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Explaining the Success of Science

Karl Popper has steadfastly held that the success of science is not to be explained; it is a miracle. '. . . [N]o theory of knowledge', he says, 'should attempt to explain why we are successful in our attempts to explain things.'[1] And even though '. . . science has been miraculously successful . . .', as he puts it, '[t]his strange fact cannot be explained.'[2] Consistency with his other views requires him, no doubt, to disavow any presupposition that a scientific theory is likely to be true. Yet explanations of the success of science often make that very assumption; a theory's *success* is explained by assuming that the theory is *true*. Hence Popper's quandary. But throwing up our hands in despair or embracing miracles seem neither the heroic nor the reasonable thing to do. I have nothing heroic to offer by way of accounting for the success of science either, but I will try a moderately reasonable stab at it. My proposal, however, will not be all that different from Popper's point of view.

Before proceeding further, something should be said about the term 'success'. There are several ways in which science is an overachiever: Its technological accomplishments are undeniable: it's very handy for building bridges and curing diseases. It is a glorious entertainer: many of us would rather curl up in bed with a good piece of popular physics than with any novel. And science has also been a great success at extracting tax dollars from us all. (I do not say that cynically; I would gladly pay more.)

By calling science successful I do not mean that everything that is called science is successful, only that many presently accepted theories are. And by calling these theories successful I simply mean that

1 They are able to organize and unify a great variety of known phenomena.

From *Ratio* 27 (1985): 49–66.

2 This ability to systematize the empirical data is more extensive now than it was for previous theories.
3 A statistically significant number of novel predictions pan out; that is, our theories get more predictions right than mere guessing would allow.

This, I think, is roughly what is involved in the normal use of the phrase 'the success of science', and I am simply following tradition here. At any rate these are the senses of success that I shall be dealing with. Even though they are common ingredients, they are not, however, always clearly distinguished by writers on this topic.

Before getting on to the main arguments I should also make the real motivation clear. Few concerned with this question care why science is successful *per se*. What they really care about are the ontological consequences of the various explanations. Realists, for example, think that if they can explain the success of a theory by appeal to the *truth* (or approximate truth) of that theory, then the ontological issue will be decided in their favour. Anti-realists, on the other hand, propose rival accounts which they see as ontologically innocuous. Since what is really at stake is the ontological question, it is best if I set the stage in terms of realists vs anti-realists.

■ | Miracles, Darwin, and 'The Truth'

The thing to be explained is the success of science and the way realists often explain this fact is by claiming that theories are true, or at least approximately true, and that any conclusion deduced from true premisses must itself be true. So the assumption that theories are (approximately) true explains the success of those theories. Realism, as Hilary Putnam puts it, is the only explanation which doesn't make the success of science a miracle. J. J. C. Smart states the case this way:

> If the phenomenalist about theoretical entities is correct, we must believe in a *cosmic coincidence*. That is, if this is so, statements about electrons, etc., are of only instrumental value: they simply enable us to predict phenomena on the level of galvanometers and cloud chambers. They do nothing to remove the *surprising character* of these phenomena. . . . Is it not odd that the phenomena of the world should be such as to make a purely instrumental theory true? On the other hand, if we interpret a theory in a realist way, then we have no need for such a cosmic coincidence: it is not surprising that galvanometers and cloud chambers behave in the sort of way they do, for if there really are electrons, etc., this is just what we should expect. A lot of surprising facts no longer seem surprising.[3]

We can reconstruct the argument which lies buried in this passage in the way which makes it seem quite reasonable and convincing.

1 Conclusion O can be deduced from theory T.
2 O is observed to be the case.
3 If T is true then the argument for O is *sound* and so O *had* to be true.
4 If T is false then the argument for O is *merely valid* and the probability of the arbitrary consequence O being true is very small. (I.e., it would be a miracle if O were true.)

∴ The argument for O is probably sound.
∴ T is probably true. (That is, even T's theoretical statements are probably true.)

This argument uses the realist's explanation of the success of science to draw ontological morals. Let us contrast it with the rival Darwinian view of some anti-realists. The latter goes something like this: Just as there are a great many species struggling for existence, so too, a great many theories have been proposed. And just as species which are not adapted to their environment become extinct, so too, theories which are not making true observational predictions are dropped. The belief that our theories might be true, or even approximately true, is an illusion. It is similar to the illusion that Darwin undermined, that species are evolving *toward some goal*. Van Fraassen, to pick the most prominent recent anti-realist, gives a Darwinian account of the success of science in *The Scientific Image*:

> I can best make the point by contrasting two accounts of the mouse who runs from its enemy, the cat. St. Augustine . . . provided an intentional explanation: the mouse *perceives that* the cat is its enemy, hence the mouse runs. What is postulated here is the 'adequacy' of the mouse's thought to the order of nature: the relation of enmity is correctly reflected in his mind. But the Darwinist says: Do not ask why the *mouse* runs from its enemy. Species which did not cope with their natural enemies no longer exist. That is why there are only ones who do.

And so, continues van Fraassen:

> 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.[4]

'Truth' plays no rôle at all in the success of science for the Darwinian anti-realist. Yet for the realist it is the central explanatory factor. So here

we have the two main contenders, but could either of these explanations of the success of science be right?

■ | The Darwinian Answer

I characterized the success of science as having three ingredients. Van Fraassen's Darwinian explanation seems to account for two of these features, but not the third. He has an apparently adequate answer to the two questions, Why do theories get so much right?, and Why do newer theories get more right than the ones we have tossed out? The simple answer is that we have tossed out any theory which didn't organize, unify, and generally get a lot right; and we have tossed out theories which have done less well, comparatively, than others.

The third question seems to be still unanswered. Why do our theories make correct predictions more often than one could expect on the basis of mere chance? Here the Darwinian analogy breaks down since most species could not survive a radical change of environment, the analogue of the novel prediction.

There is also a more general problem with this Darwinian approach. It is a problem which stems from the empiricism of the anti-realists. An implicit assumption is that rational choice and success go hand in hand. On this assumption it is not surprising that science is successful in senses (1) and (2) since we choose theories, says the empiricist, on that very basis. This, I think, is not so. Success, as characterised by the anti-realist, is a totally empirical notion. But in reality theories are rationally evaluated on the basis of several other considerations besides empirical factors. I don't wish to argue here for any in particular, but let us suppose that conceptual, metaphysical, and aesthetic concerns play a role in actual theory choice. Consequently, it is not a trivial analytic truth that the rational thing to believe is also the most successful. Anyone who is not an extreme empiricist must concede that it is quite *possible* that the most rationally acceptable theory is not the most successful theory.

So even the Darwinian answers to (1) and (2) which above I tentatively conceded to be adequate are, in fact, not adequate after all. And (3), of course, remains entirely unexplained. The Darwinian account, linked to an empiricist methodology, yields a plausible account of two of the three aspects of success, but unlinked from this untenable methodology, it accounts for nothing.

Realism and Reference

A belief common to scientific realists is that the succession of theories is getting closer to the truth. This belief may well be true (I hope it is), but it is often tied to a doctrine that says that the central terms of one theory refer to the same things as the central terms of its successor and predecessor theories. Moreover, the intuitive idea of getting-closer-to-the-truth will itself need fleshing out in the form of an explicit doctrine of verisimilitude. Unfortunately, there are terrible problems with both of these. Beliefs about the constancy of reference run afoul of the history of science, and the concept of verisimilitude is plagued with technical problems. Even a cursory glance at the past suggests that there is no royal road to the truth such as that implied by the convergence picture, and every explication of verisimilitude so far proposed has been a crashing failure.

Let's look at things now in some detail. Putnam gives a forthright version of the realist's explanation of the success of science in the following passage:

> The positive argument for realism is that it is the only philosophy that doesn't make the success of science a miracle. That terms in mature theories typically refer (this formulation is due to Richard Boyd), that the theories accepted in a mature science are typically approximately true, that the same term can refer to the same thing even when it occurs in different theories—these statements are viewed by the scientific realist not as necessary truths but as part of the only scientific explanation of the success of science, and hence as part of any adequate scientific description of science and its relations to its objects.[5]

In the next section I will examine the idea that mature theories are 'typically approximately true' by looking at Newton-Smith's views since they are much more developed than Putnam's. This section will be devoted solely to examining the claim that 'terms in mature theories typically refer'. Let us begin by looking at a very simple theory:

T_1: JRB went to Dubrovnik in April.

For the sake of the argument, let us suppose that it is quite a successful theory (there were reports of his being seen there, etc.) and that all the terms in it refer. Is the fact that all the terms refer *sufficient* to explain why the theory is successful? The simplest consideration completely undermines this supposition. The following theory is very unsuccessful:

T_2: JRB did *not* go to Dubrovnik in April.

Yet all the terms just as surely refer.

Not all counter-examples are so artificial; historical illustrations of the problem abound. Consider the succession of atomistic theories; some were successful, but many were not. So clearly, having the term 'atom' in the theory does not lead to success even though (we believe) the term 'atom' refers.

Reference is not sufficient for success, but is it *necessary*? This, too, seems most unlikely. Phlogiston theories, caloric theories, aether theories, and numerous others have all had a definite heyday, yet by our present best guesses the central terms of these theories do not refer.

In the Putnam-Boyd explanation of the success of science there is a caveat. The term 'typically' is used; 'terms typically refer' and theories are 'typically approximately true'. This seems to leave one free to dismiss the occasional example such as phlogiston or caloric as a tolerable aberration. It would then appear to be a question of degree and consequently the historical case for or against this sort of realism is going to be rather difficult to establish.

One could seriously doubt that the historical cases will come out the way they expect, that is, with successful theories *typically* having terms which refer. But even if this should be the case with almost every theory, there still remains one great problem. A *single* example of a successful theory with at least one term which does not refer must count as a miracle. Thus, the success of the caloric theory of heat, by the lights of Putnam and Boyd, must rank with the raising of Lazarus from the dead. And what Priestly achieved with his phlogiston theory was no less an amazing feat than if he had turned water into wine. By weakening the claim to just saying that reference is *typical*, easy counter-examples drawn from the history of science might be avoided. But the cost is impossibly high: every untypical example is a miracle.

Realism and Verisimilitude

It is time now to look at the other key idea in the Putnam-Boyd explanation of the success of science, the idea that theories are 'typically approximately true'. Unfortunately, neither Putnam nor Boyd have bothered to unpack this notion, so I will examine the similar but rather more developed views of William Newton-Smith instead.

Newton-Smith's approach to verisimilitude is a 'transcendental' one as he puts it. He too is looking for an explanation of what he sees is an undeniable fact: *science has made progress*. And how has this remarkable achievement come about? His realist answer is disarmingly simple: If our theories were getting closer to the truth then this is exactly what we should expect.[6]

To maintain a doctrine of increasing *verisimilitude*, or truth-likeness, is to maintain that the succession of past theories, up to the present, have been getting *closer to the truth*. There may be several respects in which later theories are better than earlier ones; they may be better predictors, more elegant, technologically more fruitful. But the one respect the realist cares about most is veracity; later theories, it is hoped and claimed, are better in this regard. Verisimilitude is an intuitive notion that most people subscribe to; but it is extremely problematic. The most famous instance of trying to come to grips with it, namely Popper's account, is a clear-cut failure. And unless someone is able to successfully explicate the notion soon, it is likely to have the same fate as such other intuitive notions as 'neutral observation' and 'simplicity'. It will be tossed on the junk pile of history.

There is one virtue of Newton-Smith's account of verisimilitude which needs stressing. Constancy of reference across successive theories is not required. The kind of problems phlogiston, caloric, and the aether present for the convergence account of Putnam and Boyd have no bearing on Newton-Smith's version. This is what makes his recent account interesting, initially promising, and worthy of special attention.

Let me now focus on some of the details of his account of verisimilitude. What is required, as he sees it,[7] is an analysis of the notion which will then justify the crucial premiss in his argument. That is, he must show that, on unpacking, the concept of verisimilitude yields this: *An increase in verisimilitude implies the likeliness of an increase in observational success*. And he is quite right to worry about this, for in spite of its intuitive nature, we cannot count on the properties of *truth* carrying over for *truth-likeness*. The consequences of a true theory are true; but the consequences of a theory which is approximately true may not themselves be approximately true.

Before getting to his analysis of verisimilitude, we need to set the stage with Newton-Smith's characterization of a few key notions. A *theory* is the deductive closure of the postulates and appropriate auxiliary hypotheses; an *observational consequence* is a conditional, $p \rightarrow q$, where p is a statement of the observable initial conditions and q the observable final conditions; the consequences of a theory must be *recursively enumerable*, (i.e., can be mechanically produced in a sequence. Newton-Smith does not defend this dubious condition.) A theory *decides* p if it implies either p or its negation. The *content* of a theory is a fairly technical notion, but we can say roughly that a theory has more content than another if it decides more sentences. Since typically both will decide infinitely many sentences some technical complications in the definition are required. Imagine two theories, T_1 and T_2, with their consequences recursively enumerated. The *n*th member of the sequence generated from T_1 either will or will not be decided by T_2. We are to determine which it is. (Newton-Smith fails to note that given Church's theorem, this is not going to be generally possible.) This process is generalized and finally we are able to form the appropriate ratio from the sentences decided by the two theories. In this way Newton-Smith is able to define which theory has the greater content, and he is able to do so in a manner which seems to capture our intuitive requirements. Of course, the definition is based on an infinite sequence, but for practical purposes, greater content could be determined after a large, but finite, number of sentences have been examined.

The last important notion is that of *relative truth*. Consider again the theories T_1 and T_2 with their consequences enumerated recursively. After n terms there will be a number of truths and a number of falsehoods for each. The ratio of these numbers is the *truth ratio*. We then pick a third theory T_3 to appraise the truth values of the sentences in the sequence generated by T_1 and T_2. (T_3 could be either from a God's eye point of view or it could be our presently held theory.) Newton-Smith then defines T_2 as having *greater truth relative to* T_3 than T_1 has, if and only if the infinite sequence of ratios, which give the ratio of truths in T_1 to the truths in T_2 as judged by reference to T_3, has a limit greater than ½.* Now we come to the main idea:

> T_2 has greater *verisimilitude* than T_1 if and only if both: (1) the relative content of T_2 is equal to or greater than T_1; (2) T_2 has greater truth relative to T_3 than T_1.[8]

So the rough idea is this: To have more verisimilitude is to say more about the world and to say more true things in doing so. Does this solve the initial problem which was to show the greater verisimilitude implied the likelihood of greater observational success? The answer, says Newton-Smith, is yes. Here is his argument: Pick an arbitrary sentence from T_2 which we will assume has greater verisimilitude than T_1 according to the definition. The chances of it being true, since it came from T_2, are greater than the chances of some arbitrary sentence which comes from T_1 being true. And since the set of arbitrary sentences of T_2 include the observational sentences it follows that T_2 will likely have more observational successes.

This account of the notion of truth-likeness certainly has its attractions. It is not obviously plagued with the same problems which beset Popper's account; it is simple and elegant; and it satisfies several of our most basic intuitions about the concept. However, it still seems to be not entirely satisfactory, as a number of considerations show.

Is Newton-Smith's explanation good at accounting for all three senses of success? Not entirely. It is very good at accounting for (2) and (3). But it doesn't say why present theories get much right; it is perfectly compatible

* There is an error in this definition, which Newton-Smith has since corrected. See the section "Verisimilitude and Success" in the commentary on this chapter.

with Newton-Smith's theory that our present beliefs organize the data poorly and generally get very little right. His theory guarantees that our present scientific theories do better than our past theories, and he can also explain why present theories get more novel predictions right than mere chance would allow for. But there is still one important sense of success left unexplained.

Second, consider the following situation:

The Truth: John is 180 cm tall.
Theory$_1$: John is 178 cm tall.
Theory$_2$: John is 179 cm tall.

These two theories say only one thing each. Intuitively, I think we should all agree that T_2 is closer to the truth. But on Newton-Smith's account we cannot say this. The two theories are both totally and equally false. The reason for this is simply that Newton-Smith takes the appropriate measure to be a comparison of the true sentences with the false ones. Individual sentences are taken to be one or the other, and never something in between. Yet a proper theory of verisimilitude would, I think, take this into consideration. And it would have to in order to do full justice to the history of science. Consider a succession of mini-theories which are really about nothing more than the value of some physical constant, say, Planck's constant, the gravitational constant, or the magnetic moment of the electron. Progress in such a case would be a sequence of claims which are becoming ever more accurate, though none as yet exactly right. Every claim as to what the value is would be false, yet surely it is correct to see this sequence as getting closer to the truth, if anything does.

A third problem that I see with Newton-Smith may be yet more serious. Historical considerations make his requirement of increasing content in the definition of greater verisimilitude implausible. Any event in the history of science where the domain shrunk, and there are several of them, will stand as a counterexample. Newton-Smith's requirement is that the later theory must have equal or greater content than the former. But this did not happen in the following example which most of us would likely consider a progressive move: Once there were theories which combined astronomy and astrology together, then a transition was made to purely astronomical theories. The earlier theories which combined both astronomical and astrological claims obviously said more about the world, so the later astronomical theories had less content. However we characterize truth-likeness, it must be compatible with such domain-shrinking transitions in the history of science. Newton-Smith's account is not.

Is Hypothetico-Deductivism the Problem?

What about the style of Newton-Smith's argument which links greater verisimilitude with the likelihood of greater observational success? Anti-realists often decry the hypothetico-deductive (H-D) form of inference. That is, they reject arguments which go:

Theory → Observation
Observation
———
∴ (Probably) Theory

Given that they find H-D arguments unconvincing (claiming that it is a simple fallacy of affirming the consequent), why should anti-realists be persuaded to become realists by an argument that goes: Verisimilitude would explain greater observational success and there has been greater observational success; thus, there must be greater verisimilitude? The style is the same in both cases:

Greater verisimilitude → Greater observational success
Greater observational success
———
∴ (Probably) Greater verisimilitude

The anti-realist will simply say that the question has been begged. Some of us may like Newton-Smith's argument for verisimilitude and the realist approach in general, but then we *already* liked H-D inference. Laudan gives voice to this anti-realist sentiment when he writes:

> . . . 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. . . . Now enters the new breed of realist . . . who wants to argue that epistemic realism can reasonably be presumed to be true by virtue of the fact that it has true consequences. But this is a monumental case of begging the question.[9]

Can the blame for the failures to explain the success of science be pinned on H-D inference? At first glance the fight between realist and anti-realist over the success of science seems but a dressed up version of the old problem of induction. If there is no hope of solving that problem, then how can we hope to explain the success of science? The answer, I think, is that they are not really the same problem. If H-D reasoning were really the issue here it would be a problem for anti-realists too. But van

Fraassen, a paradigm anti-realist, relies on H-D inference regularly, as he must for instance, in the following type of argument:

T is empirically adequate → Observation O
Observation O

∴ (Probably) T is empirically adequate.

Van Fraassen wants to go as little beyond the observable evidence as he can, but he does take some risks. He resists inferences to the truth but in accepting a theory as empirically adequate he recognizes the need for ampliative inference.

Similarly, Laudan, when he has his historian's hat on, says the shift to the H-D style of inference with Hartley and LeSage was a great step forward in the history of methodology.[10] Before their work, the Newtonian tradition of doing science was based on the famous dictum, *hypotheses non fingo*; theories were to be deduced from the phenomena. The introduction of H-D [reasoning] in the 18th century marked a definite advance.

Anti-realists such as van Fraassen and Laudan are not *sceptics* about induction. They need and use inductive inference as much as realists do. If realists are committing a fallacy at the meta-level of explaining science, then so is everyone else (except perhaps Popper) at the theory level of explaining the world. But to give up inductive inference entirely, which neither realists nor anti-realists wish to do, is just to stop doing science at all.

There is, in fact, a range of possibilities here where one might be tempted to draw a line. Consider the following:

I Evidence E is true.
II Theory T is empirically adequate.
III The entities T posits exist.
IV T is true.

They are ordered in terms of decreasing probability, given evidence E. An inductive sceptic will, of course, accept I given E, but will go no further. Van Fraassen will accept the likes of II, given E, but resists III and IV. The niche between II and IV is interesting, though it is not a common position. Nancy Cartwright, for example, believes that there are electrons, but that the electron theory is false. (Her half realist/half anti-realist view is partly revealed in the title of her new book, *How the Laws of Physics Lie*.[11])* The full-blooded realist is prepared in principle to accept IV. All of this makes it clear that there are anti-realist positions between full realism and inductive scepticism. The fight, contrary to Laudan, is not over the legitimacy of induction.

* See Nancy Cartwright, "Do the Laws of Physics State the Facts?" in chapter 7.

■ | The Truth Matters (A Little)

It is now time to take stock. By explaining success, remember, there are three things to be accounted for: (1) the fact that theories organize, unify, and generally account for a wide variety of phenomena. (2) Theories are getting better and better at this; they are progressing, and (3) a statistically significant number of their novel predictions are true. It is now time to stand back and see where we have gotten to.

(A) Well, for one thing, this attempt to account for the success of science is not just the problem of induction. So there is some hope of coming up with an answer. Realist explanations of success may well beg the question against that age old problem, but then we all (including the anti-realist) do that all the time. Induction, in principle, is not what is at issue here; rather it is a *particular* inductive inference that is being debated.

(B) The Darwinian account has no answer at all for (3), that is, for the fact that theories make novel predictions which are found to be true. It has an explanation of (1), the significant degree of empirical adequacy, and (2), the increasing degree of empirical adequacy over time, but it can explain these only by linking rational theory choice analytically to success. Since this is methodologically implausible, even the explanations of (1) and (2) are thus not acceptable. So the Darwinian account completely collapses; and quite possibly the hopes of the anti-realist to explain the success of science without appeals to *truth* also collapse. So the realist is now without a plausible rival.

(C) Let us turn now to the realist's account of things. Explaining the second aspect of success (i.e., that the succession of theories is getting better and better at accounting for the phenomena) is probably the most popular approach. Leplin thinks it is the most promising[12] and Newton-Smith, as we saw earlier, builds his doctrine of verisimilitude around it. Actually, it may be the least promising. The realist explanation of this sense of the success of science quite explicitly needs a theory of truthlikeness, that is, it is entirely dependent on the in principle existence of some theory of verisimilitude. However, none is available. I criticized Newton-Smith above and other versions of the doctrine have not gone unscathed either. The historical record makes the prospects for one look rather dim. The whole notion of verisimilitude may have to go the way of, say, 'simplicity'.

(D) The third sense of success (i.e., the making of novel predictions) seems also to be promising. Predictions about the future which turn out to be true are not just lucky guesses on the realist's account. These predictions are deduced from the truth, says the realist, so it is no wonder the 'guesses' panned out. There is no rival explanation for this; the Darwinian explanation didn't even try to account for it. In Laudan's very detailed attack on convergent realism[13] there is very little mention of this sense of success. So it remains, it seems to me, something the realist might point to as a genuine accomplishment, something the anti-realist fails to do justice to. But how strong is this? How much support does this give to the realist? Unfortunately many theories now thought to be false made true novel predictions. Ptolemaic astronomy, for instance, predicted eclipses fairly accurately. And Fresnel rather surprisingly got right his prediction of a bright spot in the middle of a shadow cast by a disk. Being true is not necessary for making successful predictions.

(E) It is the first sense of success that seems the least promising for the realist; and the reason is obvious. *Ad hoc* moves can always be employed to do justice to the known phenomena. It is very easy to suspect that this could be going on here. Moreover, the historical record is full of theories which were successful but false, or theories which were unsuccessful but (by out present lights) true. Truth is neither a necessary nor a sufficient condition for success in the first sense, so its explanatory prospects seem dim. This assessment, though it seems obvious, may not be right. In fact, it is with the first sense of success that the realist may have the most hope.

It is hard to say why realist accounts of the success of science have gone wrong. Of course, one answer is that realism itself is wrong. But this is an answer I am loath to accept, so before I do I want to explore at least one different kind of approach to the problem. What realists need, I suggest, is a different style of explanation entirely. I will now try to spell this out, if only briefly, in the balance of this paper. I stress the tentative, exploratory, and sketchy nature of the proposal below; it is intended merely as a beginning.

The last three decades have seen considerable quarreling over the form of a proper explanation. The dominant theory has been the so-called deductive-nomological or covering law model proposed by Hempel. For probabilistic situations there is the so-called inductive-statistical account.* Either way, on Hempel's view, an explanation is an argument. Given the explanans, the explanandum is shown to have been expected. (In the deductive case it is certainly expected and in the inductive case the explanandum is expected with high probability.) In short, an explanation is a sufficient or *almost* sufficient condition for what is being explained.

Here lies the difficulty. The preceding considerations show that truth is neither a necessary nor a sufficient condition for the success of science.

* See Carl Hempel, "Two Basic Types of Scientific Explanation" and "Inductive-Statistical Explanation" in chapter 6.

It does not meet the Hempelian conditions at all. And since it is not even close to being sufficient we cannot subsume it under the inductive-statistical version of the covering law model either. But the idea that it might have something to do with statistical considerations is, I think, an idea worth exploring.

Wesley Salmon has proposed[14] an account of explanation which rivals the covering law account of Hempel. An explanation is not an argument for a conclusion, it is instead the marshalling of the statistically relevant facts which have a bearing on the outcome. His view was introduced to cope with examples such as this: 'Why does Jones have paresis?' Explanation: 'Because he had syphilis.' This seems intuitively like a good explanation, yet the outcome, Jones's paresis, is not likely at all. The chances of getting paresis are very small with syphilis, but larger than they would be without it. Having syphilis, says Salmon, is statistically relevant; that is why it explains Jones's paresis. (According to Salmon, A is statistically relevant to B if and only if $\mathrm{Prob}(B/A) \neq \mathrm{Prob}(B)$.)[15]

We know that false premisses can yield true conclusions, so the truth is not (logically speaking) necessary for success. The reason truth is not sufficient for success is because of the presence of auxiliary assumptions which are also at work in any explanation. Though truth is neither sufficient nor necessary for success, it is, I shall say following Salmon, statistically relevant. The truth matters to the outcome, though it only matters a little.

Salmon's statistical relevance model is not the only challenger to the Hempelian account. Some philosophers of biology[16] and other philosophers of history[17] have advocated a narrative style of explanation. An event or condition is explained by telling a story in which it is embedded. In this way the explanandum is said to be rendered 'intelligible'. It is often claimed that Darwinian evolution, for instance, is unable to satisfy the Hempelian form, but that it is explanatory nevertheless. It provides neither necessary nor sufficient conditions, but it succeeds in some sense or other in explaining things.

Consider a brief example: Why does the giraffe have a long neck? Explanation: The ancestors of the modern giraffe fed on trees, and those with long necks were able to reach more when food was scarce (such as in the occasional drought which may have occurred.) There might have been survival value in having a long neck, so there was, consequently, differential selection in its favour. Is this meant by the evolutionist to be true? Not with any degree of confidence. It is only meant to be an evolutionary ***possibility***, one of the many courses that nature ***might*** have taken.

Narrative explanations are very similar to statistical relevance explanations. Neither provide necessary or sufficient conditions for what is being explained. What both do, however, is provide something which is relevant to the outcome. There is also a difference between them. The statistically relevant information in, for example, the Jones paresis case is the *known* fact that Jones had syphilis. In typical narrative explanations the statistically relevant fact in the explanation is not known to be true. It is conjectured. (We might, if we wanted to coin a barbaric phrase, call the combined view 'the hypothetico-statistical relevance model of explanation'.)

My suggestion now is simply this: The realist has an explanation for the success of science: Truth is the explanation and the style of the explanation is narrative. The truth is not known to obtain; it is hypothetical. But even if it did obtain, success would not automatically follow. The presence of the truth does make a difference, however; truth is statistically relevant.

The Hempel model of explanation was tied to confirmation. By deducing the data from the theory the theory explained the data and in turn the data confirmed the theory. Alas, this is not the case here. Saying that a theory is true does not lead to any testable predictions over and above those already made by saying that the theory is empirically adequate. There is no additional predictive power to this sort of narrative explanation. But even though predictive power is lost, this does not lead to the demise of the claim to have explanatory power. We cannot predict why a radio-active atom decays at the precise moment that it does; but after it happens we can explain it. The fact that the quantum theory can give such *post hoc* explanations does count in its favour, though only very little. The explanatory power of truth is similar.

It may sound as if the power of truth has become pretty vacuous. We all know the story about a scientist who was asked by an assistant how the theory explained some puzzling data. 'That's easy.' the scientist said, and proceeded to give an account. Later the assistant rushed anxiously back in and reported that the actual data were quite contrary to the earlier report. 'Oh', said the scientist, 'That's even easier to explain.'

Things are not quite that bad. But then, the explanatory power that truth does have in accounting for the success of science is not of the sort to make us believe in realism. For that we will need other considerations, such as, say, the (*a priori*) common cause argument of Salmon,[18] or Hacking's intervening arguments.[19*] We cannot rely on the success of science as characterized above.

In most explanations there is a connection to justification. That is why Popper does not want truth to explain success. But there are also

* For Salmon's appeal to the principle of the common cause in defense of scientific realism, see Wesley C. Salmon, *Scientific Explanation and the Causal Structure of the World* (Princeton, N.J.: Princeton University Press, 1984), ch. 8. "The common cause principle states, roughly, that when apparent coincidences are too improbable to be attributed to chance, they can be explained by reference to a common causal antecedent" (Salmon, 158). Hacking's intervening argument for realism about theoretical entities can be found in "Experimentation and Scientific Realism," which is the next reading in this chapter.

explanations which are not linked to justification and that, I think, is what is going on here. We show how, given realism, the success of science is possible, why it is not a miracle. But the style of the explanation does not let us infer its correctness.[20]

■ | Notes

1. *Objective Knowledge*, Oxford: Oxford University Press, 1973, p. 23.

2. *Ibid.*, p. 204.

3. Smart, *Between Science and Philosophy*, New York: Random House, 1968, p. 39.

4. B. van Fraassen, *The Scientific Image*, Oxford: Oxford University Press, 1980, p. 39f. [Excerpted in this chapter, 1064–87]

5. H. Putnam, *Philosophical Papers*, vol. 1, Cambridge: Cambridge University Press, 1975, p. 73.

6. W. Newton-Smith, *The Rationality of Science*, London: Routledge and Kegan Paul, 1981, p. 196.

7. *Ibid.*, p. 198.

8. *Ibid.*, p. 204.

9. Laudan, 'A Confutation of Convergent Realism', *Philosophy of Science*, 1981, p. 45. [Excerpted in this chapter, 1114–35]

10. See Laudan, 'Sources of Modern Methodology', reprinted in his *Science and Hypothesis*, Dordrecht: Reidel, 1981.

11. Oxford: Oxford University Press, 1983. The idea is that laws and theories generally are false, but the things they talk about, electrons, genes, etc., are quite real and certainly exist, according to Cartwright.

12. J. Leplin, 'The Historical Objection to Scientific Realism', Asquith and Giere (eds.) PSA *1980*, vol. I.

13. Laudan, 'A Confutation of Convergent Realism', *loc. cit.*

14. See his 'Statistical Explanation' reprinted in Salmon (ed.) *Statistical Explanation and Statistical Relevance*, Pittsburgh: Pittsburgh University Press, 1971.

15. There are problems with this account; see for example the relevant discussion by Cartwright in *How the Laws of Physics Lie*, *loc. cit.* Salmon has further fine tuned his view in "Why Ask 'Why'?", *Proceedings of the American Philosophical Association*, 1978.

16. For example, Goudge, *The Ascent of Life*, London: George Allen and Unwin, 1961.

17. For example, Dray, *Philosophy of History*, Engelwood Cliffs: Prentice-Hall, 1964.

18. "Why Ask 'Why'?", *loc. cit.*

19. *Representing and Intervening,* Cambridge: Cambridge University Press, 1983.

20. This paper was presented to the philosophy of science conference in Dubrovnik, Yugoslavia, April 1984. It has benefited from the comments of those present. Research supported, in part, by a grant from S.S.H.R.C.

IAN HACKING

Experimentation and Scientific Realism

Experimental physics provides the strongest evidence for scientific realism. Entities that in principle cannot be observed are regularly manipulated to produce new phenomena and to investigate other aspects of nature. They are tools, instruments not for thinking but for doing.

The philosopher's standard "theoretical entity" is the electron. I shall illustrate how electrons have become experimental entities, or experimenter's entities. In the early stages of our discovery of an entity, we may test hypotheses about it. Then it is merely an hypothetical entity. Much later, if we come to understand some of its causal powers and to use it to build devices that achieve well understood effects in other parts of nature, then it assumes quite a different status.

Discussions about scientific realism or anti-realism usually talk about theories, explanation and prediction. Debates at that level are necessarily inconclusive. Only at the level of experimental practice is scientific realism unavoidable. But this realism is not about theories and truth. The experimentalist need only be a realist about the entities used as tools.

A Plea for Experiments

No field in the philosophy of science is more systematically neglected than experiment. Our grade school teachers may have told us that scientific method is experimental method, but histories of science have become histories of theory. Experiments, the philosophers say, are of value only when they test theory. Experimental work, they imply, has no life of its own. So we lack even a terminology to describe the many varied roles of experiment. Nor has this one-sidedness done theory any good, for radically different types of theory are used to think about the same physical phe-

FROM *Philosophical Topics* 13 (1982): 154–72.