'm not saying that observations we in fact have made are not relevant to our beliefs about what exists. But the mere fact that something is observable does not give us any reason to think that it ever has or will in fact be observed. The issue between us is whether mere observability—as distinct from actual observation—is relevant to our beliefs about what exists. I submit that it is not.

Another difficulty for your view derives from the fact that an observable entity may have unobservable properties. The sun, for example, is observable

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but the temperature of its interior is not. What then is your attitude toward the claim that the temperature at the sun's center is about 20 million degrees Centigrade? It would be odd not to accept it: After all, the claim is very well supported by a calculation based on observed facts (the average temperature of the earth, etc.) If these observed facts were appropriately different, the calculation would yield a temperature of the center of the sun that is observable (e.g., about 10 degrees Centigrade). Since you would accept the truth of the result of the calculation in the latter case, it's hard to see how you could coherently not accept its truth in the former case. But, if you accept the claim that the temperature of the sun's center is about 20 million degrees, then you've implicitly given up your principle that observability is relevant to the justification of existence claims.

CE: Not necessarily. The principle might distinguish between unobservable entities and unobservable properties of observable entities.

SR: Possibly. But then we'd need an explanation of why such a distinction is epistemically relevant.

CE: Of course, but you can see where this would lead. To respond to this objection—and your other one about the star that turns out to be unobservable—in a convincing way would require a very elaborate excursion into the theory of knowledge. "But we cannot settle the major questions of epistemology *en passant* in philosophy of science."[18] I'll just acknowledge the relevance of your objections but maintain that more careful epistemological analysis would disarm them. Furthermore, even if I can't answer your objections and your argument stands, remember that the argument is only inductive. This means that, even if I can't directly refute it, I might be able to blunt its force by pointing to overriding considerations that make realism implausible. There is, for example, the fact that, for any theory whose ontology you propose to accept, we can always formulate another theory with a different ontology that is just as well supported by the evidence as the theory you favor. Also, there's the strong historical evidence that scientific postulations are not converging to any single picture of what the unobservable world is like. Until you've dealt with these historical and logical objections to realism, you can't be content with your case for realism.

SR: I agree that the issue isn't fully settled, but your remarks strike me as a strategic retreat that, at least for the present, leaves me in control of the battlefield.

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NOTES

1. Wilfrid Sellars, *Science, Perception, and Reality* (New York: Humanities, 1963), p. 91.
2. Cf. Bas van Fraassen, *The Scientific Image* (Oxford: The Clarenden Press, 1980), p. 12.
3. Sellars, *Science, Perception, and Reality*, cited in n1, above, pp. 121–22.
4. Ibid., p. 122.
5. Bas van Fraassen, "Wilfrid Sellars and Scientific Realism," *Dialogue* 14 (1975): 611.
6. Wilfrid Sellars, "Is Scientific Realism Tenable?," *PSA 1976*, ed. F. Suppe and P. Asquith (East Lansing, MI: Philosophy of Science Association, 1977), p. 315.
7. Ibid.
8. Ibid., p. 319.
9. Bas van Fraassen, "On the Radical Incompleteness of the Manifest Image," 2PSA 1976, ed. F. Suppe and P. Asquith (East Lansing MI: Philosophy of Science Association, 1977), p. 325.
10. van Fraassen, *The Scientific Image*, p. 39, cited in n2, above.
11. Ibid., p. 40.
12. Sellars, "Is Scientific Realism Tenable?," cited in n6, above, p. 316.
13. Ibid., p. 318.
14. Sellars, *Science, Perception, and Reality*, p. 118, cited in n1, above.
15. van Fraassen, *The Scientific Image*, p. 17.
16. Ibid., p. 18.
17. Ibid.
18. Ibid., p. 19.

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