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## Explaining the Success of Science: Beyond Epistemic Realism and Relativism<sup>1</sup>

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### 1. Introduction

Throughout the first half of the eighteenth century, scientific opinion concerning the structure of the cosmos was deeply polarized; numerous "systems of the world" found their advocates among prominent natural philosophers, but the two leading rival systems were those of Descartes and of Newton. Cartesian physics held sway in France and on much of the rest of the continent; Newton's reigned in England. The young Voltaire journeyed from Paris to London in the spring of 1727. He was confused by the contrasting world-views he found. With an acute case of culture shock, he wrote to a friend back home:

A Frenchman who arrives in London finds a great shift in scientific opinion that makes the mind weary. He left the world full; he finds it empty. At Paris you see the universe composed of tiny vortices of subtle matter; in London we see nothing of the kind. . . . With the Cartesians, all change is explained by collisions between bodies, which we don't understand very well; with the Newtonians it is done by an attraction which is even more obscure. In Paris you fancy the earth's shape like a round melon; at London it is flattened on the two sides.<sup>2</sup>

One gets the same dizzying and disorienting feeling in our time if one moves between circles of philosophers and sociologists of science. Many, perhaps most, philosophers in the analytic tradition (and especially philosophers of science), take it for granted that science is, at least in its essentials, largely true and substantially correct. These philosophers argue that, especially in the "mature" and well-developed parts of the physical sciences, scientists have come very close to discerning the way the world *really* is. Our theories about such matters are, they say, highly verisimilar. Even where science turns out not to be strictly true, most philosophers (present writer included) are still apt

to consider science as our best exemplification of rationality and cognitive progress—our best guess as to how things stand.

Sociologists, by contrast, especially sociologists of knowledge, tend to see science differently. Many of them regard scientific theory, like science itself, simply as a social construct, a set of conventions which Western culture since 1700 has used for conceptualizing experience, but which has no particular purchase on reality. Every culture, they point out, has its myths and its sacred beliefs; we happen to call ours by the name 'science'; but those beliefs are no better, no more secure, objective, or rationally grounded than the guiding ideologies of other cultures. These two points of view are known as 'realism' and 'relativism' respectively. The pair of them and the injustices that each does to an understanding of science will form the foci of this paper.<sup>3</sup>

But before I turn to that task, one crucial qualification is in order concerning the compass of this essay. Both realism and relativism have received numerous (and often conflicting) formulations by a wide variety of writers. While there may be fewer realisms and relativisms than there are realists and relativists respectively, it is a close call. There are many species of both these groups which I shall not be discussing in this essay. Perhaps the best way of locating my concerns is to say that I will be grappling with the *specifically epistemic* formulations of realism and relativism. Equally familiar to most readers will be various *ontological* versions of both realism and relativism. I must emphasize at the outset that the latter, metaphysical theories are *not* the targets of my criticism. My preoccupation in this essay with epistemic and methodological matters should explain why I shall have little to say about many varieties of relativism (e.g., Quine's ontological relativity) which might otherwise be expected to occupy center stage in a critique of relativism.

Epistemic realism or, to be more precise, 'scientific realism', has been the reigning orthodoxy among philosophers of science for almost a generation. Philosophers as diverse in orientation as Popper, Grünbaum, McMullin, Sellars, Reichenbach, and Putnam have espoused one or other version of it. Its rival, epistemic or cognitive relativism, has found occasional philosophical advocates (e.g., Feyerabend, the later Wittgenstein, Hesse and Rorty) but cognitive relativism—at least in that variant of it which I shall treat here—is associated primarily with work in the sociology of knowledge. Mannheim, Durkheim and Kuhn have developed what are probably the three most familiar versions of this species of relativism.<sup>4</sup>

Both realism and relativism are theories of knowledge in the broadest sense, and both have complex ramifications for our understanding of science. To put it briefly, the realist insists that science, in the course of its develop-

ment through time, provides us with an ever more accurate, an ever more nearly true, representation of the natural order. Scientific theories, if not strictly true, are nearly so; and later scientific theories are closer to the truth than earlier ones. More than that, the realist typically asserts that science or the scientific method represents the *only* (or, more weakly, the *most*) effective instrument for discovering truths about the world. The relativist, by contrast, characteristically eschews notions of truth and falsity, focussing rather on the specific and local features which shape (and, in his view, inevitably distort) the scientific image of the world. The relativist would have us believe that science is but one among indefinitely many ways in which man might represent the world; in his view, it has no special claim to validity or veracity. If we lived in a different place and time, says the relativist, we would have a fundamentally different vision of the natural order. Still worse, there is—the relativist maintains—no neutral point on which we can stand to adjudicate impartially the rival claims of these contrasting images of the world, the scientific and the nonscientific. Because we ourselves are products of a scientific culture, we cannot step outside the presuppositions of that culture to compare the legitimacy of its claims with those of nonscientific cultures. Where the realist sees the history of science as a triumphal march ever closer to the truth, the "cutting edge of objectivity" (in Gillispie's apt if notorious phrase), the epistemic relativist sees nothing more than a succession of rival and mutually incompatible representations, each reflecting various subjective and transitory interests. Where the realist sees progress in the history of science, the relativist sees only change. The realist believes that science comes as close to truth and objectivity as is humanly possible; the relativist fears that he is probably right! But they draw very different conclusions from this one point of consensus.

There is nothing especially new about this polarity. Struggles between realist and relativist perspectives span the entire history of epistemology. Precisely because of their age-old opposition, there is a tendency to see these doctrines as mutually exhaustive rivals. Any weakness in relativism (e.g., its allegedly self-indicting character) is translated into an argument for realism; while any flaw in realism (e.g., the unsatisfactory status of realist semantics) comes to be widely regarded as evidence for relativism.

A number of considerations move me to take strong exception to the view that these two doctrines more or less exhaust the range of alternatives open to us. Both seem to me to be fundamentally flawed and open to anomalies which are beyond their resources to grapple with. But more important, each fails to resolve one of the most central conceptual questions about sci-

ence. In a nutshell, that question is simply: why does science work so well? In what follows, I shall seek to show:

- 1). that the realist recognizes the importance of this question but fails to answer it;
- 2). that the relativist is scarcely prepared to grant the legitimacy of the question, let alone to answer it;
- 3). that the question can be interestingly answered, provided that we are prepared to lay aside some of the core assumptions associated with both realism and relativism.

## 2. Establishing the Phenomenon: The Success of Science

As every student of scientific controversy understands, one man's fact is another man's fiction. (Recall, if you have any doubts, the difference between London and Paris in 1727). Nowhere is this difference in "perception" more marked than with respect to the question of the success of science. Many of us incline to the view that nothing could be more obvious than the fact that science is a successful and effective knowledge-gathering enterprise. We may be unsure how to account for that success and our efforts to *characterize* it precisely have not been very illuminating (witness the failure of the many inductive logics and theories of confirmation); but our intuition remains unshaken that science does what it does very well indeed. It is quite another matter, however, where sceptics and relativists are concerned. It is not clear whether they positively deny that science is successful; in general, they simply do not reckon the achievements of science to be something which they are called upon to explain. Insofar as they deal with the phenomenon at all, it is to point out that the "success" attributed to science is of an ambiguous and amorphous sort. "Successful according to whom? and for what purposes?" they ask. "Successful compared to what?" "Successful by which standards?" To put the most sympathetic gloss I can on the relativists' failure to grapple with the problem of the success of science, I would say that relativists are inclined to withhold judgment on the claim that science is successful for these reasons (among others): (1) a belief that, until the notion of success is spelled out with some care, the concept is too unclear to be worthy of systematic analysis, let alone explanation; and (2) a lingering suspicion that 'success' is an evaluative rather than a descriptive term which should play no role in an empirical and naturalistically-based sociology of knowledge.<sup>5</sup>

What I want to do in this section is to meet that challenge by describ-

ing some notion(s) of success which should allow both relativists and realists to grant that science is, more than occasionally, successful. We can then proceed to explore the resources of realism and relativism respectively for accounting for that success.

We must begin by freeing 'success' of some of its more normative and judgmental overtones. As I shall be using the term, judgments of success in an activity imply no endorsement of that activity. There can be successful bank robbers, rapists, military campaigns, or scientific theories. One may or may not regard science and technology as forces for good; but no such evaluation is presupposed or implied by my claim that science is a successful activity. In the most general sense of the term, success in any activity always has to do with relations between ends and means and, more specifically, between aims and actions. To say that an activity is successful is simply to say that it promotes the ends of (at least some of) those engaged in it (or, and this is an important codicil, of those judging it to be successful). Just as we say before the fact that an action is rational if the actor has good reason to believe that it will achieve his goals, so do we say *post hoc* that an action is successful just insofar as it actually furthers some agent's goals.<sup>6</sup> Putting it this way makes it clear that success is a *relational* concept. Because agents' goals can differ, one and the same action may be unsuccessful or successful, depending upon the goals in question.

Of even greater importance is the fact that success, so conceived, is not a valuational or a normative concept. To claim that a certain action was successful is to make a contingent, empirical claim about the relation of that action and its outcomes to certain goal states. Claims about success are thus (at least in principle) as factual and as testable as any other sort of empirical claim about the world. (Although one must concede the fairness of the relativist's charge that, in practice, 'success' is too often treated as a primitive term.)<sup>7</sup>

Accordingly, the thesis that science is successful (or unsuccessful) amounts to the empirical assertion that the actions of scientists have in fact brought about or otherwise promoted (or failed to promote) certain goals or aims. But with respect specifically to which goals is science to be judged successful or unsuccessful? This question is both simpler and more complex than it might first appear. It is simpler because, unlike judgments about the rationality of an action, judgments of success do not require a scrutinizing of an agent's aims or motives. We can ask whether an agent's actions in fact brought about certain outcomes, quite independently of whether those outcomes were the ones which the agent intended to achieve. Just so long as we make it clear what outcomes we regard as constituting 'success', we can happily make de-

terminations of success or failure without skating on the comparatively thin ice of attributions of intentionality to agents. Of course, we will often be interested to know whether an action is successful specifically with respect to (what we take to be) the goals of the agent who embarked on the action. But making determinations of 'success' parasitic on the agent's goals is not necessary, and in this particular case is almost certainly not desirable.<sup>8</sup> Through time, scientists have had a highly heterogeneous set of cognitive aims or goals.<sup>9</sup> There almost certainly is no such thing as *the* aims of the scientific community, any more than any other large and diverse group has universally shared and univocal aims. When we say that science has been successful (at least those of us who are prepared to venture such a conjecture), we do not usually even bother to engage in a detailed analysis of the goals or aims of all or most of the actors who have constituted the scientific community. Rather, we typically *impute* certain goals to a highly idealized caricature of the scientist (or, even more abstractly, to science as an institution) and then ascertain whether science has achieved those goals. It seems to me that it would be a more forthright and intellectually honest approach to admit that, at least for these purposes, we are not trying to ascertain whether science has managed to achieve what, as a matter of fact, working scientists have always or invariably been trying to achieve. That is certainly an interesting question, but it is not the most important question for epistemic or methodological purposes. Rather, we should say straight out that, when we are judging whether science is or has been successful, we are going to be making determinations of success with respect to the ability of science to achieve certain cognitive attributes which we find especially interesting. As long as we acknowledge what we are doing, we can avoid vexed questions about intentionality, incompatible goals, shifting explanatory ideals, and the host of other difficulties which confound efforts to ascertain what the aims of science have actually been through history. By taking this route, we can make our task a good deal easier than it otherwise would be.

What makes the task rather more complicated than one might expect is the necessity of spelling out clearly and precisely exactly what criterion of success we are utilizing. Since virtually any action will have some consequences or other, some outcome or other, it is always possible after the fact to find some set of descriptions under which any particular action can be made to appear to be successful. Such an approach would obviously trivialize the undertaking. Accordingly, we need to find an interesting, unusual, and demanding set of outcomes, with respect to which we will proceed to make judgments of success or failure. (There is obviously no unique set of that sort.) But there

is one set of cognitive outcomes which has interested epistemologists and philosophers of science for a long time. These goal states concern themselves with certain interesting epistemic and pragmatic attributes. Consider a typical list of some of those aims:

- a). to acquire *predictive control* over those parts of one's experience of the world which seem especially chaotic and disordered;
- b). to acquire *manipulative control* over portions of one's experience so as to be able to intervene in the usual order of events so as to modify that order in particular respects;
- c). to increase the *precision* of the parameters which feature as initial and boundary conditions in our explanations of natural phenomena;
- d). to integrate and *simplify* the various components of our picture of the world, reducing them where possible to a common set of explanatory principles.

If we define cognitive 'success' along these lines, then it seems uncontroversial to say that portions of the history of science in the last 300 years have been a striking success story. For instance, we are now in a position to predict a much broader range of phenomena than we were in 1700. We can intervene in the natural order (e.g., with respect to the course of many diseases) so as to make things go more to our liking far more effectively than we could formerly. Our instruments for measuring various variables and constants are incomparably more precise than they once were. (Consider, for instance, the refinements in the last two hundred years of determinations of the velocity of light.) Finally, even if the ultimate unification of science still eludes us, it is quite clear that we can now explain a more diverse set of phenomena in terms of a smaller number of general principles than our forebears could.

In saying that science has been successful in this cognitive sense, I am certainly not claiming that science has managed to achieve all the goals of all of its practitioners. Nor am I making any judgment about whether, all things considered, science is worthy or admirable. At least for purposes of this analysis, we need make no judgment about the moral or social value of the sorts of outcomes which science has achieved. I am here simply noting certain facts, and very striking facts they are, about the diachronic development of science. Because these facts are so striking, because there was no reason *a priori* to expect man to be able to achieve such cognitive feats, because no undertaking can guarantee success of *this particular sort*, we are confronted with a genuine problem: why is science so successful? What is it about the manner in which scientists formulate and test their theories which makes this sort of success possible?<sup>10</sup>

Any account of science which fails to answer, or even to address, such questions is (to put the criticism in its mildest form) fundamentally *incomplete*. Whatever one's disciplinary or philosophical orientation, one cannot pretend to be accounting for, or explaining, science in a comprehensive manner unless one has an answer to such questions as these. As we shall see, such is the sorry state of both realism and relativism.

### 3. Realism and Success

By and large, scientific realists have recognized the pivotal importance of the problem of explaining the cognitive success of science. Indeed, especially in the last few years, numerous realists have claimed that one of the chief arguments in favor of realism is precisely that it, allegedly unique among rival epistemologies, can explain why science is successful. Hilary Putnam, for instance, asserts that "the positive argument for realism is that it is the only philosophy that doesn't make the success of science a miracle".<sup>11</sup> McMullin, Newton-Smith, Boyd, and Niiniluoto have made similar claims on behalf of realist epistemology.<sup>12</sup> In this section, I want to examine briefly the claims of contemporary realism to be able to explain the success of science.

Realists argue that scientific theories, at least in such "mature sciences" as physics, are approximately true and that the central terms in such theories genuinely refer to objects in the physical world. They go on to insist that the approximate truthlikeness of our theories (and the related authenticity of reference exhibited by their central concepts) explains why science works as well as it does. Our theories are successful, the realist maintains, precisely because they come close to representing things as they really are.

What the realist is trading on here is the perfectly sound intuition that if our theories were true unqualifiedly, then all their consequences would likewise be true; and if all those consequences were true, we would indeed expect theories to exhibit just that sort of predictive accuracy and reliability which (on the account mentioned earlier) constitutes "the cognitive success of science".<sup>13</sup> Sadly, the realist is enough of a "realist" (in the hard-headed, ordinary language sense of that term), to recognize that the relevant antecedent conditions in this intuition are unsatisfied. We have overwhelmingly good reasons to suspect that our theories about the world, even our best-tested ones, are not true *simpliciter*. Yet the realist still wants to cash in on the hunch that the "truthlikeness" of our theories is responsible for their success. Accordingly, the realist maintains that, even if our best theories are only approxi-

mately true, or nearly true, then he is in a position to explain the success of science. The core idea here is that an approximately true theory will have consequences *most* of which are true, or at least which are close to the truth.

As I have shown elsewhere in detail,<sup>14</sup> this argument is fundamentally flawed. There is as yet no coherent sense of 'approximate truth' which entails that an approximately true theory will be uniformly successful in *any* of the senses sketched above. We can put the point more strongly: there is as yet no semantic account of truthlikeness which entails that a theory, all of whose central explanatory claims are approximately true, will be any more successful than a theory whose central explanatory claims are wildly inaccurate. It is entirely conceivable, for instance, that a theory might be approximately true, in any explored sense of the term, and still be massively inaccurate in those domains where it can be tested. Indeed, on the best-articulated sense of truthlikeness (namely Popper's theory of verisimilitude), it can be shown that a theory may have a high "truth content" yet have all its observable consequences false. To make a long story short, it can be shown that—because there is no reason to believe that approximately true theories need be (or are even likely to be) empirically successful—the near-truthlike status of theories—even assuming we had an epistemology which would warrant such attributions of truthlikeness—cannot be invoked to explain their pragmatic success.

But the situation is even gloomier than this for the realist. As even a brief glance at the history of science will show, there are many theories which have been highly successful for long periods of time (e.g., theories postulating spontaneous generation or the aether) which clearly have not been approximately true in terms of the deep-structure claims they have made about the world. Thus, Newtonian optics—which predicted a wide variety of phenomena, and which inspired the construction of a plethora of successful instruments and measuring devices—was committed to a basic ontology of light which (so we now believe) is desperately wide of the mark. Because there seems to be nothing in the world which even approximately corresponds to Newtonian light corpuscles, it is clear that Newton's theory was not, indeed could not have been, approximately true. (Assuming that, as I have argued elsewhere,<sup>15</sup> the scientific realist's notion of approximate truthlikeness presupposes genuineness of reference.) How then is the realist to account for the success of Newtonian optics? Even assuming that the realist could show that an approximately true theory would be successful (which he cannot), how can he explain the success of a theory, like Newton's, which is—by his lights—not even close to the truth? The same goes in spades for most theories in the history of science. Because they have been based on what we now believe

to be fundamentally mistaken theoretical models and structures, the realist cannot plausibly hope to explain the empirical success such theories enjoyed in terms of the truthlikeness of their constituent theoretical claims. So it appears that the realist is in a bind. Theories which are approximately true need not be successful; many theories which have been strikingly successful are evidently not approximately true. Under such circumstances, truthlikeness is a decidedly unpromising explanans for empirical success.

But the problems facing the realist go even deeper than this. There is a crucial ambiguity in the problem of scientific success, an ambiguity which highlights another weakness in the realist approach to that problem. When we ask why scientific theories work so well, we might be asking (and the realist response assumes as much) to be told what semantic features theories possess in virtue of which they have such an impressive range of true consequences. Alternatively, when we ask why science is successful, we might be asking an epistemic and methodological question about the selection procedures which scientists use for picking out theories with such impressive credentials. If, as I suspect, it is generally the latter which we are driving at, then the appropriate response to that problem will address itself to the probative and evaluative procedures which scientists use for identifying those theories which are likely to be reliable. And insofar as that is what is at stake, the realist response becomes even less availing than it already appears to be; for the realist's "explanation" of success (viz., theories work because they are true or nearly true) sheds no light whatever on how scientists come by these putatively true or truthlike theories. Because the realist makes no reference to the methods of investigation and warranting which scientists use for selecting their theories, he must leave that side of the question unaddressed. That side of the problem of success remains a mystery on realist principles, even if the realist can get his theory semantics in order.

I will conclude this section by offering several caveats. I have asserted here neither that realist epistemology is wholly irrelevant to the explanation of scientific success nor that realism's failure to explain the success of science is a disproof of realism. (Although that failure does raise serious questions about whether realism has any empirical content.) What I have insisted is that current realist approaches to this issue provide little more than pseudo-explanations of the success of science. Beyond that, I have suggested that realists have largely missed the point about the success of science, for they have failed to see that what is chiefly called for is an epistemic analysis of the methods of theory testing rather than an account of theory semantics.

#### 4. Relativism and Success

There are numerous variants of relativism (cultural, historical, and epistemological among others). What most seem to have in common is a conviction that no method of inquiry can claim special or privileged status. Different cultures, different societies, different epochs will exhibit conflicting views about the appropriate ways of authenticating beliefs. Confronted by these differences, the relativist insists on remaining agnostic about the respective merits of different methodological and evaluative strategies for testing claims about the world. More than that, the relativist generally denies in principle that there can be any way of showing one doxastic or belief-forming policy to be superior to another, or one set of methods to be objectively preferable to another.<sup>16</sup>

The relativist circumvents the problem of success by writing it off his explanatory agenda. As he sees it, his task is the descriptive and explanatory one of explaining why agents believe what they do. He will thus quite happily offer us an explanation for why a particular scientist or group of scientists believes that their theories are successful. But what he is reluctant to confront head-on, in part because he mistakenly imagines it to be a purely normative and philosophical puzzle, is the question why certain theories or beliefs are, in fact, successful.

Indeed, many latter-day relativists explicitly repudiate any effort to acknowledge that certain systems of belief have been more successful than others. David Bloor, Barry Barnes, and a host of other sociologists of knowledge have argued for such agnosticism;<sup>17</sup> so too have such philosophers as Paul Feyerabend. My aim in this section is to explore the relations between relativism, so understood, and the problem of the success of science.

As I said earlier, this reluctance to grapple with the success of science is due, in part, to the relativists' failure to recognize that 'success' can be characterised in a thoroughly descriptive rather than an evaluative way. Equally, they have been understandably skeptical about approaches which assume that all human agents have the same cognitive goals. As the relativist sees it, terms like 'success' and 'failure' smack of cultural chauvinism because they seem to suggest that all human actors have the same aims. But if one takes seriously the arguments offered above, it becomes clear that the claim that a certain piece of science is successful relative to certain aims is not tantamount to the claim that all agents (or even all scientists) have the same aims. It is simply the empirical claim that the developments in science have in fact promoted certain aims. Since relativists do grant that there are some features of science



which can be treated as data to be explained or accounted for, the relativist who refuses to countenance the success of science (so understood) must explain why the success of science is any less a fact about science than, say, that science is a social activity or that scientists use certain mechanisms for generating consensus.

But the relativist's uneasiness about the seemingly judgmental aspects of 'success' is only a part of the story. I submit that a second factor which encourages the relativist to finesse the issue of explaining why science is successful is his realization that his epistemology lacks the explanatory resources to give a plausible analysis of that success. Indeed, the fact that science is so successful constitutes a powerful anomaly to relativism; not because the success of science refutes relativism, but rather because it points up its descriptive and explanatory incompleteness as an empirical theory of the scientific enterprise.<sup>18</sup> Like some Victorians, who hoped venereal disease would go away if no one mentioned it, the relativist apparently thinks that aloof indifference to success is the preferred vehicle for wishing it away. Let me explain why I make this charge of incompleteness against relativism.

Consider one specific part of the success of science, its predictive ability. With respect to many sorts of phenomena, it is quite clear that science puts us in a position to anticipate what the world will do next with a rather higher reliability than can many systems of belief commonly regarded as nonscientific.<sup>19</sup> I submit that we have enormous support for the success thesis in that form. Let me particularize it still further in terms of a familiar example. In virtually every society which cultivates its own food, there is (what Habermas has called) a "technical interest" in anticipating when floods will occur, in judging their intensity, and in taking appropriate action to control the damage they wreak. Every agrarian and post-agrarian society has means for anticipating when and where major rivers will overflow their banks. Let us suppose, for the sake of argument, that modern Western scientific techniques yield predictions which are both more detailed and more accurate with respect to the phenomena of river flooding. If, as I believe, this greater accuracy could be convincingly established (even to those who were not products of Western culture), then we would be confronted with a situation where different cultures have a common technical interest in predicting a certain sort of phenomena and where scientific culture, by the standards of all concerned, yields more accurate predictions.<sup>20</sup>

What is the relativist to say about such a case? Well, what most of us non-relativists would say is that science has available certain methods of theory selection and theory testing which, over the long run, tend to pick out theo-

ries of high reliability. We might go on to point out precisely why one would expect methods of the sort scientists use to yield fairly reliable theories.<sup>21</sup> In the case in hand, we could contrast the methods of theory evaluation used in scientific predictions of rainfall and water run-off quantities with those rule-of-thumb methods utilized for generating predictions about flooding in non-scientific societies. I expect we would be able to show that our methods of theory selection are more robust than those of other cultures, thereby explaining why our theories about flooding were more efficacious in producing reliable predictions than the theories used by other societies.<sup>22</sup>

But, of course, such explanatory manoeuvres are not open to the relativist, for he denies that any methods can objectively be said to be better than any others.<sup>23</sup> Precisely because he makes that denial, he is in no position to cite the superior methods of science as the explanation of the greater predictive success of science compared to other forms of knowledge.<sup>24</sup> Since the relativist cannot explain the predictive superiority of science by invoking the greater rigor or robustness of its methods, what is he to say? Well, he might say that it is just a large cosmic coincidence that science is so successful; that the success of science reveals nothing about, and owes nothing to, the specific methods of inquiry used in science. But this is not to explain the success of science; it is, rather, to renounce any effort to account for that success.

I have suggested two causes for the relativist's reluctance to grapple with scientific success; one concerned the relativist's explanatory agenda (i.e., his uneasiness about "evaluative" concepts); the other, his limited explanatory resources. But I think that we must probe still further before we fully understand why many relativists are so reluctant to acknowledge the success of science as a datum to be explained. For more than a decade, relativist sociologists have been committed to the idea that the same sort of institutional analysis which they offer for other social structures and systems of belief (e.g., religion or the kinship system) can be applied indifferently to science. In their view, science is simply one among many institutions for the formation and perpetuation of beliefs. These new-wave sociologists have thereby sought to distance themselves from the older sociological tradition (e.g., associated with Robert Merton, among others) which tried to establish the cultural or sociological *uniqueness* of science as an institution. As soon as the relativist grants that science has been cognitively more successful than many other belief-building enterprises, then he can no longer argue for a monolithic or unitary account of cognitive practices. Put differently, the latter-day sociologist of knowledge wants to reduce the sociology of knowledge to the sociology of belief, and to conjoin that reduction with the thesis that the problem of explaining belief-

or consensus-maintenance is to be handled in a unitary fashion across all institutions and cultures. To maintain this homogeneity thesis, the relativist must either deny that science has been successful, or insist that it has been no more successful than any other system for the generation of action-related beliefs (or, finally, hold that the success of science is fortuitous). In either case, the relativist's denial that science is *sui generis* disposes him to deny that the success of science is a datum requiring special explanation.

I have ventured into this lengthy digression about the causes that have evidently pushed the relativist in the direction of ignoring the problem of the success of science only because, when we find a group of thinkers denying what most of us take to be obvious, we have to cast about for some explanation of their apparently pathological behavior. But however far one goes in trying to understand why relativists might want to avoid grappling with the problem of the success of science, the fact remains that it is a phenomenon which they have left unexplained. To that extent, relativism is radically incomplete as an explanatory theory about science.

If there is any plausibility in the arguments of these last two sections, we seem in fairly dire straits. Neither of the major epistemologies of our time seems to show much promise of handling one of the core intellectual issues of our time. The better part of valor might suggest that the problem of the success of science is simply intractable, a problem well beyond our limited explanatory capacities. It is, after all, conceivable, that—as Karl Popper once suggested<sup>25</sup>—why science works is just an insoluble problem which is best left well enough alone. But before we acquiesce too quickly in that view, and before the relativists and realists imagine that they are off the hook (for who can reasonably be expected to solve an insoluble mystery?), it is worth exploring briefly whether we really are in such a desperate position so far as explaining why science succeeds.

### 5. Accounting for the Success of Science

This is, of course, a tall order, and I have no intention of offering here a perfectly general solution to the problem. What I will do is to take one or two typical, if slightly idealized, cases of scientific success and offer a story about them, a story which will make it plausible why science works well in those circumstances. A different story would have to be told about other cases. But if the tale I have to tell is at all convincing, it will be easy to see how it could be adapted to a wide range of other situations.

But before I offer my narrative, I need to make some important disclaimers. It has often been assumed that the demand for an explanation for the success of science (i.e., an account of why science “works” so well) is really just a re-formulation of the hoary old problem of induction. And it is true that a solution to the riddle of induction, assuming one could be had, might well give us a solution to the problem of success. After all, if we could show under what circumstances it was reasonable to assume that unobserved instances of a generalization or theory will resemble observed ones, we would have shown the reasonableness of (at least some) inductive methods. I do not have a solution to the problem of induction; come to that, I do not regard it a particularly interesting problem in this form. The point I want to make here is that the problem of the success of science can be formulated in such a way that its solution does not require a prior solution to the problem of induction. We can see the independence of the two problems if we cast the problem of success in the following form: why is it that many of the theories of the natural sciences enable us to predict nature and to intervene in the natural order in ways we want to so much more frequently and more accurately than (say) the theories of the ancient Greeks permitted them to do? This is clearly a *comparative* version of the problem of success. Its solution does not require us, as the problem of induction apparently does, to show that our theories are always (or even usually) reliable guides to the course of nature. The problem of success requires us only to explain why certain sorts of theories, authenticated by certain sorts of probative procedures, tend to promote certain cognitive ends more effectively than other sorts of theories, grounded in other forms of legitimation, do. As I shall be construing the problem of success in this section, it is fundamentally the challenge of explaining why certain modes of knowledge authentication produce more reliable results than others do.<sup>26</sup> We can thus leave the problem of induction in its general form conveniently to one side.

So let us now turn to my pair of stories. For the first, let us imagine that I am having problems getting my car started on a cold morning. My mechanic hauls it into the garage and replaces the brake drums, returning the car to me the next day. In the meantime, the weather takes a decided turn for the better. I crank up the engine, and the car starts without difficulty. When the mechanic bills me for replacing my brakes, and I complain that he did not do what he was supposed to, he replies by pointing out that my car starts now, and that that was what I wanted all along. Moreover, he points out that *all* the cars in his shop that day suffering from ignition problems had their brakes replaced and invariably the problem was solved. In sum, he



claims that his tinkering with the brakes cured my starting problem, and cites as evidence for his claim the acknowledged fact that my car—along with the others suffering similar problems in his shop—now starts without difficulty. In exasperation, I explain to him that the state of the brake drums could have nothing whatever to do with the operation of the starter. He replies that such happens to be *my* theory about how cars work, but that *he* has a different theory, according to which brake wear can be a cause of poor ignition. Being a reasonable fellow of sorts, he cites as evidence for *his* theory the fact that the car started smoothly once the brakes were replaced. What am I to do in this case? Well, the first thing I might do is to point out that there is a different explanation than his for the sudden improvement in my starter's performance (namely, the warmer weather). Because there is, and because he hopes to get paid, he must show me some empirical evidence which supports his explanation of the starter's improvement rather than my meteorological hypothesis. If, moreover, I can point to plenty of other cars whose starting performance has improved dramatically with warming weather when no one was let loose on their brakes, my case is won. (At least as far as I and the Better Business Bureau are concerned.) Now, what is going on here? In effect, my mechanic and I are comparing *different probative strategies for the evaluation of beliefs*. My mechanic is evidently quite willing to shape his beliefs according to a simple *post hoc ergo propter hoc* policy. By contrast, I am insisting that discriminating tests be designed in order to rule out some of the many incompatible hypotheses which his strategy supports. (After all, my hypothesis is, like his, supported on *post hoc* grounds.) Beyond that, I can point to improved starting performance in other automobiles, whose brakes were not replaced. I deny that anyone who thinks carefully about these two strategies for the evaluation of empirical claims can have any doubts about which one is more likely to produce reliable results. My mechanic's failure to impose any form of experimental controls on his causal claims is likely to lead him to make far less reliable predictions than I will. In short, my strategy will save me from several sorts of failure to which my mechanic friend will sometimes fall prey. This is not to say that hypotheses which pass my sorts of tests will never be mistaken, nor that theories which pass his tests will never lead to correct predictions. It is simply to say that my strategy will produce conjectures which break down less frequently and less quickly than his will, and that is precisely why we say that one theory is more successful than another.

Consider, as a second example, the testing of a new drug said to be efficacious in curing arthritis. If I want to test its effectiveness, I might begin by enlisting a group of physicians who would prescribe the drug for their

arthritic patients. Suppose, in the first run of the test, the only evidence reported back to me is that 55% of those who took the drug reported a reduced level of pain 24 hours after the onslaught of an acute attack of arthritis. Well, what conclusion will I draw? I might, if I am hasty, pronounce the drug a qualified success. But if I am the least bit careful, I will draw no such conclusion whatever, for the test itself is very badly designed. For all I know, for instance, it might well be that 55% (or more!) of patients who take no medication whatever also report improvement after 24 hours.

So, in the second stage of the testing, we need to devise a more complex experiment. We might divide the patients into two groups, administering no drugs to one group and the drug being tested to the other. Suppose, after the experiments are performed, that it emerges that 55% of those given the drug reported improvement, while only 20% of those given no treatment reported improvement. Well, these results are rather more impressive, but again, we have to be careful about drawing any conclusions about therapeutic efficacy. We have introduced certain controls on the experiment, it is true, but we can still imagine all sorts of ways in which the reported results might be compatible with the fact that the drug is of no therapeutic value at all. Specifically, given what we know about the placebo effect and the psychosomatic character of pain, it may well be that patients given any pill, even a worthless one, will report an improvement—just because they expect medication to make them better and that expectation itself will sometimes have the desired effect. Because the control group was given no pill at all, the different results in the two cases might have nothing whatever to do with the specific character of the drug under investigation.

Realizing this, we re-design our test. For the third run of it, we give pills to *both* the control group and the test group, but only the administering physicians know that the control group receives sugar pills. Suppose the results of this experiment are as follows: the group given the real drug reports a 55% improvement in 24 hours while the group given the placebos reports a 30% improvement. Well, the evidence for therapeutic value is getting more impressive, but there are still causes for concern about the significance of the results. We have learned from many studies that those conducting experiments often have a way of transmitting their knowledge and their expectations to the human subjects on whom they are experimenting. There are all sorts of documented forms of conscious and unconscious suggestion that might be going on, even though the doctors are conscientiously trying to treat the two groups of patients identically. In short, the physicians might be conveying to their patients their knowledge of which pills are placebos, or the doctors might

be interpreting their patients' comments so as to support their own expectations. If we want a strict test of the drug, we need to set up a situation where those giving out the pills and interviewing the patients have no idea whether the patients they are dealing with have been given the real drug or the placebo. With suitable precautions, such an experiment can be devised; indeed, this technique now represents a standard part of the repertoire for assessing the efficacy of therapies of many sorts, whether drugs or psychoanalysis.

Those familiar with experimental design will recognize in the chronology I have described the transition from an uncontrolled to a controlled experiment, from a controlled but unblinded experiment to a single-blind experiment, and finally from a single-blind to a double-blind experiment. For *each* transition, we can lay out some good reasons to expect the results of the later test to be more reliable than the results of the earlier one. Everyone who studies experimental methods knows the story to be offered in each case. Thus, in the first test (where we gave everyone the drug), we were using the simple method of agreement. Once we introduced the control group, we were using the joint method of agreement and difference. Anyone who thinks about these two methods will realize why theories which stand up to the latter sort of test are more likely to endure than theories which pass tests associated only with the method of agreement. *Some* of the mistakes into which the unaided method of agreement will lead us can be guarded against by using the two methods in conjunction. Even those unfamiliar with scientific methods can surely see why, if our concern is to find out whether the specific drug being tested is efficacious, procedures such as control groups and blinding will allow us better control over many extraneous variables and influences which can creep into experimental design.

What such examples vividly illustrate is that we need not engage in "high epistemology" to understand what is going on and why. The comparative reliability of various testing procedures can be explained without resorting to the realist's ambitious claims about the truthlikeness of scientific theories.<sup>27</sup> We already have in hand an informal logic for testing causal hypotheses which will rationalize and justify many of the methods of the natural sciences. (Every good textbook on experimental design goes much further toward explaining why science works than all the writings of scientific realists put together!) So far as I can see, scientific realism is just not needed to give a viable account of those methods. The "logic" of theory testing, imperfect as it is, puts us in a position to make some comparative judgments about the reliability of various methods of inquiry and, via those judgments, we can explain why theories which pass certain sorts of tests tend to endure longer than theories

which pass other, less demanding sorts of tests. The explanation of the success of science, I submit, is no more mysterious and no more elusive than that.

This explanation of the success of science has the added virtue of being straightforwardly testable. It predicts, for instance, that where there are individuals or whole societies which shape their beliefs without the controls associated with science, those beliefs will be less reliable on the whole than the beliefs of a "scientific" culture. To be more specific, it predicts for instance that the medical practices of so-called primitive societies will tend to be less reliable and less efficacious than the practices of modern Western medicine (provided, that is, that physicians in those societies use less robust testing procedures for their theories of disease than their Western counterparts do—which may or may not be so). This is not to assert that non-scientific cultures can never discover "cures" which have eluded Western medicine, since weak heuristic and probative methods are sometimes capable of producing useful discoveries. The claim here would rather be that the frequency and reliability of such "discoveries" should be lower in societies which do not use controlled methods than in societies which do. I do not have the evidence at hand to confirm such predictions. My only point in making them is to show that the explanation of success offered here is distinctly non-vacuous.

If there are those, like the relativists, who refuse to accept this account, they must, for instance, counter the claim that a controlled experiment is an improvement on an uncontrolled one, and they must establish that a double-blind procedure is no improvement over a single-blind one. They must make plausible their claim that such experimental techniques are nothing more than socially-sanctioned conventions whose limited validity, such as it is, applies only to our culture and our time. They must show that we are just kidding ourselves in thinking that we have learned something in the last 300 years about how to put questions to nature.

## 6. Conclusion

Science is successful, to the extent that it is successful, because scientific theories result from a winnowing process which is arguably more robust and more discriminating than other techniques we have found for checking our empirical conjectures about the physical world. On a case-by-case basis, we can usually indicate why these methods and procedures are more likely to produce reliable results than certain other methods are.<sup>28</sup> Those procedures are not guaranteed to produce true theories; indeed, they generally do *not* produce

true theories. But they do tend to produce theories which are more reliable than theories selected by the other belief-forming policies we are aware of. The methods of science are not necessarily the best possible methods of inquiry (for how could we conceivably show that?), nor are the theories they pick out likely to be completely reliable. But we lose nothing by conceding that the methods of science are imperfect and that the theories of science are probably false. Even in this less-than-perfect state, we have an instrument of inquiry which is arguably a better device for picking out reliable theories than any other instrument we have yet devised for that purpose. We can explain in great detail why that instrument works better than its extant rivals. Because we can, the success of science ceases to be quite the mystery which some philosophers and sociologists have made it out to be.<sup>29</sup>

### Notes

1. I am grateful to a variety of friends who commented on previous versions of this essay and helped clarify my thinking about several of its central themes. They include Alberto Coffa, Clark Glymour, Rick Creath, Tom Nickles, Peter Barker, Arthur Donovan, David Hull, Rachel Laudan, Adolf Grünbaum, Andrew Lugg, Robert Butts, Ilkka Niiniluoto, Nicholas Rescher, and Gary Gutting, as well as several of my immediate colleagues.

2. See letter xiv in Voltaire's *Lettres Philosophiques* (Rouen, 1734).

3. Careful readers will take exception to my juxtaposition of realism and relativism in this way, pointing out that they are really not so much "opposites", as they are orthogonal to one another. Strictly speaking, after all, those who deny realism tend to be instrumentalists or idealists (rather than relativists per se), while the natural opponents of relativism are what we might call "objectivists". Nonetheless, it is illuminating to play realist and relativist perspectives off against one another since (a) they are rival *epistemic* traditions which are genuine contraries of one another (i.e., they cannot both be correct), and (b) as I point out below, there is a widespread tendency to assume that weaknesses in either count as arguments for its rival.

4. To be more specific, the form of relativism which I shall be discussing chiefly involves the denial that any techniques or methods for warranting knowledge claims are "better" than any others. One might call this the thesis of 'methodological relativism'; it is different from, and much more ambitious than, the thesis (often called 'ontological relativism') to the effect that no ontological framework is "privileged".

5. See, for instance, B. Barnes and D. Bloor, "Relativism, Rationalism, and the Sociology of Knowledge", forthcoming.

6. As I shall show shortly, however, it is important not to draw too strong an analogy between judgments of rationality and judgments of success.

7. See, for example, Hilary Putnam's treatment of the success of science in his *Meaning and the Moral Sciences* (London, 1978).

8. I am grateful to Alberto Coffa (private correspondence) for persuading me of the urgency of avoiding pinning our characterizations of scientific success on the aims, real or avowed, of working scientists.

9. For a lengthy discussion of some of the changes which have taken place in the cognitive goals of scientists, see my *Science and Hypothesis* (Dordrecht, 1981) and *Science and Values* (Berkeley, forthcoming).

10. No claim about the relative success of science would be complete without reference to Paul Feyerabend's recent tirade against the thesis that science has been successful. In his *Science and a Free Society* (London, 1978), especially pp. 100ff., Feyerabend asserts that the results of science (i.e., its successes) are no more impressive than the successes achieved by the cosmologies of many "primitive" societies. Feyerabend goes on to claim that science *appears* to be more successful than other systems of nature only because of the systematic suppression of other approaches in our culture: "*Today science prevails not because of its comparative merits, but because the show has been rigged in its favour*" (p. 102; italics in original). Earlier ways of studying nature "have disappeared or deteriorated not because science was better but because the apostles of science were the more determined conquerors . . . [who] materially suppressed the bearers of alternative cultures" (p. 102; Feyerabend's italics). As he warms to his topic, his claims become even more vitriolic: "The superiority of science is the result not of research, or argument, it is the result of political, institutional, and even military pressures." *Ibid.* As with most of Feyerabend's more provocative theses, this is more bluster than substance. Pointing (quite rightly) to the fact that pre-scientific cultures have made many very useful discoveries about how to manipulate nature, he concludes that the ideologies of those cultures are as successful empirically as the theories of science (or that they would have been if we had not systematically eradicated their advocates). This, clearly, is a monumental *non sequitur*. The judgment that science is more successful (in the sense spelled out above) than the nature philosophies of other cultures is, as Feyerabend is more than clever enough to realize, entirely compatible with the claim that non- or pre-scientific cultures have produced theories and methods which sometimes work well for their purposes. But since an acknowledgment that science has been more successful than those rivals would undercut Feyerabend's epistemic anarchism, he conveniently fails to alert his readers to the fact that the slide from the claim that pre-scientific theories have enjoyed some successes to the thesis that those theories have been as successful as, or more successful than, science is a monumental piece of bad reasoning.

11. H. Putnam, *Mathematics, Matter and Method* (Cambridge, 1975), p. 73.

12. See references to, and criticism of, the work of these authors in my "A Confutation of Convergent Realism", *Philosophy of Science* 48 (1981): 19-49.

13. To say as much is probably to make life too easy for the realist. Since Duhem, it has been widely recognized that theories typically do not impinge on experience directly but only in conjunction with a wide variety of other auxiliary assumptions. Under such circumstances, it is entirely possible that a theory could be true *simpliciter*, and yet such that all the observed consequences we attribute to it (derived from conjunctions of that theory with auxiliary assumptions) could be false. (I owe this point to Jarrett Leplin.)

14. See my "A Confutation of Convergent Realism".

15. *Ibid.*

16. I shall resist the temptation to dwell on the fact that the relativist evidently *exempts* his own methods of theory evaluation from this general relativist critique. Since numerous authors have pointed to this apparently self-refuting feature of relativism, I shall not discuss it at length. (See, for instance, my "A Note on Collins's Blend of Relativism and Empiricism", *Social Studies of Science*, 12 (1982), 131-132.)

17. Perhaps the most strenuously relativist of this group is Harry Collins, who seem-

ingly denies that there is any sense in which we can say that science is successful in predicting the world. Because Collins believes that "the natural world has a small or non-existent role in the construction of scientific knowledge" (H. Collins, "Stages in the Empirical Program of Relativism", *Social Studies of Science* 11 (1981): 3) and that "reality [does nothing] to circumscribe possible individual beliefs" (H. Collins and G. Cox, "Recovering Relativity: Did Prophecy Fail?", *Social Studies of Science* 6 (1976): 437) he is presumably forced to deny that any meaning can be attached to the claim that any system of belief is any more successful than another.

18. In fact, I believe that the success of science does refute most of the extreme forms of relativism, but it would require an independent argument to show that, and its development would carry the narrative too far afield.

19. This is not to say that all parts of science are predictively reliable, nor even that science is always more reliable than other ways of second-guessing the future. The specific claim is a limited one: to wit, that certain scientific theories have a much better predictive track record than most of their extant, non-scientific counterparts.

20. In using this example, I do not mean to suggest that all the cognitive aims or interests associated with Western science correspond to identifiable technical interests which we can identify across a broad spectrum of cultures. I do mean to insist, however, that there are some interests which cut across boundaries of culture and society. It is an issue of great consequence, both intellectually and practically, whether the methods of warranting associated with science do or do not promote those aims.

21. Throughout this essay, I adopt three simplifying assumptions to make my task more manageable: (a) that there is a set of methods which we can identify as "scientific"; (b) that these methods are shared between proponents of rival theories; and (c) that these methods do not radically underdetermine theory choice. All these assumptions have been hotly contested by various relativists. I have elsewhere tried to defuse the force of the relativist's arguments on these issues. (I take on the first claim in "What Remains of the Scientific Method", forthcoming; the second in my *Science and Values* [Berkeley, forthcoming], and the third in my "Overestimating Underdetermination" forthcoming.) Readers will have to judge for themselves whether my arguments make it plausible for me to adopt here the simplifying assumptions indicated above.

22. I am not asserting categorically that Western hydrology would necessarily surpass all the rival, apparently "non-scientific" techniques for treating these phenomena. I do not know whether it would or not. I am, rather, showing how one might go about ascertaining which parts of science exhibit a degree of success which calls out for special explanation. (Obviously, should the folk wisdom of some societies produce theories which are consistently more successful than science, that would equally call for some form of special explanation.)

23. One sees precisely this assumption in the influential work of Mary Douglas. She argues, for instance, that "it is no more easy to defend . . . objective scientific truths than beliefs in gods and demons" (Mary Douglas, *Implicit Meanings* [London, 1975], p. xv). In effect, her analysis denies in principle that any methodological defense of the claims of science could be given which would show that those claims were better grounded than the beliefs of any non-scientific culture.

24. The relativist's cause is aided and abetted here from some unusual quarters, not least from the arch-rationalist, Imre Lakatos. He has claimed on numerous occasions that any point of view, however bizarre—if only it is provided with enough funds and talented advocates—can accumulate impressive empirical successes. (One should add, for the historical record,

that Lakatos never provided any evidence for this assertion of his. One suspects he threw it in as a sop to his relativist friend Paul Feyerabend!)

25. Popper wrote: "No theory of knowledge should attempt to explain why we are successful in our attempts to explain things" *Objective Knowledge* (Oxford, 1972). The problem, of course, is that if epistemology cannot illuminate that problem, it is not clear what interesting tasks would remain for epistemology.

26. When I say that one theory is more reliable than another, I simply mean to refer to the fact that one theory is apt to be more useful, to be able to digest a larger and more disparate range of phenomena before it breaks down, than a theory which is less reliable.

27. The astute reader may note that neither of my examples of testing procedures involved the testing of that sort of deep-structure theory which is beloved by realists. But that omission reflects no limitation on the explanatory technique sketched out here. Basically, we test our most deep-structure theories, and credit them with success or failure, in precisely the same way that we test theories which are "closer" to "observation" (such as the two examples I discussed). Thus, if one wanted to explain the relative success of the atomic theory, one might venture to show that the battery of tests to which that theory had been subjected was more demanding than the sorts of tests which (say) hermetical theories of chemical structure had passed.

28. My discussion of the last few pages probably suggests that the justification of these methods is more straightforward and less problematic than it actually is. It would be less than candid not to note that there is some serious disagreement about exactly what rationale to give for some of the standard procedures of empirical control. But I would claim that the broad outlines of a rationale for such methods are clearly understood; that we know what technical and justificational problems confront us, and that we have some ideas about how to resolve them. In no case does it seem that such justificatory moves require us to go in the direction of scientific realism, i.e., of basing our explanation of the success of the methods of science on the thesis that the theories which science produces are true or nearly true.

29. Before I close, it is worth noticing how the approach to the problem of success sketched here exhibits the gratuitousness of the realist's would-be solution to the problem. If we can explain why the methods of science are apt to produce theories which are more reliable than theories produced by other methods, then we need not commit ourselves, as the realist evidently must, to a dubious claim about the truth or truthlikeness of the theories of science. Going beyond that reliability to postulate that our theories correctly characterize the world via their deep-structural commitments is to assume both more, and less, than is necessary to explain why scientific theories work as well as they do.