SCIENTIFIC REALISM

Edited, with an Introduction, by

JARRETT LEPLIN

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UNIVERSITY OF CALIFORNIA PRESS
Berkeley Los Angeles London

University of California Press
Berkeley and Los Angeles, California

University of California Press, Ltd.
London, England

Library of Congress Catalog Card Number: 84-40311
ISBN 0-520-05155-6 cloth
0-520-05326-5 paper

Printed in the United States of America

1 2 3 4 5 6 7 8 9

Preface

This book originated in a conference on scientific realism sponsored by the Department of Philosophy of the University of North Carolina, Greensboro, March 26-28, 1982. The first five essays in this book are the principal papers from that conference. Commenting on these papers were, respectively, Larry Laudan, William Boos, John King, Roger Jones, and Jarrett Leplin. Commentaries are not included since they were taken into account in preparing the final versions of the essays.

The remaining six essays were selected to represent the major current positions and directions of research on the topic of scientific realism. Represented only indirectly are such seminal writers on realism as Thomas Kuhn and Paul Feyerabend, whose works are well known and widely available.

Six of the essays have been published previously; Ronald Laymon's in *PSA* 1982; Hilary Putnam's as lecture 2 of *Meaning and the Moral Sciences* (London: Routledge and Kegan Paul, 1978); Ian Hacking's in *Philosophical Topics*, vol. 13, no. 1; Jarrett Leplin's in *Studies in History and Philosophy of Science*, vol. 12, no. 1; Larry Laudan's in *Philosophy of Science*, vol. 48, no. 1; and Bas van Fraassen's in *Journal of Philosophy*, vol. 73, no. 18.

I wish to thank the participants in the Greensboro Symposium, Ernan McMullin especially, for cooperation and assistance. I also wish to thank the University of North Carolina at Greensboro for its financial support of the Annual Symposium in Philosophy.

J. L.

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Introduction

Jarrett Leplin

Hilary Putnam seems to have inaugurated a new era of interest in realism with his declaration that realism is the only philosophy that does not make the success of science a miracle. None of the authors of the present papers either denies that science is successful or holds the success of science to transcend human comprehension. But there is much disagreement as to what that success consists in, how it is to be explained, and the role of realism in its explanation. Like the Equal Rights Movement, scientific realism is a majority position whose advocates are so divided as to appear a minority. The following theses are characteristic realist claims no majority of which, even subjected to reasonable qualification, is likely to be endorsed by any avowed realist:

1. The best current scientific theories are at least approximately true.
2. The central terms of the best current theories are genuinely referential.
3. The approximate truth of a scientific theory is sufficient explanation of its predictive success.
4. The (approximate) truth of a scientific theory is the only possible explanation of its predictive success.
5. A scientific theory may be approximately true even if referentially unsuccessful.
6. The history of at least the mature sciences shows progressive approximation to a true account of the physical world.

7. The theoretical claims of scientific theories are to be read literally, and so read are definitively true or false.
8. Scientific theories make genuine, existential claims.
9. The predictive success of a theory is evidence for the referential success of its central terms.
10. Science aims at a literally true account of the physical world, and its success is to be reckoned by its progress toward achieving this aim.

What realists do share in common are the convictions that scientific change is, on balance, progressive and that science makes possible knowledge of the world beyond its accessible, empirical manifestations. Unless progress is understood in purely pragmatic terms and knowledge is held not to require truth by correspondence, antirealists will reject these convictions. The objections they offer center on two principal problems.

One problem is historical. Whatever continuity may be discerned in the growth of empirical knowledge, theoretical science has been radically discontinuous. Scientific views about the ultimate structure and lawlike organization of the world have frequently been overthrown and replaced by incompatible views. Much of this discarded science was, for an appreciable time, eminently successful by the standards we employ in assessing current science. The inference seems inescapable that the evidence available to support current science is by nature unreliable and systematically underdetermines what ought to be believed about the world beyond our experiences. Scientific theories, however well secured by observation and experiment, are inevitably fallible. Nor is there any basis for expecting the future evolution of scientific standards and methods to provide a more secure foundation for scientific knowledge. For methodological developments that have occurred thus far, whatever improvements they have generated at the level of human interaction with nature, have failed utterly to resolve the basic dilemma of the underdetermination of theory.

Theory change alters the characterization science gives of the entities and processes alleged to constitute the world. Even where the same entities and processes appear to be countenanced by successive theories, their descriptions are so altered as to make it impossible to discern referential stability if reference is at all dependent on accounts of the nature of the referent. Thus, history appears to discredit our ability to identify the actual constituents of the world as much as it does our ability to learn their true natures.

The second problem concerns the explanation which an imputation of truth or approximate truth to a theory can give of its empirical success. Even if a theory were true or approximately so, that fact about it could easily fail to be reflected in success. A true theory, unless complete in some global sense, might be too remote from our experience to affect it in any way, or to affect it differently from some false alternative. Inaccuracies in the background assumptions made in applying a theory might produce predictive failure. And the retreat to approximate truth, in addition to the vagueness it introduces, forfeits even a presumption of success should the area of inaccuracy happen to be crucial to our experience of the world. A theory that gets most everything right, missing just some fact about photons, say, might easily number among the least successful in laboratory appraisal.

Conversely, a theory not even approximately true could be empirically impressive through the invocation of opportune auxiliary assumptions or chance agreement at the level of testable generalizations with one more verisimilar. As alternative theoretical structures can often be posited for the same phenomena, such agreement should occasion no surprise. And the presence of true statements within the consequence classes of false ones needs no explanation. One may hope that the application of theories in new areas will yield differential success, that false theories will eventually yield a preponderance of false, testable consequences. But whether theories are necessarily discriminable in this way is dubious, and at any given time the evidential picture is indecisive. The successful extension of a theory to new areas yields a greater body of corroborations from which further experience may yet diverge. And the idea that successful extendibility has any special epistemic significance as against the sheer quantity of the resultant successes is difficult to sustain, extendibility reflecting as much on the limitations of our initial perspective as on the merits of our theory.

A further and more fundamental aspect of the alleged connection between truth and success has recently emerged as a source of antirealist argumentation. This aspect concerns the assumption implicit in realist views based on theses 3 and 4 of the legitimacy of abductive reasoning or "inference to the best explanation." If such reasoning is indeed legitimate, it may be used within science to infer the truth of hypotheses directly from their explanatory and predictive successes, thus obviating recourse to the explanatory power of realism with respect to scientific success generally. If, instead, such reasoning is suspect, if explanatory status is judged an insufficient basis for inference, then an explanationist defense of realism can be no more cogent than the suspect support which observational evidence provides theoretical hypotheses within science. In either case, realism gains nothing from its alleged explanatory status; if not superfluous it is question-begging.

This argument, powerful as it may appear, has an important limitation. It is ineffective where it is not abduction itself that is questioned, but the need within science to posit unobserved entities in suitable explanations. Even if it is possible to account nonrealistically for individual successes of individual theories, there may yet be explananda for realism in the overall successfulness of scientific method. The proper target of explanationist realism is not the antirealist who distrusts inference to the best explanation, but the nonrealist who doubts that reference to unobserved entities provides the best explanation of what is observed.

I believe it is fair to say that neither the problem posed by the historical record of theory change nor the problem about the connection between truth and success has been solved even to the satisfaction of realists. At present, the most promising realist strategies are to argue that these problems are indecisive or to argue for realism independently, so that one has, as it were, an existence proof for the solutions one lacks.

Thus, one might follow Putnam in invoking the causal theory of reference on which radical change in accounts of the nature of the referents of scientific terms is compatible with referential stability through theory change. Or one might deny that approximate truth requires referential success. Approximate truth, after all, is a concept in need of analysis; even if the truth of a statement requires that its purportedly referential terms be genuinely referential, it is not clear that approximate truth requires this. Or one might deny the alleged datum of discontinuous conceptual change, insisting on a sufficiently cumulativist reading of history to permit referential stability on a more traditional, Fregean theory of reference.

Alternatively, one might attempt to identify features of scientific method or scientific reasoning that are unintelligible or empty but for realism. The argument then is that no form of antirealism can do justice to the scientific enterprise. This approach must either supply a noncircular defense of abduction, or show in just what respects a realist explanation of science is superior to the explanations which theoretical hypothesis give of the data they successfully predict.

Among the present papers, those of Levin, Glymour, Laymon, Boyd, and Hacking pursue the latter alternative; those of Putnam, McMullin, and Leplin, the former. Levin's concern is to refute instrumentalism by portraying it as a view incapable of yielding distinctions crucial to understanding the content of theories. Thus Levin endorses thesis 8. He finesses the historical problem by endorsing thesis 9, denying that the record of scientific failure supports any inference as to the credibility of current science. He denies, however, that realism gains any support from the explanationist argument reflected in theses 3 and 4. Glymour argues that the comparative assessment of explanations offered within science often requires thesis 2, showing inter alia that extant accounts of scientific explanation fail to sustain comparative assessments even in paradigm cases. Laymon argues that recognition of the role of idealization in theory testing leads to an account of confirmation requiring thesis 9. Boyd's principal concern is to show that certain features of scientific method, in particular the instrumental reliability of the theory-dependent methodology of the "mature sciences," lead inevitably to realism in the form of theses 1, 2, and 4. Thus he argues that neither the empiricist tradition, which invalidates all inference to unobservables on the basis of the underdetermination in principle of theories, nor the constructivist tradition, which denies the independent reality of the objects of scientific knowledge on the basis of the theory dependence of method, has the resources to explain the empirical success of the mature sciences. This focus on the explanatory resources of realism in contrast with that of its major alternatives is supposed, by Boyd, to provide the ultimate rejoinder to antirealist arguments which attack the legitimacy of abductive reasoning as such. Hacking's version of realism is closest, among the options distinguished, to thesis 2. But it purports to differ significantly from familiar approaches in focusing on the nature of experimentation in science as against theories and their successes. Hacking sees experimentation as a largely autonomous activity; when liberated from the presumed constraints of theory testing, it is seen to have realist implications. He supposes that these implications obviate questions about the reliability and underdetermination of theories.

Putnam formulates the basic explanationist case for realism as the only philosophy that accounts for the success of scientific method. He allows that realism requires theoretical continuity across conceptual revolutions; it must be possible to recognize stability of reference, at least to the extent of assigning referents to past theories from the viewpoints of their successors, and to recover past theories as limiting cases of their successors. Should this not be possible, should it turn out that paradigmatically successful theories get replaced by theories postulating radically different entities and laws to which nothing formerly recognized approximates, then realism would not be a tenable position. This would not mean, however, that we would abandon altogether the notions of truth and reference. We would still have important uses for concepts possessing the formal properties of truth and reference, such as are captured by Tarski's theory of truth; only the concepts we would use would be theory relative—such as warranted assertability or provability within a system—rather than classical ideas based on correspondence to theory-independent fact.

Putnam does not, however, believe that the history of science requires such a retreat from the classical ideal. By denying that the descriptions used in fixing the reference of scientific terms are synonymous with them, it becomes possible to preserve reference across the substantial descriptive changes that accompany theory replacement. The key is a principle of charity according to which a historical figure is to be credited with having referred to whatever entity countenanced by current theory answers to enough of the descriptions he used so that it is reasonable to suppose he would have identified this entity as the referent, had he known more of the relevant facts. Of course, "would have identified this entity as the referent" can only mean "would have altered his descriptions," which indeed he would on the supposition that he knows them to be mistaken. So what the principle of charity amounts to is the requirement that some substantive claims of past science survive as a condition for preservation of reference, together with a plea that some is enough.

The papers of McMullin and Leplin attack the historical problem directly. Leplin maintains that the historical record allows for referential stability, and develops an explanationist defense of realism along the lines of theses 3 and 4 which presupposes such stability. Leplin's approach purports to advance beyond the defense embodied in these theses by adducing a variety of independent forms of scientific progress to serve as explananda for realism. McMullin's realism is close to thesis 6, but the progress it diagnoses does not depend on achieving truth. Rather it occurs when a rejected theory is successful in indicating the direction to be taken by its successor. McMullin's analysis of such indications invokes the notion of metaphor. The existential commitments of theories are to be read metaphorically, and it is on the fertility and continuity of metaphor that the progressiveness and referential stability of theory change depend. Formal incompatibilities among theories fail to defeat realism because they do not preclude continuity at the level of metaphor.

The papers of Laudan, van Fraassen, and Fine are avowedly antirealist. Laudan develops the historical problem and the problem about truth and success in great detail, arguing that the realism of theses 1, 2, 3, 6, and 9 is empirically refuted. He attempts to convict the realist of violating, in his philosophical interpretation of science, standards of evidence he would certainly impose on science itself. This, in effect, is a version of the antiexplanationist argument described above: if hypotheses are not confirmed by the evidence they are introduced to explain, neither is realism confirmed by the success it purports to explain; if hypotheses are thus confirmed, realism is unnecessary. Leplin's attempt to distinguish independent forms of progress is motivated by just this problem. Van Fraassen focuses on the problem of underdetermination, arguing for the possibility of empirical equivalence in principle of theories differing in their ontological commitments. Van Fraassen rejects thesis 10, insisting that the aim of science is the empirical adequacy of theories. Fine, proclaiming the death of realism, offers the most sweeping and original attack on explanationism as a defense of realism. A .cording to Fine, the circularity of explanationism could be broken only by employing in defense of realism a form of reasoning more stringent than the abductive reasoning found wanting within science. But no such additional stringency is available, as the contents of individual theories cannot be compared with theory-independent facts to provide the basis for an inductive inference to realism. Fine does, however, sanction the inference from evidence to hypothesis within science, which is very close to realism in the form of thesis 9. The difference is that acceptance of a hypothesis need not, for Fine, involve any of the metaphysical implications which the truth of the hypothesis has been thought to carry in the realist tradition. What Fine calls the "natural ontological attitude" views the explanatory and inferential structures of scientific reasoning as autonomous; metaphysical interpretation is a dispensable superaddition. Thus, Fine agrees with Levin that philosophical accounts of truth and reference have no explanatory role in understanding scientific success or underwriting scientific conclusions. Science all by itself says all that needs to be said or can defensibly be said in response to philosophical questions about the nature and status of scientific knowledge.

As my contribution to this volume indicates, I number among the realists. The qualification is that I see serious historical problems in the way of crediting extant science with the sort of success which it is possible to argue that realism alone can explain. Realism is among the growing number of philosophical theories which like many scientific theories are partly metaphysical and partly empirical; it has implications beyond experience but is testable by experience. Most parties to the dispute tend to suppose that insofar as realism is an empirical thesis, the facts needed to assess it are in. Realism is either warranted by the impressive record of scientific success, or refuted by the discontinuities of theory change or the substantive findings of quantum mechanics. If problems remain in the way of assessing realism, they are not of a kind to be solved by further evidence. Despite this attitude, there is controversy over the nature and interpretation of the evidence as well as over the doctrine itself. We will need more history as well as more philosophy to settle the issues aired in this volume and to reach a viable theory of the nature and scope of scientific knowledge.

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A Case for Scientific Realism

Ernan McMullin

When Galileo argued that the familiar patterns of light and shade on the face of the full moon could best be accounted for by supposing the moon to possess mountains and seas like those of earth, he was employing a joint mode of inference and explanation that was by no means new to natural science but which since then has come to be recognized as central to scientific explanation. In a retroduction, the scientist proposes a model whose properties allow it to account for the phenomena singled out for explanation. Appraisal of the model is a complex affair, involving criteria such as coherence and fertility, as well as adequacy in accounting for the data. The theoretical constructs employed in the model may be of a kind already familiar (such as "mountain" and "sea" in Galileo's moon model) or they may be created by the scientist specifically for the case at hand (such as "galaxy," "gene," or "molecule").

Does a successful retroduction permit an inference to the existence of the entities postulated in the model? The instincts of the working scientist are to respond with a strong affirmative. Galaxies, genes, and molecules exist (he would say) in the straightforward sense in which the mountains and seas of the earth exist. The immense and continuing success of the retroductions employing these constructs is (in the scientist's eyes) a sufficient testimony to this. Scientists are likely to treat with incredulity the suggestion that constructs such as these are no more than convenient ways of organizing the data obtained from sophisticated instruments, or that their enduring success ought not lead us to believe that the world actually contains entities corresponding to them. The near-invincible belief of scientists is that we come to discover more and more of the entities of which the world is composed through the constructs around which scientific theory is built.[1]

But how reliable *is* this belief? And how is it to be formulated? This is the issue of scientific realism that has once again come to be vigorously debated among philosophers, after a period of relative neglect. The "Kuhnian revolution" in the philosophy of science has had two quite opposite effects in this regard. On the one hand, the new emphasis on the *practice* of science as the proper basis for the philosophy of science led to a more sensitive appreciation of the role played by theoretical constructs in guiding and defining the work of science. The restrictive empiricism of the logical positivists had earlier shown itself in their repeated attempts to "reduce" theoretical terms to the safer language of observation. The abandonment of this program was due not so much to the failure of the reduction techniques as to a growing realization that theoretical terms have a distinctive and indispensable part to play in science.[2] It was only a step from this realization to an acknowledgment that these terms carry with them an ontology, though admittedly an incomplete and tentative one. For a time, it seemed as though realism was coming into its own again.

But there were also new influences in the opposite direction. The focus of attention in the philosophy of science was now on scientific change rather than on the traditional topic of justification, and so the instability of scientific concepts became a problem with which the realist had to wrestle. For the first time, philosophers of language were joining the fray, and puzzles about truth and reference began to build into another challenge for realism. And so antirealism has reemerged, this time, however, much more sophisticated than it was in its earlier positivist dress.

When I say 'antirealism', I make it sound like a single coherent position. But of course, antirealism is at least as far from a single coherent position as realism itself is. Though my concern is to construct a case for realism, it will be helpful first to survey the sources and varieties of antirealism. I will comment on these as I go, noting ambiguities and occasional misunderstandings. This will help to clarify the sort of scientific realism that in the end can be defended.

SOURCES OF ANTIREALISM: SCIENCE

CLASSICAL MECHANICS

It is important to recall that scientists themselves have often been dubious about some of their own theoretical constructs, not because of some general antirealist sentiment, but because of some special features of the particular constructs themselves. Such constructs may seem like extra baggage—additional interpretations imposed on the theories themselves—much as the crystalline spheres seemed to many of the astronomers of the period between Ptolemy and Copernicus. Or it may be very difficult to characterize them in a consistent way, a problem that frequently bedeviled the proponents of ethers and fluids in nineteenth-century mechanics.

The most striking example of this sort of hesitation is surely that of Newton in regard to his primary explanatory construct, *attraction*. Despite the success of the mechanics of the *Principia*, Newton was never comfortable with the implications of the notion of attraction and the more general notion of force. Part of his uneasiness stemmed from his theology; he could not conceive that matter might of itself be active and thus in some sense independent of God's directing power. The apparent implication of action at a distance also distressed him. But then, how were these forces to be understood ontologically? *Where* are they, in what do they reside, and does the postulating of an inverse-square law of force between sun and planet say anything more than that each tends to move in a certain way in the proximity of the other?

The Cartesians, Leibniz, and later Berkeley, charged that the new mechanics did not really *explain* motion, since its central notion, *force*, could not be given an acceptable interpretation. Newton was sensitive to this charge and, in the decades following the publication of *Principia*, kept trying to find an ontology that might satisfy his critics.[3] He tried "active principles" that would somehow operate outside bodies. He even tried to reintroduce an ether with an extraordinary combination of properties—this despite his convincing refutation of mechanical ethers in *Principia*.[4] None of these ideas, however, were satisfactory. There were either problems of coherence and fit (the ether) or of specification (the active principles). After Newton's death, the predictive successes of his mechanics gradually stilled the doubts about the explanatory credentials of its central concept. But these doubts did not entirely vanish; Mach's *Science of Mechanics* (1881) would give them enduring form.

What are the implications of this often-told story for the realist thesis? It might seem that the failure of the attempts to interpret the concept of force in terms of previously familiar causal categories was a failure for realism also, and that the gradual laying aside in mechanics of questions about the underlying ontology was, in effect, an endorsement of antirealism. This would be so, however, only if one were to suppose the realist to be committed to theories that permit interpretation in familiar categories or, at the very least, in categories that are immediately interpretable. Naive realism of *this* sort is, indeed, easily undermined. But this is not the view that scientific realists ordinarily defend, as will be seen.

How should Newton's attempts at "interpretation" be regarded, after the fact? Were they an improper intrusion of 'metaphysics', the sort of thing that science today would bar? The term 'underlying ontology' that I have used might mislead here. A scientist *can* properly attempt to specify the mechanisms that underlie his equations. Newton's ether *might* have worked out; it was a potentially testable hypothesis, prompted by analogies with the basic explanatory paradigm of an earlier mechanical tradition. The metaphor of "active principle" proved a fruitful one; it was the ancestor of the notion of field, which would much later show its worth.[5]

In one of his critiques of "metaphysical realism," Putnam argues that "the whole history of science has been antimetaphysical from the seventeenth century on."[6] Where different "metaphysical" interpretations can be given of the same set of equations (e.g., the action-at-a-distance and the field interpretations of Newtonian gravitation theory), Putnam claims that competent physicists have focused on the equations and have left to philosophers the discussion of which of the empirically equivalent interpretations is "right." But this is not a good reading of the complicated history of Newtonian physics. First and foremost, it does not apply to Newton himself nor to many of his most illustrious successors, such as Faraday and Maxwell.[7]

Scientists have never thought themselves disqualified from pursuing one of a number of physical models that, for the moment, appear empirically equivalent. As metaphors, these models may give rise to quite different lines of inquiry, leading eventually to their empirical separation. Or it may be that one of the alternative models appears undesirable on other grounds than immediate empirical adequacy (as action at a distance did to Newton). If prolonged efforts to separate the models empirically are unsuccessful, or if it comes to be shown that the models are in principle empirically equivalent, scientists will, of course, turn to other matters. But this is not a rejection of realism. It is, rather, an admission that no decision can be made in this case as to what the theory, on a realist reading, commits us to.

What makes mechanics unique (and therefore an improper paradigm for the discussion of realism with regard to the theoretical entities of science generally) is that this kind of barrier occurs so frequently there. This would seem to derive from its status as the "ultimate" natural science, the basic mode of explanation of motions. The realist can afford to be insouciant about his inability to construe, for example, "a force of attraction between sun and earth . . . [as] responsible for the elliptical shape of the earth's orbit" in ontological terms, as long as he *can* construe astrophysics to give at least tentative warrant to his claim that the sun is a sphere of