

THE ULTIMATE ARGUMENT FOR SCIENTIFIC REALISM

Realism and relativism stand opposed. This much is apparent if we consider no more than the realist *aim* for science. The aim of science, realists tell us, is to have true theories about the world, where 'true' is understood in the classical correspondence sense. And this seems immediately to presuppose that at least some forms of relativism are mistaken. The truth which realists aim for is absolute or objective, rather than relative to 'conceptual scheme' or 'paradigm' or 'world-view' or anything else. And the world which realists seek the truth about is similarly independent of 'conceptual scheme' or 'paradigm' or 'world-view' or anything else. If realism is correct, then relativism (or some versions of it) is incorrect.

But is realism correct? As it stands, this question is ill-defined because realism itself is ill-defined. Obviously, there is more to scientific realism than a statement about the aim of science. Yet what more there is to it is a matter of some dispute among the realists themselves. Whether or not realism is correct depends crucially upon what we take realism to assert, over and above the minimal claim about the aim of science.

My way into these issues is through what has come to be called the 'Ultimate Argument for Scientific Realism'.¹ The slogan is Hilary Putnam's: "Realism is the only philosophy that does not make the success of science a miracle".² Slogans are not arguments, and the first task is to find out exactly what this Ultimate Argument is. Surprisingly, this is not an easy task. Clarifying the argument will simultaneously clarify what the realism is for which it is an argument. And then we must of course ask whether the argument is a good argument.

As Putnam's slogan already makes clear, the argument appeals to the (alleged) *success* of science. Such appeals are nothing new: Clavius, Kepler and Whewell made them long before Popper, Smart, Putnam or Boyd. The early appeals were meant to show that the realist aim for science had been *achieved*. Thus Clavius argued that the predictive success of Ptolemaic astronomy showed that the theory was true and that its 'theoretical entities' (eccentrics and epicycles) really existed:

But by the assumption of Eccentric and Epicyclic spheres not only are all the appearances already known accounted for, but also future phenomena are predicted, the time of which is altogether unknown. . . . it is incredible that we force the heavens to obey the figments of our own minds, and to move as we will, or in accordance with our principles (but we seem to force them, if the Eccentrics and Epicycles are figments, as our adversaries will have it).³

Clavius was wrong. Eccentrics and epicycles were figments of the Ptolemaic astronomer's imagination. The predictive success of a theory does not entail that it is true or that its theoretical entities really exist. Clavius simply committed the fallacy of affirming the consequent.

Now Clavius was actually aware of this ancient sceptical objection — but he had nothing but hot air to offer against it:

Next if it is not right to conclude from the appearances that eccentrics and epicycles exist in the heavens, because a true conclusion can be drawn from false premises, then the whole of natural philosophy is doomed . . . all the natural principles discovered by philosophers will be destroyed. Since this is absurd, it is wrong to suppose that the force and weight of our argument is weakened by our opponents. It can also be said that the rule that truth follows from falsehood is irrelevant.⁴

Obviously, Clavius tried to prove too much. So did Kepler when he said that a habitual liar will always be found out, and that a lot of predictive success must establish truth. A string of fallacies does not add up to a valid argument.⁵ So did Galileo when he said that the earth must move because postulating that it does explains the tides. So, finally, did Whewell when he said (if he did say it) that predictive success in the form of a 'consilience of inductions' proves truth.

The most that a realist can say is that predictive success yields (inconclusive) evidence for the truth of theory and that such evidence might sometimes make it reasonable to presume that a theory is true and that its theoretical entities really exist. The realist can add, in support of the last point, that it may be reasonable to presume true what subsequently turns out to be false — so that it might have been reasonable for Clavius tentatively to presume that Ptolemaic astronomy was true and the eccentrics and epicycles real.

But realists are not the only philosophers who value predictive success — nor are they the philosophers who value it most. Instrumentalists will say that predictive success gives us (inconclusive) reason to think we have an efficient theoretical instrument of prediction. Van Fraassen's constructive empiricist will say that predictive success gives

us (inconclusive) reason to think we have an empirically adequate theory. Laudan's problem-solver will say that predictive success gives us (inconclusive) reason to think that we have a theory which is a good empirical problem-solver. Anti-realists value predictive success as much, if not more, than realists, and can make similar epistemological use of it. So far, then, we have no argument for scientific realism.

Laudan thinks that this is the end of the matter: modern realists simply commit the fallacy of affirming the consequent all over again. He talks of '*The Realist's ultimate Petitio Principii*' as follows:

It is time to step back a moment from the details of the realists' argument to look at its general strategy. Fundamentally, the realist is utilizing . . . an abductive inference which proceeds from the success of science to the conclusion that science is approximately true, verisimilar, or referential (or any combination of these). . . .

It is little short of remarkable that realists would imagine that their critics would find the argument compelling. . . . ever since antiquity critics of epistemic realism have based their scepticism upon a deep-rooted conviction that the fallacy of affirming the consequent is indeed fallacious.⁶

Quite so. But has Laudan correctly construed the Ultimate Argument as the '*ultimate Petitio Principii*' of affirming the consequent? I think not.⁷

Before I say how the Ultimate Argument is to be construed, there is another point to be made about Clavius's argument. It concerns his view that the predictive success of Ptolemaic astronomy would be 'incredible' if that theory were not true. This can simply be denied. After all, Babylonian astronomers detected periodicities in astronomical phenomena and devised algebraic rules for predicting them. It is hardly incredible or miraculous that a rule expressly devised to capture some periodic phenomenon should successfully predict future instances of that periodic phenomenon. (What might be said to be incredible or miraculous is that eclipses are periodic phenomena, not that we can devise a rule to capture this. Except that miracles are commonly thought of as *violations* of general laws of nature, rather than as the obtaining of those laws!) Nobody thinks that the Babylonian algebraic rules truly describe some hidden reality. Now if Hellenic astronomers (including Ptolemy) devised geometrical models rather than algebraic rules to accomplish the same predictive tasks, it would hardly be incredible that those models successfully predicted future instances of periodic phenomena such as eclipses.

But what if a theory designed to accommodate one phenomenal

regularity (or set of them) should successfully predict a quite different regularity (or set of them)? That *would* be surprising. A conceptual tool designed to do one job turns out to do a quite different job equally well, a 'figment' dreamt up for one purpose turns out to be well-adapted to a different purpose. It would be as if a plane designed for smoothing wood proved capable of remote tuning a TV set! (I owe the last sentence to Homer Le Grand.)

Hence careful realists, beginning with William Whewell, distinguished two kinds of predictive success, predicting known effects and predicting novel effects. Whewell claimed that no theory which had enjoyed novel predictive success had ever subsequently been abandoned. He seems to have thought that novel predictive success provides *conclusive* evidence for the truth of the theory:

No accident could have given rise to such an extraordinary coincidence. No false supposition could, after being adjusted to one class of phenomena, exactly represent a different class, where the agreement was unforeseen and un contemplated.⁸

Again, Whewell's view is too strong. The principle "If a theory has novel predictive success, then it is true" still falls foul of the fallacy of affirming the consequent. A weaker view than Whewell's would be that novel predictive success gives us the best kind of evidence for truth. And a better principle than Whewell's would be: "If a theory has novel predictive success, then it is reasonable to presume (tentatively) that it is true".

All of this depends, of course, on our being able to make good the intuitive distinction between prediction and novel prediction. Several competing accounts of when a prediction is a novel prediction for a theory have been produced. The one I favour, due to Elie Zahar and John Worrall, says that a predicted fact is a novel fact for a theory if it was not used to construct that theory — where a fact is used to construct a theory if it figures in the premises from which that theory was deduced. But this is not the place to elaborate or defend that view.⁹

Popper also draws attention to Whewell's distinction, but makes a quite different point with it:

There is an important distinction . . . between two kinds of scientific prediction, . . . the prediction of *events of a kind which is known* . . . and . . . the prediction of *new kinds of events* . . . It seems to me clear that instrumentalism can account only for the first kind of prediction: if theories are instruments for prediction, then we must assume that their purpose must be determined in advance, as with other instruments. Predictions of the second kind can be fully understood only as discoveries.¹⁰

Here the argument seems to be that 'instrumentalism' cannot *account for* or *explain* novel predictive success, whereas scientific realism can account for or explain this. Novel predictive success is not a premise from which we argue *to* something (truth, presumed truth, or whatever). Rather it is a conclusion, an *explanandum*, of which scientific realism is to be part of the *explanans*.

Before analysing this argument any further, it is worth noting that Duhem, Popper's arch-instrumentalist, had already acknowledged that arguments like Popper's *and* Whewell's had some force. They actually led him to spice his (alleged) instrumentalism with a whiff of realism. Duhem writes:

. . . the consequences that can be drawn from [a theory] are unlimited in number; we can, then, draw some consequences which do not correspond to any of the experimental laws previously known, and which simply represent possible experimental laws. . .

Now, on the occasion when we confront the [novel] predictions of the theory with reality, suppose we have to bet for or against the theory; on which side shall we lay our wager? If the theory is a purely artificial system, . . . if the theory fails to hint at any reflection of the real relations among the invisible realities, we shall think that . . . [we] will fail to confirm a new law. [That we should] would be a marvelous feat of chance. It would be folly for us to risk a bet on this sort of expectation.

If, on the contrary, we recognise in the theory a natural classification, if we feel that its principles express profound and real relations among things, we shall not be surprised to see its consequences anticipating experience and stimulating the discovery of new laws; we shall bet fearlessly in its favour.

The highest test, therefore, of our holding a classification a natural one is to ask it to indicate in advance things which the future alone will reveal. And when the experiment is made and confirms the predictions obtained from the theory, we feel strengthened in our conviction that the relations established by our reason among abstract notions truly correspond to relations among things.¹¹

Here Duhem operates, not with realist notions of truth and falsity, but with the notion that some theories are 'purely artificial systems' and others 'natural classifications'. It is not easy to explain how a 'natural classification' differs from a true theory, especially when we are told that in a natural classification "the relations . . . among abstract notions truly correspond to relations among things". No matter. Let us grant that a theory can be a natural classification without being true, and that Duhem gives us only a whiff of realism rather than realism proper. Still, he seems to be saying two things. First, that the highest test which yields the best evidence that we have a 'natural classification' is a successful test of a novel prediction. Second, that only if we think we have a

'natural classification' will we regard successful novel prediction as anything more than 'a marvelous feat of chance'. It is the second point which bears upon the Ultimate Argument.

According to Whewell, Duhem and Popper, then, what is really surprising or miraculous about science, what really needs explaining, is *novel* predictive success rather than predictive success *simpliciter*. I dwell on the point because it is notable by its absence from recent discussions of the Ultimate Argument, by both defenders of the argument (such as Putnam and Boyd) and by those who attack it (such as Laudan and van Fraassen). This will turn out to be important. But we have yet to get clear what the Ultimate Argument actually is.

Popper said that scientific realism could explain science's novel predictive successes while instrumentalism could not. Putnam, warming to the idea that realism *explains* things, says that it is "an over-arching scientific hypothesis".¹² This is odd. A philosophical view about science is to explain historical facts about science. Realism, as presented so far, is the view that science aims at true theories, that sometimes it is reasonable tentatively to presume that this aim has been achieved, and that the best reason we have to presume this is novel predictive success. Thus characterised, realism explains nothing about the history of science. In particular, realism does not explain why some scientific theories have had novel predictive success.

Perhaps what does the explaining is not the philosophical generalities of scientific realism at all. Perhaps what does the explaining are specific realist conjectures that some scientific theory is true (or nearly so). Perhaps what we have (in the simplest case) are explanations of the following kind:

Theory *T* is true.

Theory *T* yielded several novel predictions.

Therefore, *T*'s novel predictions were also true.

Is this an explanation? Well, its (alleged) *explanandum* certainly follows from its (alleged) *explanans*, as we require. *Pace* Laudan, no fallacy of affirming the consequent is involved. And as in all non-circular explanations, its (alleged) *explanans* transcends its (alleged) *explanandum*. Should the realist proffer an (alleged) *explanans* of this kind? As characterised above, the realist thinks that novel predictive success is the best reason tentatively to presume truth. And now what is claimed

is that the presumed truth of theory *explains* novel predictive success. (Laudan calls these two the realist's 'upward' and 'downward' paths¹³.)

Putnam formulates the realist explanation of science's success roughly as I have done:

... the typical realist argument against idealism is that it makes the success of science a *miracle*. Berkeley needed God just to account for the success of beliefs about tables and chairs (and trees in the Quad) ... And the modern positivist has to leave it without explanation (the realist charges) that "electron calculi" and "space-time calculi" and "DNA calculi" correctly predict observable phenomena if, in reality, there are no electrons, no curved space-time, and no DNA molecules. If there are such things, then a natural explanation of the success of these theories is that they are *partially true accounts* of how they behave. ... But if these objects don't really exist at all, then ... it is a *miracle* that a theory which speaks of curved space-time successfully predicts phenomena ...¹⁴

Here Putnam appeals to partial truth instead of truth, a complication I shall ignore for the moment. He does not emphasise novel predictive success as one should, a complication I shall also ignore. Further, Putnam directs the argument against the Berkeleyan idealist and the positivist, both of whom assert the strong negative thesis that the 'theoretical entities' postulated in science *do not exist*. As a result, Putnam's main point here is that electrons, curved space-time, and DNA molecules *do exist* and that this explains why theories about them are successful.

Yet it is important to see that it is not the mere existence of 'theoretical entities', not the mere fact that 'theoretical terms' have referents, which can explain success. Laudan notes Putnam's emphasis on reference for theoretical terms and attributes the following explanation to him:

A theory whose central terms genuinely refer will be a successful theory.

All the central terms in theories in the mature sciences do refer.

Therefore, the theories in the advanced or mature sciences are successful.¹⁵

The argument is vague: can we locate the *central* terms of a theory in a non-circular way?; can we locate the *mature* sciences in a non-circular way? Laudan does not dwell on this. He accepts that the argument is valid and even that its conclusion is true. But he thinks it a poor explanation because its premises, especially the first, are obviously false. Laudan seeks to show this on both historical and philosophical grounds.

On the historical side, he gives examples of theories whose 'central terms' referred (or so we now think) but which were unsuccessful: chemical atomism in the 18th century, Prout's hypothesis in the 19th century, the theory of continental drift in the early 20th century. (It would be better to speak of '18th century chemical atomism', and so on, to make it clear that we are speaking of *different* theories than later successful ones such as 19th century chemical atomism.) Laudan also gives examples of theories whose 'central terms' did not refer (or so we now think) but which were successful: Ptolemaic astronomy, phlogiston theory, 19th century ether theories.

Now one might quarrel with Laudan's claims about some of these historical examples. How successful was phlogiston theory, for example? Such quarrels would be intensified if emphasis was placed on *novel* predictive success (though neither Putnam nor Laudan give any emphasis to this). How much *novel* predictive success did Ptolemaic astronomy have, for example?

But we need not pursue any of these historical questions. For Laudan has a simple and devastating philosophical argument which divorces successful reference from success. We can easily construct a referring theory which will be unsuccessful: take a successful referring theory, retain its existential claims, and negate its theoretical ones. "Richard Nixon is tall, blonde, honest and never swears" refers to Richard Nixon all right, but it says a lot of false things about him and would be very unsuccessful in predicting Nixon-phenomena. Obviously, in any realist explanation of science's success it is truth or near-truth which is going to be important, rather than mere successful reference.

This does not mean that all the ink spilled over the reference of theoretical terms has been wasted ink. Realists think that theories typically assert the existence of their 'theoretical entities', so that successful reference is typically a *necessary condition* for truth or near-truth. However, it is equally important for realists that reference, while a necessary condition for truth, is not a *sufficient* condition. Realists hold that we know more about, say, electrons than our ancestors did, that while our ancestors had false theories about electrons we have true (or truer) ones. But we can only say this if the false theories of our ancestors referred to electrons just as our own theories do. A theory may be referential yet false.

But if reference is only a necessary but not a sufficient condition for truth, then it is clear that it is hopeless to try to explain why a theory is

successful merely by pointing out that its theoretical terms do successfully refer. If the realist is to explain the success of some scientific theory, it is truth (or near truth) that is needed rather than mere successful reference. Putnam's explanation is of this kind: it is because electrons exist *and* electron-theory gives a true (or partially true) account of them that electron-theory is successful. And the argument is that the Berkeleyan or positivist, who denies the existence of electrons, can give no account of the success of electron-theory.

An immediate worry about the argument is that Putnam has chosen his opponents carefully. Not every anti-realist is a Berkeleyan or a positivist (in Putnam's sense). There are anti-realists who do not *deny* the existence of 'theoretical entities', but who prefer to remain *agnostic* on the matter and fashion their philosophy accordingly. It remains to be seen whether Putnam's argument can be directed against them too.

Before we turn to that question, we can at last be clear about what the Ultimate Argument actually is. It is an example of a so-called *inference to the best explanation*. How, in general, do such inferences work?

The intellectual ancestor of inference to the best explanation is Peirce's *abduction*. Abduction goes something like this:

F is a surprising fact.

If *T* were true, *F* would be a matter of course.

Hence, *T* is true.

The argument is patently invalid: it is the fallacy of affirming the consequent again. One might say (nobody has) that although abduction is *deductively* fallacious, it is actually a perfectly valid argument in a special abductive or ampliative or inductive logic. But it conduces to clarity if we say instead that abduction is a deductive *enthymeme* and supply its missing premise. Its missing premise is obviously the (metaphysical) principle that any explanation of a surprising fact is true. This conduces to clarity because we can now see clearly that abduction is something no sane philosopher should accept. The metaphysical premise which validates the inference is obviously *false*. Any sane philosopher knows of countless cases where an explanation of some surprising fact is false.¹⁶

But what if an explanation of some surprising fact is *better* than any other explanation that we have? Inference to the best explanation goes something like this:

F is a fact.

Hypothesis *H* explains *F*.

No available competing hypothesis explains *F* as well as *H* does.

Therefore, *H* is true.¹⁷

This argument too is patently invalid. Most say that although inference to the best explanation is *deductively* invalid, it is actually a perfectly valid argument in a special abductive or ampliative or inductive logic. But again it conduces to clarity if we say instead that inference to the best explanation is a deductive *enthymeme* and supply its missing premise. Its missing premise is obviously the (metaphysical) principle that the best available explanation of any fact is true. This conduces to clarity because we can now see clearly that inference to the best explanation thus construed is something that no sane philosopher should accept. Again, the metaphysical premise which validates the inference is obviously *false*. Any sane philosopher knows of countless cases where the best available explanation of some fact turned out to be false.

Reconstructing inference to the best explanation as a deductive *enthymeme* conduces to clarity in another way — it gives us a clue as to how the inference might be rescued from absurdity. It is absurd to say that the best available explanation of any fact is true. It is not obviously absurd to say that it is reasonable to accept the best available explanation of any fact as true (tentatively, of course), or to presume (tentatively) that it is true. For it is plain, is it not, that it may be reasonable tentatively to accept something as true which subsequently turns out to be false. (If it does turn out to be false, we say that what we accepted was wrong, not that we were wrong to accept it.) This suggests that we replace the obviously false metaphysical premise of the argument by this epistemological premise (amending the conclusion accordingly). The resulting pattern of argument is deductively valid and its major premise is not obviously mistaken.

Inference to the best explanation, thus reformulated, will still not quite do. What if our best explanation of some fact is a perfectly lousy one? Would it be reasonable to accept it tentatively as true? Obviously not. What we need is a principle to the effect that it is reasonable to accept a *satisfactory* explanation which is the best we have as true. And

we need to amend the inference-scheme accordingly. What we finish up with goes like this:

It is reasonable to accept a *satisfactory* explanation of any fact, which is also the *best* available explanation of that fact, as true.

F is a fact.

Hypothesis *H* explains *F*.

Hypothesis *H* *satisfactorily* explains *F*.

No available competing hypothesis explains *F* as well as *H* does.

Therefore, it is reasonable to accept *H* as true.

Of course, for this argument-scheme to be applicable, the 'explanationist' owes us an account of when an explanation is minimally adequate (or 'satisfactory'), as well as an account of when one satisfactory explanation is *better* than another. But this digression on inference to the best explanation has gone on long enough, so I will simply assume that such explanationist accounts can be given.

To return to the Ultimate Argument for scientific realism. It is, I suggest, an inference to the best explanation. The fact to be explained is the (novel) predictive success of science. And the claim is that realism (more precisely, the conjecture that the realist aim for science has actually been achieved) *explains* this fact, explains it *satisfactorily*, and explains it *better* than any non-realist philosophy of science. And the conclusion is that it is reasonable to accept scientific realism (more precisely, the conjecture that the realist aim for science has actually been achieved) as true.

Suppose that we now have the argument right. If so, to repeat, it is not realism that explains facts about science, and realism is not an "over-arching scientific hypothesis". If realism could explain facts about science, then it could be refuted by them too. But a philosophy of science is not a description or explanation of facts about science. It is fashionable to identify scientific realism with the view that all (or most) scientific theories are true (or nearly so), or with the view that all (or most) *current* scientific theories are true (or nearly so), or with the view that all (or most) *current* theories in the '*mature*' sciences are true (or nearly so). But a pessimistic scientific realist might think none of these things *without thereby ceasing to be a realist*. A slightly more optimistic

realist might tentatively accept some particular theory as true. And the suggestion is that such a realist can then give the best explanation of that particular theory's success.

Is the suggestion correct? That partly depends upon whether it is true that non-realists have no explanation, or only an inferior explanation, of (novel) predictive success. As we have seen, Putnam directed his argument against Berkeley and the 'positivist'. It seems to be right that Putnam's positivist (for whom no theoretical entities exist and for whom all theories are false) can only regard (novel) predictive success as a lucky accident or 'miracle'. We think poorly of a person who 'explains' why the light goes on when we press the switch by saying "It is just a lucky accident". And we should think equally poorly of the positivist who says the same thing of science's (novel) predictive success.

The case of Berkeley is more interesting. Berkeley denies not only the theoretical entities of science, but also the 'theoretical entities' of commonsense realism, tables and chairs and trees in the Quad. (He tries to soften the latter denial by re-defining words like 'table' and by telling us a tale about what we 'really mean' by such statements as "My table is in my study though nobody is perceiving it". No matter.) But if there are no tables and chairs and trees in the Quad, how come our false beliefs about such things are so successful? Such commonsense beliefs yield innumerable successful predictions: "If I return to my study, I shall again see my table and chair", "If you come into the Quad with me, we shall both see the tree", "If I shut my eyes for a second, when I re-open them I will see things as I do now", and so on. As Putnam notes, Berkeley gives a *theological* explanation: God directly causes our perceptions, God is good, so God causes our perceptions in a regular fashion. Berkeley would deny that the success of commonsense realist beliefs is a miracle: phenomenal regularities are only to be expected, given Berkeley's metaphysic. What would be miraculous would be a 'sensible thing' which looked, smelled and felt like an orange, but tasted like a banana. God might work such a miracle. But so as not to confuse us, He does not (or not often).

Berkeley's theoretical posit (God) introduces all sorts of problems which the commonsense realist's posits (independently existing objects) do not. Hence it is widely (and rightly) regarded as the weak link of his system. But if you remove God from Berkeley's picture, you have a metaphysic (phenomenalism) which has no explanation of the success

of commonsense beliefs. If Berkeley's theory and phenomenalism were the only available theories, an inference to the best explanation should lead us to prefer Berkeley!

As for science, Berkeley takes a thoroughgoing instrumentalist view of it. (In my opinion, he was the first to do so, *pace* Duhem and his countless followers.¹⁸) So what can Berkeley make of the predictive successes of science? He can say that it is no accident that 'mathematical hypotheses' contrived to summarise some phenomenal regularities should successfully predict new instances of those regularities. That just testifies to man's ingenuity (in the contriving) and to God's benevolence (in the maintaining of the known phenomenal regularities). We cleverly concoct a fiction called 'geometrical optics', which trafficks in non-existent light rays, and in 'mathematical hypotheses' concerning the rectilinear propagation, reflection and refraction of these non-entities, to summarise phenomenal regularities about things casting shadows, how things look in mirrors, sticks looking bent in water, and so on. The regularities being correct (God willing), and the fiction being cooked up to yield them, it is no accident that it successfully predicts future instances of them.

But what if geometrical optics yields a *new* regularity? What if it predicts that looking at a thing through a certain arrangement of lenses will make it look bigger? (I know that the example lacks historical veracity.) The realist who accepts geometrical optics as true will expect this prediction to be true also. Berkeley can have no such expectation. For all he knows, God could fix it that objects viewed through telescopes will look smaller, disappear altogether, turn into ducks, or whatever. Only after Berkeley has learned from experience that they do none of these things, but look bigger instead, can he say "Ah, that is how God's benevolence manifests itself in this case". But he could not have predicted it — and he could not have explained it, in terms of the truth of geometrical optics, either. (It may be objected that Berkeley could explain the novel predictive success of geometrical optics in terms of its *empirical adequacy*. I am not aware that Berkeley did or could give such an explanation. I consider it soon.)

So I think that Putnam is right. The realist can give a better explanation of science's (novel) predictive success than either the positivist or the Berkeleyan idealist. (This is *not* to say that the realist's explanation is a good one.) But what of other anti-realists, such as van Fraassen or Laudan? They do not deny (as the instrumentalist does) that theories

are either true or false. They do not assert (as the positivist does) that they are all false — they concede that some theories might be true. What they deny is that it can ever be reasonable to presume (however tentatively) that any theory is true. Accordingly, they do not think true theories are a sensible aim for science, and they put something else in its place. They are anti-realists on epistemological grounds; we might call them *epistemological anti-realists*. What explanation might they give of (novel) predictive success?

Van Fraassen replaces truth by empirical adequacy as an aim for science. A theory is empirically adequate if all of its 'observational consequences' are true. So an explanation which van Fraassen might give and which parallels the realist explanation is:

Theory *T* is empirically adequate.

Theory *T* yielded several novel predictions.

Therefore, *T*'s novel predictions were true.

This 'explanation' invokes the fact that all of a theory's observational content is true to explain why some particular observational consequences are. This is like explaining why some crows are black by saying that they all are. The realist explanation seems better than this, because the postulated truth of a theory (and the implied existence of its theoretical entities) transcends the truth of some or all of its observational consequences. (One wonders how the empirical adequacy of a theory might be explained if not by postulating its truth.)

In fact, van Fraassen offers us a quite different explanation of science's predictive success. The success of current scientific theories is no miracle, he says, because only successful theories survive the fierce Darwinian competition to which all theories are subjected.¹⁹ But this changes the subject. It is one thing to explain why only successful theories survive, and quite another thing to explain why some particular theory is successful. van Fraassen's Darwinian explanation of the former can be accepted by realist and anti-realist alike.²⁰ But it yields no explanation at all of the latter. You do not explain why (say) electron-theory is (scientifically) successful by saying that if it had not been it would have been eliminated. Just as you do not explain why (say) the mouse is (biologically) successful by saying that if it had not been it would have been eliminated. Biologists explain why the mouse is successful by telling a long story about its well-adaptedness. Realists want to explain why electron-theory is successful by telling a shorter story about its 'well-adaptedness', that is, its truth.

Laudan replaces truth by problem-solving ability as an aim for science. A theory solves an empirical problem if it yields a correct answer to it. So Laudan might give an explanation of success like the following:

Theory *T* correctly solves all its empirical problems.

Theory *T* yields several novel predictions.

Therefore, *T*'s novel predictions are true.

Again, this 'explanation' invokes the fact that all of a theory's empirical consequences are true to explain why some particular ones are. Again, this is like explaining why some crow is black by saying that they all are. Again, the realist explanation seems better than this. And again, one wonders how the problem-solving ability of a theory could be explained without postulating its truth.

I should make it clear that Laudan himself does not propose an explanation of this kind, or indeed of any other kind. He says that the realist explanation is "attractive because self-evident". But he objects to it on epistemological grounds, saying that we can never "reasonably presume of any given scientific theory that it is true".²¹ Further, he argues historically that past theories which we now think false (and non-referential) were just as successful as present theories which realists think true (and referential). Given such views, Laudan must think that success just is a lucky accident and eschew all attempts to explain it.

Finally, let us consider what Jarrett Leplin calls the *surrealist explanation* of success, 'surrealism' being short for 'surrogate realism'. It goes like this:

The world is *as if* theory *T* were true.

Theory *T* yields several novel predictions.

Therefore, *T*'s novel predictions are true.

Is this explanation as good as the realist one?

It is not easy to answer this question, because it is not easy to say what "The world is *as if* theory *T* were true" actually *asserts*. For the explanation to go through it must assert *at least* that the world is *observationally* as if *T* were true. If it asserts no more than this, then it is just a fancy way of saying that *T* is observationally or empirically adequate. In this case, the surrealist explanation comes from the same stable as those already considered, and is subject to the same objections.

So perhaps "The world is *as if T* were true" is meant to be more

than a fancy way of saying "*T* is empirically adequate". Perhaps it is meant to entail everything that *T* entails *except* just for *T* itself. But this is not a coherent position. Let *S* be any statement entailed by but not entailing *T*. On the view suggested, "The world is *as if T* were true" entails both *S* and "Either *T* or not-*S*". But these in turn entail *T* (by double negation and disjunctive syllogism). Hence "The world is *as if T* were true" entails *T* also (by transitivity of entailment). Given the logical principles just mentioned, "The world is *as if T* were true" cannot entail everything that *T* entails except just for *T* itself.

The surrealist's dilemma is plain. If he invokes less than empirical adequacy, then he has no explanation of empirical success.²² If he invokes empirical adequacy and no more, then he has only a poor explanation. If he invokes more than empirical adequacy, then he has to tell us *what* more in a way that does not collapse "The world is *as if T* were true" into "*T* is true".

It seems, then, that the realist has a better explanation of novel predictive success than the epistemological anti-realists. Van Fraassen's empirical adequacy and Laudan's problem-solving ability and the surrealist's *as if* ploy all yield alternative descriptions of empirical success in general. As such, they do not give good explanations of particular instances of it. Positivist *atheism* about theories and their theoretical entities makes a mystery of the novel predictive success of those theories. And so does the *agnosticism* about theories and their theoretical entities recommended by epistemological anti-realists. The realist explanation seems better because it posits the truth of a successful theory and the existence of its theoretical entities.

In any case, realists will be thoroughly impatient with the theoretical agnosticism of these anti-realists and with the rival 'explanations' to which it leads. Those 'explanations' are clearly parasitic upon the straightforward realist explanation (as emerges most clearly in the surrealist case). Moreover, impatience can prompt argument here. Anti-realisms, atheistic or agnostic, must all operate with some distinction between observation and theory, or between 'observable entities' and 'theoretical entities'. Without that distinction, truth cannot be distinguished from empirical adequacy or from problem-solving ability, and surrealism collapses into realism. Anti-realists draw the observable/theoretical line in different places — but they all draw it somewhere *and* give it crucial ontological and/or epistemological significance. Now realists steadfastly argue that no such distinction can be drawn, at least

none sharp enough to bear the ontological and/or epistemic burdens which anti-realists place upon it. The distinction between what we happen to be able to observe and what not is irredeemably vague. And why should my ontological commitments be limited to the 'observable' or my epistemic commitments to statements about the 'observable'? This is not the place to rehearse these familiar realist arguments.²³ Suffice it to say that anyone persuaded by them will object that the explanations we have pitted against the realist explanation all rest upon dubious and human-chauvinistic philosophy.

I concluded a paragraph back that the realist explanation of success *seems* better than anti-realist ones because it posits truth and reference for successful theories. But is it really any better? We objected to explaining why *some* empirical consequences of a theory are true by invoking the fact that they *all* are. Cannot a similar objection be levelled at the realist? The realist explanation is "Theory *T* is true", which is the same as saying "All the consequences of *T* are true". So the realist explains why some consequences are true by saying that they all are. The only difference between the realist and anti-realist explanations is one of *scope*: the realist invokes the fact that all of a theory's consequences are true, the anti-realist invokes the fact that all of its empirical consequences are true.

The realist explanation is better, one might say, because broadness of scope is an explanatory virtue. Other things being equal, we prefer the broader explanatory theory because it tells us more, excludes more possible states of affairs, is more testable. Whatever the virtues of this maxim in science, its application to our case is problematic because our case is a curious one. The realist explanation in terms of truth is *not* more testable than the anti-realist explanation in terms of empirical adequacy. The realist explanation may 'tell us more', but in the nature of the case there can be no *empirical evidence* that the more it tells us is correct.

Such reflections convince Arthur Fine that the realist explanation is actually *worse* than the anti-realist one. He writes:

Metatheorem 1. If the phenomena to be explained are not realist-laden, then to every good realist explanation there corresponds a better instrumentalist one.

Proof: In the proffered realist explanation, replace the realist conception of truth by the pragmatic conception [of truth as empirical adequacy]. The result . . . will be the better instrumentalist explanation.²⁴

Fine's intuition (and here he follows van Fraassen) is that the realist's explanation of success involves 'metaphysical excess baggage', since there can be no *evidence* for truth over and above empirical adequacy. In science we try to avoid encumbering theory with ingredients which demonstrably have no empirical pay-off — why not do the same in *philosophy* of science?

In reply to this, the realist can simply say that there are explanatory virtues which are neither evidential nor obviously connected with scope. In science and in philosophy of science, one of a pair of empirically equivalent theories can possess explanatory virtues that the other lacks. Ancient astronomers thought that the stars move as they do because they are fixed on a sphere which rotates once a day around the central earth. Compare that theory with its surrealist transform, the theory that stars move *as if* they were fixed on such a sphere. Is not the first theory explanatory in a way that the second is not, despite the fact that the second is expressly designed to be empirically equivalent with the first? Nineteenth-century geologists devised an elaborate theory of fossil formation, to which twentieth-century geologists have added an equally elaborate theory of radio-carbon dating. Call the amalgam of these theories *G*, and compare it with its Gossian transform *G**, the theory that God created the universe in 4004 BC in such a way that it would *appear* that *G* was true. Is not *G* explanatory in a way that *G** is not, despite the fact that *G** is expressly designed to be empirically equivalent with *G*? And has not *G** been rejected in favour of *G*, despite the fact that no empirical evidence can decide between them?²⁵ We are actually in old logical positivist territory here (as Wade Savage has pointed out to me): compare *any* scientific theory *T* with its Craigian transform *T_C*; is not *T* explanatory in a way that *T_C* is not, despite the fact that *T_C* is expressly designed to be empirically equivalent with *T*? We are just re-running Hempel's 'theoretician's dilemma' all over again, and the battle-lines are the same as they always were. If the name of the game is 'saving the phenomena' (van Fraassen, Fine), then one of a pair of empirically equivalent theories is just as good as the other. If the name of the game is explaining the phenomena (realism), then this is not the case.

As in science, so also in philosophy of science. Compare the realist explanation of science's success in terms of truth and reference, with the anti-realist explanation in terms of empirical adequacy. Is not the former explanatory in a way that the second is not, despite the fact that

the second was expressly designed to be empirically equivalent with the first?

The thrust of this realist rhetoric is the same both at the scientific and at the meta-scientific levels. It is that explanatory virtues need not be evidential virtues. It is that you should feel *cheated* by "The world is *as if T* were true", in the same way as you should feel *cheated* by "The stars move *as if* they were fixed on a rotating sphere". Realists do feel cheated in both cases. But anti-realists do not. If you are an anti-realist who does not mind surrealist and other transforms of *scientific* theories, then you will not be impressed by the Ultimate Argument on the meta-scientific level either.

Michael Levin has a deeper worry about the Ultimate Argument, indeed, about the entire project of giving a meta-scientific explanation of science's success. He claims that *science* can explain its own success, and that we do not need philosophy of science (whether realist or anti-realist) to do this:

The explanation of the success of a theory lies within the theory itself. The theory itself explains why it is successful . . .

A theory's successes are the true predictions it has made with its own internal resources. Conjoin them and you have its success, but you do not have any further phenomenon which the theory in question fails to explain and which may perhaps be explained by some such other hypothesis as truth. To explain a conjunction, explain its conjuncts.²⁶

But this cannot be *quite* right. Granted, a theory itself explains its predictive successes (assuming it is not a surrealist theory). But what needs explaining here are not those predictive successes, facts about the world, but rather the fact that the theory had those successes, a fact about the theory. A theory itself cannot "explain why it is successful": electron-theory, for example, is about electrons, not about electron-theory.

It transpires that Levin's real worry is whether a theory's *being true* could explain anything about that theory. He argues that scientific theories are (intellectual) artefacts, that an explanation of the success of an artefact is always *mechanistic*, and that truth is not a mechanism:

And here is my problem: what kind of *mechanism* is truth? How does the truth of a theory bring about, cause or create, its issuance of successful predictions? Here, I think, we are stumped. Truth . . . has nothing to do with it. "By being true" never satisfactorily answers the question, Why did such and such a belief lead to correct expectations? The answer always lies elsewhere.²⁷

Is it true that the truth of a belief never explains why that belief led to correct expectations? Suppose Hopalong succeeds in finding gold in them-thar-hills. How might we explain his success? A natural explanation (though not the only one) is that Hopalong believed that there was gold in them-thar-hills, acted accordingly, *and that his belief was true*. But semantic descent being what it is, we might as well say that Hopalong believed that there was gold in them-thar-hills, acted accordingly, *and that there was gold in them-thar-hills*. Thus Levin: it was not the truth of Hopalong's belief that made it successful, rather it was the fact that there was gold in them-thar-hills just as Hopalong believed there to be.

This is playing with words. Semantic *ascent* being what it is, we do not have *rival* explanations here, but rather equivalent formulations of the *same* explanation. "*H* believed that *G* and *G*" is equivalent to "*H* believed *truly* that *G*" (given the theory of truth that Levin and the realists both accept).

Levin insists upon the first formulation because of his worries about truth not being a 'mechanism'. Of course truth is not a mechanism. It is a property which beliefs or theories may possess. But when a belief possesses it, this fact can figure in an explanation of why acting upon that belief leads to success. Such an explanation can even be described as 'causal' or 'mechanical'. Levin insists that "By being true" cannot explain why Hopalong's belief was successful, that the answer "lies elsewhere", presumably in them-thar-hills. But "By being true" takes us to them-thar-hills and tells us that gold lies there.

So I do not insist, as Levin does, on semantic descent. It is worth noting that if we *do* insist upon it, we will be equally sceptical of the anti-realist explanations of success that we have considered. If truth is not explanatory because it is not a 'mechanism', then neither, presumably, is empirical adequacy. More important, suppose we follow Levin and say that the *real* explanation of the success of electron-theory (say) is that there really are electrons, they really do carry a certain elementary charge, and so on. This explanation yields everything that the realist wants to say about electron-theory (say). It is just that Levin forbids him from ascending to say it. He insists upon "Electrons really exist" rather than "The term 'electron' really refers", and upon "Electrons carry a certain elementary charge" rather than "The statement 'Electrons carry a certain elementary charge' is true". The realist, puzzled, might easily comply.

So what, ultimately, of the Ultimate Argument? It is best construed as an inference to the best explanation of facts about science. The facts which need explaining are best construed as facts about the *novel* predictive success of *particular* scientific theories. The realist explanations of such facts are best construed as invoking (conjecturally) the truth of those theories (or their near-truth if we can develop such a notion) and the reference of their theoretical terms. Positivist anti-realists have no competing explanation of such facts about science. Epistemological anti-realists give no competing explanation either. But we can give such explanations on their behalf. And when we do, we find that the situation is curiously circular: realist explanations of success are preferable *on realist grounds*; anti-realist explanations of success are preferable *on anti-realist grounds*. The attempt to make realism explanatory of facts about science, which is what the Ultimate Argument does, will fail to convince anti-realists who doubt that science itself is explanatory.

University of Otago

NOTES

¹ It was christened thus by van Fraassen in his (1980), p. 39.

² Paraphrased from Putnam, (1975), p. 73.

³ As cited by Blake (1960), p. 34 (the passage comes from Clavius's *Commentary on the Sphere of Sacrobosco*).

⁴ As cited by Blake (1960), p. 33.

⁵ Attempts to rebut this sceptical argument by Clavius and Kepler are documented in Jardine's (1979).

⁶ Laudan (1981), p. 45.

⁷ Laudan's accusation here is not without foundation. Brown explicitly reconstructs the Ultimate Argument (which he calls the 'miracle argument') as an argument *for* the (probable) truth of theories which make true predictions: see his (1982), pp. 98–9, or his (1985), p. 51. The argument Brown reconstructs is deductively invalid and is a souped-up version of affirming the consequent.

But no fallacy *need* be involved in taking predictive success as a reason for believing (tentatively) in the truth of a theory. The argument can be reconstructed as follows:

If a theory is predictively successful, then it is reasonable to accept it tentatively as true.

Theory *T* is predictively successful.

Therefore, it is reasonable to accept theory *T* tentatively as true.

This argument is *logically* impeccable. Sceptical doubts about it must focus upon the

'inductive principle' which forms its major premise. But you do not refute that premise by pointing out that predictive success does not entail truth: it may be reasonable tentatively to accept a theory as true even though that theory subsequently turns out to be false. I mention this because Laudan, in criticising realism on sceptical grounds in his (1981), thinks it sufficient to dispose of the idea that predictive success *entails* truth. (I do not myself think that predictive success *simpliciter* is a reason for acceptance, and I will be saying why.)

Here, as elsewhere, I prefer to construe so-called 'inductive arguments' as deductive arguments with 'inductive principles' of one kind or another among their premises. This conduces to clarity and obviates the need for any special inductive logic.

⁸ Whewell (1837), volume II, p. 68.

⁹ For an elaboration and defence of it, see Worrall's (1985).

¹⁰ Popper (1963), pp. 117–8. Popper obviously has *successful* novel predictions in mind here, for an *unsuccessful* one would hardly count as a discovery.

¹¹ Duhem (1954), p. 28; see also pp. 297ff.

¹² Putnam (1978), p. 19.

¹³ Laudan (1981).

¹⁴ Putnam (1978), pp. 18–19.

¹⁵ Laudan (1981), p. 23.

¹⁶ Peirce did not formulate abduction as I have. The only important difference is that Peirce's conclusion is not "*T* is true" but rather "There is reason to suspect that *T* is true" [Peirce (1931–58), 5.189]. Peirce's original scheme is also invalid, and the missing premise that would validate it is also unacceptable. Is there reason to suspect that any explanation, *however bizarre*, of some surprising fact is true? However, Peirce's intuition that abductive arguments are *epistemological* (as evidenced by the epistemic modifier in his conclusion) was sound: I shall be saying the same of inference to the best explanation. (Incidentally, both abduction and the misleadingly labelled 'inference to the best explanation' are located firmly in the context of justification rather than the context of discovery, despite what many think.)

¹⁷ This is a slightly simplified version of Lycan's formulation in his (1985), p. 138. Some make truth a defining condition of explanation, so that we do not have an explanation *at all* unless what does the explaining is true. They would have to reformulate the argument so that it becomes inference to the best *putative* explanation. I prefer to make truth an adequacy condition on explanation rather than a defining condition of it, so that it makes sense to speak of a false explanation.

¹⁸ The orthodox view is that an instrumentalist tradition regarding astronomical hypotheses was inaugurated by the great Hellenic astronomers (Eudoxus, Hipparchus, Apollonius, and Ptolemy). I criticise that orthodoxy in my (1981).

¹⁹ van Fraassen (1980), pp. 39–40. For a further discussion of this explanation, and of the biological analogy on which it is based, see my (1985), pp. 209–10.

²⁰ It was actually proposed by the realist Popper in 1934; see Zahar (1983), p. 169. Incidentally, Brown says [in his (1985), p. 49] "Karl Popper has steadfastly held that the success of science is not to be explained; it is a miracle". But what Popper holds is that no theory of knowledge should try to explain *how we have come up with* successful theories sometimes. This is quite consistent with explaining why a particular theory is successful, and with explaining why only successful theories survive. In this area it is crucial that we get the *explanandum* right.

²¹ Laudan (1981), p. 30. Laudan does not actually argue for this strong epistemic

thesis: rather, he seems to think that it follows from a weaker thesis that he does argue for, the thesis that the evidence does not *entail* that a scientific theory is true. But the latter thesis does not entail the former at all, of course (see also footnote 7 above). Indeed, it can be shown that Laudan himself thinks that we can reasonably presume of *some* theories that they are true: see my (1979), pp. 459–60.

²² Actually, the surrealist *as if* ploy is sometimes used to invoke less than empirical adequacy. Historians of astronomy say that Eudoxus cannot have thought his theory of planetary motion true, because 'interpreted realistically' it clashes with the observed fact that the planets vary in brightness. Eudoxus was saying 'The world is *as if E*' rather than just *E* (where *E* is Eudoxus's theory). But if 'The world is *as if E*' is not to clash with observed brightness variations, it cannot mean 'The world is *observationally as if E*' (or '*E* is empirically adequate'). It must mean something like 'As far as planetary positions go (but not their brightnesses), the world is *as if E*'. Here the surrealist ploy eliminates some of the empirical content of the theory to which it is applied. A similar case arises if it is applied (as it has been by some historians) to Ptolemy's theory of the moon.

²³ I rehearse some of them against van Fraassen in my (1985), especially pp. 204–9, and against Laudan in my (1979).

²⁴ Fine (1986), p. 154. According to Fine, anti-realists like van Fraassen or Laudan do not so much *replace* truth by empirical adequacy as an aim for science, as retain truth as the aim but give an *empirical adequacy theory of truth*. One can see them this way. It is not the way they see themselves, nor is it the clearest way to see them. But this issue does not affect the matters being discussed here, so I shall not pursue it.

²⁵ It is not that *G** yields no explanation *at all* of fossils and decay elements in rocks, it is merely that it yields a quite *different* explanation than *G* does. – *G**'s explanatory mechanism is *essentially* divine and unintelligible by humans, while for its explanatory *details* it is entirely parasitic upon *G* (converting them into details about what God had in mind on Monday – or was it Tuesday? – one week 4004 years ago). Science rejects *G** in favour of *G*, despite their empirical equivalence. Perhaps this is because *G**'s essential mechanism is (and is meant to be) unintelligible. Perhaps it is because the empirical character of *G** is a sham: if future geologists should abandon *G* in favour of *H*, the Gossian will happily switch to *H** and preserve the *essence* of his position. Perhaps science prefers *G* to *G** for a mixture of these two reasons, since they are not unconnected reasons: it is because it is unintelligible that *G**'s essential mechanism can be preserved. The important point for my purposes is only that we do here have a rational choice between empirically equivalent theories. (It may be objected that Gosse's *original* hypothesis, unlike *G**, was not empirically equivalent with nineteenth century theories of fossil formation. Perhaps. Still, was it rejected on *empirical* grounds?)

²⁶ Levin (1984), p. 127.

²⁷ Levin (1984), p. 126.

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RADICAL PLURALISM – AN ALTERNATIVE TO REALISM, ANTI-REALISM AND RELATIVISM

Realism, the dominant 20th century position in Anglo-American thought, is, in the relevant sense, a one world position. There exists a unique actual world, or reality, external to "us", which not only determines how things are locally and globally, but determines as well truth, and thus also uniquely fixes correctness in science, *the* correct theory being that which corresponds to reality. Anti-realisms such as idealism and phenomenalism reject, in one way or another, the tricky externality requirement. Relativism and pluralism, by contrast, reject one of the uniqueness requirements, but in significantly different ways. Relativism resists, in one fashion or another, the imposition of any ranking better than "equally good" and of any rankings warranting differential choice, on the multiple interpretations or, very differently, multiple realities or worlds disclosed. Pluralism, however, to set down at once the crucial contrast, permits and typically makes rankings, which enable choice (including realist and idealist and theist choices, among many others). Pluralism thus comes in two distinct forms: theory or meta-pluralism, according to which there are many correct theories (especially larger philosophical positions) but at most one actual world; and radical or deep pluralism which goes to the root of these differences in correctness, to be found in things, and discerns a plurality of actual worlds as well as of theories.

Realism, then, characteristically involves not only the (*existential*) claim that *there is an actual world* with various prized properties such as externality and mind-independence; but it further involves the claim that there is only one such world, that *the world is unique*. The *uniqueness* claim is essential: otherwise Reality is not fully determinate, and the actual world cannot perform expected realist functions of determining truth, correctness and the like, in a way that is single-valued and entire.¹ It is the rarely considered, but normally simply assumed, uniqueness claim that is a main focus of concern here. It will be contended that uniqueness fails, that not only is uniqueness not established, but it cannot be nonlegislatively, because there is not a unique actual world. A central thesis to emerge is then that there are many