CHAPTER SIX

A Theory's Predictive Success does not Warrant Belief in the Unobservable Entities it Postulates

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6.1 Introduction

One problem facing the epistemology of science concerns the fact that many scientific theories consist of claims about unobservable entities such as viruses, radio waves, and quarks. If these entities are unobservable, how are we to know that the theories that make reference to them are true? The question we ask is "Does the success of a scientific theory ever give us reason to believe in the unobservable entities it postulates?" The existence of the unobservable entities postulated by a theory is, of course, entailed by the truth of that theory. If a theory is true, then the entities it postulates exist (if they didn't, the theory wouldn't be true). Accordingly, where we talk of the truth of a theory, we mean that to include the existence of the unobservable entities it postulates, as will be noted.

The fact is that scientists *do* hold theories about unobservable entities, and for the most part, they hold them to be *true*. At the risk of overgeneralization, then, we might say that most practicing scientists are realists, where realism is the thesis that scientific theories are true (with all that that entails). It must be said that this view is a particularly strong (and therefore fragile) form of realism. This chapter aims more ambitiously to unseat the argument from scientific success for the much weaker realist thesis that there are nomologically possible circumstances (circumstances which are possible given the laws of physics in this world) wherein we would be justified in believing in the unobservable entities postulated by a theory. Henceforth, we use the term "realism" to refer to this view – which Leplin has called "minimal epistemic realism" – that it is logically and nomologically possible to attain a state that warrants belief in a theory. It is worthy of note that there may be *other* good reasons for being a (minimal epistemic) realist – the aim of the present chapter is to show that the success of science isn't one of them: the *success of science* does not warrant even the weakest realist thesis.

We should start by saying what we mean by the "success of science." Let us say that by this phrase, we mean that our scientific theories allow us to make significantly more correct predictions than we could without them. A less circumscribed, but nonetheless consistent, version is Laudan's broadly pragmatic definition:

> A theory is successful provided it makes substantially more correct predictions, that it leads to efficacious interventions in the natural order, or that it passes a suitable battery of tests. (1984, p. 109)

Nothing substantial in our argument will hinge on the difference between these two formulations.

The argument for realism from the success of science is often referred to as the "no miracles" argument, following Putnam's (1979) contentious formulation. In his view, "[t]he positive argument for realism is that it is the only philosophy that doesn't make the success of science a miracle" (p. 73). In order to block Putnam's conclusion, then, we need to show that there is a coherent explanation for science's success, which neither requires nor entails realism. This is to say that even if we grant that realism does explain the success of science, Putnam's claim can be undermined by showing that there are comparably good anti-realist explanations.

6.2 Anti-Realist Explanations for the Success of Science

Leplin (1987, 1997) has conducted a thorough investigation of this topic and concludes that there are no viable anti-realist arguments for the success of science. He considers two categories of putative anti-realist explanations for the success of science: those that allude to evolutionary-type mechanisms of theory selection, and those that regard a theory's empirical adequacy (i.e., the extent to which the theory correctly describes observations and experimental results) to be an explanation of its success. According to Leplin, both strategies fail, so the only remaining explanation of scientific success is the truth of theories. His critique of the evolutionary alternatives is persuasive – more so than his critique of the empirical adequacy alternative – so let us consider them in that order.

The archetypal evolutionary explanation for the success of science can be traced to van Fraassen (1980), according to whom the success of our theories is due to their having been chosen by a mechanism of Darwinian selection:

> Species which did not cope with their enemies no longer exist. That is why there are only ones who do. In just the same way, I claim that the success of current scientific theories is no miracle. It is not even surprising to the scientific (Darwinist) mind. For any scientific theory is born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive – the ones which *in fact* latched on to actual regularities in nature. (p. 39)

This thesis often goes by the name of "evolutionary epistemology," and amounts to the view that theories are selected by the scientific community for predictive success

and that this practice suffices to account for the success. It has to be defended against the charge that the competition among theories fails to satisfy some of the conditions necessary for a process of Darwinian selection to take place. Leplin's (1997) analysis, which we now summarize, shows that van Fraassen's account fails to provide an explanatory alternative to theoretical truth.

Leplin draws an analogy with the tennis-playing abilities of Wimbledon finalists: How do we explain the fact that Wimbledon finalists are so good at tennis? The question admits of two different interpretations. If we want to know why it is that Wimbledon finalists are in general good tennis players, it is appropriate to cite the stringency of the selection procedures for entry into the tournament. This answer, however, does not tell us why particular individuals – Sampras and Agassi, for example – are good at tennis. To answer *this* question, we would have to cite the relevant properties of the individuals, such as their training and genetic endowment. To make the analogy explicit, the problem with evolutionary epistemology is that the question it answers is not the same as the question answered by theoretical truth. The Darwinian account explains how we come to be in *possession* of successful theories, but it doesn't explain what it is about *these* theories, which we happen to possess, that makes them successful. Theoretical truth does not explain how we come to possess these particular theories. But it does answer the second type of question with regard to predictive success: it is an attribute of *these particular theories* that accounts for that success. We must conclude that evolutionary epistemology and theoretical truth are not explanatory rivals – van Fraassen's account does not give us an anti-realist explanation for the success of science and the "no miracles" argument remains unscathed.

Leplin notes that the same can be said for Laudan's (1984) elaboration of van Fraassen's Darwinian account, which specifies the mechanism by which theories are selected. According to Laudan, success is to be explained by the fact that scientists use appropriate experimental controls and methodologies in testing their theories. Indeed, the fact that our theories have been tested by such-and-such a methodology may explain why we possess successful theories, but it does not explain why quantum mechanics and not some other theory has survived all those tests. The hypothesis that quantum mechanics is *true*, by contrast, provides at least *an* explanation for its success.

Before we turn to his critique of the empirical adequacy alternative (i.e., the part with which we disagree), let us briefly set a few ground rules for the discussion. We will suppose, for now, that no other potential problems with the "no miracles" argument need to be worried about. For the sake of argument, let us grant the realist an awful lot, and assume that science *is* successful, that the realist argument from the success of science is not question-begging or invalid, that the notion of (mere) approximate truth is both meaningful and measurable, and that approximate truth *does* explain the success of science. Given these assumptions, it will make no difference whether we talk of a theory's truth or its approximate truth. We can, therefore, play fast and loose with the distinction as well as with the distinction between empirical adequacy and approximate empirical adequacy. For the sake of expository convenience, we speak of the truth of theories where we mean their truth or approximate truth, and of empirical adequacy where we mean empirical adequacy or approximate

empirical adequacy. Provisionally, let us also define "T is (approximately) empirically adequate" as "T is (approximately) empirically equivalent to a true theory." (Note that this definition of "empirical adequacy" differs from the conventional one whereby a theory is empirically adequate if it gives a correct description of observations and experimental results.)

Leplin (1987) defines *surrealism* as the thesis that the explanation for the success of our theories is that they are empirically adequate in the sense just defined – that is, our theories are empirically equivalent to true theories. Surrealism holds that the world behaves *as if* our theories of the world were true, but it "makes no commitment as to the actual deep structure of the world." Surrealism "allows that the world has a deep structure," which is to say that it allows that there *is* a true theory of the world whose theoretical terms denote real entities, but it "declines to represent" these structures (Leplin, 1987, p. 520). Leplin attributes the development of this view to Fine (1986) but, as Leplin rightly notes, Fine does not actually endorse surrealism, he simply cites its coherence to discredit Putnam's "no miracles" argument.

Leplin presents two arguments designed to show that surrealism collapses into realism. The first is that the surrealist explanation for the success of science *presupposes* the realist explanation:

> To suppose that Surrealism explains success is to suppose that for the world to behave as if the theory is true *is* for the theory's predictions to be borne out. But this is to suppose that theoretical truth is normally manifested at the observational level, and thus to *pre*suppose the original, realist explanation of success rather than to provide a weaker alternative. (1987, p. 523)

If this is right, then surrealism does not qualify as an *alternative* to the realist explanation. But this claim merely equivocates between two senses of "explanation." Let us explain.

We need to disambiguate the phrase "A explains B," and do so without becoming entangled in the various philosophical questions concerning explanation that are peripheral to this chapter. To say that A is *an* explanation for B is to make a claim which is consistent with A being false and the possibility of there being some C, incompatible with A, which is also an explanation for B. To say that A is *the* explanation for B is clearly to imply that A is true, and that the truth of A is the reason why B is true. "A explains B" is ambiguous between these two options.

Returning to the issue at hand, to suppose that the empirical adequacy of a theory T explains its success (in either sense of "explains") is indeed to presuppose that T's truth must also qualify as *an* explanation of its success. But it is not to presuppose that T's truth is *the* (i.e., the correct) explanation of its success, or even that T is true at all. Both realists and anti-realists acknowledge that scientists frequently make use of theories that are known to be false in predicting observations. Scientists sometimes represent gas molecules as minute billiard balls. The explanation for why these patently false representations succeed is presumably surrealistic: they are approximately empirically equivalent to other theories about gases that are true. If we accepted Leplin's claim about surrealism's presupposition of realism, then we would have to conclude that gases *are* made up of tiny billiard balls.

One might argue that, whilst empirical adequacy does not presuppose truth *simpliciter*, it does presuppose approximate truth: the success of the billiard ball theory of gases does not entail that gas molecules are billiard balls, but it does entail that gas molecules are relevantly *like* billiard balls. Most realists would be content with the verdict that our best theories are approximately true, so the surrealist explanation of the success of science may presuppose the realist explanation after all. The temptation to think that approximate empirical adequacy entails approximate truth stems from the following considerations. For a theory to be approximately true means, let's say, that it gets most or many of its claims right, even though it may also get some things wrong. Similarly, for a theory to be approximately empirically adequate, it must get a lot of its *observational* claims right, even though it may also get some of them wrong. But to get a lot of observational claims right is to get a lot of claims right, *tout court*. Therefore, approximate empirical adequacy entails approximate truth.

The problem for realists, though, is that this notion of approximate truth has no connection with scientific realism. If getting *any* sizeable part of the story right is enough to count as approximate truth, then it is possible for an approximately true theory to get the *theoretical* part of the story *entirely* wrong despite getting the observational part right. Getting a lot right is certainly necessary for approximate truth, but if approximate truth is to have any bearing on the issue of realism, it should also be stipulated that approximately true theories get at least some things about *theoretical* entities right, not just observational statements. There is no need here to specify just what part or how much of the theoretical story an "approximately true" theory has to get right. But however slight that theoretical requirement may be, the result is a notion of approximate truth that is no longer presupposed by approximate empirical adequacy. The surrealist explanation does not presuppose the realist explanation because getting an arbitrarily large part of the observational story correct is logically compatible with getting the theoretical story totally wrong.

Let's call the hypothesis that every theory has empirically equivalent rivals EE. Leplin's second argument is that surrealism threatens to collapse into realism unless EE is true. For suppose that we have a successful theory T which has no empirically equivalent rival. Then the surrealist explanation for T's success – namely that T is empirically equivalent to a true theory – entails that T itself is true. This is to say that the surrealist explanation entails the realist explanation. To maintain its distinctiveness, surrealism must be *committed* to the truth of EE. Thus Leplin regards it as unproved that surrealism provides a genuine explanatory alternative, since it is an open question whether EE is true. The first response to make is that it is *not* an open question as to the universal availability of empirically equivalent rivals. Kukla (1998) argues for several algorithms for transforming any theory into an empirically equivalent rival theory (see chapter 5 in particular). We shan't insist on that point here – rather, let us concede, for the sake of argument, that the status of EE is uncertain, and hence that the distinctiveness of the surrealist explanation is also uncertain. We can nonetheless weaken surrealism in such a manner as to block the derivation of realism from the negation of EE whilst still leaving an available explanation for the success of science.

Let us here revert to the more traditional definition of empirical adequacy: T is empirically adequate if all of its empirical consequences are true. As before, the empir-

ical adequacy of T entails that the observable world behaves as if the "deep structures" postulated by T do exist, but it is no longer required that the observable phenomena be consequences of any *real* "deep structures." This is to say that, in virtue of the fact that it drops the requirement that T's empirical consequences also be the consequences of a true theory, this notion is logically weaker than the previous one. We might give the name *weak surrealism* to the thesis that our best theories are empirically adequate in the new diminished sense, and *strong* surrealism to the thesis that our best theories are empirically equivalent to true theories.

Now consider the hypothesis that weak surrealism explains the success of science. In fact, such a hypothesis is immune to the second difficulty that Leplin makes for the strong surrealist explanation. Leplin claims, however, that such an escape from the collapse of strong surrealism merely lands us in another dilemma:

> The statement that things happen as they would happen were our theories true invites two directions of analysis. On one analysis, Surrealism simply tells us what goes on at the observational level. Then it is not simply metaphysically weaker than realism; it is not metaphysical at all. But neither is it explanatory. Rather, it restates the explanandum. (1987, p. 522)

The "direction of analysis" alluded to in this passage is the direction of weak surrealism. The other direction is strong surrealism. According to Leplin, in their choice between strong and weak surrealism, anti-realists are caught between the Scylla of realism (if they choose strong surrealism, their explanation threatens to be indistinguishable from that of the realists) and the Charybdis of vacuity (if they opt for weak surrealism, then their explanatory hypothesis amounts to no more than a recapitulation of the explanandum).

But there is a position between strong surrealism and the explanation that Leplin regards as vacuous, and so it seems his argument conflates three putative explanations of the success if science into two. Let's call T* the set of all empirical consequences of T. Weak surrealism is the view that T* is true. But one can hold that T* is true in two different ways. On the one hand, one may believe that the statement "The observable world behaves as if T is true" is *itself* a nomological truth about the world – a law of nature and part of T just like any other law-like statement about the world contained in T. On the other hand, one may believe in the truth of a set, X, of law-like empirical generalizations, along with the (nonnomological) hypothesis that, *as it happens*, X is coextensive with T*, perhaps by accident. In the latter case, one believes two things: (1) for each statement in X, that statement is a true law-like generalization; and (2) these generalizations exhaust T*. The conjunction of (1) and (2) amounts to saying that one does *not* believe in a further *law* that X is coextensive with T*; rather, one accepts this coextensiveness as a contingent accident.

The difference between the two can be brought out by considering what happens if some previously unconsidered empirical hypothesis E is found to be a consequence of T. If you believe that it is a law of nature that the world behaves as if T is true, then you will add E to your stock of beliefs. But if you think that the coextensiveness of T* and your empirical beliefs is merely accidental, then the same discovery

will leave the status of E open; its derivation from T will not be a rationally compelling reason for its adoption into T*. Indeed, the discovery that E is a consequence of T might be a good reason for giving up the belief that the true empirical generalizations are coextensive with T*. We can, therefore, reserve the name "weak surrealism" for the first view, and call the second view *fragmentalism*. Fragmentalism is logically weaker than weak surrealism.

We thus have a series of progressively weakened views about theories: (1) realism (our theories are true); (2) strong surrealism (our theories are empirically equivalent to true theories); (3) weak surrealism (the observable world behaves as if our theories were true); and (4) fragmentalism (the true empirical generalizations happen to be coextensive with the empirical consequences of our theories). The further down the list we go, the less danger there is of a collapse into realism, but also the fewer resources we have for explaining the success of science. Leplin allows that strong surrealism explains the success of science but notes the danger of its collapse into realism. On the other hand, a fragmentalist account of success falls prey to the charge of vacuity, since according to that view, the ultimate truth about the world – or as much of it as we can ascertain – is given by a collection of empirical generalizations, and the truth of our empirical generalizations is exactly the explanandum in the success of science argument. But Leplin doesn't distinguish vacuous (4) from (3) – and (3) seems to avoid both Scylla and Charybdis. According to weak surrealism, the truth of our empirical generalizations is explained by the fact that (i) these generalizations are consequences of T and (ii) the observable world behaves as if T were true. Since this claim goes beyond what is contained in the explanandum, weak surrealism avoids the charge of vacuity. But there is no danger of it collapsing into realism, even if it turns out that some theories have no empirically equivalent rivals. Thus, weak surrealism is an anti-realist rival to the realist explanation for the success of science.

Of course, the existence of coherent and nonvacuous rivals to realism does not show that these explanations are *as good* as theoretical truth. The realist may abandon Putnam's own formulation of the "no miracles" argument and weaken her thesis to the claim that the realist explanation is *better* than the anti-realist explanation. The realist intuition is that the weak surrealist claim that the empirical adequacy of T is a fundamental fact about the world is unintelligible. We have no idea what could keep all the disparate empirical consequences of T in line, if not the causal mechanisms postulated by T itself, or at least, as strong surrealism asserts, the mechanisms posited by some other theory that is empirically equivalent to T: simply saying that they *are* kept in line doesn't explain anything. But what is the epistemic import of intelligibility?

It used to be thought that teleological explanations gave us understanding, but now we want to know the mechanism behind an appearance of teleology. Explanations that posit action at a distance have been considered intelligible and unintelligible by successive generations. The point is that the notion of "intelligibility" is at least relative if not vague. It would seem that the realists have in mind an archetypal explanation as one that satisfies our explanatory hunger, likely mechanistic in flavor. But it doesn't seem that the "intelligibility" of this kind of explanation signifies anything more than a psychological willingness to stop inquiry at that point. It would

be, of course, premature to claim that there *cannot* be an epistemically relevant notion of intelligibility that brands weak surrealism as unintelligible, but at present there does not seem to be such a notion formulated. In fact, the only relevant discussion, to our knowledge, concludes that the intelligibility of scientific theories is a matter of "psychological acclimation" (Cushing, 1991, p. 346).

Does this critique break one of the ground rules? Recall that we granted the validity of the "no miracles" argument, and then went on to argue that weak surrealism is a coherent, nonvacuous, and distinct explanation of the success of science. If we are right, then realists need to show that theirs is a *better* explanation than that of the weak surrealists. They do not have to show that explanatory goodness has epistemic import, since that much was assumed when the validity of the "no miracles" argument was granted – if explanatory goodness were *not* epistemically relevant, then the conclusion of the "no miracles" argument wouldn't follow from its premises. It is precisely this principle that we call into question, but to grant that explanatory goodness is epistemically relevant is not to give the realists license to use any features of explanations they like in making a comparative evaluation. It would be cheating, for example, for the realist to claim that their theory is superior on the grounds that the positing of theoretical entities is an indicator of explanatory goodness.

The problem is that the assumption of the epistemic relevance of explanatory goodness doesn't tell us *which* theoretical properties are indicative of this epistemically relevant explanatory virtue. We can no longer deal with Leplin's problem in isolation from other problems faced by the "no miracles" argument. The realists' task is to find a property, p, of explanations which simultaneously satisfies this pair of criteria: (1) theoretical truth has more p than empirical adequacy; and (2) the possession of p is relevant to beliefworthiness. The realist must at the very least show that there exists some p such that theoretical truth has more of it than empirical adequacy, and that p is not demonstrably *ir*relevant to beliefworthiness. It is easy to satisfy either requirement by itself. On the one hand, theoretical truth is undoubtedly a more intelligible explanation of success than empirical adequacy. But if, indeed, intelligibility is relative, then it cannot play the role that realists need it to. On the other hand, an example of a property of explanations whose epistemic credentials cannot be denied is the *probability that it is true*. But since theoretical truth is logically stronger than empirical adequacy, it cannot be maintained that the realist explanation has more of this property than the surrealist explanation. The trick is to satisfy both requirements at once, and realists have not yet managed to perform this feat.

6.3 Two Realist Arguments from the Success of Science

Having shown that realism is not the *only* explanation for the success of science, it seems appropriate to return to the realist argument from the success of science and examine it in more detail. We might schematize Putnam's (1979) argument, which we hereafter refer to as SS, as the nonparenthetical reading in what follows:

(SS_1) The enterprise of science is (enormously) more successful than could be accounted for by chance.

(SS_2) The only (or best) explanation for this success is the truth (or approximate truth) of scientific theories (and therefore the existence of the unobservable entities postulated by those theories).
(SS3) Therefore, we should be scientific realists.

Notice that, as Putnam originally phrased it, the truth of theories is the *only* available explanation for science's success. This makes his view a particularly fragile version of the argument; our formulation of a single coherent alternative explanation, that of weak surrealism, is enough to block his conclusion. A stronger version of the argument would be to adopt the parenthetical reading of SS_2 and say that we should be scientific realists because the truth (or approximate truth) of theories is the *best* explanation of science's success, thus circumventing the issue of whether or not there is an anti-realist alternative.

The canonical anti-realist objections fall into three groups. First, one can follow the line suggested in section 6.2 and contend that even if truth or approximate truth does explain the success of science, there are equally good (or perhaps even better) anti-realist explanations. Secondly, one might claim that neither truth nor approximate truth is good enough to count as a passable explanation for the success of science. Thirdly, one can allege that even if truth or approximate truth were the only or the best explanation for science's success, the realist conclusion does not follow from the premises without additional (possibly question-begging) assumptions. We will examine the second and third of these claims in turn. But first, we should examine a fourth type of objection, conspicuous in its absence from the anti-realist literature.

It would appear that anti-realists have never tried to deny the truth of SS_1. But it is certainly not unthinkable to deny that science is successful. Indeed, the denial of SS_1 is central to many social constructionist and other epistemically relativist analyses of science. For example, Latour and Woolgar (1979) contend that the apparent success of science is explicable by the fact that scientists *construct* the data that confirm their own theories. Indeed, such a process of self-validation could be said to result in some kind of success – maybe even on Laudan's notion – for a self-validated theory *does* correctly predict the data that will be constructed to confirm it. But self-validating theories do not depend for their success on any of the *intrinsic* properties of the theory; our predictions would be just as successful with any other theory that was equivalently situated within our social milieu. So it is clear that neither realists nor anti-realists have this kind of performance in mind when they think of success for the purposes of SS. In fact, the characterization of success that we proposed in section 6.1 above gives the correct judgment in this case: self-validated theories are not successful because they don't enable us to make more correct predictions than it would be possible to make with some *other* theories which were also appropriately embedded in our social life.[1]

1 Note that this does not commit us to saying that a theory that has "empirically equivalent" rivals (i.e., rival theories that predict all and only the same observations) cannot be successful. It simply means that empirically equivalent theories must be regarded as equally successful. Since self-validation depends on social processes that are that are independent of the *intrinsic* properties of theories, the social constructionist theory must be that *all* theories are equally successful – which amounts to the assertion that the concept of success doesn't apply to theories.

Given the propensity of philosophers for occupying every nook and cranny of logical space, it is an interesting question as to why, despite the conceivability of doing so, anti-realists have never tried to undermine SS with the denial of SS_1. If the thesis of anti-realism *presupposed* the success of science, the absence of this strategy would be understandable. But there is no logical impropriety in endorsing either realism or its negation whilst simultaneously contending that all scientific theories suggested to date – or that will ever be suggested – are empirical failures. SS_1 is not entailed by anti-realism or by realism, or by a disjunction of the two. The supposition that both realists and anti-realists must make is that it is nomologically *possible* for us to have successful theories, for the issue at hand would not even make sense if empirical success were not possible – there is no point arguing about whether empirical success warrants belief in a theory, if there can be no such thing as empirical success. Such success does not have to be *actual* – in principle, the debate about scientific realism could have got started by cave-dwellers, long before the first remotely successful scientific theory was even conceived.

So, since SS_1 is independent of anti-realism, we might ask: Why have anti-realists not attempted to refute the realist argument from the success of science by asserting that science is not successful? Anti-realists may not have to concede that, in fact, there have ever been, or ever will be, successful theories. But they do have to concede that it would be at least *possible* for us to possess a successful theory. So one reason why anti-realists have not tried to deny SS_1 could be that although SS is about actual theories, it can be reformulated as an argument about *possible* theories, whilst maintaining all of its plausibility. And then the realist could claim that it is only the truth of such a theory that could account for its success. This new argument for realism, an argument from the possible success of science, which we call SS′, runs as follows:

(SS_1') Some nomologically accessible theories would be successful if we used them to make predictions.

(SS_2') The only (or best) explanation for this success would be the truth (or approximate truth) of the theories in question (and therefore the existence of the unobservable entities postulated by those theories).

(SS_3') Therefore, there are nomologically possible circumstances – that is, the circumstance of being in possession of a successful theory – wherein we would be justified in believing a theory true.

Note that SS′ is an existential generalization of SS. SS′ is the stronger argument, and the negation of SS_1 would show SS to be unsound, but would not, by itself, adversely affect the status of SS′. This is to say that if SS is sound, then so is SS′, but that to refute SS is not to refute SS′.

It seems likely that relativists such as Latour and Woolgar would want to deny SS_1' as well as SS_1 – and such a strategy would undermine SS′ as well as SS. The truth of SS′, however, is not up for grabs in the realist/anti-realist debate. But the fact that SS_1 is independent of both realism and anti-realism means that both sides are free to avail themselves of any argumentative strategies that involve its negation. One such strategy is deployed in a *realist* defense of SS′ and will be discussed in the next section.

6.4 Truth and Truthlikeness as Explanations for the Success of Science

Recall that we earlier argued that "A explains B" is ambiguous between two options – A's being *an* explanation of B, and A's being *the* explanation of B, where only the latter entails the truth of A. In what follows we will always use "A explains B" to mean (only) that A is *an* explanation for B.

The first anti-realist blow is that neither truth nor truthlikeness can be the explanation for the success of science (we use "truthlikeness" as a synonym for "approximate truth"). Such a view is developed at length by Laudan (1981). According to Laudan, a quick glance at the history of science shows that truth fails because the paradigmatically successful scientific theories of the past are now known not to be true. Truthlikeness fails for two similar reasons. First, approximate truth does not explain the success of science anyway – it is not even *an* explanation of success, let alone *the* explanation. Secondly, many of the successful theories of the past have not only failed to be true, but have also failed even to be approximately true.

As we shall see, the argument against theoretical truth as the explanation for the success of science preempts most of the issues relating to truthlikeness, so very little additional work will be needed to deal with the latter. If successful theories of the past were false, then it follows that either there must be some acceptable explanation for their success other than their truth, or that success sometimes has *no* acceptable explanation. Either way, SS is refuted by the repudiation of SS_2 – success is not a compelling ground for ascribing truth. Following such considerations, realists have mostly abandoned truth as an explanation for success and have taken refuge in approximate truth. But there is a rejoinder available for realists, and such a defense is similar to that used by McAllister (1993).

In fact, McAllister was not addressing the status of truth as an explanans for success. He was trying to defuse Laudan's parallel argument against the claim that *approximate* truth can explain success. Many of the successful theories of the past failed even to be approximately true. Theories of ether and phlogiston, for example, posited ontologies that are now completely rejected – there is nothing remotely like ether or phlogiston in our current world view. So Laudan's conclusion, as in the case of truth *simpliciter*, is that predictive success does not compel us to take our theories to be even approximately true.

There are two realist responses to this argument. First, the realist can insist that, in fact the successful theories of the past *were* in fact approximately true (see, e.g., Hardin and Rosenberg, 1982). This strategy fails because the more liberally we construe approximate truth, the more likely the strategy is to succumb to the charge that the approximate truth of our theories does not warrant a realist attitude towards them. Here, the truth of SS_2 is protected by weakening it to the point at which it ceases to entail SS_3. If phlogistic theories are to be counted as approximately true, then a theory may be approximately true despite the nonexistence of anything remotely like its theoretical entities. So, the strategy of maintaining that the theories of the past were in fact approximately true will not work. Secondly, the realist can admit that the theories cited by Laudan fail to be approximately true, but also deny that they are successful – this is the line taken by McAllister. According to McAllister, such judgments

were made according to criteria for observational success that are now considered inadequate, even though, of course, these theories were *thought* to be successful at the time they were posited. Therefore, the examples given by Laudan do not actually show that theories far from the truth may nevertheless be successful and so do not warrant the anti-realist charge.

Interestingly, the same argument can be used to give another lease on life to truth itself (as opposed to mere truthlikeness) as the explanation for success. The fact that past theories were all false has no bearing on the truth-based version of SS if we deny that they were successful. Turning to our current theories, we might suppose that even the best of those is not successful. The assumption that no successful theory has ever been formulated contradicts premise SS_1. Hence it would undo SS. But, as noted above, it would not affect the status of the more general argument SS′, according to which the very possibility of having a successful theory is to be explained by the possibility of having a true theory. Here, the assumption that science is not successful can be used to reinforce the case for realism: it unseats Laudan's argument against the thesis that truth is the explanation of success, without harming SS′. Laudan's argument about truth doesn't make trouble for a minimal epistemic realist who claims only that it is possible to have a rationally warranted belief in theoretical entities. Laudan's argument does, however, create problems for stronger forms of realism; for example, the view that we are *already* entitled to believe in some theories. This may have been all that Laudan wished to claim; our point is that this is all he *can* claim.

Yet realists may be unwilling to pay what it costs for this argument to protect SS′ from Laudan's confutation. They have to assume that quantum mechanics (amongst other things) is empirically unsuccessful, and whilst this may be a possibility for the general skeptic about the possibility of there ever being a successful theory, as we've noted, anti-realists and realists alike must concede that empirical success is at least a possibility. Why doesn't quantum mechanics fit the bill for success? What more stringent standards for success could one possibly adopt than those which quantum mechanics has already passed? So let us assume that at least some our current theories are successful. If those successful theories are also *false*, then it follows either that there are other explanations of success besides truth, or that the success of science is inexplicable. In either case, the versions of SS and SS′ that refer to the truth of theories must fail.

What kind of a case can be made for the contention that our best current theories are false? The so-called "pessimistic meta-induction" bears on this question: all the theories of the past are now known to be false, so it is likely, by induction, that our present theories will turn out to be false as well. Indeed, the premise of the pessimistic meta-induction does provide us with *some* evidence for the view that our current theories are false but, as is the case with all inductions based on bare enumeration, the force of its conclusion is easy to resist. The fact that our past theories were false, *all by itself*, is a weak basis for projecting that falsehood onto our current theories. Of course, if we are progressing toward an attainable goal of absolute theoretical truth, our interim theories might nevertheless be false until we came very close to the end of this process of progress, but maybe we are close to that end *now*.

In fact, there are background-theoretical considerations that count against the pessimistic meta-induction. Recall that we are considering the realist contention that theoretical truth is the explanation for theoretical success. Laudan's objection is that this argument fails on the ground that the successful theories of the past were *not* true. Against Laudan, realists can claim that these false theories were not really successful either. But it is much more difficult to suppose that our current theories are also unsuccessful if the Laudan argument is run on our current theories as well as our past theories. On the other hand, it is not as obvious that our most successful current theories must be considered false – the only basis for that view is the pessimistic meta-induction. Finally, it has been conceded that reasonable realists have to admit that the best current theories are successful, but they still don't have to admit that the best theories of the past were successful. Moreover, realists are of the opinion that the inference to truth is warranted by success, so the data cited in support of the pessimistic meta-induction are consistent with the realist view. If it is accepted that the best theories of the past were unsuccessful whereas the best theories of the present are successful, then realists will rightly conclude that it is inappropriate to project that falsehood, which was possessed by theories of the past, onto theories of the present. The pessimistic meta-induction carries weight only if it is assumed that success is irrelevant to truth. So this anti-realist argument against the contention that truth is the explanation of success is a failure because the assumption begs the question against realists.

We now turn to the pair of counter-arguments relating to approximate truth. The realist argument here is that the approximate truth of our theories accounts for their success, and that approximate truth entails realism. The first of the two counter-arguments from Laudan is that there are successful theories that fail to be even approximately true. As noted above, anti-realists cannot secure the claim that there are successful theories that fail to be *true*, so the present, stronger claim can only be more problematic.

The second Laudanian counter-argument is that there is no reason to believe that approximate truth entails success. Laudan writes:

> [V]irtually all proponents of epistemic realism take it as unproblematic that if a theory is approximately true, it deductively follows that the theory is a relatively successful predictor and explainer of observable phenomena. (1984, p. 118)

But, of course, such an assumption is problematic, since it is conceivable that an arbitrarily minute error in our characterization of theoretical entities might result in a drastically incorrect account of observable phenomena. The root problem, as Laudan notes, is that realists have given us no clear account of how to assess claims of approximate truth. Until they do, the claim that approximate truth is an explanation of success will remain unwarranted.

Whilst this criticism of SS must be accepted, we should also note, however, that it does not constitute a *refutation* of SS. It does not follow from the vagueness of "approximate truth" that approximate truth does not *explain* success; what follows is that it is *hard to tell* whether approximate truth explains success. The moral that ought

to be drawn is not that SS is unsound but, rather, that this particular issue – whether approximate truth explains success – is not the proper arena for a decisive confrontation over the status of SS.

To recap: the Laudanian counter-arguments are (1) that success does not warrant an inference to truth or truthlikeness because there are successful theories which are neither true nor approximately true, and (2) there is no reason to believe that truthlikeness is even a formally adequate explanation of success. Neither counter-argument is decisive – the first can be defused by insisting on a standard for success so high that all the theories whose truthlikeness is in doubt fail the test. Indeed, if necessary, the standard for success can be raised so high that all extant theories fail the test, rendering Laudan's conjunctive existential claim (that there are theories that are both successful and far from the truth) false. The second counter-argument, whilst sound, leaves the status of SS unsettled. So the fate of SS thus hinges on the results of the other pair of argumentative strategies deployed by anti-realists – namely, the attempt to establish the existence of *anti*-realist explanations of success and the attempt to show that realism doesn't follow from the premise that truth is the best explanation of success. We have established the former in section 6.2. We conclude by turning to the latter.

6.5 The Circularity of the Argument from the Success of Science

Let us recap on the current status of SS. In section 6.2, we noted that Leplin's contention that there is no anti-realist explanation for scientific success (at best) fails to decide the issue. In section 6.4, we argued that Laudan's argument that neither truth nor truthlikeness can explain the success of science was inconclusive. So the anti-realist side in the debate cannot yet be declared the victor. Yet the most powerful anti-realist argument is still to come. The charge, made independently by both Laudan (1981) and Fine (1984), is that SS is circular.

Even if we grant that science is successful and that the truth of our scientific theories is the *only* viable explanation for that success, the conclusion of SS – that we have grounds for believing our theories – does not follow unless we assume additionally that the explanatory virtues of hypotheses are reasons for believing them. This, however, is an assumption that the anti-realist need not accept – indeed, anti-realists generally do not. Van Fraassen (1980), for example, distinguishes between the *epistemic* and *pragmatic* virtues of theories. There are many pragmatic virtues, an example being the property of allowing us to make quick and easy calculations. Suppose that T_1 and T_2 are empirically equivalent, but with T_1 the process of calculating empirical predictions is easier. This is an excellent reason for *using* T_1 rather than T_2 in deriving predictions. But it is not necessarily a reason for thinking that T_1 is closer to the truth. We would surely want to say that ease of calculation is (merely) a pragmatic virtue. Both types of virtue are desirable features of theories, but only the former actually bear on a theory's beliefworthiness.

According to van Fraassen, the only epistemic virtues of theories are the *empirical* virtues of getting more observable consequences right, or fewer of them wrong.

A corollary of this view is that the *explanatory* virtues of theories, since they are other than empirical, can only count as pragmatic virtues. The fact that a theory, T, provides the best (or only) explanation of a set of phenomena has no bearing on its beliefworthiness. So if anti-realists are correct on this score, then SS fails even if both of its premises are true. And whether they are right or wrong, it is question-begging to wield an argument against anti-realists that merely *presumes* that explanatoriness is a reason for belief. This is exactly what is presumed in SS, so the argument from the success of science accomplishes nothing in the realism/anti-realism debate.

Boyd (1984) responds to Laudan and Fine's circularity counter-arguments. He claims that scientists routinely decide what to believe about the observable world on the basis of which hypothesis best explains the data by a process of inference to the best explanation. This is to say that they use "abductive inference" in choosing between empirical hypotheses. But then, Boyd argues, it must be permissible for *philosophers* to use abductive inference to defend a philosophical thesis about science. Sober (1990) counters this move by claiming that the problem with SS is not that its use of abduction is question-begging but, rather, that it is a *very weak* abductive argument. We propose that, in fact, the original charge of circularity can be sustained despite Boyd's objection. His defense belongs to a recurring pattern of realist arguments designed to show internal inconsistency in the anti-realist position. Such arguments note that anti-realists refuse to give epistemic weight to some nonempirical theoretical virtue (such as simplicity or explanatoriness) when these apply to theoretical statements, but that they are willing to use the very same principle when dealing with observational claims. But the evidence does not warrant the charge of inconsistency. It is false that the appeal to nonempirical virtues in assessing the status of observational hypotheses logically commits the anti-realist to applying the same principles to theoretical hypotheses.

Suppose we use some rule, R, for giving epistemic weight to, say, the explanatory virtues of hypotheses: R tells us to give greater credence to hypotheses on the basis of how well and how much they explain. R may or may not specify circumstances under which we should elevate the epistemic status of theoretical hypotheses. In case it does, let R* be the same rule with the specification that it only applies to observational hypotheses. Anti-realists commit no logical fault in subscribing to R*. Yet R* allows for abductive inferences to observational hypotheses whilst blocking abductions to theoretical hypotheses.

It must be conceded that this account of the matter leaves the anti-realist open to the (lesser) charge of arbitrariness. But arbitrariness is not, by itself, a decisive point against a philosophical position. (Indeed, it can be a legitimate part of a philosophical position, as in the arbitrariness of prior probability assignments in personalism.) Thus, a small change in Laudan and Fine's argument insulates it against Boyd's objection. Laudan and Fine claim that the use of abduction is question-begging, since anti-realists deny the validity of abduction. Boyd counters that everyone, including the anti-realist, uses abduction. This may be so for observational hypotheses, but anti-realists can consistently disallow the use of abduction in the service of theoretical hypotheses. Realism itself is a theoretical hypothesis, for it entails that some theoretical entities exist. Thus, those who endorse SS are guilty of begging the question by

engaging in abduction to *theoretical* hypotheses, when such abductions are *precisely* what anti-realists regard as illegitimate.

Our answer to the question that was formulated at the beginning of section 6.1, then, is a resolute "No": the predictive success of a theory does not give us reason to believe in the unobservable entities postulated by that theory. The argument from the success of science fails to do the job for which it was designed.

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